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Page 335, line 17 from the bottom, *for* the poison that, *read* that the poison.

ANNALS

OF

PHILOSOPHY.

JULY, 1826.

Article I.

Notice of a new Form of Carbon supposed to be the pure Metallic Basis of the Substance; and also of several other interesting Aggregations of Carbon, especially in so far as they elucidate the History of certain Carbonaceous Products found in Coal Gas Manufactories. By Hugh Colquhoun, MD.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *May* 20, 1826.

There is scarcely any substance which acts a more conspicuous part in the economy of nature than carbon. In the animal, the vegetable, and the mineral kingdoms, it is equally abundant and useful, nor can any thing be more instructive than to study the infinite variety of purposes which it serves, or more interesting than to observe the wonderful diversity of forms which it puts on. What appearances can be in more perfect contrast to each other than those of soot on the one hand, and of the diamond on the other? Yet carbon embraces both extremes; and by a chain of not very imperfect gradation, it may even be traced in different shapes through intermediate degrees, from the dull, black aspect, and soft texture of soot, up through states of brighter and brighter metallic lustre, and increased compactness, until at last it attains the hardness, and the sparkle, and the crystal of the diamond. It was a discovery which excited great attention at the time when Lavoisier, and after him Mr. S. Tennant, unveiled carbon from beneath this last disguise; if, indeed, this expression be philosophically admissible, and if it be not impossible to say that any one state of existence is more natural or more proper to the substance than another. For although a form very different from that under which a substance has been commonly known may be popularly termed a *disguise*, under which it has concealed itself, yet it is not more truly so than any other of its shapes; and it is always a decided step in the progress of science, when an entirely new form, especially of an important body, is obtained. This is in fact to approximate the laboratory of the chemist to the great officina of nature, where many a substance must exist in states very different indeed from any which the industry of the experimenter has yet enabled him to discover, or his art to make it assume.

It is to several highly interesting states of aggregation of carbon, one of which is not only of a very singular structure, but also an entirely new form, that it is the object of this notice to direct attention.

This last-mentioned formation occurred in the new steelifying process of Charles Macintosh, Esq. of Crossbasket. I had undertaken, at the request of that gentleman, who is an eminent practical chemist, to superintend, during his temporary absence, a series of experiments intended to ascertain the best details of practice and apparatus for his most ingenious process of the conversion of iron into steel. The principle of his invention is probably well known ere this to a great part of the men of science, as well as to the mechanics of this country. It consists in introducing into the contact of iron, ignited to nearly a white heat in an air-tight earthen vessel, a current of a gas containing *carbon* as one of its constituents, and which in practice is usually carburetted hydrogen gas. In this operation the gas undergoes a partial decomposition, occasioned probably by various causes, but principally by the influence of the high temperature to which it is exposed, and by the mutual reaction which takes place in that temperature between it and certain other of those gaseous bodies, such as carbonic oxide, with which it is always, to a certain extent, intermixed. And the iron also, by virtue of its own peculiar affinity for carbon, will undoubtedly co-operate with the elevated temperature in rendering the disengagement of that substance from the hydrogen more complete.

The result of these decompositions is an abundant precipitation of carbon. This carbon, as it has just before been in chemical union with another body, exists of course in a state of the most minute division at the moment of its precipitation; and it is probably owing greatly to this circumstance that its particles are found to penetrate the iron more rapidly and more equably, and to form a more intimate union with the whole mass of it than was formerly practicable under the old method. Besides, there is found to result the additional advantage of a great saving of fuel on the system of Mr. Macintosh, because nothing could be more difficult than to ignite iron thoroughly when imbedded, as under the previous system, between two masses of solid charcoal, which is one of the very worst conductors of heat. When fresh gas continues to be introduced in proportion as the previous supply is deprived of its carbon, the process of precipitating the carbon in an impalpable powder, and of its penetrating and uniting with the metal, continues to go on, until the iron is saturated, and its conversion into steel is complete.

Such is the principle of the process, the practical details of which I was occupied for some time in superintending; and in the various experimental assays of different kinds of manipulation, nothing could be more interesting than the diversity of results attending the several separations of the carbon from the gas. In particular, when the steel chest, or vessel containing the iron, was traversed by an excess of carburetted hydrogen gas; that is, by a quantity from which the heat precipitated carbon in much greater abundance than the iron was capable of absorbing, the deposited particles of carbon, on their transition from an aëriform state, and free as they were from external communication with any foreign body, arranged themselves in singular varieties of form, and texture, and colour. The greater part of these I have met with in several other situations, as I shall by and bye notice, but there was one, and the most remarkable of all, which I have never even heard of as having any where else been found. On opening one day that part of the apparatus where the carbon formation, if any, was to be discovered, there appeared, lying irregularly in various parts of it, a quantity of long capillary threads of carbon, lustrous, and slender, each portion collected into a small bunch, as it were, of parallel lines, and in form extremely similar to a tress of fine hair. A single lock of this *mineral hair* seemed to contain thousands of these thin filaments. There is nothing to which this new form of aggregation of carbon can be better compared than a bunch of fine glass spun hair, or a tuft of one of the most perfect varieties of asbestus. There was nothing peculiar or uncommon about the working of the apparatus, or in the state of any of the materials, in so far as I could discover, to which the formation of so uncommon a product could be even conjecturally ascribed; and although in subsequently carrying forward the series of experiments, the same formation occurred, I was never able to trace it to any probable cause.

In this variety of carbon formation, there obtain many shades of difference, although the basis in every case, as I have established by careful experiment, is neither more nor less than pure carbon. Thus the hairs in point of length have a range of from an inch, or even less, to eight inches. In thickness, some of them may equal a hair from a horse's mane, while others are as delicate as the filaments of the lightest spider-web. The colour of the whole has been always black, and the lustre invariably bright and metallic. When one tries to bend their two ends together, they prove brittle, and snap short across; yet when the finger is pressed against the point of one of them, its resistance is such that it almost penetrates the skin before giving way. And in so far as the eye of even the metallurgist could guide him, their whole appearance would indicate that they had been in a state of fusion at the moment of their formation.

These are some of the characteristics of the more perfect specimens which I have yet obtained of this variety of pure carbon. But a much more imperfect formation, in which the colour, form, and texture, of more common specimens were blended and mixed with some of those distinguishing the kind just described, occurred not unfrequently in the course of my superintendence of the steelifying process. Sometimes the hairs were short, ragged, and uneven; sometimes quite destitute of lustre; and sometimes they existed so immingled in a loose, mossy, or spongy mass of carbon, as to be scarcely distinguishable.

I am quite aware that a species of appearance so uncommon and extraordinary, notwithstanding the very unequivocal circumstances under which it occurred, cannot be admitted to exist in a substance composed of *pure carbon* without the matter being put thoroughly to the test of repeated experiments. And I shall now, therefore, detail one or two of those which I immediately tried with a view to the determination of this point, and which seem to be satisfactory and conclusive.

When a few of the filaments above described were heated to redness in a glass tube, they gave off neither smoke nor vapour, and retained in all respects their original form unaltered, and their lustre unimpaired. Upon being again heated to redness in the flame of a candle, they remained in like manner entire and unchanged. But when they were intensely ignited in the interior flame of the blowpipe, they rapidly underwent combustion, the silent progress of which was occasionally interrupted by vivid coruscations, and by this treatment, they were quickly and completely dissipated.

I further found that these filaments possess the property of decomposing certain substances which part readily with oxygen, and of deflagrating with them exactly after the manner of charcoal or plumbago.

A quantity of the filamentous substance, to the weight of about half a grain, was taken, and intermixed thoroughly by trituration with upwards of thirty times its weight of chlorate of potash: the whole was then exposed in a platinum crucible to a strong red heat over a spirit-lamp. On the commencement of liquefaction in the salt, a few slight flashes broke out from those parts of the mixture near the heated sides of the crucible; but in a short time the whole salt had passed tranquilly into a state of fusion without undergoing any other apparent alteration. After the heat had been continued for some time longer, there took place an instantaneous and vivid deflagration, while the whole mixture became at once intensely red-hot. The saline residue resulting from this deflagration had a blackish colour, and contained diffused through it many particles of the filamentous carbon still entire and unconsumed. When the soluble part of this product was dissolved out with water, it appeared that the portion which had escaped oxygenation, probably owing to the rapidity with which the action of the chlorate of potash was begun and completed, amounted to little less than one-half of the whole original quantity of filamentous matter.

When a few of these filaments were similarly exposed to the action of nitre, a deflagration also took place; but in this instance the action of the salt was much more durable and prolonged, and the oxygenation of the whole filaments was complete. In this case, the saline residue proved to be of a pure white colour, and, on the addition of a little water, was to the last particle dissolved.

When muriatic or nitric acid was poured upon the aqueous solutions of these saline residues, a strong effervescence took place, and much carbonic acid was disengaged, accompanied with nitrous vapours.

To ascertain whether any hydrogen could be detected in this carbon, a small tuft of very delicate filaments, weighing 0·30 gr. was put into an agate mortar along with six grains of the black oxide of copper, and the whole thoroughly intermingled by trituration. The mixture was transferred into a tube of green glass closed at one extremity, and freed from hygrometric moisture by cautious exposure for a sufficient length of time to a temperature of about 240°. It was then, after having been allowed to cool, brought to a very strong red heat over a spirit-lamp. The result was the deoxidation of the oxide of copper, and the abundant disengagement of carbonic acid gas; but, during the whole progress of the experiment, there was not the deposition of the smallest trace of moisture. On decomposing, by a similar process, a small quantity of ordinary cherry coal, weighing 0·05 gr. a very distinct deposition of water was formed.

The conclusion from all these experiments seemed to be of necessity, the highly interesting demonstration, that in the composition of this carbonaceous substance, there is not present *a trace of hydrogen:* there was also the strongest indication, although in a manner not so decisive, that no foreign ingredient of a more fixed nature, whether earthy or metallic, made any part of it. In order to ascertain, however, a point of such importance, and to determine whether these long needles or filaments were really composed of pure carbon, I subjected them anew to the following experiments. Some of the filaments, weighing 0·62 grain, and which had been previously cut across into small pieces, were deflagrated in a platinum crucible along with 12·4 grains of nitre. The oxidation of the whole carbonaceous matter was in this experiment complete; for there was left nothing but a saline residue of a pure white colour, which dissolved in water without leaving any remainder whatever. This solution, after being mixed with an excess of muriatic acid, was heated moderately for some time in a sand-bath: it was then supersaturated with caustic ammonia, and again digested. But no precipitation whatever resulted from the addition of the alkali; and it followed, therefore, that the solution contained neither *oxide of iron nor alumina.* The clear liquid was now evaporated to dryness, and the dry residue digested in water. The whole dissolved, except an inconsiderable residue of a whitish-coloured matter, which, after careful washing and desiccation, amounted to 0·02 gr. This did not seem of consequence enough to merit a particular investigation of its nature, especially as, from the circumstances under which it was obtained, there can be little doubt of its having been silica. Indeed the quantity was so very minute, that it would perhaps be hazardous to say that some portion of it at least was not derived from part of the apparatus, or of the reagents employed in the course of the experiment.

The result of all these experiments seems therefore to be the very important one, that the substance formed out of carburetted hydrogen gas as previously described, in the moment of extrication from an aëriform state, is the *genuine basis of the gas,* or, in other words, *pure, solid,* and to all appearance *true metallic carbon.* And this filamentous conformation will only add one striking variety more to the list, already long, of singularly diversified appearances which that body has already been found to assume in one or other of the kingdoms of nature, in all of which it acts so prominent a part.

It is not without regret that I find myself quite unable to offer any satisfactory explanation, or even probable conjecture, respecting the peculiar cause which produced this very uncommon aggregation of carbon. Its occurrence in the steelifying process was, generally speaking, a rare event, and, in so far as I could judge, irregular and capricious. I remarked, however, that in all the cases in which it was found, it was either half imbedded among a mass of carbonaceous matter, or deposited on the surface of such a mass, which had accumulated above the metal undergoing steelification. It seems, therefore, not improbable, that in all these cases, the apparatus had been traversed by a much larger quantity of carburetted hydrogen gas than was strictly necessary to carry forward the conversion of the iron exposed to it into steel; and the excess of carbon, in proportion as it was disengaged from its aëriform state from the expansion of the gas by heat, or from whatever other cause, seems to have formed itself, in the act of deposition, into the pure filamentous shape.

Besides this singularly beautiful carbon formation, which (not only as it seemed unique, but as the apparent extrication of the pure metallic basis of that substance) was certainly the most important phenomenon observed by me while superintending the steelifying process, there occurred several other aggregations of that substance during the progress of the same investigation, which, upon various accounts, deserve to be noticed here. In particular, their structure seemed so extremely analogous to that of certain products which are formed in all manufactories of coal gas, and as to the origin of which there is a schism among men of science, that the advantage of comparing together the two classes of products seemed to be too obvious to be passed over. For the history of the one set of substances, so analogous, so identical, indeed, as are these now alluded to, can scarcely fail to throw considerable light on the history of the other, and to assist in determining the question now at issue on this subject among chemists.

It is perhaps hardly necessary to mention that these aggregations of carbon were found by me within the steel chest or vessel containing iron while under steelification by the absorption of carbon precipitated from the gas in contact with it: the same accumulation of precipitated carbon, which, in one or two rare cases, was accompanied by the formation of filamentous carbon, frequently producing those less uncommon and less striking aggregations of which I am now to speak.

It would be difficult to class the different specimens I have met with by any well defined lines of distinction, and it would be equally useless to attempt a minute description of each; for though the colour and aspect of the carbon vary from a dull sooty appearance to that of a bright metallic lustre, though different portions break with a very various fracture, and though the substance is found sometimes in grains, sometimes in lumps, and sometimes, as has been already noticed, in beautiful long thin threads or hair, yet all these widely distinct kinds nevertheless shade away so much into one another, that it is often extremely difficult to determine the boundary between the one species and the other; and, in the same mass, most of the varieties are found occasionally blended together.

One of the most common appearances under which the carbon thus presented itself was that of a quantity of finely divided powder, bearing a strong resemblance to lamp-black, but, in general, very considerably harder in the grain and denser, or of greater specific gravity. Frequently, however, the particles of carbon were not in this loose pulverulent state, but were found collected into solid masses, possessing very different properties. While some of these were soft and friable, others were of a hard and compact structure. Some broke with a dull earthy fracture, and crumbled readily away between the fingers; while others with difficulty received any impression even from the scratch of a knife, and broke with a smooth conchoidal fracture, the lustre of which resembled in quality that of the finest specimens of indigo. Some of the harder varieties exhibited on their exterior surface a beautiful mammillated structure, and a very brilliant and perfect metallic lustre.

Such are the leading characters of the different forms of aggregation under which the carbon presented itself on different occasions in the steel chest. It would be easy to enter into much more minute details respecting them. I consider it sufficient, however, to have particularized them, only in so far as appears necessary for the purpose of illustrating such of their counterparts, as are of the most frequent occurrence in coal gas manufactories.

The manner in which the analogous carbonaceous formations occur is well known to the mechanics employed in that department of the arts. Within the elliptical retort, there is a gradual deposition and accumulation of them, first at the farther end, and next along the sides and towards the mouth of the retort. Such is the strong adherence of the carbon to the metal, that this process goes on notwithstanding all the endeavours of the workman to check it by breaking away the carbon as it forms, and in the end it causes such a waste of fuel before the fresh coal within the retort can be distilled, that the retort becomes useless. It generally happens long before this, however, that as carbon and iron expand with a very different rapidity on the application of equal heat to both, the sudden changes of temperature which are invariably consequent on the introduction of fresh coal into a retort in the process of gas making, occasion the cracking of the vessel long before the "burning out," as it is called by the workmen, or complete oxidation of the metal, has taken place.

The carbonaceous matter thus formed within the gas retorts, possesses considerable variety of appearance, a variety, however, which is completely parallel to that of the carbon found within the steel chest. Its structure, in the great, is stratified, large, and very irregular slaty, with a powdering of a black substance, like lamp-black or soot, between the strata; and it breaks more easily in the direction of the strata than across them. In the small, the stratified portions seem to the eye quite compact; they break with a smooth earthy fracture, and are almost always devoid of lustre. Their colour is iron grey, and their texture hard. They are difficult to reduce to powder, but are easily scratched by a knife. Occasionally specimens are found possessing a mammillated structure, with a brilliant and perfectly metallic lustre on their surface. In general, however, there is no peculiarity below the surface, which distinguishes this from the more common carbon formation.

When a retort has been cracked in the gas manufactory, as frequently happens from the causes above-mentioned, it is often an object to continue still to employ it for some time longer in the distillation of coal, so long as the fissure remains of minute dimensions. In these cases the gas which issues through the rent gradually deposits, on the inner surface of that crust of brickwork with which all such retorts are originally coated, or on the arch of brickwork by which the retort is supported, a carbon formation in general of the most perfect kind of the mammillated variety, in form and in metallic lustre. It is frequently composed of distinct columnar concretions, lying apparently nearly parallel to each other, but more correctly in the direction of the radii of a circle, the curvature of which is coincident with the surface of that particular part of the retort to which they are attached. The form of these concretions is stalactitic; and they closely resemble in beauty and appearance the grey stalactitic ores of oxide of manganese. These mammillations differ from those formed within the retort, both by being of a more friable composition, so as to be more easily reduced to powder, and in the powder of the former always possessing a distinct shining micaceous aspect, which in the latter is hardly discernible.

Such are a few of the formations of carbon found in coal gas manufactories, in so far as they are parallel in point of form, texture, and colour, to specimens of the same substance obtained from the steelifying process. It has been unnecessary to particularize minutely the points of resemblance between the individuals of the two sorts, for there is none of the aggregations generally mentioned to be met with in the one process, which does not find at least its miniature counterpart in the other. It is probable, however, that besides the coal gas retort, there may be many other situations in which a more extensive acquaintance with the arts would discover carbon formations of the same character with those we are now considering. And in particular I have more than once had occasion to remark a simultaneous production, both of the filamentous and of the mammillated varieties, take place in the close ovens in which coal is converted into coke for the use of the iron founder. On charging one of these ovens with a coal of the caking species, it first agglutinates into a single mass, and remains stationary for some time, till by and bye it sustains a general contraction and shrinkage, the consequence of which is that it becomes traversed by numerous wide clefts or rents. When the manufacture is complete, it is by no means unfrequent to find the sides of these fissures incrusted by beautiful specimens of the mammillated variety of carbon, and also, on the surface of this latter body, a small collection of delicate filamentous carbon. At the same time this filamentous variety was decidedly inferior in every characteristic to that which occurred in the steelifying process.

It was smaller in quantity, its fibres were short, curled, and confusedly interlaced among each other; it was also generally without lustre, and was therefore very different from the long parallel and slender filaments of the more perfect formation. These carbon formations among the rents of the coke, which are hailed by the manufacturer as the surest test of the success of his process, are found to occur generally in dry frosty weather, and seldom or never when the atmosphere is overcharged with moisture.

Those whose attention has been already attracted by the formation of mammillary carbon in the coal gas manufactory in the manner just related, have divided themselves into two parties with regard to the question of its origin. One considers it to be a deposition from volatilized coal tar, which certainly cannot be said to seem at all an inadequate cause, while another maintains that it is a precipitate from carburetted hydrogen gas; founding their opinion on the well ascertained fact, that this gas undergoes a partial decomposition when exposed to an elevated temperature. It is the latter account which the facts we have been detailing seem to prove to be in all probability the correct one; because the aggregations of carbon formed in the steelifying process out of carburetted hydrogen gas completely depurated from tar, are so congruous in their external characters to the metallic-looking mammillary carbon, that it is scarcely possible not to ascribe them, under all the circumstances, to a common origin. Besides, after subjecting the latter kind of carbon to experiments similar to those already detailed in relation to the former, it appeared that their essential chemical characters are the same. For both the compact variety of mammillary carbon, which accumulates within the interior of the gas retort, and those more beautiful mammillations deposited upon the inner crust of that brickwork which coats the outside of the retort, on being deflagrated with nitre, proved to be composed wholly of pure carbon, with the exception of some mere evanescent traces of earthy matter.

In making this statement, I think it right to add, that the results obtained by me in examining this mammillary carbon, are certainly somewhat different from those of Mr. Conybeare, who has given an interesting description of the varieties of carbon or plumbago, as they are commonly termed, which are found to form and accumulate within the retorts of coal gas manufactories. That gentleman mentions * that in deflagrating portions of such carbon with nitre, he discovered in them traces of iron. And he adds, that "his experiments gave him reason to think that the quantity of iron varies in different specimens, and that it scarcely amounts, at the most, to the nine per cent.

* *Annals of Philosophy*, New Series, v. 52.

stated by Berthollet to exist in native graphite." I was particularly observant, therefore, in trying the effect of the tests to which I submitted the mammillary carbon, to discover whether any traces of iron entered into its composition, and I can affirm with perfect confidence that in the specimens examined by me, not the slightest indications of its presence existed. It follows, therefore, that iron is in no respect necessary to the formation of the carbon of the mammillated and metallic-looking variety, and that if it ever occur as a constituent of such masses, it may nevertheless be regarded as foreign and extraneous.

Such are a few of the appearances presented by these interesting varieties of carbon, of which it has been the object of this notice to give some account. Of all the different forms assumed by that substance, it is believed there are few more singularly elegant and beautiful than that of the long slender lustrous filaments, which indeed require to be seen in order to be justly appreciated. The new and unprecedented nature of this shape of carbon also gave it a strong claim to our first regard. And yet, perhaps, the most surprising of its characteristics, that which points to the most important views of the wonderful and mysterious operations of nature, though carrying the onward eye towards a goal that is placed as yet beyond the ken of chemistry, is one that is possessed in common both by the filamentous and by some of the mammillated varieties. This is the strong indication which their external characters afford *of their forms having been assumed out of a state of fusion:* although, from considerations soon to be noticed, they probably owe their form to a totally different cause. The metallic and fused-like aspect is so strongly marked, that it may be safely said, the mere eye or tact of even the most experienced observer, could not point out any character which seems to lead to another origin. Now it is well known that hitherto, in the most intense heats which artificial means have been able to create, carbon has been found to exhibit not the slightest traces of even a tendency towards fusion. Nevertheless, in the varieties just described, in temperatures certainly below an ordinary white heat, we have almost all the evidence of this substance having been completely fused, which we can possess, short of actually seeing it melted to liquid. We have it, in one case, assuming a form entirely unknown before, which of itself would indicate that it passed through a state of existence before inexperienced; and we have in all respects the external characters and appearances of a body which had been fixed from a state of fusion. It should also be remarked that on the unlikely hypothesis of the carbon having undergone fusion, this could have taken place only in the moment of mammillary or filamentous formation; for when the substance so formed is again subjected to intense artificial heat, it remains, as happens with all the other forms of carbon, unchanged and unalterable. And what seems at present to be perhaps the most unaccountable part of the phenomenon, though, not improbably, the apparent anomaly may itself contain the elements of the future elucidation of the cause of this apparent fusion, is that the production of these specimens in the gas manufactories is neither sudden nor irregular, but the accumulated results of a gradual process going on for days and weeks together.

It is in vain at present to offer any account or hypothesis relative to the source of this apparent fusion of carbon. Difficult, however, as seems the chemical problem involved in the unexpected production of these aggregations, it cannot be denied that too many of the facts disclosed to us by the indefatigable research of modern chemistry, infer a similar appearance of inexplicable incongruity, and which, like the present, it is in vain to attempt to soften over or to shade away. What, for example, can seem more anomalous than the immediate formation of a solid ingot of pure copper from the aqueous solution of one of the saline combinations of that metal? or that of regular cubes of metallic titanium, within the substance of an iron slag, and which have been formed therefore at a temperature far below that which is necessary to operate their subsequent fusion?

These and other similar instances are as far from admitting a satisfactory explanation or solution, as the phenomenon of carbon being found in certain cases to assume the fused appearance at temperatures greatly below that in which it has long resisted every endeavour to effect its liquefaction. But *the fact* remains untouched, and must be fairly stated, even when it leaves all theory the most at fault. At the same time, upon the supposition of actual fusion, or upon the far more probable assumption (in the case of the steelifying process), that the particles of carbon newly extricated from their aëriform state are in their minutest subdivision as they cross the limit and enter upon the state of a solid, and being the most widely free of all relation to any definite previous shape, and excluded also from communication with external bodies, it may be permitted to conjecture that they aggregate themselves, or, as it were, crystallize, according to the laws of their natural polarity, and, in the elegant and lustrous tress of metallic filaments, assume an appropriate, though entirely new, and previously unknown, form.

But the solution of problems like these at present marks the knowledge and the ingenuity of the chemist. How long they are destined to remain among the secrets of nature, time only can discover. This much at least is certain, that while the details of each new chemical anomaly are interesting to the man of science from mere curiosity, they are doubly so to the philosopher who reflects, that the labour of compiling facts must go before the pleasure of constructing theories, and that fairly to state and appreciate a real difficulty is the first step towards surmounting it. For it is only from a careful compilation of details, from the joint accumulation of seemingly detached experimental results made by various investigators, that we are able to gain a vantage ground, from which we may enlarge the boundaries of the chemical horizon, discover relations previously unknown, and trace connexions between cause and effect before unsuspected. Indeed to one who takes a justly comprehensive view of chemistry, no fact, however apparently anomalous and paradoxical, can be truly said to be detached or unconnected with any part of the science, and to hold any other opinion would be to suppose the laws of nature variable and capricious. It is thus by gradually collecting facts and experiments, which may at first seem to puzzle from apparent diversity, that a careful comparison among them may at length discover certain general analogies and relations, which shall guide some happy genius to a system of principles in which the rationale of all that was once anomalous shall be explained.

It is indeed the humbler task to be the compiler of facts which are admitted to be beyond one's power even conjecturally to explain, when compared with the efforts of him for whom the former but paves the way, who extracts principles out of facts, infuses connected relation among previously detached and disjointed phenomena, and forms a harmonious system out of confusion. The latter is indeed a godlike flight, which few, save such as Newton or Lavoisier, dare aspire to. But it is a pleasure not unmingled with pride to think that the accumulation of new and interesting facts, while it adds to the general stock of our present knowledge, may also one day prove the means of leading some one forward on the higher quest. It is hoped, therefore, the facts detailed in the present notice may prove neither uninteresting now while they seem anomalous, nor useless hereafter in guiding some one directly, or by analogy, to give a satisfactory account of the causes which produce them.

Article II.

Remarks on some of Mr. Ritchie's Experiments on Radiant Heat. By the Rev. B. Powell, MA. FRS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

In a paper in the Edinburgh Philosophical Journal, No. 22, p. 281, Mr. Ritchie has given some experiments which he considers as establishing the fact, that radiant heat passes freely through very thin glass. This conclusion is in the first instance deduced from a very simple and elegant experiment, which consists in placing a hot body midway between the thin bulbs of a large differential thermometer; the liquid becomes stationary; the exterior half of one bulb is then blackened; and the liquid on that side is found to fall the instant the hot body is placed as before. Hence the author infers, that by rendering that part of the glass opaque, a portion of the heat is stopped; which therefore radiates through the transparent part of the bulb in this case, and through both parts when the whole is transparent. With a view to examining the validity of this conclusion, I conceived it desirable, in the first place, to repeat the experiment, and verify the result. This I did with a differential thermometer, having its bulbs about an inch in diameter. The half of one bulb away from the source of heat was coated in some instances with a cap of thin paper previously moulded to fit it exactly, by which a facility of removing and replacing the coating was obtained; in others with indian ink, or the smoke of a candle.

A hot iron ball was placed at an equal distance from each bulb; and before experiment, the liquid was observed to remain stationary.

On the bulb being placed in its position, the action was invariably *greatest on the coated bulb.* When the coating was removed (care being taken to keep the whole exactly in the same position), and to heat the ball exactly to the same point; viz. having made it red-hot, to let it cool down precisely till it ceased to be visibly luminous in the same dark place, the action was greatest on the plain bulb.

The results in one experiment were as follow; the divisions on the scale are arbitrary; the graduation *from* the plain bulb.

Min.	Sec.	One bulb half coated.	Both bulbs plain.
0		26	25·5
	30	25	28·5
1		22·5	31·5
	30	21	32·5
2		19	29·5
	30	18·75	28·5
3		18·5	27
	30	18	27
4		17·5	26·5
	30	17	25
5		15·5	26

Here we perceive, with the coating, a considerable action on the coated bulb; and the coating being removed, the action is entirely on the plain bulb.

In one instance the iron was so placed that there was a slight action on the plain bulb; the coating being removed this action was much greater.

All the trials I made in this way tend to confirm the *naked fact* observed by Mr. Ritchie; but with respect to the *explanation* he gives of it, I feel obliged to differ from him: the peculiar property which he would thus ascribe to radiant heat does not appear to me a necessary inference from these experiments. The result, I think, may be accounted for on another principle which will not require any new supposition; this is the circumstance of the expansion of the bulb by heat. When half coated, its radiating power is obviously increased; hence it will cool faster, the expansion of the glass will be less, and consequently the apparent initial expansion of the inclosed air greater than when it is plain. But though it is easy to conceive an effect of the nature described may be explained on the supposition of an expansion of the bulb; still a question may arise, and perhaps some hesitation be felt, as to whether this cause could act to so large an amount. In order to remove all doubt on this point, and especially as this is a source of fallacy in many experiments of a similar kind, I thought it advisable to give it a further examination.

This was done by observing the indications of the instrument during the process of cooling, after both bulbs had been equally heated by contact with a hot substance. The liquid was stationary before experiment, and continued so on the application of the heat; on removing the hot body (both bulbs being plain) in several repetitions, it remained stationary; in others there were slight oscillations.

The same thing was repeated with one bulb blackened as before, when there was always a considerable motion *towards* the plain bulb.

In some other trials, only one bulb was heated to a given point by conduction, and then left to lose its heat by radiation, both when plain and when coated. The following is one set of indications:—

Min.	Sec.	Bulb half coated.	Bulb plain.
0		24	24
	30	26	27
1		29	29·75
	30	31	31·5
2		33	33
	30	34	34
3		35	35
	30	36	35·5
4		36·5	36
	30	36·5	36·5
5		37	36·5
	30	37	36·75
6		37·5	37
	30	37·5	37·25
7		37·75	37·25
	30	37·75	37·25
8		37·75	37·25
	30	38	37·25
9		38	37·25
	30	38	37·25
10		38	37·25

Here the apparent rate of cooling when the bulb was plain was at least equal to that when coated, or perhaps even greater.

This result agrees with the last, and in both cases it is evident that if no other cause interfered, we ought to find the coated bulb cool considerably faster than the plain; but if no difference appear, or a contrary action take place, we must ascribe to the expansion of the glass an action equal to or greater than that of the opposite kind due to the radiating power of the coating.

Mr. Ritchie, in the same paper, has maintained his position by various other experiments. He used as screens pieces of a glass bulb blown to a great degree of tenuity, and compared the effect of such a screen when transparent with that exhibited when it was opaque; care being taken that the nature of the surface was the same in each case, by forming a compound screen of several thicknesses of glass, the opaque coating or lamina being interposed. But in all these experiments the effect was observed by means of a large differential thermometer; and considering the acknowledged uncertainty which attaches to the action of such an instrument, we can hardly feel implicit confidence in any results deduced with it, unless corroborated by those obtained by a more unexceptionable mode of observation. I have accordingly tried experiments of this kind, using a mercurial thermometer with the bulb blackened; but have not succeeded in obtaining any difference between the effect when the screen was opaque and when transparent.

These last experiments are detailed in the latter part of a paper which was read before the Royal Society on Thursday, June 1, the primary object of which was to examine a particular instance, in which M. De la Roche had concluded that simple heat permeates glass by direct radiation. If my experiments are to be relied on, that conclusion is rendered unnecessary, the facts having been shown to admit an explanation on the common principle of a secondary radiation. The experiments in the present paper were not made till after that communication had been sent to the Royal Society, which was on the 9th of March last, or they should have formed a part of it.

Article III.

On the Production of Acetic Acid, in some original Experiments with Metallic and Non-metallic Substances over Ether, Alcohol, &c. By H. B. Miller, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Bristol, May* 11, 1826.

Having observed in your number for April, a notice of some experiments by Mr. Murray, in which it had been discovered that gum arabic, heated to redness on a slip of platinum foil, became incandescent when held over ether in a state of spontaneous evaporation (similar to the well-known experiment of Sir H. Davy with a coil of platinum wire), I immediately instituted a series of experiments, of which the following is a detail, to ascertain whether the carbonaceous residue after the decomposition of the gum by heat alone produced this effect; and also to discover if the vapour of acetic acid is produced by the combination of the gases, as is the case when platinum continues in a state of ignition over alcohol, ether, &c. In the following experiments, ether, alcohol, essential oils, and mixtures of the oxygen and hydrogen gases, carburetted hydrogen and air, olefiant gas and air, produced similar effects, viz. the incandescence of the substance experimented on.

Exp. 1.—Charcoal from wood effected the combination of the gases very rapidly; it continued incandescent for a considerable time over ether.

Exp. 2.—Charcoal from the decomposition of bones.

Exp. 3.—————————————— gum arabic.

Exp. 4.—————————————— tragacanth.

Exp. 5.—————————————— myrrh.

These substances all yielded results similar to common charcoal.

Exp. 6.—Charcoal from the decomposition of indigo. The heat volatilizes the pure indigo; while the residue, after the carburetted hydrogen gases, &c. have been consumed, is simply carbonaceous matter.

Gums are compounds of

Oxygen,
Hydrogen,
Carbon.

By heat amounting to redness, they are decomposed; the oxygen and hydrogen gases, with a portion of tne carbon, suffer combustion; while the other part remains behind, and is precisely similar to charcoal; it frequently contains a notable quantity of siliceous matter, together with the other impurities of gums.

In the simple experiment of exposing a heated coil of platinum wire to the vapour arising from the evaporation of ether or alcohol, when the metal effects the combination of the oxygen and hydrogen gases from that substance so *rapidly*, that the whole of the coil continues incandescent, *water** seems to be produced; but as the temperature of the coil diminishes, the combination still ensues, though less rapidly, and a blue sulphureous-like flame appears waving round the wire. *Acetic acid* vapour is now formed in considerable quantity. It speedily converts the blue solution of litmus to a bright red, and by holding a wine-glass over the wire, it will condense, and speedily moisten the interior.

This blue flame and acetic acid are produced in the following experiments.

Ether, alcohol, volatile oils, were the fluids over which these substances were held, having been previously heated to redness in the flame of a spirit-lamp.

1. Charcoal from wood.	In carbonaceous experiments.
2. ———————— bones.	
3. ———————— indigo.	
4. ———————— gum tragacanth.	
5. ———————— myrrh.	
6. ———————— arabic.	

7. Platinum wire.
8. Platinum foil.
9. Palladium foil.
10. Gold wire.

* Although a solution of litmus is well known to be a most delicate test for acidity, yet in this experiment its blue colour remained unimpaired, and the liquid produced by the condensation of the vapours did not, as in the other instances, possess a sour taste, so that I consider the fluid produced during the *rapid* combination to be water, arising from the union of the hydrogen and oxygen gases of the ether and oxygen of the air.

11. Silver wire, a very beautiful experiment. The blue flame and acid vapour are very evident in this experiment.

12. Copper wire.

13. Copper plate.

14. Iron wire.

15. Steel wire.

16. Copper and steel wires united.

17. Brass wire.

18. Brass foil, a very pleasing experiment.

19. Watch spring.

20. Lead wire, a very beautiful cone of blue flame, plays for some time round the coils of this wire.

21. Dobereiner's pellets, containing spongy platinum, mixed with clay. These over ether and alcohol produce acetic acid; while by a jet of oxygen and hydrogen gases, water results.

22. Glass tube. This is most certainly a curious experiment. The tip of the glass rod held over the ether emits the blue flame from the whole of its surface; acetic acid formed in abundance.

23. Piece of porcelain.

24. Lime. This is a very beautiful experiment. The portion next the ether seems to continue incandescent; it emits a white light very similar in appearance to that produced by the phosphorescence of certain substances.

Acetic acid is a compound of

Carbon,
Oxygen,
Hydrogen.

Are those vapours of this acid (which condense in a cool glass receiver held over the wire) formed by the union of the carbon and hydrogen of the ether, with the oxygen of the air and ether? or is carbonic acid gas first formed, and then by its uniting with the hydrogen of the ether does it produce acetic acid?

Brass, iron, and copper wires, as well as glass, lime, &c. require to be heated to redness (in the dark), then transferred speedily over the ether or alcohol, the blue flame instantly appears, and will be visible for some time until the temperature is too low to decompose the vapour.

Lead wire must be heated until it is just ready to melt, and then quickly held over the ether.

The reason why brass, iron, copper, &c. do not effect the combination sufficiently rapidly so as to arrive at incandescence is, because the heat generated by the union of the gases is as quickly conducted away by the metal, so that the heat is not retained, as is the case in platinum, palladium, &c. where at that part which favours the union, it is rendered sensible by its incandescence to the eye-sight, but that copper, iron, &c. *effect* the combination of the two gases from the above-mentioned substances, is evident from the production of the acetic acid vapours and the blue flame.

Platinum stands to copper (in relation to its power of conducting heat) almost as 1 to 9.

As many experiments with platinum wire do not succeed unless the wire be sufficiently thick so that the heat may be retained, and a larger surface afforded for action, yet the success of the above experiments is not materially influenced by attending to this circumstance, the conducting power of copper, iron, &c. being increased with the bulk and surface of the metal.

One circumstance it may be worthy to notice, viz. that the ether or alcohol used in repeating the above experiments should be very pure, and frequently changed, a fresh portion being allotted for the trial of each, and the evaporating dish in which it is poured should have a moderate capacity; if it be too wide, the vapour of ether is not in sufficient quantity to produce the desired effect; or if very contracted, the atmosphere round the wire will solely consist of ether; the air not gaining admission, this may be remedied by raising the heated wire to that point where the two (the air and stratum of alcoholic or ethereal vapour) may be said to mingle.

Article IV.

On the Oxidation of Palladium during its effecting the Union of the Hydrogen and Oxygen Gases from Ether, Alcohol, &c. By H. B. Miller, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Bristol, May* 11, 1826.

In a paper which I had the honour to read at a meeting of the Literary and Philosophical Society annexed to the Bristol Institution, on the 26th of August, 1824, the Very Reverend the Dean of Bristol in the Chair, I stated an anomalous fact which had occurred in experimenting on a slip of palladium. Its inferior surface, after it had become incandescent either over the ether, alcohol, essential oils, or gaseous mixtures (a property which this metal possesses in common with platinum) became covered with a *black* substance. By uniting the slip of palladium with various metals, no alteration ensued, neither by insulation, so that electricity had no influence on this phenomenon. Many circumstances then induced me to consider it as carbon deposited from the various fluids and gases experimented on; but why it should rather adhere to the palladium than the platinum, I was then unable to ascertain, unless it was attributable to the former metal effecting the combination of the oxygen and hydrogen gases from the ether more rapidly than the platinum. Hence a superabundance of carbon remained which was precipitated on the inferior surface of the palladium.

This opinion may be seen by reference to Felix Farley's Bristol Journal for Aug. 28, 1824, in the report of the above Society's proceedings during that week.

But about the latter end of September, having repeated the experiment frequently in the interim, I found that the slip of palladium had sensibly decreased in size and weight, and having collected a small quantity of this black substance, and dissolved it in nitric acid, with the assistance of heat, a black residue remained, which floated on the acid solution. By its deflagrating with nitre, I considered it to be carbonaceous matter.

The nitric solution was of a reddish-brown colour, and it contained palladium; for it precipitated olive-brown with ferrocyanate of potash; orange with chloride of tin. Iron reduced it to the metallic state.

It was then very evident from all these facts that the palladium had combined with the oxygen of the air or ether, while it effected the combination of the two gases, and was converted into an oxide at a red heat—a circumstance which has not, I believe, been before noticed.

Its colour, when freed from the carbon it contained, seemed to be of a blackish-brown tinge.

As I did not possess any considerable quantity of palladium, it was impossible for me to ascertain the proportion of oxygen contained in this oxide.

Article V.

Addition to the List of Substances that cause a Coil of Platinum Wire to continue in a State of Incandescence (having previously been heated to Redness) when held over the Vapour, arising from their Evaporation. By H. B. Miller, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Bristol, May* 11, 1826.

The substances discovered by Sir H. Davy, and known to produce this effect, were, I believe, the following:

Camphor,	Sulphuric ether,
Alcohol,	Phosphorized ether,
Spirits of wine,	Nitrous ether.

By a numerous series of experiments which I made in 1824, and which I communicated to the members of the Bristol Institution at a meeting of their Philosophical Society, the following substances also produced a similar effect.

Solids.—Camphor,
Benzoic acid.

Fluids.—Oil of turpentine,
Oil of ether,
Spirits of camphor, as well as the following essential oils.

Essential oil of amber,
anniseed,
carraway,
cinnamon,
cloves,
juniper,
lavender,
origanum,
nutmeg,
savine,
rosemary,
peppermint.

Gaseous Mixtures. { Hydrogen / Oxygen, } { Hydrogen, / Air, } { Carburetted hydrogen, / Air, } { Olefiant gas, / Air.}

All substances, whether solid, liquid, or gaseous, containing oxygen, hydrogen, and carbon, in their composition (the two former are the only essential, though the latter is one of the constituents of most of the bodies experimented on) *provided they are volatile either at the temperature of the atmosphere, or by the application of heat,* not sufficient to decompose them, *seem to produce this effect.*

The caloric communicated to the platinum wire by heating it in the flame of a spirit-lamp is taken up in decomposing the portion of the vapour which arises from the evaporation of the fluid, or solid substance, experimented on. The decomposition of these substances (at least the vapour, which is identical in chemical composition with the body itself) being thus effected, oxygen and carburetted hydrogen gases are set at liberty;* these, by the medium of the metallic coil, whether it be palladium, or platinum wire, or foil, again unite, forming in some cases water; in others, acetic acid.

By this condensation of the gases into a fluid, sufficient heat is evolved, enabling the wire to decompose successive portions

* It may be necessary here to remark, that ethers, oils, alcohol, &c. are compounds of oxygen, hydrogen, and carbon, united in different proportions; they are all (camphor and benzoic acid included) decomposed at a red heat, affording the above-mentioned gases, and a residue of carbon.

of vapour, until the metallic coil becomes incandescent, inflaming ether, sulphur, essential oils, streams of hydrogen and carburetted hydrogen gases, camphor, and gunpowder.

These processes continue until the pure fluid is consumed by spontaneous evaporation, the impurities remaining.

In mixtures of oxygen and hydrogen, the gases are ready for combination, and the wire acquires a most intense heat, frequently detonating them while in the fluids and solids; their vapour is required to be decomposed by the heat of the coil, before the gases are present to favour the combination. This proves the accuracy of the above theory.

Article VI.

Reply to Mr. Phillips. By R. Christison, MD.

SIR, *Edinburgh College, April 25, 1826.*

I am sorry I have been so long in acknowledging your invitation conveyed in the *Annals of Philosophy* for last October; namely, that I would repeat your experiments on decolorizing arsenical fluids with animal charcoal, and candidly state the results. The delay has arisen chiefly from my wish, that you might see, at the same time with this answer, some further remarks I have published on the detection of arsenic, in the second volume of the Edinburgh Medico-Chirurgical Transactions.

The two first criticisms you have done me the honour of making on my objections have nothing whatever to do with your decolorizing process; and are intended, you say, as a criterion for trying the trustworthiness of my experiments.

If I ever have occasion to make public use of my paper on arsenic again, I shall be happy to avail myself of the correction contained in your first criticism. I must have had my head full at the time of Orfila and Smith, two of my standard authors; and thinking naturally of the books generally read by students of medical jurisprudence, rather than of all those actually in existence, I unfortunately wrote *most*, instead of *some* authors.

Do not be so unmerciful, however, as to conceive me ignorant, that oxide of arsenic is the better of [for] a little charcoal to reduce it. I find, indeed, that in the note which gave origin to this accusation, there is an ambiguity which might lead one, who had never read a chemical book before, to omit the charcoal. But you must have seen that when I said "the charcoal of the black flux is not necessary in the process," I meant the process *in the text*,—the process for reducing the *sulphuret;* and I distinctly remember that my reason for introducing the clause was, that, if I did not, some critic might accuse me of thinking charcoal necessary for that process.

I come now to the real subject of dispute between us. The sum of your third criticism is, that I misunderstood your directions, and instead of *mixing*, actually *boiled* my fluids with the ivory-black. My reasons for so doing were, that at Paris, my school of instruction in this matter, I was taught that coloured liquids required less charcoal for their decolorization at the boiling than at the mean temperature;—that I believed there were obvious advantages in using as little charcoal as possible;—and consequently that as some of your directions were given succinctly, and you used in one place the ambiguous term *digest*, I inferred you meant the charcoal to be applied according to use and wont. I was wrong it seems. But as you do not accuse me of wilful error, I cannot understand how it did not occur to you that you were wrong too,—in not giving us ignorant people more precise directions.

I confess, however, I was at a loss to imagine what difference this unhappy boiling of my fluids was to make. Animal charcoal possesses the power of absorbing saline as well as colouring matters from solutions; and cannot, I apprehend, remove the one without also removing the other. From your expression, that my error is fatal to my objection, it appears your opinion is the reverse, although it is not any where stated categorically. As I presume your opinion is founded on strict experiment, I shall not at present dispute the general rule. But assuredly it does not hold with all coloured fluids; for I find that strong tea, with cream and sugar, containing a grain of arsenic per ounce, parts with most of the arsenic before losing its colour.

I have not repeated your experiments, because, notwithstanding the example of distrust you set me, I am willing to believe them correct. There is one thing, however, I am doubtful about; and that is, the validity of the indication you obtained, in a decolorized solution of arsenic in port-wine, with the copper test. For the arsenite of copper is soluble in a small quantity of many vegetable acids, and indeed in most vegetable and animal infusions, even though they do not contain a free acid. At the same time a greenish precipitate is really thrown down sometimes; but it is not the arsenite of copper. I suspect your precipitate, therefore, was not the arsenite.

Nevertheless, although I believe your experiments correct, I cannot concede to you the importance claimed for your decolorizing process. And my objections are still the following:—

1. *It does not decolorize every fluid.*—This fact, with all submission, I hold to be a material objection. Suppose an analysis has been committed to an inexperienced person [and nine-tenths of medico-legal analyzers are inexperienced], who

does not know that his fluid cannot be decolorized. He betakes himself of course to your process, adds charcoal in portion after portion without effect, and at last becomes satisfied that the fluid will not part with its colour. But what has become of the arsenic when all is over? it is gone to be sure,—gone with the charcoal.

You say you were aware that some fluids cannot be decolorized; and I believe you were;—I never said your were not. But you add that you stated the fact in your paper. I am sorry I cannot find any statement of the kind,—any hint of it even. The readers of the *Annals* will not wonder at my imagining I saw directions to *boil* in the author's injunctions to *mix* and *digest*,—when he himself finds a statement of facts where there exists not even a shadow of them.

2. I repeat that *the arsenic is sometimes removed as well as the colour*, when the proportion is small, even although the charcoal is not *boiled* with the fluid. You say indeed this is not the case with port-wine; and I have said I am willing to believe you correct. But try strong tea with cream and sugar.

I mixed a solution containing one grain of arsenic with an ounce of such tea; and having ascertained pretty nearly the quantity of well-washed ivory-black required for decolorizing the mixture, I allowed it to act in the cold for two or three minutes. The filtered fluid had a pale straw-yellow opalescence. The silver test caused a cream-coloured precipitate; the copper test a bluish-green haze, but hardly any precipitate; lime-water a whitish haze, and very scanty precipitate; sulphuretted hydrogen [with the previous addition of acetic acid]; a copious lemon-yellow precipitate. Hence none of the tests gave a true indication, except the last; and as to that one, I find the colour is brighter without the previous decolorizing process. Here then is a fluid, to which your process is inapplicable, *although it does destroy the colour*. Porter, with the same proportion of arsenic, may be decolorized, so as to restore the correct action of the silver test; but lime-water and the copper test do not act at all, although the colour is quite destroyed.

3. This leads me to remark, thirdly, that your process *does not take away the power which many fluids possess of retaining the precipitates in solution*. The arsenites of lime and copper are soluble in many vegetable and animal fluids, particularly when they contain a free acid. But a free acid is not the only cause. The porter used above, which contained an obvious quantity of arsenic after decolorization, did not contain a free acid; neither did the tea. This is a material objection when we consider that, for satisfactory evidence on a trial, the concurrent indications of *many* fluid tests are, in the opinion of every medical jurist, absolutely necessary.

To these objections I may add, that they are doubly strong when the solution is considerably diluted. A solution, containing, like yours, a grain per ounce is stronger than will generally occur in the practice of medical jurisprudence. What would be the result with your process if, as in two cases which were lately submitted to me, and in which, as you will see by my paper in the Edinburgh Medico-Chirurgical Transactions, I detected the poison by my method, the proportion was twenty-five, or a hundred and seventy times less? Would not an attempt to destroy the colour inevitably remove the whole arsenic?

Another criticism you make on my objections to your process is, that I probably did not wash my ivory-black. Most assuredly I did not. First, because I ascertained that the muriate or muriates contained in my ivory-black were far too scanty to affect the colour of the silver precipitate even in a pure solution; and secondly, because, as you should know very well, the fluids I examined gave a copious white or coloured precipitate with the silver test, independently of the impurities in the charcoal. At your request, however, I have repeated the experiments with washed charcoal; and I find the washing does not make any material difference.

Before concluding, I must take the liberty of saying, that in common with other distinguished writers on this subject, you have fallen into the error of treating it too much as a chemist, and too little as a medical jurist. You must be aware that evidence which will satisfy the former will not satisfy the latter; —that the indications of one or two liquid tests would be satisfactory in a common chemical analysis; but that far greater precision is required when the analysis will decide a question of life and death. The analyzer must be able not only to satisfy his own mind, but likewise to satisfy the Court that his conclusions are legitimately drawn. My attention was first turned to the subject, because the complexity of the ordinary processes rendered the latter object unattainable; and my first view was rather to render the process simpler, than to make it delicate, because I perceived from what took place on several trials that judges and jurymen were inclined to distrust the chemical evidence altogether, seeing that it was surrounded by so many fallacies, and was too complex to be comprehended by their uninitiated understandings. My aim, therefore, was to substitute a process which might be so simple as to be easily applied by unpractised operators, and easily explained to unscientific persons in a court of law. Whether I have succeeded it becomes not me to judge. The method I propose [in which of course there is nothing new but the combinations and precautions] has at all events many advantages over your decolorizing process, and every other process now in use. 1. It is much simpler. 2. It is applicable to every possible case. 3. It is more delicate than some, and, looking to the object in view, the procuring *decided* evidence, it is more delicate than any, even than the method by the fluid tests. For the proof of what is now stated I must refer your readers to my paper just published. I may merely mention, that in two judicial cases to which I have lately applied it, I was able to detect a twentieth part of a grain in the contents and coats of the stomach; and that I am satisfied I could detect half that quantity.

In your criticism you have done me the injustice of condemning my process with obscure hints, and without even mentioning what the process is. I must, therefore, add a single sentence, that your readers may know what we are contending about, and be encouraged to look at what I have said elsewhere on the subject. My plan now consists in presenting the *same portion* of the poison *successively* in the state of the *sulphuret*, the *metal*, and the *oxide*:—the sulphuret in the state of an impure, sometimes shining precipitate; the metal in that of a brilliant crystalline crust; the oxide in that of adamantine, octahedral crystals: and the fluid tests are discarded altogether. The characters I mention may be all seen distinctly with a fortieth part of a grain; and I have even sometimes succeeded with a hundredth part only.

I hope I may request you will insert the foregoing observations in the *Annals;* and I have the honour to remain,

Your obedient servant,

To R. Phillips, Esq. &c. R. Christison.

*** In a future number of the *Annals,* I shall probably offer a few observations on the foregoing communication.—R. P.

Article VII.

Abstracts of Papers in the Philosophical Transactions for 1825, *on the peculiar Magnetic Effect induced in Iron, and on the Magnetism manifested in other Metals, &c. during the Act of Rotation.* By Messrs. Barlow, Christie, Babbage, and Herschel.

(*Continued from* vol. xi. p. 449.)

2. *On the Magnetism of Iron arising from its Rotation.* By S. H. Christie, Esq. MA. FRS. (With a Plate.)

For some time previously to his observation and investigation of the phænomena detailed in this communication, Mr. Christie had been engaged in making several series of experiments, with a view to discover the precise manner in which unmagnetized iron acts upon a magnetic needle. For this purpose, he had made use of an iron ball 13 inches in diameter, and also of a shell 18 inches in diameter, and observed their effects on the needle in various positions, as referred to certain planes passing through its centre. The shell and the needle were placed in the relative positions which he wished to give them, by determining a radius and an angle on a horizontal plane, and a vertical ordinate. The requisite computations becoming, from their number, laborious, Mr. Christie resolved to supersede the necessity of them, if possible, by the construction of an instrument, by which he could adjust the iron and the needle in their proper relative positions, without any previous computation. In this he succeeded, by means of the instrument he proceeds to describe in the following manner, but in which it became necessary to make use of a plate of iron instead of the heavy iron shell.

"The instrument is represented in Plate XL, fig. 1. The principal part consists of two strong limbs of brass: one, S Q N, a semicircle, 18 inches in diameter, 2·15 inches broad and ·3 inch thick: the other consist of two semicircles joined together; S Æ N, 1·2 inch broad and ·22 thick, and its outer diameter 18 inches; *s æ n* ·9 inch broad, ·22 thick, and its inner diameter 9·2 inches. S Æ N *n æ s* and S Q N are attached to each other by strong brass pins passing from S to *s* and N to *n*; so that *s* Æ *n* will revolve about the axis S *s n* N, while S Q N is fixed. S Æ N and S Q N are graduated from Æ and Q towards S and N, as is likewise *s æ n* from *æ* towards *s* and *n*. The semicircle S Q N passes freely through an opening in the support G I, but may be clamped firmly in any position by means of two strong screws, working into the parts G, G′, from the back of the instrument. On the chamfered edge of the opening *g*, in the face of G G′, is an index showing the inclination of the axis S N to the horizon; and on the part K *k* at the foot of the pillar, and attached to it, is an index pointing out on the graduated circle L *l*, fixed on the table T *t*, the situation of the fixed limb S Q N with respect to the magnetic meridian. R *r* is another graduated circle, fixed to the moveable limb S Æ N; which, by the index at *x* on the fixed limb S Q N, shows the angle described by S Æ N from the plane of S Q N. A very strong brass pin, soldered to the foot of the pillar, passes through the table T *t* and a thick circle of wood, to which the legs are attached, and has below a clamping screw, to fix the whole firmly together in any position. The compass box N′ S′ is fitted on to a stand fixed to the support F *f*, which consists of two parts; *f* fitted to G, and F sliding on a tube attached to *f*; so that the compass may be elevated or depressed. An arm A B, to carry the circular plate of iron C *c*, is connected with the moveable limb S *æ* N. The part A *a* consists of two flat pieces, having the limb of the instrument between them, so that the arm may be moved into any position on it, and be fixed in that situation by means of a strong screw working into the face A *a*. On the cylindrical par

Fig.2.

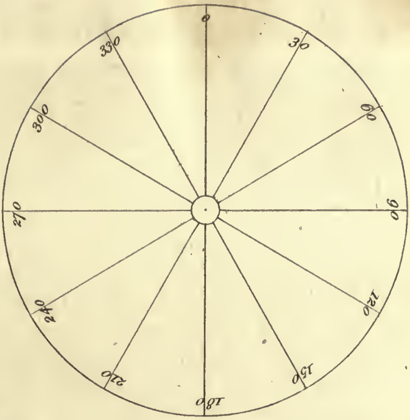
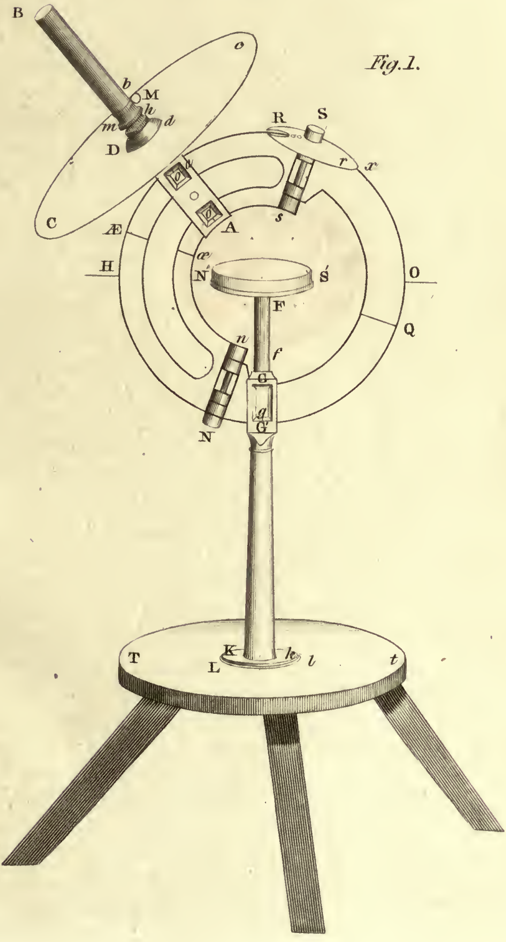


Fig.1.



J. Shury, sc.

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B *b*, a short hollow cylinder slides freely, having a circular rim raised ·6 inch from it, to support the iron plate C *c* at right angles to the axis of the cylinder. Over the plate of iron is a wooden washer D *d*, which is pressed on it by the screw *h* working on the short cylinder. The cylinder with the plate is fixed in any position on the arm by the clamp M *m*. In the part A *a* of the arm are two openings *o*, *o′*, on the chamfered edges of which are indexes in a line with the axis of the cylinder B *b*, so that when each points to the same arc on the semicircles S Æ N, *s æ n*, the axis of the cylinder B *b* is directed towards their centre, and every point in the edge of the plate is at the same distance from that centre. As the weight of the plate was a considerable strain on the instrument, a scale to contain a counter-weight was suspended from the ceiling of the room, and the line from it passed through a moveable pulley, attached to the arm B *b*, so that the weight might easily be adjusted to relieve nearly altogether the strain of the plate on the arm in any position. The arm was also occasionally supported, and kept steady in its position, by a sliding rod resting on the table T *t*. The compass consists of a circular box, containing a circle 6 inches in diameter, very accurately divided into degrees, and again into thirds of a degree; and a very light needle, having an agate in its centre, and its point of suspension only ·07 inch above the surface of the needle. The extremities of the needle are brought to very fine points; so that by a little practice, with the assistance of a convex lens, I could read off the deviations very correctly to two minutes, being the tenth of the divisions on the circle.......... In the experiments which I had previously made, and in those which I proposed making with this apparatus, I conceived a sphere to be described about the centre of the needle, referring the situation of the iron to a plane, in which, according to the hypothesis I had adopted, it should equally affect the north and south ends of the needle. The line in which the needle would place itself, if freely suspended by its centre of gravity, I considered as the magnetic axis; the points where this axis cuts the sphere, the poles, the upper being the south, and the lower the north pole; and the great circle at right angles to the axis, the equator, being the plane above mentioned. The position of the iron was thus determined by its latitude and longitude; the longitude being always measured from the eastern intersection of the equator with the horizon. The angle which the axis makes with the horizon I considered to be, according to the most accurate observations, very nearly 70° 30′."

"After making a very few sets of experiments with this instrument, I found that it was necessary to attend very particularly to the situation of certain points on the iron plate with respect to the limb, since, with one point coinciding with it, the deviation of the needle, when the centre of the plate was on the meridian, would be easterly, and with another point coinciding, westerly; whereas had the iron possessed no partial magnetism, which was the case I wished to investigate, there would have been no deviation when its centre was on the meridian. My first object was to find what points on the plate must coincide with the limb, in order that the plate, when its centre was on the meridian, should cause no deviation in the needle; and it was in my attempts to effect this, which at first sight appears sufficiently easy, that I discovered the leading feature in all the phænomena which I am about to describe.

" *General Description of the Phænomena arising from the Rotation of an Iron Plate.*

"In order to find the points which I have mentioned, I adjusted the instrument so that the plane of the fixed limb was exactly in the magnetic meridian, and then brought the other limb into the same plane: the centre of the plate was then on the magnetic meridian, and its plane perpendicular to that plane, as represented in fig. 1. I now made the plate revolve in its own plane about the axis B *b*, and noted very carefully its effect on the needle. In doing this I found that if I placed the plate on the arm, so that a certain point, *c* for instance, coincided with the plane of the limb, the deviation was different when the same point, by the revolution of the plate, coincided with the limb again. As it appeared by this that the revolution of the plate had an effect upon the needle, independent of the partial magnetism of particular points, I considered that if the plate were made to revolve the contrary way, the deviation ought to be on the opposite side, and this I found to be the case. I will illustrate this by the observations made when I first noticed the effect. The plate was divided at every 30° of its circumference (fig. 2) by lines drawn through the centre, and being placed on the arm, so that 0° coincided with the upper part of the limb, the north end of the needle pointed 10′ east; but when this point again coincided with the limb, by the upper edge of the plate revolving from *west* to *east*, the needle pointed 30′ east: making the plate revolve the contrary way, that is, its upper edge from *east* to *west*, when 0° coincided with the limb, the north end of the needle pointed 28′ west: so that there was a difference of 58′, when every point of the plate had the same position with respect to the needle, according as the plate was brought into that position by revolving from *west* to *east*, or from *east* to *west*. As this appeared extraordinary, I made repeated observations at the time to ascertain that the effect was independent of any accidental circumstances, and found that the results always accorded with the first, the difference caused by the rotation of the plate being however greater or less according to the position of the plate.

"Having fully satisfied myself that, in whatever manner the rotation of the plate might cause this difference, such was really the effect, I next endeavoured to ascertain the nature and degree of the difference, according to the different situations of the centre of the plate. For this purpose I made a great variety of experiments, of which I shall not however here give the details, as I afterwards repeated them in a more convenient manner, and with greater precision; but shall merely point out the nature of them in general, and the conclusions which I at the time drew from them. The instrument being adjusted, and the arm fixed so that the centre of the plate was in the position which I required, I made the plate revolve so that its upper edge moved from *west* to *east*, and noted the greatest and least deviation of the north end of the needle; I then made the corresponding observations when the plate revolved in the contrary direction; a mean of the differences between the two greatest and between the two least I considered as the effect produced on the needle by the rotation of the plate in opposite directions. Repeating these in a variety of positions, I found that when the centre of the plate was in the magnetic meridian, its plane being always a tangent to the sphere circumscribed about the centre of the needle, the deviation of the needle caused by the rotation of the plate in its plane was the greatest when the centre of the plate was in the equator, and that it decreased from there towards the poles, where it was nothing;* that when its centre was on the equator, this deviation was the greatest when the centre of the plate was on the meridian, or in longitude 90°, and decreased to nothing in the east and west points, or when the longitude of the plate was 0° or 180°; and that when the centre of the plate was in the secondary both to the equator and meridian, the rotation of the plate, whatever might be its latitude, caused no deviation of the needle. In these experiments, the plate which I made use of was a circular one 17·88 inches in diameter, and ·099 inch in thickness, weighing 112 oz. The further I had pursued this inquiry, the more I was disposed to attribute the effects I have mentioned to a general magnetic action, arising in a peculiar manner from the *rotation* of the iron; and my next experiments were with the view of ascertaining how far this idea was correct. As similar results might not be obtained with any other plate, I next made use of a plate 12·13 inches in diameter and ·075 inch in thickness, weighing 38·75 oz. and with it obtained results precisely of the same nature, though considerably less in quantity. Another objection which occurred to me

* I should here mention, that, from the nature of my instrument, I could not make observations at the *north* pole; but as the results, as far as I could observe, were of the same nature on this side of the equator as on the south side, I think I am warranted in concluding, that *at* the *north* pole the results would likewise be of the same nature as at the south pole."

was this—that the iron being evidently slightly polarized in particular points, the effect might be supposed to arise from an impulse given to the needle by the motion of these points in a particular direction, and that the directive power of the needle not immediately overcoming the slight friction on the pivot, a deviation might thus arise from the rotation of the plate. Had this, however, been the cause of the deviations, I should have expected that, when the centre of the plate was in the meridian, the greatest effect would be produced with the plate parallel to the horizon, and its centre vertical to that of the needle; but I had seen that the greatest deviation took place when the centre of the plate was in the equator, its plane being perpendicular to it; and the deviation arising from the *rotation*, when the plate was parallel to the horizon, was not a fifth of the deviation when the plate was perpendicular to that plane. Besides it was manifest that if this were the cause, any other impulse would have a similar effect. I therefore made the needle revolve first in one direction, and then in that opposite, by means of a small bar-magnet, and invariably found that it settled at the same point, in whichever direction the impulse was first given, and the results obtained by the rotation of the plate were in these cases of the same nature as before. It was also evident, that if the deviations I have mentioned arose from this circumstance, the needle being agitated after any particular point of the plate was brought to the limb of the instrument, it ought to settle in the same direction, whether that point were brought into this position by revolving from *east* to *west* or from *west* to *east;* but this, except in the cases I have mentioned, where the rotation produced no deviation, was not found to take place. In order wholly to obviate this objection, in all my future experiments, after any point had been brought to the limb of the instrument, I agitated the needle, and let it settle before I noted the deviation.

"*Description of particular Experiments.*

"As I had found in my first experiments that I could obtain the nature of the deviation caused by the *rotation* by noting the greatest and least deviations when the plate was made to revolve in contrary directions, but that the quantity of that deviation could not by this means be determined with any degree of precision, I resolved to make my future observations differently. The method I adopted, when the change in the deviation from one point of the plate to another was considerable, was this: the plate being placed in any required position, I made it revolve once, for example, the upper edge from *east* to *west*, without noting the deviations, bringing the point marked 0° to coincide with the line indicating the position for observation; from hence I continued the revolution of the plate until the point marked

30° coincided with the same line, and, after slightly agitating the needle, noted the deviation; and in the same manner were the points 60°, 90°, 120°, 150°, 180°, 210°, 240°, 270°, 300°, 330°, 360°, or 0°, brought successively to coincide, and the deviations noted. I now made the plate revolve once from *west* to *east*, without noting the deviations, bringing 0° or 360° to coincide with the same line, and then brought in succession 330°, 300°, 270°, 240°, 210°, 180°, 150°, 120°, 90°, 60°, 30°, 0° to coincide, noting the deviations as before. The sum of the first set divided by 12, I considered as the mean deviation, when the plate revolved from *east* to *west;* and the sum of the others divided by 12, as the mean deviation, when the plate revolved from *west* to *east:* their difference was the mean effect of the *rotation* in contrary directions. This I call the *Deviation due to Rotation;* and to distinguish it from the deviation caused simply by the position of the iron, I call this last the *Absolute Deviation.* When the change in the deviation from one point of the plate to another was not so considerable, I made the observations only for the points 0°, 90°, 180°, 270° on the plate.

"I now proceed to the detail of the experiments, and the conclusions I draw from them. In those which I shall first describe, the centre of the plate was always in the magnetic meridian; its plane was perpendicular to the meridian, and a tangent to the sphere, whose centre was the centre of the needle; and the plate revolved, as in all other cases, in its own plane: they are a repetition of those by which I first discovered several of the facts I have mentioned, but made for the purpose of determining more precisely the deviation caused by the rotation. In making these, the instrument was adjusted so that the index at *g*, fig. 1, pointed to 0°, that at K to 90°, and those at 0, 0′ to zero."

Mr. Christie now gives a table of observations, the results of which he states as follows:

"From these observations it appears, that when the centre of the plate was in the pole of the magnetic sphere, its plane being parallel to the equator, the position of the needle, for any situation of the several points of the plate, was the same whether they were brought into that situation by the plate revolving from *east* through *south* to *west*, or from *west* through *south* to *east;* that is, that the *deviation due to rotation* was nothing:

"That the *deviation due to rotation* increased from this point towards the equator, where it was the greatest:

"And that the horizontal needle was affected by the rotation of the plate, not according to the situation of the centre of the plate as regarded the poles and equator of the horizontal needle, but as regarded the poles and equator of an imaginary dipping needle passing through the centre of the horizontal needle.

"This last is not so evident, from the circumstance of the deviation being nothing when the centre of the plate was in the pole of the dipping needle, and a maximum when in the equator, as from its being very nearly equal at equal distances on each side of the pole, and also of the equator; that is, at very unequal distances from the axis of the horizontal needle; and from the deviations at equal distances from the axis of the horizontal needle being very unequal. For if we compare the *deviation due to rotation* in lat. 70° 30′ S, long. 90°, with that in lat. 70° 30′ S, long. 270°, the difference is only 1′; in the first case, the centre of the plate was at the distance of 90° from the axis of the horizontal needle, and its plane parallel to it; and in the other at the distance of 51°, and its plane making an angle of 39° with this axis. Again, in the four corresponding situations of lat. 19° 30′, the mean *deviation due to rotation* is 1° 32′, and none of the deviations differ from this by more than 5′, although in two cases the centre of the plate was in the axis of the horizontal needle, and its plane perpendicular to it, and in the two others the centre of the plate was at the distance of 39° from this axis, and its plane made an angle of 51° with it. The mean of the *deviations due to rotation* in the three* corresponding situations of lat. 45° is 49′, from which none of the deviations differ by 3′, notwithstanding the difference in the situations of the centre and plane of the plate, in these cases, with respect to the axis of the horizontal needle. In long. 90° lat. 45° S, the centre of the plate was 64° 30′ above the horizontal axis, and its plane made an angle of 25° 30′ with it; in long. 90° lat. 45° N, it made an angle of 64° 30′ at 25° 30′ below it; and in long. 270° lat. 45° S, it was in a position above it similar to the last. Any doubt, however, on the subject will be removed, if we compare the deviation in long. 90° lat. 39° N with that in long. 270° lat. 0; the one deviation being nearly double of the other, although the centre of the plate was at the distance of 19° 30′ from the axis of the horizontal needle, and its plane made an angle of 70° 30′ with it in both cases. The difference is even more striking, if we compare the deviation in lat. 70° 30′ S, long. 270°, with that in lat. 31° 30′ S, long. 90°, the centre of the plate being in each case at the distance of 51° from the axis of the horizontal needle, and its plane making an angle of 39° with it. The differences which we have noticed in the deviations observed at the same distance from the equator, is not more than I have found to arise from a slight change in the adjustment of the centre of the needle to the centre of the instrument, the plate remaining in the same position. These errors of adjustment I found it almost impossible to avoid, owing probably in a great

"* The nature of the instrument would not admit of observations being made so near to the north pole in long. 270° as lat. 45°, or so near as lat. 70° 30′ on the other side of the support G I."

measure to the magnetic centre of the needle not being in the centre of suspension; and it was to counteract their effects, that I generally made observations on contrary sides of the centre.

"With respect to the direction in which the *deviation due to rotation* took place, it appears, that the rotation of the plate always caused the *north* end of the needle to move in the same direction as the edge of the plate nearest the *south* pole of the magnetic sphere; so that the deviation of the *north* end of the needle was in the direction in which the *south* edge of the plate moved, and that of the *south* end of the needle in the direction in which the *north* edge moved, referring the edges to the poles of the sphere.

"Having ascertained, that when the centre of the plate was in the pole, and its plane *parallel to* the equator, the *deviation due to rotation* was nothing; and some of the first experiments which I had made having indicated that this was also the case when the centre of the plate was in the secondary to the equator and meridian, and its plane, as before, a tangent to the sphere, I wished to ascertain whether such were really the fact."

The experiments made accordingly, the results of which are given in another table, left no doubt in Mr. C.'s mind on the subject, and from them, combined with the preceding, "we may infer, that if the centre of the plate were made to describe any parallel of latitude, the *deviation due to rotation* would be nothing when the longitude was 0° or 180°, and a maximum when the longitude was 90° or 270°, which is precisely the reverse of the *absolute deviations* that would be produced by the plate describing the parallel of latitude.

"The next experiments which I made were with the view of determining whether the *rotation* of the plate would produce any deviation, when its plane *coincided with the equator*. For this purpose an axis was fixed perpendicularly on the arm of the instrument in such a manner, that when the plate revolved on it, its plane was parallel to the limb.

"In order to make these observations, it was necessary to adjust the whole instrument twice; since the deviations for the longitudes 90° and 270° could not be observed with the same adjustment as those for the longitudes 0° and 180°. For the longitudes 90° and 270°, the axis of the instrument was horizontal, and pointed east and west, and the moveable limb revolved on the axis until its plane, and therefore also that of the iron plate, made an angle of 90° 30′ with the horizon, rising towards the north; so that the compass being elevated until the centre of the needle was in the plane of the plate, the plate was then in the equator. For the other longitudes, the axis of the instrument was inclined to the horizon at an angle of 19° 30′, and in the plane of the meridian; and the moveable limb adjusted at right angles to the fixed one: the compass was then elevated to coincide with the plane of the plate.

"In these experiments the distance of the centre of the iron from the centre of the needle was 13·2 inches; but as its edge was only 4·26 inches distant, the differences between the deviations corresponding to the several points on the plate were greatly increased; and therefore to obviate any inaccuracies that might arise, from the points not being brought into precisely the same situation when the plate revolved in the opposite directions, I increased the number of observations, making twenty-four for each position, namely, twelve points on the plate, as I have before described, the deviation for any point being observed when that point coincided with the line joining the centre of the plate and needle."

The observations on this subject, which are given in a third table, "show very clearly, that when the centre of the plate is in the equator, and its plane also coincides with the plane of the equator, the *deviation due to rotation* is always nothing, since the small differences to be observed here in the revolutions in opposite directions are only such as may justly be attributed to slight errors in the adjustments of the centre of the needle or of the plane of the plate, which are almost unavoidable. With regard to the several deviations in the different columns, I should notice that they are not those actually observed, but derived from them by subtracting the same number from all the deviations observed in two corresponding columns, so that they indicate the same difference of deviations in the two revolutions as those actually observed, and therefore give the same *deviation due to rotation*. The necessity of this reduction arose from the circumstance of my having to adjust the compass to the proper height, so that its centre might be in the plane of the plate, while it was under the influence of the partial magnetism of particular points in the plate; and having done this, when zero of the compass was brought to coincide with the point of the needle it was not necessarily in the magnetic meridian, since the needle was under the influence of this partial magnetism; and as I wished the deviations to be those from the meridian, I reduced the observed deviations as I have mentioned.

"Being convinced that the *rotation* of the plate *in* the plane of the equator caused no deviation of the needle, I proceeded to determine the effects produced by its rotation in other planes. In the first set of observations which I made, the centre of the plate was in the meridian, and its plane perpendicular to the plane of the meridian and passing through the centre of the needle. Before however making these, to avoid the necessity of moving the compass as in the last, I made a slight alteration in the instrument. Instead of having the axis on which the plate revolved perpendicular to the arm, and the plate consequently parallel to the limb, this axis was inclined in such a manner that the plane of the plate passed through the axis of the instrument; so that the axis of the instrument being horizontal, and passing through the centre of the needle perpendicularly to the meridian, when the arm of the instrument was adjusted to zero on the limb, the revolution of the limb caused the centre of the plate to describe the magnetic meridian, and at the same time the plane of the plate always passed through the centre of the needle. The distance between the centre of the plate and that of the needle was, as in the last, 13·2 inches."

"From these observations I find, directly contrary to what took place when the plane of the plate was a tangent to the sphere, that the *deviation due to rotation* increases from the equator to the pole, where it is a maximum. In this case, however, as in the other, the deviations are very nearly equal at equal distances on each side of the equator; so that, as before, it appears that the horizontal needle was affected by the rotation of the plate, not according to the situation of the centre of the plate with respect to the poles and equator of the horizontal needle, but with respect to the poles and equator of an imaginary dipping needle passing through the centre of the horizontal needle.

"With regard to the direction of the *deviation due to rotation*, it appears, that when the centre of the plate had *north latitude*, the *north end* of the needle deviated *in the direction* of the motion of the plate's *inner edge;* and when it had *south latitude*, the *north end* deviated in a *contrary direction* to that of the *inner edge* of the plate, and therefore the *south end* deviated *in the direction* of the *inner edge:* so that *the end of the needle of the same name as the latitude, always deviated in the direction of the motion of the plate's inner edge.*

"Let us compare this with the inference we have drawn from the observations, viz. that when the centre of the plate is in the meridian, and its plane a tangent to the sphere, the north end of the needle, by the rotation of the plate, deviates in the direction of the motion of the south edge, and the south end in the direction of the north edge of the plate; that is, either end of the needle deviates in a direction contrary to that of the motion of the edge of the plate nearest to the pole of the sphere of the same name as that end. Now, if from the position which the plate had in the last experiments, namely, its plane passing through the centre of the needle, it be conceived to revolve about its diameter, which is perpendicular to the plane of the meridian, until its plane be a tangent to the sphere, the direction of the revolution about this diameter being of the inner edge towards the pole of the same name as the latitude of the plate's centre, the inner edge will become the edge of the same name as the end of the needle, which, in its first position, according to our inference from the last observations, deviated in the direction of its rotation; but according to the inference drawn from the first series of observations, the end of the needle of the same name as this edge, will, in the new position, deviate in a direction contrary to that of its rotation; so that the rotation of the plate being in the same direction in both positions, the deviations by rotation will be in contrary directions in the two cases: and consequently, between the two positions, the plane of the plate must have passed through one in which the rotation would produce no deviation. If we conceive the plate to come into the position of the tangent-plane by revolving about its diameter in the opposite direction, that is, by the inner edge moving towards the pole of a contrary name to the latitude, the inner edge will become the edge of the contrary name to the end of the needle, which in the first position deviated in the direction of its rotation; and therefore that end of the needle will still continue to deviate in the same direction; that is, the direction of the rotation being the same in the two positions, the deviation by rotation will be in the same direction in both cases; and consequently between the two positions, either there is no position of the plane of the plate in which the rotation will produce no deviation, or there are two, or some even number of such positions.

"I have not been able to determine in all cases experimentally, the situation of the plane in which the *deviation due to rotation* vanishes, or whether there may be more than one plane in which this takes place; but all the observations which I have made confirm me in the opinion which I formed on comparing the preceding results, that when the centre of the plate is in the meridian, there is only one plane between the tangent-plane and the plane passing through the centre of the needle in which the deviation due to rotation vanishes, and that that plane is parallel to the equator.

"Another conclusion which we may draw from these experiments compared with those above referred to, is this, that when the centre of the plate is in the meridian, and its plane perpendicular both to the meridian and equator, then, supposing the plate always to revolve in the same direction, the deviation will always be in one direction, in whatever point of the meridian the centre of the plate may be.

"As I had already found, that when the centre of the plate was in the secondary to the equator and meridian, and its plane a tangent to the sphere, the rotation caused no deviation of the horizontal needle: it appeared to me that there ought to be no *deviation due to rotation* when the plane of the plate was in any other plane perpendicular to this secondary. To ascertain how far my views were correct, or otherwise, I adjusted the plate on the arm, the same as in the last experiments, and the instrument so that the axis Æ Q being in the plane of the meridian and inclined to the horizon at an angle of 19° 30′, the centre and plane of the plate were, during the revolution of the limb, always in the position I required. The distance between the centres of the needle and plate was, as before, 13·2 inches."

"Although the *deviations due to rotation* in these observations are in some cases greater than might perhaps on a first view be expected, if in the position in which I have supposed the plate, its rotation would really produce no deviation, yet the differences are not in any case more than may, I consider, be fairly attributed to errors in the adjustments. That the deviations, when the plate revolved from south to north, had a tendency most generally to be greater than when it revolved in a contrary direction, as is evident by referring to the Table, appears at first sight more unfavourable to my opinion than the magnitude of the difference; but on further consideration, I think that this will be allowed rather to point out the source of the errors in the results, than the incorrectness of my views, and that these errors arose from the plane of the plate not being in those cases perpendicular to the plane of the secondary to the equator and meridian. The proximity of the edge of the iron to the ends of the needle, varying from 5·16 inches to 4·27 inches at the south end, and from 5·16 inches to 5·92 inches at the north end, I considered to be another source of error; the inequalities arising from the effects of particular points near the edges of the iron on the ends of the needle being the more sensible when the distances are small. All my observations were made as near to the centre of the needle as the instrument would admit, in order that the effects of the rotation, since they were in many cases extremely small, might be the more sensible; and by this means I discovered the nature of the effects produced on the needle by the rotation of the plate; but I am fully convinced, that for the purpose of comparing the results of observation with the conclusions from theory, it is always desirable, that the observations should be made when the iron is at such a distance from the centre of the needle, that the effects of particular points near its edges, on the ends of the needle, are nearly insensible. Taking these circumstances into consideration, I was quite satisfied from these experiments, that, if the centre of the plate be in the secondary to the equator and meridian, and its plane perpendicular to the plane of that circle, the rotation of the plate will produce no effect on the absolute deviations caused by the mass.

"In order to determine what effects would be produced by the rotation of the plate when its centre was in the secondary to the equator and meridian, and its plane in the plane of this circle, the instrument was adjusted as in fig. 1, the index at *g* pointing to 70° 30′; the limb S Æ N was then placed at right angles to

S Q N, and the arm A B attached to it with the iron plate on the axis; and that the centre of the needle might be in the plane of the plate, the compass box was moved in the direction of the meridian.

"Some of my first observations were made with the centre of the plate in the equator, and I immediately found, that the *deviation due to rotation,* instead of being 0, as in the cases when the plate revolved in the planes at right angles to its present position, was here considerable; and also that, that of the south end of the needle was in the direction of the upper, or south edge of the plate, contrary to what had been observed in the same plane at the pole. This indicated that there must be, at least, one point in this circle on each side of the pole, where the *deviation due to rotation* was 0; and to determine nearly the latitude of this point, I made observations at every 10° of latitude on each side of the south pole. Before, however, giving these observations, it is necessary that I should state the kind of reliance I place on them as forming a complete set. In order to make the observations near the pole, it was necessary to adjust the instrument with the axis horizontal and pointing east and west, and after having made the complete set, I suspected that in the change from the one adjustment to the other, the centre of the plate had been nearer to that of the needle in making the observations near the equator, than those near the pole; and that consequently, the *deviations due to rotation* in the former case, were proportionally too great. I was confirmed in this suspicion on comparing these observations with those which I had, in the first instance, made in lat. 0° and in lat. 90°; and still further on comparing them with others, which I subsequently made at the several distances 15, 17, 19, 20 inches; in the corresponding situations. For example, in my first observations, the *deviations due to rotation* in lat. 0°, long. 0°, and in lat. 0° long. 180° were 3° 10′, and 3° 14′, giving a mean 3° 12′ in lat. 0; and in lat. 90 S, 1° 31′; when the centres of the plate and needle had been carefully adjusted to the same distance 13·2 inches, in the two cases; whereas the corresponding deviations in the table are 5° 43′ and 1° 29½′; and, by subsequent observations, I found the sum of the deviations at the distances 15, 17, 19, and 20 inches to be in these two cases, 7° 20′ and 3° 32′, to which 3° 12′ and 1° 31′ are very nearly proportional. These differences however do not in the least affect the conclusions which I at the time drew from this set of observations."

"It appears, from these observations, that when the plate revolves in the plane of a secondary to the equator and meridian,

"1st. The deviation due to rotation is a maximum when the centre of the plate is in the equator.

"2d. It decreases as the plate approaches the pole, and is 0 between the latitudes 50° and 60°, apparently very nearly at 55°; and from this point it increases till it attains a maximum in a contrary direction at the pole.

"3d. At the south pole and on each side down to the latitude 55°, the deviation of the *south* end of the needle due to rotation is in the direction of the *north* or *lower* edge of the plate; or, from the *south pole* down to the latitude 55°, the *south* end of the needle moves *towards* the plate, when the *inner* edge of the plate moves *from* the *south* pole, and *from* the plate when the *inner* edge moves towards the *south* pole.

"4th. From the equator towards either pole as far nearly as the latitude 55°, the *south* end of the needle moves in the direction of the *south* edge of the plate; that is, it moves *towards* the plate when the *inner* edge of the plate moves *towards* the *south* pole, and *from* the plate, when that edge moves *from* the *south* pole; also the *north* end of the needle moves *towards* the plate, when the *inner* edge moves *towards* the *north* pole, and *from* the plate, when that edge moves *from* the *north* pole. Consequently towards whichever pole the *inner* edge moves, the corresponding end of the needle will move *towards* the plate from the equator to the latitude of 55° nearly, and the contrary will take place from the latitude 55° to the pole.

"The observations which I made with the plate on the north side of the equator, though not so multiplied as those on the south, were sufficient to show, that the deviations due to rotation observed the same laws on that side of the equator as I had noticed on the south side.

"The *deviation due to the rotation of the plate*, when its centre is in the secondary to the equator and meridian, having a peculiar character, namely, two greater maxima when the centre is in the equator, two less maxima, in a contrary direction, when the centre is in either pole, and four points where it vanishes, I consider to be particularly well adapted for forming an estimate of the correctness of any theory which may be adopted for the explanation of the phænomena in general; since the theory must be perfectly compatible with these peculiarities, before it can be applied to the explanation of the less marked phænomena.

"As it appeared from these observations, that the point where the deviation due to rotation vanishes, is not far from lat. 55°, the complement of which, 35°, is nearly half the angle of the dip, I wished to ascertain whether the deviation were really 0 in latitude 54° 45′, which I considered to be correctly the complement of half the dip 70° 30′, although I could not see how the angle which the plane makes with the horizon could have an influence on an angle in the plane itself. Subsequent observations showed, that in this instance the deviation due to rotation vanishes, or nearly so, when the polar distance of the centre of the plate is equal to half the angle which the dipping needle makes with the horizon. Whether this coincidence is purely accidental, or is a necessary consequence of the manner in which the effect is produced, must remain doubtful, until it can be shown how the action takes place; it, however, led me to ascertain precisely the point at which the deviation due to rotation vanishes."

"*General Law of the Deviation due to Rotation deduced from the Experiments.*

"Having now ascertained the nature of the effects produced on the horizontal needle by the rotation of the plate in different planes, I endeavoured to discover some general law, according to which the direction of the deviation depended on the direction of the rotation of the plate; so that the situation of the centre of the plate, the plane in which it revolved, and the direction of rotation being given, we might point out immediately the direction in which the deviation would take place.

"On comparing together all the facts which I have detailed, I found that this might be effected in the following manner. I refer the deviations of the horizontal needle to the deviations of magnetic particles in the direction of the dip, or to those of a dipping needle passing through its centre; so that, in whatever direction this imaginary dipping needle would deviate by the action of the iron, the horizontal needle would deviate in such a manner as to be in the same vertical plane with it: thus, when the north end of the horizontal needle deviates towards the west, and consequently the south end towards the east, I consider that it has obeyed the deviation of the axis of the imaginary dipping needle, whose northern extremity has deviated towards the west and its southern towards the east; so that the western side of the equator of this dipping needle has deviated towards the south pole of the sphere, and its eastern side towards the north pole. It would follow from this, that if the north and south sides of the equator of the dipping needle (referring to these points in the horizon) deviated towards the poles, no corresponding deviation would be observed in the horizontal needle; the effect, in this case, taking place in the meridian, would only be observable in the angle which the dipping needle made with the horizon. As it is not my intention at present to advance any hypothesis on the subject, I wish this to be considered only as a method of connecting all the phænomena under one general view. Assuming it then for this purpose, it will be found that the *deviations of the horizontal needle due to rotation* are always such as would be produced *by the sides of the equator of this imaginary dipping needle deviating in directions contrary to the directions in which the edges of the plate move,* that edge of the plate nearest to either edge of the equator producing the greatest effect on it. By referring to the particular laws which I deduced at the time of making the experiments in different planes, it will be seen that they are all comprised under this general law; but this will be rendered more evident by taking an instance.

"When the centre of the plate is in the meridian, and its plane a tangent to the sphere, the eastern side of the equator of the imaginary dipping needle, according to the above law, will deviate in a direction contrary to that of the motion of the eastern edge of the plate, and consequently the northern extremity of the axis will deviate in a contrary direction to that of the motion of the plate's northern edge, or it will deviate in the direction in which the southern edge of the plate moves. Hence the horizontal needle obeying the deviations of this dipping needle, the deviations of its north end due to the rotation of the plate will be in the direction in which the south edge of the plate moves, which is the law deduced from the experiments first detailed." E. W. B.

(To be continued.)

Article VIII.

Analysis of Acorns.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

I beg to inclose the notes made of some experiments to ascertain the component parts of the acorn. Although the analysis is not complete, and I have since found no leisure to supply what is wanting in it; yet it seems to contain some facts on the subject that I am not aware can elsewhere be found; and for that reason you may, perhaps, think the inclosed paper entitled to a place in your pages. Yours, &c. W. B.

[We could wish that the experiments of our correspondent had been more complete, as we are not fond of admitting fragments into the *Annals of Philosophy*. We publish his communication, however, in the hope that he will pursue the subject to a satisfactory conclusion.—*Ed.*]

Exper. 1.—350 grains of acorns were triturated in a marble mortar with water (about $2\frac{1}{2}$ pints), and passed through a fine hair sieve; what remained on the sieve, pressed and dried at a heat of about 130° for three hours, was a light-brown matter (B) weighing 63 grains.

(A.) What passed through the sieve deposited a white sediment, which, after standing twelve hours, was separated by pouring off the supernatant liquid; and after being dried five hours at about 100°, weighed 71 grs. It was digested in dilute nitric acid, and was dissolved in two days; from this it was precipitated by alcohol.

(B.) The 63 grains of dry matter were macerated in cold water two days, and the water (C) being poured off, was dried, and alcohol of commerce added to it, but no part was dissolved, although the colour of it became deeper, and it obtained a more friable and harsh texture. The alcohol being poured off, nitric acid was added, and a partial solution took place; for at the end of twenty-six days, I found one-third of it dissolved. The solution was decanted off, and evaporated to dryness; the residue was very soluble in water, and of a wax-yellow. With this solution, muriate of lime gave no precipitate; muriate of tin, nitrate of silver, acetate of lead, gave each a copious white precipitate.

(C.) This water did not seem to have taken up any part of the 63 grs.

(D.) The water that passed through the hair sieve was evaporated to two ounces, and gluten coagulated in flakes, which, separated by the filter and dried, weighed 25 grs. Isinglass then precipitated about 10 grs. of tannin, the remaining part was owing to an accident unexamined.

The composition, therefore, appears to be,

	Grains.
Starch	71
Insoluble matter	63
Gluten	25
Tannin	10
Extract, &c. and loss	181
	350

Exper. 2. *Analysis of the Ashes of Burnt Acorns.*—The shells, or external coats (not the cups, or calyces), were taken off.

	Grains.
The shelled acorns weighed	1500
The shells, or coats	360
Total	1860

(A. 1.) The shelled acorns were placed in a brown earthenware cup, uncovered at the top, and continued at a red heat for five hours, until burnt to ashes, which weighed, while hot, 20 grs. Upon these ashes, three ounces of water were poured, and after sometime filtered, the insoluble matter on the filter (A. 2.) being dried, weighed five grains. The solution (A. 1.) turned turmeric paper brown.

This solution was evaporated to dryness, gave a brown deliquescent mass, to which alcohol was added. The alcoholic solution evaporated to dryness weighed 1·1 gr. and appeared by dissolving in water, and adding nitrate of silver, to be composed of ·4 muriate of magnesia and ·7 potash. The portion insoluble in alcohol was saturated with dilute nitric acid with effervescence, and evaporated to dryness. It weighed 20 grains, and burnt like nitre; 200 grs. of water were added, which dissolved the whole, except ·5 gr. which was sulphate of lime. This solution gave no precipitate with nitrate of silver, or oxalic acid; carbonate of potash showed a very slight cloud. It appeared to be nitrate of potash = 19·5 grs. which contain 9·17 of potash.

Consequently the soluble part (A. 1.) contains

	Grains.
Carbonate of potash	8·8
Potash	5·3
Sulphate of lime	0·5
Muriate of magnesia	0·4
	15·0

(A. 2.) The insoluble part being digested in dilute muriatic acid was dissolved, except ·5 gr. silex, with a trace of alumine. From the muriatic solution was precipitated the iron by means of prussiate of potash, which gave a precipitate of prussiate of iron of ·2 gr. = ·1 gr. carbonate of iron; after adding a drop or two of muriatic acid to the filtered solution, carbonate of potash was added in excess, which precipitated 6 grains of carbonate of lime = 3·4 lime, and on boiling gave a further precipitate of ·5 gr. of carbonate of magnesia.

Hence the part insoluble in water consists of

	Grains.
Silex	0·5
Iron	0·1
Lime	3·4
Magnesia	0·5
Loss	0·5
Alumine	Trace
	5·0

The 360 grains of acorn shells being exposed to a red heat for two hours, were reduced to ashes, which weighed 4·5 grs.

(B.) The ashes were digested in three drams of water and filtered, the insoluble part weighed 2·75 grs. (B. 2.)

(B. 1.) To the solution was added nitrate of silver as long as any precipitate fell. The precipitate filtered and dried weighed 2·25 grs. Dilute nitric acid dissolved 1·25 gr. of it, leaving 1 gr. muriate of silver, the other part being the carbonate. The remaining solution evaporated to dryness, left a moist residuum of 2·25 grs. carbonate of potash, with a trace of magnesia. 1 muriate of silver = ·18 muriatic acid = ·3 muriate of magnesia. 1·25 carbonate of silver = ·2 carbonic acid = ·63 carbonate of potash.

Hence we have the soluble part composed of

Carbonate of potash.................	·63
Potash................................	·82
Muriate of magnesia..................	·30
	1·75

(B. 2.) The insoluble matter was dissolved in dilute muriatic acid. The residuum weighed ·125. Into the solution was poured prussiate of potash, which gave a precipitate of about ·05 gr.; carbonate of potash was then added in excess, which gave a precipitate of 3·85 grs. and after boiling a further precipitate of ·6 gr.

Hence the composition of the insoluble matter is,

Silex..................................	·125
Iron..................................	·025
Lime..................................	2·0
Magnesia..............................	·6
	2·75

Exper. 3. *Analysis of Acorns.*—5000 grains of peeled acorns were pounded in a mortar tolerably fine, and in this state they stood two or three days. The colour of the mass darkened, and became a deep-brown. They were triturated repeatedly in a marble mortar with water, and thrown on a seive, upon which they were well washed with water until it came off colourless: 13 pints of water (E) were employed. It was of a pale leather-brown, and deposited starch (F). It did not change the colour of turmeric or litmus papers, or of turmeric reddened with an alkali. The insoluble matter (G) still remained of a chocolate colour, and had a chaffy appearance; three pints of cold water (H) were added to it, and put in the oven all night at a heat of about 180°. The water had acquired a deep-brown colour owing to the extract it had dissolved. It was poured off in the morning, having stood about 10 hours. The matter (G) had then a pale leather colour, which soon became darker on exposure to the air; two pints more water (I) were then added to it, and it was again placed in the oven for four hours. This water having dissolved another portion of the extract was poured off, and two pints more (J) added, which, having stood in the oven several hours, was, in like manner, poured off, and two more pints added, heated, and poured off. These several portions of water were then poured together.

The insoluble matter (G) was then dried at a moderate heat, and weighed 872 grs.

A few grains of this insoluble matter (G) being put into a solution of carbonate of potash, which, after digesting awhile at a moderate heat, dissolved nearly the one-half of it.

The solution of extract (H, I, J,) was tested with the following reagents.

Nitrate of silver, a copious deep-reddish brown precipitate, redissolved in dilute nitric acid, leaving a residue.

Oxalic acid, no change.

This solution being evaporated to dryness left a deep-brown matter (K), weighing 180 grs. being extractive. 50 grs. of it were put into about an ounce of alcohol of commerce to digest at a heat of about 45° or 50° for seven days, when it had lost 9 grs. of its weight, which were taken up by the alcohol. The alcohol was evaporated, and the residuum, a dark-brown matter, appeared to be a part of (K) altered; for when redissolved in water, it gave with

Nitrate of silver, a precipitate in white flakes.

Carbonate of potash, a brown precipitate.

Sulphuric, nitric, and muriatic acid, no change.

Prussiate of potash, no change.

(E.) The soluble matter (E) was poured off the starch (F), which was washed with water, and the washings added to (E) after the starch had subsided.

The starch (F) was thrown upon a filter, and then dried, at a moderate heat, when it weighed 910 grains.

The water (E) was partly evaporated, and then filtered. It gave a smell resembling mushrooms, and an oily matter floated on the top, of a brown colour. What was left on the filter (E) was of a glutinous nature, brown, and weighed, when dried, 145 grs. The water was again evaporated, until it was reduced to about 12 ounces, when a further precipitate was formed, which was separated by the filter, and weighed 17 grs. (M.) It was then evaporated to dryness, and the residuum weighed 207 grs.

A portion of this when redissolved in water gave a white precipitate with muriate of tin, acetate of lead, and nitrate of silver.

Article IX.

On the Use of continued Fractions with unrestricted Numerators in Summation of Series. By W. G. Horner, Esq.

(*Concluded from* vol. xi. p. 421.)

5. The word "much" in the preceding sentence was written inadvertently: a distinction must be made. It will then appear that the convergency is most promoted where it is most desirable; in the first and second Examples, for instance, more than in the third and 4th. The reason is obvious; for any series of slow convergency, such as the former two, or indeed any series which varies little in passing from term to term, can differ but little from a recurring series commencing with the same course of terms.

The mode of estimating the actual degree of approximation has been already noticed. It may not be irrelevant to exemplify it in this place. The *first three* terms of the continued fraction produce the converging fraction $\frac{1 + P}{1 + p} \times a$. The numerator may be supposed to contain only the first terms of a series $1 + P + Q +$ &c. indefinitely extended; while the denominator $1 + p$ is complete. The value of p being then found from the Equation $Q = 0$, and substituted in the expression for R (Art. 2), gives $R = -\frac{nr - ms}{n + s \,.\, n + 2s} a_1 a_2$ .... (7) for the error of the fourth term in the recurring series equivalent to $\frac{1+P}{1+p}$.

The *first five* terms produce the fraction $\frac{1+P+Q}{1+p+Q} \times a$; and here, as before, the values of p and q being found from $R = 0$, and $S = 0$, produce

$$T = \frac{nr - ms}{n + 2s \,.\, n + 3s} \times \frac{2(nr - ms + rs)}{n + 3s \,.\, n + 4s} \times a_1 a_2 a_3 \text{ .... } (8)$$

for the error of the sixth term in the recurring series.

Example V.—Two varieties of the Binomial Theorem become well adapted to arithmetical purposes, by receiving the fractional form. Putting N = the number whose root is to be found, and P = the nearest complete power; the first variety is $N^{\frac{m}{n}} = (P + x)^{\frac{m}{n}} =$

$$P^{\frac{m}{n}} \div \left\{1 - \frac{m}{n}\left(\frac{x}{P}\right) + \frac{m \,.\, m+n}{n \,.\, 2n}\left(\frac{x}{P}\right)^2 - \frac{m \,.\, m+n \,.\, m+2n}{n \,.\, 2n \,.\, 3n}\left(\frac{x}{P}\right)^3 + \,..\right\}$$

which, after the usual reduction (Art. 4), becomes

$$P^{\frac{m}{n}} \times \left\{ 1 + \frac{mx}{nP +} \frac{n - m \,.\, x}{2 +} \frac{n + m \,.\, x}{3nP +} \frac{2n - m \,.\, x}{2 +} \frac{2n + m \,.\, x}{5nP +} \,\&c. \right. \quad \text{(9)}$$

The other variety is $N^{\frac{m}{n}} = (P - x)^{\frac{m}{n}} =$

$$P^{\frac{m}{n}} \times \left\{ 1 - \frac{m}{n} \left(\frac{x}{N}\right) + \frac{m \,.\, \overline{m+n}}{n \,.\, 2n} \left(\frac{x}{N}\right)^2 - \frac{m \,.\, \overline{m+n} \,.\, \overline{m+m}}{n \,.\, 2n \,.\, 3n} \left(\frac{x}{N}\right)^3 + \cdot \cdot \cdot \right\}$$

which reduces to

$$P^{\frac{m}{n}} \times \left\{ \frac{1}{1 +} \frac{mx}{nN +} \frac{n - m \,.\, x}{2 +} \frac{n + m \,.\, x}{3nN +} \frac{2n - m \,.\, x}{2 +} \frac{2n + m \,.\, x}{5nN +} \,\&c. \right. \quad \text{(10)}$$

The first or second of these theorems may be used, according as the assumed root is less or greater than the true.

It deserves to be remarked, that Halley's rational method, or the common rule for approximate evolution, is contained in the first three terms of each of these formulæ. The error, therefore, of that method may be estimated by formula (7).

Likewise the general form of the fraction equivalent to any odd number of terms of the continued fraction will be found to coincide with the formula for $(a + b)^n$ given by Euler in Inst. Calc. Diff. vol. 2, § 239, without investigation.

The facility of aggregating a continued fraction, and the opportunities it affords of simplifying its terms, and of making allowance for the effect of the final portion which is omitted, are peculiar recommendations of the praxis by the continued fractions (9, 10) in preference to either of the other modes of evolution.

Example VI.—Extract the seventh root of 2. By formula (9), we find $(1 + 1)^{\frac{1}{7}} =$

$$1 + \frac{1}{7 +} \frac{6}{2 +} \frac{8}{21 +} \frac{13}{2 +} \frac{15}{35 +} \frac{20}{2 +} \frac{22}{49 +} \frac{27}{2 +} \,\&c.$$

$$= 1 + \frac{1}{7 +} \frac{3}{1 +} \frac{4}{21 +} \frac{13}{2 +} \frac{3}{7 +} \frac{2}{1 +} \frac{11}{y}$$

putting y = the neglected part $49 + \frac{27}{2} +$ &c. = 61 in the nearest integers. Hence we have

1	7	1	21	2		7		1	61	
$\frac{1}{0}$	$\frac{1}{1}$	$\frac{8}{7}$	$\frac{11}{10}$	$\frac{263}{238}$	$\left(\frac{669}{606} = \right)$	$\frac{223}{202}$	$\left(\frac{1824}{1652} = \right)$	$\frac{912}{826}$	$\frac{1135}{1028}$	$\frac{79267}{71794}$
1	3	4	13	(3)		(2)		11		

Wherefore $\frac{79267}{71794} = 1{\cdot}10408947$ is the root required.

The mode of aggregating progressively is well known: to the product of each numerator by the number which stands over it is added the product of the next preceding numerator by the number beneath it in the lowest line; the sum is the succeeding numerator. The denominators are found in the same way.

The parenthetic abbreviations are in accordance with the principle of reduction already so often alluded to.

6. Among diverging series, those which Euler has named *hypergeometrical,* present, along with the difficulty of determining their sum, a paradox in regard of the equivalence between the series and its sum, which is only to be solved by viewing them in relation to recurring series. Thus, having

$$\frac{1}{1+x} = 1 - x + x^2 - x^3 + \ldots.$$

$$\frac{1+x}{1+2x} = 1 - x + 2x^2 - 4x^3 + 8x^4 - \ldots.$$

$$\frac{1+3x}{1+4x+2x^2} = 1 - x + 2x^2 - 6x^3 + 20x^4 - \ldots. \text{ \&c.}$$

we infer that making $x = 1$, the series of fractions $\frac{1}{2}$, $\frac{2}{3}$, $\frac{4}{7}$, &c. converge towards the value of $1 - 1 + 2 - 6 + 24 - 120 + 720 -$ &c. whose sum has been, correctly in this view, determined by Euler to be ·5963473, &c.

The mode of solution employed by that great analyst was chiefly valuable for the new and useful artifices in integration, which it elicited from his fertile genius. See Lacroix's Diff. and Int. Calc. English Edition, § 414, 218. The result was verified through a widely different process by Dr. Hutton (see his Tracts). Any solution whatever must, very probably, prove sufficiently laborious; but it appears to me that that which is deduced from the principles detailed in this paper, is less operose than either of the above, and leaves less obscurity about the rationale of equivalence.

Example VII.—The general hypergeometric series

$$1 - mx + m\,.\,\overline{m+r}\,.\,x^2 - m\,.\,\overline{m+r}\,.\,\overline{m+2r}\,.\,x^3 + \ldots..$$

compared with formulæ (4, 5,) becomes

$$\frac{1}{1+}\,\frac{mx}{1+}\,\frac{rx}{1+}\,\frac{\overline{m+r}\,.\,x}{1}+\frac{2rx}{1+}\,\frac{\overline{m+2r}\,.\,x}{1}+\frac{3rx}{1+\ldots.} \qquad \ldots\ldots (11)$$

In the particular case already spoken of, $m, r, x,$ are each $= 1$; whence

$$1 - 1 + 2 - 6 + 24 - 120 + 720 - \ldots.$$

$$= \frac{1}{1+}\,\frac{1}{1+}\,\frac{1}{1+}\,\frac{2}{1+}\,\frac{2}{1+}\,\frac{3}{1+}\,\frac{3}{1+}\,\frac{4}{1+}\,\frac{4}{1+} \text{ \&c.} \ldots\ldots\ldots (12)$$

The operations by which the value of this series has been determined at ·5963473, &c. as found by Euler and Hutton, are left for the reader's exercise. They are very similar to those in the following example, where the coefficients are the continued products of the odd series 1, 3, 5, 7, &c.

Example VIII.—If $m = 1$, $r = 2$, $x = 1$, we have

$$S = 1 - 1 + 3 - 15 + 105 - 945 + 10394 - \&c. = \frac{1}{1+}\frac{1}{1+}\frac{2}{1+}\frac{3}{1+}\frac{4}{1+}\ldots\ldots\frac{19}{1+y}\ldots\ldots\ldots\ldots \quad (13)$$

putting y, as before, to represent the residual portion $\frac{20}{1}$ + &c.

The converging series proceeds thus:

$$\begin{matrix} 1 & 1 & 1 & 1 & 1 & 1 & & 1 & 1+y & \\ \frac{0}{1}, & \frac{1}{1}, & \frac{1}{2}, & \frac{3}{4}, & \frac{6}{10}, & \frac{18}{26} & \ldots\ldots & \frac{p}{q}, & \frac{P}{Q}, & \frac{P + 19p + Py}{Q + 19q + Qy} = \\ 1 & 2 & 3 & 4 & 5 & 6 & & 19 & & \end{matrix}$$

$$\frac{15566830368 + 3156467520y}{23758664096 + 4809701440y} = \frac{486463449 + 98619610y}{742458253 + 150303170y} = S$$

Serviceable limits to the value of y may be thus found. Say, generally, $y = \frac{m}{1+}\frac{m+1}{1+}\frac{m+2}{1}$ + &c. Assuming this to be nearly $= \frac{m}{1+y} = \frac{1}{2}(\sqrt{4m+1} - 1)$, it is still more nearly $= \frac{m}{1+}\frac{m+1 \cdot y}{m} = \frac{m(\sqrt{4m+5} - 1)}{2m+2}$. But if one of these is too great, the other is obviously too little. Use then the mean, or say if

$$y \text{ is nearly} = \frac{m}{1+}\frac{m+\frac{1}{2}\cdot y}{m} = \frac{m(\sqrt{4m+3} - 1)}{2m+1} \ldots\ldots\ldots\ldots \text{(A)}$$

$$\text{it is still more nearly} = \frac{m}{1+}\frac{m+1}{1+}\left(\frac{m+\frac{3}{2}}{1+}\right)^{\infty} =$$

$$\frac{m\{(m+1)\sqrt{4m+7} - (m+2)\}}{2m^2 + 4m - 1} \ldots\ldots\ldots\ldots \text{(B)}$$

and there also are one greater and the other less than the correct value of y. When $m = 20$, these limits are,

$$\text{(A)} = \frac{20\sqrt{83} - 1}{41} = 3{\cdot}95631, \text{ (B)} = \frac{20(21\sqrt{87} - 22)}{879} = 3{\cdot}95619$$

Accounting $\therefore y = 3{\cdot}9563$, we find $S = \frac{876632213}{1337102685} = {\cdot}655620711$

$$y = 3{\cdot}9562 \ldots\ldots S = \frac{876622351}{1337087655} = {\cdot}655620704$$

The latter is, therefore, the value of the series, very probably correct in every figure; certainly in all but the last.

W. G. H.

ARTICLE X.

Astronomical Observations, 1826. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Eclipses of Jupiter's satellites.

May 8.	Emersion of Jupiter's first satellite	10h	44′	40″	Mean Time at Bushey.
		10	46	01	Mean Time at Greenwich.
May 15.	Emersion of Jupiter's first satellite	12	39	16	Mean Time at Bushey.
		12	40	37	Mean Time at Greenwich.

Occultations of Stars by the Moon.

May 12.	Immersion of a small star, about the sixth magnitude	12	22	47	Sidereal Time.
May 13.	Immersion of A 2 Cancer	11	55	22·6	Sidereal Time.
May 14.	Immersion of a small star, about the seventh magnitude	12	25	17·3	Sidereal Time.
May 15.	Immersion of a small star, about the seventh magnitude	13	39	38·8	Sidereal Time.
May 15.	Ingress of Jupiter's second satellite	10h	14′	10″	M. T. at Bushey. Contact.
		10	21	29	M. T. at Bushey. On Planet's disc.

ARTICLE XI.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London, for 1826. *Parts I. and II.*

THE first part of the Philosophical Transactions for the present year is occupied by Mr. South's observations of the apparent distances and positions of 458 double and triple stars, made in the years 1823, 1824, and 1825; together with a re-examination of 36 stars of the same description, the distances and positions of which were communicated in a former memoir by himself and Mr. Herschel. Of the results of these observations, it is of course impossible to give any general idea within the space to which we are limited in this article: for powerful testimony to the importance of the subject, and to the success with which Mr. South's labours in this branch of astronomy have been attended, we may refer the reader to the last number of the *Annals*. He will there find the address lately delivered by the President of the Astronomical Society, on presenting the gold medals, awarded by that body, to Mr. Herschel, the author of the memoir before us, and Prof. Struve, for their zealous and indefatigable pursuit of this subject of *Double Stars.*

To these observations Mr. South has appended a synoptical view of the results afforded by them, and by those detailed in the former communication; which itself occupies eighteen large and closely-printed quarto pages. We now proceed to the papers contained in the second part of the Phil. Trans. for 1826.

I. *An Account of the Construction and Adjustment of the new Standards of Weights and Measures of the United Kingdom of Great Britain and Ireland.* By Capt. Henry Kater, FRS.

The labours in this interesting application of the refinements of modern science to the arts and purposes of civil life, in which some of the most distinguished natural philosophers of the present day have, for about ten years past, been engaged, terminate, we presume, with the verifications recorded in this paper. When we reflect on the unremitting diligence with which those labours have been prosecuted,—on the manner in which so many distinct branches of mathematical and physical research have been concentrated, as it were, and directed towards the objects to be attained,—and on the final results, as well philosophical as practical, of the whole inquiry, we think we may with justice congratulate our readers, and the country at large, on the satisfactory establishment of the long-desired uniform system of weights and measures. And we are far from considering, as appears to have been done by some writers on the subject, that the objections lately urged, with so much accuracy of reasoning, by Capt. Sabine, against the means at present appointed for ascertaining and recovering the standard of linear measure, tend essentially to invalidate the new system. They seem to us, on the contrary, to contribute powerfully to its support; by showing the pendulum to afford the most appropriate natural standard for the purpose, requiring, however, corrections and modifications hitherto unemployed. Nor should it be forgotten, as an evidence of the soundness of the principles on which the system has been founded, that the experiments which indicate the expediency of these corrections, constitute, in fact, a portion of the train of researches to which the means pursued for "ascertaining and establishing uniformity of weights and measures," have given rise. They were undertaken by Capt. Sabine, in consequence of the discrepancies, with regard to the figure of the earth, of the results obtained, by combining the lengths of the pendulum observed by Capt. Kater at the different stations of the trigonometrical survey of Great Britain, with those observed in France. We would suggest, then, that the proper view to be taken of the subject is the following: That a high degree of precision in the means of determining the natural standard, and one in all respects worthy of the existing state of science, has already been attained; but that a portion of the investigation for the ascertainment of those means, has shown a further refinement of them to be desirable. And it may be observed, that the imperial standard-yard, which is the unit of the measures of length, and an aliquot part of which gives the means of recovering the standard of weight, from which again the measures of capacity are derived, will remain unaltered, together with all the derivations from it, whatever rectification the natural standard may be found susceptible of in future.

The legislative recognition of two denominations of weight, in retaining both the troy and the avoirdupois pound, has been mentioned as a defect in the system. But we think that the inconvenience in numerous commercial concerns, which must have been experienced, had either of them been rejected, would have much more than counterbalanced the departure from strict uniformity, (a departure, indeed, which we conceive to be rather imaginary than actual) that has ensued from the retention of both.

In their determination of this and similar points, the Commissioners of Weights and Measures appear to have acted, and in our opinion wisely so, in the spirit of Sir G. Shuckburgh Evelyn's remarks on the propriety of retaining the commonly-received denominations of quantity. These remarks are so apposite on the present occasion, that we must be permitted to quote them; premising, however, that they contain some implied reflections on the French *Systême métrique* not altogether deserved. They occur in Sir George's "Endeavours to ascertain a Standard of Weight and Measure," (Phil. Trans. 1798) and it will be remembered with gratitude to his memory, that in these very accurate "Endeavours," was laid the foundation of the new system.

After having ascertained the length of the proposed natural standard, the pendulum, and determined the weight of any given bulk of water compared with it, Sir G. S. proceeds to deduce the proportion of these to the commonly-received weights and measures of this kingdom. "It is perfectly true," he observes, "that if I chose to indulge in fanciful speculation, I might neglect these comparisons, as an unphilosophical condescension to modern convenience, or to ancient practice, and might adopt some more magnificent integer than the *English pound* or *fathom;* such as the *diameter* or *circumference* of the world, *&c. &c.* and, without much skill in the learned languages, and with little difficulty, I might ape the barbarisms of the present day. But in truth, with much inconvenience, I see no possible good in changing the quantities, the divisions, or the names of things of such constant recurrence in common life; I should therefore humbly submit it to the good sense of the people of *these* kingdoms at least, to preserve, with the measures, the language of their forefathers. I would call a yard a yard, and a pound a pound, without any other alteration than what the precision of our own artists may obtain for us, or what the lapse of ages, or the teeth of time, may have required."

An abstract of Capt. Kater's paper now before us, has already appeared in the *Annals* for February last; but there are some particulars respecting the construction of the standards, and the adjustment of the national copies of the imperial standard-yard rigorously identical in length with Sir G. Shuckburgh's scale, that demand insertion in this place.

Brass being peculiarly liable to decomposition in the atmosphere of London, the standards of weight and of measure of capacity, have been formed of an alloy, consisting of 576 parts of copper, 59 of tin, and 48 of brass; and this is equal in hardness to hammered brass, and can be worked with the same facility.

The construction of the standards of measure of capacity, and of the weights, is thus described:

"In order to avoid any innovation but such as might be absolutely necessary, it was deemed expedient in constructing the bushel, to adhere as nearly as possible to the form of that known by the appellation of the Winchester bushel. It was therefore directed to be made cylindrical, the interior diameter being about 18½ inches, the exterior 19½ inches, and the depth about 8¼ inches, and intended to contain eighty pounds avoirdupois of distilled water. In order to give the bushel additional strength, it was cast with two projecting hoops, one to which the bottom was screwed, and another at the distance of about half an inch from the top.

"Considerable difficulties arose in casting the bushel; out of twelve, only five proved sound enough for use; but by varying the process, they were at length procured sufficiently perfect. Much credit is due to Mr. Keir, the engineer employed by Mr. Bate in turning the bushels, for the beauty and perfection of his work.

"The form of the gallon measure occupied much of my attention. It was necessary that it should be such as to enable me to determine the weight of distilled water it should contain with the least liability to error. The conical form was therefore adopted; the mouth being made cylindrical, and one and a half inch diameter: the top was ground perfectly flat, and the edge so rounded off, that the contents might be poured from it into any other vessel without running down the side. The cone was placed in a cylinder about four inches high, in which handles were formed, and which served at the same time to protect the gallon from injury, and to prevent any change of temperature which might arise from handling. The quart and the pint measures were of the same form, on a smaller scale.

"The weights were of brass, and nearly of a spherical form, but flattened at the bottom. Into the top was screwed a button; beneath which a small cavity was left to receive such minute pieces of wire as might be found requisite to make up the standard-weight. This button served also to lift the weight by means of a strong wooden fork."

Four standard-yards were made by Mr. Dollond, of brass, one inch square; having firmly screwed to their extremities rectangular pieces of steel of the same width as the bar, and projecting above its surface. The distance between the interior faces of the steel terminations was intended to be equal to the length of the imperial standard-yard; and one of them, since deposited at the Exchequer, Westminster, Capt. Kater found to be perfectly correct; whilst one of the others was only ·00038 of an inch too short, and of the remaining two, one was ·00021 of an inch too long, and the other the same minute quantity too short.

The following section of the paper is so important, and furnishes an example of extreme precision in these adjustments so truly philosophical and interesting, that we must give it entire.

"*Adjustment of the Standard-Yards with Gold Points.*

"The standard yards last described are intended merely for the purpose of sizing those employed in commerce, and the trifling differences above stated may be utterly disregarded; but the Commissioners of Weights and Measures thought it desirable, that accurate copies of the imperial standard-yard should be made, to be carefully preserved and transmitted to posterity, solely for the purpose of being referred to upon extraordinary occasions, or upon questions important to science.

"The difficulty of transferring a given distance from one scale to another, is well known to all who are acquainted with the subject; the operation is one of considerable delicacy; and notwithstanding every precaution is seldom absolutely free from error. But a national standard should be accurately that which it professes to be. It is not enough to determine its error, as the record of this may in process of time be lost; it therefore became necessary to devise a method by which any perceptible error in those standards which are the foundation of all the others, might ultimately be annihilated.

"The four standard-yards which I am about to describe are of brass, an inch and a quarter wide, and half an inch thick. This thickness is the same as that of Sir G. Shuckburgh's scale, and was chosen in order that both might be affected with equal readiness by any change of temperature; for as the imperial standard-yard of 1760 is one inch square, I thought it preferable to adjust the new standards by means of Sir G. Shuckburgh's scale, which, as I have before remarked, does not sensibly differ from it.

"A disk of gold being let into the surface near one extremity, a hole was drilled through the bar at the distance of thirty-six inches from the centre of the disk, and being made slightly conical, a plug of brass was ground in the hole so as to fit it perfectly. A gold disk was let into the top of the plug, and reduced to a level with the surface of the scale. The other end of the plug projected beneath the scale, and had a small hole through it to admit a wire, by means of which it might be turned round. A very fine deep dot was then made by Mr. Dollond upon each of the gold disks, as nearly as it could be done at the distance of thirty-six inches from each other, the dot upon the moveable disk not being exactly in its centre.

"Before the plug was ground in its place a small hole was drilled through the side of the scale into the conical aperture.

"The microscopical apparatus employed on the present occasion, has been described in the paper upon the comparison of various British standards of linear measure, before quoted.

"The cross-wires of the microscopes being brought respectively over zero, and thirty-six inches upon Sir G. Shuckburgh's scale, the apparatus was transferred to the new standard, and the intersection of the cross-wires of one of the microscopes placed upon the centre of the fixed dot. The moveable dot was then brought, by turning the brass plug, to the intersection of the cross-wires of the other microscope.

"The distance of the dots was repeatedly compared with Sir G. Shuckburgh's standard upon different days, in order to ascertain that no perceptible error remained. A drill was passed through the hole in the side of the scale, and the brass plug carefully pierced through; a pin was then driven into the plug so as to render any change of position impossible, and the projecting part of the plug was cut off.

"The standards being thus finished, they were again compared with Sir G. Shuckburgh's scale, and it was with surprise and disappointment that I found the whole of them apparently too short. They had been adjusted upon a board of mahogany carefully planed, and the table upon which they were now placed was so flat as to occasion little alteration in a spirit-level passed along it. The error of the standards was, however, far too considerable to be attributed to any curvature which on this occasion could take place, and it was not until after several days that I discovered the cause of this perplexing circumstance. I found that by placing a card, the thickness of which was accurately one-fiftieth of an inch, under the middle of the standard, the distance of the dots was much increased, and by placing a card of the same thickness under each of the extremities, and withdrawing that which was under the centre, the distance of the dots was considerably diminished. The total difference amounted to no less than ·0016 of an inch, whilst the double of the error which would have arisen from mere curvature under similar circumstances, would not have been one ten-thousandth of an inch.

"The cause was now evident, by elevating the middle of the standard, the under surface was shortened, and the upper surface extended; and on the contrary, when the extremities were elevated, the upper surface was compressed, and the lower surface lengthened, the quantity of the effect evidently depending upon the thickness of the bar.

"Having thus assured myself of the source of the error, a method of obviating it soon presented itself. As the upper and under surfaces of the bar are in different states, the one being compressed and the other extended, there must be an intermediate plane which suffers neither extension nor compression, and this plane must be nearly midway between the two surfaces. I therefore caused Mr. Dollond to reduce the thickness of the bar for the distance of an inch and three quarters from its extremities to one-half; the gold disks and plugs were then inserted as before, and the adjustment completed in the manner which has been described. The plugs being secured, and the projecting parts removed, the standards were repeatedly compared with Sir G. Shuckburgh's scale (the standard being placed upon the scale) when no perceptible difference could be detected. Pieces of card were now placed under the standard as before, without occasioning any appreciable alteration; and I had thus experimental proof of the perfect efficiency of the remedy I had employed.

"I have been thus particular in detailing the difficulties I experienced, because they exhibit a source of very considerable error which may arise from the thickness of a standard-scale, and which, I believe, has never before been suspected.

"It may be here not unnecessary to remark, that on every occasion on which I have used Sir G. Shuckburgh's scale, it has fortunately been placed not only upon the same table, but upon the same part of it."

The various standards described in this paper, with the exception of the yards with steel terminations, are not intended for common use, but to be carefully preserved for reference upon extraordinary occasions. In addition to them, other weights and measures of capacity were made with great care by Mr. Bate. The following is a list of the whole; the numbers of the standards corresponding with those inscribed on them by Capt. Kater, during the adjustment, as detailed in the memoir.

"*Standards deposited at the Exchequer, Westminster.*

1 Imperial standard-yard with gold points.
1 Standard-yard with steel terminations, No. 1.
1 Imperial troy pound, No. 5.

1 Avoirdupois pound, No. 1.
1 Avoirdupois pound, No. 5 (in a box with smaller weights.)
1 Weight of imperial gallon of water, No. 1.
1 Imperial gallon measure, No. 3.
1 Bushel, No. 3.
1 Quart, No. 4.
1 Pint.
A copy of the imperial gallon.
quart, and
pint.

1 Bushel. 1 Half-bushel. 1 Peck.	for common use.
1 Gallon. 1 Half-gallon. 1 Quart. 1 Pint. 1 Half-pint. 1 Gill. 1 Half-gill.	cylindrical, for common use.

1 Set of avoirdupois weights, from 56 lbs. to half a drachm.
1 Set of counterpoises for the above set of weights.
1 Set of troy weights, from one pound to one grain, with counterpoises."

"*Standards deposited at Guildhall, London.*

1 Imperial standard-yard with gold points.
1 Standard-yard with steel terminations, No. 4.
1 Imperial troy pound, No. 1.
1 Avoirdupois pound, No. 2.
1 Weight of imperial gallon of water, No. 3.
1 Imperial gallon measure, No. 5.
1 Bushel, No. 4.
1 Quart.
1 Pint.
1 Set of avoirdupois weights, from 56 lbs. to half a drachm."

At Edinburgh and Dublin sets of standards have been deposited, corresponding with the set at Guildhall.

As an Appendix to this paper, a table is given of the correction on account of temperature to be applied to the contents of the gallon; and in a postscript Capt. Kater awards to Professor Bohnenberger, the honour of having first proposed to determine the length of the seconds pendulum by means of the convertible pendulum, in an astronomical work, published at Tübingen in 1811.

II. *Description of an improved Hygrometer.* By Mr. Thomas Jones: communicated by Capt. Kater.

This brief communication is as follows:

"The attention of the scientific world has been lately so much occupied in experiments on atmospheric phænomena, that it is hoped any simplification or improvement in the instruments employed for that purpose may not be unacceptable.

"The principle of the hygrometer which I am about to describe, is that of enabling the observer, readily and accurately, to ascertain by direct and simple means, the degree of temperature at which the moisture of the atmosphere is condensed, and the instant at which that operation commences.

"The hygrometer is composed of a mercurial thermometer, the graduated scale of which is about four inches and a half long; at the lower part of the scale the glass tube is bent to form a right angle, at the end of which the bulb of the thermometer rises parallel to the scale, and about one inch from it; the bulb is about one inch long, and of a cylindrical form, with a black convex top, the diameter of which is a little more than that of the cylindrical part, which is covered with silk. The scale is attached to a piece of cylindrical wire, three inches long, and turns upon a joint screw passing into its edge, the other end of which wire being placed in a tubular foot fixed to the inside of one end of the case, forms a stand for the instrument. The case contains a small bottle for ether.

"The thermometer thus constructed will give both the temperature of the air and that of the dew-point, which last is effected by placing the mouth of the bottle containing the ether, in contact with the upper part of the covered surface of the bulb, when, by gently inclining the bottle, the ether will flow downwards without wetting the top of the bulb, which will almost immediately become dull by the deposition of moisture on its surface; when the observed temperature may be taken and the difference ascertained.

"Should it be objected against the principle of the instrument here proposed, that the indications do not exhibit the true temperature of the upper surface of the bulb, on which the deposition of dew takes place, but that of the lower part, to which the ether is applied; it may be answered that by inclining the whole instrument so as to render the axis of the bulb horizontal, and establish thereby a free circulation of the mercury in every part, this objection may be obviated; but on repeated trials I have not found this to produce any difference in the results.

"I ought also perhaps to mention that an instrument somewhat similar in principle has been used in Vienna, and was mentioned by Prof. Baumgarten of that capital to a friend, who communicated the fact to myself."

A representation of this hygrometer is given in an accompanying plate. E. W. B.

(*To be continued.*)

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

May 25.—The Right Hon. Sturges Bourne, and Dr. A. P. Wilson Philip, were admitted Fellows of the Society; and the reading of Mr. Osler's paper, On the Burrowing and Boring Marine Animals, was concluded.

In this paper, the operations and mechanism of burrowing and boring, as practised by various marine animals, belonging to the classes *Mollusca* and *Annelides,* are first minutely described. Facts are then adduced, tending strongly to prove, that the *Lithophagi* effect their perforations, not by mechanical means, but by a solvent fluid, which, however, being secreted only when required for use by the animal, the author has not been able to detect by chemical tests. These animals perforate calcareous stone, and shell, but their progress is stopped by siliceous or argillaceous matter, on which they are unable to act; thus a thin layer of clay occurring in a rock which they are perforating, forms to them an impassable obstacle. Another important fact related in this paper, having the same bearing, is as follows:—The *Saxicavæ* often exert their boring powers on the shells of contiguous individuals of their own species; and so long as they have not penetrated through them, no notice is taken of it by the animals attacked; but when the perforation is complete, or very nearly so, the aperture is immediately filled up, not with shell, but with a yellow animal-substance, insoluble even in the mineral acids.

June 1.—The following papers were read:—

An Account of some Experiments relative to the Passage of Radiant Heat through Glass Screens; by the Rev. Baden Powell, MA. FRS.

The object of this paper was to examine a question arising from De la Roche's experiments, as to a particular case in which that experimenter supposed there must be a direct transmission of simple radiant heat through glass. This case is that of a second glass screen interposed between a first and the thermometer, when M. De la Roche found the additional diminution much less in proportion, than that occasioned by the first screen on the total effect. He hence supposed the heat to have acquired a property analogous to polarization, by which it was enabled to penetrate the second screen without loss.

The experiments here detailed were designed to examine, first, whether this effect could be verified; and, secondly, whether, if so, it could be accounted for without introducing any new or peculiar property of heat. The results show that the fact is completely verified, and at the same time the peculiar explanation rendered unnecessary; as from observing the temperatures acquired by the screens, it appeared that the effect was exactly such as would be accounted for from the simple circumstance of a secondary radiation from the screen.

In the sequel of the paper the recent experiments of Mr. Ritchie were adverted to, who has maintained that simple heat radiates directly through very thin glass when transparent, but not when opaque. This result was tried by a different method from Mr. R.'s, and no difference was found to be occasioned by the transparency of such a screen.

Thus two apparent exceptions to the general law "that simple heat cannot permeate glass," are done away.

An Account of a Telescope having only one Reflector, and of easy Management in observing; by the Rev. Abram Robertson, DD. FRS.

Account of some Experiments on the Laws of Electrical Accumulations on coated Surfaces; On the Construction and Use of a Magnetic Balance; and On the Electrical Conducting Power of various Metallic Substances; all by W. S. Harris, Esq.: communicated by the President.

June 8.—*The Bakerian Lecture; On the Relations of Electrical and Chemical Changes;* by Sir H. Davy, Bart. PRS. was read.

The experimental investigations and results brought forward in this Lecture, are prefaced by a historical sketch of the origin and progress of electrochemical science, with a view of correcting the erroneous statements that have appeared on the subject. The origin of this branch of knowledge is stated to be the discovery of the decomposition of water by the voltaic pile, by Messrs. Nicholson and Carlisle in 1800. This was followed by the experiments of Cruickshank and of Dr. Henry, and by several papers by the author himself, the chief contents of which are stated, and in which the appearances of acids, and oxygen, at the positive, and of alkalies, sulphur, and the metals, at the negative pole, were described.

The experiments of Hisinger and Berzelius, in 1804, are placed next in order, which establish similar results; and in 1806, on the occasion of the agitation of the question respecting the production of muriatic acid and fixed alkali from pure water, the author presented to the Royal Society his Bakerian Lecture on the Chemical Agencies of Electricity, in which he drew the general conclusion that the combinations and decompositions by electricity, were referrible to the law of electrical attractions and repulsions—a theory in which, he observes, he has hitherto found nothing to alter, and which, after a lapse of 20 years, has continued, as it was in the beginning, the guide and foundation of all his researches. The instruments used in the experiments of the present paper for detecting and estimating electric currents of low intensity, were constructed on the principles of the multiplier of Prof. Schweigger, and the galvanometer of Prof. Cumming. For determining weak electricities of tension, Volta's condenser, connected with Bennet's electrometer, or with one consisting of a silk filament rendered conducting by charcoal dust, was employed. Much dependence was, however, never placed on these instruments, unless their indications were otherwise confirmed.

The author now proceeds to the experimental inquiries which form the chief object of his lecture, and to the general views of electrochemical agency to which they appear to lead. And first, he considers the electrical and chemical effects exhibited by combinations of one metal and one fluid : the nature of these effects is best explained by an example. When two pieces of polished copper connected with one extremity of the wire of the multiplier, are plunged into a solution of an alkaline hydrosulphuret, if introduced at the same instant, there is no action ; but if in succession, a sensible interval being allowed to elapse, there is a distinct or even a violent electrical effect, and the piece of metal first introduced is negative with respect to the other: this effect depends on the formation of a coat of sulphuret of copper *on* the plate first introduced, while it is negative with respect to metallic copper. Hence the combination is in strictness one of three elements; copper, sulphuret of copper, and the solution. In like manner, protoxide of copper is negative with respect to pure copper, and to the sulphuret.

The production of electrical currents by single metals and single fluids occurs generally whenever new products adhering to the metallic surfaces are produced ; and if the same products be applied artificially, the effects are the same, as if the adhesion had been caused by the natural action of the fluid on the metal. The chemical changes produced in the fluid by the ternary combinations thus formed are, in all cases, such as tend to restore the deranged equilibrium, hydrogen passing to the negative side, and oxygen to the positive, until the oxides are revived.

The case of two imperfect and one perfect conductor is next considered, as two fluids and a metal or charcoal. Here the author controverts an opinion advanced on high authority, respecting the alleged development of electricity on the combination of acids and alkalies, which he refers to the contact of metals with these agents, to change of temperature, evaporation, &c. and never to the mere union of these bodies : several experiments are adduced in support of this opinion.

When platinum is brought into contact with an acid, the pole touching the acid is negative, the opposite pole positive, and *vice versâ* where it touches an alkali; and the same is the case with rhodium, iridium, and gold, the effect being greater as the action of the acid on the metal is greater. From this it follows, that when a metal is in contact with an acid or alkali in one cup, and water or a neutro-saline solution in another, on completing the circuit, the contact of the metal with the acid or alkali will determine the character of the pole in contact with it, and that in contact with the other fluid will of course be of the opposite name, and this result is confirmed by experiment. In such combinations, the chemical changes are such as might be expected; oxygen and the acids tending to circulate towards the negative surface, and hydrogen and the alkalies towards the positive.

In combinations consisting of two perfect conductors and one fluid, the order in which the metals exhibit their electricities is connected with their oxidability, the more oxidable metal being positive with respect to all below it. It is not, however, any inherent quality in the metals which determines this effect, but their fitness for chemical action; for if the state of aggregation be altered, and the cohesive force, which always acts as an antagonist force to chemical changes, be weakened, the positive energy is exalted in proportion: thus the amalgams of the positive metals are positive with respect to the pure metals of which they are amalgams. In general, the electricities developed by metallic contact are too strong to be subverted by an opposite action with the fluids with which both are in contact. Such, however, is sometimes the case; and in all instances, the influence of the fluid is perceptible.

The author next considers the accumulation of electricity, and the chemical changes it produces in voltaic arrangements. According to Volta's view of the action of the pile, the metals were regarded as the only agents, and the chemical changes arising in the fluids as mere results not essential to the developement of the electricity. This view, however, may be regarded as altogether disproved by an experiment here described, in which, when two glasses, filled with solution of nitrate of potash, in which were plunged respectively zinc and platinum connected by the multiplier, were connected by substances capable of conducting electricity, but not of propagating chemical action, such as unoxidable metals, the circulation of the current was altogether destroyed.

Since the chemical changes always tend to restore the equilibrium, destroyed by the contact of the metals, in the fluids of a pile, it is evident that the relation between the fluids themselves and the surfaces with which they are in contact, will be altered by a continuance of the action of the pile. Hence it is easy to perceive the possibility of a re-action taking place when the circuit is broken, or the disposition of the parts of a pile is changed, or one or more parts of a compound circuit abstracted. Many curious phænomena, of which hitherto no explanation has been offered, may be explained by this view of the subject; such as the secondary piles of M. Ritter,—the supposed polarization of electricity, concluded by M. de la Rive from his experiments on the interposition of metallic plates in the fluids of a pile,—and the continuance of electro-motive action of detached portions of a circuit, after the destruction of the circuit itself. This *re-action* is illustrated in the present paper by an experiment, in which a circuit primarily inactive, consisting of six arcs of platinum in vessels filled with solution of nitre, was made part of a battery consisting of 50 pairs of plates of a combination primarily active. After continuing the circuit some time, it was broken, and the platinum arcs, detached and formed into a circuit, were found to possess independent action, contrary to that of the pile, which had thus rendered them re-active.

This singular consequence is pursued yet further in another experiment here stated, in which detached portions of a battery of 50 plates which had been some time in action, were examined as separate piles, after breaking up the combination. When they had been placed *conformably* in the original battery, their independent action was found to be very much weakened by the re-action thus produced, which in this case opposed their natural effect; whereas, when *unconformably* placed in the original battery, their action when detached was found exalted to three or four times its natural intensity.

The author next proceeds to point out some general observations and practical applications which suggest themselves on a view of the foregoing results. The chemical changes in a conducting liquid, he first shows, take place only in the immediate vicinity of the immersed poles, the rest of the liquid affording only a tranquil passage to the electricity. This leads him to consider the motions produced in mercury when interposed in the circuit under an electrified fluid, which he regards as arising from the two electricities, acting as transporters of ponderable matters which assume their own peculiar characters when they reach their point of rest. The lecture concludes with some suggestions as to the use of the multiplier to obtain exact numerical measures of the electro-dynamic relations of chemical elements; and with some applications of the preceding results to the useful arts, especially in the preservation of the copper on ships, and of the iron boilers of steam-engines.

A paper was also read, On the Discordances between the Sun's observed and computed Right Ascensions as determined at the Blackman-street Observatory; by James South, Esq. FRS.

June 15.—Sir G. Nayler, Knt. Garter King at Arms, was admitted a Fellow of the Society; and the following papers were read, or their reception announced:

Observations on a Case of Restoration of Vision: by James Wardrop, Esq: communicated by the President.

In this paper is described the operation of forming an artificial pupil, by which sight was given to one eye of a lady, forty-six years of age, who had been blind from infancy. The globe of the other eye was collapsed. The phænomena ensuing, in the gradual acquisition of the various discriminations of sight, agreed with those detailed in similar cases by Cheselden, and others.

On the Existence of a Limit to Vaporization; by M. Faraday, Esq. FRS.

Some notice of the arguments brought forward in this communication, and of the facts on which they are founded, may be seen in our report of the Proceedings of the Royal Institution, at p. 390 of the last volume of the *Annals*.

On Electric and Magnetic Rotations; by Charles Babbage, Esq. MA. FRS.

On the Progressive Compression of Water by High Degrees of Force, with some Trials of its Effects on other Fluids; by Jacob Perkins, Esq.: communicated by W. H. Wollaston, MD. VPRS.

In this paper Mr. Perkins first describes in detail, with the aid of illustrative drawings, the apparatus for experiments on the compression of water, suggested by him in his paper on the subject published in the Philosophical Transactions for 1820. He then briefly relates some of the experiments performed by its means, referring to a plate annexed representing by a curve, the law of condensation under pressures of from 10 to 1000 atmospheres, and also to a table showing the results numerically. In one experiment, the water was compressed one-twelfth of its volume, by a pressure of 2000 atmospheres. Some experiments on other liquids, and on aëriform fluids, are also adverted to: among the former, acetic acid was crystallized, and among the latter atmospheric air and carburetted hydrogen gas were liquefied, by the same apparatus.

On the Figure of the Earth; by G. B. Airy, Esq. MA.: communicated by the President.

Observations for determining the Amount of Atmospheric Refraction at Port Bowen; by Capt. W. E. Parry, FRS.; Lieut. H. Foster, FRS.; and Lieut. Ross.

On the Crystallization of Uric Acid; by Sir Everard Home, Bart. VPRS.

Microscopical Observations on the Muscular Fibre of the Elephant; by Herbert Mayo, Esq.: in a letter to Sir E. Home.

The reception of papers, on some phænomena in magnetism, and on a shell exploding by percussion, by Mr. Christie and

Col. Millar respectively, was also announced; and the Society then adjourned over the long vacation, to meet again on Thursday, the 16th of November next.

PROCEEDINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN, AT THE FRIDAY-EVENING MEETINGS.

May 26.—Dr. Harwood read the second part of his paper on the Elephant genus, his observations being at this time confined to the natural history of the African species, with an account, however, of peculiarities in the structure and in the senses of Elephants generally. The communication was illustrated by a very numerous set of drawings, and specimens, a great number of them from the magnificent collection of Mr. Brookes.

Several models of ancient buildings were placed on the library tables by Mr. West, the general aspect and appearance of decay being given them by a peculiar method of colouring the substance, as well as the form of the building.

June 2.—Mr. S. Solly completed his observations on the porphyry of Christiania.

A rifle, of a new construction, was laid upon the table, remarkable for its lightness. The length of the barrel was 24 inches, and the weight of the whole only 4½ lbs. It was constructed under the direction of Mr. Leigh.

June 9.—The subject of the evening was the tunnel at Rotherhithe. Its history was given by Mr. Faraday from the lecture-table for Mr. Brunel, and illustrated by numerous fine drawings, models, and apparatus. The undertaking was followed from its commencement to the present time; the beautiful contrivances of Mr. Brunel explained, the weights and measurements given, and the present condition of the work stated. It has been conducted to its present state with the greatest success, and from the experience obtained, there is every reason to anticipate that success will attend it to its conclusion.

The meetings of the members were then adjourned till next season.

GEOLOGICAL SOCIETY.

May 19.—A paper, entitled Notes on the Geological Position of some of the Rocks of the NE. of Ireland, by Lieut. Portlock, Roy. Eng. FGS., was read.

In this paper, the author alludes to the communications on the same subject by Dr. Berger, Dr. Buckland, and the Rev. W. D. Conybeare, published in the Geological Transactions; and, after some remarks on the granite and mica-slate rocks of the Mourne Mountains, the Carlingford, and another groupe occupying a large portion of the north of Derry, (the barometrical admeasurement of which is given,) he proceeds to notice the phænomena of the basaltic range, and to observe the connection of the indurated chalk with the basalt; beginning at the south near Belfast, where it is underlying, and almost in contact with the basalt of Mount Divis, tracing it at various points northwards to Ben Evanagh, and high up in Benbradda, and describing the gypsiferous marle, having the same dip (30° NW.) and line of direction as the chalk; next to which, and between it and the basalt, there is generally a thin stratum of ochre. To the south of the line of chalk, and resting on the Dromore porphyry, a highly indurated argillaceo-siliceous schist is found, passing by various shades into a claystone porphyry, being, however, in its simple state harder than the basis of the porphyry.

The author concludes by giving his opinion that the density and crystallized structure of basalt are not affected by the amount of pressure, and stating that he has not been able to make out any decided proof of the stratification of that rock.

June 2.—A paper, entitled, On the Freshwater Strata of Hordwell, Beacon, and Barton Cliffs, Hants, by C. Lyell, Esq. FGS., was read.

The author, after confirming Mr. Webster,s discovery of a distinct freshwater formation on the Hampshire coast, corresponding with the lower freshwater formation in the Isle of Wight, states, that in consequence of the suspicions entertained of the possible occurrence of the upper marine formation in some of the upper strata of Hordwell cliffs, he has examined the beds; a minute detail of which, in their order of superposition, together with the organic remains peculiar to each, is given. Bituminized wood, seeds, and capsules of plants (among them *Carpolithes thalictroides*, Brongnt.), with freshwater shells, abound therein; and, in a bed of calcareous marle, sometimes slightly indurated, from 6 to 8 inches thick, and consisting of an aggregate of *Planorbes* and *Lymnææ*, an abundance of Gyrogonites (*Chara Medicaginula*) was found. In the bed immediately above were discovered the scale of a Tortoise, and the teeth of a Saurian, probably a Crocodile.—From the presence of two species of *Serpula* the author supposes that this series of strata might have been formed in an estuary. The shells, from the occurrence of which the existence of marine strata in Hordwell cliff had been before inferred, prove to be species of *Potamides*, a freshwater genus; and the beds which lie above these are exclusively freshwater.

Of the new organic remains, the valves of a *Cypris*, smaller than that found in the Weald clay, but in as great proportion, are characterized as the most interesting, and a small *Ancylus* is also noted; whilst the presence of gyrogonites and *Carpolithes thalictroides* is quoted as completing the resemblance of the Hordwell strata to those of the Paris basin.

The author further observes, that the freshwater strata do not crop out in Beacon Cliff, as had been supposed, but are continued for about a quarter of a mile or more in Barton Cliff, interposed between the diluvium and white sand that cover the London clay: and, scarcely hesitating to refer the white silicious sand (which rises in Beacon Cliff, and is continued through Barton as far as the High Cliff, near Muddiford), and, consequently, the analogous bed resting on the London clay in Alum Bay, to the freshwater series, he concludes, from the inclination of the strata in the latter place, that the freshwater formations suffered, though in a less degree, the disturbance to which the vertical strata of the Isle of Wight were subjected.

June 16.—A paper was read, entitled, Notes on the Geological Structure of Cader Idris; by Arthur Aikin, Esq. FGS.; an abstract of which will appear in our next number. E. W. B

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *On the Absorption of Gases by Liquids.* By T. Graham, MA. (Communicated by the Author.)

1. Liquids are in general miscible with one another, in all proportions as water and alcohol, or in a limited degree as ether and water; ether agitated with water taking up one-tenth of its weight of that liquid.

2. Frequently the mixing of liquids exhibits the closeness of chemical union, among other points, in the manner in which the *volatility* of the compound liquid is affected. Thus the vapour from pure alcohol at 170° Fahr. supports a column of mercury of 30 inches, but by mixing the alcohol with a quantity of water, we impair the volatility of the alcohol; and we may form mixtures which require a temperature of upwards of 200°, to produce vapour capable of supporting such a column. In the same way portions of water are retained with so great force by sulphuric acid, as to require, in order to drive them off, a degree of heat greatly higher than the boiling point of water. From these, and other instances of this affinity, we learn, that in a mixture of a volatile and more fixed liquid, the tendency of the

more volatile ingredient to pass into vapour may be checked in a considerable degree by its connexion with the other liquid.

That many reputed gases, at a low temperature, or under great pressure, assume the liquid form, has been demonstrated by Mr. Faraday.* The researches of that ingenious chemist on gaseous liquefaction, strongly impress the doctrine, that in the physical states of gas, liquid and solid, there is nothing of absolute permanency, and that any body may assume consecutively all these forms. Hence it follows that those bodies, which at the temperature of the atmosphere, we experience to be gases, may be considered without impropriety as volatilized liquids; and we may predicate of such bodies, the common properties of liquids. Of these properties, two have been mentioned, which alone will be applied to the elucidation of the phenomena of absorption.

It is then assumed that the gases if liquefied (by pressure or any other means), would in general mix in some proportion or other, with such ordinary and reputed liquids as we had it in our power to present to them; and that they would be retained in part by these liquids, through the agency of the mutual attraction evinced in liquid mixture, even although the pressure under which the union took place were considerably reduced, and the temperature raised. In this way there might result a mixture of a liquefied gas and a common liquid in reduced proportions, at the ordinary atmospheric pressure and temperature.

But it is not necessary to suppose that the gaseous bodies, whose absorption by liquids it is attempted to explain, be presented in a liquefied state. Analogy will lead us to expect that the mere injection into our absorbing liquids, of such gases in their elastic state, will occasion their liquefaction, and consequently bring into play the affinities of liquids, and the concomitant diminution of volatility, on which the explanation is founded.

Thus: sulphuric acid, concentrated as much as it can be, boils at about 620°. Let a quantity of sulphuric acid so concentrated be heated to 600°, and kept at that temperature, and let the steam of water previously raised to the same temperature, be conducted into it. We would predict, without the least hesitation, as the result, the detention and absorption of the steam, notwithstanding its high elasticity, until the boiling point of the acid was reduced by the dilution to 600°. Here then we have an instance of the absorption of a gaseous body (steam at 600°), by a liquid at the same temperature; yet in order to liquefy the gaseous body absorbed, in the ordinary way, it would be neces-

* Philosophical Transactions, 1823.

sary to cool it down through the long space of nearly 400°, or to 212° Fahr. Such a reduction below the degree of temperature at which the absorption took place, would be productive, in all probability, of liquefaction in the case of the most refractory of the gases. Now a composition of sulphuric acid and water, the same in every respect, might be obtained more directly by simply mixing together the ingredients, both being in the liquid state. This case of the absorption of a gaseous body by a liquid is, therefore, dependent upon the affinity which occasions the miscibility of liquids, and is, in fact, an instance of the mixture of two liquids. Many similar illustrations might be adduced if required.

We are, therefore, authorized in concluding that gases may owe their absorption by liquids,—to their capability of being liquefied, and to the affinities of liquids (apparent in their miscibility), to which they become in this way exposed. These properties may, therefore, be considered as the proximate or immediate causes of the absorbability of the gases. Upon this supposition, solutions of gases in liquids are mixtures of a more volatile with a less volatile liquid; and to them may be extended the laws which hold in such mixtures.

Several circumstances are favourable to this view of the absorption of gases.

1. The cause assigned is one which we know to exist, and to be in operation. It is no suppositious cause of the existence of which we can adduce no other evidence, than its conveniency in explaining certain phenomena. We possess evidence that almost all the gases may be condensed into liquids. They are, therefore, necessarily under the influence of those causes which we have supposed to occasion gaseous absorbability. Thus: Mr. Faraday condensed sulphurous acid gas into a liquid, and found that its vapour possessed an elasticity which was balanced by the weight of about 2 atmospheres at 45° Fahrenheit. Here then is a liquid, which, from the frequency of the intermiscibility of liquids, might be expected to possess the property of mixing so intimately with certain of our reputed liquids, as to admit of being detained by them in considerable quantity at the ordinary pressure and temperature. And, accordingly, sulphurous acid is absorbed and detained in large proportions by sulphuric acid and by alcohol, and in a considerable measure by water.

2. It is a coincidence which appears more than accidental, that the gases which yielded to Mr. Faraday are, generally speaking, of easy absorbability. This will appear from the following table of the gases which were liquefied by that experimenter, of the pressure of their vapours in atmospheres, and of the amount of their absorption by water and alcohol at 60°,

according to the experiments of Thomson, Henry, Dalton, and Saussure.

Gases liquefied.	Pressure of vapours in atmospheres.	1 vol. water absorbs in vols. at 60°.	1 vol. alcohol absorbs in vols. at 60°
Ammoniacal	6·5 at 50°	780	—
Sulphurous acid	2 at 45	43·78	115·77
Muriatic acid	40 at 50	516	—
Cyanogen	3·7 at 45	4·5	23
Chlorine	4 at 60	2	—
Sulphuretted hydrogen	17 at 50	1	6·06
Carbonic acid	36 at 32	1	1·86
Nitrous oxide	50 at 45	1	1·53
Euchlorine	—	8	—

With the exception of fluosilicic and fluoboric gases, all the gases absorbed in considerable quantity by water, are contained in the foregoing table. While the other gases, such as oxygen, hydrogen, &c. which are condensed into liquids with great difficulty, are absorbed by water in very minute quantities indeed. This, however, is more than the theory requires.

3. Mr. Faraday was enabled to give approximations to the specific gravities of some of the liquids into which the gases were reduced. Now it would be an objection to the hypothesis, if there were an excessive discordance between the specific gravities obtained by Mr. Faraday, and the specific gravities which these liquids maintain in mixture, or when in solution with water, &c. For although the specific weight of a mixture of two liquids is rarely the mean of the weights of the liquids, yet in general the variation from the mean is not excessive. There exists, however, no such discordance. Indeed, a comparison of these specific weights, which I have made, remarkably confirms the theory.

In addition to these facts, this hypothesis has in its favour all those circumstances which are thought to recommend the chemical theory of the absorption of gases, so ably illustrated by Berthollet, Thomson, and Saussure. Indeed, the account here given may be considered as a development of that theory.

By the latent heat which becomes sensible in the condensation of vapours, and also by the heat which is frequently evolved in the mixing of liquids, that increase of temperature, which always marks the absorption of gaseous bodies, is explained. The same liquid absorbs different quantities of different gases, and different liquids absorb unequal quantities of the same gas, from the attraction between the absorbing liquids and the gases when liquefied being variable, as is the case among ordinary liquids. Diminution of pressure, or increase of temperature uniformly lessens the quantity of a gaseous body retained by a liquid, because the absorbed gas is itself then in a liquid state; and the volatility of all liquids, whether by themselves or mixed with others, is dependent upon pressure and temperature. The law, however, which Dr. Henry deduced from his experiments upon carbonic acid, viz. that the quantity of a gas which water absorbs is *directly* proportional to the pressure, is at variance with this theory. It is not likely that Dr. Henry would have come to the same conclusion, had he experimented upon the more absorbable gases. In the case of muriatic acid gas, for instance, it is unlikely that he would have succeeded in impregnating water with a double portion by doubling the pressure. There may, nevertheless, be an approximation to such a law, when the quantity of gas absorbed is inconsiderable, as it is in the case of carbonic acid gas; our knowledge of the laws by which a volatile is retained by a more fixed liquid, being too superficial to enable us at present to decide the point in question. The existence, however, of a general mechanical law of that description is incompatible with any chemical theory which can be given. Supposing such a law to hold, it is remarked by Dr. Thomson, that "the proportion of the ingredients in this case is entirely regulated by the bulk, whereas, in chemical combinations it is regulated by the weight." Dr. Thomson, notwithstanding this admission, attempts ingeniously to reconcile such a law to his modification of the chemical theory.*

The same objections are applicable to the analogous mechanical law, that the quantity of a gas absorbed, estimated by the bulk, is unaffected by variations in temperature. Such a law would be agreeable to the theory illustrated, if it were true that the pressure of vapours from liquids is exactly proportional to the temperature. But we know that the elasticity of vapours, over their liquids, increases in a much higher ratio than the temperature. Hence we are led to propose a different law, viz. that by increasing the temperature of a liquid, we diminish its capacity to absorb any gas, not in the same but in a much greater proportion.

Dr. Henry and Mr. Dalton have proved, that the amount of any gas, absorbed by a quantity of water in a vessel, depends greatly upon the gaseous residue. This fact is deducible from the supposition, that the gases are liquefied when absorbed. For all liquids continue to evaporate until they are pressed upon by an atmosphere of their own vapour, equal in elasticity to that which they are capable of evolving at the temperature of the experiment. In a solution of carbonic acid in water, we ought, therefore, to expect carbonic acid to be given out or to evaporate, till an atmosphere of that gas be formed of elas-

* System of Chem. vol. ii. p. 61.

ticity sufficient to counteract the tendency to assume the gaseous form of the remaining liquid carbonic acid. If the solution be freely exposed to the air, the whole of the carbonic acid will in a short time assume the gaseous form, from the impossibility of forming such an atmosphere. But if the solution be exposed to a limited quantity of any foreign gas, the carbonic acid will cease to evaporate, when the elasticity of the gaseous portion can counteract the volatility of the liquefied part. The greater the quantity of the foreign gas with which the solution is in free communication, the less carbonic acid will be detained, or would be taken up, were the absorption but commencing. Hence the influence of the gaseous residue, as it is called.

To the partial displacement of one gas absorbed by a liquid, by another gas, parallel cases may be adduced from the mixture of liquids. Thus, if alcohol holding a volatile oil in solution be poured into water, the greatest part of the oil separates, while the alcohol unites with the water.—The simultaneous absorption of several gases by a liquid belongs to this class of appearances. From Mr. Dalton's theory it follows that two gases absorbed into a liquid should really occupy always the same room as they would occupy, if each of them had been absorbed singly, at the degree of density which it has in the mixture. This law is inconsistent with the explanation given here; but it has been fully disproved by the subsequent experiments of Saussure.

It may be stated in conclusion, that all that is insisted upon in the foregoing sketch is, that when gases appear to be absorbed by liquids, they are simply reduced into that liquid inelastic form, which otherwise (by cold or pressure) they might be compelled to assume. That their detention in the absorbing liquid is owing to that mutual affinity between liquids, which is so common. An affinity which occasions the miscibility of liquids, affects the bulk or density of the mixture, and frequently impairs the volatility of the more easily vaporized liquid in the mixture. In this way, the phenomena of the absorption of gases are brought into the same class, as those of the miscibility of liquids.—(Scots Mechanic's Magazine.)

MISCELLANEOUS.

2. *Lieutenant Drummond's Station-Light.*

We are enabled to furnish some additional particulars to the account of this interesting and useful invention, given in the last number of the *Annals*, p. 451.

Among the applications of the Station-light suggested by the inventor, in his paper on the subject, is the adapting of it to the very important purpose of illuminating light-houses; which he recommends especially with regard to those light-houses first made by vessels approaching land. The chief distinction of the light emitted by the incandescent lime, when compared with that from the combustion of oil, is a *deficiency* of the *yellow rays;* whereas, compared with day-light, it has the same rays in *excess.*

3. *Butter in a Bog.*

A letter from the Viscount Dunlo, of which the following is an extract, was read at the meeting of the Royal Dublin Society, June 15th, 1826.

"In a bog upon an estate of Lord Clancarty's, adjoining Ballinasloe, has just been dug up a *tub of butter,* which, from the circumstance of the wood-work having been quite rotten, so as to fall off when touched, must be of great antiquity. It was this morning discovered by turf-cutters at the depth of eight feet from the surface of the bog. Upon probing it with a long knife some hard substance was found to resist, in consequence of which it was cut into two pieces. The resistance appears to have arisen from a great part of it having become hard and dry; about one half of it is in this state, the rest to all appearance fresh and good, and emitting no smell.

"The two parts have been put together again, and at present lie in Lord Clancarty's cellar at Garbally. The marks of the tub on them are quite distinct."

4. *Luminous Meteor.*

On the 2d of January, 1825, about 5 a. m. M. Antonio Brucalassi, on his return to Arezzo, observed, between S. Giovanni and Montevarchi, a singular electric phenomenon. About a hundred paces off, and at the height of about ten fathoms or less from the ground, appeared, on a sudden, a luminous meteor, of the form of a truncated cone. This meteor appeared to be formed of a globe of fire situated in its fore part, which was the narrower, and which, by its rapid motion, left behind a track of light, which gave it the appearance of a cone. This light became gradually less intense towards the base, and seemed to split into rays, issuing from the opposite extremity. The whole surface of the cone was illuminated, and cast out sparks of the greatest brilliancy, in brightness like the electric sparks, but in the effect resembling those exhibited by filings of iron, when thrown upon the flame of a candle. The whole length of the meteor appeared to be about two fathoms, and the diameter of the base half a fathom. At the centre of this base, there was a total absence of light, which formed in that part a dark spot; the direction of its motion was from west to east, and nearly horizontal, inclining, however, a little towards the earth. Its motion was very rapid; for in less than five seconds it traversed a space of about 350 paces. During this passage it shed a most brilliant light, so that a certain extent of land was illuminated, as in full day light. The emanations of this luminous body were lost in the air, instead of being extinguished in the ground; it left behind no smell, produced no explosion or noise of any kind, not even that hissing made by artificial fire works. The night in which this phenomenon occurred was calm, but very cold, and the sky clear. A great number of shooting stars were seen before and after the appearance of the meteor. *Antólogia*, Feb. 1825.—(Edin. Phil. Journ.)

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

The Hunterian Oration delivered last February at the Royal College of Surgeons, on the Natural History of the Oyster, and some of the principal points in its anatomy. By Sir A. Carlisle.

A Narrative of a Voyage in his Majesty's Ship Blonde, under the command of Capt. Lord Byron, undertaken for the purpose of conveying to the Sandwich Islands the bodies of the late King and Queen of those Islands; with the natural history of this interesting group of islands, &c. By R. B. Bloxam, MA. Chaplain of the Blonde.

JUST PUBLISHED.

Euclid's Elements of Geometry, translated from the edition of Peyrard; to which are added Algebraical Demonstrations to the Second and Fifth Books. By Geo. Phillips, of Queen's College, Cambridge. Part I. Book 1 to 6. 6*s*.

Practical Observations on the Convulsions of Infants. By John North, Surgeon Accoucheur.

Practical Observations in Surgery, more particularly as regards the Military and Naval Service. By Alexander Copland Hutchison. Second Edition, considerably enlarged. 8vo. 12*s*.

Delafons' Description of a new Patent Instrument for extracting Teeth, and a Patent Method of fixing Artificial Teeth. 5*s*.

A Treatise on Diet, with a view to establish some general principles for the prevention and cure of the diseases incident to a disordered state of the Digestive Functions. By A. J. Paris, MD. FRS. &c. 8vo.

Article XV.

NEW PATENTS.

L. Zachariah, jun. of Portsea, pawnbroker, for a combination of materials to be used as fuel.—May 8.

D. Dunn, King's-row, Pentonville, manufacturer of essence of coffee and spices, for improvements upon the screw press used in the pressing of paper, books, tobacco, or bale goods, and in the expressing of oil, extracts, or tinctures, and for various other purposes in which great pressure is required.—May 23.

T. Hughes, Newbury, Berks, miller, for improvements in the method of restoring foul or smutty wheat, and rendering the same fit for use. —May 23.

F. Molineux, Stoke Saint Mary, Somersetshire, for an improvement in machinery for spinning and twisting silk and wool, and for roving, spinning, and twisting flax, hemp, cotton, and other fibrous substances. —May 23.

T. P. Birt, Strand, coach-maker, for improvements in wheel carriages.—May 23.

J. Parker, Knightsbridge, iron and wire fence manufacturer, for improvements to park or other gates.—May 23.

D. P. Deurbroucq, Leicester-square, for an apparatus to cool wort, and also for the purpose of condensing the steam arising from stills during the process of distillation.—May 23.

W. H. Gibbs, Castle-court, Lawrence-lane, warehouseman, and A. Dixon, Huddersfield, manufacturer, for a new kind of piece goods formed by a combination of threads of two or more colours, the manner of combining and displaying such colours in such piece goods constituting the novelty thereof.—May 23.

J. Smith, Tiverton, Devonshire, lace manufacturer, for an improvement on the stocking frame.—May 23.

J. Loach, Birmingham, brass-founder, for a self-acting sash fastener, which fastening is applicable to other purposes.—May 23.

R. Slagg, Kilnhurst Forge, near Doncaster, steel manufacturer, for an improvement in the manufacture of springs chiefly applicable to carriages.—May 23.

L. J. Marie, Marquis de Combis, Leicester-square, for improvements in the construction of rotatory steam-engines, and the apparatus connected therewith.—May 23.

J. B. Fernandez, Norfolk-street, Strand, for improvements in the construction of blinds or shades for windows, or other purposes.— May 26.

R. Mickleham, Furnival's-inn, civil engineer and architect, for improvements in engines, moved by the pressure, elasticity, or expansion of steam gas or air, by which a great saving in fuel will be effected. —June 6.

H. R. Fanshaw, Addle-street, silk embossor, for an improved winding machine.—June 13.

J. Ham, Holton-street, Bristol, vinegar-maker, for an improved process for promoting the action of acetic acid on metallic bodies.— June 13.

Article XVI.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
May 23	29·88	29·84	29·860	66	54	60·0		NW	Clear.
24	29·74	29·70	29·720	64	48	56·0		N	Clear.
25	29·70	29·70	29·700	64	50	57·0		N	Light showers.
26	29·68	29·67	29·675	62	54	58·0		N	Clear.
27	29·64	29·62	29·630	64	52	58·0		E	Light showers.
28	29·70	29·70	29·700	64	52	58·0		NE	Light showers.
29	29·74	29·70	29·720	64	52	58·0		NW	Light showers.
30	29·78	29·78	29·780	64	54	59·0		N	Clear.
31	29·80	29·78	29·790	64	54	59·0	0·100	N	Clear.
June 1	29·80	29·80	29·800	64	50	57·0		N	Clear.
2	29·85	29·84	29·845	64	52	58·0		N	Clear.
3	30·02	30·98	30·000	63	52	57·5		NW	Clear.
4	30·08	30·08	30·080	65	54	59·5		NW	Fair.
5	30·20	30·20	30·200	66	52	59·0		NW	Clear.
6	30·15	30·14	30·145	70	53	61·5		NW	Clear.
7	30·14	30·14	30·140	70	56	63·0		NE	Fair.
8	30·10	30·02	30·060	66	55	60·5		NE	Cloudy; a shower.
9	29·88	29·80	29·840	70	55	62·5	0·510	NE	Thunder storm.
10	29·85	29·80	29·825	70	55	62·5		Var.	Clear.
11	29·96	29·95	29·955	72	56	64·0		N	Clear.
12	30·14	30·12	30·130	72	56	64·0		NW	Clear.
13	30·18	30·15	30·165	70	58	64·0		NW	Cloudy, clear.
14	30·12	30·12	30·120	69	58	63·5		W	Clear; cloudy.
15	30·18	30·16	30·170	62	56	59·0		NE	Clear.
16	30·18	30·16	30·170	67	58	62·5		NE	Clear.
17	30·22	30·22	30·220	68	52	60·0		NW	Clear.
18	30·20	30·20	30·200	70	56	63·0		NW	Clear.
19	30·22	30·22	30·220	70	58	64·0		NW	Clear.
20	30·22	30·22	30·220	70	57	63·5		Var.	Clear.
21	30·22	30·22	30·220	68	56	62·0		Var.	Clear.
22	30·18	30·14	30·160	68	56	52·0		Var.	Clear.
	30·22	29·62	29·980	72	48	61·0	0·610	NW	

RESULTS.

Barometer, mean height .... 29·980

Register Thermometer, ditto .... 61·0°

Rain, No. 1, 0·610, No. 2, 0·810.

Prevailing wind, NW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, June 24, 1826. EDWARD C. GIDDY.

Article XVII.

METEOROLOGICAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
5th Mon.							
May 1	N W	30·43	30·27	58	29	—	
2	N E	30·27	30·20	64	31	—	
3	N E	30·22	30·20	50	37	—	13
4	N W	30·22	30·20	50	38	—	—
5	N E	30·20	30·19	55	38	—	
6	N E	30·22	30·20	50	36	—	16
7	N E	30 22	30·21	54	38	—	
8	N E	30·22	30·22	62	28	—	
9	N E	30·22	30·22	66	36	—	
10	N W	30·24	30·18	65	36	—	
11	S E	30·39	30·24	68	37	·97	
12	N E	30·40	30·39	56	37	—	
13	N E	30·41	30·28	62	29	—	
14	N E	30·28	30·27	64	29	—	
15	N E	30·30	30 27	61	32	—	
16	S E	30·30	30·26	70	45	—	
17	N W	30·26	30·24	74	45	—	
18	S E	30·24	30·08	76	45	—	04
19	S W	30·08	29·92	75	50	—	10
20	E	30·18	29·92	68	37	·96	
21	N W	30·19	30·18	72	44	—	
22	N W	30·19	30·14	75	42	—	
23	N	30·14	30·01	70	48	—	
24	N W	30·01	29·89	69	50	—	68
25	N E	29·89	29·84	70	52	—	16
26	S E	29·96	29·84	68	48	—	03
27	E	30·04	29 96	71	45	—	
28	N	30·04	29·93	65	51	—	—
29	N E	30·04	29·93	55	50	—	1·34
30	N W	30·08	30·04	65	55	—	07
31	E	30·08	30 03	60	51	·92	06
		30·41	29·84	76	28	2·85	2·77

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

REMARKS.

Fifth Month.—1, 2. Fine. 3. Showery. 4. Cloudy. 5. Fine. 6. Hail showers during the day. 7, 8. Fine. 9. Fine: very distinct solar halo about one, p. m. of unusually large diameter. 10. Cloudy. 11—23. Fine. 24—26. Showery. 27, 28. Fine. 29. Very rainy day. 30. Drizzly. 31. Cloudy.

RESULTS.

Winds: N, 2; NE, 13; E, 3; SE, 4; SW, 1; NW, 8.

Barometer: Mean height

For the month.. 30·156 inches.

Thermometer: Mean height

For the month.. 52·532°

Evaporation.. 2·85 in.

Rain.. 2·77

Laboratory, Stratford, Sixth Month, 21, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

AUGUST, 1826.

Article I.

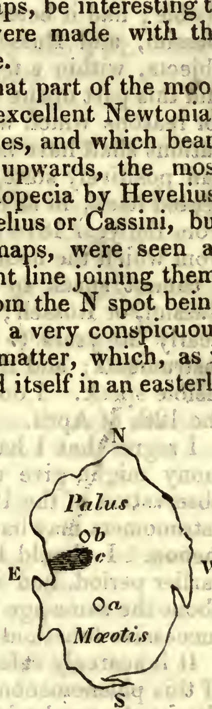
An Account of a curious Phænomenon observed in the Moon. By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Great Ouseburn, near Boroughbridge, July* 5, 1826.

The following communication will, perhaps, be interesting to some of your readers: the observations were made with the greatest care, and with a very fine telescope.

On the 12th April, 8^h, whilst observing that part of the moon called Palus Mæotis by Hevelius, with an excellent Newtonian reflector, which has an aperture of six inches, and which bears a beautifully distinct power of 800, and upwards, the most southerly of two spots in Mæotis, called Alopecia by Hevelius, and the most northerly, not noticed by Hevelius or Cassini, but which is included in Russell's beautiful maps, were seen as usual. Between, and very nearly in a right line joining them, but nearest to the N spot (the distance from the N spot being about one-fourth that from the S) appeared a very conspicuous spot, wholly enveloped in black nebulous matter, which, as if carried forward by a current of air, extended itself in an easterly direction, inclining a little towards the S, rather beyond the margin of Mæotis. The powers used were 70 and 130; the state of the air was such, that greater magnifying powers could not be employed with advantage.



April 13^d, 8^h to 9^h; the cloudy appearance was reduced, both in extent and intensity: the spot from which it seemed to issue, had become more distinctly visible; it resembles the small circular, or if near the limb, as in this instance, elliptical cavities, which are visible in almost every part of the moon.

April 17^{d}, 9^{h}. Scarcely a trace of the nebulous matter. Powers 70, 130, 200; all which were used in the last observation.

May 11, 12, 13, 15; June 9, 10. The three spots distinctly visible; but no trace of the nebulous matter; powers, 130, 200, 400, 800. Appearances the same on all the above-mentioned days, with an aerial refractor of 18 feet; powers 60 and 200. The general appearance is represented in outline in the figure, in which *a* represents Alopecia; *b* the N spot in Russell's maps, but not noticed by Hevelius or Cassini; *c* the spot which is the subject of this paper, surrounded with the nebulosity. The nebulous appearance here spoken of is not to be confounded with a darkish shade which is always to be seen near the same part of Mæotis, and which was distinctly visible at the same time: the nebulosity in question was very much blacker; it was so conspicuous as to strike the eye immediately. On the 12th April it was so intense that the spot *c* could not be very readily discerned. It was a more conspicuous object than any spot in Mæotis, and therefore it has not been permanently visible; had it been so, it is almost impossible that Hevelius, Cassini, and Russell, who have noticed far less conspicuous objects, within a few seconds of it, should all have omitted it; and this part of the moon being one which during several years, for reasons which will appear when a sufficient series of observations shall have been made, I have examined very minutely, I can positively state, that the nebulous appearance never presented itself, in any of the numerous observations I have made, from the year 1814 to the present time. Respecting the spot *c* from which the nebulosity seemed to issue which is now visible, I cannot speak so positively. It is not noticed by Hevelius, Cassini, or Russell; and it is well known that the latter devoted nearly 30 years to making his maps; that he used the best instruments; and that he has carefully delineated almost every visible speck. I do not recollect to have seen it previously to the 12th of April.

I regret that I had no scientific friend with me whose testimony might give additional weight to this account of the observation of the 12th April; however, I hope that some other astronomers may have been fortunate enough to see the phænomenon. I should have communicated the intelligence at an earlier period, had it not been requisite to observe the moon at about the same age afterwards: this has been done during two successive lunations.

It is scarcely safe to hazard a conjecture respecting the cause of this phænomenon: I shall, therefore, merely propose a query. Was it the smoke of a volcano? or was it cloudy matter? The former part of the query might have been decided by observation about three or four days after opposition; but the air was uniformly cloudy, and the moon quite invisible.

The moon presents the same general aspect which it did to the first telescopic observers; yet from my own observations I am convinced that if a number of astronomers would take separate and small portions of the lunar disc, and observe the same on every clear evening with large instruments, and make use of high powers, as 500 or 600, that changes would be observed; from such observations, important conclusions may be derived, some of which are pointed out by Hevelius in his Selenographia.

P.S. June 10^{d}, 8^{h}; there remains a little blackness about the spot *c*; it is rather faint, of small extent, and nearly uniformly diffused. J. B. EMMETT.

ARTICLE II.

A New Catalogue of the Fall of Stones, Iron, Dust, and soft Substances, dry or moist, in Chronological Order. By M. E. F. F. Chladni.*

IN this corrected and complete catalogue which M. Chladni has sent me, the sign ? indicates those falls which this able naturalist does not consider as perfectly verified.—(Ar.)

Falls of Stones or Iron before the Commencement of the present Era.

? 1478 years before our era in Crete; the thunder-stone of which Malchus speaks, and which was probably believed to be a symbol of Cybele. *Chronicle of Paros*, l. 18 and 19.

(The shower of stones mentioned by Joshua was probably nothing but hail.)

1200.—Stones preserved at Orchomenos. *Pausanias.*

? 1168.—A mass of iron on Mount Ida, in Crete. *Chronicle of Paros*, l. 22.

? 705 or 704.—The Ancyle, probably a mass of iron, nearly of the same form as that of the Cape and of Agram. *Plutarch.*

654.—Stones on Mount Albanus. *Liv.* i. 30.

644.—In China. *De Guignes.*

465.—At Ægospotamus. *Plutarch, Pliny,* and others. A stone near Thebes. *Scholiast on Pindar.*

211.—In China. *De Guignes,* and *General History of China.*

205 or 206.—Ignited stones. *Plutarch, Fab. Max.* c. 2.

* From the Annales de Chimie.

192.—In China. *De Guignes.*
176.—A stone in the lake of Mars. *Liv.* xli. 3.
90 or 89.—A shower of bricks. *Pliny* and *Jul. Obs.*
89.—In China. *De Guignes.*
56 or 52.—Spongy iron, in Lucania. *Pliny.*
? 46.—Stones at Acilla. *Cesar.*
38, 29, 22, 19, 12, 9, 6.—Falls of stones in China. *De Guignes.*

Stones fallen at undetermined Epochs.

The mother of the gods fell at Pessinus.

The Elagabalus at Emisa, in Syria.

The stone preserved at Abydos, and that of Cassandria. *Pliny.*

? The black stone and another preserved in the caaba or temple at Mecca.

(The stone preserved in the coronation chair of the kings of England is not, as has been believed, a meteoric stone.)

Falls of Stones or Iron after the Commencement of our Era.

In the years 2, 106, 154, 310, and 333, some stones fell in China. *Abel Rémusat, Journ. de Phys.* May, 1819.

(The stone which it was pretended fell from heaven in 416, at Constantinople, of which Sethus Calvisius makes mention in his Op. Chronolog. was only a stone from Constantine's great column, which, by its fall, injured the pedestal.)

....A stone in the country of the Vocontini. *Pliny.*
452.—Three large stones in Thrace. Cedrenus and Marcellinus.
6th century.—Stones on Mount Libanus, and near Emisa in Syria. *Damascius.*
? 570 (or thereabouts.)—Stones near Bender, in Arabia. *Koran*, 8, 16 ; cv. 3 and 4, and the Commentators.
616.—Stones in China. *Abel Rémusat.*
648.—An ignited stone at Constantinople.—*Abel Rémusat.*
839.—Stones in Japan. *Abel Rémusat.*
852, in July, or August.—A stone at Tabaristan. *De Sacy* and *Quatremére.*
856, in December.—Five stones in Egypt. *The same.*
885.—Stones in Japan. *Abel Rémusat.*
897.—At Ahmed-Dad. *Quatremére;* according to the *Chron. Syr.* in 892.
921.—Large stones, at Narni. *Manuscript Chronicle* of the monk *Benedictus de Saint-Andrea,* which is in the library of the Prince Chigi, at Rome.
951.—A stone at Augsbourg. *Alb. Stad.* and others.
998.—Stones at Magdebourg. *Cosmas* and *Spangenberg.*

1009, or soon after.—A mass of iron in the Djorjan. *Avicennes.* (They have murdered the name in *Lurgea* and *Cordova.*)
1021, between the 24th of July and the 21st of Aug.—Stones in Africa. *De Sacy.*
1057.—A stone in Corea. *Abel Rémusat.*
1112.—Stones or iron, near Aquileja. *Valvasor.*
1135, or 1136.—A stone at Oldisleben. *Spangenberg* and others.
1164, at the feast of Pentecost.—Iron in Misnia. *Geog. Fabricius.*
1249, 26th July.—Stones at Quedlinburg, &c. *Spangenberg* and *Rivander.*
? 13th century.—A stone at Wurzburg. *Schott. Phys. Cur.*
Between 1251 and 1363.—Stones at Welikoi-Ustiug, in Russia. *Gilbert's Ann.* vol. 35.
? 1280.—A stone at Alexandria, in Egypt. *De Sacy.*
1300, or thereabouts.—Large stones in Arragon, according to a *manuscript Chronicle* preserved in the National Museum of Pest, in Hungary, being the continuation to that of *Martinus Polonus.*
1304, 1st October.—Stones at Friedland or Friedberg. *Kranz* and *Spangenberg.*
1328, 9th Jan.—In the Mortahiah and Dakhaliah. *Quatremére.*
? 1368.—In the country of Oldenburg, a mass of iron. *Siebrand Meyer.*
1379, 26th May.—At Minde, in Hanover. *Lerbecius.*
1421.—A stone in the island of Java. *Sir Thomas Stamford Raffles,* vol. ii. p. 137.
? 1438.—Spongy stones at Roa. *Proust.*
? . . . . —A stone near Lucern. *Cysat.*
1474.—Near Viterbo, two large stones. *Biblioteca Italiana,* vol. xix. (Sept. 1820) p. 461.
1491, 22d March.—A stone near Crema. *Simoneta.*
1492, 7th Nov.—At Ensisheim.
1496, 26th or 28th Jan.—Stones at Cesena, &c. *Buriel* and *Sabellicus.*
1511, near the middle of September.—A great fall of stones at Crema. *Giovani del Prato,* and others.
1516.—In China, two stones. *Abel Rémusat.*
1520, in May.—Stones in Arragon. *Diego de Sayas.*
? 1528.—Large stones at Augsburg. *Dresseri Chron. Saxon.*
? 1540, 28th April.—A stone in the Limousin. *Bonav. de Saint-Amable.*
1540 to 1550.—A mass of iron in the forest of Nannhof. *Albinus Meisniche Bergchronik* (that is to say, Chronicle of the Mines of Misnia).
. . . . —Iron in Piedmont. *Mercati* and *Scaliger.*
1552, 19th May.—Stones in Thuringia. *Spangenberg.*

1559.—Stones at Miskoltz, in Hungary. *Isthuanfi*, in his *Hist. Hung.*

1561, 17th May.—At Torgo and Eilenbourg (described by *Arcem Juliam*). *Gesner* and *De Boot.*

1580, 27th May.—Stones near Gottingen. *Bange.*

1581, 26th July.—A stone in Thuringia. *Binhard Olearius.*

1583, 9th Jan.—At Castrovillari. *Costo, Mercati*, and *Imperati.*

1583, 2d March. In Piedmont. *Mercati.*

1596, 1st March. Stones at Crevalcora. *Miturelli.*

.... In the same century.—A stone in the kingdom of Valencia. *Cæsius*, and the *Jesuits of Coimbra.*

1618, in August.—A great fall of stones in Stiria. *Fundgruben der Orients.* (Mines of the East, by M. de Hammer.)

1618.—A metallic mass in Bohemia. *Kronland.*

1621, 17th April.—A mass of iron near Lahore. *Jehan Guir.*

1622, 10th Jan.—A stone in Devonshire. *Rumph.*

1628, 9th April.—Near Hatford, in Berkshire. *Gentlem. Mag.*

1634, 27th Oct.—Stones in Charollais.—*Morinus.*

? 1635, 7th July.—A stone at Calce. *Valisnieri.*

1636, 6th March.—In Silesia. *Lucas* and *Cluverius.*

1637 (not 1627), 29th Nov.—In Provence. *Gassendi.*

1642, 4th Aug.—In Suffolk. *Gentleman's Magazine.*

? 1643 or 1644.—Stones in the sea. *Wurfbain.*

1647, 18th Feb.—A stone near Zwickau. *Schmid.*

1647, in Aug.—Stones in Westphalia. *Gilbert's Ann.*

Between 1647 and 1654.—A mass in the sea. *Willman.*

1650, 6th Aug.—A stone at Dordrecht. *Senguerd.*

1654, 30th March.—Stones in the island of Funen. *Bartholinus.*

....—At Varsovia, a large stone. *Petr. Borellus.*

....—At Milan, a small stone, that killed a Franciscan. *Museum Septalianum.*

(The account of stones fallen in 1667 at Schiras appears fabulous.)

1668, 19th or 21st June.—A great fall of stones at Verona. *Valisneri, Montanari, Pr. Carli.*

1671, 27th Feb.—Stones in Swabia. *Gilbert's Annals*, vol. 33.

1674, 6th Oct.—Stones near Glaris. *Scheuchzer.*

? Between 1675 and 1677.—Stones near Copinsha. *Wallace* and *Gent. Mag.* July, 1806.

1677, 28th May.—Stones at Ermendorf, which probably contained copper. *Misc. Nat. cur.* 1677, *app.*

1680, 18th May.—Stones at London. *King.*

1697, 13th Jan. near Sienna. *Soldani*, according to *Gabrieli.*

1698, 19th May.—A stone at Waltring. *Scheuchzer.*

1706, 7th June.—A stone at Larissa. *Paul Lucas.*

1715, 11th April.—Some stones not far from Stargard, in Pomerania. *Gilbert's Ann.* vol. 71, p. 215.

1722, 5th June.—Stones near Scheftlar, in Freisinge. *Meichelbeck.*

1723, 22d June.—At Plescowitz. *Rost* and *Stepling.* (The pretended fall of metal in 1731 at Lessay was only an electrical phosphorescence of drops of rain, for Dom Stalley does not say that there fell drops of ignited and melted metal, but there fell, as it were, drops, &c.)

1727, 22d July.—A fall near Liboschitz, in Bohemia. *Stepling.*

1738, 18th Aug.—Near Carpentras. *Castillon.*

1740, 25th Oct.—Stones at Rasgrad. *Gilbert's Annals,* vol. 50.

1740 and 1741, in winter.—A large stone in Greenland. *Egede.*

? 1743.—A stone at Liboschitz. *Stepling.* (Possibly the same remarked in the year 1723.)

1750, 1st Oct.—Stones near Coutances. *Huard* and *Lalande.*

1751, 26th May.—Iron at Hradschina, near Agram.

1753, 3d July.—Stones at Tabor. *Stepling* and *Mayer.*

1753, in September.—At Laponas. *Lalande* and *Richard.*

1755, in July.—A stone in Calabria. *Domin. Tata.*

1766, in July.—At Alboreto. *Troili.*

? 1766, 15th Aug.—At Novellara. *Troili.* (Perhaps a stone melted by lightning.)

1768, 13th Sept.—A stone at Luce. *Mem. de l'Ac.*

. . . . —A stone at Aire. *Mem. de l'Ac.*

1768, 20th Nov.—A stone at Maurkirchen. *Imhof.*

1773, 17th Nov.—A stone at Sena, in Arragon. *Proust.*

1775, 19th Sept.—Near Rodach, in Cobourg. *Gilbert's Ann.* vol. 23.

1775 or 1776.—Stones at Obruteza, in Volhynia.—*Gilbert's Annals,* vol. 31.

1776 or 1777, in Jan. or Feb.—Near Fabbriano. *Soldani* and *Amoretti.*

1779.—Stones at Pettiswood, in Ireland. *Gent. Mag.*

1780, 1st April.—Near Beeston in England. *Lloyd's Evening Post.*

1780, or thereabouts.—Masses of iron in the territory of Kinsdale, and between West River Mountain and Connecticut. *Quarterly Review,* No. 59, April, 1824.

1782.—A stone near Turin. *Tata* and *Amoretti.*

1785, 19th Feb.—Stones at Eichstaedt. *Pickel* and *Stutz.*

1787, 1st Oct.—In the province of Charkow, in Russia. *Gilbert's Ann.* vol. 31.

1790, 24th July.—A great fall at Barbotan, &c.

1791, 17th May.—Stones at Castel-Beradenga. *Soldani.*

1791,* 20th Oct.—At Menabilly, in Cornwall. *King.*

* The late Philip Rashleigh, Esq. of Menabilly, showed me several years ago some models of pieces of ice, inclosing hail, which fell in his neighbourhood; and his brother, my friend the Rev. Peter Rashleigh, of Southfleet, lately showed me another, with a ticket attached, stating the storm to have occurred on the 20th Oct. 1791, the day and year mentioned in the text. The latter gentleman also informs me that he never heard

1794, 16th June.—In the environs of Sienna.
1795, 13th April.—At Ceylon.—*Le Beck.*
1795, 13th Dec.—A stone, in Yorkshire.
1796, 4th Jan.—Near Belaja Zerkwa, in Russia. *Gilbert's Annals,* vol. 35.
1796, 19th Feb.—In Portugal. *Southey.*
1798, 8th or 12th of March.—At Sales. *De Drée,* &c.
1798, 19th Dec.—Stones in Bengal. *Howard, Valentia.*
1801.—On the island of Tonneliers.—*Bory de Saint-Vincent.*
1802, in September.—Stones in Scotland. *Monthly Magazine,* Oct. 1802.
1803, 26th April.—Stones in the environs of Aigle.
1803, 4th July.—At East Norton. *Phil. Mag.* and *Bibl. Brit.*
1803, 8th Oct.—A stone near Apt.
1803, 13th Dec.—Near Eggenfelde. *Imhof.*
1804, 5th April.—Near Glasgow. *Phil. Mag.* and *Bibl. Brit.*
From 1804 to 1807.—At Dordrecht. *Van Beck Calkoen.*
1805, 25th March.—Stones at Doroninsk, in Siberia. *Gilbert's Annals,* vols. 29 and 31.
1805, in June.—Stones at Constantinople. *Kougas-Ingigian.*
1806, 13th March.—At Alais.
1806, 17th May.—A stone in Hampshire. *Monthly Mag.*
1807, 13th March.—Near Timochin, in Russia. *Gilbert's Annals.*
1807, 14th Dec.—Stones near Weston, in Connecticut.
1808, 19th April.—At Borgo San-Donino. *Guidotti* and *Spagnoni.*
1808, 22d May.—Near Stannern, in Moravia.
1808, 3d Sept.—At Lissa, in Bohemia. *De Schreibers.*
? 1809, 17th June.—In the sea, near North America. *Medical Reposit.* and *Bibl. Brit.*
1810, 30th Jan.—In Caswell, in America. *Phil. Mag.* and *Medical Reposit.*
1810, in July.—A large stone at Shahabad, in India. The meteor caused great havock. *Phil. Mag.* vol. 37.
1810, in August.—A stone in the county of Tipperary, in Ireland. William Higgins has published its analysis.
1810, 23d Nov.—Stones in Charsonville, near Orleans.
1811, 12th, 13th March.—A stone in the province of Pultowa, in Russia. *Gilbert's Ann.* vol. 38.
1811, 8th July.—Stones at Berlanguillas.
1812, 10th April.—Near Toulouse.
1812, 15th April.—A stone at Erxleben. *Gilbert's Annals,* vol. 40 and 41.
1812, 5th Aug.—At Chantonay. *Brochant.*

that any meteoric stone, or other substance, besides the ice, fell at that time in the neighbourhood of Menabilly.—This singular phenomenon appears, therefore, to have been mistaken for a fall of meteoric stones or iron.—J. G. C.

1813, 14th March.—Stones at Cutro, in Calabria, during the fall of a great quantity of red dust. *Bibl. Brit.* October, 1813.

? 1813, in summer.—Many stones near Malpas, not far from Chester. *Annals of Philosophy*, Nov. 1813. (This account does not appear worthy of entire confidence, because it is anonymous, and above all this event is not elsewhere noticed.)

1813, 10th September.—Stones in Limerick, in Ireland. *Phil. Mag.* and *Gent. Mag.*

1813, 13th December.—According to Nordenskiold. *Ann. de Chim.* vol. 25, p. 78.

1814, in March.—From an account communicated to the Academy of Petersburgh. Stones in the neighbourhood of Loutolax and Sawitaipal, not far from Wiborg, in Finland. These stones do not contain nickel.

.... (Mr. Murray, in the *Phil. Mag.* July, 1819, p. 39, speaks of a stone fallen at Pulrose in the Isle of Man, without fixing the date; he says the event is certain, and that the stone was very light, and resembled a slag: it therefore must resemble the stones that fell in Spain, in 1438.)

1814, 3d Feb.—A stone near Bacharut, in Russia. *Gilbert's Annals*, vol. 50.

1814, 5th Sept.—A stone near Agen.

1814, 5th Nov.—In Doab, in India. *Phil. Mag.; Bibl. Brit.; Journal of Sciences.*

1815, 18th Feb.—A Stone, at Duralla, in India. *Phil. Mag.* Aug. 1820, p. 156.

1815, 3d Oct.—At Chassigny, near Langres. Pistollet. *Ann. de Chim.*

1816.—A stone at Glastonbury, in Somersetshire. *Phil. Mag.*

? 1817, between the 2d and 3d May.—There were probably some masses fell in the Baltic sea: after the appearance of a large meteor at Gothenburg, a shower of fire was seen at Odensee, descending very rapidly towards the south-east. *Danish Journals.*

? 1818, 15th Feb.—A large stone appeared to have fallen at Limoges, in a garden to the south of the town. After the explosion of a large meteor, a mass which fell made a hole in the earth equal in size to a large cask. *Gazette de France*, and *Journal de Commerce*, 25th Feb. 1818.

(The mass should have been dug up, and it would even still be desirable to do so.)

1818, 30th March.—A stone near Zaborzica, in Volhynia. (Analysed by M. Laugier.) *Ann. de Museum*, 17 *ann.* Second Number.

1818, 10th Aug.—A stone at Slobotka, in the province of Smolensk, in Russia, according to several journals.
1819, 13th June.—At Jonzac, Department of Lower Charente. These stones do not contain nickel.
1819, 13th Oct.—Stones near Politz, not far from Gera, or Kostritz, in the Principality of Reuss. *Gilbert's Ann.* vol. 63.
? 1820, between the 21st and 22d March.—In the night at Vedenburg, in Hungary. *Hesperus*, vol. 27, cal. 3.
1820, 12th July.—Stones near Likna in the circle of Dunaborg, province of Witepsk, in Russia. *Theodore Grotthus. Gilbert's Ann.* vol. 67.
1821, 15th June.—Stones near Juvenas. They do not contain nickel.
1822, 3d June.—At Angers. *Ann. de Chimie.*
1822, 10th Sept.—Near Carlstadt, in Sweden.
1822, 13th Sept.—Near Baffe, Canton of Epinal, Departement des Vosges. *Ann. de Chim.*
1823, 7th Aug.—Near Nobleborough, in America. *Silliman's American Jour.* vol. 7.
1824, near the end of January.—Many stones near Arnazzo, in the territory of Bologna. One of them weighing 12 pounds is preserved in the observatory of Bologna. *Diario di Roma.*
1824, the beginning of Feb.—A large stone in the province of Irkutsk, in Siberia. Several journals.
1824, 14th Oct.—Near Lebrak, circle of Beraun, in Bohemia. The stone is preserved in the National Museum of Prague.

Masses of Iron probably of Meteoric Origin.

The masses of iron, probably meteoric, are distinguished by the presence of nickel, by their texture, by their malleability and their insulated position. Some of these masses are spongy or cellular; the cavities are filled with a stony substance, similar to peridot. Amongst these, we must place,

The mass found by Pallas, in Siberia, the meteoric origin of which was known to the Tartars.

? A piece found between Eibenstock and Johanngeorgenstadt.

A mass preserved in the Imperial Cabinet of Vienna, coming probably from Norway.

A small mass, weighing four pounds, now at Gotha. Other masses are solid; in which case the iron consists of rhomboids or octahedrons, composed of layers, or parallel folia.

The only fall known of masses of this kind is that at Agram, in 1751. Some others similar have been found:—

On the right bank of the Senegal river. *Compagnon, Forster, Golberry.*

At the Cape of Good Hope. *Van Marum* and *De Dankelmann.*

At Mexico, in different places. *Sonneschmidt, de Humboldt*; see also the *Gazette of Mexico,* vols. 1 and 5.

At Brazil, in the province of Bahia. *Wollaston* and *Mornay.*

In the jurisdiction of Saint Jago del Estero. *Rubin de Celis.*

At Elbogen, in Bohemia. *Gilbert's Ann.* vols. 42 and 44.

Near Lenarto, in Hungary. *Gilbert's Ann.* vol. 49.

Near the Red River. The mass was sent from New Orleans to New York. *American Mineralogical Journal,* vol. 1. Col. Gibbs has analysed it, and found it contained nickel.

(There are other similar masses in the same country, according to *The Minerva* of New York, 1824.)

In the environs of Bitbourg, not far from Treves. (This mass weighs 3300 pounds; it contains nickel. The analysis made by Col. Gibbs is in the *American Mineralogical Journal,* vol. 1.)

Near Brahin, in Poland. (These masses, according to the analysis of M. Laugier, contain nickel and a little cobalt.) In the republic of Colombia, on the eastern Cordillera of the Andes. *Boussingault* and *Mariano de Rivero, Ann. de Chim.* vol. 25.

At some distance from the northern coast of Baffin's Bay, in a place called Sowiallik. There are two masses; one appears solid, the other is stony, and mixed with pieces of iron, of which the esquimaux make a sort of knife. *Capt. Ross.*

? Perhaps we must place in this class a large mass nearly 40 feet high, found in the eastern part of Asia, not far from the source of the Yellow River, and of which the Moguls, who call it *Khadasutsilao,* that is to say, polar rock, say, it fell after a fiery meteor. *Abel Rémusat.* There exist masses of problematical origin. Of this number are:—

A mass at Aix-la-Chapelle, which contains arsenic. *Gilbert's Ann.* vol. 48.

A mass found in the Milanais. *Gilbert's Ann.* vol. 50.

The mass found at Groskamsdorf, containing, according to Klaproth, a little lead and copper.

(It appears that they fused it, and that the pieces preserved at Freyberg and Dresden are only fused steel, substituted for the fragments of the primitive mass.)

Falls of Dust and soft Substances, dry or moist.

All that has been observed in these falls makes us presume they do not differ materially from falls of stones. Sometimes they have been accompanied by falls of stones, as well as by fiery meteors. The dust appears to contain nearly the same substances as the meteoric stones. There appears no other difference but in the rapidity with which these heaps of chaotic matter dispersed in the universe arrive in our atmosphere; but, from that time, these substances must undergo, more or less, great changes, according to the intensity of the heat which the compression develops in the air. In the red and black dust, the oxide of iron is probably the principal colouring matter. In the black dust, there is no doubt carbon will also be found. I consider the black and very friable stones which fell at Alais in 1806, as affording the passage from the black dust to common meteorites, the heat not having been sufficient to burn the carbon of these stones and fuse the other substances.

In the year 472 of our era (according to the chronology of Calvisius, Playfair, &c.) the 5th or 6th of November, a great fall of black dust (probably in the environs of Constantinople); the sky seemed on fire. Procopius and Marcellinu attributed it to Vesuvius. *Menæa, Menolog. Græc.; Zonaras, Cedrenus, phanes.*

652.—At Constantinople, a shower of red dust. *Theophanes, Cedrenus, Matthew Eretz.*

743.—A meteor, and dust in different places. *Theophanes.*

. . .—In the middle of the ninth century. Red dust, and matter resembling coagulated blood. *Continuat. du Georg. Monachus, Kazwini, Elmazen.*

869.—Red rain in the neighbourhood of Brixen during three days. *Hadrianus Burlandus.* (Probably this phenomenon may be the same as the preceding.)

929.—At Bagdad. Redness of the sky and a fall of red sand. *Quatremere.*

1056.—In Armenia, red snow. *Matth. Eretz.*

1110.—In Armenia, in the province of Vaspouragan, in winter, in a dark night, fall of an inflamed body in the lake Van. The water became the colour of blood, and the earth was split in different places. *Matth. Eretz.*

1222 or 1219.—Red rain in the environs of Viterbo. *Bibl. Italiana*, vol. 19.

1416.—Red rain in Bohemia. *Spangenberg.*

? . . .—In the same century at Lucern, fall of a stone and of a mass resembling coagulated blood, with the appearance of a fiery dragon (or meteor). *Cysat.*

1501.—Rain of blood in different places, according to some chronicles.

1543.—Red rain, in Westphalia. *Suni Commentarii.*

1548, 6th Nov. (probably in Thuringia).—Fall of a ball of fire, with much noise. A reddish substance was afterwards found on the ground, resembling coagulated blood. *Spangenberg.*

1557.—In Pomerania. Large plates, of a substance resembling coagulated blood. *Mart. Zeiler*, vol. ii. epist. 386.

1560, Whitsunday.—Red rain at Emden and Louvain, &c. *Fromond.*

1560, 24th Dec.—At Lillebonne, a fiery meteor and red rain. *Natalis Comes.*

?1582, 5th July.—At Rockhausen, not far from Erfort, fall of a large quantity of a fibrous substance resembling human hair, at the end of a dreadful tempest, analogous to those produced by earthquakes. *Michel Bapst.*

1586, 3d Dec.—At Verden (in Hanover), fall of much red and blackish matter, with lightning and thunder (fiery meteor and detonation). This matter burnt the planks on which it fell. *Manuscrit de Salomon,* Senator at Bremen.

1591.—At Orleans, at the Magdalen, rain of blood. *Lemaire* (Ln.)

1618, in Aug.—Fall of stones, a fiery meteor, and rain of blood in Stiria. *De Hammer.*

1623, 12th Aug.—At Strasburg. Red rain. *Elias Habrecht,* in a memoir printed at Strasburg in 1623.

1637, 6th Dec.—Fall of much black dust in the Gulf of Volo, and in Syria. *Phil. Trans.* vol. i. p. 377.

1638.—Red rain at Tournay.

1643, Jan.—Rain of blood at Vachingen and at Weinsberg, according to a *Manuscript Chronicle, in the town of Heilbronn.*

1645, 23d or 24th Jan.—At Bois le Duc.

1640, 6th Oct.—Red rain at Brussels. *Kronland* and *Wendelinus.*

1652, May.—A viscous mass, after a luminous meteor, between Sienna and Rome. *Miscell. Acad. Nat. Curios.;* ann. 9, 1690.

?1665, 23d March.—Near Laucha, not far from Naumburg, there fell a fibrous substance, like blue silk, in large quantities. *Joh. Praetorius.*

1678, 19th March.—Red snow near Genoa. *Philos. Transac.* 1678.

1686, 31st January, near Rauden, in Courland, and at the same time in Norway and Pomerania.—A large quantity of a membranous substance, friable and blackish, resembling half burnt paper. *Miscell. Ac. Nat. cur. ann.* 7, *pro. ann.* 1688 *in append.* (M. the Baron Theodore de Grotthus has analysed a portion of this substance which had been preserved in a cabinet of natural history, and found in it silica, iron, lime, carbon, magnesia, a trace of chromium and sulphur, but no nickel.)

1689.—Red dust at Venice, &c. *Valisnieri.*

1711, 5th and 6th May.—Rain at Orsion, in Sweden. *Act. Lit. Sueciæ,* 1731.

1718, 24th March.—Fall of a globe of fire, in the island of

Lethy, in India. A gelatinous matter was found afterwards. *Barchewitz.*

1719.—Fall of sand in the Atlantic Sea (lat. 45° N. long. 322° 45′), accompanied by a luminous meteor. *Mem. de l'Acad. des Sciences*, 1719, *Hist.* p. 23. This sand deserves to be examined with more attention.

1721, towards the middle of March, Stutgard.—A meteor and red rain in great quantity, according to a note written the 21st March by the Counsellor. *Vischer.*

1737, 21st May.—A fall of earth attractable by the magnet, on the Adriatic Sea, between Monopoli and Lissa. *Zanichelli*, in the *Opuscoli di Calogera*, vol. 16.

1744.—Red rain at St. Pierre d'Arena, near Genoa. *Richard.*

1755, 20th Oct.—On the island of Jetland, one of the Orkneys. Black dust that did not come from Hecla. *Philos. Transac.* vol. 50.

1755, 13th Nov.—Redness of the sky and red rain in different countries. *Nov. Act. Nat. Cur.* vol. 2.

1763, 9th Oct.—Red rain at Cleves, at Utrecht, &c. *Mercurio Historico y Politico de Madrid*, Oct. 1764.

1765, 14th Nov.—Red rain in Picardy. *Richard.*

1781, in Sicily.—White dust not volcanic. *Gioeni, Phil. Trans.* vol. 72.

1792, 27th, 28th, and 29th August without interruption.—A shower of a substance resembling cinders in the town of Paz, in Peru. This phenomenon could not be attributed to a volcano. Explosions were heard, and the sky was observed to be illuminated. The dust occasioned great disorders in the head, and produced fever in several persons. *Mercurio Peruano*, vol. 6, 1792.

1796, 8th March.—In Lusatia, after the fall of a ball of fire, a viscous matter was found.* *Gilbert's Annals*, vol. 55.

1803, 5th and 6th March, in Italy.—A fall of red dust, dry in some places; in others moist. *Opuscoli Scelti*, vol. 22.

1811, in July, near Heidelberg.—Fall of a gelatinous substance, after the explosion of a luminous meteor. *Gilbert's Annals*, vol. 66.

1813, 13th and 14th March.—In Calabria, Tuscany, and Friuli. A great fall of red dust and red snow, with much noise. There fell at the same time some stones at Cutro, in Calabria. *Bibl. Brit.* Oct. 1813, and April, 1814.

(Sementini found in the dust; silica, 33; alumina, 15½; lime, 11¼; iron, 14½; chrome, 1; carbon,

* I have a small portion which has the consistence, colour, and smell, of very dry brownish varnish. I believe it to be composed principally of sulphur and carbon. MM. Guyton—Morveau, and Blumenbach, also had portions of it.

9; loss 15. It appears that Sementini did not look for magnesia or nickel.

1814, 3d and 4th July.—A great fall of black dust in Canada, with an appearance of fire. The event was similar to that of 472. *Phil. Mag.* vol. 44.

1814, in the night of the 27th and 28th Oct.—In the valley of Oneglia, near Genoa, red rain. *Giornale di Fisica,* &c. vol. 1, p. 32.

1814, 5th Nov.—It was found in the Doab, in India, that every stone which fell was surrounded by a little heap of dust. *Phil. Mag.*

1815, towards the end of September, the sea to the south of India was covered with dust to a very great extent, probably after a similar fall. *Phil. Mag.* July, 1816.

1816, 15th April.—Red snow in different places, in the northern parts of Italy. *Giornale di Fisica,* &c. vol. 1, 1818, p. 473.

1819, 13th Aug.—At Amherst, in Massachusets. After a luminous meteor, there fell an offensive gelatinous mass. *Silliman's Journal,* 2, 335.

1819, 5th Sept.—At Studein, in Moravia, in the jurisdiction of Teltsch, between eleven and twelve o'clock at noon, the sky being serene and tranquil, a shower of small pieces of earth, proceeding from a small insulated transparent cloud. *Hesperus,* Nov. 1819, and *Gilbert's Annals,* vol. 68.

1819, 5th Nov.—Red rain in Flanders, and in Holland. *Ann. Génér. des Sciences Physiques.* (Cobalt and muriatic acid were found in this rain.)

1819, in Nov.—At Montreal, and in the northern part of the United States. Black rain and snow, accompanied by an extraordinary darkness of the sky, concussions like an earthquake, detonations resembling discharges of artillery, and igneous flashes which were taken for intense lightning. *Ann. de Chim.* vol. 15. Some persons attributed the phenomenon to the conflagration of a forest, but the noise, the concussions, &c. prove that it was really a meteor similar to those of 472, 1637, 1762, and 1814 (in Canada). It appears that the black and friable stones which fell at Alais, in 1806, were nearly the same substance in a state of greater aggregation.

1821, 3d May, at nine o'clock in the morning.—Red rain in the environs of Giessen. M. Professor Zimmerman having analyzed the reddish brown sediment left by this rain, found in it chrome, oxide of iron, silica, lime, carbon, a trace of magnesia, and some volatile particles; but no nickel.

1824, 13th Aug.—City of Mendoza, in the republic of Buenos Ayres. Dust that fell from a black cloud. At a distance of 40 leagues, the same cloud discharged itself again. *Gazette de Buenos Ayres*, Nov. 1, 1824.

M. Chladni has extracted from the work of Ma-Touan-Lin, a Chinese author of the 13th century, lately translated by M. Abel Remusat, merely the dates of those aërolites, fragments of which could be collected. If we suppose all globes of fire which are accompanied with loud explosions before they disappear, to be true aërolites; Ma-Touan-Lin would supply 96 additional instances. M. Remusat shows that the Chinese and Japanese observed with much accuracy every thing connected with the appearance of these singular phenomena. They remarked that stones sometimes fall in perfectly calm weather; they compared their detonations to thunder, to the noise of a falling wall, and to the lowing of oxen; and the hissing noise which accompanies their fall to the rustling of the wings of wild birds, or the tearing of a piece of cloth. According to them the stones are always in a state of ignition at the moment they reach the ground; their external surface is black; and some of them sound when struck like metallic substances. The name by which they called them signifies *falling stars changed into stones.*

The Chinese believed that the appearance of aërolites was connected with passing events, and for that reason made catalogues of them; I do not know that we have much right to ridicule this prejudice: were the savans of Europe much wiser when, rejecting the evidence of facts, they affirmed that falls of stones from the atmosphere were impossible? Did not the Academy of Sciences, in 1769, declare that a stone, whose fragments were collected at the moment when it fell, near Lucé, by several persons *who followed it with their eyes to the point where it reached the ground*, did they not declare that it did not fall from the atmosphere? Lastly, was not the attestation of the municipality of Lagrange de Juliac, affirming that on the 30th of August, 1790, a large quantity of stones fell in the fields, on the roofs of the houses, and in the streets of the village, treated by the contemporary journals *as a ridiculous fable, calculated to excite the contempt, not only of savans, but of all rational persons?* Naturalists, who refuse to admit as facts, only those phænomena which they are able to account for, certainly do more injury to the advancement of science, than they who are liable to the imputation of being too credulous.

Article III.

Methods of experimentally determining the Quantity of Vapour in the Atmosphere, and the Specific Gravities of Gases mixed with Vapour. By John Herapath, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Cranford, July* 18, 1826.

In the *Annals of Philosophy* for Nov. 1821, p, 375, &c. I have briefly explained the theory of two methods of determining the quantity of vapour at any time in the atmosphere. One of these methods was invented by Mr. Dalton who, I have shown in the above cited pages, has, by some oversight, miscomputed the experimental results. Aware that simple and easy methods of ascertaining the relative and absolute quantities of vapour in the air are in many researches of importance, especially as the sensibility and accuracy of the common hygrometers are known so to vary as in a few years to become almost useless, I have thought it advisable to detail a little more methodically than I have the principles and application of the above methods, and to add a third which, I believe, is new. This last method has the advantage of combining greater accuracy with more extensive utility.

First, or Mr. Dalton's Method.

Let the water in a clean thin glass vessel be gradually cooled, either by pouring into it colder water, or by mixing with it nitre, sal ammoniac, &c. until the vapour of the air begins to be deposited on the surface of the glass in the form of dew, which is just when the glass begins to appear dull. Let the temperature of the water at this moment be noted; and from Mr. Dalton's experiments, or his theorem, or from that one which I have given, *Annals* for Dec. 1821, p. 434, let there be taken the corresponding tension of the vapour, and it will be equal to the elastic force of the vapour in the air. For since the cold stratum at the surface of the glass must have the same elastic force as the air, and since, at the very commencement of the deposition, the proportions of air and vapour in this stratum are the same as in any other part of the air, the ratio of these elasticities must be the same; and consequently the elastic force of the vapour in the stratum is the same as in other parts of the air.

Mr. Dalton, in order to get the elastic force of the vapour in the air, increases the before found tension in the ratio of 448 + the Fahr. temperature of the water to 448 + the temperature of the air, not considering that from the very principles of fluidity every part of the air, whether in contact with a colder body or not, must be pressed by the same force, and therefore have the same elasticity. Hence the vapour found by Mr. Dalton's calculation always exceeds the quantity actually existing in the air.

Suppose τ_1 be the tension just found, and p the barometric pressure of the air, then $p - \tau_1$ is evidently the elastic force due to the dry air; $p - \tau_1 : \tau_1$ is the ratio of the volumes of dry air and vapour; and $\frac{\tau_1}{p}$ is the absolute volume of vapour in any unity of the compound mass of vapour and air, supposing the vapour could exist as an air under the pressure p. And if τ denote the tension of vapour corresponding to the temperature of the air, the humidity of the air is $\frac{\tau_1}{\tau}$, absolute humidity being 1.

If f, f_1 be the Fahr. temperatures of the air and water, then the absolute volume of vapour by Mr. Dalton is $\frac{\tau_1 (f+448)}{p\,(f_1+448)}$, and the humidity of the air $\frac{\tau_1}{\tau} \cdot \frac{f+448}{f_1+448}$. The greater the difference between f and f_1, that is, the less the vapour in the air, the greater will be the proportional error of Mr. Dalton's method. If $f = 60°$ and $f_1 = 40°$, the error will amount to more than 4 per cent.

It has been justly observed by M. Biot, that this method of experimenting "may not have in ordinary hands all the sensibility which its author is pleased to attribute to it." In such hands as Mr. Dalton's, almost any method will succeed; but certainly the above process, elegant and simple as it is, requires no ordinary care to insure success. I have often thought whether a vertical glass cylinder open at the top only, and successively dipped for short times in water or mercury of different temperatures, might not afford more uniform results, since the cold air in the cylinder being heavier than the air above, would keep its contact with the glass better than the air cooled on the outside of the glass vessel; but I have never brought this method to trial.

M. Biot has also objected to Mr. Dalton's method on the score of its not been applicable to small portions of gas; but a very trifling modification of this method, which no doubt has occurred to its author, will easily enable us to apply it to any portion of gas. For example, if a small glass vessel furnished with a stopcock or two, and filled with the gas at a given temperature f and pressure p as ascertained by a manometer, be immersed in a vessel of clear water which is gradually cooled down, the incipient dulness of the immersed glass might be easily seen, and thence the volume, &c. of the enclosed vapour be determined. Suppose the temperature of the water at this moment is f_1 and the corresponding tension of vapour τ_1 then

$$\frac{f+448}{f_1+448} \tau_1 \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

is plainly the absolute force of the vapour at the temperature f when the whole pressure is p and

$$p - \frac{f+448}{f_1+448}\tau_1 \ldots\ldots\ldots\ldots (2)$$

is the elastic force of the enclosed gas.

If we put unity for the specific gravity of dry atmosphere at the Fahr. temperature 32° and barometric pressure 30, its specific gravity, it may be easily shown at any other temperature f, and pressure p is

$$\frac{16\,p}{f+448} \ldots\ldots\ldots\ldots\ldots (3)$$

Putting therefore for p the absolute force of the vapour (1) just found, the specific gravity of the air becomes

$$\frac{16\,\tau_1}{f_1+448},$$

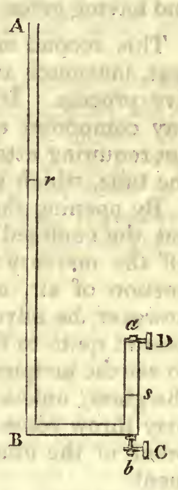
five-eighths of which, or

$$\frac{10\,\tau_1}{f_1+448},$$

is, by Lussac's observations, the specific gravity of the vapour confined with the gas. And if this be subtracted from the specific gravity of the mixture supposed to be already determined, it will leave the specific gravity of the gas at the temperature f and pressure (2).

Second Method.

Let A B C D be any rectangular glass tube open at A, and having stop-cocks at C, D, whose orifices are b, a; and let the glass be attached to a board or any other thing in a vertical position. Having closed the cocks C, D, pour into A mercury of the same temperature as the air and tube, until the air enclosed in C D just begins to cloud the glass with the deposition of its vapour; and suppose at this moment r, s, are the respective surfaces of the mercury in the legs B A, C D; and let the difference of their altitudes be m, the barometric pressure being as before p. Then the elastic force of the air in D s is to the elastic force of the atmosphere, or, which is in the same proportion, the elastic force of the vapour in D s is to the elastic force of the vapour in the air, as $p + m : p$. But the vapour in D s just beginning to deposit itself, and being of the same temperature as the external air, must manifestly have an elastic force equal to the tension of vapour corresponding to the temperature of this external air. That is, τ being the tension



$$\tau \frac{p}{p+m} \ldots\ldots\ldots\ldots\ldots\ldots (4)$$

is the elasticity of the vapour in the atmosphere, and of course

$$p - \frac{p\tau}{p+m} = \frac{p+m-\tau}{p+m} p \ldots\ldots\ldots\ldots\ldots (5)$$

is the force of the dry air. Dividing the preceding by p, gives

$$\frac{\tau}{p+m}$$

for the absolute volume of vapour in a unity of the atmosphere. And if instead of by p we divide by τ, the result

$$\frac{p}{p+m}$$

expresses the humidity of the air.

The formulæ in this method ought to give the same results as the corresponding of the preceding method. Equating therefore $\frac{\tau}{p+m}$ with $\frac{\tau_1}{p}$, we shall find

$$\frac{\tau_1}{\tau} = \frac{p}{p+m}$$

the left hand member of which is the formula for the humidity of the air as given by the first method, and the right hand member the formula for the same thing given by the second method. In the same way we may deduce other expressions in either method, by only knowing its expression in the other method, and having given the equation $\frac{\tau}{p+m} = \frac{\tau_1}{p}$.

This second method has a considerable advantage over the first, inasmuch as it is free from the uncertainty of the tentative process. It may also be applied to almost any portion of any compound air however small. Another advantage is its not requiring attention to regularity in the caliber of any part of the tube, which may be regular or irregular in any degree.

By opening the cock D, and allowing the mercury to force out the confined air, and then turning the cock C so as to let off the mercury to any extent, we may draw into C D a fresh portion of air, and commence the experiment anew. It will, however, be advisable in this case not to allow the mercury to ascend quite to the top of C D, as it will be extremely difficult to see the incipient deposition wherever the mercury has touched the glass, unless it be cleaned before re-commencing; which may throw some doubts on the real temperature of the tube, a point of the utmost consequence to the success of the experiment.

If instead of atmospheric air we experiment on a mixture of any gas and vapour at the temperature of the air f, and it be required to find the specific gravity of the gas; then replacing p in (3) by (4) and taking five-eighths of the result from the specific gravity of the mixture at the temperature and pressure of the atmosphere, leaves the specific gravity of the gas at the temperature f, and pressure (5).

These two methods being independent in principle and practice, will, if made at the same time, afford a mutual check and corroboration. As they both, however, rest in a great measure on a certain delicacy of ocular observation, I shall now explain a

Third Method

free from those defects.

Having introduced any quantity of the atmosphere in D C of our rectangular tube, as well as some mercury in the other leg B A, close the cock D and open C, until the surface of the mercury in B A sinks as much as it can below the other surface in C A, so as however not to descend into the horizontal part B C. Then having shut C, measure the pressure on the enclosed mixture, which will be less than the barometric pressure by the difference of the altitudes of the mercury in C D and B A, and call it p. Let s also be the space occupied by the mixture, which, supposing C D perfectly cylindrical, may be represented by the depression of the mercury in C D, below the interior upper part of it D. At this time it is clear that all the vapour is in the airiform state, its elasticity being less than in the open air.

This done, pour in some more mercury in A, until, from the dulness of the glass D s, we are certain that some of the vapour is condensed; and let the measured pressure and space occupied be p_1 and s_1.

Repeat these admeasurements after having poured in as much more mercury as you conveniently can; and suppose the new pressure and space are p_2 and s_2. Let τ be the tension of the vapour corresponding to the temperature of the air f, which, from the well known laws of vapour, must be the same as the elastic force of the vapour in both of the last experiments. Then $p_1 - \tau : p_2 - \tau ::$ elast. of gas in s_2 : elast. of gas in s_1 $:: s_2 : s_1$. Therefore

$$\tau = \frac{p_1 s_1 - p_2 s_2}{s_1 - s_2} \ldots\ldots\ldots\ldots\ldots (6)$$

Consequently the elastic force of the gas in s_1 is

$$p_1 - \tau = \frac{p_2 - p_1}{s_1 - s_2} s_2. \ldots\ldots\ldots\ldots (7)$$

and in any other space s, it is

$$(7) \times \frac{s_1}{s} = \frac{(p_2 - p_1) s_1 s_2}{(s_1 - s_2) s} \ldots\ldots\ldots (8)$$

But in s the whole pressure of all the vapour and air is p, and therefore the pressure due to the vapour alone is

$$p - (8) = \frac{p\,s\,(s_1 - s_2) - s_1\,s_2\,(p_2 - p_1)}{(s_1 - s_2)\,s} \ldots\ldots (9)$$

Hence supposing the vapour could exist as an air under the same pressure as air, vol. gas : vol. vapour :: (8) : (9); and vol. gas + vol. vapour : vol. vapour :: (8) + (9) : (9). Therefore what we have constantly in the course of our inquiries called the absolute volume of vapour is equal to

$$\frac{(9)}{(8) + (9)} = 1 - \frac{s_1\,s_2\,(p^2 - p_1)}{p\,s\,(s_1 - s_2)} \ldots\ldots\ldots\ldots (10)$$

which if we had chosen might have been more concisely deduced from (8).

Again, putting s_r for the space occupied by the mixture at the incipient state of condensation $s_r : s :: (9) : \tau$, and

$$s_r = (9)\,.\,\frac{s}{\tau} = \frac{p\,s\,(s_1 - s_2) - s_1\,s_2\,(p_2 - p_1)}{p_1\,s_1 - p_2\,s_2} \ldots\ldots (11)$$

The whole pressure at this time will consequently be

$$p \times \frac{s}{s_r} = \frac{(p_1\,s_1 - p_2\,s_2)\,p\,s}{p\,s\,(s_1 - s_2) - s_1\,s_2\,(p_2 - p_1)} \ldots\ldots\ldots\ldots (12)$$

Dividing P τ by this, P being the barometric pressure at the time of the experiment, gives the force of the vapour in the atmosphere equal to

$$\mathrm{P}\,.\,\frac{p\,s\,(s_1 - s_2) - s_1\,s_2\,(p_2 - p_1)}{p\,s\,(s_1 - s_2)} \ldots\ldots\ldots\ldots\ldots\ldots (13)$$

and consequently, by what we have already shown,

$$\frac{p\,s\,(s_1 - s_2) - s_1\,s_2\,(p_2 - p_1)}{p\,s\,(s_1 - s_2)} \ldots\ldots\ldots\ldots\ldots\ldots (14)$$

is the absolute volume of the vapour in a unity of space supposing it could exist as an air under the pressure P, at the temperature of the atmosphere. Again dividing (13) by (6), and we have

$$\mathrm{P}\,.\,\frac{p\,s\,(s_1 - s_2) - s_1\,s_2\,(p_2 - p_1)}{p\,s\,(p_1\,s_1 - p_2\,s_2)} \ldots\ldots\ldots\ldots\ldots (15)$$

the humidity of the air, absolute moisture being unity. And if f be the temperature of the mixture, and we substitute (13) for p in (3) and take five-eighths of the result from the specific gravity of the mixture previously obtained, it will leave the specific gravity of the enclosed gas of whatever kind it may be, when the temperature is f, and pressure P − (13).

This method of making the experiment is independent of any knowledge of the tension of vapour, which indeed it brings out, and may be applied to determine. The only things necessary to attend to are, the accuracy of the capacity of that part of the glass tube in which the mixed airs under examination is contained, and the temperature of this same part of the tube, which it is most essential should be exactly determined and not afterwards affected by handling or otherwise. A little change will also be made by the expansions and contractions of the air, and the condensation of the vapour; but if proper time be taken, the results perhaps will not be sensibly affected. Should they, however, it might be obviated by immersing this part of the apparatus in a vessel of water of a known temperature.

Probably a joint in the base B C of the tube, enabling the leg A B to have a vertical circular motion about it, would render the apparatus more practically convenient, by making the compression in C D more gradual, and taking away the irregular impetus occasioned with pouring the mercury in at the orifice A.

The greatest merit of this method is its being so complete within itself. It not only gives the tension of the vapour exclusive of any previous determination, and clears the results from the uncertainty of delicate ocular phenomena, but it may be applied to almost any portions of any gases with the same ease and success as to the atmosphere. JOHN HERAPATH.

ARTICLE IV.

On a recent Species of the Genus Hinnita of De France, and some Observations on the Shells of the Monomyaires of Lamarck. By J. E. Gray, Esq. FGS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *British Museum, July* 15, 1826.

IN the list of species of shells not taken notice of by Lamarck, which was published in a former number of the *Annals of Philosophy*, I described as a new species of the genus *Lima* a shell, of which I had observed an old very much worn specimen, in the British Museum; having, since that period, observed two specimens of a fossil species, which agreed with all the characters that were peculiar to my *Lima? gigantea*, I therefore was inclined to consider them as forming together a distinct genus, and I was farther confirmed in this opinion when I re-examined the other allied genera, for I found, by the assistance that the fossil specimens afforded me, that the recent shell was most probably attached (immediately) to the marine bodies, and that it was certainly much more nearly allied to the genus *Spondylus* of Linnæus, than to the genus which I had from the examination of the mutilated recent specimen referred it to.

Thinking that perhaps the fossil shells had been described, I compared the specimen with the characters and observations which De France has given for his genus *Hinnites*, which he established for two species of fossil shells, and to which he observes there are no recent species known. I found that it agrees in every particular with his remarks, and therefore I feel myself perfectly satisfied that it may be considered as the recent type of that genus; thus, at the same time, adding the genus *Hinnita* (for now the name must be changed, as the termination *ites* is only used in those genera where the species have only hitherto been found in a fossil state), to the list of recent shells, and erasing it from the catalogue of those genera which are considered by geologists as only to be found in a fossil state.

The following character may be given to the genus, which should be referred to the family *Spondylidæ* (nob.)

Hinnita, nob. Hinnites *De France.*

Shell bivalve, inequivalved, adherent immediately, by the apex of the right valve. Valves eared, radiately striated, beaks slightly produced into a triangular area. Byssal groove, none. Hinge without teeth; elastic cartilage placed in a deep groove, in each valve; ligament marginal, linear, straight.

Animal unknown.

The chief character by which this genus is to be distinguished from *Spondylus*, is that the hinge is destitute of teeth, and that the facets of both valves are nearly equally extended by the growth of the shell; instead of the attached one being extended into a long shelving projection, as in the latter, and that the valves are more equal and more distinctly radiately ribbed, and not quite so spinose as most of the species of the latter genus.

De France, in the work above quoted, has described two fossil species, 1 H. Cortesyi, figured in the Dictionaire des Sciences Naturelle, t. 61, f. 1, and 2, H. Duboissoni, to which I now add as the recent type,

3. *H. Gigantea.*

Shell oblong, outside pale brown, finely radiately-grooved, inside white, hinge margin purple.

Lima gigantea. *Gray, Annal. Phil.*

Icon. *Wood, Catalogue Suppl.* t. 2, f. 7, inedited.

Inhabits. *Mus. Brit.*

Shell oblong, rather thick, solid, outside pale brown, ornamented with numerous small rays, the left valve the most convex, the inside white with the hinge margin fine dark purple, the area left by the moving forward of the ligament is also purple, and rather narrow, the grooves for the elastic cartilage is very large and distinct in each of the valves, and quite open, they are extended the whole length of the facet, and considerably produced into the cavity of the shell. The muscular impressions are large, and the submarginal impression is orbicular ovate, with a considerable inflexion just even with the anterior ear. The length is four inches, the height from hinge to basal edge five inches.

I shall here take the opportunity, as I neglected it in my conchological essays, of pointing out how the back and the front edge, and consequently the right and left valves may be distinguished in those shells which are provided with only one adductor muscle. Indeed, I only need to speak of those monomyaires of Lamarck, which have the elastic ligament placed in an internal cavity, as the others may be easily distinguished by the position of their external cartilage, as pointed out in my former remarks. In the whole of these shells, the adductor muscle, and consequently the muscular impression is placed eccentrically, except in the genera Anomia and Placuna. Now by examining the animals, figures of which may be seen in the appendix to Lister, who has given the anatomy of the oyster and scallop, and the plates of Poli models, the mouth will be found to be always placed on the opposite edge, and the anus on the same edge of the shell as the adductor muscles, consequently that edge of the shell to which the muscular impression is nearest, must be its posterior edge, and now if the hinder margin of the shell be placed towards the observer with the hinge edge uppermost, the sides of the animal and shell will correspond with that of the observer.

I have been thus particular in pointing out this distinction of the valves of these shells, because the observance of this fact will materially assist the natural disposition of the micropodous bivalves, and enable them to be separated into natural families, a division which is now absolutely requisite from the number of genera being so much increased by the great attention which has lately been paid to this interesting study.

The animals of this groupe, which are attached to the rocks, &c. by a byssus, as the *Pectens*, have always the groove for the transmission of the byssus, placed under the ear of the right valve; this character at once distinguishes the *Pectinidæ*, a family which contains the genera Pecten, Anusium, Janira, Neithea, Pallium, Pedum, and Lima. Amongst those genera which are attached to the rocks by the means of the outer surface of the shell themselves, which appear to be the typical groupe of micropodous mollusca, the oysters are always fastened by the left valves, and the genera Ostrea and Gryphea may therefore form a family under the name of *Ostreidiæ*, while the *Spondylidæ*, which contain the genera *Spondylus*, *Hinnita* and *Plicatula*, are always attached to the marine bodies by their right valves, and consequently the muscular impressions are placed on the right side of the attached valve, which makes the impression of *Hinnita* to appear to be placed on the side of the shell opposite to that of the oyster, as described by De France, who appears not to have observed that the difference was only occasioned as in what is erroneously called the reversed chama, by the animals being fastened by the other valve.

The fact of the conical univalves being considerably altered in form and figure by the substances to which they are attached, has long ago attracted the attention of conchologists, and it also has been observed greatly to influence the form of the Anomiæ, for when they are attached to pectens and other radiated shells, they are marked by their sculpture; but I am not aware that this fact has ever been observed to take place in any of the other genera of bivalve shells, and especially in those which are thick and ponderous. The specimen of *Hinnita gigantea*, which is in the museum, has evidently been attached to a rock which had some large serpulæ growing on it, the edges of the valves, when growing, conformed themselves to the surface, and consequently the upper valve has the convex lines across, which exactly correspond with the convolutions on the outer surface of the lower one; now at first sight one might be led to believe, as indeed several of my friends have been, that the convexity of the under valve, by pressing up the body of the animal, has consequently raised the upper surface of that part of the body, but in several places the upper valve is marked externally where there are no traces of it in the inner surface of either valve, nor are the upper valves of oysters affected when the lower valves have very large pearls on them, which must considerably displace the body of the animals. Indeed, to any one who studies the formation of shells, I think it must be evident that these raised places must be formed as I have stated, by the edge of the upper valves conforming themselves when deposited to the bend of the lower valves; and when the causes which produced these curves are removed, the valves resume the usual form, leaving the convex line on the outside, the concavities of the inner part of the upper valve being obliterated by the new layers of shell.

Article V.

Abstracts of Papers in the Philosophical Transactions for 1825, *on the peculiar Magnetic Effect induced in Iron, and on the Magnetism manifested in other Metals, &c. during the Act of Rotation.* By Messrs. Barlow, Christie, Babbage, and Herschel.

2. *On the Magnetism of Iron arising from its Rotation.* By S. H. Christie, Esq. MA. FRS.

(*Continued from* p. 43.)

"*Experiments with the Dipping Needle.*

"Having found, in all the experiments which I have described, that the effects produced on the horizontal needle depended on the situation of the plate with respect to the axis and equator of an imaginary dipping needle passing through the centre of the horizontal needle, my next experiments were undertaken with the view of ascertaining whether the effects produced by the rotation of the plate on the dipping needle itself corresponded with those which I had observed on the horizontal needle. In making these it was necessary to adjust the dipping needle on a stand detached from the instrument, on the arm of which the iron plate revolved, on account of the diameter of the case of the dipping needle being greater than the distance *s n* (fig. 1.) It was therefore only in particular positions that I could observe the deviation caused by the rotation of the plate. This, however, was of the less importance, since, as I expected that the deviations of the dipping needle would be less than those of the horizontal needle nearly in the ratio of sin. 19° 30′ to 1, it was only in the cases in which they were the greatest that I was likely to have been able to observe them.

" As the dipping needle, when in the position of the dip, could only vibrate in the plane of the meridian, no effect corresponding to the deviations of the horizontal needle could be observed, either when the centre of the plate was in the intersection of the meridian and equator, and its plane perpendicular to the planes of these circles, or when the centre of the plate was in the secondary to the meridian and equator, and its plane in the plane of this secondary. In order, therefore, to ascertain the deviations of a needle suspended freely by its centre of gravity, corresponding to those of an horizontal needle, when the plate had those positions, and which I considered to be the principal points to be determined, it was necessary to observe the effect produced on the dipping needle when the centre of the plate was in the equator, and exactly east or west of the centre of the needle, and its plane parallel to the plane of vibration of the needle; and also when its centre and plane were in the plane of vibration.

" In making these observations, the instrument was adjusted as in fig. 1, the compass being however removed; the indexes at 0, 0′ were brought to Æ, æ, on the moveable limb, and that limb was placed at right angles to the fixed limb, so that the plane of the plate was parallel to the magnetic meridian. The dipping needle was then placed as nearly as possible in the required position, and the levels being carefully adjusted, the needle was made to vibrate freely and left to settle. After the plate had been made to revolve several times in the same direction, the point marked 0 was brought to coincide with the upper part of a line parallel to the magnetic axis, and passing through the centre of the plate. The needle was then slightly agitated, or made to vibrate through a small arc; and when it settled, the dip was noted both at the upper and lower extremity, or the south and north end of the needle. This was repeated for the points marked 60, 120, 180, 240, 300. The plate was now made to revolve in the contrary direction, and similar observations made of the dip of the needle when the several points 300, 240, 180, 120, 60, 0, coincided with the upper part of the line parallel to the magnetic axis. Continuing the revolution of the plate in this direction, a second set of observations of the dip were made for the several points from 300 to 0. After this, the plate was again made to revolve in its first direction, and a second set of observations made of the dip for the points from 0 to 300. I considered the mean of all the observations in the two sets, when the plate revolved from 0 to 300, as the mean dip when the plate revolved in this direction; the mean of all the observations in the two sets, when the plate revolved from 300 to 0, as the mean dip when the plate revolved in this direction; and the difference between these mean dips as the *deviation due to the rotation* of the plate.

"As I had experienced that the dipping needle, even when of the best construction, was an instrument from which accurate results could only be obtained by taking a mean of a great number of observations, I was aware that, by making only two for each point of the plate, I was liable to an error in the observations for each point taken separately, but this I considered would be counteracted in taking the mean for all the points; so that the mean results could not err far from the truth. The dipping needle which I made use of was a very good instrument, by Jones, of Charing Cross: the needle, made according to Captain Kater's construction, consisted of two arcs of a circle; its length was 7 inches. The plate was the same I had used in the experiments with the horizontal needle.

"For the better distinguishing of the edges of the plate and the direction of its rotation, I conceive two planes at right angles to each other to pass through its centre; one, the plane of the equator or a plane parallel to it, which I call the equatorial plane; the other, the plane of the secondary to the equator and meridian, or a plane parallel to this secondary, which I call the plane of or parallel to the axis. The intersections of the first plane with the edges of the plate, I call the equatorial north and south edges; and the intersections of the second, the polar north and south edges.

"From the observations thus made it appears that, in this position of the plate, the *deviation* of the *upper*, or *south* end of the needle, *due to rotation*, was in the direction in which the *north* or *lower* edge of the plate revolved, and the deviation of the *north* or *lower* end of the needle, in the direction of the rotation of the *upper* or *south* edge of the plate. It would follow from this, that if a needle could be suspended freely by its centre of gravity, and the centre of the plate were in longitude 90°, latitude 0°, and its plane at right angles to the meridian;

then also, the *deviation* of the *south* end of the needle *due to rotation*, would be in the direction of the *north* or *lower* edge of the plate, and the deviation of the *north* end, in the direction in which the *south* or *upper* edge revolved; which are precisely the directions of the deviations of the horizontal needle in this position of the plate.

"The law which I have shown to obtain in all the experiments on the horizontal needle, viz. that the sides of the equator of the imaginary dipping needle always deviated in directions contrary to those in which the corresponding edges of the plate moved, I had derived previously to having an opportunity of making any experiments with the dipping needle: a comparison of the above results with this law will more fully illustrate its nature, and at the same time show their perfect accordance. In making this comparison, it is necessary to notice that, an *increase* of the dip of the needle, corresponds to a deviation of the *southern* edge of its equator towards the *south* pole, and of the *northern* edge towards the *north* pole; and on the contrary, a *diminution* of the dip corresponds to a deviation of the *southern* edge of the equator towards the *north* pole, and of the *northern* edge towards the *south* pole. Now, when the equatorial *south* edge of the plate revolved towards the polar *south*, and consequently the equatorial *north* edge towards the *polar* north, the inclination of the needle was *diminished* by the rotation; that is, the *south* edge of its equator deviated towards the *north* pole, and the *north* edge of its equator towards the *south* pole; or *the edges of the equator, by the rotation of the plate, deviated in directions contrary to those in which the edges of the plate moved.* The same conclusion evidently follows from the observations when the equatorial south edge of the plate revolved towards the polar north, the dip being here increased by the rotation of the plate.

"The next observations which I made, were of the inclinations of the dipping needle, when the plane of the plate was in the plane of the meridian or plane of vibration of the dipping needle.

"From these observations it appears that, the plane of the plate being in the plane of vibration of the needle, and its centre in the equator, the *deviation* of the *upper* or *south* end of the needle, *due to rotation*, was in the direction of the rotation of the *upper* or *south* edge of the plate, and of the north end in that of the *north* edge; and we may therefore conclude, that if a needle could be freely suspended by its centre of gravity, and the centre of the plate were in the equator, and its plane in that of the secondary to the meridian and equator, the *deviation* of the *south* end, *due to rotation*, would be in the direction in which the *south* edge of the plate revolved, and of the *north* end, in that in which the *north* edge revolved; which, again, are precisely the directions in which we have seen, that the horizontal needle deviated by the rotation of the plate in this position.

"When the centre of the plate was in latitude 90° south, contrary to what took place when it was in the equator, the deviation of the *south* end of the needle is in the direction in which the *lower* or *north* edge of the plate revolved; and we may therefore infer that the same would be the case if a needle were suspended freely by its centre of gravity, and the plane of the plate were in the plane of the secondary to the meridian and equator, its centre being in latitude 90° S: which also agrees exactly with the directions of the deviation of the horizontal needle, due to rotation, in this position of the plate.

"It is evident from these different experiments with the dipping needle, that whatever may be the peculiar effects produced on the iron by its rotation, the *deviations* of the horizontal needle, *due to the rotation,* are of the same nature as those that would arise by referring the deviations of the dipping needle to the horizontal plane.

Theoretical Investigations.

"It has in general been considered that the different deviations of the horizontal needle, arising from the action of soft iron on it in different positions, can only be accounted for on the supposition, that the iron is polarized by position, the upper part being a north pole, and the lower a south one, each pole of the iron attracting the pole of the needle of the same name, and repelling that of a contrary name: but if we suppose that each particle of the iron simply attracts indifferently either pole of a magnetic particle, and refer the attraction of the iron to its centre, then if the angular deviations of a magnetic particle in the centre of the needle and in the line of the dip, arising from such attraction, be reduced to the horizontal plane, these reduced deviations will agree with the actual deviations of the horizontal needle. In investigating theoretically the effects that are produced by the *rotation* of a plate of iron, I will first suppose, that, independently of *rotation,* the iron acts in this manner, and that by the *rotation* it becomes polarized in a direction, making a certain angle with the magnetic axis, since from such a polarizing of the iron, the law which I have shown to include all the phænomena, would evidently result. On this supposition, each pole of a magnetic particle in the centre of the needle would be urged by an attractive force towards *the centre* of the iron plate, by an attractive force towards the pole of a contrary name, and by a repulsive force from the pole of the same name in the iron."

Mr. Christie here proceeds to investigate this theory by calculation, the results of which, he finds, "indicate that the effects are not produced in precisely the manner we have supposed. In one point our theory is unquestionably at variance with the actual circumstances of the case; for we have supposed that no partial magnetism exists in the iron, or that every part of it taken separately would equally affect the needle. It is, I believe, scarcely possible to procure iron that shall possess this uniformity of action, and it is evident that this was not the case with the plate of iron which I made use of. This species of polarity in iron is of so variable a nature, since by an accidental blow it will be transferred from one point to another, that it does not appear possible in any manner to submit its effects to calculation. It was to prevent these effects embarrassing the results, that I took the mean of twelve observations for each position of the plate; still it is possible that some of the differences between the observations and the results of the theory may have arisen from this cause.

"As the results of the hypothesis which I have advanced do not precisely agree with the observations, it will be proper to enquire whether we shall obtain a more perfect agreement by means of the hypothesis commonly assumed, in order to account for the effects produced on the needle by a mass of soft iron, viz. that the upper part of every mass of iron acts as a north pole and the lower part as a south pole. Let us then suppose such poles to exist in the iron plate, in the diameter in the direction of the dip, and that the rotation causes the line joining them to describe in the iron an angle ψ from this diameter."

The agreement between the observations and the calculated results from this theory, Mr. Christie here finds, would not be greater than in the former case.

"In the explanation of the phænomena which take place on presenting the different ends of a mass of iron to the poles of a magnetic needle, in addition to the hypothesis, that the upper part becomes a north, and the lower a south pole, by position, it is necessary to suppose also, that in every change of position of the iron there is a corresponding and immediate change of its pole; that is, the upper end becoming the lower, it also immediately becomes a south pole. Now it appears to me, that if we attempt to explain, on this hypothesis, the phænomena arising from the rotation of the iron, we shall find that there are circumstances which are wholly incompatible with it. If on turning a mass of iron end for end, the poles are immediately transferred from one end to the other, how can we suppose that the revolution of the iron will cause these poles to move forwards, so that the line joining them shall describe an angle from the line of the dip? or even granting that during the revolution of the iron they may be carried forward, they must, as soon as the iron ceases to revolve, resume their original position in the line of the dip, if they are so immediately transferred from one end of the iron to the other, as it is necessary to suppose in order to account for the phænomena which take place of attraction and repulsion, as they have been called. Immediately, then, that the iron becomes stationary in any position, the deviation of the needle ought, on this hypothesis, to become the same, whether the iron has been brought into that position by revolving in one direction, or in the contrary. It is hardly necessary for me to say that this would not be the case, since I have stated, that, in all the preceding observations, the iron was stationary previous to the observation being made.

"Whatever are the effects produced on the iron by its revolution, so far from these effects being of the transient nature which we must suppose them to be on this hypothesis, they appear to have been quite permanent, that is, so long as the iron remained in the same position. The following observation will show the small changes which took place during 12 hours.

"In order that the needle might be quite free to move, it was suspended in a balance of torsion by a brass wire, of the same diameter as the finest gold wire used for transits, free from torsion, 21.15 inches long. The plane of the plate was in the plane of the secondary to the equator and meridian, its centre in latitude 0°, longitude 180°; and it was fixed to a wooden axis passing through its centre perpendicular to its plane: the ends of this axis, which revolved with the plate, being made of brass, that I might ascertain whether the effect was independent of friction on the plate itself. The plate was made to revolve in contrary directions, as usual, and the direction of the north end of the needle noted, when the point 180° on the plate coincided with the upper part of a plane parallel to the meridian, and passing through the plate's centre. After having made the plate revolve so that its upper edge moved from west to east, and noted the direction of the north end of the needle when 180° coincided with the above plane, it was made to revolve from east to west, and 180° being again brought to coincide with this plane, the direction of the north end of the needle was noted at different times for more than 12 hours, the plate remaining stationary during that time.

Direction of rotation of plate's upper edge.	W to E	E to W	Time of observation.	
The several directions of the north end of the needle were observed when the point 180° on the plate coincided, above the centre, with the plane parallel to the meridian.	0° 04′W	2° 50′ E	9^h 35^m	During this time the plate was kept perfectly stationary, and care was taken that the apparatus should not be in the least disturbed.
		2 50	10 05	
		2 46	11 10	
		2 44	20 35	
		2 42	21 48	
	0 02 E		22 01	After 21^h 48_m the plate was made to revolve slowly once from W to E.
	0 02 E		22 17	After making the plate revolve several times and more rapidly.
		2 46	22 28	Making the plate revolve several times from E to W.
	0 04 W		22 40	Making the plate revolve once so slowly that the time of rotation was 3′ 26″.
	0 06 W		24 05	The plate kept perfectly stationary since 22^h 40^m.
	0 08 W		25 35	
		1 22		Making the plate revolve through 30° from W to E, and then bringing it back 30° from E to W.
		2 42		Making the plate revolve through 90° from W to E, and then bringing it back 90° from E to W.
		2 42		Making the plate revolve repeatedly and rapidly.

"From these investigations it appears, that the effect produced on the iron by its *rotation* is permanent, so long as the plate remains stationary: that it is independent of friction; that it is so far independent of velocity, that the iron can scarcely be moved so slowly that the whole effect shall not be produced; and that the whole effect is produced by making it perform only one fourth of a revolution.

"Shortly after I had discovered these peculiar effects to be produced by the rotation of iron, I pointed out the general nature of the phænomena, and exhibited some of them to Mr. Barlow, and he has since made some experiments on the rotation of spherical shells, in which he has found that phænomena somewhat analogous take place, but they appear to be dependent on the velocity with which the shell is made to revolve."

"Since it appears, from all the observations which I have detailed, that the direction of the magnetic polarity, which iron acquires by *rotation about an axis,* whether it be at right angles to the line of the dip, as would follow from the theory which I have investigated, or not, has always reference to the direction of the terrestrial magnetic forces, we must infer that this magnetism is communicated to it from the earth. It does not therefore appear from this, that a body can become polarized by rotation alone, independently of the action of another body: so that if from these experiments we might be led to attribute the magnetic polarity of the earth to its rotation, we must at the same time suppose a source from which magnetic influence is derived. Is it not then possible that the sun may be the centre of such influence, as well as the source of light and heat, and that by their rotation the earth and other planets may receive polarity from it? If so, further experiments and observations on the magnetic effects produced by the rotation of bodies may indicate the cause of the situations of the earth's magnetic poles, and of their progressive movements or oscillations.

Comparison of the magnetical effects produced by slow and by rapid rotation.

"With the view of ascertaining how far the effects produced on a magnetic needle by a plate of iron during its rapid rotation, corresponded with those that I have described as nearly independent of the velocity of rotation, and as continuing after the rotation had ceased, I placed the same plate of iron, which I had used in my former experiments, in the plane of the magnetic meridian, on an axis perpendicular to its plane, and about which it could be made to revolve with any velocity, not exceeding 10 revolutions in a second. I then placed a small compass, with a light needle delicately suspended, on a platform wholly detached from the iron plate, in certain positions opposite to the edge of the plate, both to the east and to the west of it, as near to the surface as the compass box would admit. The compass being adjusted, the plate was made to revolve once, slowly, so that its upper edge moved from north to south, and the point 0 coinciding with the plane perpendicular to the plane of the plate, and passing through its centre and that of the needle, the direction of the north end of the needle was observed; and also when 180 coincided with the plane, the same observation was made. The plate was now made to revolve rapidly in the same direction, about 8 times in a second; and when the needle became stationary during the rotation, the direction of its north end was observed. The point 0 on the plate was again made to coincide as quickly after the rapid rotation as possible, and the direction of the needle observed, in order to see if that rotation had produced any permanent change in the iron; the same was done when the point 180 again coincided. Observations precisely similar to these were made when the upper edge of the plate revolved from south to north.

"Although the centre of the plate was stationary, and the needle was placed in certain positions with respect to it, I consider, as before, the situation of the centre of the plate with reference to the plane passing through the centre of the needle perpendicular to the dip; and its angular distance from this plane, the equator, was measured on a circle of 9 inches radius parallel to the meridian, passing through the centre of the needle, and at the distance 1·45 inch from it, so that the centre of the needle was always at this distance from the edge of the plate, east or west. As the needle was only two inches in length, and the rim of the compass divided into degrees, the direction of the needle could not be observed nearer than to 5′, and indeed scarcely to that degree of accuracy. The mode which I was under the necessity of adopting in adjusting the compass to the several positions did not admit of extreme accuracy, so that these positions may be considered as liable to errors amounting to 1°, or perhaps rather more, in angular distance from the equator; but as my principal object was the comparison of the deviation due to the slow and rapid rotation of the plate, when its centre was in precisely the same position with respect to that of the needle, this was not very material; it will however account for any disagreements that may be noticed in the absolute deviations in corresponding positions, as the greatest accuracy of adjustment would be requisite for their perfect agreement, when the plate is so near to the poles of the needle.

"Having ascertained, by the observations when the plate was to the west of the needle, that the rapid rotation produced no permanent change in the iron beyond that arising from the slow rotation, the deviations when any particular points of the plate were opposite to the needle being, as near as could be expected, the same after the rapid rotation as they were after the slow rotation in the first instance, the errors being sometimes in excess, sometimes in defect, I did not repeat the observations on the effects of the slow rotation after the rapid, when the plate was to the east of the needle."

From the inspection of the tables containing these observations, "it appears that the forces which are exerted on the needle during the rapid rotation of the plate, are always in the same direction as the forces which are derived from the slowest rotation, and which continue to act after the rotation has ceased; but that the former forces are greater than the latter, there being only one instance of the contrary, and that in a position where the effects are so small, that a trifling error of observation would account for the difference. Taking a mean of all the observations, these forces appear to be in the ratio of 19 to 13, or very nearly 3 to 2. It is evident then that the polarising of the iron in the same direction will account for the phænomena in both cases, but that the intensity of the polarity during the rapid rotation is greater than of that which appears to be permanent after the rotation, whether slow or rapid, has ceased;

and that the phænomena observed during rapid rotation are such as we should expect from those which I have so fully described as arising from rotation, without regard to its velocity." E. W. B.

(To be continued.)

Article VI.

Analysis of some Minerals. By Mons. J. Berzelius.*

Phosphate of Yttria.

This mineral was found by M. Tank, in the neighbourhood of Lindenas, in Norway, in a gangue, chiefly consisting of large grained granite, and accompanied by another mineral, which, both by its external characters and those which it presents before the blowpipe, perfectly resembles the orthite discovered a year ago, at Skeppsholmen.

The specimen of phosphate of yttria, sent to M. Berzelius, was too small to admit of a perfect mineralogical description of the mineral; its form was irregular, with crystalline striæ, like those observed on imperfectly developed garnets.

Its colour is brownish yellow, similar to that of the zircon, from Fredrikswärns, with which, at first sight, it might easily be confounded. Its specific gravity at 60° = 4·5577; it is scratched by steel. Its fracture is foliated in several directions; its transverse fracture is uneven, and splintery; externally it is dull, but the foliated fracture has a resinous lustre, and the transverse a fatty one. In thin fragments it is semi-transparent and yellowish.

Before the blowpipe this mineral behaves much like phosphate of lime. Alone it does not fuse, but its colour becomes deeper. Heated in a matrass, it gives off no water; with borax it dissolves slowly, and forms a colourless glass, which becomes milk-white by flaming, and, if saturated, whitens on cooling. It dissolves with great difficulty in salt of phosphorus, and forms a transparent colourless glass. This mineral differs from phosphate of lime by the facility with which the latter dissolves by the salt of phosphorus, forming, if the salt be saturated, a glass which loses its transparency on cooling. The same, however, would happen with the phosphate of yttria, but the operation requires great time and labour. With carbonate of soda, the assay produces a strong effervescence, and gives an infusible, clear grey scoria. With boracic acid it dissolves with difficulty; but if a morsel of iron be introduced into the glo-

* Extracted from the Annales de Chimie.

bule, phosphate of iron is formed in abundance. The acids, even when concentrated, do not dissolve it.

The analysis of phosphate of yttria gave :

Yttria	62·58
Phosphoric acid, with a little fluoric acid	33·49
Subphosphate of iron	3·93
	100·00

The formula of the composition of this mineral is therefore analogous to that of phosphate of lime, $\ddot{Y}^3 \overset{:\cdot:}{P}^2$. As, hitherto, no other native combination of yttria with phosphoric acid has been discovered, it seems superfluous to give this any other denomination than that of phosphate of yttria.

Polymignite.

A black, brilliant, mineral, crystallized in small prisms, is sometimes found in the zircon-sienite of Fredrikswärns, whose very complicated composition has induced M. Berzelius to call it polymignite, (πολυς et μιγνυω.)

It is black, and absolutely opaque, even on the edge; the matrix, nearest in contact with it, is usually of a red colour, as occurs at Finbo, with albite, if it contain yttrotantalite.

This mineral is always more or less regularly crystallized in long thin prisms, with a rectangular base, whose edges are usually replaced by one or more planes; sometimes two of the planes of the prism are broader than the rest; its length is from one to four lines. Berzelius never had an opportunity of seeing the extremities of the prism sufficiently developed, to enable him to determine the form with any precision.

The specific gravity of polymignite = 4·806. It scratches glass, and cannot be scratched by steel. Its fracture is conchoidal, without any indication of cleavage. The surface of the crystals has a bright, almost metallic lustre, to which that of the fracture also approaches, and far exceeds what is commonly observed in minerals. Its powder is brown, the colour becoming clearer by trituration.

It undergoes no change before the blowpipe, neither fusing, nor losing its lustre; it gives off no water. With borax it fuses easily, and gives a glass, coloured by iron; if more borax be added, the glass becomes opaque by flaming, and acquires an orange colour; with a still larger quantity it becomes opaque on cooling. If the mineral be fused with tin, the colour becomes red, inclining to yellow. It dissolves also in salt of phosphorus, but with less facility; in the reducing flame the glass assumes a reddish colour, which is not altered by the addition of tin; in the oxidating flame the reddish colour becomes clearer, and inclines slightly to yellow. With carbonate of soda, the assay decomposes without fusing, and the mass becomes greyish red. A larger quantity of the salt gives an imperfect scoria. If we add borax to the assay, we obtain some traces of reduction, but it is commonly manifested only by metallic striæ, when triturated in the mortar.

The analysis of polymignite gave

Titanic acid	46·3
Zirconia	14·4
Oxide of iron	12·2
Lime	4·2
Oxide of manganese	2·7
Oxide of cerium	5·0
Yttria	11·5
Magnesia, Potassa, Silica, Oxide of tin	traces of
Loss	3·7
	100·0

The real loss is still greater than that given above, for the iron and manganese, and perhaps also the cerium, exist in the mineral in the state of oxidules, whereas, in the results, they are taken as oxides. The analysis of so complicated a mineral must always be attended with very considerable loss, and it may even contain some substances that have escaped detection. It is evident, therefore, that the composition of a mineral like the polymignite cannot be calculated.

"I have often attempted," adds M. Berzelius, "to separate titanic acid and zirconia from each other, but have not been able to discover an infallible method for the purpose. Diluted sulphuric acid separates them best, but it still dissolves a small portion of titanic acid, as well as the zirconia. The carbonated alkalies dissolve them alike, and nearly in the same proportions. Sulphate of potash, which often does not precipitate titanium from its solutions, throws it down, however, if they contain zirconia, which, in that case, carries down the titanic acid. Fluoric acid acts nearly in the same manner. Infusion of galls precipitates both titanic acid and zirconia. The analysis of polymignite might be effected with sufficient accuracy, if we had a method of separating these two substances; but it also contains two others which cannot be separated; namely, yttria, oxidulous manganese, the latter of which, in certain proportion, adheres obstinately to the former. The best mode of separating them that I know, is to dissolve them in nitric acid, evaporate the solution to dryness, and keep the salts for a long time at the temperature of melting tin; after which dissolve out the nitrate of yttria by water. If the quantity of water be small, the solution will not contain any manganese; but when we wash the oxide of manganese, a portion dissolves, and the solution assumes a dark colour, which it loses by exposure to light."

Levyine.

M. Berzelius found a specimen of levyine, (sent him from a quarter that precluded all suspicion of error,) so extremely similar to his *mesoline,* a mineral which accompanies the tessellite from Ferroe, that he was desirous of ascertaining its exact composition. The texture of the mineral was crystalline, and wherever the angles could be measured, they were found to correspond with those of chabasie.

Its analysis gave him,

Silica	48·00, containing oxygen	24·06
Alumina	20·00,	9·34
Lime	8·35 }	
Magnesia	0·40 }	
Potassa	0·41 }	3·25
Soda	2·75 }	
Water	19·30	17·16
	99·21	

"This result," M. Berzelius observes, "accords perfectly with the proportions of the component parts of chabasie," namely,

$$\left.\begin{matrix} C \\ N \\ K \end{matrix}\right\} S^2 + 3\ AS^2 + 6\ Aq.$$

as may be more distinctly seen by the following table:

	Chabasie from Ferroe.*	Chabasie from Scotland.	Chabasie from Gustafsberg.	Mesoline.
Silica	48·30	49·17	50·65	47·5
Alumina	19·28	18·90	17·90	21·4
Lime	8·70	—	9·73	7·9
Soda	—	12·19	—	4·8
Potassa	2·50	—	1·70	—
Water	20·00	19·73	19·50	18·19
	98·78	99·99	99·48	99·79

"With respect to the chabasies, I consider it certain," adds Berzelius, "that the trifling differences which occur in these analyses are occasioned by their not having been perfectly freed from the minerals which accompany them; hence mesoline ought to be considered merely as a species of chabasie, as is further observable in its granular texture."

* Analysed by Arfwedson.

Article VII.

Notices of the Excessive Heat during some Parts of the late Summer (1825.)*

1. *Observations on the Heat, &c. at Brooklyn, New York, for the month of July,* 1825. *The thermometrical observations exhibit the lowest temperature in the morning and evening, and the highest during the day. The lowest are from a thermometer always out of doors; the highest from one in an open hall, where no refraction or reflection can have effect.* Communicated by the Rev. Dr. S. Woodhull.

1825. July.	Morning. { Usually at six o'clock.	Usually at half-past two o'clock, p. m.	Usually at 10, p. m.
1	73. Clear. SW.	89. Clear. NW.	71. Clear. NW.
2	72. Clear. W.	91. Cloudy. SE.	81. Cloudy. SE.
3	71. Cloudy. SW. Drizling.	77. Cloudy. E. Rain.	73. Clear. NW.
4	74. Clear. W.	87. Clear. W.	75. Clear. SW.
5	76. Clear. S.	89. Cloudy. S. Rain.	71. Cloudy. SW.
6	67. Clear. NW.	87. Clear. NW.	74. Cloudy. S.
7	70. Clear. SW.	85. Clear. N.	73. Clear. N.
8	67. Clear. NW.	89. Clear. S.	79. Clear. NW.
9	69. Cloudy. NE.	87. Clear. E.	68. Cloudy. E.
10	75. Clear. NW.	93. Clear. W.	86. Clear. W.
11	79. Clear. W.	94. Clear. W.	87. Clear. SW.
12	79. Clear. WSW.	94. Clear. W.	88. Clear. SSW.
13	76. Clear. NW.	89. Clear. WNW.	81. Clear. NW.
14	72. Cloudy. N.	85. Clear. SE.	79. Clear. SE. Light rain.
15	71. Clear. SW.	82. Clear. S.	72. Clear. SSE.
16	70. Clear. WNW.	85. Clear. S.	74. Clear. SSW.
17	72. Clear. NW.	89. Clear. SSW.	78. Clear, calm.
18	74. Clear. NW.	94. Clear. SW.	80. Clear. S.
19	77. Clear. W.	92. Clear. SSW.	82. Clear. SW.
20	77. Clear. SW.	94. Clear. SW.	85. Clear. SSW.
21	79. Clear. SW.	96. Clear. SW.	86. Clear. SW.
22	80. Clear. W.	95. Clear. W.	84. Clear. S. Lightning and thunder.
23	78. Clear. W.	97. Clear. SSW.	79. Clear. SW. (same.)
24	74. Clear. SW.	93. Clear. SW.	80. Clear. W.
25	77. Cloudy, calm, rain, a. m.	79. Cloudy. NW.	73. Clear. SW.
26	69. Clear. N.	84. Clear. SW.	73. Clear. NW.
27	67. Clear. NE.	86. Clear. SW.	68. Clear. SSE.
28	67. Clear. NNE.	87. Clear. ENE.	71. Clear. SSE.
29	68. Clear. NE.	85. Clear. S.	70. Clear. SSE.
30	68. Clear. SSW.	81. Clear. S.	74. Clear. S.
31	74. Cloudy. S.	90. Clear. S. Light rain in the afternoon. Average nearly 90°.	76. Cloudy. NW.

* Extracted from the American Journal of Science.

2. *Temperature at Williams College during the late excessively hot weather.*

1825.	VII. a. m.	II. p. m.	IX. p. m.	Mean.	Wind.	
July 10	72·0	92·3	81·1	81·80	NW	At 3½ p. m. temp. 93·3.
11	80·0	96·8	77·0	84·60	S	At 2½ p. m. temp. 97·0 Sunset 98·5
12	75·7	93·6	74·6	81·30	NW	Thunder shower at evening.
19	66·5	91·5	78·4	78·80	NW	
20	75·5	95·1	76·3	82·30	S	
21	74·7	95·3	75·0	81·67	S	Some rain at sunset.
22	78·2	92·8	78·2	83·07	S	
23	76·5	98·0	83·6	86·03	S	Some rain. Temp. 98·5 at 3 p. m.
24	74·0	87·4	74·5	78·63	NW	
		average 93·5 nearly				

The mean temperature of the month is 74·95, which is a little less than that of July, 1820. The temperature was at no time in that year so high as that given above. The mean temperature of the month of July for the last *nine* years is 69·61, and for the last ten years including this July, is only 70·14. This shows the excessive heat of the late month of July.

There were some hot days in June, but the temperature was not above 96° in the hottest part of the day.

The thermometer is suspended six feet from the ground on the north side of a house, exposed to a free circulation of the air, but protected from all *reflected* heat.

Aug. 22. Observed three spots upon the sun—two large and black.

Note.—After these tables, follow some extracts from United States newspapers, from which it appears, that in some days in the course of the same month (July, 1825), the thermometer ranged at Hartford, in Connecticut, from 96° to 102° in the shade; at Salem, Massachusetts, it reached 104°; at a village in Yates' County, N. Y. 106°; at Wiscasset, Maine, 107°; and in a multitude of other places, the temperature was nearly as high about the same period. An article dated New York, July 25, says, "The thermometer, we believe, for the last two days, has scarcely varied during the day, from 95° in the shade, and the mercury has not fallen much in the night season. The ravages of death, yesterday, were truly melancholy. Twenty-five inquests were held upon the bodies of persons who came to their death by means of the heat, or by drinking cold water; and there have been several cases to-day—some before eight o'clock this morning. It seems to do no good for the press to admonish the public upon this subject; and those who return from the burial of friends, with a strange fatality, drink and die in a few minutes afterwards. So true is it that 'all men think all mortal but themselves.'

"We observe this morning that the civil authorities are putting cautions upon the pumps, printed in large letters."

We have ourselves seen similar laudable cautions affixed to the pumps in Philadelphia, with, we fear, no better success. To this we must add some further extracts from the American newspapers (quoted in the American Journal of Science) respecting the intense *cold* of last winter, 1825–6.

The Portland (Maine) Argus states, that the last day of January and first day of February were the coldest days experienced within the memory of the present generation. The mercury fell to 24° below zero. At Bath on the same days the mercury was at 27, and at Brunswick 29° below zero.

The Virginia papers state that the present winter has been the coldest for several seasons. On the 1st of February, at Petersburgh, the mercury ranged several degrees below the freezing point.

A man was frozen to death in Montreal on the night of the 31st ult. which was the coldest day experienced for years. Many persons had their faces frozen while walking through the streets. Thermometer 32° below the freezing point.

In Boston, Roxbury, Salem, &c. the thermometer stood from 12 to 17° below 0. The Boston papers state, that a woman was frozen to death in Southac-street on Tuesday night; and a stage coachman on the line between Groton and Concord, was found frozen stiff upon his box on the road, holding the reins in his hand. He was dead, and the reins were clenched so fast, that they were obliged to be cut, before they could be extricated from his grasp.

At Montreal, Lower Canada, on the 31st of January, the mercury fell to 38° before 0.

At Keene, New Hampshire, it was 28° below zero.

The newspapers from every quarter, make mention of the severity of the cold on the night of the 31st January and morning of the 1st February.—*Ed.*

Article VIII.

Remarks on the Rev. Mr. Powell's Paper on Radiant Heat. By William Ritchie, AM. Rector of Tain Academy.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Paris, July* 20, 1826.

Having observed in the 67th number of the *Annals of Philosophy,* a paper by the Rev. Mr. Powell, in which he calls in question the truth of some of my experiments and deductions on the properties of radiant heat, published in the Edinburgh

Philosophical Journal, I consider myself bound, in justice to the cause of science, to answer his objections. This will be best accomplished by a single deduction from the following

Experiment.

Let two air thermometers be procured having their bulbs large, and blown *extremely* thin (this condition is absolutely necessary to the success of the experiment) with scales divided into any number of equal parts. Place these at a convenient distance from each other, and then place a heated iron ball between them in such a position that the fluids in the two stems will sink exactly the same number of degrees. Let one of the hemispheres in the ball A, formed by a plane passing through the centres of the two balls, be coated with china ink. Let two of the alternate quarters of the ball B, formed by a plane cutting the former at right angles, be also coated with china ink. Place the thermometers in their original position, raise the iron ball to an elevated temperature (though still invisible in the dark), place it in its former position, and carefully observe the number of degrees the fluid descends in each stem. A striking difference will now be observed. The fluid will be found to have sunk several degrees lower in the thermometer B than in A. The same experiment may also be performed with a differential thermometer, having the bulbs coated as formerly described.

Whatever be the cause of this striking difference, it cannot possibly be the one assigned by Mr. Powell. For the surfaces of the two balls having exactly the same quantity of coating must radiate the absorbed heat with equal rapidity. I have viewed the subject in every way I could think of, and can find no cause adequate to produce this striking difference, except the one which I formerly assigned; viz. that the portion of caloric which radiated freely through the transparent hemisphere in one of the balls, was interrupted by the opposite posterior coating on the other ball.

With regard to the experiments with coated and transparent screens, I would only remark that a common mercurial thermometer is quite inadequate to determine the fact. If a quantity of water at the temperature of 50° be mixed with a hundred times its bulk of water heated to the temperature of $50\frac{1}{2}$°, the common thermometer will not detect the difference. In like manner, if the quantity of heat which freely permeates a thin plate of glass, amount only, in peculiar circumstances, to $\frac{1}{100}$ or $\frac{1}{200}$ of a degree of Fahrenheit's thermometer, the common mercurial thermometer can not possibly determine its existence. I would, therefore, humbly recommend to Mr. Powell to procure screens of *extreme* tenuity, and repeat the experiments with a more delicate instrument than a common thermometer, and he will assuredly find that the results which I have stated are not hasty conjectures but *absolute* facts. I might also mention that M. Arago informs me, that he has performed numerous experiments with transparent screens, and has uniformly arrived at the same results. I may further state, that I have performed various experiments with transparent screens of different kinds, and have, without a single exception, arrived at the important conclusion,—that a portion of *caloric* from an elevated source, though invisible in the dark, freely permeates a thin transparent screen in the same manner as *light* instantaneously finds a passage through thick plates of glass. These experiments and deductions will form the subject of a paper which I intend to lay before the Royal Society at one of its earliest meetings, in which I shall endeavour to establish on a solid foundation the striking connection between light and heat discovered by the ingenious French philosopher De la Roche.

Article IX.

*Statement of a Plan for making a minute Survey of the Heavens, and for the Formation and Publication of some New Celestial Charts, under the Superintendence and Direction of the Royal Academy of Sciences at Berlin.**

[The Council of the Astronomical Society are happy in being able to lay before the members, a plan which has been suggested for a minute survey of the heavens;—a grand desideratum in modern astronomy;—and, in fact, one of the principal objects for which this Society was originally established, and which it has constantly laboured to promote.

The plan, here alluded to, appears to have originated with M. Bessel, who has himself observed upwards of 32,000 of the smaller stars, situated between 15° north and 15° south declination. With a view to render the survey of this zone of 30° more perfect (so as to comprehend many other stars not yet observed by him or by any preceding astronomer), it is proposed that it should be divided into 24 equal parts; each part containing 1^h in AR. And that every person, who is disposed to take a share in the undertaking, should devote himself to a minute examination of all the stars situated in that portion of the heavens which may be allotted to him:—1°. by reducing to the year 1800 all the stars hitherto observed in that district; and laying them down on a chart of given dimensions:—2°. by inserting also on the same chart, from estimation by the eye, or from actual observation with an instrument, all the remaining

* This statement has been printed by the Astronomical Society of London for circulation among the members; and as the plan cannot be made too public, we reprint it.—*Ed.*

stars (to the 9th and 10th magnitude) that have escaped the observation of preceding astronomers.

In order to prevent any confusion in the distribution of these portions of the heavens, it has been thought proper that the whole plan should be placed under the superintendence and direction of the Royal Academy of Sciences at Berlin: and they have accordingly issued a Prospectus, giving a detail of the plan proposed. A copy of that Prospectus was forwarded to the Astronomical Society: but some of the parts requiring explanation, Mr. Herschel was requested to obtain further information on those points which appeared to be ambiguous. In reply thereto, M. Encke (the Secretary to the Academy) has addressed a letter to Mr. Herschel, which more fully and clearly developes the views of the Academy.

Translations of the prospectus and of the letter above alluded to are subjoined. And the Council of the Astronomical Society trust that, in thus giving publicity to the plan proposed, and circulating it amongst the members, it will be needless to add any arguments in favor of a proposal, which promises, much more fairly than any other that has yet been suggested, to accomplish so important a desideratum in modern astronomy.]

The modern celestial charts, by Flamsteed, Bode, and Harding, contain only those fixed stars whose places at the time of their publication were astronomically determined. The number of these, however, has gradually increased from 3000, (marked in Flamsteed's catalogue and the atlas founded on it,) to nearly 50,000 as given in the *Histoire Céleste* and in Piazzi's catalogue; the whole of which are marked in Harding's charts. Nevertheless, these celestial charts are very far from containing all the stars visible by means of the telescope; the number of which seems to be immense, or to increase without limit with the increased power of this instrument. Indeed we can never expect to obtain charts that are absolutely perfect; and if we aim at any degree of accuracy, it can only refer to the assumed limit of the magnitude or brightness of the stars.

Before the discovery of telescopes such a limit was fixed by the power of the eye, and the charts were capable of receiving a certain degree of perfection founded upon it. Flamsteed, however, although he added many new stars, remained far behind the perfection attainable even in his time: and it was probably the immensity of the number of the stars which prevented this great astronomer and his successors from attempting to perfect their charts beyond a certain limit, and induced them to remain contented with noticing only those stars that were astronomically determined; leaving many others unnoticed, which, although of equal brilliancy, had not yet been considered.

Nevertheless it is desirable that we should possess charts that may be perfect to a certain limit; and the more desirable the further this limit be removed. If we determine that limit by the smallest stars yet visible through one of Fraunhofer's comet-seekers of 34 lines aperture and a magnifying power of 10 times, (and which can be observed without difficulty by Reichenbach and Ertel's meridian circles, provided with Fraunhofer's telescopes of four inches aperture, in an illuminated field,) we shall seldom or never find a deficiency in the astronomical application of the charts, and shall obtain a result, the surpassing of which would not only be extremely difficult, but would be prejudicial for obtaining a general view, owing to the excessive number of stars which it would be necessary to introduce. But this detail being once attained, the charts will show us at once any thing new, on comparing any part of them with the heavens, provided the magnitude of the star be not less than the limit assumed. Besides the interest naturally attached to a more correct view of the heavens generally, and the facility thereby obtained for many astronomical observations, such charts would also offer the surest means of enlarging our knowledge of the solar system, by the discovery of new planets. Nay, such a result will be highly probable, whilst without such special celestial charts they can only be found by some lucky chance.

Indeed, there have been repeated attempts towards constructing charts of this description: and although they have not been crowned with success, it will be sufficient to enumerate the causes that have impeded their execution, in order to show that they are not now insuperable. The perfection of the celestial charts to a certain limit can only be attained by first laying down on a *net work*,* or scale, those stars that have been determined by meridional observations, in order that all the rest, intended to be introduced, may be added from estimation by the eye, perhaps assisted too by some instrument. By meridional observations alone, even if repeated more than once, we cannot acquire the certainty of having all the stars within the assumed limit. Even the *Histoire Céleste* contains much fewer stars than are necessary as a basis for perfect charts; wherefore it was necessary to make *de novo*, a more numerous series of meridional observations. Such a one has now been made at the observatory of Königsberg, extending over a circular zone of the heavens from $-15°$ to $+15°$ declination, and containing about 32,000 stars; which, according to an experiment made in a part of the

* [This *net work* is delineated on the copper-plate engraving which accompanied the original communication, and which was sent as a pattern. It consists of 100 small squares, formed of faint lines, half an inch (Eng.) asunder; each square comprehending a degree. It is formed on the plan, and on the same scale, as Harding's Atlas; and therefore it is unnecessary to give a specimen of it here. The plate itself is given in Schumacher's *Astron. Nach.* No. 88; and it may be seen by application to the Secretary of the Astronomical Society.—*Sec.*]

heavens most filled with stars, are quite sufficient. Besides this difficulty, now removed in a zone of 30° of declination, there is another, viz. the perfecting of the charts by the eye, which is so laborious and requires so much time, that a single individual can make but little progress in it. This may, however, be removed by the co-operation of several; and the active zeal now prevalent among astronomers allows us to indulge in the hope that many will assist in promoting so great and useful an undertaking.

It is therefore the wish of the Academy of Sciences to unite for this object the friends of astronomy; and to procure for them every possible facility. It invites all astronomers to assist in filling up the 24 sheets of a complete celestial atlas, for which the foundation has already been laid; viz. from − 15° to + 15° of declination and the 24 hours of right ascension: laying down at the same time the following rules to be observed in the execution.

1°. The net work, or scale, to consist of squares for the degrees of declination and right ascension: each degree measuring 5¾ Parisian lines (or 0·51 English inch). It should extend from 4 minutes of time before the beginning of an hour, to 4 minutes of time after its termination: and thus contain 510 squares.

2°. In this net work are to be marked the stars observed at Palermo, Paris, and Königsberg, reduced to the beginning of the year 1800.*

3°. The largest of them should be marked after the manner of the pattern sheet attached to the present plan: those stars which are visible only through a telescope, by larger and smaller black rings; and those visible by the naked eye, by the addition of rays.†

4°. If a star has been observed but once, the same should be marked by a short faint line projecting from one side of it; if twice, or more frequently, by two such lines, one on each side of it.‡ For stars visible to the naked eye, this kind of designation would lead to indistinctness, and is in fact needless, since they are all described in Piazzi's catalogue; and therefore show, by their rays, that they have already been observed.

5°. The sheets in this state must be compared with the heavens: and all the stars, within the limits proposed for the intended sheet, must be estimated by the eye, as correctly as

* [The stars observed at Palermo are given in Piazzi's Catalogue: those observed at Paris are given in the *Histoire Céleste:* and those observed at Königsberg are given in M. Bessel's Observations.—*Sec.*]

† [These marks are similar to those adopted by Mr. Harding in his charts. The exact mode of delineating the different magnitudes may be seen in the pattern sheet.—*Sec.*]

‡ [For specimens of this mode of distinguishing the different stars, see the pattern sheet, alluded to in the note in page 126.—*Sec.*]

possible, and be inserted therein. And it must be observed that the stars of the chart must be such as can be seen under favourable circumstances with one of Fraunhofer's comet-seekers of 34 lines aperture, and a magnifying power of 10 times.

6°. When stars stand too closely together to be separated in the drawing, their magnitude only need be delineated, and the number of them indicated by an equal number of lines underneath it, as in the pattern sheet, $19^h\ 29^m$ and + 11° 55′. Where two stars are found double stars, i. e. such as are not above 15″ or 20″ distant from each other, they should be distinguished by such distance being mentioned: ex. gr. at $19^h\ 52^m$ and + 10° 12′.*

7°. The sheet thus far advanced must be frequently compared with the heavens, partly for the purpose of discovering the changes that may have occurred during the drawing, and partly also for the purpose of finally fixing the magnitudes which the observer may be disposed to give to the stars. It will perhaps not be possible to notice in the drawing the minute distinctions between the magnitudes of the smaller stars marked on the pattern sheet, such as the 9th and (9.10th) magnitude, nor will it be essentially necessary.

One may be convinced by the pattern sheet (which represents one of the most starry parts of the heavens), that it is possible to attend to all these rules:† and that the great multitude of stars, marked upon it in the manner they are represented, neither crowd the space, nor render a general review difficult. To name and describe in such charts either the constellations and their limits, or single stars, would be both useless and injurious.

The Academy have appointed a committee, consisting of Messrs. Ideler, Oltmanns, Dirksen, Encke, and Professor Bessel of Königsberg. And whoever is disposed to undertake the execution of a sheet, should apply to any one of the members of that committee, who will point out to him a portion not yet undertaken by others. Such a district will remain open for him during two years: and if, after that period, he cannot show to the committee that he has made some considerable progress in it, it will be transferred to another.

As soon as any sheet is completed it must be sent to the committee; who, after having examined and approved of it, will cause it to be engraved and published, without waiting for any others. The name of the author will be engraved on it, and

* [For specimens of this mode of distinguishing the different stars, see the pattern sheet, alluded to in the note in page 126.—*Sec.*]

† [The greatest number of stars in any one of the squares in the pattern sheet is 16, and they are all perfectly distinct, even with the distinguishing marks attached to them.—*Sec.*]

any observations that he may have had an opportunity of making,—such as errors of the pen, or of the press, in lists of observations,—on stars observed, but no longer existing,—on variable stars, &c. &c.—will be published in the Memoirs of the Academy.

The Academy entertain no doubt that the fact of being able to promote, without any expensive apparatus, such a great and useful undertaking, as well as the prospect of discovering new planets even during the construction of the charts, will be sufficient to excite the friends of astronomy to participate in it. Nevertheless, it has been thought proper to announce a reward of 25 Dutch ducats for the author of every chart made according to the plan.

As the Academy enjoy the privilege of free postage within the limits of the Prussian post, astronomers in addressing the members of the committee, or in sending in their charts, may take advantage of this circumstance.

Berlin, 1*st November*, 1825.

Letter from M. Encke, to J. F. W. Herschel, Esq.

Berlin, *May* 19, 1826.

I hasten to answer the letter of the 29th April which you were so good as to send me. I set too great a value on the interest which the Astronomical Society takes in our plan, to delay for a moment giving you all the explanation that you wish for.

The principal object of the Academy is to procure a knowledge of the heavens as perfect as the present instruments will enable us to obtain. If in Flamsteed's time we might content ourselves with possessing maps of all the stars as far as the fifth and sixth magnitude, it appears that at the present period we cannot even limit them to those of the seventh and eighth magnitude, but ought to extend them, so as to include in the same sheet all the stars of the ninth magnitude. Or at least, the continual use we make of such stars renders it desirable to possess observations sufficiently correct of all the stars as far as the ninth magnitude inclusive. If we wish to observe such stars in the same manner as Lalande has done in his *Histoire Céleste,* or Bessel in his *Zones,* we could never be certain of having observed them *all,* their number being too great. It seems, then, that we should first of all endeavour to procure a knowledge of the whole of the above-mentioned existing stars, more detailed than that which can be obtained by an instrument fixed in the meridian. Afterwards we may propose to make on each of those stars the necessary observations, in order to assign more accurately its true place.

Such then is the object of the new astronomical maps. They are intended as a guide to future astronomers, whereby they may know, at one view, whether there exists a star that has never yet been observed. In this point of view these new maps cannot by any means render superfluous the Atlas already published by M. Harding, which contains all the necessary details to be able to distinguish exactly in what place of the heavens a comet or a new star is seen. But the different objects of these two maps require also a different arrangement. M. Harding has taken his stars from the *Histoire Céleste* and from other catalogues, and in the regions where the observations were not sufficiently numerous, he has made up the deficiency as well by his own observations as by drawings. We wish that, in the new maps, only those stars already observed should be noted, (viz. with one or two dashes,) which are found in books that are in the hands of every astronomer; and in order not to increase uselessly their number, we propose to limit those books to the following ones; 1°. The Catalogue of Bradley (Bessel Fund. Ast.) 2°. Piazzi's Catalogue (Palermo 1814); 3°. The *Histoire Céleste* of Lalande; 4°. Bessel's Zones. If a star shall be found in any two of these books we may be certain that it is a fixed star; if it is found in one only, it may be a planet or a moving star. It is therefore necessary that every one who wishes to take a part in this plan should also take upon himself to reduce, to the same epoch, the observations of the *Histoire Céleste* and of M. Bessel's Zones, in order to be able to decide whether a star is either the same, or has only been affected by a very remarkable proper motion. Fortunately this reduction will be found neither difficult nor long, by means of the Tables of Reduction that M. Schumacher has caused to be computed for the *Histoire Céleste*,* and by the help of those Tables that M. Bessel has adjoined to his Zones. I have no hesitation to assert, by my own experience, that I should be able in eight days to compute all the necessary reductions for a whole hour in AR: and that at the utmost 15 days would be sufficient for every case.

M. Bessel's tables of reduction give the formula

$$(1825)\ \text{AR} = t + k + k'(\delta - \text{D}) \times \cdot 01$$
$$(1825)\ \text{Decl.} = \delta + d + d'(\delta - \text{D}) \times \cdot 01$$

For example, Bessel gives for the 135th zone, the first of the ninth book,

		k		*k'*	*d*		*d'*	
4h	0	+0″288	63	+0″060	−56″30	4,73	3″68	D = +8°
	30	0,225		+0,062	−61,03		3,73	

whence, we have for the first five stars,

* [Sammlung von Hülfstafeln. Vol. ii.—*Sec.*]

δ−D	t	k	$k' \frac{\delta - D}{100}$	AR for 1825.
−17	3h 54m 28s43	+0s30	−0s01	3h 54m 28s72
+44	56 6,60	+0,30	+0,03	56 6,93
+ 2	56 45,02	+0,29	0,00	56 45,31
−16	57 28,00	+0,29	−0,01	57 28,28
− 9	58 44,20	+0,29	−0,01	58 44,48

δ	d	$d' \frac{\delta - D}{100}$	Dec. for 1825.
+7° 43′ 15″2	−55″44	−0″62	+7° 42′ 19″1
8 44 6,3	−55,70	+1,62	8 43 12,2
8 1 51,9	−55,79	+0,07	8 0 56,2
7 43 38,2	−55,91	−0,59	7 42 42,9
7 50 32,0	−56,10	−0,33	7 49 35,6

Now it is only required to subtract the precession from 1800 to 1825, which can be done by a small table with double entry, which any one may compute for himself.

For the stars in the *Histoire Céleste* M. Schumacher's tables will in the same manner enable us to reduce the observations at once to the epoch of 1800; so that it will not even be necessary in this case to compute the precession.

I hope, Sir, that these reductions which require only the addition of three numbers, will not appear to you either too long or too complicated. They comprehend at the same time all the corrections of the instrument, and of the apparent place; and a computer ever so little versed in such calculations will not find the application of it troublesome or tedious. The degree of accuracy is as great as may be attained by any other means; since nothing indeed has been neglected in it.*

It is the desire of the Academy that each astronomer should himself make these reductions, and that he should then place these observed stars on his chart; distinguishing (in the manner above mentioned) those which have been once or twice observed. This part of the work is in my opinion a great deal more difficult, and requires a more scrupulous attention than the computation, where the two columns of the values of k and d follow a regular order, and the other two columns, k' and d', have never much influence on the result. Each sheet will represent two thousand observed stars at least; every one of which will have its mode of delineation prescribed according to its magnitude and the number of observations. It will not be possible to commit this operation, which cannot even be easily verified, *to any other person than the astronomer himself;* who, by putting his name to the sheet, will render himself responsible for the accu-

* [For the convenience and accommodation of those persons who are disposed to take a share in this undertaking, the Astronomical Society have caused *skeleton forms* to be printed, by means of which much of the trouble and risk of error attending the reductions will be saved. Any number of these forms may be had, by application to the Secretary before the 1st of January next, after which day the press will be broken up.—*Sec.*]

racy of his work. It is highly probable that many errors, both of observation and of writing, will be made amongst that immense mass of stars which are observed only once. If the astronomer himself makes both the reduction and the drawing, he will be able to find out the cause of such errors more easily than if the whole were computed and arranged by another hand. In fact, the execution itself of the drawing will render the person who undertakes it so well acquainted with the region he describes, that it will very much facilitate to him the accomplishment of the remaining part of the work, which consists in noting down the stars (down to the 9th and 10th magnitude) not yet observed. I think that the reduction and the drawing of the stars already observed (made in such a manner that one may be certain that each star in the heavens corresponds to its place on the chart, which can only be obtained by making a revision of the heavens), is about half of the whole work, and that this is also the part that has the greatest influence upon the general accuracy of the whole.

These are the principal motives which have induced the Academy to propose the plan in the manner they have done in the Prospectus. The Academy could not, as a body, itself undertake so extensive a work, and thereby render itself in some measure responsible for its accuracy. These maps will form part of the Memoirs that are published by the Society. Each member will be answerable for his own portion, and the duty of the Academy can only be that of committing this work to persons who have already given proof of their being able to fulfil the task which they engage to undertake. It is on this account that you will find, in the Prospectus, that the Academy have determined that the name of each author shall be put on his map. This is the best proof that they do not mean to render themselves responsible for the correctness of the maps, as far as the authors are concerned; but that they intend only to defray the expenses,—to encourage astronomers by prizes,—to pay attention that a perfect conformity be kept up among the observers, to ascertain that every one who takes part in it, intends to accomplish the proposed object,—and lastly to superintend the engraving of the maps.

The Academy had conceived this project before my coming to this situation; but their arrangements appear to me so proper, that I cannot add any thing to them to insure more fully the approbation of astronomers. I hope indeed, besides the principal object, that the discovery of comets or even of some planet, and the opportunity that it will afford to many astronomers of acquiring a more complete knowledge of a portion of the heavens, will be some of the valuable results of this undertaking. On this account it has met with considerable approbation. The greatest part of the districts are ready for distribution, and the whole will probably be finished by the time assigned by the Academy for completing the work, viz. the 1st January, 1829.

I ought to apologize if I have been too prolix, and I hope you will ascribe it to the desire I have to insure also the approbation of the Astronomical Society. I am much flattered that you should have entertained the same opinion with me, as to supplying astronomers with sheets already prepared;—a method which, if it could be executed, would certainly be preferable. On my first coming here, and on being made acquainted with the views of the Academy, I thought it right to propose this idea to my fellow academicians; but having made trial of the time necessary for the execution of such a plan, I have been induced to alter my opinion. M. Harding's maps,—a work whose merit is perhaps not sufficiently known,—embrace nearly the half of the stars that have been at present observed; or perhaps about one-third: nevertheless they have occupied this industrious astronomer almost twenty years. Taking into the account that part of the heavens which is not comprised between $-15°$ and $+15°$, I believe that 10 or 12 years would elapse before one person, or even two co-operating for the same purpose, would be able to finish both the drawing and the engraving of the maps. My present employment does not allow me to apply exclusively to it, even if I had the confidence, which certainly I have not, that every thing would succeed well. The undertaking would in such case be put off so long that perhaps we could never be certain of finishing it. By dividing this work, however, into hours, we may hope that the honour and character of each astronomer that may take a share in it, will induce him to carry his own portion to the greatest possible degree of perfection. And if the uniformity in the drawings should not be so great as if a single person had carried on the whole, yet we shall gain in point of time: and likewise have the advantage of making a revision of all that part of the heavens in the course of two years,—a period very little longer than that which would be required to execute a fine engraving of the maps.

In No. 93 of the *Astronomische Nachrichten* published by M. Schumacher, there is a description of a machine which M. de Steinheil has tried and found very convenient and correct for marking the precise place of an observed star. If all the astronomers would make use of it, it would produce results having a great degree of conformity amongst the whole.

I remain, dear Sir, with the greatest consideration,

Yours, &c. J. F. Encke.

Article X.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1826. *Parts I. and II.*

(*Continued from* p. 60.)

III. *Observations on the Changes which have taken Place in some ancient Alloys of Copper;* by John Davy, MD. FRS.: in a Letter to Sir H. Davy, PRS.

An abstract of this paper will be found in the *Annals* for Dec. 1825, p. 465.

IV. *Additional Proofs of Animal Heat being influenced by the Nerves:* by Sir E. Home, Bart. VPRS.

V. *The Croonian Lecture.—On the Structure of a Muscular Fibre from which is derived its Elongation and Contraction;* by the same Author

The following extracts contain the substance of this lecture:

"As far back as the year 1818, while considering the mode in which coagulated blood is rendered vascular, I brought forward a magnified drawing of a muscular fibre made by Mr. Bauer, showing it to be composed of a single row of globules $\frac{1}{2000}$ parts of an inch in diameter, or, in other words, of red globules deprived of their colouring matter."

"In this former examination of muscular structure, that the integrant fibre might be more easily separated from the fasciculus to which it belonged, we had gone into the same error with those physiologists who have made diagrams of the internal appearance of the brain, after coagulation, and had boiled the muscle previous to the examination; not being aware that this process must decompose red globules, should any exist, and cause the colouring matter to be separated. Boiling would also destroy any connecting medium by which the globules are united together; so that, if I may use the expression, there would only be the skeleton of a muscular fibre remaining to be examined.

"Upon the present occasion, therefore, the fibres belonging to the fasciculi that compose the great muscle that lies upon the back of the bullock's neck, to raise the head, were selected, and were examined in 24 hours after the animal was killed; and we know that in all violent deaths, the muscular fibres continue capable of contraction beyond that period, after apparent death has taken place.

"In this muscle the fasciculi are more loosely connected together than in almost any other animal body; and in the interstices between them there is no fat; but Mr. Bauer found that in this recent state the fibres are held so firmly together by the mucus which surrounds them, and forms them into fasciculi, that it was only under water he could separate an integrant fibre for examination in the field of the microscope.

"In its mechanism, he found it to correspond with the nervous fibre of a ganglion, differing only in the size of the globules, which were larger than those of the fibre in the ganglion in the proportion of $\frac{1}{2000}$ parts of an inch to $\frac{1}{3000}$ and $\frac{1}{4000}$ parts.

"The elastic transparent jelly uniting the globules together, had not the same elasticity as in the nervous fibre, so that it could not be drawn out from the contracted state to double its length without breaking.

"The muscular fibre of a trout was treated in the same way, and the result was the same; the fibres were however more brittle than those in the bullock's neck.

"From these facts, in addition to those communicated in the examination of the structure of ganglions, it is at last ascertained, that the structure of the fibres of nerves in general, and those peculiar to ganglions, as well as those that compose muscles, is so far the same, that they consist of single rows of globules united together by an elastic gelatinous transparent matter; they differ however in the size of the globules, and the degree of elasticity of the medium by which they are united; so that a less power will elongate a nerve than the fibres of a muscle, and to a greater extent, and it will restore itself with more velocity to a state of rest.

"This structure of nerves and muscles, I consider to be demonstrated in the annexed drawing; since I cannot believe Mr. Bauer has been led into any error upon this occasion; as no error has been detected in his microscopical observations for so many years continued, and the accuracy of his representations, of what he has seen, no one can doubt.

"It is a curious confirmation of the acuteness of his eye, and the accuracy of his glasses, that Leuwenhoek, who used a single microscope, and says it is the best that can be made, since the magnifying glass is the smallest speck that can be seen, declares a muscular fibre to be made of globules less than the red globules of the blood; and Dr. Monro of Edinburgh, who published his microscopical observations on nerves and muscles, in the year 1783, made chiefly in the solar microscope, goes so far as to consider muscular fibres to be the continuation of nervous fibres, and gives an engraving of the mode in which the one terminates, or is lost in the other. Dr. Monro, it is evident, had never seen a single fibre either of a nerve or muscle, only fasciculi of them, and found them so much alike as to be led to consider them the same. Both Leuwenhoek and Monro, from the want of a micrometer, were left to guess at relative dimension, and in such guesses were often very unsuccessful.

"The globules in the nervous fibre being smaller than in the muscular, oversets Monro's theory of their being the same; but that both authors, with means so very inadequate to those employed by Mr. Bauer, should have made such approaches to the truth, is highly creditable to them, and must prove highly satisfactory to Mr. Bauer, as well as to the public."

VI. *An Account of the Heat of July*, 1825; *together with some Remarks upon sensible Cold;* by W. Heberden, MD. FRS.

Some particulars of this communication will be found in the *Annals* for February last, p. 138; and we subjoin the remarks on the estimation of sensible cold with which the paper concludes.

"I am tempted to add to the above some other observations, which, if they are not immediately connected, are not entirely unconnected with this subject; for it cannot have escaped the attention of any person moderately conversant with natural philosophy, that the index of a thermometer is a very imperfect measure of what I may call the *sensible cold*, that is, of the degree of cold perceptible to the human body in its ordinary exposure to the atmosphere. For while the thermometer truly marks the temperature of the medium in which it is placed, the sensations of the body depend altogether upon the rapidity with which its own heat is carried off. And this is by no means confined to the actual temperature of the air; but whatever alteration of quality increases its power of conducting heat; and, above all, whatever currents increase the succession of its particles in contact with the body, the same will increase the sensation of cold. Hence it is, that in very hot weather, the same stream of air which would heat a chamber, will nevertheless be cool to the feeling; on the other hand, when the thermometer was more than 80° below the freezing point, Captain Parry observed, that while the air was still, the cold was borne without inconvenience.

"It therefore occurred to me, that the proper way to estimate the *sensible cold*, would be, first to raise a thermometer to a height something exceeding the natural heat of the human body, and then to observe at what rate the quicksilver contracted upon exposure to the air. For this purpose I used a thermometer with a very small bulb, which might show the alteration of heat in a short time. This I held to the fire till it rose to about 120°, and then carried it in a warm glove into the open air. I had with me an assistant with a watch in his hand: and as soon as the mercury had descended to 100°, he began to count the seconds, while I continued to observe the thermometer, marking the degree of heat at the end of every ten seconds during half a minute. The result rather exceeded my own expectations; and (being, as far as I know, the only experiments of the kind,) I have thought the Society might not dislike to be made acquainted with them.

"The circumstances that particularly engaged my attention were wind, and moisture. With these views the following experiments were made, and verified by repeated trials.

"*Exp.* 1.—1821, January 3. A strong east wind. The temperature of the air 31°.

"The thermometer in this, and all the experiments, being previously raised to 100°, in the manner before-mentioned, the descent of the mercury from that point was observed as follows:

After 10″	it was 78°	Decrement	22°
20″	—— 60°	————	18°
30″	—— 52°	————	8°

"By the decrements, it is to be understood the descent in each successive ten seconds. This is added, because I consider it as the proper measure of the *sensible cold*, so long as the thermometer retains a heat approaching to that of the human body.

"*Exp.* 2.—1821, January 4. No perceptible wind. The temperature of the air 30°, the atmosphere hazy.

After 10″	therm. 89°	Decrement	11°
20″	—— 80°	————	9°
30″	—— 71°	————	9°

"*Exp.* 3.—1821, February 10. A strong east wind. Temperature of air 47°. The atmosphere clear, with sunshine.

After 10″	therm. 82°	Decrement	18°
20″	—— 73°	————	9°
30″	—— 64°	————	9°

"*Exp.* 4.—1824, Jan. 9. A cold fog. No wind. Temperature of the air 37°.

After 10″	therm. 92°	Decrement	8°
20″	—— 85°	————	7°
30″	—— 79°	————	6°

"The most superficial view of these experiments shows the prodigious effect of wind to increase the *rate of cooling*, which, I apprehend, constitutes *sensible cold;* so that in experiment 3, though the thermometer suspended in the open air was 17° higher than in experiment 2, yet the *sensible cold* was very considerably greater; but when there was no wind, even a wet fog did not much, if at all, increase it. This, which at first sight may appear contradictory to experience, is not, I believe, really so; for though the power of such air to carry off the heat of the body be indeed increased, yet so long as we remain at rest, we are in great measure unaffected by it; so much the effect of wind exceeds that of mere moisture. It is by walking, or riding, in such a state of the atmosphere, that we produce on our bodies a current of moist air, which is then felt in proportion to the rapidity with which we pass through it. If it were thought worth while to bring this to the test of the thermometer, the instrument should be made to pass through the air at the same rate as the person would move."

VII. *On the Transit Instrument of the Cambridge Observatory, being a Supplement to a former Paper;* by Robert Woodhouse, Esq. Plumian Professor of Astronomy in the University of Cambridge.

VIII. *Account of a Series of Observations made in the Summer of the Year* 1825, *for the Purpose of determining the Difference of Meridians of the Royal Observatories of Greenwich and Paris;* drawn up by J. F. W. Herschel, Esq. MA. Sec. RS.: communicated by the Board of Longitude.

The following is Mr. Herschel's account of the manner in which these observations were made:

"Operations having been carried on to a considerable extent in France, and other countries on the Continent, for the purpose of ascertaining differences of longitude by means of signals, simultaneously observed at different points along a chain of stations; and the Royal Observatory at Paris, in particular, having been connected in this manner with a number of the most important stations, it was considered desirable by the French government that the Royal Observatory at Greenwich should be included in the general design. The British Board of Longitude was accordingly invited to lend its co-operation towards carrying into effect a plan for that purpose; and the invitation being readily accepted on their part, I was deputed, in conjunction with Captain Sabine, in the course of the last summer, to direct the practical details of the operation on the British side of the channel, and to make the necessary observations. Every facility was afforded us in making our dispositions, on the part of the different branches of His Majesty's government to which it was found necessary to apply. A detachment of artillery was placed, by his Grace the Duke of Wellington, Master General of the Ordnance, under the orders of Captain Sabine. Horses, waggons, and men, were furnished for the conveyance of a tent, telescopes, rockets, and other apparatus; and four of the chronometers belonging to the Board of Admiralty were placed at our disposal. The rockets required for making the signals were furnished us from France. It would have been easy, doubtless, to have procured them from the Royal Arsenal at Woolwich; but on the representation of Colonel Bonne, to whom the principal direction of the operations in France was intrusted, it was thought more advisable to accept an offer made to us of any number which might be required, prepared at Paris expressly for similar operations, carrying a charge of eight ounces of powder, the instantaneous explosion of which, at their greatest altitude, was to constitute the signals to be observed.

"Our previous arrangements being made, on the 7th of July I left London; and after visiting the station pitched upon at Wrotham, which was the same with that selected by Captain Kater and Major Colby, as a principal point in their triangulation in 1822; and finding it possessed of every requisite qualification for the purpose of making the signals, from its commanding situation, being unquestionably the highest ground between Greenwich and the coast, proceeded to Fairlight Down, near Hastings, where I caused the very convenient observatory tent, belonging to the Board of Longitude, to be pitched immediately over the centre of the station of 1821, which was readily found from the effectual methods adopted by the gentlemen who conducted the trigonometrical operations in that year, for securing this valuable point. Here, on the 8th, I was joined by Captain Sabine, who, it had been arranged, should proceed to the first observing station on the French side of the Channel, there to observe, in conjunction with Colonel Bonne, the signals made on the French coast, and those made at the station of Mont Javoult; which latter were to be observed immediately from the observatory at Paris; while, on the other hand, it was agreed that M. le Lieutenant Largeteau, of the French corps of geographical engineers, should attend at Fairlight, on the part of the French commission, and observe, conjointly with myself, the signals made at La Canche, the post on the opposite coast (elevated about 600 feet above the sea, being nearly the level of Fairlight Down) and also those to be fired from Wrotham Hill, which were expected to be immediately visible from a scaffold, raised for the purpose on the roof of the Royal Observatory of Greenwich. By this arrangement, and by immediate subsequent communication of the observations made at each station, it was considered that the advantage of two independent lines of connexion, a British and a French, would be secured between the two extreme stations; i. e. the two national observatories; every possibility of future misunderstanding obviated, and all inconvenience on either side, arising from delay, or miscarriage in the transmission of observations, be avoided.

"With the assistance of Captain Sabine, and by the help of exact information as to the azimuths of Wrotham and other nearer stations in the triangulation of 1821, with which Captain Kater had obligingly furnished us, and of which Fairlight Church proved the most convenient, being close at hand and favorably situated, and easily visible in the twilight; and from the previously calculated azimuth of La Canche (114° 30′ E.); four night glasses by Dollond, provided at the order of the Board of Longitude expressly for this operation, and which I had caused to be fixed on posts firmly driven into the ground beneath the tent, were then pointed, two on the station of La Canche, and two on that of Wrotham Hill. Those directed to the former were of four inches clear aperture, the others of three. In case of any difficulty arising as to the pointing, I had taken care to provide myself with an excellent eight-inch repeating theodolite, on the Reichenbach construction, by Schenck, of Berne; but it was found unnecessary to use it, as the night glasses were purposely constructed with an azimuthal motion, and a rough graduation read off by an adjustable vernier, so as to allow their being set at once a few minutes before the observations commenced, by taking Fairlight steeple as a zero point; a circumstance which proved exceedingly convenient, as it allowed of their being dismounted after each night's observations, and removed to a place of security; and thus rendering it unnecessary to harass our small party by keeping guard in our absence.

"On the night of the 8th I had directed blue lights to be fired at Wrotham, as a trial of the visibility of the stations, or rather as a verification of the pointing of the telescopes; for on the former point there could be no doubt, the station at Wrotham being situated precisely on the edge of the escarpment of the chalk which borders the Weald of Kent, and having been actually connected with Fairlight by direct observation, while no obstacle but a low copse wood, over which it might fairly be presumed that no rocket would fail to rise, separated it from a direct view of Greenwich, at about 20 miles distance. Either from haze in the atmosphere, or from the too great distance, nothing was seen that night or the next; which however caused no uneasiness, as we could depend on our instruments and information. The next morning Capt. Sabine quitted Hastings, and joined Col. Bonne, at his post, on the morning of the 10th, the day appointed for the commencement of the observations; meanwhile I was joined by M. Largeteau, who remained with me the whole time of their continuance, performing every part of a most scrupulous and exact observer, as the observations herewith communicated will abundantly testify.

"The observations were continued during 12 nights, 10 signals being made at each rocket station every night. The weather throughout the whole of this time was magnificent, and such as is not very likely to occur again for some years; a circumstance of the last importance in operations of this nature, where lights are to be seen across nearly 50 miles of sea, and also by reason of the verification of the sidereal times at the observatories by transits. One night only a local fog deprived us of the sight of 13 out of the 20 signals; but on the whole, out of 120 made at Wrotham, no less than 112 were seen from Fairlight (about 40 miles) and 89 from Greenwich; while out of the same number made at La Canche, 93 were observed at the former post. I am sorry to add, however, that owing to a combination of untoward circumstances, which no foresight or exertion on the part of Capt. Sabine or myself could possibly have led us to calculate on, or enabled us to prevent, and which the most zealous endeavours on that of Col. Bonne failed to remedy, no less than 8 out of the 12 nights' observations were totally lost, as to any result they might have afforded, and the remainder materially crippled; so that a much more moderate estimate of the value of our final result must be formed, than would otherwise have been justified. Still it is satisfactory to be able to add (such is the excellence of the method), that a result on which considerable reliance can be placed, may be derived from the assemblage of the observations of these four nights; and when it is stated that this result appears not very likely to be the tenth of a second in error, and extremely unlikely to prove erroneous to twice that amount, it will perhaps be allowed that, under such circumstances, more could hardly be expected."

The difference of the meridians by these observations is $9^{m}\ 21^{s}\cdot6$.

IX. *Observations on the Poison of the Common Toad;* by John Davy, MD. FRS.

For an abstract of this paper, see *Annals* for Feb. p. 137; and for some remarks on the subject of it, see the number for April, p. 277.

X. *On the Magnetizing Power of the more refrangible Solar Rays;* by Mrs. M. Somerville: communicated by W. Somerville, MD. FRS.

A report of the contents of this paper will be found in the *Annals* for March, p. 224.

XI. *On the Mutual Action of Sulphuric Acid and Naphthaline, and on a new Acid produced;* by M. Faraday, FRS. &c.

We shall probably give this paper in a future number; in the mean time we may refer to that for March, p. 226, for some account of the facts it describes. E. W. B.

(*To be continued.*)

Article XI.

Proceedings of Philosophical Societies.

ASTRONOMICAL SOCIETY.

May 12.—A paper, by the Astronomer Royal, was read, containing an explanation of the method of observing with the two mural circles, as practised at present at the Royal Observatory. The principal object of the method explained in this paper is to diminish as much as possible the inaccuracies occasioned, even in the most perfect instrument, by rapid and partial changes of temperature. In the Greenwich system of observations, assistance from the spirit-level or plumb-line, or indeed from any previous verification, is rejected altogether. Two circles are employed simultaneously, each of which is furnished with six microscopes, which it is desirable should be placed at *nearly* equal distances on the limb; and previous to observation each circle is placed *nearly* in the plane of the meridian, and *nearly* perpendicular to the horizon. Each circle is provided with an artificial horizon of mercury, so as to command the greatest possible portion of the reflected meridian.

The first part of the process consists in observing a number of stars simultaneously with each instrument, either by direct, or by reflected, vision: the object of this is to determine the exact quantity that one instrument marks more or less than the other, when both are directed to the same object. This is determined, not by a single observation, but by a great variety; thus obtaining the quantity denominated *the mean difference* for every 24 hours.

In the second part of the process, a series of stars is observed *reciprocally*, that is, the direct image of a star by one instrument, at the same time that its reflected image is observed by the other. This, combined with the results of the previous process, in which the *mean difference* serves the same purpose as the index error in Hadley's sextant, enables the observer to ascertain the altitude; with which is likewise obtained the knowledge of the position of the horizontal diameter of each instrument. The observer, however, does not rest contented with a single determination of one diameter; but must in a similar manner, from altitudes, observed on various points of the arc, and by taking sometimes the direct, and sometimes the reflected, observation with the same instrument, endeavour by every possible variety to obtain the maximum of precision of which the method is capable.

The position of the horizontal diameter of each instrument being thus deduced from a mean of all the preceding experiments, sufficient data are obtained for computing the places of those stars that have been observed in the first part of the process, and employed in computing the mean difference; because, without the knowledge of the position of their horizontal diameters, the instruments, with respect to the stars in question, give nothing but differences of declination, but such position being known, their altitudes can be accurately determined.

The Astronomer Royal terminates his paper by pointing out the principal advantages of the method described.

There were next read Extracts of three letters addressed by M. Gambart, Director of the Observatory of Marseilles, to James South, Esq. respecting the discovery and elements of the orbit of a comet, supposed to be the same with that, or those, of 1772 and 1805. M. Gambart first presents the summary of his observations of this comet from the 9th to the 21st (inclusively) of March this year. He then exhibits the elements as computed from these observations upon the parabolic hypothesis: viz:

Passage of the perihelion, March 1826, 18,94 days, counting from midnight.

Perihelion distance	0·961
Long. perihelion	104° 20′ 0″
Long. ascend. node	247 54 10
Inclination	14 39 15

Motion direct.

These elements were communicated March 23rd:—a week after, the elliptic elements deduced from the same observations were transmitted, and are as follow: viz.

Passage of the perihelion, March 1826, 19,5998 days, counting from midnight.

Semi-axis major	3·567
Excentricity	0·74187
Log. mean motion	2·7326487
Long. perihel	108° 54′ 19″
Long. asc. node	249 55 23
Inclination	13 50 47
Motion direct.	
Periodic time	6·567 years.

The same elements, M. Gambart observes, represent almost exactly the observations of the comets of 1772 and 1805; whence the identity of all three is inferred.

The reading of Mr. Herschel's paper on Double-stars, commenced at the last meeting, was continued.

LINNEAN SOCIETY.

Feb. 7.—The following papers were read:

A Description of the *Plectrophanes Lapponica*, a Species lately discovered in the British Islands; by P. J. Selby, Esq. FLS. MWS. &c.

Some Account of a Collection of Cryptogamic Plants formed in the Ionian Islands, and brought to this country by Lord Guildford; by R. K. Greville, LLD. FRSE. &c.

Feb. 21.—The reading of Dr. F. Hamilton's Commentary on the Fourth Part of the Hortus Malabaricus, was commenced.

March 7.—The reading of Dr. Hamilton's Commentary was continued.

March 21.—A paper was read, entitled, Descriptions of Two new Birds, belonging to the family *Phasianidæ;* by Major

General Hardwicke, FLS.; also, a Description of a New Genus, belonging to the Natural Family of Plants, called *Scrophularinæ;* by Mr. David Don, Librarian LS.; and, a Review of the Genus *Combretum;* by Mr. George Don, ALS.

April 4.—The following papers were read:

On Dichotomous and Quinary Arrangements in Natural History, by Henry Thomas Colebrooke, Esq. FRS. FLS. &c. The learned author states that what has been called the dichotomous arrangement of nature can only be represented on a superficies: whereas, the affinities of natural objects ramify in *every* direction, and cannot therefore be correctly represented on a *plane* surface. He then shows that that distribution which, taking one central or interior group, makes only a few equidistant exterior ones, is necessarily quinary. The centre of the exterior groups will represent the solid angles of a tetrahedron within a sphere of which the centre is the middle point in the interior group. He finally observes, that although the tendency to a quinary arrangement in natural history has hitherto been chiefly developed in zoology, yet the same principle may be recognised in botany.

On *Boswellia,* and certain Indian *Terebinthaceæ;* by the same author.

April 18.—The reading of Mr. Colebrooke's paper On *Boswellia, &c.* was concluded; and a paper was read, entitled, Observations on a Species of *Simia,* Linn. now alive in the collection at Exeter Change, allied to, if not identical with, the *Simia Lagothrica* of Baron Humboldt; by Edward Griffiths, Esq. FLS.

May 2.—A paper was read, On the Locusts, (*Gryllus migratorius,* Linn.) which devastated the Crimea, and the southern provinces of Russia, in 1824; by J. Smirnove, Esq. FLS. Secretary to the Russian Embassy.

Also, a paper On Indian *Annonaceæ;* by H. T. Colebrooke, Esq. FRS. LS. &c.

May 24.—On this day, being the birth-day of Linnæus, the Anniversary Meeting of the Society was held, when the following Fellows were chosen Officers and Council for the ensuing year.

President.—Sir James Edward Smith, MD. FRS. &c.

Vice Presidents.—Samuel, Lord Bishop of Carlisle, LLD. VPRS. FAS.; A. B. Lambert, Esq. FRS. AS. and HS.; W. G. Maton, MD. FRS. and AS.; and Edward Lord Stanley, MP. FHS.

Treasurer.—Edward Forster, Esq. FRS. and HS.

Secretary.—James E. Bicheno, Esq. FGS.

Assistant Secretary.—Richard Taylor, Esq. FSA. Mem. Asiat. Soc.

Council.—Charles Bell, Esq. FRSE.; John Bostock, MD.

FRS. and HS. PGS.; Robert Brown, Esq. FRS.; Charles König, Esq. FRS.; Rev. Thomas Rackett, MA. FRS. and AS.; Sir Thomas Stamford Raffles, Knt. LLD. FRS. AS. and HS.; Joseph Sabine, Esq. FRS. and AS.; Nicholas Aylward Vigors, Esq. MA. FRS.

June 6.—A paper was read on a new Genus of Insects, named Oiketicus; by the Rev. Lansdown Guilding, BA. FLS.

Also a paper on Methods and Systems in Natural History; by J. E. Bicheno, Esq. Sec. LS.

June 20.—The following papers were read:

Concise Notice of a Species of Ursus from Nîpal, a skin of which has been presented to the Linnean Society by H. T. Colebrooke, Esq. FR. and LS. &c.; by Thomas Horsfield, MD. FLS. The new species of bear partially described in this notice, appears to be nearer in affinity to the brown European species than to the tropical bears, from which the sub-genera *Prochilus* and *Helarctos* have been formed.

Description of a new British Freshwater Helix; by the Rev. Revett Sheppard, MA. FLS.

Of the term *Oistros*, or *Œstron*, of the ancients, and of the real Insect intended by them in this Expression; by Bracy Clarke, FLS. &c.

It is affirmed in this communication, contrary to the opinion maintained by Mr. W. S. Macleay, and noticed in the *Annals* for May, 1824, that the *Œstrus* of Linnæus, and not his *Tabanus*, is the true *Œstron* of the Greeks and *Asilus* of the Romans.

The Society then adjourned until the 7th of November next.

GEOLOGICAL SOCIETY.

June 16.—A paper was read, entitled, "Notes on the Geological Structure of Cader Idris," by Arthur Aikin, Esq. FGS.

The author, after describing the outline of this mountain-ridge, details the relative altitude and position of the different heights, the situation of the summit overlooking the crater (in the bottom of which lies "the Goat's Pool") and the various faces and slopes of the mountain.

Mynydd pen y Coed, the highest hill which stands out on the southern slope, is found to consist of beds of bluish grey slate, very regular, rising NE by N, at an angle of about 35°, but bending up sharply at the NE end so as to increase the angle to about 50°. The successive subjacent beds, which occupy the ground to the edge of the crater, are found to consist of greywakké, compact splintery quartz with crystals of pyrites, and, in parts, ochry and cellular, and quartz-rock, differing from the preceding only by being more vitreous; which last rest on a blueish grey quartz, rendered porphyritic by a few crystals of felspar. These beds all rise NE by N, but their angle of elevation is continually increasing, and the last forms the summit of Craig y Cae.

From hence to the margin of the crater the space is occupied by alternations in nearly vertical beds of soft glossy slate, of coarse slate with ochry spots and small cells, of greywakké, of porphyritic quartz, and slaty potstone. About the middle of the series is a single bed of brownish grey rock, appearing to be ferruginous quartz intimately mixed with carbonate of lime.

The next bed, forming part of the summit of Cader Idris, composed of globular concretions, very hard, containing specks of pyrites, and melting in very thin shivers into a black glass, is supposed to be a trap-rock.

After minutely detailing the other beds of Cader Idris, their position and angles, the author proceeds to a mountain (forming the northern boundary of the little valley wherein the Goat's Pool and another small lake are situated), extending for about two miles parallel with Cader Idris. This he calls "the Stony mountain." It is composed of rounded tubercular crags and hemispherical bosses of trap, like enormous ovens, rising group above group. Their surfaces are comparatively smooth, and generally reticulated with veins of quartz, which sometimes occurs in areas four or five yards across, several inches thick, of an obscurely slaty structure, and adhering to the surface of the trap. Many of the groups when seen in profile appear to be of a very irregular and thick slaty structure, but, when visited in front and looking down upon them, are evidently clusters of columns laterally aggregated, and intersected by oblique irregular joints.

The large quarry of sienite on the Tawyn road is noticed as showing the connection of the trap and of the stratified rocks, and this is also shown in a very interesting manner on the descent northwards from Grey Graig, the eastern extremity of Cader Idris.

From these and other facts detailed in his paper the author considers it evident that Cader Idris, and the ground between that mountain and the Mawddoch, as well as the northern boundary of the valley, consist of various well known transition rocks, rising in general N. by E. or W.—that the beds both at the northern and southern extremities are at low angles, not greater than 20°,—that the intermediate beds are at high angles, approaching to vertical,—that they rest upon and are interrupted by trap-rocks more or less columnar,—that the trap-rocks are surrounded in many places by mantle-form strata, which in some instances are obviously of the same materials as the trap, and differ only in structure, but which sometimes bear a less obvious resemblance to the trap, and from exhibiting a transition from that to the rocks that compose the regular strata, are probably the latter, more or less changed by contiguity with the trap.

WERNERIAN NATURAL HISTORY SOCIETY.

At a meeting of the Wernerian Natural History Society towards the close of last year, a letter from Mr. Meynell, of Yarm, Yorkshire, was read, on Changing the Habits of Fishes, and mentioning that he had, for four years past, kept the smelt or spirling (*Salmo Eperlanus, Lin.*) in a fresh water pond, having no communication with the sea, by means of the Tees or otherwise, and that the smelt had continued to thrive and breed as freely as when they enjoy intercourse with the sea.

At the sitting of the same society on the 14th January, 1826, Dr. Fleming, of Flisk, exhibited a specimen of the migratory pigeon of North America, shot in Fife on 31st December last, and showed, from the perfect state of the plumage, that the animal had not been in a state of confinement, but had probably been wafted across the Atlantic by strong and continued westerly gales.—(Edin. Phil. Journ.)

MEDICO-BOTANICAL SOCIETY.

April 14.—Sir James M'Gregor delivered an address to the members of the Society on being elected President.

A communication was read on the different species of Hellebore used in medicine, and on its use in maniacal cases.

May 12.—A paper entitled, "Remarks on the Bitter Principle existing in the Fruit of Laurus Persea, and on its Use as a tonic Medicine by the Natives of Demerara;" by J. Frost, Esq. FSA. FLS. Director, was read.

June 9.—A collection of specimens of the plants enumerated in the Pharmacopœa list was presented by W. Anderson, Esq. FLS.

Mr. Frost delivered a lecture on the properties of Aconitum Napellus and Conium Maculatum, and their narcotic principles.

July 14.—This being the last meeting of the Society during the present session was numerously attended, and, after the ordinary business had been gone through, a paper, entitled "A Catalogue of Plants indigenous to Switzerland," by J. P. Yosy, Esq. was read. Notice was given from the chair that communications for the gold and silver medals must be sent in before the 1st of December.

The Society then adjourned to the 13th of October.

E. W. B.

Article XII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Crystallization of Sulphur.*

The peculiar arrangement of the crystals of ice in a case of hoar frost, where every crystal appeared as if it had endeavoured to recede as far as it could from the neighbouring crystals, has been observed and described by Dr. M'Culloch, at page 40, vol. 20. of this Journal. A similar effect may be pointed out as exhibited in crystallized sulphur. The man who melts and purifies the sulphur at the gunpowder works at Waltham Abbey is very expert in introducing wires or wooden forms into the melted sulphur, which, acting as nuclei, cause a crystallization of sulphur as the whole cools, and then, by letting out the liquid portions, the substances introduced are found covered with acicular or prismatic crystals, at times an inch or more in length. In this way he forms letters, names, and the figures of animals, &c. In all these cases the arrangement noticed by Dr. M'Culloch may be observed; and wherever an angle occurs the convergence of the crystals is very striking and beautiful.—(Journal of Science.)

2. *Meconiate of Morphia.*

Dr. Menici has obtained this substance as a simple educt from opium; the following is the process:

Pour distilled water on powder of good opium, placed on a paper filter, gently stirring them. Wash the opium in this manner until it come through colourless; then pass alcohol somewhat diluted through it; and, when it runs colourless, dry the insoluble portion in the dark. In this state digest it with heat in alcohol of 36° (B) for a few minutes; the solution when cold will deposit crystals of a light straw colour. From 12 drachms of opium 20 grains of this crystallized meconiate of morphia will be obtained. *Giornale di Fisica.*—(Dublin Phil. Journ.)

3. *On the Use of Common Salt and Sulphate of Soda in Glass-making.*

Muriate of soda and sulphate of soda may be employed, and at times with advantage, in glass-making. A casting is readily obtained of very fine glass, having, when about three or four lines in thickness, a very slight green tinge. Its composition is as follows: decrepitated muriate of soda, 100 parts; slaked lime, 100; sand, 140; clippings of glass of the same quality, from 50 parts to 200. Sulphate of soda likewise offers a great economy in its employment. The results are very satisfactory. The glasses made of this salt were of a very fine quality. The following is the composition: dry sulphate of soda, 100 parts; slaked lime, 12; powdered charcoal, 19; sand, 225; broken glass, from 50 to 200. These proportions give a rich coloured glass, which may be employed with advantage in glass houses, where a fine quality is sought after. The following is the second way of operating with sulphate of soda; the proportions may be as follow: dry sulphate of soda, 100 parts; slaked lime, 266; sand, 500; broken glass, from 50 to 200. According to this process, it is obviously easy to operate in a regular manner, and to avoid expensive trials in the manufacture. *Leguay, Annales de l'Industrie Nationale.*—(Edin. Phil. Jour.)

4. *Inspiration of Hydrogen.*

Signor Cardone, after having emptied his lungs as much as possible of common air, inspired 30 cubical inches of hydrogen at two inspirations. An oppressive difficulty of breathing, and a painful constriction at the superior orifice of the stomach came on, followed by abundant perspiration, tremor of the body, heat, nausea, and violent headache. Vision was indistinct, and a deep murmur confused his hearing.

These symptoms shortly disappeared, except the heat, which increased so as to excite considerable apprehension, but soon gave way to the use of cold drinks. He was speedily recovered. *Giornale di Fisica.*—(Dublin Phil. Jour.)

MISCELLANEOUS.

5. *Intelligence from the Land Arctic Expedition, under Captain Franklin and Dr. Richardson.*

The following contains an interesting statement of the progress of the Land Arctic Expedition under Captain Franklin and Dr. Richardson, up to September last, which is the latest information from the travellers:—

"We have travelled incessantly since we left lake Superior. We overtook our boats, which, with their crews, left England in June, 1824, eight months before us, about half way to this place, or four or five days march to the southward of Mathye Portage. We embarked in them at Chepewyn on the 20th July, and arrived in Mackenzie's River on the 31st. At Fort Normans Dr. Richardson separated from the rest of the party. Captain Franklin and Mr. Kendal went down the river to the sea in one boat, whilst Dr. Richardson brought the others and their cargoes up Bear Lake River, which falls into the Mackenzie a few miles below Fort Normans. Franklin made a prosperous voyage, and on the 16th of August, exactly six months from the day he sailed from Liverpool, had an extensive view from the summit of Garry's Island of the open sea, clear of ice, with many black whales, belugas, and seals, playing about. The water at

Whale Island is, as Mackensie states in this chart, fresh; but a few miles from Garry's Island, which is thirty miles to seaward, and out of sight of the other, it changes its colour and taste. The mighty volume of water which rolls down the Mackenzie, carries shoals of sand, and a brackish stream a long way out. Captain Franklin did not join Dr. Richardson and his party before the 5th September last, at Fort Franklin, in Bear Lake, the navigation up the river being tedious, from the strength of the current. The Sharpeyes or Quarrellers of Mackenzie, who inhabit the lower parts of the river, resemble the Esquimaux a good deal in their manners and language, and that part of the tribe who live nearest the sea were partially understood by our Esquimaux interpreter. The Esquimaux being at this season inland, hunting the rein-deer, were not seen, but the Sharpeyes have promised to give them notice of our intended voyage next year. Every thing at present promises success to our future operations. The boats sent out from England answer admirably, and we are well provided with stores for the voyage. During Captain Franklin's absence Dr. Richardson surveyed this lake, which is about 120 miles long, extending from lat. 65° 10′, long. 123° 29′, where Fort Franklin is built, to lat. 67°, long. 119°, within 70 miles of the nearest bend of the Coppermine River, and about 85 miles from its mouth. Garry's Island lies in lat. 69° 29′, long. 135° 42′, about 450 miles from the mouth of the Coppermine, and about 600 from Icy Cape, distances which may easily be accomplished, even during the short period that the Arctic Sea is navigable for boats, if no greater obstacles occur than were visible from the mouth of Mackenzie's river. A canoe is to be deposited at the north eastern arm of this lake, by which the eastern party will save 200 miles of land journey on their return. But a very cursory view of the rocks was taken in the voyage down the river, as was to be expected from the rapidity with which the party travelled. The oldest rocks met with were in the portions of the rocky mountains which skirt the river, and which are composed of transition limestone. From that there is a very complete series of formations down to the new red sand stone, exposed in various parts. The rocks of the coal formation are particularly interesting, from the strong resemblance the organic remains found in the sandstone, slate, and bituminous shale, have to those seen in England. They met with several lepidodendra, compressed like the English ones; also impressions of ferns and reeds. They had not, however, found any beds of coal belonging to this formation, but large deposits of a new bituminous wood-coal, mixed with layers of mineral pitch. This is found in various parts of the river, and on Garry's Island at its mouth, sometimes deposited on the fixed rocks, but never, as far as could be ascertained, under any of them. It is generally associated with a rich earthy loam, and seems to derive its origin from great deposits of timber, compressed under alluvial, or, to speak in a newer language, diluvial matters, and impregnated with the bitumen, exuding in immense quantities from the carboniferous limestone, which exists in enormous masses in this country, constituting whole districts and ridges of mountains. The shells and corallines of the limestone are very fine and perfect. The fibrous structure, and, indeed, the shape of the trees, may still be clearly traced in the coal. From the twisted state of the woody layers, I suspect that a great portion of the coal has been formed from roots, or from trees that have grown in a climate equally severe with this; the resemblance being very perfect to the wood of the spruce fir, which grows in the surrounding country."

Additional Information.

"Here I am once more housed for the winter.

> Hebrum prospiciens, et nive candidam
> Thracem, ac pede barbaro
> Lustratum Rhodopen.

After six months of constant travelling our winter residence is pleasantly situate on the bank of a lake 150 miles long, deep, and abounding in fish, its shores well wooded, considering the high latitude, and frequented by moose deer, musk oxen, and rein deer. We have abundant stores for next year's voyage, but our party is large, and we depend on the fishery and chase for support during the winter, yet hope to fare well. In our excursion of three weeks along the lake, which I made since my arrival, I obtained a boat load of excellent venison, and our nets have occasionally given us 50 or 60 trout in a day, weighing each from 20 lb. to 50 lb. besides 200 to 300 of a smaller fish, called fresh water herrings. Notwithstanding all these comforts the wiser part of us live in some fear; for any sudden amelioration of the climate, produced by the approach of a comet to the earth, or any other of the commotions amongst the heavenly orbs, dreaded by astronomers, might cause us to be swept into the lake, as, our fort being built on an ice berg, a thaw might prove fatal to its stability. The ground, although it produces trees of considerable size, is constantly frozen; the mud with which our house is plastered was dug out by the aid of fires last month, and now, at the close of the summer, the excavation under our hall floor, which we intended to convert into a cellar, has been worked only to the depth of three feet, its walls of clay being frozen as firm and harder than a rock. I hope, however, we shall escape such a catastrophe, as Moore, in his almanack, says nothing about it; unless, indeed, he means to give us a hint, when he says, 'About this time, before or after, certain northern powers will make some stir in the waters.'

"I have had no fly fishing for want of proper tackle. The gigantic trout of this lake would disdain such a mosquito as we were wont to fish with, and I see no pleasure in bobbing for them with a cod-hook and cable. One of the monsters might take a fancy to drag the fisherman to his sublacustrine abodes.

"Captain Franklin and Mr. Kendal have been to the sea, which they found in lat. 69° 29′, quite clear of ice, on the 16th of August. Mackenzie was very near it in his voyage down the river which bears his name, but did not reach the salt water by about thirty miles. They left letters for Captain Parry and his officers from their friends in England, buried at the foot of a pole, on which they suspended a flag. They returned only yesterday, and the despatch by which I send this, sets out to-morrow, with intelligence of their proceedings to government.

"Mr., or at all events, Mrs. H., will rejoice to hear that we have a Highland piper, and a crew, hardy and hearty, sons of the mist, who foot it every night, after the labours of the day, to the sound of their native music. We lack only a little of the mountain dew to invigorate the dance. For my part I think water a more wholesome beverage; but there is a great deal in the name, and prejudices are difficult to be overcome."

To the preceding very gratifying intelligence the Scotch editor adds the following remarks: "Franklin has thus, in our opinion, succeeded in realizing, to a certain extent, the views of the learned and distinguished secretary Barrow. We ardently hope and trust, that the honour of effecting the north-west passage will not be allowed to pass from us, and that Captain Parry will be again dispatched to finish this grand nautical enterprise. The Congress of the United States, we are informed, at this moment are considering a proposal laid before them for the discovery of the north-west passage, which, from the known activity of that body, may be agreed to, and thus, in all probability, we shall hear of the American flag traversing the Polar Sea, and doubling Icy Cape. The Americans, by this atchievement, would secure to themselves, and deservedly, a splendid name in the annals of geographical discovery—a name that ought to be ours, and which would add another and enduring laurel to the wreath of glory which surrounds the maritime honour of this nation."—(Edinburgh New Philosophical Journal.)

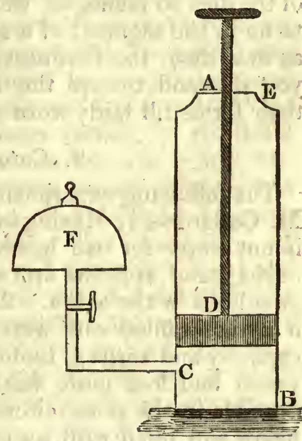
6. *Stereotype Printing.*

A new and, as it is said, improved method of stereotyping, has been announced in the Gazette de Munich. It is the invention of M. Senefelder, to whom the world is indebted for the art of lithography, and is as follows: a sheet of common printing paper is covered with a layer of earthy matter (query, plaster or clay), previously mixed with a sufficiency of water, half a line in thickness. In about half an hour it assumes the consistency of paste, and is then put into the frames over the type, composed in the ordinary way, but not inked; in this way the printing is modelled, or engraved, in the paste above. These sheets are then dried on a stone plate, and the fused stereotype metal poured over them. The writing will then be obtained in relief on a thin plate of metal, the characters being equally well formed with the original type. The proofs taken from these stereotype plates do not differ from those taken from the form of moveable characters. The author of the discovery proposes to reveal his process minutely, so soon as he has obtained thirty subscribers at 100 florins each. The expence of the apparatus required for making the castings he estimates at 100 florins, or about 11*l.* 3*s.* 8*d.*, and that of the paper covered with the earthy paste at six kreutzers, or 2.68 pence per sheet.—(Journal of Science.)

7. *On an Air-Pump, without Artificial Valves.* By W. Ritchie, AM. Rector of Tain Academy.

In the common construction of the air-pump the valves are very liable to be deranged, the repairing of which is attended with much trouble and expence. In the following construction no such derangement can possibly take place, which must of itself give this air-pump a decided advantage.

The machine consists of a barrel shut at the lower end, and having a small aperture at C, forming a free communication with the receiver F; the piston D is solid, and stuffed in the usual way. The piston rod works in a small stuffing box at A, so as to render it completely air-tight. There is a small aperture at E in the top of the barrel, to allow the air to make its escape when the piston is raised. This air-pump may be worked in the usual way, or by the method of continued motion. In commencing the exhaustion of the receiver, the piston is supposed to be below the small aperture at C.



The piston is then raised, and the air which occupied the barrel is forced out through the aperture at E. The point of one of the fingers is applied to the perforation, in the same manner as in playing the German flute. The air easily passes by the finger, which, when the piston begins to descend, shuts the opening, and completely prevents the entrance of the external air. The piston is again forced down below the opening C, the air in the receiver rushes into the barrel, and is again expelled by the ascending piston.

Since the air in the receiver has no valve to open by its elasticity, it is obvious that there is no limit to the degree of exhaustion, as in the common construction. (Ed. New Phil. Jour.)

8. *Hardening of Steel Dies.*

Mr. Adam Eckfeldt is stated to be the first who employed the following successful mode of hardening steel dies. He caused a vessel, holding 200 gallons of water, to be placed in the upper part of the building, at the height of forty feet above the room in which the dies were to be hardened; from this vessel the water was conducted down through a pipe of one inch and a quarter in diameter, with a cock at the bottom, and nozzles of different sizes, to regulate the diameter of the jet of water. Under one of these was placed the heated dies, the water being directed on to the centre of the upper surface. The first experiment was tried in the year 1795, and the same mode has been ever since pursued (at the Mint) without a single instance of failure.

By this process the die is hardened in such a way as best to sustain the pressure to which it is to be subjected; and the middle of the face, which, by the former process, was apt to remain soft, now becomes the hardest part. The hardened part of the dies so managed, were it to be separated, would be found to be in the segment of a sphere, resting in the lower softer part as in a dish, the hardness, of course, gradually decreasing as you descend toward the foot. Dies thus hardened preserve their forms till fairly worn out.—(Franklin's Journal.)

9. *Cutaneous Absorption.*

The following experiments on this subject have been made by M. Collard:—1. Having immersed his hands as far as the wrists in hot water for two hours and a half, he found that the veins of the hand and fore arm were swelled, and also the lymphatic ganglions in the axilla. 2. Having kept his hands for an hour in a vessel filled with water, of which he had ascertained the capacity and surface, he found, on withdrawing them, that the vessel had lost more water than another placed as exactly as possible in the same circumstances. 3. A funnel being closed below and filled with water, the hand was applied to the upper part; the portion of skin within the funnel was gradually drawn inwards, as if by the formation of a small vacuum. 4. The experiment was repeated with a funnel, the neck of which was graduated, and in which was a bubble of air, to indicate by its position any absorption; the results coincided with the last. 5. A glass syphon had its shortest leg enlarged into a funnel, mercury was placed in the bend, and the funnel extremity being filled with water, was covered by the hand for two hours; the mercury gradually approached the hand, proving, with the other experiments, as M. Collard thinks, the absorption of water by the skin. *Archives Gen. Fev.*—(Journal of Science.)

10. *Animal Magnetism in France.*

A commission of the Academy Royale de Medicine has actually reported relative to animal magnetism, 1. That the judgement given in 1784, by the members of the Academy of Sciences and of the Royal Society of Medicine, charged with the examination; since, in matters of science, a first judgement has been too often found defective, and because the researches made by them had not been made with all the care that the habit of experimenting has since introduced. 2. That the magnetism on which judgement was pronounced in 1784 differs entirely in theory, practice, and phænomena, from that now to be considered. 3. That magnetism, having not fallen into the hands of learned men and physicians, and being a special subject of study in most of the colleges of medicine in other countries of Europe, it is for the honour of French physicians not to be behind those of other nations. In fact, that considering magnetism as a secret remedy, it is not only an amusement but a duty of the Academy to take notice of it.—(Journal of Science.)

11. *Fossil Megalosaurus and Didelphis.*

"The bones of the Megalosaurus occur at Stonesfield, in strata of an oolitic limestone slate, which is wrought for roofing houses; and in the same quarries, which abound in organic remains, there have been found several portions of a jaw, which undoubtedly belong to a small insectivorous animal, of the order carnivora, which has been by some referred to the genus Didelphis. There occur in the same strata bones of birds and reptiles, teeth of fishes, elytra of insects, and vestiges of marine and terrestrial plants. Notwithstanding this association of fossils, hitherto regarded as foreign to the deposits beneath the chalk formation, English geologists have been led to think that the Stonesfield slate forms part of the middle oolite system; and it is very remarkable, that at Cuckfield, in Sussex (the only place in which there has hitherto been discovered a great number of fossils, similar to those of Stonesfield), the strata which contain them form parts of the formation of the iron sand, inferior to the chalk, which is much newer than the middle oolite deposits. The following, according to Mr. Buckland, is a list of the fossils, which are found equally in the limestone slate of Stonesfield and the iron sand of Tilgate Forest: bones of birds; of the megalosaurus; of the plesiosaurus; scales, teeth, and bones of a crocodile; humerus and ribs of cetacea, scales of tortoises; the same variety of shark's teeth (*Glosso petra*); spines of balistæ; palates, teeth, and scales, of various fishes; fossil wood; impressions of ferns and reeds; some fragments converted into charcoal, and some rolled pebbles of quartz."

Dr. Buckland considers these two deposits to have been formed under similar circumstances, at different and remote periods; M. C. Prevost regards them "as having been formed at a period much newer than that of the oolitic formations; in short, that they are tertiary and not secondary deposits."—(Edin. Phil. Journ.)

12. *Heart of the Frog used for Poison.*

The Javanese, it is said, employ the heart of the frog named kadok kesse for preparing a poison. The blood of the reptiles is also considered as venomous, and is used for poisoning daggers or knives. It is known that the blood of a frog is employed by the Americans for producing variegated feathers in parrots: some of the feathers are plucked out, and the place where they grew imbued with the blood of the reptile, after which there are produced very beautiful feathers of various colours.—(Edin. New Phil. Journ.)

13. *Taming Rattle-Snakes.*

Mr. Neale, it is said, has succeeded in America in taming rattle-snakes, by means of music, so as to prevent them doing any harm. This author asserts, that they really possess the power of enchanting animals, or of rendering them motionless through terror; for he says he has seen examples even in his garden. The effluviæ of these reptiles has nothing nauseous in it.—(Edin. New Phil. Journ.)

Article XIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

General Directions for Collecting and Preserving Exotic Insects and Crustacea, with illustrative Plates; by George Samouelle, ALS. Author of the Entomologist's Useful Compendium.

Institutions of Physiology; by J. F. Blumenbach, MD. Professor of Medicine in the University of Gottingen. Translated from the last Latin Edition, with copious Notes, by John Elliotson, MD.

A Concise Historical View of Galvanism, with Observations on its Chemical Properties and Medical Efficacy in Chronic Diseases; by M. La Beaume, Electrician, FLS. &c.

The Banquet; or the History of America; by Father Michael Chamich. Translated from the original American, by Johannes Avdall, and dedicated to the Asiatic Society.

JUST PUBLISHED.

Lizars' Anatomical Plates, Part X. containing the Organs of Sense and Viscera. Demy folio, 10*s.* 6*d.*; plain. 1*l.* 1*s.* coloured.

Shute's Medical Science, vol. 2. 18*s.*

Life and Character of Dr. Bateman. 7*s.* 6*d.*

Article XIV.

NEW PATENTS.

T. J. Knowlys, Trinity College, Oxford, for a new manufacture of ornamental metal.—June 13.

T. Halahan, York Street, Dublin, Lieutenant in the Royal Navy, for machinery or apparatus for working ordnance.—June 22.

L. Aubrey, Two Waters, Herts, engineer, for an improvement or improvements in the web or wire for making paper.—July 4.

J. Poole, Sheffield, shop-keeper, for improvements in the steam engine boilers, or steam generators, applicable also to the evaporation of other fluids.—July 4.

D. Freeman, Wakefield, sadler, for improvements in measuring for and making collars for horses and other cattle.—July 4.

P. Groves, Liverpool-street, for improvements in manufacturing or making white lead.—July 4.

R. Wornam, Wigmore-street, Cavendish-square, pianoforte maker, for improvements on pianofortes.—July 4.

P. Groves, Liverpool-street, for improvements in making paint or pigment for preparing and combining a substance or material with oil, turpentine, and other ingredients.—July 10.

B. Lowe, Birmingham, gilt toy manufacturer, for improvements in useful and ornamental dressing pins.—July 14.

J. Guy and J. Harrison, Workington, Cumberland, straw-hat manufacturer, for an improved method of preparing straw and grass to be used in the manufacture of hats and bonnets.—July 14.

J. P. de la Fous, George-street, Hanover-square, dentist, and W. Littlewart, Saint Mary Axe, mathematical instrument maker, for an improvement in securing or mooring ships and other floating bodies, and apparatus for performing the same.—July 14.

E. Bayliffe, Kendall, Westmoreland, worsted-spinner, for improvements in the machinery used for the operations of drawing, roving, and spinning of sheep and lambs' wool.—July 14.

J. L. Higgins, Oxford-street, for improvements in the construction of cat blocks and fish hooks, and in the application thereof.—July 14.

Article XV.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
June 23	30·12	30·10	30·110	73	52	62·5		Var.	Clear.
24	30·12	30·12	30·120	78	54	66·0		Var.	Clear.
25	30·10	30·08	30·090	77	55	66·0		N	Clear.
26	30·05	29·96	30·005	80	62	71·0		Var.	Clear.
27	29·86	29·84	29·850	75	65	70·0		NW	Clear.
28	29·95	29·90	29·925	71	56	63·5		Var.	Clear.
29	30·02	30·00	30·010	70	63	66·5		N	Clear.
30	30·06	30·04	30·050	69	57	63·0		NW	Clear.
July 1	30·12	30·10	30·110	74	57	65·5		NW	Clear; fair.
2	30·10	30·10	30·100	74	54	64·0		Var.	Clear.
3	30·10	30·02	30·060	74	53	63·5		NW	Clear; fair.
4	30·00	29·90	29·950	72	62	67·0		W	Cloudy.
5	29·80	29·78	29·790	74	63	68·5		W	Cloudy.
6	29·76	29·70	29·730	69	63	66·0	0·150	W	Cloudy; showers.
7	29·70	29·62	29·660	71	59	65·0	0·120	SW	Cloudy; showers.
8	29·56	29·54	29·550	68	60	64·0	0·100	SW	Cloudy.
9	29·66	29·60	29·630	69	53	61·0		W	Cloudy.
10	29·72	29·71	29·715	70	58	64·0		W	Clear; fair.
11	29·82	29·80	29·810	70	58	64·0		W	Cloudy.
12	29·78	29·76	29·770	70	61	64·5	0·150	SW	Showery.
13	29·52	29·50	29·510	68	60	64·0		W	Cloudy.
14	29·70	29·68	29·690	69	59	64·0		NW	Clear; cloudy.
15	29·82	29·80	29·810	69	58	63·5		NW	Clear; cloudy.
16	29·86	29·84	29·850	68	60	64·0	0·050	NW	Showers.
17	29·92	29·90	29·910	73	56	64·5		NW	Clear.
18	29·96	29·94	29·950	69	56	62·5		NW	Fair.
19	29·98	29·96	29·970	70	59	64·5		NW	Clear.
20	30·00	29·90	29·950	68	59	63·5		W	Showery.
21	29·70	29·65	29·675	63	57	60·0		NW	Showery.
22	29·80	29·80	29·800	64	55	59·5	0·120	N	Clear; cloudy; showers.
	30·12	29·50	29·872	80	52	64·5	0·690	NW	

RESULTS.

Barometer, mean height .... 29·872
Register Thermometer, ditto .... 64·5°
Rain, No. 1, 0·690, No. 2, 1·240.
Prevailing wind, NW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, July 23, 1826. EDWARD C. GIDDY.

Article XVI.

METEOROLOGIGAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
6th Mon.							
June 1	N E	30·05	30·03	64	51	—	60
2	N E	30·23	30·05	62	47	—	01
3	N W	30·34	30·23	70	42	—	
4	N W	30·43	30·34	70	52	—	
5	N W	30·44	30·43	72	45	—	
6	N W	30·44	30·36	76	54	—	
7	N	30 37	30·36	68	52	—	
8	N E	30·36	30·17	74	50	—	
9	N E	30·17	30·12	76	50	·95	
10	N E	30·20	30·12	80	52	—	
11	N W	30·36	30·20	81	50	—	
12	N W	30·38	30·36	88	52	—	
13	S W	30·36	30·35	88	53	—	
14	N W	30·35	30·31	82	56	—	
15	N W	30·38	30 27	83	49	—	
16	N W	30·50	30·38	75	45	·90	
17	N W	30·50	30·40	75	55	—	
18	N W	30·49	30·40	83	48	—	
19	S E	30·56	30·49	76	45	—	
20	N E	30·56	30·55	75	43	—	
21	N E	30·55	30·50	68	53	—	
22	N E	30·51	30·50	75	48	·94	
23	N E	30·51	30·51	80	45	—	
24	N E	30·51	30·45	84	45	—	
25	E	30·45	30·33	85	47	—	
26	E	30·33	30 16	88	57	—	
27	S E	30·17	30·16	92	62	·95	52
28	S E	30·26	30·16	91	58	—	
29	W	30·30	30·26	82	58	—	
30	S	30·30	30·28	87	62	·48	05
		30·56	30·03	92	43	4·22	1·18

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

REMARKS.

Sixth Month.—1. Night rainy: showers during the day. 2. Cloudy. 3—26. Fine. 27. Sultry: a thunder storm from 11 to 1, with heavy rain. 28. Fine: sultry. 29, 30. Fine.

RESULTS.

Winds: N, 1; NE, 10; E, 2; SE, 3; S, 1; SW, 1; W, 1; NW, 11.

Barometer: Mean height

For the month.................................... 30·343 inches.

Thermometer: Mean height

For the month.................................... 64·266°

Evaporation.................................... 4·22 in.

Rain.................................... 1·18

Laboratory, Stratford, Seventh Month, 26, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1826.

Article I.

A Chemical Essay on the Art of Baking Bread. By Hugh Colquhoun, M.D.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Few chemical processes concern the health and comfort of every individual in the country more directly and immediately than the art of baking bread, and yet there are, perhaps, not many the rationale of which is less generally familiar. There is little of attraction about the operations of a bake-house, and it is far from pleasant, to any ordinary observer, to follow the flour, through its various progressive changes, to the oven, and then to superintend it in the last stage of its formation into bread. But the remark is a true and a trite one, that the most showy and striking phænomena are not always the most truly interesting or instructive when examined; and in the most common of all the mechanical arts, it will be occasionally found, that there still remains room for amendments, even obvious to the theorist who enters unprejudiced upon the study of the system, though they may have long escaped the notice of the mechanic, bred up to follow a monotonous routine, which he is either too indolent, or too ignorant, or too timid, or too much the slave of habit, to think of disturbing. The following Essay is submitted, therefore, to the public, in the hope, that while it may prove not unworthy of the chemist's attention, it may also be the means of conveying some useful practical hints to the mechanic. At the same time, it is necessary to premise, that in regard to some of the manipulatory improvements which are proposed in the following pages, the only remarkable thing is that they should have remained till now almost entirely, if not indeed altogether, unknown to the baker's practice. It has not required much science to suggest perhaps the principal among them, and yet the advantages which their use promises seem to be far from inconsiderable.

In preparing this Essay, it has been necessary not only to consult the views and experiments of former writers, but, in order to elucidate some of the processes connected with the art, to perform various experiments entirely new, and also in many instances to verify carefully the results stated to have been obtained by others. Wherever an experiment is quoted upon authority merely, a reference is given, and where this is not done, the author holds himself responsible for the accuracy of his details.

Baked bread, simply considered, may be described as being a substance formed by mixing a portion of the seeds of any of the cereal grasses with a little water, and then cooking the whole, by means of fire, into a solid consistent state. In the earliest stage of the art, the process probably consisted of but a very few steps. And indeed the first cook who discovered that by previously moistening and then baking grain, a compact cake of food could be formed, fitted to contain within a small bulk a large supply of nutrition, to keep entire for an indefinite length of time in proper situations, and to yield when masticated a most agreeable relish to the palate, may perhaps be regarded as having made a step in the art of baking bread, of more difficulty in itself, and of greater importance to the species, than any thing that subsequent improvement has supplied. For in all the intricacies and refinements of our modern cookery of bread, there can surely be found nothing to compare with that which first taught man to use a great proportion of his food in a manner peculiar to himself, and raised him above the practice of devouring it as raw grain in common with the lower animals. What may be conjectured to have been the second leading-step of advancement in the art, that of reducing the grain to powder, before applying to it the moisture which should form the solid cake after the application of heat, seems perhaps of more natural and easy suggestion than the other; and accordingly we find at this day few nations refined enough to practise baking at all, who are yet so rude as not to make their cakes of bread out of ground grain.

But there still remained another distinct department of manipulation, to be introduced into the art of baking, before it contained all the rudiments of what has now been gradually perfected into the modern system. And this latter improvement certainly seems to savour more of refinement and civilization in its introduction and regular use, at the same time that it is of too old a date to have left even any tradition of its origin or invention. It consists in mixing with the constitution of the bread a light gaseous body; and, in actual practice, this is almost invariably of the same kind with that which gives the foam to ale and porter, and the sparkle to champaigne. This gas, when duly infused into the dough, gives us, after baking and cooling, instead of a heavy and hard, or tough dull nutriment, a light, porous, elastic, diaphanous food, which is at once more agreeable to the palate, and, as of easier digestion, more conducive to the health. Common sea biscuit is no bad specimen of the former of these kinds of bread, while a good modern plain wheat loaf is a fair example of the latter. And if one will just imagine a mass of the dough of sea biscuit baked into the bulk and shape of a common wheat loaf, the comparative qualities of the two varieties of bread will appear evident. The one would prove a hard, compact, heavy body, which it would be difficult to cut down or to chew, while the other would be light, semi-transparent, and full of little vesicles of air so as to resemble a sponge in lightness and elasticity. In addition to this, it is not immaterial to observe that these vesicles in well made bread are regularly arranged in a sort of stratification of layers one above another, all of them perpendicular to the crust of the bread. This kind of internal structure constitutes what is termed by the bakers *piled* bread; and this appearance they are in the habit of regarding as one of the surest tests of the success of their batch.

These distinctions are marked and decisive. They place in a sufficiently striking light the great advantages derived to mankind from the introduction of that part of the process of baking which consists in mingling with the bread they eat a considerable volume of what must be regarded as a foreign and innutritious body. And it may be proper to mention as a circumstance which throws some light on the increased facility of digestion possessed by well-piled bread, that if a portion of it, after having been duly baked and thoroughly cooled, be pressed between the fingers, it will crumble readily into powder; and if a piece of such a loaf be placed in hot water, it immediately softens, swells out considerably, disintegrates, and admits of being easily diffused throughout the liquid. But if a bit of unpiled bread be similarly squeezed between the fingers, it remains a solid cohesive mass, and when put into hot water, never softens further than to become a permanently-tough mass of dough.

The various modes which have been resorted to for the purpose of introducing the gaseous principle into bread, form almost the whole matter of interesting research which is connected with the modern art of baking. The rest, as has been already adverted to, resolves itself into a pretty simple and by no means very curious process of cookery, being merely the commixture, in due proportions, of flour, and salt, and water, with the occasional addition of confectionary, after which the compound is baked in the oven. The only curious chemical investigation connected with the art, is, therefore, the examination of the use and operation of the gaseous principle thus artificially introduced into bread for the purpose of rendering it light and elastic, and this shall form the subject of the present Essay.

In conducting this inquiry, we shall commence, for the sake of perspicuity, by stating shortly the mechanical history of the most ordinary process of baking. We shall next consider in a chemical point of view the use and object of each part of the process, in so far as it contributes towards the duly gasifying* of bread, so as to render it that light and spongy food which makes it at once both pleasant and wholesome. And in discussing this matter, we shall divide the Essay into two parts. The first of these we shall devote exclusively to the process of panary fermentation, by far the most interesting and extensively useful of all the methods employed by the baker in order to impregnate his dough with the elastic fluid. In the second, we shall consider more cursorily a few of the principal among the other chemical processes resorted to by the baker, with a view to gasify his bread. Among these, we shall find that there is one, used in the manufacture of gingerbread, sufficiently important in itself, and sufficiently curious and anomalous with respect to its rationale, to require a much more careful and minute examination than any of the others; and with this we shall conclude the Essay.

Mechanical Details of the most common Process in the Art of Baking Bread.

The spontaneous decomposition of a piece of wheat dough, always generates within the mass a quantity of carbonic acid gas; and it is the formation of this gas which is the baker's object in exciting fermentation. The modes employed by him may therefore be considered comparatively good, in proportion as they more perfectly and rapidly produce the internal gas. Perhaps the most simple process for effecting this, is to place a portion of common dough apart, in a warm situation, where, if allowed to remain a sufficient length of time, it will pass spontaneously into a state of decomposition, which will generate carbonic acid gas within it, and give the bread that is baked from it both lightness and vesicularity. This system, however, is attended not only with considerable delay, but with the further disadvantage, that such dough is never entirely free from acescence or putrescence, either of which is always injurious to the flavour of the bread, and may even, if existing in excess, prove detrimental to its wholesomeness. But the process of decomposition will be found to be greatly accelerated in any recent mass of fresh dough, by the addition of a small portion of old dough already in a state of strong fermentation. When this

* For shortness' sake the term gasifying is used to express the common infusion of a gaseous or elastic fluid into the system of dough.

is done, the mass is said to be *leavened*, the dough thus added while under fermentation being denominated *leaven*. The practice of leavening bread is familiar to every one, as having been known at the earliest periods of which we have any authentic records. And, in point of fact, the same system, although another process is superadded to it, forms a leading part of the art of baking in the most civilized countries at the present day; for it is the almost invariable practice of the baker to induce fermentation, not in the whole dough at once, but in a portion only, with which the rest is afterwards leavened; and thus it is found, that the tendency to decomposition may be more rapidly communicated to the entire bulk of that dough, which he intends to convert into bread.

It is no longer, however, by the addition of a little leaven that the modern baker produces the commencement of his process of decomposition; for there is a particular substance which he has discovered to possess the property of exciting fermentation in dough with a still greater degree of rapidity. This is *yeast*, or the frothy scum which is thrown up to the surface of a brewer's vat, soon after the saccharine infusion has passed into a state of active fermentation. Of this yeast, which is a very complex and impure substance, chemists are not perfectly assured which constituent it is that spreads decomposition among the dough; although there now seems to be little doubt that this is effected by its glutinous ingredient, which has itself already begun to pass into a state of decomposition.

When the baker proceeds to the preparation of dough by means of the yeast-fermentation, he at first takes, generally a portion only, but sometimes the whole of the water, which it is his intention to employ in making the required quantity of dough. In this water, which varies in temperature, according to circumstances, from 70° to 100°, there is dissolved a certain portion of salt, the quantity of which, however, is always less than that which will finally be required, in order to communicate the necessary flavour to the bread: yeast is now mixed with the water, and then a portion of flour is added, which is always less than the quantity to be ultimately employed in forming the finished dough. The mixture is next covered up, and set apart in a warm situation; within an hour after which, signs of commencing decomposition make their appearance.* The sponge begins to swell out and to heave up, evidently in consequence of the generation of some internal elastic fluid, which in this instance is always carbonic acid gas. If the sponge be of a

* The substance thus placed apart is termed, in the language of the bake-house, the *sponge*; its formation and abandonment to spontaneous decomposition is termed *setting* the sponge; and according to the relation which the amount of water in the sponge bears to the whole quantity to be used in the dough, it is called *quarter*, *half*, or *whole* sponge.

semi-liquid consistence, large air-bubbles soon force their way to its surface, where they break and dissipate in rapid succession. But where the sponge possesses the consistence of thin *dough*, it confines this gaseous substance within it, until it dilates equably and progressively to nearly double its original volume, when, no longer capable of containing the pent-up air, it bursts and subsides. This process of rising and falling alternately might be actively carried on and frequently repeated during twenty-four hours, but experience has taught the baker to guard against allowing full scope to the energy of the fermentative principle. He generally interferes after the first, or at furthest after the second or third dropping of the sponge, and were he to omit this, the bread formed from his dough would invariably prove sour to the taste and to the smell.

He therefore, at this period, adds to the sponge the remaining proportions of flour and water, and salt, which may be necessary to form the dough of the required consistence and size; and next incorporates all these materials with the sponge by a long and laborious course of kneading. When this process has been continued until the fermenting and the newly-added flour have been intimately blended together, and until the glutinous particles of the flour are wrought to such a union and consistence, that the dough, now tough and elastic, will receive the smart pressure of the hand without adhering to it when withdrawn, the kneading is for a while suspended. The dough is abandoned to itself for a few hours, during which time it continues in a state of active fermentation now diffused through its whole extent. After the lapse of this time, it is subjected to a second, but much less laborious kneading, the object of which is to distribute the gas engendered within it as equably as possible throughout its entire constitution; so that no part of the dough may form a sad or ill-raised bread from the deficiency of this carbonic acid gas, on the one hand, or a too vesicular or spongy bread from its excess, on the other. After the second kneading, the dough is weighed out into the portions requisite to form the kinds of bread desired: these portions of dough are shaped into loaves, and once more set aside for an hour or two in a warm situation. The continuance of fermentation soon generates a sufficient quantity of fresh carbonic acid gas within them to expand each mass to about double its former volume. They are now considered fit for the fire, and are finally baked into loaves, which, when they quit the oven, have attained a size nearly twice as bulky as that at which they entered it. It should be remarked that the generation of the due quantity of elastic fluid within the dough has been found absolutely necessary to be complete *before* placing it in the oven; because, as soon as the dough is there introduced, the process of fermentation is checked, and it is only the previously-contained air, which, expanded by heat throughout all the parts of the entire system of each loaf, swells out its whole volume, and gives it the piled and vesicular structure. When it is recollected, that the gas thus generally expanded has been previously distributed by the baker throughout the bread, and that the whole dough has been by kneading formed of a tough consistence, the result becomes apparent that the well-baked loaf is composed of an infinite number of cellules, each of which is filled with carbonic acid gas, and seems lined with or composed of a glutinous membrane; and it is this which communicates the light, elastic, porous texture to the bread.

Such is the mechanical history of the most ordinary and common process followed out by the baker in making a modern loaf. There is nothing of peculiar attraction about it, but the want of this is amply compensated by the interest which a *chemical* examination into the nature and principle of the fermentative process, as here exhibited, excites. This is an investigation which has at different times attracted a considerable share of the attention of several chemists. Their opinions, as will soon be found, have been extremely various in regard to almost all its details. But among the latest writers on the subject, there may be remarked a greater coincidence of views, a sounder and more consistent solution of the different phænomena which present themselves, and a gradual tendency towards unanimity of sentiment on the most important topics. How far the experiments about to be detailed may be calculated to further so desirable a consummation, as the exposition of a chemical theory explaining satisfactorily all the details of the fermentative process in the art of bread-baking, cannot be here decided. At all events there has been the greatest anxiety, on the one hand, to avoid every thing uncandid in the statement of any opinion that is combated, of which it has been necessary to mention several; and on the other, to advance nothing strained, in support of any view that may be defended in the following pages. If there be any erroneous statements, they have not been wilfully made, and will be corrected as soon as pointed out. With this explanation we proceed to our chemical inquiry.

I. *Of the Nature of the Panary Fermentation.*

There are three principal constituents of all wheaten flour; starch, which exists in the largest proportion; gluten; and a saccharine principle. About thirty years ago, when the ideas of chemists regarding the elementary constitution of organized substances were less precise than at present, the difficulty of assigning to fermentation in dough a place under any of the three usual classes of the vinous, acetous, and putrefactive fermentation, led to the conception that it was a species of decomposition entirely *sui generis*. It was accordingly denominated *Panary*, and held to consist in the simultaneous decomposition and mutual re-action of all the constituents of flour. Subsequently to that period, the action of the fermentation has been held not to take place at once upon all the constituents of flour; but has been limited at one time to the glutinous ingredient, as by the Messrs. Aikin, in their excellent Dictionary of Chemistry,* and at another to the starch; but of late, the prevailing opinion has been that the only principal subject of its action is the saccharine constituent. It is the latter theory which is to be maintained in this Essay; the fermentation in dough, so far as it is useful to the baker, being ascribed solely to the resolution of the saccharine principle of the flour into carbonic acid and alcohol, in consequence of its being brought into a situation predisposing it to pass into the vinous fermentation. Undoubtedly, if the saccharine fermentation be suffered to exhaust itself in any dough, it will be found that a new fermentation, of a different kind, will succeed it; but it is this latter decomposition alone which is conceived to be injurious to the bread, while the former is the source of all the benefits which the best fermentation is found to confer. It appears, therefore, that the first material point to be determined, in the chemical history of the bread-fermentation, is, whether the saccharine principle be truly the exclusive subject of its operation.

In order to illustrate this fundamental point, let it be first of all considered what are the only other constituents of wheaten flour besides the saccharine principle. These may be correctly enough stated to be starch and gluten; for the albuminous and gummy principles, both from their small amount and from other circumstances to be hereafter adverted to, seem to be of little influence on this question. Let the well-known phænomena of decomposition, as occurring in each of these two bodies taken separately, be attended to. They will be found to differ very decidedly from those which take place in the panary fermentation. And if the characteristics marking the decomposition of the remaining ingredient of flour, the saccharine principle, be compared with the acknowledged appearances and effects of that fermentation which does take place in dough, their similarity, or rather identity, would seem to leave little room for doubt on the subject.

In the first place, with regard to starch and gluten. There is no tendency to undergo any decomposition whatever induced in starch, by merely exposing it for a few hours to the moderate temperature used in the preparation of dough; and even moist gluten, in the short period necessary to commence and complete

* Article, Bread. Published in 1807.

the fermentation of dough, would sustain no change in its appearance or chemical properties, though exposed, either *per se*, or mixed with yeast, to the temperature just mentioned; yet the fermentative process in dough is strong and active under these very circumstances. Besides, it is certain, that if spontaneous decomposition either of the starch or of the gluten, always of comparatively tardy excitement, were once commenced and left unchecked in circumstances so favourable to decomposition as in the baking process, with respect to both moisture and temperature, it would of necessity continue proceeding, with regular and unabated energy, so long as a particle of either substance remained unaltered. But in dough, though fermentation commences *soon after* the mixture of yeast and hot water with the flour, and goes on actively and in full vigour for a given period, varying from 24 to 48 hours, it *suddenly stops short*, while yet it is *quite obvious* that much of the starch and of the gluten remains *untouched*. In fine, it may be mentioned, as conclusive of this question, that when fermentation has thus ceased in dough, neither the addition of fresh yeast, nor of fresh starch, nor of fresh gluten, nor of all the three combined, has the smallest effect in renewing the process of fermentation. And it has been ascertained by M. Vogel, that in baked bread, there exists pretty nearly the same quantity of gluten as in common wheaten flour, and that of the starch, three-fourths remained entire; while the other fourth had only been converted into a gummy matter, similar in appearance and properties to torrefied starch, a change which, it is almost unnecessary to mention, could have no effect in infusing a gaseous body into the bread. It seems, therefore, to be a point scarcely admitting of additional proof, that it is neither the starch nor the gluten which is concerned in the ordinary fermentative process, which takes place in dough.

We are too little acquainted with the chemical nature of the albuminous and gummy principles which are found to exist, in a minute proportion, in wheaten flour, to be able to reason, with equal precision, on the changes which they may undergo, or the influence which they may exert in the fermentation of dough. But besides their very trifling amount, there exists this strong probability of their remaining entirely quiescent, at least during the early stage of the fermentation of the dough;—that neither albumen nor gum appears to possess a greater tendency to pass into a state of spontaneous decomposition, than gluten on the one hand, or than starch on the other.

If we now turn to the other principal constituent of flour, the saccharine principle, a very simple solution of the difficulty will appear, and a natural account of the common fermentative process be obtained.

And here one can scarcely avoid expressing a certain degree of wonder how so many chemists, and some of these considerably distinguished, formerly attributed so great an agency to starch and to gluten in the bread-fermentation, considering that so many prominent and apparent discrepancies, as have lately been adverted to, stand opposed to their conjecture. But in point of fact, a strong idea then prevailed, particularly in the case of the Messrs. Aikin, that the saccharine matter in flour is far more minute and immaterial than it is in reality. Of course, the alcoholic fermentation of sugar has long been well known and understood, and perhaps the principal difficulty that ever attended the supposition of its operation, was to find room for its existence in the constitution of dough.

But the amount of saccharine matter naturally contained in all flour is by no means insignificant; on the contrary, it is amply sufficient to furnish in its decomposition all that quantity of carbonic acid gas, the development of which marks the progress of fermentation in dough. Thus M. Vogel, on analyzing two specimens of ordinary wheaten flour, obtained the following results. From the flour of the *Triticum hibernum*, Lin.:

Starch	68·0
Moist gluten	24·0
Mucilaginous sugar	5·0
Vegetable albumen	1·5

And from the flour of the *Triticum Spelta*, which differs only in being considered of a superior quality to the preceding:

Starch	74·0
Moist gluten	22·0
Mucilaginous sugar	5·5
Vegetable albumen	0·5*

Proust,† and Edlin,‡ have also made experiments which lead to the same conclusion; the latter, in particular, found that by merely washing wheaten flour with water, and then purifying the mucilaginous extract, he obtained 1½ per cent. of crystallizable sugar. The properties which Mr. Edlin ascribes to the

* Journal de Pharmacie, iii. 212.

† He ascertained 100 parts of wheaten flour to be composed of about

Starch	74·5
Gluten	12·5
Gummy and saccharine extract	12·0
A yellow resin	1·0
	100·0

(Ann. de Chim. et de Phys. v. 340.)

‡ In his Treatise on the Art of Bread-Making, p. 50, he gives the following statement as the result of his examination of a pound of wheat:

sugar of flour thus obtained by him differ, however, so widely from those which it has been found to possess by other and more skilful chemists, that it must be confessed, it seems necessary to receive this part of his statement with considerable qualification.

Since the presence of a saccharine constituent in flour is thus clearly established, and that to no inconsiderable extent, being not less than to the amount of five per cent. according to the several analyses just quoted; and since the alcoholic fermentation of sugar is one of perfect familiarity to the chemist, the characteristics of which correspond with the fermentation in dough, in the rapidity with which it commences, the activity with which it continues, and considering the usual amount of the sugar, the period during which it lasts, there would seem little room left to doubt wherein consists the true fermentation which occurs in the art of bread-making.

But the results of the following very simple experiment, which has been repeatedly performed with the same success, would seem still further to settle the point, and indeed to place the matter beyond the reach of controversy. After suffering the fermentative process to exhaust itself in a mass of dough, and the dough to be brought into that situation in which the addition of neither yeast, nor starch, nor gluten, had produced any effect on a similarly ex-fermented mass, I tried the renewal of a little yeast to the dough, along with a small addition of *the other constituent* of the flour, the saccharine principle. On adding common refined-sugar in these circumstances to the amount of four per cent. the *process of fermentation immediately recommenced,* and in its *appearance, activity,* and *duration,* was just a repetition of the previously-exhausted process of fermentation. After a lapse of about *the same period,* it, in the same manner, *totally ceased.*

It is difficult not to look upon this experiment, especially when taken in connexion with the others lately stated, as completely decisive of the question, that the ordinary bread-fermentation is the simple and well-known process of the alcoholic fermentation of sugar. If any thing could be added to confirm this, it is the fact, that the mere addition of sugar, as above, to an ex-fermented mass, without being coupled with any other substance, produced a renewal of the process of fermentation in the dough. In this case, however, as might have been expected

	Oz.	Dr.
Starch	10	0
Bran	3	0
Gluten	0	6
Sugar	0	2
Loss in grinding	2	0
	16	0

from the comparative weakness of the yeast, the spontaneous decomposition was more tardy in commencing, less brisk in its operation, and lasted longer than in the ordinary process; but this, as is well known, is precisely what invariably takes place, when saccharine matter is brought into a state of fermentation by means of a ferment which is already either half-exhausted, or whose fermentative power is naturally feeble.

There seems to be but one objection to the adoption of a theory, which is supported by proofs so very strong as these just mentioned, and that objection is more apparent than real. After a loaf is baked, there is found still to exist in its composition almost as great a proportion of saccharine matter as occurs in the original wheaten flour, previously to fermentation. M. Vogel found, that in a baked loaf there remained 3·60 of sugar, which was only 1 or 1·5 per cent. less than had existed in the flour before making it into dough.* And he very naturally declares that he was not a little surprized by the fact, as he held the same opinion on the subject of the fermentation in dough, which has been supported in this Essay.

But it must, in the first place, be recollected, that in every loaf, as the process of fermentation has been invariably *checked at a very early period* by the baker, that constituent, which was the subject of the fermentation, can never be wholly, and will often be but very partially, decomposed. And in addition to this it seems almost certain that another and a sufficiently interesting chemical change occurs during baking, and which, if the following account be correct, will easily reconcile the large amount of sugar found to exist in bread after baking, with the fact, that the saccharine principle was nevertheless the subject of all the fermentation which had taken place.

In the experiment of M. Vogel last referred to, besides ascertaining the amount of *gluten* to have been scarcely affected by baking, and three-fourths of the starch to retain their properties unaltered, it appeared that the remaining fourth had acquired the properties of a gummy matter, analogous to torrified starch, being readily soluble in cold water. Now the experiment to be detailed seems to point strongly to the conclusion, that when any part of a loaf of bread enters the oven in the state of gelatinous starch, the process of mere baking alters the relative con-

* In 100 parts of loaf-bread, prepared with wheaten flour, distilled water and yeast, without the admixture of any common salt, he found the following ingredients:

Sugar	3·60
Torrified (or gummy) starch	18·0
Starch	53·50
Gluten, combined with a little starch	20·75
Carbonic acid	—
Muriate of lime	—
Magnesia	—

(Journ. de Pharm. iii. 219.)

stituents of the dough, by forming a certain portion of *saccharine matter at the expense of the starch*. And it will rarely happen that no such material exists in any part of a loaf, since the use of hot water, in the ordinary preparation of dough, is just the natural means of reducing the starch to a gelatinous state.

Several masses of dough were prepared, in which pure wheat-*starch* was mixed with common flour, in very various proportions. In some of the pieces, this starch had been gelatinized, with a minimum of hot water, before it was added to the flour. After a proper allowance of salt to each separate mass of dough, and a thorough kneading, the whole were set apart for the ordinary period, and allowed to ferment to the usual preparatory extent, after which they were baked in the oven. In respect of outward appearance, the increase of volume, and the vesicularity of their internal structure, none of them varied materially from a piece of common loaf baked along with them for the purpose of comparison; at least, the only difference was that when the starch originally added to the dough very materially exceeded the proportion of common flour in the same piece, the loaf, while it was decidedly whiter in appearance, had not risen so well, nor did it possess a structure so vesicular as the others. But on tasting the bread of each loaf, the unexpected result was perceived, of the existence of unusual sweetness distinctly observable in all those loaves which had contained the largest proportions of gelatinized starch. The other loaves, where smaller quantities of the gelatinized starch had been employed, or where pure, but dry, pulverulent starch had been used in any proportion, though all were made at the same time, and mixed with flour of the same quality, had no sweetish taste whatever to distinguish them from common bread. These facts led, therefore, to the conclusion, that the presence of gelatinous starch in bread when put into the oven, is a means of forming a certain amount of saccharine matter within the loaf, during the process of baking. And as it is probable that gelatinized starch does exist more or less in all loaves which have been fermented by our usual process, it would appear that in every case there is formed, while in the oven, a certain portion of sugar within the bread. The difficulty, therefore, which suggested itself to M. Vogel, if indeed, it be not accounted for by the early period at which fermentation is interrupted in bread-making, seems thus to be completely removed; and the alcoholic fermentation of the saccharine matter in flour is sufficiently proved to be the true panary fermentation occurring in our ordinary system of bread-making. The point may therefore be assumed as scarcely admitting of further question, that it is the saccharine principle of flour in which decomposition commences, and with which it ends, while dough is under fermentation.

The first step of the investigation into the nature of the panary fermentation being now satisfactorily gained in the establishment of the precise subject of its exclusive action, the next important matter is to ascertain whether this fermentation be truly *sui generis*, or to which of the fermentations, the vinous, the acetous, or the putrefactive, does it belong.

That which first commences within the baker's dough, provided it be of ordinarily good quality, is certainly the common vinous or alcoholic fermentation. This is plain from the fact already stated, that the appearances of the vinous fermentation of simple sugar, resolving itself into alcohol and carbonic acid, are precisely the same with those which, in the process of fermentation, as it is usually conducted in the bake-house, occur in dough. But a remarkable change in the character of the panary fermentation is always found to occur if it be allowed to proceed far enough; and as this change, whenever it supervenes, has the effect of injuring the quality of the bread, and is accordingly the dread of the baker, it is material to inquire into the nature of the secondary alteration, which, at a certain stage of advancement, will always take place.

The mode in which the new substance shows itself, when generated in this subsequent fermentation of the dough, is perfectly well-known to the baker. Fermentation may, with good materials, and in ordinary circumstances, be easily carried on by him to the extent requisite to produce a light and well-raised loaf, which yet shall be sweet and pleasant to the taste. But he is well aware that if he do not check the fermentation of his dough in due time, it becomes invariably sour, and the sourness increases in proportion as the fermentation has been allowed to pass its proper limit. It is only from practice, however, which teaches him to judge by appearances, that he acquires the art of distinguishing the moment when his interference becomes necessary to stop the process of fermentation, and the consequent generation of acidity.

The source of the formation of this acid has been ascribed at different times, and by different chemists, to each of all the several ingredients of flour; to its gluten, its starch, and its saccharine principle. There appears now, however, scarcely any room left for doubting, that the greater portion of it, at least, is invariably the consequence of a *second fermentation;* and that it is produced by the very familiar process of the acetification of the alcohol which had been developed in the original fermentation of the saccharine principle. That the starch, or even the gluten, should ever contribute to its formation appears extremely unlikely, at least in the ordinary routine of bread-making; although there are reasons for suspecting, that in those cases in which the sponge or dough has been too long kept, or in which the fermentative process has been in other respects unskilfully conducted, a portion of the albuminous and mucilaginous principles may likewise pass into a state of acescence, and so contribute towards the activity of the acetous fermentation.

It has been universally taken for granted among authors, that the acid thus developed in dough is exclusively the acetic. And when we reflect on the facility and frequency with which this principle is formed during the decomposition of organized matter, and on the abundance of materials which are in this instance afforded for its production, we must admit that it generally constitutes the chief principle of acidity in sour dough. Perhaps, however, it is rarely the sole cause of this acidity; for there seem good grounds to conclude that another acid of a more fixed nature, most probably the lactic, becomes not unfrequently associated with it; particularly when the fermentation of the dough has been more tardy than usual, in consequence either of the imperfection of the yeast, or of the original bad quality of the flour. It has been proved, of late, by the experiments of Braconnot, Vogel, and others, that this acid is generated with much readiness, and to a considerable amount, during the spontaneous decomposition of a great variety of vegetable substances when in a state of humidity. And the presence of lactic acid would account for a remarkable circumstance connected with the acescence of dough, of which it is difficult to suggest any other explanation, and which occurs in a most striking manner in those cases, where fermentation has been allowed to run on to great excess. This is the fact, that the acidity of unbaked dough in this condition is greatly more perceptible to the palate than to the olfactory nerves, while the sourness of the same piece of bread after coming out of the oven, is, on the contrary, much more offensive to the smell than to the taste. Now this is precisely what might be expected to happen, if we suppose that lactic acid contributes, in conjunction with the acetic, to produce the acescence of sour dough. At the ordinary temperature of a bake-house, the former acid, although very perceptible in the mouth, is not distinguishable by the nostrils; but as it is easily decomposed by heat, no sooner is it exposed to the high temperature of the oven, than, as has been proved by the experiments of Berzelius, it is, in a great measure, resolved into the acetic acid, and so becomes more palpable to the sense of smell, and less so to that of taste.

It would appear, therefore, from what has just been stated, to be a pretty-well-established point, that in acescent dough, *a second decomposition* has always come into play; that this is probably at first of a mixed character, and consists partly of the resolution into acetic acid, of the alcoholic principle previously developed by the saccharine fermentation, and partly of the formation of lactic acid; while the heat of the oven, which checks the saccharine fermentation, at the same time decomposes at least great part of the lactic acid, and resolves it also into the acetic.

This theory seems to explain satisfactorily all the leading phænomena accompanying the progress of fermentation in baker's dough, and also to account for some of its results as appearing in the progress of baking, which do not admit easily of any other solution. Thus it appears that the subject of the bread-fermentation in common practice, is, in every case, exclusively the saccharine principle of flour. That the kind of panary fermentation is by no means peculiar, but always in the first instance the common vinous or alcoholic fermentation of sugar, accompanied as usual by a copious evolution of carbonic acid gas. That after this has gone on for a certain length of time, a second process of spontaneous decomposition commences; the alcoholic principle lately extricated ferments, and is resolved into acetic acid, at the same time that, in all probability, a considerable portion of lactic and acetic acid is generated at the expense of certain other ingredients of the flour, which, at the commencement of the fermentative process, had remained quiescent; and it seems not improbable that a contemporaneous formation of ammonia in the dough takes place to a certain extent. It has been already stated to be an ascertained point, that the lactic acid is frequently produced by the spontaneous decomposition of a vegetable substance, when exposed in a state of humidity to a moderately-warm temperature; and besides, the existence of this acid seems proved by the change produced by baking sour dough, in regard to the organs by which its acidity is perceptible before and after baking.

But if that acescence which always injures so greatly the quality of the bread in which it occurs, be truly the result of a second process of decomposition, the materials for which are only furnished after the first process has been carried on to a considerable extent, it becomes obviously the secret of the baker's art to interfere and check the progress of fermentation in dough, while yet it is confined to the simple process of saccharine decomposition, and before the resolution into acetic acid of the alcoholic principle thereby evolved begins. Indeed that may be defined to be good bread, in which the fermentative process has been so regulated and checked; the art of doing which cannot of course be otherwise acquired than by experience. But there are other methods, and these extremely simple and effectual, for enabling the baker to adopt measures either to prevent or to correct the evil of supervening acidity, and to them our attention will now be shortly turned, before concluding this general view of the several operations and changes in the most common system of baking bread.

We have already spoken of the nature of the process which generates acidity in fermenting doughs, an inconvenience too often felt by every baker to be unknown in the experience of almost any bread-consumer in the country. Against this mischief, it scarcely seems that the utmost skill or precaution that can, in the present state of the art, be employed, is sufficient to guard. For if the flour has been originally of bad quality, or if the yeast employed be of a weak or indifferent sort, or if the water be added at too high or too low a temperature, or perhaps, also, if the state of the atmosphere be not favourable, the dough may speedily be found to become acescent. In short whenever the second process of decomposition commences within dough, before the vinous fermentation of the saccharine ingredient has proceeded far enough to evolve the required quantity of carbonic acid gas, then the bread cannot, by any means at present used, be made to possess the qualities of lightness and sweetness joined. The one of these can be obtained only at the expense of sacrificing the other; for the baker must either, as soon as the incipient acescence appears, send his dough to the oven, and obtain in return a *sad* and ill-raised loaf, or if he prefers, as is generally done, to have it light and well-raised, and therefore allows due scope to the fermentative process, the bread will certainly be sour.

There is, however, a very simple and a very complete cure for this evil, a method by means of which, even after acescence shall have decidedly commenced, the baker may nevertheless be enabled to remove it entirely without sacrificing the valuable object of the vesicularity of his loaf. The remedy to be applied in order completely to neutralize an acid, as will at once suggest itself to every chemist, is the due exhibition of an alkali. And it is a striking proof how much the mechanic has been accustomed to plod, uninquiring and uninstructed, over the same ground, in past times when he was less familiar with science than he is likely now to become and for ever to continue, that a relief so obvious and so simple, from inconveniences so excessive, should at this moment remain unknown to the greater part, if not to all, of the bread-manufactories in the country. The use of a little of the carbonate of soda, or of the carbonate of magnesia, is all that is required in order to secure to the baker a dough which he may always have sweet and pleasant during the entire progress of fermentation; and even in case he may have allowed acidity to supervene to no inconsiderable extent, the same alkalies may be successfully and innocently employed in restoring dough to its original freshness.

In order to bring the matter fairly to the test, and to try the effects of the system here spoken of, a quantity of ordinary loaf-dough was taken, when just fit for the oven, and set aside in a warm situation, where, of course, the fermentation briskly proceeded. To the simple saccharine decomposition was soon added the secondary process of acescent fermentation, and the dough became gradually sour. At the expiration of twenty-four hours, upon opening up the dough, which was still in a state of strong fermentation, a very acid odour was plainly perceptible. The taste was also distinctly, though weakly acid. After taking two pieces, weighing five ounces each, from the general mass, it was once more set aside. Into one of the portions thus chosen were kneaded 10 grains of the common carbonate of magnesia, and then both were, after the usual manner, baked in the oven. The difference between the two loaves, when baked, was most striking. The bread which had been made from the sour dough alone had a taste distinctly perceptible of acidity, and a smell so sour as must have rendered it almost unsaleable, while that which contained the magnesia presented not the slightest indications of any kind of sourness, and appeared in all respects an excellent loaf.

This was certainly a very decisive proof of the advantage with which this carbonate may be employed in correcting an acidity, which had proceeded to as great a degree as it is ever met with by the baker in his ordinary practice. But it appeared desirable both in a practical and in a theoretical view, to try the effect of the same substance on a still greater degree of acidity, and also to compare its relative action with that of carbonate of soda on the same acid. Accordingly that mass of sour dough from which two portions had been taken, as above-mentioned, was allowed to remain for twenty-four hours longer in a warm situation as before. At the end of this period, the various processes of internal decomposition had not wholly ceased, and it was found to be still in a state of fermentation, though not so vigorous as on the preceding day. The acid taste of the dough had by this time very much increased, and the acid odour was strong. Four portions of this dough were now taken, all of which were baked after the usual form; but with this difference in their composition, that one was put into the oven made of the sour dough just as it stood, a second had four grains, and a third eight grains of the carbonate of magnesia kneaded up with them, and to the fourth were added 16 grains of the common crystallized carbonate of soda. The first loaf, when baked, possessed, in a very rank and strong degree, both a taste and a smell of acidity. In the second, the acidity remained faintly perceptible, especially in the smell. In the third, the loaf had no acid or other disagreeable peculiarity whatever. In the fourth, there was no acid taste, but a slightly acid smell.

These results appear quite decisive. For thus the exhibition of eight grains of the carbonate of magnesia to five ounces of dough, or about 32 grains to the pound, which is about 52 grs. to the pound of flour, proved amply sufficient to correct an acidity which had been allowed to proceed to an extreme hardly ever known in practice. And indeed in the great bulk of instances a much smaller quantity would be found completely sufficient; so that, in all probability, three ounces of carbonate of magnesia to every 100 pounds of flour would be found to serve the purpose, provided a due incorporation were effected of the magnesia throughout the substance of the bread.

The employment of the carbonate of magnesia in thus correcting the acidity of dough, appears to possess decided practical advantages when compared with the use of carbonate of soda. It has a remarkable bulkiness and elasticity, so as, when employed in excess, to produce even mechanically a considerable effect towards the lightness of the bread into which it enters. And it may be remarked that these qualities, together, perhaps, with its tendency to correct acidity, although this latter seems to have been less regarded, caused it to be recommended by Mr. Edmund Davy,* as well adapted for raising and improving the sad and doughy bread which was made from the bad flour of harvest 1816. But besides possessing these advantages, it is also more tasteless, and a less active chemical agent than the carbonate of soda. Accordingly, whenever the acid to be corrected happens to exist diffused through the sponge or dough, as it may be difficult by any care in kneading to incorporate the alkali equably with the whole mass, it is safest to use magnesia, as an accidental excess of that substance occurring in any part of the bread, will neither materially injure its flavour, nor will any activity of its alkaline powers induce a chemical change upon any of the constituents of the flour. But it may be proper to observe, that whenever the baker is led, by any circumstance, to anticipate the supervening of acescence in his dough, while yet the materials of it are unmixed up, he will do well to mingle the magnesia with the flour before either is wet, and he may thus rest secure that the salutary neutralizing effect of his corrective will be called into action throughout his dough, precisely in proportion as it is required. Its presence being thus extended through all the particles of the dough, no sooner will any acid be generated in any quarter, than it will be neutralized by the alkali. The small quantity of neutral salt which is formed by the mutual action of these two bodies does not appear at all to affect the quality of the bread. And so far from this employment of an alkaline carbonate tending to prevent the loaf from rising, wherever that substance is truly called into action, the carbonic acid gas evolved in consequence of its decomposition will materially promote the vesicularity of the bread.

It is not only, however, from the fermentative process in the dough that the baker has to apprehend the misfortune of sour bread; for it sometimes happens, though now more rarely by

* Philosophical Magazine, vol. xlviii. p. 465.

far than in former times, that the yeast becomes sour in the bake-house before it is mixed with flour at all. The remedy for this is, as may easily be supposed, of the same nature with that which has just been described. To leave no doubt upon the matter, it was subjected to the test of actual experiment, and the results were as decidedly in favour of the good effects of employing an alkali as could possibly have been anticipated. Even after the yeast had been allowed to stand over for an entire week in a warm situation, and had thereby acquired such a concentrated acidity as entirely concealed its taste and smell, the addition of an alkali had the immediate effect of restoring the natural yeasty flavour. It is only necessary to observe, that, in such a case, the alkali should be added just so long as an effervescence is thereby produced, and no longer. When sour yeast had thus been corrected, it appeared in practice to possess the fermentative principle in unimpaired activity, and to be capable of being employed in a baking process with the same success as yeast entirely recent and fresh.

There seems then to be nothing more easy and nothing more effectual than the application of this corrective of acidity. It is only surprising that an inconvenience so annoying and disagreeable, should have till now been tolerated by all classes of people, when a simple remedy lay so close at-hand.

The earlier portion of the process of bread-baking has now been discussed; we have briefly detailed the mechanical practice of the baker, and have offered a chemical explanation of the accompanying phænomena. And although the suggestion of a remedy for the mischief of supervening acescence, has proved a somewhat lengthy episode in the history of the loaf, yet it seemed a matter of too much importance to be more slightly passed over. It has now been pretty fully considered; and after the process of preparing bread for the oven has been thus examined in detail, we proceed to examine what changes are the results of baking in the oven.

The true nature of these alterations remains as yet involved in very considerable doubt and uncertainty. The first striking effect observable, is, that however active may have been the state of fermentation previously at work in the dough, before being exposed to the fire, it is immediately checked and brought to a period. But it has scarcely been yet determined what is the precise action upon the constituents of the flour which follows, and we shall now rather enumerate than discuss in detail the various alterations which probably occur.

It would appear to be the amylaceous ingredient which is the subject of the greatest subsequent change. It has already been stated as pretty certain that there does also occur in the oven the formation of a saccharine matter at the expense of any gelatinized starch which may have been formed during the early preparation of the dough. M. Vogel has further ascertained, in an experiment already quoted,* that about one-fourth of the whole quantity of the starch had been converted into a gummy matter possessing the characters of torrefied starch, and which, like it, was soluble in cold water. The gluten also, though little changed in amount, as the experiment of Vogel shows, is certainly so far affected, while in the oven, as to sustain a disunion of its particles, and to be thereby deprived of much of its adhesiveness and elasticity. But of the nature of these alterations, little further has been determined.

When these several changes have taken place, when the bread has gradually swelled to about double its bulk in the oven, and has acquired its upper and under crust, or, in other words, has become slightly torrefied in those parts which are immediately exposed to the high temperature either of the glowing floor or of the heated air of the oven, the loaf may be withdrawn, and requires only to be thoroughly cooled in order to exhibit a fair specimen of the perfection to which the modern art of bread-baking has been carried. Although it is, perhaps, impossible to assign with unerring precision to each of the several constituents of flour its peculiar function, and to each detail of process its precise effect, towards completing the perfect result of a well-made loaf, yet it may be a matter of some interest briefly to specify the peculiar share which the present state of our information would guide us to assign to each in the system. The moistening of the flour with water and the kneading of it into a homogeneous mass, is the first step towards forming the rudiments of the future loaf. The saccharine principle of flour, while it serves the purpose of communicating to the bread an agreeable relish, may also be with certainty regarded as the subject of that chemical fermentation which introduces carbonic acid gas into the system of the dough. Thus there is generated the elastic fluid within the bread which gives it lightness and vesicularity. The gluten of the flour, an ingredient peculiar to the farina of that vegetable, is of use in binding and cementing all the particles of dough into one cake, by means of the mechanical process of kneading; besides, by its tenacity, when duly diffused throughout a loaf, it extends and dilates within the oven into a thousand little cells, to imprison the contained gas, as it expands by the heat. And the remaining ingredient, starch, is not only the great basis of all bread, and the main source of nutriment in each loaf, but besides, in the oven, becoming rigid through the action of heat, it materially assists the permanent fixation of the particles of the loaf while in its most expanded form; it is often the means of yielding a certain supply of saccharine matter; and there is also a considerable portion of its

* Journal de Pharmacie, vol. iii. p. 219.

entire mass formed into a gummy substance. In regard to the albuminous principle in flour, it cannot fail to undergo coagulation in the oven, and, in consequence of its total want of retractility when thus altered, will doubtless contribute somewhat to confirm the *setting* of the bread, and enable it to retain that spongy vesicular texture which had been previously developed by the expansion of the internal elastic vapour. When these several constituents of the flour have discharged their respective functions, and the various processes of kneading, fermenting, and baking, have been duly performed, the formation of the common wheat-loaf is complete.

Such is the history of the ordinary system pursued by the baker in manufacturing the great proportion of that valuable article, bread, in so far as it may be regarded as one of the necessaries of life. For with respect to those kinds of bread, such as plain *water-biscuit* (sea-biscuit for example), into the composition of which no elastic fluid enters, their manufacture may be regarded as already described, since it merely implies that nothing relative to the fermentative process shall make part of their preparation. The mode of making them is in fact one of the simplest and least interesting pieces of common cookery that can be conceived, and even if its details had not been included within the account already given of the manufacture of the common wheat-loaf, they would scarcely have merited to be separately mentioned.

There are, however, a great many products of the baker's art, perhaps confected with spices or otherwise, which are prepared so as to belong rather to the luxuries of civilized society, than to deserve being classed among the necessaries of life. In all these cases the same essential advantage of infusing into the bread a due supply of an elastic fluid is equally felt; but from a variety of circumstances it may occur that the fermentative process may be ill adapted to attain that end. The reason of this is, that by the system of fermentation, it is in vain to expect good bread, unless a very considerable preparatory delay can be spared; and if this delay cannot be conveniently afforded, or if the more complex compound of a confected bread contains any ingredient which paralyzes the action of the ferment, the baker has found it necessary to have recourse to some other methods of introducing the elastic fluid. Many of these are sufficiently ingenious, and although none can be so interesting as that which is the means of preparing the great staple of our food, yet there is much useful and curious investigation connected with the examination of a few of these processes; to which we now proceed.

(*To be continued.*)

Article II.

Abstracts of Papers in the Philosophical Transactions for 1825, *on the peculiar Magnetic Effect induced in Iron, and on the Magnetism manifested in other Metals, &c. during the Act of Rotation.* By Messrs. Barlow, Christie, Babbage, and Herschel.

(*Continued from* p. 116.)

Account of the Repetition of M. Arago's Experiments on the Magnetism manifested by various Substances during the Act of Rotation. By C. Babbage, Esq. FRS. and J. F. W. Herschel, Esq. Sec. RS.

The curious experiments of M. Arago described by M. Gay-Lussac during his visit to London in the spring of the year 1825, in which plates of copper and other substances set in rapid rotation beneath a magnetized needle, caused it to deviate from its direction, and finally dragged it round with them, naturally excited much attention, and the investigation of their various circumstances, and of their connexion with the effects observed by Mr. Barlow in December, to be produced by the rotation of masses of iron, and described by him in a paper read to the Royal Society,* became an object of considerable interest. Accordingly, having erected at Mr. Babbage's house, in Devonshire-street, an apparatus for setting a copper-plate in rotation about a vertical axis by the aid of a turning lathe, the authors of this paper proceeded to try its effect on a magnetized needle suspended over it. The first attempt failed from the use of too small a needle; but this being replaced by a magnetic bar of considerable weight delicately suspended by a silk thread, they had the satisfaction of seeing it deviate several degrees from its point of rest in a direction corresponding with that of the rotation of the copper-plate; and on employing instead of this bar a very delicate azimuth compass, belonging to and the invention of Capt. Kater, the influence of zinc, brass, and lead, was similarly rendered sensible.

"In this first trial," they observe, "having neither the command of a very rapid rotation, nor of massive metallic discs, the deviation of the compass observed did not exceed 10 or 11 degrees. In order therefore to enlarge the visible effect, and at the same time disencumber ourselves of the limit set to it by the polarity of the needle, it occurred to us to reverse the experiment, and ascertain whether discs of copper or other non-magnetic substances (in the usual acceptation of the word)

* See the last volume of the *Annals*, p. 444.

might not be set in rotation if freely suspended over a revolving magnet. In order to make this experiment, we mounted a powerful compound horse-shoe magnet, capable of lifting 20 pounds, in such a manner as to receive a rapid rotation about its axis of symmetry placed vertically, the line joining the poles being horizontal and the poles upwards. A circular disc of copper, 6 inches in diameter and 0·05 inch thick, was suspended centrally over it by a silk thread without torsion, just capable of supporting it. A sheet of paper properly stretched was interposed, and no sooner was the magnet set in rotation than the copper commenced revolving in the same direction, at first slowly, but with a velocity gradually and steadily accelerating. The motion of the magnet being reversed, the velocity of the copper was gradually destroyed; it rested for an instant, and then immediately commenced revolving in the opposite direction, and so on alternately, as often as we pleased.

The rotation of the copper being performed with great regularity, it was evident that by noting the times of successive revolutions, we should acquire a precise and delicate measure of the intensity of the force urging it, provided we took care to neutralize the torsion of the suspending thread. To make the experiment strictly comparable proved however a matter of much delicacy, as the slightest change in the distance of the plate from the magnet was found to produce a material alteration in the time of its gyration.

Our first inquiry was directed to ascertain the effect of the interposition of different bodies as screens in cutting off or modifying the peculiar rotatory effect. The substances tried were paper, glass, wood, copper, tin, zinc, lead, bismuth, antimony, and tinned iron plate.

The metallic plates here interposed, as also the wooden ones, were circular discs of 10 inches in diameter and half an inch in thickness, the metals being all cast for the purpose, the wooden disc serving for a pattern. It was found that the various substances examined exert no sensible interceptive power. Glass in like manner had no effect; but when the substance interposed was iron, the case was widely different, the magnetic influence being greatly diminished by one, and almost annihilated by two thicknesses of common tinned iron plate. When the poles of the revolving magnet were connected by a piece of soft iron, the rotation of the copper disc was in like manner almost entirely annihilated.

Resuming now the original form of the experiment, the copper disc of 10 inches diameter and half an inch thick, was placed on the vertical axis, and made to revolve with a velocity of seven turns in a second, a velocity which it was found convenient to give, and easy to maintain, corresponding as it did with one stroke per second of the treadle of the lathe; and this velocity, unless the contrary is mentioned, is to be understood of all the rotations so communicated, spoken of in the remainder of this account.

The copper-plate thus revolving, the disc of copper first-mentioned was suspended over it; but though at first it seemed to be very slightly affected, yet on frequent and most careful repetition of the experiment, with every precaution to guard against currents of air, not the most trifling effect could be perceived. This remarkable result, while it stands opposed to any theory of magnetic vortices generated by the rotation of one body, and transferring a part of its motion to others, is, on the other hand, perfectly consonant with, and indeed a necessary consequence of the view which will be taken of the subject in the sequel.

In like manner a bar of hardened, but not magnetized steel, was very slightly, if at all, set in rotation by the revolving copper, not more than probably would correspond to the small degree of magnetism unavoidably developed in it in the act of hardening; but when magnetized to saturation, it was made to revolve rapidly. This experiment appears decisive as to the origin of the magnetic virtue exhibited by the copper and other bodies in these experiments. It is obviously *induced* by the action of the magnetic bar, compass-needle, &c. on their molecules.

Our next inquiry was directed to the degree in which this development of magnetic virtue takes place in different metals and other bodies. For this purpose two different processes were adopted. The first consisted in securing each of the 10-inch discs already spoken of, successively, on the vertical axis of our machine (which was now fitted up more firmly). Giving them thus a rotation in their own planes, the azimuth compass above-mentioned was placed on a convenient stand centrally over each at the same distance. The deviations observed, and the ratios of their sines to that of the deviation produced by one of them (copper) chosen as a standard, were as follows:

Name of the revolving body.	(Motion of the disc direct, or screwing.)	(Motion retrograde, or unscrewing.)	Mean.	Ratio of the force to that of copper.
Copper	11° 30′	11° 17′	11° 24′	1·00
Zinc............	10 7	10 15	10 11	0·90
Tin	5 30	5 12	5 21	0·47
Lead	2 50	2 55	2 53	0·25
Antimony......	1 12	1 17	1 16	0·11
Bismuth	0 6	0 6	0 6	0·01
Wood..........	0 0	0 0	0 0	0·00

The experiment was repeated (some weeks afterwards), placing the compass (by a more advantageous adjustment of the apparatus) much nearer the revolving disc. The results were as follows:

Name of the revolving substance.	Mean of deviations screwing and unscrewing.	Ratio of force to that of copper.
Copper.........	28° 54′	1·00
Zinc.	26 42	0·93
Tin............	12 54	0·46
Lead	7 0	0·25
Antimony	2 27	0·09
Bismuth	0 32	0·02

Agreeing as nearly as could possibly have been expected with the foregoing.

Of the other metals, silver appears to hold a high rank, and gold a very low one in the scale of magnetic energy. Indeed the latter metal rendered standard by copper was scarcely more powerfully set in rotation than seemed fairly attributable to the quantity of its alloy.

The examination of mercury presented peculiar interest, from its fluidity, and the facility with which iron might be excluded from the experiment; to make which a flat ring of box-wood was cemented with wax between two circular glass discs, so as to form a hollow cylinder, two inches in internal diameter, and 0·10 in its interior height. This being suspended, empty, by a long delicate silk-thread over the horse-shoe magnet, was not in the slighest visible degree affected by its rotation, however long continued. It was then detached and filled with mercury, which, from having been thrice distilled, and afterwards having stood upwards of a twelvemonth in a bottle in contact with a solution of the nitrate of that metal, might assuredly be regarded as absolutely free from iron. Being again suspended as before, it now readily, though feebly, obeyed the rotation of the magnet in either direction, being fully commanded by it, and set in motion, stopped, or reversed in its gyrations at pleasure by merely continuing or changing properly the motion of the magnet. This experiment was witnessed, among others, by our illustrious President. The place which mercury appears to hold in the scale of magnetic energy was judged to be between antimony and bismuth, certainly superior to the latter, and certainly inferior to lead.

In wood, glass, wax, rosin, sulphur, sulphuric acid, water, &c. we have not hitherto succeeded in obtaining unequivocal traces of magnetism. The experiment with unannealed glass succeeded no better than with annealed. In the case only of one non-metallic body (unless a minute portion of iron present may have deceived us) a decisive result has been obtained; and, what is very singular, this body is carbon, in that peculiar state in which its density, lustre, degree of hardness, and high conducting quality, both as regards heat and electricity, seem to give it some title to a place among the metals. This is the state in which it is precipitated by a red-heat from coal-gas. The magnetism developed in this singular substance is, however, too feeble to admit of precise measurement, and is only rendered barely sensible by delicate management.

The second process alluded to as employed by us to compare the relative magnetic forces of the different bodies examined, consists in suspending magnetized bars over revolving discs of them, and observing not the point of equilibrium but the velocity generated, or the time required for the description of certain spaces; in other words, by measuring not the statical, but the dynamical effect. These methods, for distinction's sake, may be called the statical and dynamical methods of observation.

In the original experiment of M. Arago, a magnetic needle was made to deviate or revolve by the rotation of a plate beneath it. The motion of the needle must of course be rendered irregular by the effects of its polarity, and subject to periodical accelerations or retardations; and it is obvious, that in the case of a very weak magnetic force in the plate it can never execute an entire revolution, but must oscillate backwards and forwards till reduced to rest by the friction and resistance of the air. It occurred to us, however, that much more regular and uniform results might be obtained by this means, could the polarity of the needle be destroyed without at the same time destroying its magnetism; in other words, could the earth's action on it be so precisely neutralized as to allow of its resting indifferently in all directions. The obvious mode of doing this, by the approach of a powerful magnet acting in opposition to the earth, proved much too coarse for our purpose, which, however, after a few trials, we found might be accomplished to any required degree of precision by the following simple contrivance.

If two exactly equal and similar magnets of equal strength be placed parallel to each other, but in a reverse position, and at such a distance as not mutually to affect each other's magnetism, and if in this situation they be firmly attached to a piece of wood, glass, &c. the system so formed will have no polarity, i. e. no tendency to rest in one rather than another situation, however suspended. This is clear; because whatever be the inclination (θ) of one of the magnets to the line of dip, that of the other will necessarily be ($180 + \theta$), and the directive forces, being represented by the sines of these two angles, will always be equal and opposite, so that each magnet urges the system with equal force, but in opposite directions. The truth of this proposition, it is no less evident, is independent of the axis of suspension, which may pass through a part of the system any how situated with respect to the magnets, in virtue of the property of a magnet whose force to turn a system, of which it makes a part, round a fixed centre, is the same whenever it is placed in the system, and the same as if it were in the centre.

Hence it follows, that if two equal and similar magnets be laid parallel to each other, but in a reversed position, on a horizontal glass plate, freely suspended by a thread, the system will be devoid of any polar tendency, (which we shall express by calling such a system *neutral.*) It is difficult however to procure two magnets exactly equal, and of equal force. But fortunately this is of no consequence, as a slight deviation from perfect neutrality may be corrected by inclining the stronger needle a little more or less to the plane of the plate. In fact the proposition is general; and by a proper adjustment of the positions of two magnets however unequal, with respect to the axis and to each other, they may be made to neutralize each other.

As this adjustment however is nice, and as magnets influence each other, and our object moreover called for the utmost delicacy, we adopted a more refined application of the principle just detailed. A circular glass disc was prepared, eight inches in diameter, and suspended by three silk threads from a filament of silk, descending along the axis of a copper tube about five feet long, passing with stiff friction through collars in the cieling of the apartment, and serving nicely by means of an index to regulate the height of the glass disc.

At the opposite extremities of two diameters at right angles to each other, four equal small bar magnets were fixed in a vertical position, having alternately their north and south poles downwards. This position promised to present two material advantages; first, that in neutralizing the system we have not the whole polarity of the magnets to contend with, but only the small remains of directive tendency which arises from the magnetic axis in each not being precisely coincident with its axis of figure, since it is evident that an infinitely thin magnetic cylinder placed perpendicularly to the horizon, would from that cause alone be indifferent as to situation; secondly, that in this situation their poles interfere with each other's action on the plate revolving below them, less than in any other. Instead of four we might (and as will be seen occasionally did) place a greater number of magnets round the circle, or within its area, but for the experiments now in view four were enough.

The system so constructed was found to require no after adjustment, being to all appearance perfectly neutral, so that this part of our purpose was completely accomplished, and the earth's action eliminated from the inquiry. The irregular torsion of the silk-thread however still embarrassed us a good deal. But though this undoubtedly caused individual results to differ more from the mean than we had expected, it is not sufficient to account for a singular anomaly observed not only in the mean results of a great number of trials, but in all individual cases; viz. that by this mode of observation, zinc was invariably found to stand above copper in the scale of magnetic action, whereas in the determination by the statical method, where the deviation of the compass was observed, the former metal was as invariably found to be placed below the latter, the other metals retaining their order.

Comparing the means obtained by the apparatus just described of all the lines in the table constructed of the results for copper, zinc, tin, lead, and of the six first for copper and antimony, the proportional intensity of magnetic action for each respectively will be

Zinc	1·11
Copper	1·00
Tin	0·51
Lead	0·25
Antimony	0·01

The smallness of the number for antimony is here also very remarkable. That for bismuth deduced by this means would be still more minute, so small indeed that the torsion of the thread would not allow of its magnitude being fairly determined, the suspended system merely performing extensive oscillations in very long times.

This method however requires us to operate on very considerable quantities of the substances under examination, a great disadvantage, as it cannot be applied to the scarcer metals, and does not admit of the use of the common ones in a state of rigorous purity. A method at once more simple and expeditious, and allowing of our acting on small quantities of matter, is to suspend portions of the different bodies we would try, similar in form and exactly equal in size, over the revolving magnet, and noting either, dynamically the times of successive revolutions, or, statically the point of equilibrium between the rotatory force and the torsion of the string. This method we pursued in a very interesting part of the inquiry, viz. in investigating (after M. Arago) the effect of a solution of continuity, partial or total, in the mass acted on.

A disc of lead of two inches in diameter and one-tenth thick, was suspended in a small thin wooden tray at a given distance from the horse-shoe magnet, revolving with the usual velocity, at first entire, and then successively cut with a chisel in radii nearly up to the centre, as represented in the following page.

Fig. 1.

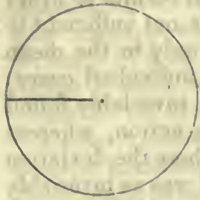


Fig. 2.

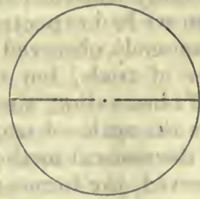


Fig. 3.

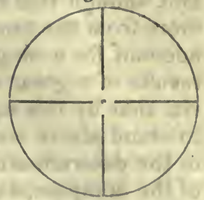
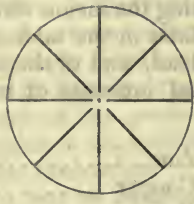


Fig. 4.

Fig. 5.



The times observed and forces deduced in the several cases were as follows:

Rev.	Disc uncut.		Disc cut as in Fig. 1.		Disc cut as in Fig. 2.		Disc cut as in Fig. 3.		Disc cut as in Fig. 4.		Disc cut as in Fig. 5.	
	$t=$	$f=$	$t=$	$f=$	$t=$	$f=$	$t=$	$f=$	$t=$	=	$t=$	$f=$
1	28·2	1258	30·9	1047	33·1	913	42·1	564	48·1	432	55·6	324
2	41·2	1178	44·5		47·4		59·8		69·0		81·4	302
3	50·6	1172	55·0		59·0		74·7		86·6		103·3	281
4	58·7	1161	63·9		68·3		88·0		102·1		124·5	258
5	66·4	1134	72·0		77·2		100·0		116·8		145·9	235

Similar effects were observed in other metals, but in different degrees. For instance, in the case of soft tinned iron, the same number of cuts, made in the same manner, produced a very slight diminution of force, while in copper the effect of the same operation was to reduce the force in the ratio of 1 to 0·20.

A thin disc of copper suspended at a given distance over the revolving magnet, performed six revolutions from rest in $54^{s}·8$. It was then cut in eight places in the direction of radii nearly up to the centre and 45° asunder, by which operation its magnetic virtue was so weakened, that it now required $121^{s}·3$ to execute the same number of revolutions. The cuts were now soldered up *with tin*, and the magnetic action was now found to be so far restored as to enable it to perform its six revolutions in $57^{s}·3$, that is to say, very nearly in the same time as when entire. This is the more remarkable, since tin, as we have seen, is not above half so energetic as copper when acting directly. This indirect mode of action, therefore, affords us a means of magnifying small magnetic susceptibilities, which may hereafter prove very valuable.

To illustrate this more strongly, we suspended a brass disc of 2·25 inches in diameter, and 0·15 inch in thickness, as in the last case, and noted the time of its performing successive revolutions as follows:

1 rev.	2 rev.	3 rev.	4 rev.	5 rev.
20s·2	29·1	35·2	40·8	45·7

It was now cut, as in the last case, but it being necessary for this purpose to use a saw, the abraded portions, which were pretty copious, were strewed over it with the intervention of a piece of thin paper, to obviate the effect of loss of weight, as nearly as might be. The times were now found increased as follows:

1 rev.	2 rev.	3 rev.	4 rev.	5 rev.
41·1	57·9	71·0	83·0	93·7

being almost exactly doubled, and of course the force was reduced in the ratio of about 4 to 1.

The cuts were now cleanly soldered with bismuth; and though, as we have seen, the direct force of bismuth is so small as to be scarcely perceptible, yet its indirect effect in restoring the magnetism of the brass was such as to cause the same arcs to be described in the following numbers of seconds:

1 rev.	2 rev.	3 rev.	4 rev.	5 rev.
28·2	39·7	48·4	56·3	63·0

which require the exertion of an accelerating force more than double that developed in the last trial.

The bismuth was now melted out, and the cuts being carefully washed with melted tin, were filled with fresh tin, which was allowed to fix, and the disc being trimmed, and replaced, the times were now found to be

21·7	30·8	38·0	43·5	48·7

The restoration of energy, as in the case of the copper disc, is here very manifest, the times of rotation being nearly reduced to their original magnitude. The comparison of these gives for the accelerating forces

Brass, uncut	1·00	Copper, uncut	1·00
cut	0·24	cut	0·20
soldered with bismuth	0·53	soldered with tin	0·91
soldered with tin	0·88		

The effects of soldering with lead and with fusible metal were also tried, and found to be both represented on the same scale by the same fraction, viz. 0·85, being but very little inferior to tin.

When the soldering is imperfect, the effect in restoring the magnetic action is proportionally weaker, but the influence of ever so small a free metallic communication is sensible.

A disc of lead cut in 8 radii as above was found to make one revolution in 58s·3. It was then wetted so as to fill the cuts with sulphuric acid, and the time of revolution was found to be 57·3; so that the influence of sulphuric acid, even when thus magnified, is still equivocal; and its magnetism, if it exists, can hardly be estimated at a thousandth part of that of copper, and is probably still lower.

The reduction of the metals to filings or to powder, was found to produce a still more striking diminution of their magnetic energy; and a class of experiments of great interest, as to the effect of the agglutination of these powders by metallic and non-metallic cements and liquids, immediately presents itself; into which want of leisure only has hitherto prevented our entering, as well as on the important subject of the magnetism of metallic alloys and atomic combinations, with which this branch of the inquiry is essentially connected.

(*To be continued.*)

Article III.

On the Rationale of the Formation of the Filamentous and Mamillary Varieties of Carbon; and on the probable Existence of but two distinct States of Aggregation in Ponderable Matter. By E. W. Brayley, Jun. ALS.

(To Richard Phillips, Esq. FRS. &c.)

DEAR SIR, *July* 7, 1826.

I have perused with much attention and interest Dr. Colquhoun's paper on the new capillary variety of carbon, &c. in the *Annals* for the present month; and I beg to offer a few remarks on some of the phænomena and substances in question, which you may, perhaps, deem worthy of insertion as supplementary to his communication. Dr. Colquhoun regards the mode of formation of the filamentous carbon he so minutely describes, as inexplicable by any facts which chemistry can as yet furnish; and he considers the evidence afforded by the external characters of that and some of the mammillated varieties, of their forms having been assumed out of a state of fusion, to be, when viewed in conjunction with the known infusibility of carbon, anomalous in the extreme. I may possibly have overlooked or underrated some fact in the case, which has a material bearing on the cause of the phænomena to be explained; but it does appear to me that these phænomena are susceptible of a ready and satisfactory explanation, for which Dr. C. himself has provided the materials.

For the purpose of showing this, a brief recapitulation of the principal facts related will be necessary. A current of an aëriform combination of carbon is made to act on iron heated nearly to whiteness, and defended from the action of atmospheric air. The gas undergoes decomposition, a portion of its carbon combines with the iron, producing steel, whilst another portion is precipitated in various forms, but in a state of purity. And the characters of some of the varieties of the carbon thus produced, indicate them to have been in a state of fusion at the moment of their formation. But Dr. Colquhoun views the infusibility of carbon at the temperature to which the gas is subjected, as being at present an insuperable difficulty in the explanation of the phænomenon; as one which renders it impossible to understand the nature of the process by which the carbon is deposited, either in the new method of making steel, or in the operation of obtaining gas from coal.

I may here observe, before proceeding to offer what I conceive to be the rationale of this process, that the statement made by Dr. C. that carbon has not exhibited even a tendency to fusion in the most intense *artificial* heats, is not exactly correct; for although it is unquestionably one of the most infusible of bodies, yet distinct evidence of the partial fusion both of graphite and of the diamond, were obtained by the late Dr. E. D. Clarke, in his experiments with the oxy-hydrogen blowpipe. But even if there were no evidence of the fusion of carbon by artificial means, it is not obvious, I think, why that circumstance should militate against the belief, that a substance bearing every appearance of having once been liquid, should in reality have been in that state. All that could fairly be said, would be, that in experiments instituted for the purpose of endeavouring to fuse carbon, it remained infusible, at a temperature equal or even superior to that to which it could have been subjected, by the process in question. And this would be merely one of the many cases in the arts dependent on chemical principles, in which certain phænomena take place, *naturally*, as it were, in certain operations, that cannot be produced by experiments expressly directed to those objects; and which, in fact, are as inexplicable, and as difficult of access, as many of the operations in which the powers of nature are alone concerned.

And now, merely remarking that my reason for regarding the fusion of *graphite* as part of the evidence of that of *carbon* having been effected, will appear in a subsequent communication, I proceed to submit, with much deference, the rationale of the formation of the varieties of carbon in question, which the consideration of the facts has suggested to me; and which, by reconciling them to each other, removes the supposed anomaly.

It is simply this: That the carbon, existing in the carburetted hydrogen in the gaseous state, retains that state upon its separation from the hydrogen, and thence, as we know that gaseous carbon must require a temperature of unknown intensity for its preservation in that form, passes into the liquid state. This, however, in consequence of the high temperature required for its liquefaction, a temperature far exceeding that of the steel-chest or the gas-retort, it cannot retain, (a circumstance which is in perfect agreement with the great infusibility of carbon as evinced by experiment;) and it passes, instantaneously, into a state of solidity; its liquefaction, of course, taking place, "only in the moment of mamillary or filamentous formation." The result of this operation would certainly be, the production of substances with characters indicating their forms to have been "assumed out of a state of fusion." And yet the fact of the great infusibility of carbon, instead of being contradicted by the apparent phænomena, would be in reality a chief cause of the appearances exhibited by the substances. The instantaneous passage from the liquid to the solid state, would prevent the agency of the surrounding gaseous substances, of the iron, or of the earthen vessel, from interfering with the cohesive affinity and tendency to union of the particles or molecules of solid carbon resulting from the transition, and they would of course unite in the freest manner, and according only to their own inherent properties, independently of the action of the contiguous bodies. And the characters of the forms of carbon so produced, appear indeed to indicate that this was actually the case. One of them consists of fine capillary filaments. Now all we know, I believe, of corpuscular forces, and of their agency in the production of solids, would lead us to believe, that the sudden production and fixation of the solid particles of carbon, supposed in the explanation I have ventured to give, would cause an aggregation of them *in one direction* only, which would of course produce a collection of filaments; the suddenness of the operation preventing the attraction of crystallization from having place. That attraction would scarcely have time for incipient agency, before the carbon would have ceased to be amenable to its influence. Nor does the mamillary form of the other variety in any way oppose this hypothesis: all the other mamillary concretions in nature with which we are acquainted, (and on examination of the carbon it will doubtless be found to be the case with that,) arise from the aggregation of fibres radiating from a centre in every direction, and tending thus to form spheres, the complete formation of which, however, is interrupted and prevented, by the fibres proceeding from different centres meeting and intercepting each other in the operation, by which the spheres become irregularly compressed, modified, and intermingled. That such is the structure of mamillary concretions, the examination of a specimen of botryoidal iron-stone, or of malachite, will convince every observer. And the tendency to unite around *centres*, which all bodies manifest, when at liberty to exert their inherent qualities, unaffected by surrounding or contiguous substances, would produce this mamillary structure, as in the instances just mentioned, from the filaments already formed by the attraction before described.

I cannot but think it probable, that if this carbon could be kept in fusion, and be slowly instead of rapidly cooled, the atoms would arrange themselves in several directions at once, or in a crystalline arrangement, and that thus the diamond would be the result. We know crystallization to be in all cases a gradual process, and to arise from liquidity continued for a sensible portion of time. On the other hand, the instantaneous transition from the liquid to the solid state appears to be the cause of the pulverulent or irregular form of the varieties of carbon, obtained by passing alcohol and certain oils through ignited porcelain tubes. And it may be remarked, that the carbon produced by the decomposition of the gas, in Mr. Macintosh's process for making steel, must necessarily be in a state to exert its inherent properties, much more freely and independently, than in the cases last referred to; and hence the greater approximation of the carbon, separated in that process, to what we may suppose to be, pre-eminently, the *proper* form of that substance. As so many other substances, when in their nascent state, exhibit their properties in a much more decided manner than at other times, this may also be one of the reasons why this carbon is in a denser state of aggregation, &c. than in its more common forms, obtained under circumstances less favourable to the free development of its inherent properties.

But it may, perhaps, be objected to the hypothesis I have advanced on the formation of the mamillary and filamentous varieties of carbon, that we have no proof that the liquid state necessarily intervenes between the aëriform and the solid states, and that, for aught we know, a body may pass directly from the former to the latter. It appears to me, however, that the great bulk of facts in Chemistry, in which the passage of bodies from one state to another is concerned, together with all that we know of the production of vapours from liquids, and the condensation of vapours into liquids reciprocally, points to the conclusion, that the liquid is a necessary and an invariable intermediate state to the solid and the gaseous. I shall not, however, content myself with this general statement. I shall first urge some reasons for thinking that the above is the case in all instances, derived from some recent discoveries in chemistry; and then proceed to adduce experimental evidence that it actually occurs in some cases, distinguished from others only by the circumstance of their taking place at temperatures sufficiently low, to allow of their being made subjects of experimental observation.

It will be sufficient merely to state that Mr. Faraday's experiments on the liquefaction of gases,* have annihilated the distinction formerly believed to exist between *gases* and *vapours*, and have shown them to differ merely in density, and consequently that the passage from the rarest gas to the densest vapour is by indefinable and continuous degrees. In like manner, the experiments of M. Cagniard de la Tour on the combined action of heat and pressure on certain liquids,† together with some now well-known facts indicated by the phænomena attending the production of steam from the *generator* of Mr. Perkins's steam-engine, show, I think, that no line of demarcation can be drawn between *vapours* and *liquids*, but that they likewise pass into each other in a perfectly continuous manner, by imperceptible degrees. It appears to me, in short, that the aggregate of our knowledge respecting the different forms of ponderable matter, as regards their relations to latent heat, according to the received doctrine on the cause of fluidity, leads directly to the conclusion, that there are only two such forms, the *solid* and the *fluid;* the distinction of the latter into *aëriform* and *liquid* being still however retainable with propriety as a matter of convenience, though there seems every reason to believe it a distinction that has no real existence. The accurate reasoning of Mr. Graham, I may also remark, in his observations on the absorption of gases by liquids, quoted in the same number of the *Annals* in which Dr. Colquhoun's paper appears, as well as the experimental evidence he cites, is entirely favourable to this view of solidity and fluidity being the only distinct physical forms, or states of aggregation, of ponderable matter; the gaseous and the liquid states being merely continuous degrees of one and the same form.‡

* See Phil. Trans. for 1823; or *Annals*, N. S. vol. vii. p. 89.

† Ann. de Chim. et de Phys. tom. xxi.; or *Annals*, N. S. vol. v. p. 290.

‡ Since the above paragraph was written, I have read a paper by Professor Oersted, published in Schweigger's Journal for December last, in which are detailed the results of a series of experiments made by him and Capt. Schwendsen, with the view of determining whether the law of the compression of aëriform bodies discovered by Mariotte extended to high pressures, which had been doubted by certain mathematicians. The result was the complete verification of Mariotte's law for all pressures, whilst the gases retain their aëriform state. At the conclusion of the paper, Prof. Oersted expresses his opinion, derived from experiment, that *liquids* are subject to the same law; and if this shall, on further investigation, prove to be the fact, it will tend to confirm my opinion, given above, that the gaseous and the liquid states are essentially the same.

The following is a translation of the passage:—"The compression of liquid bodies reducible to drops, is, as far as our experience yet goes, subject to the same law; here, too, the compression and the compressing power seem to bear a direct relative proportion. We may, therefore, assume, that the gases, converted into liquids reducible to drops, begin again to follow the same law to which they answered as gases. If this should be confirmed by further experiments, it may be said that the compression of

This being premised, the next step in the inquiry is to ascertain what are the means, exclusive of crystallization, of reducing a rarer to a denser form of matter. But two modes of doing this are at present known; viz. diminution of temperature; and the approximation of the molecules of bodies by mechanical means, in which an evolution of caloric takes place. The production of solid from aëriform matter by the combination of two gaseous bodies, is not to be regarded as a third method of effecting this; for it is merely a case of the first mode just mentioned; the temperature at which the elements of the solid which is formed are retained in the gaseous state, being insufficient to allow their combination to remain aëriform. The production of solid carbonate of ammonia by the union of the carbonic acid and ammoniacal gases, is a familiar illustration of this process.

Now, as already observed, there is every reason to believe, that no definite distinction exists between the state of gas and liquid, but that the only physical difference between the lightest gas and the heaviest liquid, is in density; the intermediate degrees being supplied by vapours and liquids increasing in density in the most gradual manner. We know too that the first result of the application of cold to a gas susceptible of reduction to the liquid state, is its condensation; we know that this condensation goes on with the diminution of temperature, until at length, when the process has been carried to a sufficient extent, the result is the successive production of dense vapour and of liquid. It affords no argument against what I am advancing, that the *combined* application of cold and pressure is in many cases necessary to effect this; for each successive stage in the condensation is produced, with the one agent, by the abstraction of caloric, and attended, with the other, by its evolution; so that the passage of latent into sensible heat takes place in the same manner, and is as materially concerned in the process, as would be the case, were either method to be employed exclusively.

With this train of consistent phænomena before us, and with

bodies ceases to conform to these rules, only in the moment of their transition from one state of aggregation to another."

The reader, however, will not fail to perceive, that a circumstance alluded to in the last clause, is unfavourable to my opinion; and as the statement is one of some importance, and I am not quite satisfied of the accuracy of the foregoing translation, it may be as well to subjoin the extract in the original German.

"Die Compression tropfbar flüssiger Körper ist, so weit bis jetzt unsere Erfahrungen reichen, demselben Gesetze unterworfen; auch hier scheint Compression und Druckkraft im Verhältniss zu stehen. Man kann daher annehmen, dass die zu tropfbaren Flüssigkeiten umgewandelten Gase von Neuem anfangen dem nämlichen Gesetze zu folgen, welchem sie als Gase entsprachen. Auch ist es ziemlich wahrscheinlich, dass die in feste Körper umgewandelten Flüssigkeiten jenem Gesetze unterworfen sind. Wenn sich diess durch weitere Versuche bestätigt, so kann man sagen, dass die Zusammenpressung eines Körpers nur allein in den Uebergangsmomenten aus einen Aggregations-Zustand in den andern aufhöre sich nach jenem Gesetze zu regeln."

the general fact, in addition, that in all cases completely within our sphere of observation, the *solid* results from the *liquid* state, may we not fairly infer, that the liquid state invariably takes place between the aëriform and the solid?—that it is a necessary and inevitable step in the production of solid from gaseous matter?

I now proceed to adduce some experimental evidence on this point. In the sublimation of bodies which do not require a very elevated temperature for their volatilization, and which are, therefore, fully open to observation during the process, such as benzoic acid, some of the salts of ammonia, and sulphur, we may observe the following facts. If the temperature of the vessel or portion of the vessel into which the vapour rises, is insufficient to retain the substance, for a sensible interval, in the liquid state, the result is an indistinctly fibrous or a compact mass, bearing, however, marks of fusion. But if the heat is sufficient to allow the condensed vapour to remain liquid a sensible portion of time, more or less perfect, though usually minute *crystallization* is the result. The former of these cases appears to me to be precisely similar to that of the production of the filamentous and the mamillary carbon. And as an instance in nature of a corresponding kind, I may refer to the various forms of volcanic sulphur, when viewed in connexion with the chemical and geological circumstances under which they are produced.

The evidence which I have given, tending to show that the liquid state always intervenes between the solid and the aëriform, also induces me to believe, that in the transfer of carbon from the negative to the positive pole of the Deflagrator, observed by Dr. Hare,* the carbon is first liquefied, and then evaporated, being driven in vapour to the opposite pole by the galvanic current. This is the converse of the deposition of carbon bearing marks of fusion: in that case the temperature is insufficient to preserve the carbon in a liquid form; in this, it is too high to allow that form to be retained, the substance immediately acquiring elastic fluidity.

It may be useful to add a few words in explanation of the various forms assumed by the deposited carbon. I have already suggested an idea on the origin of the capillary form; and I may here remark, in continuation, that though for various reasons already stated, we must not regard crystallization as having been, in any material degree, concerned in its production, yet a species of *polarity*, as Dr. Colquhoun has already observed, has undoubtedly had an influence in its formation; and it appears to me, that the tendency to a rectilinear direction, which so many of the most refined investigations of modern science have shown both ponderable and imponderable matter to possess, or the tendency to *polarization*, is prior, in its agency

* See *Annals*, N. S. vol. iv. p. 121.

in ponderable substances, even to the crystalline attraction itself; and that it is in fact the first cause of solidity. Thus the solidification of water commences with the production of fibres or needles, which, by their lateral aggregation and intersection at certain angles, at length produce the solid congeries of crystals we term ice. This view does not oppose the sagacious inference of Dr. Young, that a more or less perfect crystallization is the universal cause of solidity;* for the tendency I have mentioned would of itself produce an aggregation of matter of one dimension only, viz. that of length; whereas the tendency to aggregation in several directions, or crystalline attraction, produces the other dimensions of solidity, breadth and thickness.† In the case of the fibres of carbon, crystallization has been exerted to a sufficient extent to give them sensible thickness. Their collection into *locks* would of course result from the mutual lateral attraction of the fibres, when formed. There are many instances in the mineral kingdom, of two substances belonging to the same mineral-species, or, in other words, to the same material substance, which are very nearly as different from each other, as are these fibres of carbon from the diamond. For example: who, at first sight, would imagine, that the silky, white, light, perfectly flexible, and nearly opaque fibres of *amianthus,* were identical in their specific nature with the green, transparent, prismatic, rigid, and comparatively hard and heavy crystals of *actynolite?* yet such is the fact; and some mineralogists have accordingly classed them in their systems as varieties of the same mineral.‡ In this case, the polar attraction appears chiefly to have had place in the production of the amianthus, as in that of the carbon; whilst the actynolite has resulted from the unchecked influence of crystalline attraction.

That the slaty carbon of the gas-retorts should be the result of a gradual and continued process, Dr. Colquhoun appears to consider the most unaccountable part of the phænomena he has so well detailed. If, however, the solution I have suggested of the principal problem is correct, this gradual accumulation of carbon may be explained in the following manner. Dr. C. states, that the stratified portions of this carbon are quite compact in the small. Now is it not reasonable to infer, that each separate stratum is the result of one process of rapid solidifica-

* See his Lectures on Natural Philosophy, vol. i. p. 627.

† Some evidence, I think, that this tendency to linear arrangement, or to polarization, is the first power operating on solid particles of matter towards their aggregation, is afforded by the facts pointed out by Sir E. Home and Mr. Bauer, that the most minute fibres in nature with which we are yet acquainted, those constituting the muscles and nerves of animals, consist each of a simple row of attached globules; and that the commencement of the operation by which coagulated blood becomes vascular, is the attachment together in one direction of its constituent globules, forming such fibres.

‡ See, in particular, Prof. Mohs's Treatise on Mineralogy, translated by Haidinger, vol. i. p. 388.

tion, from the liquid to which the gaseous carbon is reduced; so that the successive depositions, arising from the repeated productions of carbon, form the stratified mass?

I intended to have concluded this paper with a few remarks, suggested by Dr. Colquhoun's details, on some other forms of carbon, existing in nature, as well as resulting from the processes of art; and on the prevalence of that body throughout nature, and its presence, as well as that of sulphur, in every stage of the formation of the crust of the earth, from that in which the primary rocks were produced, down to the era of the newest tertiary beds. But I have extended the foregoing observations to so great a length, that I must reserve these for a future opportunity. It would, however, be uncandid, as well as unphilosophical, were I to conclude, at present, without noticing a fact, that appears to militate, in some degree, against the hypothesis I have advanced, that the liquid state is necessarily intermediate between the solid and the aëriform; and which I must acknowledge I do not as yet understand. This is the *evaporation* of ice and snow, without any previous visible liquefaction, at temperatures that would immediately condense aqueous vapour from any other source, into snow itself. The most satisfactory experiments on this subject, that I am aware of, are those recorded by Mr. Luke Howard. In one of them, 2600 grains of hard snow lost 27 grains by evaporation in ten hours, the temperature varying only between 12° and 28°.* Perhaps, however, the fact may admit of this explanation. The tendency to assume the vaporous state, which is sufficiently strong to overcome the cohesive attraction of the solid ice, at such low temperatures, may also be sufficient, to cause the instantaneous passage of the water, its first operation on the ice must be supposed to produce, into aqueous vapour. At all events, the existence of the vapour of water at a temperature so far below that required for its solidification, with the additional condensing power of radiation from the surface of the snow, is as apparently anomalous, as the instantaneous production and evaporation of water, presumed in my view of the case, can be imagined to be.

Before I terminate this communication, I would also notice the production of a solid ingot of copper from the solution of a salt of that metal, which Dr. Colquhoun alludes to as being so anomalous. It is possible, that, in this case, as well as in that of the formation of the capillary carbon, I may have overlooked the difficulty. But I am unable to perceive any thing in the process, that cannot be readily explained. The oxide in the solution of a cupreous salt is diffused throughout the solution, of which every drop contains a portion. Now when this oxide is reduced, by its oxygen passing to the plate of iron immersed

* Climate of London, vol. i. notes to Tab. xc.

in the solution, an indefinite number of particles of metallic copper are produced in the fluid, at insensible distances from each other, and there is nothing that can interfere with the powerful cohesive attraction they must have for each other, especially in this nascent state: they are under the most favourable circumstances for aggregating into a mass of metal, and such a mass they accordingly form. It seems, indeed, difficult to conceive, that any other should be the result.

The consideration of these subjects has led me, perhaps, into details somewhat irrelevant; and it may be thought that there are too many *postulata* to be granted, before what I have advanced on the origin of the new varieties of carbon can be received: this may probably be the case; but I deemed it most ingenuous to state my views on the subject, in the order in which they were presented to my own thoughts; and I may be permitted to hope, that the simplicity of the explanation that has occurred to me, and the facts I have adduced in support of it, and of the inductions on which it is founded, will meet with some attention. It may also be observed, that the essential identity of the liquid and aëriform states, is not a *necessary* part of the explanation I have given of the manner in which the filamentous carbon is produced; nor of the grounds for believing, that the liquid always intervenes between the gaseous and the solid states.

Should what I have offered prove in any degree useful to the chemist to whom we are indebted for the discovery and account of the substances in question, in any further researches on them he may be disposed to undertake, I shall consider that I have not made these observations in vain. And as they have insensibly assumed a somewhat critical tone, I beg to disavow any intention of animadverting on Dr. Colquhoun's useful paper with that view; my aim having been, solely, to endeavour to develope the causes of the phænomena he describes. I am, Dear Sir, yours very truly,

E. W. Brayley, Jun.

Article IV.

On the Mutual Action of Sulphuric Acid and Naphthaline, and on a new Acid produced. By M. Faraday, FRS. Corresponding Member of the Royal Academy of Sciences, &c.*

In a paper on New Compounds of Carbon and Hydrogen, lately honoured by the Royal Society with a place in the Philosophical Transactions, I had occasion briefly to notice the pecu-

* From the Philosophical Transactions for 1826, Part II.

liar action exerted on certain of those compounds by sulphuric acid. During my attempts to ascertain more minutely the general nature of this action, I was led to suspect the occasional combination of the hydro-carbonaceous matter with the acid, and even its entrance into the constitution of the salts, which the acid afterwards formed with bases. Although this opinion proved incorrect, relative to the peculiar hydro-carbons forming the subject of that paper, yet it led to experiments upon analogous bodies, and amongst others, upon naphthaline, which terminated in the production of the new acid body and salts now to be described.

Some of the results obtained by the use of the oil-gas-products are very peculiar. If, when completed, I find them sufficiently interesting, I shall think it my duty to place them before the Royal Society, as explicatory of that action of sulphuric acid which was briefly noticed in my last paper.

Most authors who have had occasion to describe naphthaline, have noticed its habitudes with sulphuric acid. Mr. Brande, several years since,* stated that naphthaline dissolved in heated sulphuric acid "in considerable abundance, forming a deep violet-coloured solution, which bears diluting with water without decomposition. The alkalies produce in this solution a white flaky precipitate, and if diluted the mixture becomes curiously opalescent, in consequence of the separation of numerous small flakes." The precipitate by alkali was probably one of the salts to be hereafter described.

Dr. Kidd observes,† that "it blackens sulphuric acid when boiled with it; the addition of water to the mixture having no other effect than to dilute the colour, neither does any precipitation take place upon saturating the acid with ammonia."

Mr. Chamberlain states,‡ that sulphuric acid probably decomposes naphthaline, for that it holds but a very small quantity in solution. The true interpretation of these facts and statements will be readily deduced from the following experimental details.

1. *Production and Properties of the new Acid formed from Sulphuric Acid and Naphthaline.*

Naphthaline, which had been almost entirely freed from naphtha by repeated sublimation and pressure, was pulverized; about one part with three or four parts by weight of cold sulphuric acid were put into a bottle, well shaken, and left for 36 hours. The mixture then contained a tenacious deep-red fluid, and a crystalline solid; it had no odour of sulphurous acid. Water being added, all the liquid and part of the solid was dissolved; a few fragments of naphthaline were left, but the

* Quarterly Journal of Science, viii. p. 289, 1819.

† Philosophical Transactions, 1821, p. 216.

‡ *Annals of Philosophy*, N. S. vi. p. 136, 1823.

greater part was retained in solution. The diluted fluid being filtered was of a light-brown tint, transparent, and of an acid and bitter taste.

For the purpose of combining as much naphthaline as possible with the sulphuric acid, 700 grains, with 520 grains of oil of vitriol, were warmed in a Florence flask until entirely fluid, and were well shaken for about 30 minutes. The mixture was red; and the flask being covered up and left to cool, was found, after some hours, to contain, at the bottom, a little brownish fluid, strongly acid, the rest of the contents having solidified into a highly crystalline mass. The cake was removed, and its lower surface having been cleaned, it was put into another Florence flask with 300 grains more of naphthaline, the whole melted and well shaken together, by which a uniform mixture was obtained; but opaque and dingy in colour. It was now poured into glass tubes, in which it could be retained and examined without contact of air. In these the substance was observed to divide into two portions, which could easily be distinguished from each other, whilst both were retained in the fluid state. The heavier portion was in the largest quantity; it was of a deep-red colour, opaque in tubes half an inch in diameter, but in small tubes could be seen through by a candle, or sun-light, and appeared perfectly clear. The upper portion was also of a deep-red colour, but clear, and far more transparent than the lower: the line of separation very defined. On cooling the tubes, the lighter substance first solidified, and after some time the heavier substance also became solid. In this state, whilst in the tube, they could with great difficulty be distinguished from each other.

These two substances were separated, and being put into tubes, were further purified by being left in a state of repose at temperatures above their fusing points, so as to allow of separation; and when cold, the lower part of the lighter substance, and the upper, as well as the lower part of the heavier substance, were set aside for further purification.

The *heavier substance* was a red crystalline solid, soft to the nail like a mixture of wax and oil. Its specific gravity was from 1·3 to 1·4, varying in different specimens; its taste, sour, bitter, and somewhat metallic. When heated in a tube, it fused, forming as before a clear but deep-red fluid. Further heat decomposed it, naphthaline, sulphurous acid, charcoal, &c. being produced. When heated in the air it burnt with much flame. Exposed to air it attracted moisture rapidly, became brown and damp upon the surface, and developed a coat of naphthaline. It dissolved entirely in alcohol, forming a brown solution. When rubbed in water a portion of naphthaline separated, amounting to 27 per cent. and a brown acid solution was obtained. This was found by experiments to contain a peculiar acid mixed with a little free sulphuric acid, and it may conveniently be called *the impure acid.*

The *lighter substance* was much harder than the former, and more distinctly crystalline. It was of a dull-red colour, easily broken down in a mortar, the powder being nearly white, and adhesive like naphthaline. It was highly sapid, being acid, bitter, and astringent. When heated in a tube it melted, forming a clear red fluid, from which, by a continued heat, much colourless naphthaline sublimed, and a black acid substance was left, which at a high temperature gave sulphurous acid and charcoal. When heated in the air it took fire and burnt like naphthaline. Being rubbed in a mortar with water, a very large portion of it proved to be insoluble; this was naphthaline; and on filtration the solution contained the peculiar acid found to exist in the *heavier substance,* contaminated with very little sulphuric acid. More minute examination proved that this *lighter substance,* in its fluid state, was a solution of a small quantity of the dry peculiar acid in naphthaline; and that the *heavier substance* was an union of the peculiar acid in large quantity with water, free sulphuric acid, and naphthaline.

It was easy by diminishing the proportion of naphthaline to make the whole of it soluble, so that when water was added to the first result of the experiment, nothing separated; and the solution was found to contain sulphuric acid with the peculiar acid. But reversing the proportions, no excess of naphthaline was competent, at least in several hours, to cause the entire disappearance of the sulphuric acid. When the experiment was carefully made with pure naphthaline, and either at common, or slightly elevated temperatures, no sulphurous acid appeared to be formed, and the action seemed to consist in a simple union of the concentrated acid and the hydro-carbon.

Hence it appears, that when concentrated sulphuric acid and naphthaline are brought into contact at common, or moderately elevated temperatures, a peculiar compound of sulphuric acid with the elements of the naphthaline is produced, which possesses acid properties; and as this exists in large quantity in the heavier of the bodies above described, that product may conveniently be called the *impure solid acid.* The experiments made with it, and the mode of obtaining the pure acid from it, are now to be described.

Upon applying heat and agitation to a mixture of one volume of water, and five volumes of impure solid acid, the water was taken up to the exclusion of nearly the whole of the free naphthaline present: the latter separating in a colourless state from the red hydrated acid beneath it. As the temperature of the acid diminished, crystallization in tufts commenced here and there, and ultimately the whole became a brownish-yellow solid. A sufficient addition of water dissolved nearly the whole of this hydrated acid, a few flakes only of napthaline separating.

A portion of the impure acid in solution was evaporated at a moderate temperature; when concentrated, it gradually assumed a light-brown tint. In this state it became solid on cooling, of the hardness of cheese, and was very deliquescent. By further heat it melted, then fumed, charred, &c. and gave evidence of the abundant presence of carbonaceous matter.

Some of the impure acid in solution was neutralized by potash, during which no naphthaline or other substance separated. The solution being concentrated until ready to yield a film on its surface, was set aside whilst hot to crystallize: after some hours the solution was filled with minute silky crystals, in tufts, which gave the whole, when stirred, not the appearance of mixed solid salt and liquid, but that of a very strong solution of soap. The agitation also caused the sudden solidification of so much more salt, that the whole became solid, and felt like a piece of soft soap. The salt, when dried, had no resemblance to sulphate of potash. When heated in the air, it burnt with a dense flame, leaving common sulphate of potash, mixed with some sulphuret of potassium, resulting from the action of the carbon, &c. upon the salt.

Some of the dry salt was digested in alcohol to separate common sulphate of potash. The solution being filtered and evaporated, gave a white salt soluble in water and alcohol, crystalline, neutral, burning in the air with much flame, and leaving sulphate of potash. It was not precipitated by nitrate of lead, muriate of baryta, or nitrate of silver.

It was now evident that an acid had been formed peculiar in its nature and composition, and producing with bases peculiar salts. In consequence of the solubility of its barytic salt, the following process for the preparation of the pure acid was adopted.

A specimen of native carbonate of baryta was selected, and its purity ascertained. It was then pulverized, and rubbed in successive portions with a quantity of the impure acid in solution, until the latter was perfectly neutralized, during which the slight colour of the acid was entirely removed. The solution was found to contain the peculiar barytic salt. Water added to the solid matter dissolved out more of the salt; and ultimately only carbonate and sulphate of baryta, mixed with a little of another barytic salt, remained. The latter salt, being much less soluble in water than the former, was not removed so readily by lixiviation, and was generally found to be almost entirely taken up by the last portions of water applied with heat.

The barytic salt in solution was now very carefully decomposed, by successive additions of sulphuric acid, until all the baryta was separated, no excess of sulphuric acid being permitted. Being filtered, a pure aqueous solution of the peculiar acid was obtained. It powerfully reddened litmus paper, and had a bitter acid taste. Being evaporated to a certain degree, a portion of it was subjected to the continued action of heat. When very concentrated, it began to assume a brown colour, and, on cooling, became thick, and ultimately solid, and was very deliquescent. By renewed heat it melted, then began to fume, charred, but did not flame; and ultimately gave sulphuric and sulphurous acid vapours, and left charcoal.

Another portion of the unchanged strong acid solution was placed over sulphuric acid in an exhausted receiver. In some hours it had by concentration become a soft white solid, apparently dry; and after a longer period was hard and brittle. In this state it was deliquescent in the air, but in close vessels underwent no change in several months. Its taste was bitter, acid, and accompanied by an after metallic flavour, like that of cupreous salts. When heated in a tube at temperatures below 212°, it melted without any other change, and on being allowed to cool, crystallized from centres, the whole ultimately becoming solid. When more highly heated, water at first passed off, and the acid assumed a slight red tint; but no sulphurous acid was as yet produced, nor any charring occasioned; and a portion being dissolved and tested by muriate of baryta, gave but a very minute trace of free sulphuric acid. In this state it was probably anhydrous. Further heat caused a little naphthaline to rise, the red colour became deep-brown, and then a sudden action commenced at the bottom of the tube, which spread over the whole, and the acid became black and opaque. Continuing the heat, naphthaline, sulphurous acid, and charcoal, were evolved; but even after some time the residuum, examined by water and carbonate of baryta, was found to contain a portion of the peculiar acid undecomposed, unless the temperature had been raised to redness.

These facts establish the peculiarity of this acid, and distinguish it from all others. In its solid state it is generally a hydrate containing much combustible matter. It is readily soluble in water and alcohol, and its solution forms neutral salts with bases, all of which are soluble in water, most of them in alcohol, and all combustible, leaving sulphates or sulphurets according to circumstances. It dissolves in naphthaline, oil of turpentine, and olive oil, in greater or smaller quantities, accordingly as it contains less or more water. As a hydrate, when it is almost insoluble in naphthaline, it resembles the *heavier substance* obtained as before described, by the action of sulphuric acid on naphthaline, and which is the solid hydrated acid, containing a little naphthaline, and some free sulphuric acid; whilst the *lighter substance* is a solution of the dry acid in naphthaline;

the water present in the oil of vitriol originally used, being sufficient to cause a separation of a part, but not of the whole.

2. *Salts formed by the peculiar Acid with Bases.*

These compounds may be formed, either by acting on the bases or their carbonates by the pure acid, obtained as already described; or the impure acid in solution may be used, the salts resulting being afterwards freed from sulphates, by solution in alcohol. It is, however, proper to mention, that another acid, composed of the same elements, is at the same time formed with the acid in question, in small, but variable proportions. The impure acid used, therefore, should be examined as to the presence of this body, in the way to be directed when speaking of the barytic salts; and such specimens as contain very little or none of it should be selected.

Potash forms with the acid a neutral salt, soluble in water and alcohol, forming colourless solutions. These yield either transparent or white pearly crystals, which are soft, slightly fragile, feel slippery between the fingers, do not alter by exposure to air, and are bitter and saline to the taste. They are not very soluble in water; but they undergo no change by repeated solutions and crystallizations, or by long-continued ebullition. The solutions frequently yield the salt in acicular tufts, and they often vegetate, as it were, by spontaneous evaporation, the salt creeping over the sides of the vessel, and running to a great distance in very beautiful forms. The solid salt heated in a tube gave off a little water, then some naphthaline; after that a little carbonic and sulphurous acid gases arose, and a black ash remained, containing carbon, sulphate of potash, and sulphuret of potassium. When the salt was heated on platinum foil, in the air, it burnt with a dense flame, leaving a slightly alkaline sulphate of potash.

Soda yields a salt, in most properties resembling that of potash; crystalline, white, pearly, and unaltered in the air. I thought that, in it, the metallic taste which frequently occurred with this acid and its compounds was very decided. The action of heat was the same as before.

Ammonia formed a neutral salt imperfectly crystalline, not deliquescent, but drying in the atmosphere. Its taste was saline and cooling. It was readily soluble in water and alcohol. When heated on platinum foil it fused, blackened, burnt with flame, and left a carbonaceous acid-sulphate of ammonia, which by further heat was entirely dissipated. Its general habits were those of ammoniacal salts. When its solutions, though previously rendered alkaline, were evaporated to dryness at common temperatures, and exposed to air, the salt became strongly acid to litmus paper. This, however, is a property common to all soluble ammoniacal salts, I believe, without exception.

Baryta. It is easy by rubbing carbonate of baryta with solution of the impure acid, to obtain a perfectly neutral solution, in which the salt of baryta, containing the acid already described, is very nearly pure. There is in all cases an undissolved portion, which, being washed repeatedly in small quantities of hot water, yields to the first portions a salt, the same as that in the solution. As the washings proceed, it is found, that the salt obtained does not burn with so much flame on platinum foil, as that at first separated; and the fifth or sixth washing will perhaps separate only a little of a salt, which, when heated in the air, in small quantities, burns without flame in the manner of tinder. Hence it is evident that there are two compounds of baryta, which as they are both soluble in water, both neutral, and both combustible, leaving sulphate of baryta, differ probably only in the quantity of combustible matter present, or its mode of combination in the acid.

It is this circumstance, of the formation of a second salt in small but variable quantities with the first, which must be guarded against, as before-mentioned, in the preparation of salts from the impure acid. It varies in quantity according to the proportions of materials, and the heat employed; and I have thought, that when the naphthaline has been in large quantity, and the temperature low, the smallest quantity is produced. When the impure acid is used for the preparation of the salts now under description, a small portion of it should be examined by carbonate of baryta, as above, and rejected, if it furnish an important quantity of the flameless salt.

These bodies may be distinguished from each other provisionally, as the *flaming* and the *glowing* salts of baryta, from their appearances when heated in the air. The latter is more distinctly crystalline than the former, and much less soluble, which enabled me, by careful and repeated crystallizations, to obtain both in their pure states.

The *flaming* salt, (that corresponding to the acid now under description,) when obtained by the slow evaporation of the saturated solution, formed tufts, which were imperfectly crystalline. When drops were allowed to evaporate on a glass plate, the crystalline character was also perceived; but when the salt was deposited rapidly from its hot saturated solution, it appeared in the form of a soft granular mass. When dry, it was white and soft, not changing in the atmosphere. It was readily soluble in water and alcohol, but was not affected by ether. Its taste was decidedly bitter. When heated in the air on platinum foil it burnt with a bright smoky flame, like naphthaline, sending flocculi of carbon into the atmosphere, and leaving a mixture of charcoal, sulphuret of barium, and sulphate of baryta.

After being heated to 212° for some time, the salt appeared to be perfectly dry, and in that state was but very slightly hygrometric. When heated in a tube, naphthaline was evolved; but the substance could be retained for hours at a temperature of 500° F. before a sensible portion of naphthaline had separated: a proof of the strength of the affinity by which the hydro-carbon was held in combination. When a higher temperature was applied, the naphthaline, after being driven off, was followed by a little sulphurous acid, a small portion of tarry matter, and a carbonaceous sulphate and sulphuret were left.

This salt was not affected by moderately strong nitric or nitro-muriatic acid, even when boiled with it; and no precipitation of sulphate took place. When the acids were very strong, peculiar and complicated results were obtained. When put into an atmosphere of chlorine, at common temperatures, it was not at all affected by it. Heat being applied, an action between the naphthaline evolved, and chlorine, such as might be expected, took place.

When a strong solution of the pure acid was poured into a strong solution of muriate of baryta, a precipitate was formed, in consequence of the production of this salt. It was re-dissolved by the addition of water. The fact indicates that the affinity of this acid for baryta is stronger than that of muriatic acid.

The *second*, or *glowing* salt of baryta, was obtained in small crystalline groups. The crystals were prismatic, colourless, and transparent: they were almost tasteless, and by no means so soluble either in hot or cold water as the former salts. They were soluble in alcohol, and the solutions were perfectly neutral. When heated on platinum foil they gave but very little flame, burning more like tinder, and leaving a carbonaceous mixture of sulphuret and sulphate. When heated in a tube they gave off a small quantity of naphthaline, some empyreumatic fumes, with a little sulphurous acid, and left the usual product.

This salt seemed formed in largest quantity when one volume of naphthaline and two volumes of sulphuric acid were shaken together, at a temperature as high as it could be without charring the substances. The tint, at first red, became olive-green; some sulphurous acid was evolved, and the whole would ultimately have become black and charred, had it not been cooled before it had proceeded thus far, and immediately dissolved in water. A solution was obtained, which, though dark itself, yielded, when rubbed with carbonate of baryta, colourless liquids; and these when evaporated furnished a barytic salt, burning without much flame, but which was not so crystalline as former specimens. No attempt to form the glowing salt from the flaming salt, by solution of caustic baryta, succeeded.

Strontia. The compound of this earth with the acid already described very much resembled the flaming salt of baryta. When dry it was white, but not distinctly crystalline: it was soluble in water and alcohol; not alterable in the air, but when heated burnt with a bright flame, without any red tinge, and left a result of the usual kind.

Lime gave a white salt of a bitter taste, slightly soluble in water, soluble in alcohol, the solutions yielding imperfect crystalline-forms on evaporation: it burnt with flame; and both in the air and in tubes, when heated, gave results similar to those of the former salts.

Magnesia formed a white salt with a moderately bitter taste; crystallizing in favourable circumstances, burning with flame, and giving such results by the action of heat as might be expected.

Iron. The metal was acted upon by the acid, hydrogen being evolved. The moist protoxide being dissolved in the acid gave a neutral salt capable of crystallization. This by exposure to air slowly acquired oxygen, and a portion of per-salt was found.

Zinc was readily acted upon by the acid, hydrogen evolved, and a salt formed. The same salt resulted from the action of the acid upon the moist oxide. It was moderately soluble in hot water, the solution on cooling affording an abundant crop of acicular crystals. The salt was white, and unchangeable in the air; its taste bitter. It burnt with flame, and gave the usual results by heat.

Lead. The salt of this metal was white, solid, crystalline, and soluble in water and alcohol. It had a bitter metallic taste, with very little sweetness. The results by heat were such as might be expected.

Manganese. The protoxide of this metal formed a neutral crystalline salt with the acid. It had a slightly austere taste, was soluble in water and alcohol, and was decomposed by heat, with the general appearances already described.

Copper. Hydrated per-oxide of copper formed an acid-salt with the acid, and the solution, evaporated in the air, left radiated crystalline films. The dry salt, when heated, fused, burnt with flame, and exhibited the usual appearances.

Nickel. The salt of this metal was made from the moist carbonate. It was soluble, crystalline, of a green colour, and decomposed by heat in the usual manner. In one instance, an insoluble sub-salt was formed.

Silver. Moist carbonate of silver dissolved readily in the acid, and a solution, almost neutral, was quickly obtained. It was of a brown colour, and a powerful metallic taste. By evaporation it gave a splendent, white, crystalline salt; not changing in the air except when heated; but then, burning with flame, and ultimately leaving pure silver. When the solution of the salt was boiled for some time, a black insoluble matter was thrown down, and a solution obtained, which, by evaporation, gave abundance of a yellow crystalline salt. The changes which took place, during the action of heat in the moist way, were not minutely examined.

Mercury. Moist proto-carbonate of mercury dissolved in the acid forming a salt not quite neutral, crystallizing feebly in the air, white, of a metallic taste, not deliquescent, and decomposed with various phænomena by heat. By re-solution in water or alcohol, and heat, a sub-salt of a yellow colour was formed.

The moist hydrated per-oxide of mercury also dissolved in the acid, forming an acid solution, which by evaporation gave a yellowish deliquescent salt, decomposed by heat, burning in the air, and entirely volatile.

3. *Analysis of the Acid and Salts.*

When solution of the pure acid was subjected to the voltaic battery, oxygen and hydrogen gases were evolved in their pure state: no solid matter separated, but the solution became of a deep-yellow colour at the positive pole, occasioned by the evolution of free sulphuric acid, which re-acted upon the hydro-carbon. A solution of the barytic salts gave similar results.

The analytical experiments upon the composition of this acid and its salts were made principally with the compound of baryta. This was found to be very constant in composition, could be obtained anhydrous at moderate temperatures, and yet sustained a high temperature before it suffered any change.

A portion of the pure salt was prepared, and dried for some hours on the sand-bath, at a temperature about 212°. Known weights were then heated in a platinum crucible to dissipate and burn off the combustible matter; and the residuum being moistened with sulphuric acid to decompose any sulphuret of barium formed, was heated to convert it into a pure sulphate of baryta. The results obtained were very constant, and amounted to 41·714 of sulphate of baryta per cent. of salt used, equivalent to 27·57 baryta per cent.

Other portions of the salt were decomposed, by being heated in a flask with strong nitro-muriatic acid, so as to liberate the sulphuric acid from the carbon and hydrogen present, and yet retain it in the state of acid. Muriate of baryta was then added, the whole evaporated to dryness, heated red-hot, washed with dilute muriatic acid to remove the baryta uncombined with sulphuric acid, and the sulphate collected, dried, and weighed. The results were inconstant; but the sulphate of baryta obtained always much surpassed that furnished by the former method. Judging from this circumstance that the sulphuric acid in the salt was more than an equivalent for the baryta present, many processes were devised for the determination of its quantity, but were rejected in consequence of difficulties and imperfections, arising, principally, from the presence and action of so much carbonaceous matter. The following was ultimately adopted.

A quantity of per-oxide of copper was prepared by heating copper-plates in air and scaling them. A sufficient quantity of pure muriatic and nitric acid was provided, and also a specimen of pure native carbonate of baryta. Seven grains of the salt to be examined were then mixed with seven grains of the pulverized carbonate of baryta, and afterwards with 312 grains of the oxide of copper. The mixture being put into a glass tube, was successively heated throughout its mass, the gas liberated being passed through a mixture of baryta-water and solution of muriate of baryta. It was found that no sulphurous or sulphuric acid came off, or indeed sulphur in any state. The contents of the tube were then dissolved in an excess of the nitric and muriatic acids, above that required to take up all that was soluble; and a little solution of muriate of baryta was added for the sake of greater certainty. A portion of sulphate of baryta remained undissolved, equivalent to the sulphuric acid of the salt experimented upon, with that contained accidentally in the oxide of copper, acids, &c. This sulphate was collected, washed, dried, and weighed. Similar quantities of the carbonate of baryta and oxide of copper were then dissolved in as much of the nitric and muriatic acids as was used in the former experiment; and the washings and other operations being repeated exactly in the same way, the quantity of sulphate of baryta occasioned by the presence of sulphuric acid in the oxide, acids, &c. was determined. This, deducted from the weight afforded in the first experiments, gave the quantity produced from the sulphuric acid actually existing in the salt. Experiments so conducted gave very uniform results. The mean of many indicated 8·9 grains of sulphate of baryta for 10 grains of salt used, or 89 grains per cent. equivalent to 30·17 of sulphuric acid for every 100 of salt decomposed.

In the analytical experiments, relative to the quantity of carbon and hydrogen contained in the salt, a given weight of the substance being mixed with per-oxide of copper, was heated in a green glass tube. The apparatus used consisted of Mr. Cooper's lamp-furnace, with Dr. Prout's mercurial trough; and all the precautions that could be taken, and which are now well known, were adopted for the purpose of obtaining accurate results. When operated upon in this way, the only substances evolved from the salt, were carbonic acid and water. As an instance of the results, 3·5 grains of the salt afforded 11·74 cubic inches of carbonic acid gas, and 0·9 of a grain of water. The mean of several experiments gave 32·93 cubic inches of carbonic acid gas, and 2·589 grains of water, for every 10 grains of salt decomposed.

On these data, 100 grains of the salt would yield 329·3 cubic inches of carbonic acid, or 153·46 grains, equivalent to 41·9 grains of carbon, and 25·89 grains of water, equivalent to 2·877 grains of hydrogen. Hence 100 grains of the salt yielded

Baryta	27·57	78·0
Sulphuric acid	30·17	85·35
Carbon	41·90	118·54
Hydrogen	2·877	8·13
	102·517	

In the second numerical column the experimental results are repeated, but increased, that baryta might be taken in the quantity representing one proportional, hydrogen being unity: and it will be seen that they do not differ far from the following theoretical statement:

Baryta	1	proportional	78
Sulphuric acid	2	ditto	80
Carbon	20	ditto	120
Hydrogen	8	ditto	8

The quantity of sulphuric acid differs most importantly from the theoretical statement, and it probably is *that* element of the salt, in the determination of which most errors are involved. The quantity of oxide of copper and of acids required to be used in that part of the analysis, may have introduced errors, affecting the small quantity of salt employed, which when multiplied, as in the deduction of the numbers above relative to 100 parts, may have created an error of that amount.

As there is no reason to suppose that during the combination of the acid with the baryta any change in its proportions takes place, the results above, *minus* the baryta, will represent its composition: from which it would appear, that one proportional of the acid consists of two proportionals of sulphuric acid, twenty of carbon, and eight of hydrogen; these constituents forming an acid equivalent in saturating power to one proportional of other acids. Hence it would seem, that half the sulphuric acid present, at least when in combination, is neutralized by the hydro-carbon; or, to speak in more general terms, that the hydro-carbon has diminished the saturating power of the sulphuric acid to one-half. This very curious and interesting fact in chemical affinity was however made known to me by Mr. Hennell, of Apothecaries' Hall, as occurring in some other compounds of sulphuric acid and hydro-carbon, before I had completed the analysis of the present acid and salts; and a similar circumstance is known with regard to muriatic acid, in the curious compound discovered by M. Kind, which it forms with oil of turpentine. Mr. Hennell is, I believe, on the point of offering an account of his experiments to the Royal Society, and as regards date they precede mine.

It may be observed, that the existence of sulphuric acid in the new compounds is assumed, rather than proved; and that the non-appearance of sulphurous acid, when sulphuric acid and naphthaline act on each other, is not conclusive as to the non-reaction of the bodies. It is possible that part of the hydrogen of the naphthaline may take oxygen from one of the proportions of the sulphuric acid, leaving the hypo-sulphuric acid of Welter and Gay-Lussac, which, with the hydro-carbon, may constitute the new acid. I have not time at present to pursue these refinements of the subject, or to repeat the analyses which have been made of naphthaline, and which would throw light upon the question. Such a view would account for a part of the overplus in weight, but not for the excess of the sulphuric acid obtained, above two proportionals.

The glowing salt of baryta was now analysed by a process similar to that adopted for the flaming salt. The specimen operated upon was pure, and in a distinctly crystalline state. It had been heated to about 440° F. for three hours in a metallic bath. Ten grains of this salt exposed to air for 40 hours increased only 0·08 of a grain in weight. These when converted into sulphate of baryta by heat and sulphuric acid, gave 4·24 grains. Seven grains by carbonate of baryta, oxide of copper, heat, &c. gave 6·02 grains of sulphate of baryta: hence 10 grs. of the salt would have afforded 8·6 grains of the sulphate, equivalent to 2·915 grains of sulphuric acid. Five grains, when heated with oxide of copper, gave 16·68 cubic inches of carbonic acid gas, equal to 7·772 grains, and equivalent to 2·12 grains of carbon. The water formed amounted to 1·2 grain equivalent to 0·133 of a grain of hydrogen.

From these data, 100 grains of the salt would appear to furnish

Baryta	28·03	..	78·0 or 1 proportional.
Sulphuric acid	29·13	..	81·41 nearly two proportionals.
Carbon	42·40	..	118·0 approaching to 20 ditto.
Hydrogen	2·66	..	7·4 or 7·4 proportionals:
	102·22		

results not far different from those obtained with the former salt.

I have not yet obtained sufficient quantities of this salt in a decidedly crystalline state, to enable me satisfactorily to account for the difference between it and the flaming salt.

Attempts were made to form similar compounds with other acids than the sulphuric. Glacial phosphoric acid was heated and shaken in naphthaline, but without any particular results. A little water was then used with another portion of the materials, to bring the phosphoric acid into solution, but no decided combination could be obtained. Muriatic acid gas was brought into contact with naphthaline in various states, and at various temperatures, but no union could be effected either of the substances or their elements.

Very strong solution of potash was also heated with naphthaline, and then neutralized by sulphuric acid; nothing more, however, than common sulphate of potash resulted.

As the appropriation of a name to this acid will much facilitate future reference and description, I may, perhaps, be allowed to suggest that of *sulpho-naphthalic acid*, which sufficiently indicates its source and nature, without the inconvenience of involving theoretical views.

Article V.

Analyses of Books.

A Description of active and extinct Volcanos, with Remarks on their Origin, their Chemical Phænomena, and the Character of their Products, as determined by the Condition of the Earth during the Period of their Formation. Being the Substance of some Lectures delivered before the University of Oxford, with much additional Matter. By Charles Daubeny, MD. FRS. &c. &c. Professor of Chemistry, and Fellow of Magdalen College, Oxford. London, 1826: 8vo.

It is not many months since we had the satisfaction of laying before our readers an analysis of a work on volcanos by Mr. Poulett Scrope;* it now becomes our duty to notice another publication which has just appeared on the same subject, the plan of which is of a more extended description, as it embraces not only a theory of volcanic operations, but likewise a detailed statement of the phænomena, both of a geological and chemical nature, which arise from them.

The author informs us, that he was first led to the inquiries which have furnished him with materials for the present work, by a wish to obtain some further evidence with respect to the origin of basalt, the nature of which still continued the subject of warm discussion, during the time at which he was pursuing his studies at Edinburgh. Conscious that all the light that could be thrown upon this question by reference to the characters and relations of trap rocks themselves, had been already

* See *Annals* for January last.

obtained through the exertions of preceding observers, he determined to take up the subject in a different point of view, by examining the relations of these rocks to the products of active or acknowledged volcanos, and with this design to begin by making himself fully acquainted with the latter class of formations.

"For this purpose, however, a mere examination of hand specimens was not sufficient, the spots themselves were to be visited, and the circumstances of geological position, as well as the nature of the rocks associated, carefully compared with what was seen in the trap districts that had excited so much attention and dispute."

He therefore examined at different intervals the volcanic rocks of France and Germany; those of Hungary and Styria; the greater part of such as exist in Italy and the neighbouring islands; and the whole series of those which extend throughout Sicily; thus including in his examination most of the appearances of the above kind that are manifest in this quarter of the globe, except those in Iceland, already made known to us through Sir G. Mackenzie and others; in Greece and Turkey, countries at present but little accessible; and in the Spanish Peninsula, where a few indications of igneous action have been noticed as occurring.

His observations in these countries constitute, in great measure, the contents of the two first lectures, for the author has thought proper to retain this title, as indicating the sections or chapters into which the work is divided, notwithstanding the additional matter introduced since they were delivered, which has swelled all of them to a length far exceeding the legitimate limits of an oral discourse.

We observe, however, with pleasure, an abstract of Beudant's observations on the trachyte of Hungary, which, notwithstanding their interest, have hitherto met with but little notice in this country; and likewise an account of the almost unexplored volcanic district of Transylvania, communicated by Dr. Boué, a geologist well known from his papers on various parts of France and Germany, and his Geognostical Essay on Scotland.

The author's remarks on Auvergne have already appeared under the form of letters addressed to Professor Jameson, and a short account of the volcanos of Sicily, will be seen in the sketch of the geology of that island, communicated originally to the Bristol Philosophical and Literary Institution, and since published in the Edinburgh Philosophical Journal.

In other respects, the matter of the first two lectures may be considered new; but not so that of the third which is engrossed by a large assemblage of facts, compiled from various sources both ancient and modern, with regard to the volcanos that occur in parts of the globe, which have not been visited by the author.

Here we meet with a summary of the geological details comprised in the works of Sir G. Mackenzie and others on Iceland; of the accounts handed down to us with respect to those volcanic eruptions, that have taken place in Greece and the Archipelago; and of the remarks on those of the Canaries and other islands off the African Coast, which have been communicated by Humboldt, Von Buch, and other foreign geologists.

Traces of the same agency are shown to exist in Asia Minor, Palestine, Syria, and other countries of the East, and a line of volcanic operations is traced from Kamschatka to Japan, and thence, in an almost uninterrupted succession, to Java, Sumatra, and the Andaman Islands.

A few of the volcanos distributed over the Great Pacific, and the Gulph of Mexico, are afterwards noticed; and the whole concludes, by bringing together the results of Humboldt's investigations on the continent of America.

From this statement of the contents of the first three lectures, it will be evident, that we cannot pretend to present to our readers a complete analysis of the work; all we can undertake to do, will be to give an idea of the manner in which the subject is handled, by selecting certain portions for abridgment; and we shall begin by noticing the author's description of the volcanos that occur in the neighbourhood of the Rhine.

These are divided, according to his usual system, into *post-diluvial* and *ante-diluvial;* by which terms, however, nothing more is intended, than an expression of the fact, that the eruptions took place, after or before the period at which the valleys were excavated.

The volcanos of the Eyfel district, which intervenes between the Rhine and the frontier of the Netherlands, furnishes us with an instance of volcanos formed subsequently to that epoch.

"Scattered over the greater part of the district alluded to, are a number of small conical eminences, often inclosing craters, the declivities of which are usually sunk much below the present level, and have thereby, in many cases, received the drainage of the surrounding country, thus forming a series of lakes, known by the name of Maars, which are remarkably distinguished from those elsewhere seen, by their circular form, and by the absence of any apparent outlet for their waters. The sides of these craters seem to be made up, of alternating strata of volcanic sand and scoriform lava, dipping away in all directions from the centre, at a considerable angle, and the same kind of material has in many instances so accumulated round the cones, as to obliterate in great measure the hollow between them, and to raise the level of the country nearly up to the brim of the craters."

The author proceeds to a detailed description of certain of these craters, and of the accompanying streams of lava, in which, however, we have not room to follow him; and concludes, as he has done in the case of the modern volcanos of Auvergne, that although of post-diluvial origin, they were in action before the existence of historical records. There is, indeed, a passage in Tacitus, which has been supposed to refer to something of a volcanic nature that occurred in this district; but our author ridicules such an idea, and concludes that nothing more was meant, than an accidental conflagration, caused, perhaps, by setting fire to the woods or heaths, in a dry season.

"It is certain at least that the lava of Niedermennig existed in the time of Augustus, for the pillars of the ancient bridge of Treves are formed of this material.

"We are, therefore, under the necessity of attributing to the Eyfel volcanos a date historically very ancient, though, geologically speaking, modern, since geological research may be said almost to terminate where history begins: if we adopt the opinions of Prof. Buckland respecting the excavation of the valleys, we must suppose these rocks to have been formed, like some of those in Auvergne, subsequently to the Deluge recorded by Moses; or if, limiting ourselves to those views in which all geologists concur, we choose to speak more indefinitely, the date of their eruptions must be pronounced to be posterior to the event which reduced the surface of the globe to its present condition." P. 65.

The remaining volcanic rocks in this part of Europe, appear to be of an older date, and are shown by the author to belong to that period in the history of the globe, during which the rocks called tertiary were being deposited. The trachytes and basalts of the Seven Mountains near Bonn, of the Westerwald near Coblentz, and of several other chains of mountains in the same neighbourhood, belong to this class. The mode of their formation is illustrated, by considering certain conical masses of basalt, which occur detached in Hessia, the structure and relations of which are sufficiently exposed, to allow of their being studied with exactness.

In the case of the Pflasterkaute near Eisenach, "the excavations are carried to such a depth, that we are enabled distinctly to see the basalt more than 50 feet below the surface of the sandstone. The line of junction is also well-displayed, and we observe the sandstone changed from an horizontal to a vertical position, split in all directions, and rendered harder and whiter, where the basalt touches it." P. 72.

In another case, the portions of sandstone form clusters of little prisms, possessing even greater regularity of form than those of the basalt which encircles them. "It is curious," remarks our author, "to trace the resemblance between the prisms here alluded to, and those produced artificially in several parts of Derbyshire and Yorkshire, where the soft friable sandstone of the country, is rendered serviceable for road-making by exposure to heat, which hardens and causes it to split into small columnar concretions." P. 73.

The hill called Blaue Kuppe, near Eschwege, illustrates these phænomena in a still more striking manner, and it is certainly surprising, that with such phænomena almost in their neighbourhood, the geologists of the Freiburg school should so long have remained unconvinced of the igneous origin of basalt.

We have no room for the sketch of the trap formations in other parts of Germany, with which the first lecture concludes; neither can we notice the abstract, given at the commencement of the second, of Beudant's researches in Hungary. The author agrees with that geologist in opposition to Humboldt, in attributing a different origin to the trachyte of Hungary, and to the older porphyry which supports it; but he combats the theory advanced by the French naturalist, with respect to the formation of the alum diffused through the substance of many beds in this formation. This M. Beudant regards as the result of sulphureous exhalations; Prof. Daubeny as arising from the decomposition of metallic sulphurets.

Our author takes pains to show, that every one of the volcanic districts already noticed, are, at the least, as modern as the tertiary class of deposits; such is likewise the case with the little trachytic formation which he visited in Styria, and with the greater part of the volcanic products that occur in Italy. Those of the Vicentin, and of the neighbourhood of Rome are, treated of at some length; and in the Neapolitan territory a volcano is introduced to our notice, scarcely known, we believe, to English geologists, that, namely, of Mount Vultur.

The neighbourhood of Naples is of course more particularly dwelt upon, and its volcanos are treated, first, historically; and, secondly, with reference to their geological and chemical phænomena. Under the first of these heads, must be placed the question, as to whether Vesuvius was burning at any period of Roman history antecedent to the Christian era; and with the view of deciding this point, our author has brought together many passages from classical writers, in which mention is made of the mountain. Of these, the one most illustrative of its actual structure, at that period, is the account given of the occupation of these heights by the gladiators under Spartacus, and of the manner of their escape from this position. We may remark, that Dr. Daubeny differs from former geologists, with respect to the formation of the dykes of Monte Somma, and seems loth to admit, that we have any decided case, in which dykes have been formed within the crater of any volcano, or since the commencement of the present order of things.

When speaking of the Solfatara, he takes occasion to discuss the origin of those saline products, found within the craters of most volcanos: the following is the manner in which he accounts for the production of sal ammoniac which is so frequent an ingredient.

"The rock of this mountain is a sort of trachyte, which, besides a little potass, consists essentially of silex and alumine, with an occasional admixture of iron, lime, and magnesia.

"The muriatic acid (exhaled from the volcano) acting upon these ingredients, forms severally with them, a quantity of saline matter, proportionate to that in which it is emitted, but the most abundant salt of this class, is the muriate of ammonia, the formation of which may, perhaps, be thus accounted for.

"When muriatic acid is suffered to act upon an alkaline hydrosulphuret, it combines with the base, and separates the sulphuretted hydrogen; very little, however, of the latter exhales in a gaseous condition, but it is for the most part precipitated in the shape of a heavy oil, which is found by analysis to consist of one atom hydrogen, and two atoms sulphur. Now as sulphuretted hydrogen consists of one atom of each ingredient, it follows, that the formation of this body must be accompanied by the disengagement of an equal volume of hydrogen gas. But what becomes of this latter body, since it is not to be detected afterwards in a separate state? It is probable that it has united with the oxygen of the atmosphere, or with its nitrogen, perhaps indeed with both; in the latter case, the presence of the ammonia is explained; in the former it is rendered more comprehensible, since we have many examples in which nitrogen, in its nascent state, is known to unite with hydrogen, held in combination by weak affinities." P. 168.

The crater of Volcano, one of the Lipari Islands, is in a state somewhat similar to that of the Solfatara. Our author describes, however, the process there going on as so far different, inasmuch as the vapour given out in the crater of Volcano consists of sulphurous acid; whereas in that of the Solfatara, it was composed of sulphuretted hydrogen. The operations, too, of the former appear to be going on with much greater vigour than those of the latter, and exhibit, says our author, perhaps the nearest approximation to a state of activity, during which a descent into the crater would have been practicable.

"Nor (he continues) can I imagine a spectacle of more solemn grandeur, than that presented in its interior, or conceive a spot better calculated to excite, in a superstitious age, that religious awe which caused the island to be considered sacred to Vulcan, and the various caverns below as the peculiar residence of the god."

Quam subter, specus, et Cyclopum exesa caminis
Antra Etnea tonant, validique incudibus ictus
Auditi referunt gemitum, striduntque cavernis
Stricturæ Chalybum, et fornacibus ignis anhelat
Volcani domus, et Vulcania nomine tellus.

"To me, I confess, the united effect of the silence and solitude of the spot, the depth of the internal cavity, its precipitous and overhanging sides, and the dense sulphureous smoke, which, issuing from all the crevices, throws a gloom over every object, proved more impressive than the reiterated explosions of Stromboli, contemplated at a distance, and in open day." P. 193.

This lecture concludes by an account of the volcanic formations of Sicily, which are divided, like most of the rest, into those of ante-diluvial and post-diluvial origin.

The former are chiefly found in the Val de Note, but our author observes, that there are certain rocks in the vicinity of Mount Etna, that were probably formed antecedently to the mountain at whose foot they lie. The Cyclopean Islands, for instance, (with which every classical reader is acquainted, as the rocks which Polyphemus is described by Homer, as hurling against the bark, in which Ulysses and his crew were taking their flight,) "though now detached, must at one period have formed a connected stratum, for they are covered with a bed of marl, which seems evidently to have been continuous from the one to the other of these islands. This circumstance, and their general compactness, prove that these formations took place under the surface of water. ....Nothing of this kind is indicated by the structure of Etna. This mighty and imposing mountain, which rises in solitary grandeur to the height of above 10,000 feet, and embraces a circumference of 180 miles, is entirely composed of lavas, which, whatever subordinate differences may exist between them, all possess the appearance of having been ejected above the surface of water, and not under pressure.

"In the structure of this mountain, every thing wears alike the character of vastness. The products of the eruptions of Vesuvius may be said almost to sink into insignificance, when compared with these coulees, some of which are four or five miles in breadth, 15 in length, and from 50 to 100 feet in thickness, and the changes made upon the coast by them, is so considerable, that the natural boundaries between the sea and land may be said almost to depend upon the movements of the volcano.

"The height, too, of Etna is so great that the lava frequently finds less resistance in piercing the flanks of the mountain, than in rising to its summit, and has in this manner formed a number of minor cones, many of which possess their respective craters, and have given rise to considerable streams of lava.

"Hence, an ancient poet has very happily termed this volcano *the parent of Sicilian mountains,* an expression strictly applicable to the relation which it bears to the hills in its immediate neighbourhood, all of which have been formed by successive ejections of matter from its interior.

"The grandest and most original feature indeed in the physiognomy of Etna, is the zone of subordinate volcanic hills, with which it is encompassed, and which look like a court of subaltern princes waiting upon their sovereign.

"Of these, some are covered with vegetation, others are bare and arid, their relative antiquity being probably denoted by the progress vegetation has made upon their surface; and the extraordinary difference that exists in this respect, seems to indicate that the mountain to which they owe their origin must have been in a state of activity, if not at a period antecedent to the commencement of the present order of things, at least at a distance of time exceedingly remote." P. 204.

The Professor then exposes the mistake into which Brydone was led by the Abbe Recupero, with regard to the existence of vegetable mould, intervening between the beds of lava at Jace Reale, thus overturning the argument founded on the above fact, which that lively writer has brought forward, to prove the great antiquity of the eruptions of this volcano. Here, however, there is the less necessity for following him, as the details may be seen in Dr. Daubeny's Sketch of the Geology of Sicily, published in a contemporary journal.

In the third lecture we are glad to find an abstract of Von Buch's valuable Memoirs on the Canary Islands, which, having never been translated into our language, remain in some measure a sealed book to English geologists.

Dr. Daubeny has shown the probable existence of volcanos in the East, from many concurrent circumstances. Thus the accounts of volcanic matter in Asia Minor, and the occurrence of a Grotto del Cane near Smyrna, described by Strabo, and rediscovered by Chandler; the correspondence between the traditions that existed in Persia and Greece with respect to the supposed volcano of Demavend in the former country, and those of Sicily and Campania; the analogy between the Typhœus of the Greek poets and the Zohag of the Zend-Avesta; the frequent allusions to volcanic phænomena in Holy Writ; and, above all, the destruction by fire of the five cities in the plain of Siddim, all tend to establish the operation of subterranean fire in these countries, at a period, which, (geologically speaking,) must be accounted modern.

He even explains the formation of the Dead Sea, by imagining a stream of lava to have flowed across the river Jordan, and to have obstructed its channel, which in all probability extended at some former period to the Red Sea, as the late interesting researches of Burckhardt have indicated.

Dr. Daubeny shows that there is nothing contrary to analogy in such a supposition, for we have instances of lakes formed in a similar manner, in Auvergne, if not near the Rhine; and remarks, that, "if the little rivulet that flows at the foot of the Puy de la Vache in the former country was adequate to produce the lake of Aidat, there seems no disproportion in attributing to a river of the size of the Jordan, to say nothing of other streams nowise inconsiderable which must have been affected by the same cause, the formation of a piece of water, such as the Dead Sea; which, according to the best authorities, is, after all, not more than twenty-four leagues in length, by six or seven in breadth." P. 287.

In his account of the volcanos of the New World, it will be seen that the Professor concurs with Humboldt, with respect to the formation of Jorullo; indeed the views of that naturalist, though expressed, perhaps, in language more metaphorical, like that employed by the Roman poet in speaking of the sudden rise of the Promontory of Træzue in Ærgolis,* are, upon the whole, conformable with those entertained by our author, relative to the origin of the dome-like masses of trachyte in Auvergne, and even the basaltic dykes, (if they may be so called,) of Hessia.

The three first lectures or chapters, being occupied by a detail of facts with regard to volcanos, the author proceeds, in the fourth, to some general conclusions on the phænomena themselves.

He begins, by considering the theories which have been proposed to explain the cause of volcanos, and having decided in favour of the one suggested by Sir H. Davy's discovery, with respect to the metallic bases of the earths and alkalies, endeavours to deduce, in detail, the phænomena attendant on an eruption from this hypothesis.

"Let us suppose," he says, "that the nucleus of the earth at a depth of three or four miles either consists of, or contains as a constituent part, combinations of the alkaline and earthy metalloids, as well as of iron, and the more common metals, with sulphur, and possibly with carbon.

"These sulphurets are gradually undergoing decomposition, wherever they come into contact with air and water, but, defended by the crust of the globe, just as a mass of potassium is by a coat of its own oxide, when preserved in a dry place, the action goes on too slowly to produce any striking effect, unless the latter of these agents be present in sufficient quantity.

"Hence, under our continents, the elastic fluids generated by this process are compressed by the superincumbent mass of rock, until they enter probably, into new combinations, or diffuse themselves through the solid strata.

* Extentam tumefecit humum, ceu spiritus oris
Tendore vesciam solet, aut derepta bicornis
Terga capri.—*Ovid, Metam.*

"But under the sea, where the pressure of an enormous column of water assists in forcing that fluid through the minutest crevices of the rock, the action must go on more rapidly, and the effects consequently be of a more striking nature.

"These effects, however, will take place in the middle of the sea less generally than on the coast, because the pressure of the ocean itself opposes an impediment; and it will in general not be constant, but intermittent, because the heat generated by the process itself, will have a tendency to close the aperture by which the water entered, first, by injecting the fluid lava into the fissure; and, secondly, by causing a general expansion of the rock; nor will the water again find admission, until, owing to the cessation of the process, the rock becomes cool, and consequently again contracts to its original dimensions."

"Now the first effect of the action of water upon the alkaline and earthy metalloids, will be the production of a large volume of hydrogen gas, which, if air be present, will combine with oxygen and return to the state of water; if it be absent, will probably combine with the sulphur, both being at the high temperature favourable to their union. In the former case, nitrogen gas will be given off; in the latter, sulphuretted hydrogen.

"But in case of the presence of oxygen, the sulphur will also become inflamed, and give rise to the production of sulphurous acid, which will predominate among the gaseous exhalations emitted from the mouth of the volcano, provided a sufficient quantity of air be present to combine with the hydrogen, and re-convert it into water. So soon, however, as the oxygen is consumed, the hydrogen, no longer entering into combustion, unites with the heated sulphur, and escapes in the form of sulphuretted hydrogen, which, towards the latter period of the eruption, will predominate over the sulphurous acid, because it continues to be formed, long after the want of oxygen has put a stop to the production of sulphurous acid. Now it is well known, that these two gases mutually decompose each other, and, therefore, cannot exist at the same time, so that the appearance of sulphuretted hydrogen from the mouth of the volcano, may indicate, if not the entire absence of sulphurous acid, at the place at which the process takes place, at least that its formation is stopped by the consumption of oxygen, or is going on with less energy than heretofore.

"The very circumstance of the reproduction of water by the mutual decomposition of these two gases, might be the means of keeping up the action, in a languid manner, for an indefinite period. The slowness with which lava cools, would cause it to give out, for a considerable time, sufficient heat to the adjoining strata, to place the sulphur at the temperature necessary to cause its combination with oxygen; hence a certain portion of sulphurous acid would be continually emitted, which, however, would be soon decomposed by the hepatic gas present. The water resulting from this process, would percolate into the recesses of the rock, act upon any portions of the alkaline and earthy metalloids, that might have escaped the original action, and give birth to a fresh volume of hydrogen gas, ready in its turn to dissolve a new portion of sulphur, and thereby to contribute to a repetition of the same phænomena." P. 392.

We have no room for further extracts, but must proceed to give some account of the second department of the inquiry; namely, as to the degree in which volcanos have contributed towards the production of the older constituents of our globe.

He decides in favour of the general, if not the universal volcanic origin of trap, by considering the analogy in chemical and geological characters that exists between it and lava, and by accounting for the distinctions between the two, from the differences of circumstances under which they were respectively produced. He even shows, that as these circumstances became more similar to those that exist at present, the characters of the erupted masses approach more nearly to those of the actual products of subterranean fire; and he therefore establishes three classes of volcanic formations, the first produced since the commencement of the present order of things; the second, during the deposition of the tertiary rocks; the third, cotemporaneous with the more ancient strata.

Of these, the first class, being formed in the open air, possesses the characters of bodies exposed at present to artificial heat; the third, being of submarine origin, has those characters modified by the influence of great pressure; whilst the second, being formed under water, but under a body of fluid less considerable than existed in the former case, possesses characters intermediate between the other two.

The author details, at some length, the subordinate differences arising out of these fundamental distinctions, and then proceeds to notice the arguments that have been advanced, both for and against the igneous origin of granite and serpentine.

These questions, however, he leaves undecided, or at least considers to require some further elucidation.

The author likewise regards the opinion which prevails, as to the increasing heat of the earth from the circumference to the centre, as open to some objection; and is led from his own observations in mines, to consider their temperature as influenced by local causes in a greater degree than is generally suspected.

He concludes by speculating on the final causes of volcanos, which he regards as the safety-valves, through which elastic fluids, generated by processes going on in the interior of the earth, find a vent, and consequently as the best safeguards against destructive earthquakes.

He also imagines that volcanos may be among the means, that nature employs for increasing the extent of dry land, in proportion to that of the ocean, a notion rendered more probable, by considering, that coral reefs are mostly founded upon shoals caused by volcanic eruptions.

"Hence a sort of consistency will appear, in this case, to exist in the arrangements of nature, which leads to the belief that fire and water are both working together to a common end, and that end the preparation of a larger portion of the earth's surface for the maintenance of the higher orders of animals."

The additional notes relate to a subterraneous noise heard near the Red Sea; to the origin of the fables respecting the Typhon or Typhœus of the Greeks, which seem to be often meant as allegorical representations of volcanic phænomena; to the revolutions of opinion that have taken place with respect to geological theories; and to some other topics.

The whole concludes with a list of books, from which information may be gathered as to the different volcanos, mentioned in the course of the work.

The volume is illustrated with several wood-cuts, giving sections, &c. of geological phænomena, by a copper-plate engraving of Jorullo, as represented by Humboldt, and by two maps, the one of Mexico, the other of a part of Judea, illustrative of the author's views with respect to the formation of the Dead Sea.

Philosophical Transactions of the Royal Society of London, for 1826. *Parts I. and II.*

(*Concluded from* p. 141.)

XII. *On the Nervous Circle which connects the Voluntary Muscles with the Brain;* by C. Bell, Esq. communicated by the President.

The principal results of the investigation detailed in this paper, are contained in the following extracts:

"I hope now to demonstrate—*that where nerves of different functions take their origin apart and run a different course; two nerves must unite in the muscles, in order to perfect the relations betwixt the brain and these muscles.*

"It may be in the recollection of the Society, that my first paper showed the difference of the nerves of the face; by dividing one nerve, sensation was destroyed, whilst motion remained; and by dividing the other, motion was stopped, whilst sensibility remained entire.

"Other parts of the nervous system since that time have engaged my attention; and it is only now that I am able to make full use of the facts announced in my first paper, which were indeed expected to lead to further improvement of our knowledge of the animal œconomy. When I distinguished the two classes of nerves going to the muscles of the face, and divided the motor nerve, and when the muscles were deprived of motion by this experiment, the natural question suggested itself—of what use are the nerves that remain entire?

"For a time I believed that the fifth nerve, which is the sensitive nerve of the head and face, did not terminate in the substance of the muscles, but only passed through them to the skin; and I was the more inclined to this belief on observing, that the muscular parts when exposed in surgical operations did not possess that exquisite sensibility which the profusion of the sensitive nerves would imply, or which the skin really possesses.

"Still dissection did not authorise this conclusion. I traced the sensitive nerves into the substance of the muscles: I found that the fifth pair was distributed more profusely to the muscles than to the skin; and that estimating all the nerves given to the muscles, the greater proportion belonged to the fifth or sensitive nerve, and the smaller proportion to the seventh or motor nerve. On referring to the best authorities, as Meckel,* and my excellent preceptor Monro, the extremities of the fifth were described by them as going into the muscles, so that of this fact there cannot be a doubt.

"Having in a former paper demonstrated that the portio dura of the seventh nerve was the motor of the face, and that it run distinct from the sensitive nerve, the fifth, and observing that they joined at their extremities, or plunged together into the muscles, I was nevertheless unwilling to draw a conclusion from a single instance; and therefore cast about for other examples of the distribution of the muscular nerves. It was easy to find motor nerves in combination with sensitive nerves, for all the spinal nerves are thus composed; but we wanted a muscular nerve clear in its course, to see what alliance it would form in its ultimate distribution in the muscle. I found in the lower maxillary nerve the example I required.

"The fifth pair, from which this lower maxillary nerve comes, as I have elsewhere explained, is a compound nerve; that is to say, it is composed of a nerve of sensation, and a nerve of motion. It arises in two roots, one of these is the muscular nerve, the other the sensible nerve; on this last division the Gasserian ganglion is formed. But we can trace the motor nerve clear of the ganglion; and onward in its course to the muscles of the jaws, and so it enters the temporal masseter, pterygoid, and buccinator muscles.

"If all that is necessary to the action of a muscle be a nerve to excite to contraction, these branches should have been unaccompanied; but on the contrary, I found that before these

"* Meckel de quinto pare nervorum cerebri."

motor nerves entered the several muscles, they were joined by branches of the nerves which came through the Gasserian ganglion, and which were sensitive nerves.

"I found the same result on tracing motor nerves into the orbit, and that the sensitive division of the fifth pair of nerves was transmitted to the muscles of the eye, although these muscles were supplied by the third, fourth, and sixth nerves.

"A circumstance observed on minute dissection remained unexplained,—when motor nerves are proceeding to several muscles they form a plexus; that is, an interlacement and exchange of fibres takes place.

"The muscles have no connexion with each other, they are combined by the nerves; but these nerves, instead of passing betwixt the muscles, interchange their fibres before their distribution to them, and by this means combine the muscles into classes. The question therefore may thus be stated: why are nerves, whose office it is to convey sensation, profusely given to muscles in addition to those motor nerves which are given to excite their motions? and why do both classes of muscular nerves form plexus?

"To solve this question, we must determine whether muscles have any other purpose to serve than merely to contract under the impulse of the motor nerves. For if they have a reflective influence, and if their condition is to be felt or perceived, it will presently appear that the motor nerves are not suitable internuncii betwixt them and the sensorium.

"*I shall first inquire, if it be necessary to the governance of the muscular frame, that there be a consciousness of the state or degree of action of the muscles?* That we have a sense of the condition of the muscles appears from this: that we feel the effects of over exertion and weariness, and are excruciated by spasms, and feel the irksomeness of continued position. We possess a power of weighing in the hand:—what is this but estimating the muscular force? We are sensible of the most minute changes of muscular exertion, by which we know the position of the body and limbs, when there is no other means of knowledge open to us. If a rope-dancer measures his steps by the eye, yet on the other hand a blind man can balance his body. In standing, walking, and running, every effort of the voluntary power, which gives motion to the body, is directed by a sense of the condition of the muscles, and without this sense we could not regulate their actions.

"If it were necessary to enlarge on this subject, it would be easy to prove that the muscular exertions of the hand, the eye, the ear, and the tongue, are felt and estimated when we have perception through these organs of sense; and that without a sense of the actions of the muscular frame, a very principal inlet to knowledge would be cut off.

"If it be granted, that there must be a sense of the condition of the muscle, we have next to show that a motor nerve is not a conductor towards the brain, and that it cannot perform the office of a sensitive nerve.

"Without attempting to determine the cause, whether depending on the structure of the nervous cord, or the nature, or the source of the fluid contained, a pure or simple nerve has the influence propagated along it in one direction only, and not backwards and forwards; it has no reflected operation or power retrograde; it does not both act from and to the sensorium.

"Indeed reason without experience would lead us to conclude, that whatever may be the state, or the nature of the activity of a motor nerve during exertion, it supposes an energy proceeding *from* the brain *towards* the muscles, and precludes the activity of the same nerve in the opposite direction at the same moment. It does not seem possible therefore that a motor nerve can be the means of communicating the condition of the muscles to the brain.

"Expose the two nerves of a muscle; irritate one of them, and the muscle will act; irritate the other, and the muscle remains at rest. Cut across the nerve which had the power of exciting the muscle, and stimulate the one which is undivided —the animal will give indication of pain; but although the nerve be injured so as to cause universal agitation, the muscle with which it is directly connected does not move. Both nerves being cut across, we shall still find that by exciting one nerve the muscle is made to act, even days after the nerve has been divided; but the other nerve has no influence at all.

"Anatomy forbids us to hope that the experiment will be as decisive when we apply the irritants to the extremities of the divided nerves which are connected with the brain; for all the muscular nerves receive more or less minute filaments of sensitive nerves, and these we can trace into them by the knife, and consequently, they will indicate a certain degree of sensibility when hurt. To expose these nerves near their origins, and before any filament of a sensitive nerve mingles with them, requires the operator to cut deep, to break up the bones, and to divide the blood-vessels. All such experiments are much better omitted; they never can lead to satisfactory conclusions."

* * * * *

"*Between the brain and the muscles there is a circle of nerves; one nerve conveys the influence from the brain to the muscle, another gives the sense of the condition of the muscle to the brain.* If the circle be broken by the division of the motor nerve, motion ceases; if it be broken by the division of the other nerve, there is no longer a sense of the condition of the muscle, and therefore no regulation of its activity.*

"* Thus led to conclude that there is motion in a circle, we nevertheless cannot adopt the hypothesis of circulating fluids. That a fluid does not proceed from the brain, "We have noticed, that there is a plexus formed both on the nerves which convey the will to the muscles, and on the nerves which give the sense of the condition of the muscles. The reason of this I apprehend to be that the nerves must correspond with the muscles, and consequently with one another. If the motor nerve has to arrange the action of several muscles so as to produce a variety of motions, the combinations must be formed by the interchange of filaments among the nerves before they enter the muscles, as there is no connexion between the muscles themselves. As the various combinations of the muscles have a relation with the motor nerves, the same relations must be established by those nerves which convey the impression of their combinations, and a similar plexus or interchange of filaments therefore characterizes both."

XIII. *On the Constitution of the Atmosphere;* by John Dalton, Esq. FRS. &c.

The *Annals* for April, p. 289, contains an abstract of this communication. E. W. B.

Article VI.

Proceedings of Philosophical Societies.

ASTRONOMICAL SOCIETY.

June 9.—The reading of the Rev. Fearon Fallows's paper on the Small Transit Instrument, was concluded. Mr. Fallows's directions may be comprehended briefly in the following particulars: 1. Place the transit instrument as near the meridian as possible, and also substantial meridian-marks at a considerable distance both to the north and south. 2. The clock must be set forward to sidereal time, and its daily rate obtained. 3. Observations of pairs of high and low Greenwich stars must be made each evening, *along* with others whose right ascensions are required. 4. The apparent right ascensions of the Greenwich stars must be computed up to the time of observation, or taken from the Nautical Almanac. 5. The azimuthal error must be found, if possible, by several pairs of those. Also, 6. The error of the clock at the transit of one of the Greenwich stars. 7. Reckon this error constant to every observation made during the same night. 8. The azimuthal error must be considered, with a contrary algebraic sign, for stars between the zenith (of the Cape)

we may learn from this; that on touching the end of a motor nerve which has been some days separated from the brain, the muscle is excited as when the nerve was first divided. The property, however it may be defined, is therefore in the nerve. Our language might perhaps be made more precise if we used terms which implied the course of nervous influence, whether from or towards the brain; but it will be difficult to express this without the aid of hypothesis."

and the pole. 9. A proportional part of the daily rate must be applied to every observation from the first. 10. The error of each star from the true meridian, must be computed from tables prepared for the purpose. 11. To the time of transit of each star, add the error of the clock (6), the proportional part of the daily rate (9), and the error from the meridian (10); the respective sums will give the true apparent right ascensions required. 12. Compute the sum of the corrections for precession, aberration, lunar and solar nutation, for every star at the time of observation, and apply each sum with a contrary algebraic sign to each true apparent right ascension; the result will give their mean right ascension for the beginning of the year. 13. Let a series of these, for each star, be registered, and the mean of each series (if the observations be good) may be expected to give the mean right ascensions at the beginning of the year, with considerable accuracy.—The author concludes with observing, that frequent applications of the *level* to the axis of the instrument, during a night's observations, are indispensable.

The same evening there was read, "An Appendix to a former Paper on the Latitude of the Royal Observatory, by the Astronomer Royal." The author of this Appendix defines the latitude of a place to be the observed altitude of the centre of a small circle described by the pole-star, the state of the barometer and thermometer being given, *minus* the refraction due to that altitude. The last correction he regards as altogether arbitrary, and states that he employs Bradley's refractions. The observations of the last eighteen months at Greenwich, with the two circles, as described in a former paper, include 720 of the pole-star, from which the co-latitude deduced is 38° 31′ 21″·045.

There was next read, "A Summary of the Observations made for the Determination of the Latitude of the Observatory at Wilna, by M. Slawinski." The observations amount to 260, and were made in the months of October and November, 1825. The author gives an account of his researches to determine the flexure in the repeating circle, and explains that his reductions are made, both by means of the places of stars given in Bessel's Tables, and the positions announced in the Nautical Almanac for 1827. The latitude referred to the centre of the transit instrument is 54° 40′ 59″·09 deduced by comparison with Bessel, and 54° 41′ 0″·05 by comparison with Naut. Alm. The greatest of these determinations is less by about 2″ than the latitude of the same observatory, as given by M. Slawinski's predecessors, Poczobut and Sniadecki.

The reading of M. Slawinski's paper, was followed by that of one on "Micrometrical Observations of the Planet Saturn, made with Fraunhofer's large Refractor at Dorpat, by Professor Struve." These observations were made with a refracting wire micrometer attached to Fraunhofer's large telescope, now so well-known, employing the power 540: Professor Struve describes both the instrument and the manner of observation; but it will be simply necessary here to record the results for the planet's mean distance, which are as below: viz.

1.	The external diameter of the	external ring =	40″·215
2.	internal	ditto.	35 ·395
3.	external	internal ring	34 ·579
4.	internal	ditto.	26 ·748
5.	equatorial diameter of Saturn		18 ·045
6.	breadth of the external ring		2 ·410
7.	ditto	chasm between the rings	0 ·408
8.	ditto	internal ring	3 ·915
9.	Distance of the ring from Saturn		4 ·352
10.	The equatorial radius of Saturn		9 ·022

The mean value of the inclination of the ring to the ecliptic, is 28° 5′·9, with a probable error not exceeding 6′·9.

M. Struve has detected no trace of a division of the ring into many parts; but he observes that the outer ring is much less brilliant than the inner. The five longest-known satellites are readily distinguished, through Fraunhofer's telescope, even in the illuminated field. The 4th appears like a small disc, diameter 0″·75. M. Struve saw the 6th several times; but he has never seen the 7th; of whose existence indeed Schröeter entertains doubts.

The same paper also details the results of micrometrical measurements of Jupiter and its satellites, made with the same instruments, and with the same power 540, or from thence to 600. The mean results at the mean distance of the planet from the earth, are,

1.	Jupiter's major axis		38″·442
2.	minor axis.		35 ·645
3.	compression		0 ·0728 or $\frac{1}{13\cdot 71}$
4.	Mean diam. ♃'s 1st Sat. . .		1 ·018
5.		2d	0 ·914
6.		3d	1 ·492
7.		4th	1 ·277

Schröeter and Harding have often imagined, that they have detected a deviation of Jupiter from the elliptical form; and so thought Struve at first; but a closer examination enables him to explain the illusion. On March 7th this year, he thought the diameter which extended from 61°·4 lat. preceding S. to 61°·4 lat. following N. was obviously smaller than the ellipsis would allow. But the micrometric measurement proved that that was not the case. That evening the major axis, A, was 44″·75; the minor axis, B, was 41″·72; and the diameter in question taken with the same micrometer was 42″·34. Calling this diameter x, and the latitude on the planet, l, we have $x = \frac{A \cdot B}{(A^2 \sin.^2 l + B^2 \cos.^2 l)^{\frac{1}{2}}}$, and the numerical result is $x = 42''\cdot 38$, differing only $0''\cdot 04$ from the measurement. Most probably it is the slanting position of the axes of the ellipse, with regard to the vertical circle, which causes this illusion.

Lastly, there was terminated on the same evening, an "Account of some Observations made with a Twenty-feet Reflecting Telescope, by J. F. W. Herschel, Esq. Sec. RS. and For. Sec. of this Society." This valuable communication is divided into four sections. The first contains descriptions and approximate places of 300 new double and triple stars. The telescope with which the observations were made, is one of the "front view" construction; aperture 18 inches, focal length 20 feet. It was constructed in the year 1820, under the joint superintendence of Mr. Herschel and his venerable father. Its light, with its full aperture, enables it to reach the faintest nebulæ of the third class, while, with an aperture of 10 or 12 inches, it serves to define double stars of the first class of an average degree of closeness. Mr. Herschel briefly describes the method of differences employed in *sweeps* of the heavens, the modifications introduced into the process on account of Mr. Herschel's being deprived of the valuable assistance of his aunt, Miss Caroline Herschel, his classification and characteristics of the *magnitudes* of the stars from the 7th to the 20th inclusive, of which none of the last three can be seen with the least illumination, but comprehend the stars seen or suspected in resolvable nebulæ. Mr. H. then presents an example of the method, in which the business of "a sweep" is conducted, and of the method of obtaining from it the approximate right ascensions and polar distances of the objects which it comprises; accompanied by several instructive remarks. The table exhibits, in eight columns, the approximate places of 321 new double and triple stars, for Jan. 1, 1825, with their estimated angles of position, distances, magnitudes, and other particulars. A great many of the double stars tabulated in this paper, exhibit the highly interesting and curious phænomenon of contrasted colours; in combinations of white and blue or purple, yellow, orange, or red, large stars, with blue or purple small ones: red and white combinations also sometimes occur, but with less frequency. In all these cases, the excess of rays belonging to the less refrangible end of the spectrum falls to the share of the large star, and those of the more refrangible portion to the small. Another fact not less remarkable, and rendering highly probable some other relation than that of mere juxta-position, is, that though red single stars are common enough, no example of an insulated blue, green, or purple one has yet been produced.

The three remaining sections of this paper comprise observations of the second comet of 1825; an account of the actual state of the great nebula in Orion, compared with the observations of former astronomers; and observations of the nebula in the girdle of Andromeda. The account of the comet, and that of the great nebula in Orion, are accompanied with illustrative drawings, and the latter also with a kind of map representing the whole as a constellation, in which the parts are named, agreeably to a rude resemblance which the whole nebula presents to the head, snout, and jaws, of some monstrous animal. Aided by these drawings, the verbal account presents an instructively perspicuous description of the truly interesting phænomenon to which it relates.

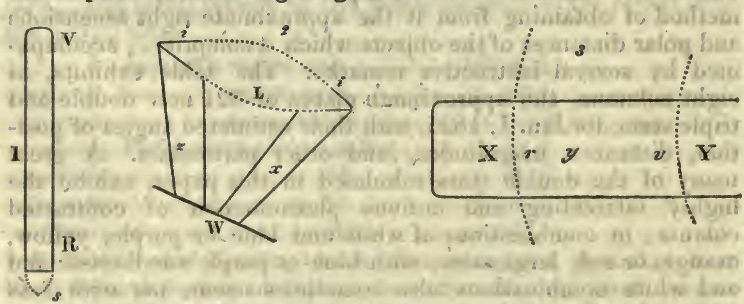
Article VII.

SCIENTIFIC NOTICES.

Miscellaneous.

1. *Solar Spectrum—Light and Heat.*

Prof. Leslie exhibited some interesting experiments in his Class-room this week, with the view of showing the inaccuracy of the received opinion with regard to the heat of the solar spectrum. Profs. Jamieson, Russel, and Monro, Mr. Adie, optician, Mr. Stevenson, engineer, and several other gentlemen were present. We had also an opportunity of witnessing the experiments, the nature of which we shall endeavour to explain, with the help of the following diagrams.



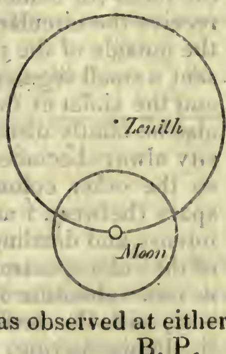
By some experiments made about thirty years ago, Dr. Herschel was thought to have established a conclusion, which has ever since been regarded as extremely curious. On trying the temperature of the different coloured rays, which form the solar spectrum (V, R, fig. 1), he found that the heat was very unequally divided, that it was smallest in the violet rays, V, rather greater in the indigo, and went on increasing through the blue, green, orange, yellow, and was greatest of all in the red, R, at the other end of the spectrum. But what was most remarkable, the heat was found to extend beyond the limits of the spectrum, and reached its maximum, not within the red rays, but at a point *s* in the dark space, about half an inch beyond their outer boundary, from which it diminished in both directions. This conclusion has been long received in Britain as indisputable, and now finds a place in all our elementary work on Natural Philosophy. Prof. Leslie was led, however, to question its accuracy, by an experiment he made twenty year ago, and which was substantially the same with that made the other day, except that, instead of a common double-convex lens, he had now the advantage of employing one of Fresnel's lenses (surrounded by concentric prismatic rings) belonging to the Northern Lights, with which he was accommodated by Mr. Stevenson. The result was stated briefly in a note to the article *Climate*, published in the Supplement to the Encyclopædia Britannica eight years ago, but not in a way to attract the attention which the subject merited. The experiment is remarkably simple, yet extremely well calculated to bring Herschel's doctrine to the test.

Let L in figure 2 represent a double-convex lens of 20 inches diameter. If the middle of this lens (marked by dotted lines) is covered with a sheet of opaque paper, the uncovered rim *i i*, two inches broad, will form a circular prism, which, if it were extended in a straight line, would be five feet long. If the lens thus covered is exposed to the sun, the rays or pencils of rays *x z*, which pass through the rim, will be refracted exactly as they are in the spectrum, but they necessarily converge, and thus the heat and light of a prism five feet long can be accumulated in a small point. Let a piece of paper be held at W a little before the focus (or behind, for it answers equally either way), so as to receive the circular ring of light *x z*, the red rays will be seen at the outside of the ring at *r* (fig. 3, where the dotted lines represent a small segment of the luminous ring, and X Y the paper) and the violet at the inside, *v*. The orange and indigo rays may also be faintly discerned ; but as light in a state of great intensity always becomes white, of whatever rays it may be composed, so the other colours which should occupy the intermediate space (between *v* and *r*), blue, green, and yellow, are lost in one intense and dazzling white. We have here, in short, the colours of the solar spectrum, but in a state of great concentration. Let us now substitute for the piece of paper a stick of black sealing-wax, with the surface roughened; and let this be placed across the luminous ring, in the position W, fig. 2, or X Y, fig. 3. In the course of a minute, the surface of the wax begins to shine about *y* (fig. 3), and then melts. The fusion extends towards *v* and towards *r*, but always stops at the extreme edge of the red rays. Now, if Dr. Herschel's opinion were correct, *the fusion should begin on the outside of the dotted line, about half-way between r and* X, and from this point it should extend inwards towards *r* and *y*, and also outwards in the opposite direction. The experiment was repeated many times, but the result was invariably the same. The fusion always began about that part of the spectral image where the yellow or orange rays are placed; and, on the other hand, when a plate of wax was placed close to the boundary of the red *on the outside*, where Dr. Herschel says the *maximum* of heat is found, the fusion never commenced at all. The breadth of the luminous ring was about one inch, its diameter three or four inches.

This experiment, which is extremely simple, and exempted from many sources of error which attach to experiments with the thermometer, seems clearly to show, *that the maximum of heat is not beyond the red rays as Herschel supposed, but distinctly on the inside of them.* Herschel's doctrine ought, therefore, to be expunged from our scientific treatises: but perhaps it might be no more than a proper mark of respect to the talents of that eminent philosopher, to repeat his experiments, with the view of tracing the circumstances that led him into error. The judicious and accurate Haüy, in his Elementary Treatise published more than twenty years ago, observes that Herschel's experiments stand in need of repetition (Gregory's Haüy, ii. 258, 1807); and we learn that when they have been repeated at a more recent period by the French and German philosophers, they have led to different results.

Prof. Leslie's note descriptive of his experiment will be found in the article *Climate*, in the Supplement, at the foot of p. 193. —(Scotsman, July 29.)

2. *Luminous Circle around the Moon.*



On the 26th of Oct. 1825, at about half-past ten in the evening, the following phænomenon was observed by two gentlemen at Kensington,—a faint luminous circle surrounding the moon, not sensibly tinged with any colour, and intersected by a larger one parallel to the horizon, passing through the moon, and likewise colourless. The circles were not quite continuous in all parts, and there were a few thin clouds: the whole appearance lasted nearly a quarter of an hour. Nothing like a paraselene was observed at either of the points of intersection. B. P.

3. *Geckoes used for catching Flies.*

In Java, the inhabitants rid themselves of flies in their apartments by means of Geckoes, a species of lizard, named, from their cry, toké and gogok, which continually pursue these insects for the purpose of feeding upon them.—(Edin. New Phil. Journ.)

4. *On the Serpents of Southern Africa.*

"I have made a great many experiments upon such serpents as I have been able to procure alive, and have thereby ascertained which of them are or are not poisonous," and "by actual experiments I have found, that not a greater proportion than one to six of the species found here are noxious; we have three species of the viper, the bites of all of which are bad, though not invariably fatal; also three species of Naia, the bites of all of which produce almost certain death; and two species of Elaps, which, from my observations, are also very dangerous."—(Extract of a letter from Mr. Thomas Smith, of Cape Town, to Prof. Jameson.)

5. *Manner of the Serpent-Eater* (*Gypogeranus*) *in destroying Serpents.*

"Some time ago, as a gentleman was out riding, he observed a bird of the above-mentioned species, while on the wing, make two or three circles, at a little distance from the spot on which he then was, and after that suddenly descend to the ground. On observing the bird, he found it engaged in examining and watching some object near the spot where it stood, which it continued to do for some minutes. After that, it moved with considerable apparent caution, to a little distance from the spot where it had alighted, and then extended one of its wings, which it kept in continual motion. Soon after this artifice, the gentleman remarked a large snake raise its head to a considerable distance from the ground, which seemed to be what the bird was longing for, as the moment that took place, he instantly struck a blow with the extremity of the wing, by which he laid his prey flat on the ground. The bird, however, did not yet appear confident of victory, but kept eyeing his enemy for a few seconds, when he found him again in action, a circumstance that led exactly to a repetition of the means already detailed. The result of the second blow appeared, however, to inspire more confidence; for almost the moment it was inflicted, the bird marched up to the snake, and commenced kicking it with his feet; after which, he seized it with his bill, and rose almost perpendicularly to a very considerable height, when he let go the reptile, which fell with such violence upon the ground, as seemingly to satisfy him, that he might now indulge himself with the well-earned meal in perfect safety." *From the Same to the Same.*—(Edin. Phil. Journ.)

6. *New Species of North American Quadruped.* By Richard Harlan, MD. Professor of Comparative Anatomy to the Philadelphia Museum, &c.

Arvicola Ferrugineus. (nob.)
Vulgo.—*White-bellied Cotton Rat.*

Char. Body large, ferruginous, brown above, whitish beneath; fore legs very short and slender; tail more than half the length of the body.

Dimensions. Total length from the snout to the root of the tail seven inches; length of the tail four inches.

Description. Head long; snout tapering; whiskers white, fine, and sparse, some long, others short; ears rather large, broader than long, sparsely hairy within, naked without, anterior borders covered with long hairs—the teeth do not differ essentially from those of the *A. hortensis* (nob.)* the upper molars are rather more compressed in their antero-posterior diameter, and the curved lines of enamel on the crowns of the inferior, assume, in some instances, the form of the Greek epsilon. Body massive, tapering towards the root of the tail in the same manner, though not to the same degree, as in the Norway rat; covered with fine long hairs of a dark plumbeous colour, tipped with brown, and intermixed with black. Inferior parts of the body plumbeous-white, the hairs being plumbeous, tipt with white; tail slender, tapering, covered with hair, brown above, whitish beneath; feet grayish, white anteriorly, in form and structure resembling those of the *A. palustris* (nob.),† but in proportion are exceedingly small and slender, being very little larger than those of the common mouse—in an animal seven inches in length of body, and nearly six inches in girth, the fore legs measure less than one inch and a half to the extremity of the nails; the latter are black, compressed, sharp, and hooked, as in the squirrel.

Habit. According to Mr. J. J. Audubon (to whom I am indebted for this specimen), this animal never burrows, but conceals itself in hollow trees, generally forming a hole in the side, somewhat after the manner of a woodpecker, where they retreat in case of emergency. They inhabit the cotton fields exclusively; carry their young on their back; and, with their family thus secured, climb dead trees as nimbly as the squirrel.

Inhabit the borders of the Mississippi—the present specimen from Beech woods near Natchez.

On the whole, the present species bears a near resemblance to the *Arvicola hortensis,* but is sufficiently distinguished by the extreme proportional minuteness of the fore legs and feet, by the colour of the fur, as also in size and in the tapering form of the body at the root of the tail, the manners of the animal, &c. —(American Journal of Science.)

* Vid. Fauna Americana, p. 138. † Ibid. p. 136.

Article VIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Elements of Chemical Science, in one vol. 8vo. by Dr. E. Turner.

Dr. Forbes, of Chichester, is preparing a Translation of the improved edition of Laennec's Treatise on Disorders of the Chest, with Notes and Commentaries.

The Tenth Part of the Animal Kingdom, described and arranged in Conformity with its Organization; by the Baron Cuvier, &c.: with additional Descriptions of all the Species hitherto named, of many not before noticed, and other original Matter; by E. Griffith, FLS. and others.

Dr. W. J. Hooker is preparing a Third Edition of his Muscologia Britannica, containing the Mosses of Great Britain and Ireland systematically arranged.

JUST PUBLISHED.

Researches into the Nature and Treatment of the several Forms of Dropsy. By Joseph Ayre, MD. 8vo. 8*s*.

The Surgery of the Teeth, exhibiting a new Method of treating Diseases of the Teeth and Gums, with Remarks on the present State of Dentist Surgery, and the more prevalent Abuses of the Art. By Leonard Koecker, Surgeon Dentist. 8vo. 14*s*.

Sweet's Hortus Britannicus, Part I. 10*s*. 6*d*.

An Inquiry concerning the disturbed State of the Vital Functions, usually denominated Constitutional Irritation. By B. Travers, FRS.

Journal of a Third Voyage for the Discovery of the North-west Passage. By Capt. W. E. Parry. 4to. With Plates.

Article IX.

NEW PATENTS.

J. Barron, Birmingham, brass-founder and venetian blind-maker, for a combination of machinery for feeding fire with fuel.—July 24.

W. Johnston, Caroline-street, Bedford-square, jeweller, for improvements on ink-holders.—July 24.

W. Robinson, Craven-street, Strand, for a new method of propelling vessels by steam.—July 24.

W. Parsons, Dock Yard, Portsmouth, naval architect, for improvements in building ships, which are calculated to lessen the dangerous effects of internal or external violence.—July 24.

W. Davidson, Glasgow, surgeon and druggist, for processes for bleaching or whitening bees'-wax, myrtle-wax, and animal tallow.—Aug. 1.

T. J. Knowlys, Trinity College, Oxford, and W. Duesbury, Bousal, Derbyshire, collar manufacturer, for improvements in tanning.—Aug. 1.

Article X.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
July 23	30·00	30·00	30·000	66	58	62·0		NE	Clear.
24	30·02	30·02	30·020	67	56	61·5		NE	Clear.
25	30·06	30·04	30·050	68	59	63·5		NE	Clear.
26	30·08	30·08	30·080	72	58	65·0		NE	Clear.
27	30·12	30·10	30·110	70	58	64·0		SE	Clear.
28	30·12	30·08	30·100	70	56	63·0		SE	Clear.
29	30·00	29·98	29·990	70	56	63·0		SE	Clear.
30	29·90	29·86	29·880	72	58	65·0		SE	Clear.
31	29·82	29·80	29·810	76	60	68·0		SW	Clear.
Aug. 1	29·82	29·78	29·800	72	60	66·0		NE	Cloudy; thunder.
2	29·70	29·70	29·700	75	62	68·5		SW	Cloudy.
3	29·78	29·72	29·725	70	60	65·0		NE	Some showers.
4	29·76	29·74	29·750	70	60	65·0		N	Showers.
5	29·82	29·80	29·810	68	60	64·0	0·15	N	Cloudy.
6	29·96	29·90	29·930	73	58	65·5		NW	Clear.
7	30·12	30·10	30·110	70	58	64·0		NW	Clear.
8	30·08	30·06	30·070	71	58	64·5		NW	Clear.
9	29·90	29·88	29·890	70	58	64·0		N	Clear.
10	29·86	29·80	29·830	71	58	64·5		NW	Cloudy.
11	29·80	29·78	29·790	68	60	64·0	0·08	NW	Showers; clear.
12	29·90	29·82	29·860	68	56	62·0		Var.	Clear.
13	30·00	29·98	29·990	68	55	61·5		NW	Clear.
14	29·68	29·60	29·640	70	58	64·0		NW	Clear.
15	29·78	29·76	29·770	74	56	65·0		SW	Clear; rain at night.
16	29·80	29·70	29·750	68	56	62·0	0·05	SW	Clear.
17	29·96	29·90	29·930	70	58	64·0		SW	Clear.
18	30·12	30·10	30·110	74	56	65·0		S	Clear; fair.
19	30·06	30·00	30·030	72	58	65·0		S	Clear.
20	29·90	29·88	29·890	70	58	64·0		W	Clear.
21	29·88	29·87	29·875	70	57	63·5		N	Clear.
22	29·80	29·78	29·790	72	56	64·0		W	Cloudy.
	30·12	29·60	29·910	76	55	65·0	0·280	NW	

RESULTS.

Barometer, mean height .... 29·910
Register Thermometer, ditto .... 65·0°
Rain, No. 1, 0·280, No. 2, 0·610.
Prevailing wind, NW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, Aug. 22, 1826. EDWARD C. GIDDY.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1826.

Article I.

*Biographical Notice of Joseph-Louis Proust, Member of the Institute, of the Legion of Honour, of the Royal Academy of Medicine, and Professor of Pharmacy.**

France has recently lost one of the most illustrious of her learned men. Joseph-Louis Proust died at Angers, his native city, on the 5th of July last. This loss, which affects all who are interested in the progress of science, will be most severely felt by the professors of pharmacy, who have been deprived of one of their most honoured members;—a man whose name will be associated with those of Scheele and Rouelle; with all the most celebrated discoveries in pharmacy; and in whom an elevated genius was accompanied by a simplicity of manners and a modesty that heightened its splendour.

Joseph-Louis Proust was born at Angers, in 1755, in which city his father was an apothecary, and from his youth he was intended for the same profession. Having completed the early part of his education at home, he came to Paris, to work under the direction of M. Clerambourg, a respectable apothecary. He was there remarked for the zeal with which he studied chemistry and the practice of his art, and he was not long in reaping the reward of his labours.

The office of Chief Apothecary at the Hospital de la Salpêtrière becoming vacant, it was left open to competition; young Proust did not become a candidate, but some of his friends, knowing his modesty and merit, placed his name in the list. This competition was attended with brilliant success, and formed the commencement of his reputation; he obtained the office with every vote in his favour, which procured him an honourable subsistence, and the means of readily prosecu ing the study of chemistry, for which he had an irresistible inclination.

* From the Journal de Pharmacie, for July, 1826.

From the lectures and conversations of Rouelle, he imbibed his decided taste for this science, and also, perhaps, that original and acute mode of thinking which was so eminently characteristic of his master. Rouelle was a competent judge of talent; he assisted the young chemist with his advice, honoured him with his friendship, and facilitated the commencement of that career which he continued with so much success.

During the time that he retained his situation at the Salpêtrière, Proust wrote and published several notices and memoirs, among which may be mentioned his Researches on Urine, his Essay on Phosphoric Acid, a Memoir on Pyrophori without Alum, Experiments on the rapid Combustion of Essential Oils by Nitric Acid, &c. He discussed the opinions which had been previously broached on the subjects of which he treated, and he early evinced that independence of mind which afterwards appeared in his scientific researches.

Proust was known at this period not only by the works which he published, but also by teaching chemistry with great success in the Musée du Palais Royal, and also in a private establishment founded by the unfortunate Pilatre de Rosier. He accompanied this philosopher in his first aërostatic ascent, which took place at Versailles on the 23d of June, 1784, in the presence of the Court, and of the King of Sweden, who witnessed this exhibition for the first time. The balloon rose at first with some difficulty, and the rapid oscillations caused by the wind occasioned a momentary apprehension for the safety of the two aëronauts; but being soon freed from the shackles which detained it on earth, it rose majestically into the lofty regions of the atmosphere, and speedily allayed the fears of the multitude for the safety of the intrepid navigators.

This aërial voyage, the longest which had till then been made, and the account of which is extremely curious, was crowned with complete success; the balloon descended without any damage, in an hour and seven minutes, at thirteen leagues from the place of ascent. Proust peremptorily refused to have any thing to do with the second ascent, the danger of which he foresaw.* It was not his fault that the unfortunate Pilatre did not escape the deplorable accident of which he was the victim, and which too well justified the melancholy forebodings of the chemist.

The Spanish government, observing the progress which the physical sciences were then making in France, and foreseeing the resources which they might afford to the industry of a people, offered Proust a professorship in the School of Artillery

* This second ascent was made by combining the process of Montgolfier with that of Charles; there were two balloons, the upper one was filled with hydrogen gas, and the lower one with air expanded by heat; that which Proust had foreseen occurred, the hydrogen gas took fire, and Pilatre de Rosier was precipitated from the air, with a companion, who, wishing to participate in his glory, shared his fate.

at Segovia: he set out for Spain, and quickly realized the high hopes which his reputation had inspired. He proposed or perfected a great number of processes interesting to the industry of that country, and on several occasions he received the most flattering proofs of the satisfaction of its Sovereign. This monarch determined to found a central school of chemistry at Madrid, and Proust was there appointed Professor of this science; he was treated at this place with great attention and respect and the whole establishment was endowed with a truly royal magnificence. Almost all the utensils, even those most commonly employed, were of platina, which the king presented to him.

It was in this laboratory and in that at Segovia that his principal operations were performed, among which a great number may be mentioned, the chief object of which was the direct benefit of the country. It is to him that we owe the first analysis of the native phosphate of lime of Lograsan, in Estremadura. He also made some experiments upon saltpetre and sulphate of magnesia, both of which occur native in Spain. He published a very minute account of the essential oils of Murcia, and showed that camphor might be advantageously procured from them. He also made many experiments to determine the quantities of charcoal which are yielded by different kinds of wood, and upon that which is procured from the coal and peat of Spain.

We are indebted to him for an analysis of the native iron of Peru, in which he found nickel, and also for a great number of experiments upon several American minerals, and particularly upon the ore of platina. He also published a work on the means of bettering the sustenance of the soldier; these means are chiefly derived from the nutritive property of gelatine procured from bones. Papin had before him proposed to extract it, by subjecting the bones to a very strong heat in the digester which bears his name. Proust endeavoured to revive the project of Papin, but he substituted a more ready method of extraction.

This work, which is less remarkable in a scientific point of view than with respect to the extremely important question of which it treats, is well calculated to give an idea of the lively and original manner of its author, and especially of the zeal with which he endeavoured to diminish the privations of the lower classes of society.

It was with the same intention of offering to the poor a new and nourishing substance, which was both abundant and cheap, that he afterwards published a memoir on the *lichen islandicus.* In this, as well as the preceding memoir, and indeed as in almost all his works, there appear the same deep feeling of humanity and the same active zeal for the benefit of his race, which incessantly excited him to discover the means of ameliorating their condition, by teaching them to take advantage of the resources with which benevolent nature has so profusely surrounded them.

To him, true science was, as he says in this memoir, "that which teaches us to obtain from the productions with which the Creator has abundantly supplied the world we inhabit not only the best methods of increasing the means of subsistence, but of enriching medicine, domestic economy, and the arts."

He also wrote several memoirs on the sugar of grapes, and on the methods of preparing it; and in these he still kept in view the supplying of the poorer classes with an agreeable and wholesome food, of which they were particularly destitute at the time at which he undertook the investigation.

Besides the labours which we have mentioned, Proust published a great number of memoirs, which enriched science, and ranked their author among the first chemists of the age.

Favoured by fortune, honoured with the esteem of the public and the protection of the Sovereign, possessing extremely curious collections of the most remarkable and precious productions of both the Indies, which would have supplied an inexhaustible source for his researches, it remained only for Proust to enjoy the happiness he had himself created; when in one day all his hopes vanished. He was in France on leave of absence, when the chances of war brought the French to Madrid, and ruin on his establishment—a ruin which was so complete that his personal loss may without exaggeration be estimated at more than half a million of francs. Reduced to a state bordering upon indigence, he bore this reverse with stoical courage, and if some expressions of regret escaped him, they were not for his fortune, but for the collection of chemical and mineral substances which he had formed with so much care. Who would not share his regret, when, in speaking in one of his memoirs of some minerals which he had intended to analyze, but which want had forced him to sell, he exclaims, with affecting simplicity, "Oh! destiny of human affairs, instead of analyzing these minerals, it is necessary to deliver them to one of those persons to whom we say, *Fac ut lapides isti panes fiant.*"

Napoleon wishing to encourage the preparation of sugar of grapes, engaged Proust to establish a manufactory for it at the expence of government, and he decreed him a gratuity of 100,000 francs. Proust refused it, his health and age not permitting him to fulfil the duties attached to the imperial liberality. He retired to Craon (Mayenne), where he lived on his moderate patrimony.

Although he did not reside at Paris, he was nevertheless, by particular favour, nominated Member of the Academy of Sciences, on the 12th of Feb. 1816, in the place of M. Guyton de

Morveau: this favour was as flattering to him as it was honourable to those who granted it. The advantage belonging to the title of Academician, which he enjoyed from this period, and a pension of 1000 francs, which he derived from the liberality of the late king, Louis XVIII. contributed to render his existence more happy towards the end of his life.

After his nomination to the Academy of Sciences, Proust returned to Angers, his native city; he there wrote several memoirs, some of which were addressed to the Institute; he also sent to the Academy of Medicine some researches into one of the causes which occasion the formation of calculi. Lastly, he was occupied in an important work upon urine, when death snatched him from science; it is, however, to be hoped, that this work, which was considerably advanced, will not be entirely lost, and that the first part of it will be published.

As we can only give a concise account of the scientific claims of Proust, we shall not give the titles of all the memoirs which he published in most of the records of science, especially in the Journal de Physique, from 1771 to the present period. We shall name only the principal: we first mention his Memoirs on Prussian Blue, in which he shows that the colour of this substance depends upon the degree of oxidation of the iron, that this oxidation cannot exist in all proportions, but that it stops at two fixed points, and that all the shades observed in prussian blue are derived from a mixture of the two prussiates of iron; he states a great number of the properties of this singular product, which has since exercised the sagacity of the most distinguished chemists, and upon the nature of which they are not yet agreed. We will also mention his work upon tin, which is a very remarkable memoir, replete with new and curious facts, and in which he decidedly proves, that tin, as well as iron, is susceptible of only two degrees of oxidation; he very minutely describes the two chlorides of tin; he explains the deoxidizing action of the protochloride upon indigo, the salts of peroxide of iron and of copper; he first showed the existence of a chloride of copper differing from the green chloride, and of a protoxide of the same metal, which was not suspected before his labours. His researches on the oxides of cobalt and nickel are of great importance, and so also are those upon antimony, arsenic, mercury, silver, and gold, upon the metallic sulphurets and upon gunpowder.

We observe in reading these memoirs, that independently of the peculiar merit of each, they are all intended to prove that no combinations of bodies with each other occur in indefinite proportions, but that they are subject to invariable and fixed proportions; to that *pondus naturæ,* as he himself says, "which characterizes all the true compounds of art and of nature." This opinion of Proust respecting definite proportions, which was his favourite subject, and which is found in every page of his memoirs, was not, however, admitted by all the chemists of the day. A chemist, whose recent loss the sciences still lament, the learned author of the *Statique Chimique*, long disputed an opinion which ill accorded with his ingenious theory of chemical affinities; and he disputed with so much sagacity, that he left the question long undecided.

The chemists, however, who afterwards occupied themselves with the same subject, fully confirmed the opinion of Proust, which has been greatly extended by the more exact knowledge of the composition of a great number of bodies that has been acquired. It is, in fact, one of the best demonstrated truths of modern science, and it forms the basis of the atomic theory.

We are undoubtedly far from having enumerated all the claims to renown which belong to M. Proust; it would be necessary to dedicate more room to it than can be allowed to a mere notice. A Member of the Academy of Sciences, and of the Academy of Medicine, he will find in these two societies philosophers who are more eloquent and more worthy than we to pay that homage to his genius which is its due; but it particularly belongs to the editors of a journal dedicated to the professors of pharmacy, to announce the loss which they have suffered, and to strew the first flowers on the grave of so illustrious a philosopher and so good a man.

Article II.

Abstracts of Papers in the Philosophical Transactions for 1825, *on the peculiar Magnetic Effect induced in Iron, and on the Magnetism manifested in other Metals, &c. during the Act of Rotation.* By Messrs. Barlow, Christie, Babbage, and Herschel.

(*Concluded from* p. 192.)

Account of the Repetition of M. Arago's Experiments on the Magnetism manifested by various Substances during the Act of Rotation. By C. Babbage, Esq. FRS. and J. F. W. Herschel, Esq. Sec. RS. (*concluded.*)

When we come to reason on the above facts, much caution is doubtless necessary to avoid over-hasty generalization. Whoever has considered the progress of our knowledge respecting the magnetic virtue, which, first supposed to belong only to iron and its compounds, was at length reluctantly conceded to nickel and cobalt, though in a much weaker degree—then suspected to belong to titanium, and now extended, apparently with an extraordinary range of degrees of intensity to all the metals—will hardly be inclined to stop short here, but will readily admit, at least the probability, of all bodies in nature participating in it more or less. Yet if the electro-dynamical theory of magnetism be well founded, it is difficult to conceive how that internal circulation of electricity, which has been regarded as necessary for the production of magnetism, can be excited or maintained in non-conducting bodies. Without pretending to draw a line, however, in what is perhaps at last only a question of degree, one thing is certain, that all the unequivocal cases of magnetic action observed by us, lie among the best conductors of electricity. Another feature, no less striking, is the extreme feebleness of this species of action compared with that which takes place in cases of sensible attraction and polarity. This will appear more evidently, if we consider the mode of action which probably obtains in these experiments, and the mechanism, if we may so express it, by which the effects of such almost infinitesimal forces are rendered perceptible in them.

The rationale of these phænomena, as well as of those observed by Mr. Barlow in the rotation of iron, which form only a particular case (though certainly the most prominent of any) of the class in question, seems to depend on a principle which, whether it has or has not been before entertained or distinctly stated in words, it may be as well, once for all, to assume here as a *postulatum*, viz. that *in the induction of magnetism, time enters as an essential element*, and that *no finite degree of magnetic polarity can be communicated to, or taken from, any body whatever susceptible of magnetism, in an instant.*

This principle will, if we mistake not, be found to afford at least a plausible explanation of most, if not all the phænomena above described, without the necessity of calling in any additional hypothesis, or new doctrine in magnetism. For the other principle we shall have occasion to employ, that magnetic bodies differ exceedingly, both in *susceptibility* of this quality and in the degree of the pertinacity with which they retain it, (which may be called their *retentive* power,) is not an hypothesis, but an acknowledged fact. It is only in the mode of its extension to new cases of *magnetics* that we can be led into any fallacies. Whether these two qualities (susceptibility and retentive power) be, or be not mutually dependent, this is not the place to inquire. Probably they are not so, at least directly: and the new facts almost convert this probability into certainty; at all events, at present, we shall, for greater generality, suppose them independent.

Conceive now a plate of any thickness, and of indefinite superficial extent, of a metal or other magnetic, whose retentive power is very small. If either pole (suppose the north) of a magnet be brought vertically over a point in its surface, it will there produce a pole of the contrary name in the plate, the maximum of polarity being immediately under the magnet. Now let the magnet be moved horizontally along the surface, preserving the same distance from it. The points over which in succession it becomes vertical, not *instantly* receiving all the magnetism of which they are susceptible, will not have reached their maximum of polarity at the precise moment of nearest appulse, but will continue to receive fresh accessions during the whole of that certain small portion of time when the distance (being at or near its minimum) undergoes no change, or only a certain very minute one. In like manner, the points which have attained their maximum of polarity, being left behind by the magnet, will by degrees lose their magnetism; but the loss not being sudden, they will continue near their maximum for a certain finite time, during the whole of which the magnet continues receding from them, and leaving them further and further behind. Thus from both causes, there will be always in arrear of the magnet a space both more extensive and more strongly impregnated with the opposite polarity, than in advance of it; and as the magnet moves forward, the point of actual maximum (or the pole) of the plate, instead of keeping pace with it, and being always precisely under it, will lag behind. There will thus arise an oblique action between the pole of the magnet and the opposite pole of the plate so lagging behind it; and were the plate free to move in its own plane, the resolved portion of this action parallel to its surface, would continually urge it in the direction of the magnet's motion.

But besides the attracting pole of the opposite name (south) produced by the (north) pole of the magnet at the spot immediately under it, there will also be developed a corresponding repulsion, or north polarity in the plate. This, however, will not, like the attractive, be concentrated nearly in one spot immediately below the magnet, but must of necessity be diffused round it in a much less intense and more uniform state throughout the more distant parts of the mass, and may be conceived as arranged in spherical or other concave strata about the point vertically under the magnet as a centre. Now when the magnet by its motion is carried out of the axis of these strata, it is obvious that the resultant force of each of them will be less and less oblique to the surface as its radius is greater. The general resultant, therefore, of all the repulsive forces exerted throughout the whole extent of the plate is necessarily less oblique to the surface than that of the attractive ones, whose influence, from this cause alone, must, therefore, preponderate, and must necessarily produce a *dragging* or oblique action, such as above described. This force, however minute, acting constantly, must at length produce a finite and sensible velocity, provided the whole mass of the plate to be set in motion be finite, and the force of the magnet sufficient to overcome friction, resistance, &c.

Vice versâ, if the plate be drawn along in its own plane, and the magnet be free to move in a horizontal direction, the former ought to drag the latter along in the same direction with a velocity continually accelerating, till they move on together with equal velocities.

It is manifest that, *cæteris paribus,* the greater the relative velocity, the more will the pole developed in the plate lag behind the magnet, or the magnet (in the reverse case) behind the pole. The more oblique, therefore, will be the action, and the greater the resolved part of the force, and the velocity produced by it *dato tempore.* The same effect must also be produced by an increase in the absolute force, or lifting power of the magnet; so that in such experiments there is an advantage in using large magnets which have great lifting powers, over small ones with intense directive forces, and this is perfectly consonant to experience.

Hitherto we have only considered the case of rectilinear motion. If we regard the magnetism of the plate as very transient, and the velocity moderate, the whole space occupied by the magnetized portion of the plate will still be small, and confined to the immediate neighbourhood of the point vertically under the magnet. If the motion of the latter change its direction, the momentary pull communicated to the plate will always be in the direction of a tangent to the curve described. If, therefore, it describe a circle, it will tend at every instant to impress a gyratory motion on the plate about a centre vertically under the centre of its own motion, and *vice versâ,* if the plate be made to revolve about a centre, it will tend to drag the magnet round with a continually accelerated motion, provided its rectilinear recess from the centre of motion (or its centrifugal force) be prevented by a proper mechanism. The former is the case of a disc of copper suspended by its centre, and set in rotation by a magnet revolving beneath it. The latter is that of a compass-needle, or of our neutralised system of vertical magnets suspended over a revolving disc of copper. A very pretty illustration of the direction of these forces is obtained by suspending a circular disc of zinc or copper from the end of a counterbalanced arm, which is itself suspended by its middle, thus constituting a kind of double balance of torsion. If the length of the arm be so adjusted, that the circumference of the disc shall be an exterior tangent to the circle described by the poles of a revolving magnet, the whole disc will be swept round in an orbit concentric with the motion of the magnet, while it at the same time acquires a rotatory motion on its own centre in the contrary sense. The centrifugal force is here overcome by the arm and the weight of the disc, and the velocity goes on accelerating till the increase of resistance puts a stop to further accessions.

In Mr. Barlow's experiments, the earth is our inducing magnet; its two poles both act on every particle of the revolving shell employed in that gentleman's experiments, and their action when complete produces two poles, a north and a south, at opposite extremities of the diameter parallel to the dip. This is the case when the shell is at rest. Let it now be set in motion about any axis, anyhow inclined to the dip. If the communication and loss of magnetism were instantaneous, the places of the poles (i. e. the points of maximum polarity) would be unaffected by the rotation; but as that is not the case, these points, in virtue of the principles already stated, will shift their places, and decline from the direction of the dip in the same direction as the shell's motion, that is to say, in the direction of a tangent to a small circle, whose axis is the axis of rotation, and whose circumference passes through the extremities of the diameter parallel to the dip. The extent of this declination will depend on the velocity of rotation and the diameter of this small circle, and will be proportional to both, that is, to the velocity of rotation multiplied into the sine of the angle made by the axis of rotation with the direction of the dip. It will, therefore, be a maximum when the axis of rotation is perpendicular to the magnetic meridian, and vanish when the shell is made to revolve on an axis parallel to the line of dip. These consequences are perfectly consonant to the results obtained by Mr. Barlow in his paper; and, in fact, the general result announced by him in p. 449 of this volume, comes to the very same thing as above stated; for it is obvious, that the new axis of polarization there spoken of, acting in combination with the original, or, as we may call it, the primary axis developed in the quiescent state of the shell, will exert a compound force on the needle, such as would be exerted by a single equivalent axis situated intermediately between them, but much nearer to the more intense than to the more feeble one. The position of this equivalent axis will necessarily be in the great circle passing through the two component ones. Now the small circle described by the point which was first the pole of the stronger or primary axis about the axis of rotation is a tangent to this great circle, and the equivalent axis (being but little removed from the primary one, by reason of the small intensity of the other), will, therefore, have its pole situate indifferently in either circle. Or conversely, the single axis produced in our view of the subject being resolved into two; one of which is that corresponding to the quiescent state of the shell, and the other 90° removed from it in the same place, this latter will be identical with Mr. Barlow's secondary axis.

In what has been said, the velocity of rotation has been supposed commensurate to the velocity with which magnetism is propagated through the iron of the shell. But if we conceive in this, or in the general case, either the retentive power of the shell, disc, or lamina, great, or the velocity of motion excessive, it may be instructive to consider the modifications thus introduced into the effect. It is evident that the induced pole will lag farther and farther behind the magnet in proportion as either of these conditions obtains. In the case of rectilinear motion, this will, up to a certain point, increase the oblique action, and the dragging effect will be strengthened; but if the velocity be excessive, or the retentive force considerable, as in steel, the pole may lag so far behind as to carry it altogether out of the sphere of the magnet's attraction; and the magnetized portion, remaining within its limits, may have not had time enough to acquire a high degree of polarity. From both causes the drag (the expression, though uncouth, is convenient) should be weakened. In the case of circular motion this effect may go so far, that a complete circumference shall have been described before the polarity of any one point shall have been either completely induced, or completely destroyed. In this case the effect observed will be a general weakening of the total polarity of the disc or sphere; and (supposing the latter of iron, or soft steel) a directive virtue on a small compass-needle placed near it, not probably towards any particular place, but to a resultant imaginary point depending on the situation of the compass, the dip, and the axis of rotation, by laws not very easy to assign. This will explain some expressions quoted by Mr. Barlow from his correspondence with one of the authors of this paper, which may appear otherwise to militate against the general view here taken.

This diminution of the total effect by a more general distribution of the magnetism, was imitated by sticking a great number of needles vertically through a light cork circle, all being strongly magnetized, and having their north poles downwards, so as to form a circle, or, as it were, a coronet of magnets. This apparatus suspended centrally over a revolving copper disc, was not sensibly set in rotation. In this case, when at rest, the south polarity induced in the plate would be disposed in spots accumulated under each needle; but these spots, elongated and blended by the effect of rotation, must produce a nearly uniform circle of south polarity, whose equal and contrary actions on all the needles would keep up the equilibrium, and prevent the coronet from acquiring a tendency either way.

One consequence of this reasoning, which deserves trial, is this—that if the axis of rotation of an iron shell be situated in the direction of the dip, the spots occupied by its poles will not change their places by rotation, and consequently no deviation of the compass ought to take place from that cause. The experiment, however, is very delicate; and care must be taken to remove any magnetized bodies whose influence might induce subordinate poles in the shell, whose places would shift by rotation. The compass, therefore, in this case cannot be neutralized by a magnet;* but we must have recourse to some neutral system, such as that described in the foregoing pages, in its place, or it may be left unneutralized. It ought too to be so small, or so remote, as not to produce induced polarity in the shell, which would react on itself when the sphere is set in motion, and destroy the success of the experiment.

The effect of a solution of continuity in the revolving bodies comes next to be considered. It is difficult; but the difficulty is not a consequence of our principles of explanation, but of our ignorance of the very complicated laws which regulate the distribution and communication of magnetism in bodies of irregular figure. So far, however, as the operation of the general principle can be traced, its results are consonant to observation.

In the first place, it is obvious that where one or more slits are cut in a metallic plate, over which the pole of a magnet is revolving, that immediate and free communication between particle and particle, on which probably the rapid, and certainly the intense developement of magnetism depends, is destroyed. The induced pole (by which we mean now the whole of that space in which sensible magnetism is developed, and which is, of course, a spot of sensible, and probably considerable magnitude—of a figure more or less elongated according to the velocity of the motion)—instead of travelling regularly round, retaining a constant magnetism and force, will now be in a perpetual state of change. Instead of being carried uniformly across the slit, it will die away in intensity, and shrink into a point in dimension on the hinder side, and be again renewed on the side in advance, but at first not in its full intensity; so that it is not merely the diminution of surface arising from the abstraction of a part of the metal, but a much more considerable defalcation of magnetic force which takes place on either side of the slit, that operates. Now this operation is always to weaken the drag between the magnet and the disc, and no reason, *a priori*, can be assigned why this effect should not take place to any extent.

The validity of this reasoning is shown by taking the extreme case in which the substance acted on is in the state of powder. Each particle of this becomes necessarily a feeble magnet, and its north and south poles, being at the same distance (almost precisely) from the pole of the magnet, counteract each other's action. The extreme feebleness of their magnetism prevents the particles from affecting each other by induction across the intervals which separate them; so that each acts as an individual, and destroys in great measure its own effect. The moment,

* In Mr. Barlow's experiments, the large and powerful bar-magnets used to neutralize the earth's action on the compass-needle, cannot be without some disturbing influence of this kind.

however, a *metallic*, i. e. *a magnetic contact* is established between them, their mutual induction acts, and the result is a general developement of one polarity in the region adjacent to the magnet; and of the other, feebler and more diffused, in the parts of the mass remote from it. This is probably the rationale of the restoration of virtue which takes place when a cut disc is soldered up. And it is not difficult to conceive that a weak magnetism may be thus very faithfully transmitted through substances, such as bismuth and lead, whose direct action is very small, because, as we have seen, the intensity of their direct action depends, for one of its causes, on the retentive power of the substance, which is out of question in the indirect mode of action here considered. In fact, if the retentive power of the solder were reduced to nothing, i. e. if it gained and lost magnetism instantaneously, it would still act as a conductor, and probably the better for this quality; so that the communication between opposite sides of a slit, or contiguous portions of two adjacent particles of a powder, would still be kept up by it, provided it were susceptible of magnetism at all. The observed and very striking fact then of the powerful action of bismuth as a conductor, while its action as a magnet is so extremely feeble, is in itself a strong argument for the independence of these two qualities, which we have designated by the expressions—*susceptibility*, and *retentive power*, and may possibly be made the foundation of a mode of distinguishing and measuring their degrees in different substances.

On the Magnetism developed in Copper and other Substances during Rotation. In a Letter from Samuel Hunter Christie, Esq. MA. &c. to J. F. W. Herschel, Esq. Sec. RS.

After having made experiments with a thin copper disk suspended over a horse-shoe magnet, similar to those which I witnessed at Mr. Babbage's, I made the following.

A disc of drawing paper was suspended by the finest brass wire (No. 37) over the horse-shoe magnet, with a paper screen between. A rapid rotation of the magnet (20 to 30 times per second) caused no rotation in the paper, but it occasionally dipped on the sides, as if attracted by the screen, which might be the effect of electricity excited in the screen by the friction of the air beneath it.

A disc of glass was similarly suspended over the magnet: no effect produced by the rotation.

A disc of mica was similarly suspended: no effect.

The horse-shoe magnet was replaced by two bar-magnets, each 7·5 inches long, and weighing 3 oz. 16 dwt. each, placed horizontally parallel to each other, and having their poles of the same name contiguous. These produced quick rotation in a heavy disc of copper six inches in diameter, and suspended by a wire, No. 20.

A bar-magnet, four inches long, and having both its ends south poles, was made to revolve rapidly under a copper disc. The disc revolved in the same direction as the magnets.

The two bar-magnets before-mentioned were adjusted to the axis of rotation, so that their upper ends were at the distance of five inches from each other, and their lower ends 1·8 inch apart. They were first made to revolve rapidly under the copper disc with poles of the same name nearest to the disc, and then with poles of a contrary name: the times in which the several rotations of the disc took place were as nearly as possible the same in the two cases.

In the first three, I could only remark the time to the nearest second, having no assistance. Should the times agree precisely, which I have very little doubt they would be found to do, the result would, I think, be singular. It would show that the magnetism in the disc is instantaneously developed by one pole of the magnet, and as instantaneously destroyed, and a contrary magnetism developed by the contrary pole; or rather it would indicate, that the time during which the disk retained the induced magnetism was less than the time of half a revolution of the magnet.

The same two bar-magnets were laid horizontally by the side of each other, four-tenths of an inch a part. They were first made to revolve rapidly under the disc with their poles of the same name adjacent, and then with those of a contrary name adjacent.

From these it appears that the effect was but little diminished by placing poles of a contrary name so close to each other.

The adjacent poles being of the same name, they were connected by a piece of soft iron, one-eighth of an inch thick, and half an inch wide. After $4\frac{1}{2}$ revolutions of the disk (screw), the torsion of the wire was equal to the force of the magnets, and the same was the case at $4\frac{5}{4}$ revolutions (unscrew). So that although the effects were greatly diminished by connecting the poles, they were by no means destroyed.

The magnets were now placed over each other, first with poles of a contrary name, and then with those of the same name contiguous; so that although the upper magnet was nearer to the disc by its own thickness than in the fourth experiment, the effect when poles of contrary name were contiguous was not half what it was when they were connected by the iron.

A thick copper plate, eight inches in diameter, and one inch thick, was placed on the axis of rapid rotation, its plane horizontal. A thin copper disc, four inches diameter, and weighing

23·5 dwts. was very delicately suspended over it by a fine brass wire (No. 37), with a paper screen between the plate and the disc. The distance between the surfaces of the plate and disc five-tenths of an inch. The plate being put in rapid rotation, no sensible effect was produced on the disc.

A bar magnet was placed on the screen under the disc: still no effect produced by the rotation.

A light needle, weight 42·5 grains, six inches long, on a pivot in a compass-box, being placed over the plate, the rotation caused a deviation of 20°; but when a heavy needle, weighing 197 grains, and of the same length, was similarly placed over the plate, it immediately revolved rapidly with the plate.

A bar-magnet, weighing 3 oz. 15 dwts. 19 grs. suspended by a wire, No. 20, revolved rapidly with the plate.

A horse-shoe magnet, weighing nearly a pound, and suspended by the same wire, revolved with the disc.

The following experiments were made with the view of ascertaining whether the effects increased nearly according to any power of the decrease of the distance.

A strong needle, six inches in length, weighing 197 grains, and vibrating 22 times in a minute, delicately suspended on an agate within a rim accurately graduated, was placed with its centre exactly over that of the copper-plate, and being accurately adjusted, so that the distance between the centre of the copper and that of the needle was such as I required for the observation, the copper was made to revolve rapidly (always as nearly as possible 12 times per second), and when the needle became stationary, the direction of its south end (being that most convenient for observation) was noted. This was done with the the copper revolving in both directions, "screw" and "unscrew." The direction of the south end of the needle was also observed before the rotation.

Distance	4·0 in.	3·5 in.	3·0 in.	2·5 in.	2·0 in.	
Screw...	1° 46′ W	3° 20′ W	6° 20′ W	14° 30′ W	29° 40′ W	Direction of south end of the needle.
Unscrew.	1 32 E	3 08 E	6 00 E	13 50 E	29 00 E	
Mean...	1 39	3 14	6 10	14 10	29 20	

On diminishing the distance to 1·5 inch, the needle revolved with the plate, and very shortly so rapidly, that it had the appearance of an entire circle.

After this I replaced the needle by others which were lighter, letting every thing else remain the same, that is, the distance still 1·5 inch.

	Needle weighing 42·5 grs.	Needle weighing 25·5 grains.
Screw	24° 40′ W	10° 30′ W
Unscrew.	25 20 E	10 40 E

(I should mention that the needles were not at all neutralized).

From the latter observations, it is evident that the effect produced depends upon the intensity of the magnetism in the needle employed; and this, I think, proves clearly that the effect arises from the magnetism induced in the copper from the needle itself.

If we suppose the tang. of the deviation to vary as $\frac{1}{(\text{dist.})^n}$, then θ and θ' being two deviations at the distances d and d', we shall have $n = \frac{\log.\tan.\theta' - \log.\tan.\theta}{\log. d - \log. d.}$.

Computing n from this, by a comparison of every two observations we have the following values of n:

When distance is measured from centre of copper.	When distance is measured from surface of copper.
5·04	4·37
4·60	3·93
4·62	3·88
4·29	3·51
4·20	3·60
4·45	3·64
4·10	3·31
4·65	3·80
4·07	3·23
3·59	2·78
Mean 4·361	Mean 3·605

If we suppose that the poles of the needle are urged by forces in the direction of the motion of the copper, which being constant in the copper, would affect the needle reciprocally as the square of the distance; then these forces in the copper being derived from the needle itself, we must suppose that their intensity will vary also reciprocally as the square of the distance; so that the force on the needle arising from this mutual action, would vary reciprocally as the fourth power of the distance. Taking the mean between the mean values of n above, when the distance is measured from the centre of the copper and from its surface, would give the value of n for an intermediate point 3·983, which is as near to 4, supposing that such ought to be the value, as we could expect the observations to give.

The next experiments which I made were with the view of determining the law of force as regards the distance, when magnets act upon a copper disc. For this purpose I made use of the suspending wire as a balance of torsion. The results which I have obtained in this manner give a much less rapid diminution of the force, as the distance increases, than appears to take place when a thick copper-plate acts upon a small magnet, as in the former experiments, which agrees with what you have mentioned as following from your results. The results obtained in the former case appear to indicate, that every particle in the copper urges the needle from the magnetic meridian with a force varying as $\frac{\text{vel. of particle}}{(\text{distance})^4}$, which law would arise from the magnetism in the needle developing the magnetism in the particles of copper, so that its intensity would vary as $\frac{1}{(\text{dist.})^2}$, and this magnetism again acting on the poles of the needle with a force varying as $\frac{1}{(\text{dist.})^2}$. Supposing this to be the case, if z is the distance of a lamina of copper from the plane of the needle, s the arc of a circle in this lamina at the distance r from the axis of rotation, R the radius of the copper cylinder, t its thickness, c the distance of its upper surface from the needle, and a the distance of the pole of the needle from its centre: then the whole force with which the cylinder urges the needle will be proportional to

$$\iiint \frac{r\, d s\, d r\, d z}{\{z^2 + (a - r)^2\}^2}$$

Although this may be integrable, the integral would be in so complicated a form, that it would be very ill suited for comparison with the results obtained from observation; but if we consider only the annulus of the copper immediately under the pole of the needle, which will be the most efficient part, we may readily make this comparison. For calling θ the deviation, we should have $\sin.\,\theta = \int \frac{d z}{z^4} \times$ const. or $\sin.\,\theta = \left(\frac{1}{c^3} - \frac{1}{(c+t)^3}\right)$ $\times$ const; and consequently $\frac{\sin.\,\theta}{\frac{1}{c^3} - \frac{1}{(c+t)^3}} =$ const.

From my experiments t being 1, I should obtain the following values of $\frac{\sin.\,\theta}{\frac{1}{c^3} - \frac{1}{(c+t)^3}}$

C	θ	$\frac{\sin.\,\theta}{\frac{1}{c^3} - \frac{1}{(c+t)^3}}$	
3·5	1° 39′	2·3316	Mean 2·505
3·0	3 14	2·6341	
2·5	6 10	2·6409	
2·0	14 10	2·8144	
1·5	29 20	2·1040	

Although there is a considerable difference in the numbers, especially the last, yet as the parts whose action is not considered have here the greatest effect, and all the observations are liable to errors arising from the difficulty of making the copper revolve with the same velocity in all cases, I think the agreement is sufficiently near to indicate that the copper acts as I have supposed. A thick copper ring would be best adapted for obtaining results for comparison; and when I have leisure I propose making use of one.

For the purpose of determining the law according to which magnets act upon a copper disc at different distances, I suspended, successively, two copper discs over the bar magnets placed horizontally by the side of each other, with their poles of the same name adjacent. The magnets were made to revolve until the torsion of the wire caused the disc to return in the contrary direction, when I considered that the force of torsion would be double the force with which the magnets urged the disc. The time in which this took place was noted, and also the degree of torsion. After this the magnets were made to revolve again with the same velocity, and the torsion noted where the disc remained stationary by the action of the opposite forces of torsion and of the magnets. This was done at several distances; and those distances, between the magnets and the disc ascertained very accurately. In the observations with the disc which I have named A, the magnets were made to revolve with two different velocities; one of nearly 12 revolutions per second, the other of nearly 24 revolutions per second; but with the disc C the magnets always revolved with the velocity 24 revolutions per second, as I found that I could keep more steadily to this velocity than to the other. The length of the suspending wire (No. 22) was the same in both cases 34·25 inches. The thickness of the magnets is one-fifth of an inch, so that I have added $\frac{1}{10}$ to the measured distances between the upper surface of the magnets and the copper, to reduce them to the distances between the plane of the copper and a horizontal plane passing through the axes of the magnets.

It is evident from these results, that the force with which the magnets urge the disc, as the distance increases, decreases much less rapidly than in the case of the copper-plate revolving. If we suppose it to vary as $\frac{1}{\text{dist.}^n}$, then calling c and c' two distances and T and T′ the corresponding torsions, which are equal to the forces of the magnets, $n = \frac{\log. T - \log. T'}{\log. c' - \log. c}$.

Comparing the preceding results, the several values of n will be,

	Disk A.	Disk C.
Values of n	1·723	1·285
	1·995	1·556
	2·087	1·864
	2·271	2·065
	2·436	2·118
	2·429	2·406
	2·658	2·614
	2·420	2·803
	2·831	2·998
	3·354	3·246

These differ too widely from each other for us to suppose that the force varies as any exact power of the distance; but the approximation is evidently towards the inverse square.

With regard to the forces with which different discs are urged at the same distance, they appear to be very accurately proportional to the weights of the discs when their distances from the magnets are small; but as the distances are increased, the forces appear to increase in a greater ratio than that of the weights of the discs.

Distance	·6	1·1	1·6	2·1	2·6	
$\frac{\text{Torsion}}{\text{Weight}} =$	1·372	·483	·194	·100	·049	Disk A.
$\frac{\text{Torsion}}{\text{Weight}}$	1·380	·633	·286	·134	·067	Disk B.

As it was only by a rough estimate, that I considered the velocity with which the magnets revolved under the disc A was double in one case of what it was in the other, I would not, from these observations, pretend to determine the ratio of the forces as depending upon the velocities, but I should have little doubt that they are proportional.

From these experiments it appears, that the time in which the disc begins to return, by the torsion of the wire, is the same at all distances; and from another experiment, it appeared to be independent of the velocity of rotation. This ought to be the case, the force accelerating the disc being constant; and the retarding force, the torsion, varying as the distance from a fixed point.

E. W. B.

Article III.

On the Heat of Friction. By T. Graham, MA.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Edinburgh, Sept.* 7, 1826.

It is generally allowed, that the heat extricated in friction is inexplicable upon the theory of the materiality of heat, as at present entertained. It would be easy to show that this heat does not arrive at the bodies rubbed together, by the ordinary and admissible methods of conduction or radiation, or, that no reduction of bulk takes place, or diminution of capacity for heat. Yet the materiality of heat is involved in the principal doctrines of chemistry, while the simplicity and easy application of the theory render its establishment exceedingly desirable. In these circumstances, an attempt to reconcile the substantial existence of heat with its appearance in friction, may not be unworthy of attention, even although the suppositions on which it is founded should be altogether novel; elucidating, as they do, other departments of science.

Heat is observed by us, either radiant in motion, and possessed of great velocity; or in union with matter, and capable of regaining this velocity.

Probably this velocity is necessary to its entering into bodies and uniting with them; at least we never observe heat do so without it. For, when communicated by radiation, this is evident; and in conduction, which in close contact supplies the place of radiation, it is evident that heat is communicated with a force. Indeed conduction may be reduced with considerable plausibility to an internal radiation.

It appears that this motive power, which is essential to the communication of heat and our perception of it, is really never annihilated. It disappears when heat passes into a body, but it is merely overpowered for a time, and not altogether lost; for upon reduction of temperature, the heat emanates from the body, evincing its pristine velocity. We may compare the state of the heat in union with matter to that of a bent spring, or a compressed elastic substance, the attraction of the matter for heat being the restraining force. Sensible heat, therefore, we never find destitute of this motive power, nor to lose it—at least heat is never so divested of it as to be incapable of resuming it.

These observations prepare us for the conception of heat in a different state from that in which it is generally supposed to exist. Let us suppose that the calorific principle is capable, likewise, of existing destitute of this motive power; and yet not in combination with matter, which this motive power seems necessary to effect. We may suppose it capable of existing in a state similar to our ordinary conceptions of the electric fluid. As a fluid, powerfully repelling its own particles, and attracting those of other matter, spread equally over the surfaces of all bodies, independently of their composition or temperature, without combining with these bodies, diffused (to borrow an illustration from chemistry) like a drop of oil upon the surface of water, without being in combination with it. To the matter of heat in this quiescent state, we shall, for the sake of convenience, give the name *superficial heat*, from its covering the superficies or surfaces of bodies. It is merely heat to which there has not been imparted that original velocity, upon which the characteristic properties of sensible heat depend. Superficial heat, it is evident, must be insensible. But project it with the necessary velocity, and you render it sensible. This might result from extraordinary accumulation of our idio-repulsive body, or its concentration upon a particular spot. Dissipation, by means of radiation, appears to be a natural effect of the repulsion between the particles of superficial heat, aggravated to a great degree.

Now in friction, circumstances are favourable to the conversion of superficial into sensible heat, in this manner. The surfaces of the bodies rubbed together are brought rapidly into exceedingly close contact, so that as surfaces they virtually cease to exist. From the violent approximation, the idio-repulsive power of the superficial heat investing both the surfaces, is powerfully exerted; so that a portion of the superficial heat is expelled as radiant heat, and impinges upon the rubbing surfaces. But more superficial heat is supplied from the earth; and as long as the friction is continued, superficial heat is converted into sensible, and the bodies become hotter and hotter. Hence the heat attending friction; and the reason why more heat is elicited, when the surfaces are smooth than when they are rough, their approach in the former case being more close, and the investing superficial heat more condensed.

In a course of experiments upon the heat produced in friction, M. Haldat attempted to insulate his apparatus for that purpose, by means of non-conductors of electricity. Upon reference, however, to his paper,* it will be found, that notwithstanding the body of the apparatus was electrically insulated with great care, yet the insulation of the machine and contrivance by which motion was conveyed to the rubbing surfaces, was overlooked. The result of this imperfect insulation was a diminution of one-third in the amount of heat evolved. New experiments upon this subject are very desirable.

The theory which we have applied to friction admits of very great extension. We may suggest that the phænomena of elec-

* Nicholson's Journal, vol. xxvi. p. 30.

tricity are caused by an accumulation, or a deficiency, of our superficial heat. That the electric fluid is really superficial heat, and convertible into sensible heat in the manner explained.

Hence we never perceive any thing which we can call the radiation of electricity. We never find, that one electrified body communicates any of its electricity to another body at a distance by this means. For it follows from the doctrines illustrated, that should ever electricity (superficial heat) emanate from bodies in this manner, it should be in the shape of radiant heat.

We scarcely need adduce instances, in which heat, in its sensible form, does attend the accumulation of electricity. When a powerful current of the electric fluid is concentrated, by being passed along a thin wire, the wire is heated to a great degree, so as to become strongly radiant. In this way, charcoal, or any other body, may be kept in the voltaic arc in a state of intense ignition. Here, from the great repulsive force that must attend such an accumulation of superficial heat, which will be much enhanced by the retardation of the passage of the fluid, occasioned by the imperfect conducting power of the substance, a large portion is expelled with the necessary velocity, and becomes thereby sensible heat. According to this theory, electrical light and heat are derived from the same source as the heat of friction; and in neither case is there any production or actual generation of these principles.

The simplicity of this theory is its chief recommendation. That heat, possessed of a substantial existence, should be found alone, uncombined with matter, and that this combination, of a most elementary kind, should, at all times, be brought about by the calorific principle impinging with force upon the material body, are not hard postulates. Most material substances, however strong their affinities for each other, require peculiarly favourable circumstances to enable these affinities to act, otherwise the bodies appear to a certain extent repulsive of each other. Moreover, when we attribute to the matter of heat diffused over the surfaces of bodies, an attraction for these substances which yet does not amount to the production of a combination, we are but extending to heat properties which all other material substances evince, in adhesion, capillary attraction, &c.

It is hoped likewise, that the theory of superficial heat is not chargeable with that barrenness and want of practical application, which generally characterize premature speculations upon abstract subjects. The knowledge of the existence of such an agent, of its influence in friction and electricity, and of its convertibility into sensible heat, affords a clue of no small importance to guide us in our researches. Its application in galvanism, we shall, perhaps, hereafter, have an opportunity of exhibiting.

Article IV.

A Chemical Essay on the Art of Baking Bread. By Hugh Colquhoun, MD.

(*Concluded from* p. 182.)

II. *Of certain Processes for introducing an Elastic Fluid into the System of Dough, without having recourse to the Panary Fermentation.*

It is to the use of the sesqui-carbonate of ammonia (the common subcarbonate) that the baker, in this department of his art, has most generally recourse; and, perhaps, also, with the surest success, as a means of duly gasifying his bread. When this salt is employed, it is almost always in a proportion varying between the quarter and the half of an ounce to the pound of flour. It is dissolved in the water which is to be employed in forming the dough, of which the proposed bread is to be made. As soon as the due proportion of flour is mixed with the water holding this salt in solution, and sufficiently kneaded into dough, the composition is ready for the oven; and whether baked immediately, or after a moderate interval, to suit the convenience of the manufacturer, the same light spongy bread is obtained. The heat of the oven operates directly in expanding the carbonate of ammonia into an elastic vapour. In its endeavours to escape, the pent air opens up and detaches from each other the compact particles of the dough; the whole mass is heaved up to a large increase of volume, and is maintained for some time in a highly dilated bulk, notwithstanding the constant escape of gas, which is forced out into the oven, by the continued energy of the elastic fluid, until it is almost entirely expelled from the bread. When the whole has nearly evaporated, the bread subsides a little; but it has already attained, through the continued heat, a degree of stiffness and dryness through all the parts of its texture, which prevents its shrinking back to nearly its former dimensions. It remains not only increased in bulk, but also light and porous.

But the structure of bread which has been thus prepared, and indeed of all bread in which a sudden formation and development of elastic fluid has been generated *within* the oven, differs remarkably, when examined, from that of a loaf which has been made after the preparatory fermentation by yeast. Bread which has been raised with carbonate of ammonia, is certainly porous, and the pores are numerous and excessively minute; but that which has been prepared from regularly fermented dough, is not so properly porous as spongy and vesicular. And the former kind of bread never presents any trace of that stratification of layers of vesicles, which is held so high in the estimation of the baker.

It is generally supposed, that after passing through the oven, the carbonate of ammonia has been so completely dissipated by the heat, as to leave no traces behind of its having ever been present in the bread, except a light tinge of yellow colour, and a slight, unpleasant flavour, which latter quality is easily disguised, in all confectionary, by the use of a little sugar. But in addition to these vestiges, a small portion of the salt itself almost constantly lurks in the substance of the bread; for it has very generally a strong odour of ammonia when taken fully baked from the oven; and, though for the most part it becomes inodorous when cold, yet on being again heated, the presence of ammonia is again indicated by the smell. It can only happen, however, in cases of the most extreme carelessness that any such quantity shall remain behind, as may tend either to affect sensibly the flavour of the bread, or to prove injurious to even a delicate constitution.

Nothing can be simpler in its operation, than the sesquicarbonate of ammonia, as to the mode in which the dough prepared with it becomes filled with a large supply of elastic fluid as soon as it is placed in the oven. Without dwelling any longer upon it, therefore, let us proceed to consider another mode that has been proposed for gasifying bread, and which, perhaps, derives its principal claim to regard from the circumstance, that it has gained the support of more than one chemist of eminence; for it has hardly yet been found of much practical efficacy, nor does it appear likely soon to become so.

The process alluded to, is that of impregnating the dough artificially with free carbonic acid gas, at the very commencement of the baking process, when the flour is originally mixed up with water; and it has been supposed that the carbonic acid thus introduced will have the effect, in consequence of the expansion which it suffers in the oven, of communicating a sufficient degree of vesicularity and lightness to the bread.

The possibility of expanding dough by saturating it in this manner with carbonic acid gas, has been long asserted by various authorities of a more or less questionable description; but Mr. Edlin may be said to have been the first person who brought it forward, in a formal manner, into general notice. In his Treatise on the Art of Bread-Making, p. 56, it is stated unqualifiedly by this author, as the result of repeated trials, that if warm recent dough and some flour be kneaded with a saturated aqueous solution of carbonic acid gas; and if the mass of dough thus prepared be placed in a warm situation for about half an hour, it will expand, exactly like dough in a state of regular fermentation; and that if it be now baked in the oven, it will yield an excellent light porous bread, not distinguishable in quality from that obtained with the assistance of yeast. He quotes also in support of this, certain accounts which have been published of the employment of various mineral springs in baking bread;

particularly those of Gonnesse, as used in the beautiful bread with which the inhabitants of Paris have long been extensively supplied from that town; of Selzer, in Germany; and of two others near Saratoga, in America, which, owing to their being naturally impregnated in a high degree with carbonic acid gas, serve to the surrounding district as a perfect substitute for the fermentation of yeast in the manufacture of bread. All of which facts, if strictly correct, certainly tended to establish the theory of Edlin, which was, that the activity of yeast in exciting the saccharine fermentation of dough, resides exclusively in the carbonic acid gas with which that liquid is always nearly saturated, when kept properly excluded from the open air.

There is another opinion deserving to be mentioned on this subject, which is pretty much to the same effect with Edlin's, and is said to be built on the respectable authority of Mr. Henry, of Manchester. This is stated in the Supplement to the Encyclopædia Britannica, under the article Baking, where it is related, as the result of certain experiments made by the gentleman just mentioned, "that if flour be kneaded into dough with water, saturated with carbonic acid gas, the dough rises as well, and the bread is as light and well-tasted as when it is baked with yeast." It is farther added by the author of the article, that if, instead of the ordinary dose of common salt, or muriate of soda, being mixed with the dough in the usual way, its constituents, soda (combined with carbonic acid, in the state of the common carbonate), and muriatic acid, in their due proportion, "be kneaded as rapidly as possible with the dough, it will rise immediately, fully as much, if not more, than dough mixed with yeast, and, when baked, will constitute a very light and excellent bread."

If these opinions were well founded, they certainly might often be of no small consequence to the baker, in saving him from the delay of the yeast-fermentation, and from much of the labour of kneading. But we find, on the one hand, however, standing directly opposed to them, the experiments of M. Vogel, who assures us that, contrary to what has been asserted with so much appearance of plausibility by Mr. Edlin, he was unable to obtain the slightest trace of real fermentation, in dough which had been prepared merely with a saturated aqueous solution of carbonic acid gas, instead of the customary mixture of yeast and water. He states further, that such dough, when baked, after having been kept in a warm situation during the usual time, afforded nothing better than a hard cake, which had no resemblance to ordinary bread. And he adds also as illustrative of the general necessity of providing a sufficient supply of disengaged elastic fluid within the dough, before baking it at all, that, when he made various attempts to form a well-raised vesicular loaf, within the oven, by mixing flour with carbonate of magnesia, or with zinc filings, and then kneading it into a paste by means of water acidulated with sulphuric acid, he always met with complete failure and disappointment.*

As it was a question certainly of considerable interest to ascertain how the truth lay between the conflicting statements which have just been detailed, and also of much practical importance, to prove the effect of carbonic acid gas introduced into bread without the aid of fermentation, it seemed a matter well worthy of attention to subject the point anew to the test of experiment. This inquiry implied, of course, a two-fold investigation. It was desirable, in the first place, to determine the practicability of obtaining a well-raised loaf, from dough formed by mixing flour with a saturated aqueous solution of carbonic acid gas. To resolve this point conclusively, it was necessary to try the effect of baking such dough, both when perfectly recent, and also after having been kept for some time, in order to ascertain whether the saturated solution might in this latter case be capable of exciting the saccharine fermentation, without the assistance of yeast. And, in the second place, it was important to decide whether the effects of the slow yeast-fermentation might be imitated, with reference to the lightness and porosity of the bread, by blending the dough intimately with an alkaline carbonate, and subsequently causing a sudden disengagement of carbonic acid gas within its substance by the addition of an acid. The results that were obtained seem to be conclusive on both branches of the research.

Four ounces of flour were made into dough with four cubic inches of a saturated aqueous solution of carbonic acid gas at the temperature of 51°. A second portion of dough was prepared by mixing two ounces of flour into a paste with two cubic inches of water, at the temperature of 80°, and immediately afterwards kneading it with two ounces additional of flour, and two cubic inches of carbonic acid gas. For the purpose of comparison, a third portion of dough was prepared, with four ounces of flour, and four cubic inches of a mixture of yeast and warm water at a temperature of about 70°. To each of these three masses of dough there were besides allowed 30 grains of common salt, added, according to the usual practice of the baker, with a view to the flavour of the bread. Immediately after their preparation, a portion (about one-fourth) was detached from each, and baked in the oven. The products of all the three trials were identically the same; being a compact, unvesicular bread, differing in no respect from what would have been obtained by treating in a similar manner a simple mixture of flour and water.

In order to promote the fermentative process, the remainders

* Journal de Pharmacie, vol. iii. p. 216.

of each of the doughs were set aside, according to the customary practice, for about six hours. Before the half of this period had elapsed, the dough prepared with yeast was in a state of strong fermentation, and had swelled out to fully thrice its original volume. On the contrary, the two other pieces of dough, throughout the whole course of the six hours, never exhibited the slightest appearances, either of fermentation or of expansion. Portions of each were now again detached; and after they had been kneaded and set aside for about half an hour, in a warm situation, with a view to permit a fresh accumulation of carbonic acid gas, they were then baked, as before. The bread formed out of the dough which had been fermented in the regular manner by means of yeast, was light and spongy, and possessed all the characters of ordinary bread; but that which was the product of the doughs prepared with a saturated aqueous solution of carbonic acid gas, was still as dense, tough, and unvesicular, as in the first trials that had been made, immediately after the intermixture of the flour with the water. There still remained a portion of each of the three original masses; these, after having been again abandoned to themselves in a warm situation, as before, for about twelve hours, were carefully examined; but even at the expiration of this period, those which had been prepared merely with flour and a solution of carbonic acid gas in water did not appear to have undergone any fermentation or expansion. The same series of experiments was afterwards repeated with no other variation than this, that brisk soda-water was substituted in the room of the original solution of carbonic acid gas. The results were identical in every respect with those which have been just detailed: it would be unnecessary, therefore, to enter into a more particular account of them.

The conclusions from all these experiments are, therefore, wholly inconsistent with the opinions of Edlin, and those attributed to Henry, and seem to be a convincing proof, both that carbonic acid gas is incapable of exciting the panary fermentation, and that it is impracticable, by the mere employment of a saturated aqueous solution of carbonic acid gas, to cause the dough to expand in the process of baking into a light, spongy bread.

The experiments made on the decomposition of an alkaline carbonate within the substance of the dough, afforded results rather more favourable to the views of Messrs. Edlin and Henry, although they at the same time proved decisively that this process by no means possesses the efficacy which is ascribed to it in the statements of these chemists. The carbonates which were selected for the purpose of these assays, were the sesquicarbonate of soda, and the common carbonate of magnesia, and due care was always taken to employ the acid and alkali in those proportions in which they pretty exactly saturated each other. The mode in which these experiments were conducted, was by first intimately blending the flour with the alkaline carbonate, in the state of a fine powder, and next making this mixture into dough with the requisite quantity of a water holding the acid in solution. And especial care was taken during the kneading to confine as large a quantity of gas as possible within the dough, in order to give the system a fair trial. The mixtures employed in these experiments were, respectively, in the four proportions following:

1.— 4 ounces flour.
42 grains sesqui-carbonate of soda.
90 grains dilute muriatic acid.

By previous experiment, this quantity of dilute acid had been ascertained to be requisite to saturate exactly 42 grains of sesqui-carbonate of soda.

2.— 4 ounces flour.
20 grains sesqui-carbonate of soda.
19 grains tartaric acid.

3.— 4 ounces flour.
30 grains carbonate of magnesia.
15 grains tartaric acid.

4.— 4 ounces flour.
60 grains carbonate of magnesia.
30 grains tartaric acid.

These several masses of dough, after being duly kneaded, were set aside for about 20 minutes, so as to afford full space for a sufficient reaction to ensue between the acid and the carbonate: they were then baked, in the usual manner, in an oven.

During the process of kneading the specimens of dough into loaves, they had all of them felt loose, light, and spongy, to an uncommon degree, and they were also vesicular and bulky when first introduced into the oven. These characters plainly indicated the sudden generation of a great volume of elastic fluid within the dough. Yet in every instance the bread formed from them proved doughy and sad, possessed but a few diminutive vesicles, and was never piled. Of all the varieties, that in which the flour had been mixed with sesqui-carbonate of soda and tartaric acid approached the nearest to a good loaf, and might have been termed light or porous when compared with bread made of unfermented dough. But even this specimen in point of true lightness and elastic vesicularity was decidedly inferior to our common loaf-bread.

Indeed it seems plain, from reflecting on the use and necessity of the present laborious process of kneading, that no loaf-bread can be well made by any of the extemporaneous systems above considered, because they are all inconsistent with the thorough kneading of the dough. It is this process which is found to render dough at once elastic enough to expand when carbonic acid gas is generated within it, and cohesive enough to confine this gas after it is generated. In reality, according to the present system, almost the whole gas which is used in the making of each loaf is actually generated within it, by a continuation of the regular panary fermentation, after all the processes of kneading have been finished. For the loaf, after having been weighed-out, kneaded, and shaped, is set aside until it expands gradually to a double bulk, preparatory to its entering the oven. But of course when dough has been artificially impregnated with carbonic acid gas under any of the methods lately considered, as the gas has not the slightest affinity for any one constituent of the dough, it is impossible to subject that substance to a thorough kneading, without literally squeezing and expelling out of it almost every particle of the air; and when this has once been fully accomplished, as it infallibly will be in the common process of thorough kneading, the internal supply of elastic fluid cannot be afterwards renewed, because the temporary cause which produced it is no longer in existence. Thus the baker who should attempt to use it, seems reduced to the hard alternative, of either abandoning the kneading process altogether, in which case he will never obtain a single *piled* or even well-raised vesicular loaf, or adhering to the kneading, in which case he will lose even the little benefit which the carbonic acid gas would otherwise have conferred, and obtain a bread, doughy, compact, and sad.

But although the water of acidulous mineral springs is incapable of being used by the baker with success in forming good ordinary bread, there is another manner in which he often employs the simple element as a means of procuring for him the desideratum of gasified bread with considerable effect; for its vapour, expanded in the oven, is often a useful agent in raising many kinds of bread. When the vapour of water is thus to be employed as the expanding agent, it is customary to give an adventitious degree of adhesion to the particles of the dough, by making it of thinner consistency than usual, and by mixing up with it some glutinous or gelatinous substance, as eggs, an aqueous solution of isinglass or gum, or any gelatinized amylaceous substance. It is by no means unfrequent, however, to add also a small proportion of carbonate of ammonia, in order to assist the vapour of water in acting as the expansive principle in the oven.

There is nothing very peculiar or remarkable in the general application of these means of expansion. But there is one instance of its use in forming a product, with which every one is familiar, and in which the address of the mechanic is so conspicuously shown, that it seems worthy of being particularly alluded to. It is in the manufacture of puff-paste that this ingenuity is displayed, and here it is probable that not only the vapour of simple water, but also that disengaged from heated butter may be called into operation.

The requisite quantity of dough is first of all made up in the usual manner with flour and water, and with a small quantity of butter in its composition. After being sufficiently kneaded, it is rolled out into a flat plate, and the whole of one surface is spread over with a thin coat of butter. When this has been done, the plate of dough is folded together, care being taken that the upper and under surfaces exactly correspond in size to each other, while there is of course one layer of butter between them. The baker now again rolls out this double plate of dough to cover as great a space as before, and the upper surface is again spread over with butter. The doubling of the double plate is now repeated, in a similar manner, when, of course, there are four layers of dough above each other, with a little butter intervening between each two layers, and keeping them separate. This alternate process of spreading out into a thin plate, and then doubling up first into two, next into four, next into eight folds, and so on, is repeated for about ten times, by the last of which the original mass of dough obviously consists of about a thousand thin laminæ, lying parallel above one another, and having a layer of butter interposed between each. When this is put into the oven, the elastic vapour disengaged from the water, and from the butter, insinuates itself between each of the thousand thin laminæ, and being prevented, in consequence of their tenacity, from escaping, causes them to exfoliate from each other, and thus ultimately swells up the mass into a puff; and when the baking is completed, the bread is found to be extremely light, and to consist of an aggregation of very thin membranes, no two of which are in a state of thorough coalescence, but on the contrary stand out distinct from each other, with even a pretty large volume of intervening air. From this mode of preparing the puff, it is plain that in each *plate* of dough, which has never been fermented, there is but little lightness or elasticity to be expected, since the gaseous fluids which distend and puff out all the plates between which they are confined, nevertheless do not penetrate thoroughly into the system of any one; and, accordingly, upon examining any of them by itself, it will always be found of a tough doughy consistence.

Such are a few of the methods which are either in daily use by the baker, or have been strongly recommended to him for the purpose of thoroughly gasifying his bread without having any recourse to the aid of fermentation. Some of them are not a little ingenious, but the rationale of each is so very simple in itself, and the kind of bread manufactured is of so little importance when compared with that of the common loaf-bread, that it has required but little time to discuss their merits. But there remains an extensive department of the baker's art, which has not yet been considered, and which on many accounts deserves to be carefully treated of before concluding this Essay. It is one of the most curious, and certainly also one of the most difficult processes, in regard to its rationale, of all those which occur in the bakehouse; and the result of an investigation into its nature seems to reflect considerable light upon many parts of the art of baking bread.

It is the mode of manufacturing that composition of flour and treacle, so commonly known by the name of gingerbread, which is now to be subjected to examination. The dough of this kind of bread cannot be fermented by means of yeast; every such attempt has proved fruitless, and even though, on several occasions, the presence of yeast may seem to excite appearances of fermentation in the dough, the gingerbread nevertheless which is baked from it always comes from the oven as solid, hard, and compact as a piece of wood.

Of the various striking peculiarities which mark this process, it is believed, no explanation has yet been offered. In the first attempt towards their exposition, if it be too much to anticipate that the views adopted shall be at once complete and satisfactory, at least it may be hoped that some advance is made towards an end so desirable.

The present manufacture of gingerbread is, generally speaking, carried on in the following manner: The ingredients are flour, treacle, butter, common potashes, and alum. After the butter is melted, and the potashes and alum are dissolved in a little warm water, these three ingredients, along with the treacle, are poured among the flour which is to form the basis of the bread. The whole is then thoroughly incorporated together, by mixture and kneading, into a stiff dough. Of these several constituents, the alum is found by the baker to be the least essential, although it is useful in having a decided tendency to make the bread lighter and crisper, and in accelerating the tardy period at which the dough is in the most advantageous condition for being baked into bread. For it is one of the most remarkable parts of the present system of manipulation, that gingerbread-dough, however thoroughly kneaded, almost invariably requires to stand over for the space of from three or four to eight or ten days, before it arrives at that state which is best adapted for its rising to the fullest extent, and becoming duly gasified in the oven. And experience has shown, that it may be allowed to stand over even for the period of several weeks, rather with advantage than loss in this respect. It is true, that, from causes not well understood by the baker, the dough of gingerbread becomes thus matured and ripe for the oven, on some occasions much more speedily than on others; but, in general, if the dough were fired at an earlier period than has just been mentioned, the baked bread would more or less resemble in compactness a piece of wood, in proportion to the time by which its baking had been prematurely hastened.

As the alum could be easily dispensed with by the baker, without at all materially affecting the rising of his bread in the oven, it was plain that it might be laid altogether out of view in the course of an investigation into the peculiarities of this process. And indeed that its presence could not have the effect of paralyzing the yeast-fermentation is a matter sufficiently plain from the well-known circumstance, that it is not unfrequently employed in baking common wheat-loaves, to render whiter the colour of inferior flour. It was, therefore, in the action either of the butter, or of the potashes, or of the treacle, or in the combined action of these upon each other, or upon some other ingredient in the flour, that the source of the uncommon results attending the preparation of gingerbread was to be traced. And from the experiments made, it seems to be clearly established, that the mutual action of the potashes and treacle upon each other is, the gasifying principle in the present process of gingerbread-making.

In order to ascertain in what this principle is situated, a mass of dough was made ready, from which *butter* was entirely excluded, but which differed in no other respect from common bakers' gingerbread-dough. After being allowed to stand over the usual time, it was baked in the oven, and, when taken from it, proved to be a well-raised gingerbread-loaf. There were next prepared several pieces of dough, having all the usual ingredients except the *carbonate of potash,* and it was found, that neither when baked immediately upon being made, nor afterwards at various intervals during several weeks, did the bread come from the oven otherwise than solid and compact, just such as common bread is, the dough of which has never been fermented. In the next place, two portions of dough were prepared, from both of which *treacle* was excluded; and in one of them its place was supplied by an equal weight of refined sugar dissolved in a minimum of hot water. But in neither case did the bread return in the least degree porous or vesicular from the oven; and exactly the same results were obtained, both when the dough was baked immediately after its preparation, and at successive intervals during the lapse of several weeks. From these experiments, the inference seemed to be clear, that the simultaneous presence of the treacle and of the carbonate of potash, and their mutual action, must be essential to the formation of good elastic gingerbread.

It was scarcely to be doubted that the action of the treacle upon the carbonate consists in evolving from it a quantity of carbonic acid gas. But in order to bring out this point more unequivocally, the substitution of carbonate of soda and of carbonate of magnesia for carbonate of potash, was tried, and it invariably turned out that the bread in these cases expanded just as well in the oven, as when an equivalent quantity of carbonate of potash had been employed. And when, on the contrary, in place of these substances, there was mixed up with the dough either caustic potash or caustic magnesia, the bread never expanded in the slightest degree in the process of baking, whether the dough was baked when recent, or after being kept a considerable time. From this it resulted, that the presence of an alkaline carbonate was clearly essential to the gasifying of the gingerbread-dough; and it seemed almost a necessary inference, that the rising of the bread during the baking is produced by carbonic acid gas, and that this gas is developed in consequence of some mutual action which takes place between the treacle and the alkaline carbonate.*

* The following is a note in detail of the variously-compounded doughs employed in these experiments, with a statement of the general results obtained.

1.—Flour	4 ounces.
Treacle	3 ounces.
Potashes	60 grains.

Rose well in the baking: not distinguishable in appearance from that made with ordinary gingerbread-dough.

2.—Flour	4 ounces.
Treacle	3 ounces.
Flour	4 ounces.
Treacle	3 ounces.
Butter	¼ ounce.

The bread was quite compact, hard, and might even be termed flinty.

3.—Flour	4 ounces.
Potashes	60 grains.
Flour	4 ounces.
Butter	¼ ounce.
Potashes	60 grains.
Flour	4 ounces.
Refined sugar	3 ounces.
Potashes	60 grains.
Flour	4 ounces.
Refined sugar	3 ounces.
Butter	¼ ounce.
Potashes	60 grains.

These four mixtures were made into dough with the requisite quantity of hot water; and portions of each mass of dough were baked in an oven, both immediately after its preparation, and at an interval of five days subsequently. In both instances, the results of all the four trials proved uniformly the same, and all alike unfavourable. The bread never exhibited the slightest indications of expansion, being quite compact and sad. It

It is not very easy to penetrate the mode in which the treacle thus acts upon the alkaline carbonate. By far the most probable cause seems to present itself in the probable existence of a certain portion of uncombined acid in treacle, which, entering into union with the alkali of the carbonate, releases a quantity of carbonic acid gas. That such an acid does, in a greater or less degree, always exist in treacle, seems proved by the fact, that of many specimens which were examined in the course of the experiments just mentioned, all possessed distinct traces of acidity, and to an extent sufficient to enable them to communicate a red colour to vegetable blues; but the amount of uncombined acid present in all these cases appeared to be very trifling, and it was difficult to ascribe to its sole agency the production of effects so striking. It cannot be doubted, however, that this uncombined acid must operate to a certain extent in producing a decomposition of the alkaline carbonate; and it may be conjectured also, that the superiority in the expanding power of *old* dough is occasioned either by the additional acidification of a small quantity of the treacle, to which it would be disposed during the keeping, by its state of mixture with the flour, or by the circumstance that the carbonic acid gas disengaged by the

had also a deep yellow colour, a sickening, and very unpleasant odour, and a nauseous taste; effects which were probably to be ascribed to some chemical action of the potashes upon the flour.

5.—Flour	4 ounces.
Treacle	3 ounces.
Butter	¼ ounce.
Common crystallized carbonate of soda	124 grains.
Flour	4 ounces.
Treacle	3 ounces.
Butter	¼ ounce.
Common carb. of magnesia.	60 grains.

The bread obtained in both of these trials possessed exactly the appearance and flavour of gingerbread prepared in the usual manner with potashes. And in these cases also, the expansive property of the doughs was decidedly improved by keeping, so that the results obtained by the substitution of either of these alkaline carbonates in the place of that more commonly used by the baker, were, in all respects, perfectly parallel to those which characterize his ordinary process.

6.—Flour	4 ounces.
Treacle	3 ounces.
Butter	¼ ounce.
Caustic potash	40 grains.
Flour	4 ounces.
Treacle	3 ounces.
Butter	¼ ounce.
Calcined magnesia.	25 grains.

The two doughs, compounded as above, were assayed both immediately after their preparation, and at three successive intervals, a week being allowed to elapse between each experiment. But in none of the trials was the bread possessed of the least vesicularity: it was as compact as if it had been prepared without the exhibition of any alkali whatever.

uncombined acid contained originally in the treacle, has thereby more leisure to penetrate into the system of the dough, and to produce a more complete separation of its particles. And it may be mentioned as a circumstance in support of this explanation, that though the period of keeping requisite in the preparation of gingerbread-dough is generally from five to ten days, it is sometimes materially less, and that without the manufacturer's being able to assign any cause for the variation. But this, of course, might be readily accounted for on the supposition, that treacle generally contains a variable quantity of uncombined acid, and that this ingredient is the true agent in developing carbonic acid gas within the dough, by its action upon the alkaline carbonate. Upon the whole, therefore, it seems not improbable, that the mutual action of the potashes and treacle, out of which results the gasifying of gingerbread-dough, consists in the treacle containing a little uncombined acid, which, uniting with the potashes, sets carbonic acid gas at liberty, and thereby renders gingerbread light and elastic.

In the course of performing these experiments, the details of which have been subjoined in a note to p. 278, and the results of which have led to the above conclusions, it was impossible not to be impressed with a sense of the inconveniences that often arise to the baker from the delay occurring in the process, and of the injury which may not unfrequently accrue to the consumer, from the deleterious nature of one of the ingredients which is essential in the present system. This is the carbonate of potash, which it is always necessary to use in such a quantity as gives a distinct disagreeable alkaline flavour to the bread, whenever this is not disguised by mixture with some aromatic ingredient. Nor can there be much doubt, that if gingerbread, as now made, were eaten in any considerable quantity, it would prove injurious to any delicate constitution, in consequence merely of the large amount which it contains of this alkaline substance; and if such a consequence as this may follow, even in the case of the most carefully-baked gingerbread, it is plain that in the hands of a careless or unskilful mechanic, the employment of such an ingredient is extremely inconvenient. It appeared, therefore, to be a very desirable matter to procure some substitute, which, while it formed an equally well-raised bread, might save the delay of the baker, be less disagreeable to the palate, and quite harmless to the constitution; and, accordingly, it was not without experiencing very considerable pleasure, that after having made various trials, a mode of compounding and preparing the dough was actually found out, which appears to unite all these advantages. The substitute which proved the most perfectly successful was a mixture of common carbonate of magnesia and tartaric acid; and in mixing up the dough, there will be found a practical expediency in employing a considerably larger quantity of the alkaline carbonate than is strictly necessary to neutralize the acid. But the shortest and simplest mode of explaining how this process is found to work, will be to quote an example of its use: the following statement is therefore submitted, of the mode of preparing what will be found in practice to be a very good dough, particularly for that kind of thin gingerbread, well-known under the name of parliament cakes.

Take a pound of flour, a quarter of an ounce of carbonate of magnesia, and one-eighth of an ounce of tartaric acid: let the butter, treacle, and aromatic ingredients, be added in the same manner as at present. The use of alum will not be found of any advantage, and will be better dispensed with; as it is in itself an unwholesome substance, and any good effects which it can produce are in all probability completely supplied by the tartaric acid. It is necessary that the alkali employed, the magnesia, should be uniformly diffused throughout the whole mass of the dough, an object which will be always best effected by intermixing it, bruised to an impalpable powder, with the flour, previously to the addition of any other ingredient. After these have been well mingled, dissolve the tartaric acid in a small quantity of water, and, having melted the butter, pour it, the treacle, and the acid solution, into the mixture of flour and magnesia. Let the whole be incorporated into a mass of dough by kneading, and then set aside the dough, for a period varying from half an hour to an hour. It is then fit for being baked into bread. The delay of at least half an hour has the practical benefit of giving full time for the acid to act upon the alkaline carbonate, so as to render the dough loose and short, or, as a baker would say, to bring it into a state of strong fermentation. The dough prepared in this manner, should never be kept longer than two or three hours before being put into the oven, from which it will in due time be obtained, in the state of a light, spongy, pleasant bread.

By the method now proposed, not only is the delay avoided which is so inconvenient in the system at present practised, but there is no unpleasant flavour discernible even when the bread is not at all confected with sugar or spices, and there is no ingredient in it at all injurious to the most delicate constitution. The expense of making gingerbread in the manner above stated is a trifle greater than that in which carbonate of potash is employed. The difference, however, is so extremely small, as scarcely to make any sensible addition to the price of even the most ordinary kinds of gingerbread.*

* Tartaric acid may now be purchased at 4*s*. 6*d*., and carbonate of magnesia at 1*s*. 4*d*. per pound: it is obvious, therefore, that the cost of the quantity of these materials necessary to convert seven pounds of flour into gingerbread, will amount to only about 5*d*.

I find a good gingerbread, which proves extremely agreeable to the palate, to be

As a matter of curiosity, the mode now mentioned as having been successfully employed in rapidly gasifying the dough of gingerbread, was tried upon the dough of plain bread, to see whether it might there have the effect of proving a complete substitute for the common yeast-fermentation. The result was in the highest degree favourable, and the biscuit which had been the subject of the experiment was as light and pleasant as if it had been prepared upon the fermentation-system. This experiment was more, however, a matter of curiosity, as already mentioned, than of much practical utility; for although the present process of the baker is slow and somewhat tedious, yet it is also cheap, and simple, and sure; and it is only in those comparatively rare cases, when, either from want of yeast, or from deficiency of time, it would be impossible to have recourse to fermentation, that the use of the process here suggested might be a matter of some advantage to the manufacturer. It should not be omitted to mention, that the presence of the neutral salt, the tartrate of magnesia, necessarily formed by that union of the acid and alkali which furnishes the supply of carbonic acid gas, was found to impart to the simple bread a slightly vapid taste; but the addition of a very trifling quantity of sugar is quite sufficient to conceal this. There is subjoined in a note, the process followed in preparing biscuit with these ingredients, which is indeed so simple, as scarcely to require any particular explanation.*

made in the form of the thin *parliament cakes*, from a dough composed of the following ingredients:

Flour	1 pound.
Treacle	½ pound.
Raw sugar	¼ pound.
Butter	2 ounces.
Carbonate of magnesia	¼ ounce.
Tartaric acid	⅛ ounce.
Ginger	⅛ ounce.
Cinnamon	⅛ ounce.
Nutmeg	1 ounce.

This composition differs from the one which is employed in the preparation of ordinary parliament cakes, not only in regard to the use of a substitute for potash, but also in containing a larger proportion of butter, and in subtracting one-third of the treacle, the place of which is supplied by an equal weight of sugar. These alterations, I think, materially improve the relish of the bread as a piece of confectionary; but they are rather unfavourable to its full expansion in the process of baking.

* The dough was prepared with the following ingredients:

Flour	1 pound.
Butter	3 ounces.
Sugar	2 ounces.
Carbonate of magnesia	¼ ounce.
Tartaric acid	⅛ ounce.

The flour, previously mixed with the pulverized carbonate of magnesia, the sugar, and the butter, was made into dough with *cold* water, holding the tartaric acid in solution; and the dough, after its preparation, was set aside for about half an hour, in order to allow the acid to act to the necessary extent upon the carbonate of magnesia. It was then rolled out into biscuits, and baked, in the usual manner, in the oven.

Such is the mode of preparing a well-raised gingerbread, which, out of a variety of trials, seemed by far the most successful, and the most advantageous, both to the manufacturer and to the consumer. But there are various other ingredients which may be effectually enough employed for the same end, and some of which deserve to be mentioned, as tending to throw light upon the rationale of the process, which is, in principle, the same in every case.

Thus, for example, the bitartrate of potash, instead of tartaric acid, may be employed, along with the carbonate of magnesia. When this substance is used, there is a degree of sourness, just perceptible to the palate, in the flavour of the bread, and which it is not impossible that some tastes might regard in the light of an improvement. Another method, and quite an effectual one, is to use the carbonate of magnesia alone, without any acid admixture, only to an extent doubly or trebly greater than when it is conjoined with tartaric acid; and the result will be that the dough becomes as speedily fit for being baked, and yields as spongy and as light a bread. If again the carbonate of potash, along with an equivalent quantity of sulphuric acid, be intermixed with dough, it has the effect of fitting it for the oven as speedily as any of the other methods above-mentioned. But it communicates to the bread a taste decidedly bitter.*

* The following is a note of the proportions of the several compounds employed in these trials, and of the most interesting characters of each experiment.

1.—Flour	4 ounces.
Treacle	3 ounces.
Butter	½ ounce.
Carbonate of ammonia	60 grains.

Rose nearly, but not quite so well, as ordinary gingerbread dough. Flavour, decidedly more pleasant than that of gingerbread, prepared with potashes. The cake had also a darker colour on the outer surface than is the case with ordinary gingerbread.

2.—Flour	4 ounces.
Treacle	3 ounces.
Butter	½ ounce.
Cream of tartar	160 grains.
Carbonate of ammonia	53 grains.

Expansion, similar in extent with that in last experiment. But it happened here, probably because the tartrate of ammonia had undergone decomposition during the process of baking, that the bread had an excessive flavour of sourness or bitterness.

3.—Flour	4 ounces.
Treacle	3 ounces.
Butter	½ ounce.
Cream of tartar	160 grains.
Carbonate of magnesia	60 grains.

Flour	4 ounces.
Treacle	3 ounces.
Butter	½ ounce.
Cream of tartar	160 grains.
Common crystallized carbonate of soda	120 grains.

There have now been detailed a few of the modes, in which much delay and trouble may be saved to the manufacturer, by his employing the mutual action of an acid and of an alkaline carbonate, which shall take a speedy effect, and generate a due supply of carbonic acid gas within the dough, after it is made. It is only, however, the first mentioned substitute for the present noxious ingredients of carbonate of potash and alum which can

The bread obtained in both these trials proved extremely porous and light; fully equal, in this respect, to the best ordinary gingerbread. It had also a taste slightly, but not disagreeably sour.

4.—	Flour	4 ounces.
	Treacle	3 ounces.
	Butter	¼ ounce.
	Sulphuric acid	24 grains.
	Carbonate of potash (common)	40 grains.

The dough was prepared, in the usual manner, with treacle and butter, and the sulphuric acid was sufficiently diluted with water: it was next hastily kneaded with the carbonate of potash, which had been previously brought to the state of an impalpable powder, and was then baked in the oven. The expansion was sufficiently favourable, though rather less than is the case with ordinary dough; but the bread had a bitterish taste, which was quite disagreeable.

5.—	Flour	4 ounces.
	Treacle	3 ounces.
	Butter	¼ ounce.
	Carbonate of magnesia	60 grains.

The object of this trial was to compare the efficacy of carbonate of magnesia with that of carbonate of potash, as an agent in producing expansion. The dough was made into bread, both immediately upon its preparation, and also after an interval of several days. The expansion in both cases was considerable, especially in the latter, but still rather inferior in this respect to that of ordinary gingerbread.

6.—	Flour	4 ounces.
	Treacle	4 ounces.
	Butter	¼ ounce.
	Carbonate of magnesia	¼ ounce.
	Flour	4 ounces.
	Treacle	5 ounces.
	Butter	¼ ounce.
	Carbonate of magnesia	½ ounce.

These mixtures were made with a view to ascertain the extent to which the appearance and taste of the bread would be influenced, by introducing into the system of the dough a very large excess of carbonate of magnesia. The doughs in both cases rose remarkably well in the process of baking; fully as well indeed as ordinary gingerbread dough. The bread also ate very pleasantly; and even in the second mixture, the presence of the magnesia was scarcely, if at all, discernible.

Upon this account, it may prove a matter not unworthy of the physician's attention, how far the method of baking up magnesia along with parliament cakes, may not be an advisable mode of exhibiting that medicine. The quantity used in the last-mentioned preparation was rather more than one-twentieth of the whole compound, yet its presence was far from very palpable in the cake, and it seemed that even had it been used in a considerably greater proportion, it would not have made the bread unpalatable. A dose of that medicine might thus have been easily conveyed into the stomach, to no inconsiderable extent, especially in the case of children, who are well known to have often an extreme repugnance to the vapid flavour of magnesia, which besides always feels unpleasantly gritty when taken by itself.

be considered the best adapted for practice, both as being in itself the most convenient and simple, and also as possessing the advantage of containing no element in the least degree prejudicial to health. The others have been quoted principally to show the true nature of the action which takes place in gingerbread-dough, in the present tardy process, as well as in the other methods. There is yet another process of gasification, however, which should be mentioned, as it is occasionally resorted to in the manufacture of this kind of bread, as well as in that of many others, and with the same complete success. This is by using the sesqui-carbonate of ammonia, whose efficacy and the nature of whose action in expanding all kinds of dough in the process of baking, have already come under our notice. If this salt be employed in the proportion of half an ounce to the pound of flour, the dough containing it, however recent when baked, will always form itself into a good light bread; and it is on this account a very common practice with the baker to add a certain quantity of it to his ordinary gingerbread-dough, when he is under the necessity of employing it in its recent state, before it has been sufficiently matured by keeping. The bread so formed is found to possess an extremely agreeable flavour, and it is also marked by the peculiarity of having the upper surface unusually dark and glossy. In this bread, also, as in others similarly aërified, there remains always a certain trace of ammonia, which would be plainly perceptible, but for the confections which disguise it.

We have now reviewed some of the principal processes which are employed in the prosecution of his art by the modern baker of bread. After briefly discussing the mechanical details of the most common process of bread-making, our earliest and our principal attention was directed to the fermentative process in dough, as being by far the most curious and important in the eye both of the chemist and of the mechanic. It is hoped that the true subject of the action of fermentation has been clearly pointed out, and also that the nature of the secondary supervening decomposition, and that of the acids which it generates, have been satisfactorily explained. In inquiring into the nature of these acids, it was impossible not to have our attention directed to the numerous inconveniences resulting from the occurrence of sour dough, one of the greatest annoyances to which both the manufacturer and consumer of bread, embracing a circle which is co-extensive with civilized society, are exposed. And as the remedy here lies fortunately on the very surface, and is at least as obvious as the evil is great, a few pages were devoted to explain its application. This indeed is a matter too simple in itself to have required so much time, were it not that its extreme practical importance, coupled with the singular fact of its being yet a practical novelty, made it necessary to verify what is in truth one of the most elementary principles of chemistry, by the details of experiments, in which its operation was demonstrated in those very cases where it is required by the baker.

We next proceeded to consider the methods now in use for introducing a gaseous body into bread, independently of the employment of fermentation; and the rationale of the manner in which a few of the most remarkable among these are found to work, then formed a subject of investigation. But the most interesting and difficult process connected with this branch of the art, was certainly that of the manufacture of gingerbread, with which the Essay has concluded. The different experiments which have been detailed in regard to this important department of the art of bread-baking, it is conceived, will tend to throw considerable light upon the chemistry of the process, of which there does not seem to have been yet offered any complete solution.

If the perusal of this Essay should have the effect of inducing the man of science to direct his knowledge, more to the practical exposition and improvement of so important an art as that of baking bread, or if it should convey to the mechanic any hints which he will find really useful to assist him in his business, or to guide him in his inquiries, the author will then have obtained all the success which he ventures to anticipate.

Article V.

Report of the Length of the Pendulum at the Equator. By John Goldingham, Esq. FRS. *From Experiments and Observations made on an Expedition fitted out under his Direction, from the Observatory at Madras: by Order of the Madras Government in the Year* 1821. *Together with a Deduction of the Figure of the Earth, by combining the Equator, Madras, and London Experiments. Also the Geographical Situation of different Places seen on the Expedition.**

In order to give the greatest possible value to the experiments, which have lately been completed for ascertaining the length of the pendulum, and thence the figure of the earth, it was essential to have the length of the pendulum at the equator

* A few copies of this Report, published at Madras in 1824, having been received in England, but no account of the expedition, or of the results obtained by it, having yet been given in any scientific journal, or other readily accessible medium of information, our readers will not be displeased at being presented with Mr. Goldingham's report, verbatim, together with a selection of tables, &c. containing the results. Some parts of the Report may perhaps appear tedious; but we think it due to Mr. G. and his observers, to afford those who are interested in the inquiry the full means of estimating the value of the results.—*Edit.*

deduced from experiments made with the same care, and with the same accurate description of apparatus, as had been made use of in other parts of the world.

With a view to this inquiry, and to make the pendulum apparatus, which I had received from England, as useful as possible in this quarter of the globe, I turned my attention towards discovering a good position upon the equator, where to have the requisite experiments made; being certain, at the same time, that the government of Madras,* whose aid I proposed to ask, would order every preparation to be completed, and every thing to be done, calculated to insure the success of this interesting inquiry. It appeared to me that some one of the islands off the western coast of Sumatra would furnish a good position, on which to make the necessary experiments; and, though personally unknown to Sir Stamford Raffles, the present Lieutenant-Governor of Bencoolen, I felt no hesitation in addressing him upon the subject of affording us assistance.

The reply of Sir S. Raffles was such as I had anticipated—it was dated the 5th of September, 1821; and the subjoined are extracts from it.

"I have been much gratified by the receipt of your communication, and lose no time in assuring you of my most zealous co-operation and assistance. There are several small islands to the southward of Nattal, many parts of which must lie on the Equator, and I think you may have a choice of situation.

"I lament to say that our geographical knowledge, even of the coast, is most defective, with the exception of Acheen Head, and Bencoolen, and, perhaps, Flat Point to the south, neither the latitude or longitude of any place along the whole line of coasts is laid down correctly; I am endeavouring to supply the deficiency, but I am in great want of scientific assistance, and get on but slowly. The Poggy islands and Pulo Nias appear to be laid down not less than 40 miles to the westward of their true situation, and no two charts agree in the longitude of the main island to the northward of Bencoolen. I mention this, in order that your observations may commence at Madras, and be brought on direct to Bencoolen.

"That the party should come to Bencoolen, in the first instance, is indispensable also, on many accounts; it will enable me personally to communicate with your assistant, to give him such advice and information as may be necessary, and to supply the stores, necessaries, and personal guard, that may be required. The season for going from Bencoolen to the north-

* At the time the expedition was proposed and ordered, the government consisted of The Hon. Major-General Sir Thomas Munro, KCB. Governor.

His Excellency General Sir Alexander Campbell, Bart. and KCB. Commander in Chief; the Hon. George Stratton, Esq.; and the Hon. William Thackeray, Esq. Members of the Council.

Edward Wood, Esq. being Chief Secretary.

ward will commence in April, and the sooner the party arrive here after January the better; it is to be wished they should be at their station in March, in which case, should the weather prove unfavourable, and their object not to be otherwise accomplished, they will have the September change before them, and the command of both seasons.

"We have a regular station at Nattal and Ayer Bonghey, and also on Pulo Panjong, which lies directly off the latter, and would seem to be on the Equator. Mount Ophir is stated by Mr. Nairne to be five miles to the north of the Line; but in the maps it is as much to the south.

"I would recommend your sending a *double* establishment of practical men, to supply casualties by sickness or otherwise; and I think your party should be as complete in every respect as practicable.

"They had better bring tents (light ones of course); and a few pioneers with their axes would be found useful.

"A watchmaker, carpenter, and brazier, can be furnished from Bencoolen—that is to say, of the ordinary class.

"I should be very happy to find, on the arrival of your assistant, that we can mutually agree upon some general plan, which shall include not only the experiments on the pendulum, but the general geography of the island, to which my attention has long been directed; at all events, he will be under the necessity of taking observations for laying down some positions with accuracy, and these will be so many points gained in geography.

"The party should be most complete in instruments—they must not rely on finding any, even the commonest, here."

Another letter from Sir Stamford Raffles, of the 17th of Dec. following, expresses the same warm interest respecting the projected expedition, with the same promises of assistance.

Immediately upon the receipt of the first of these letters, I waited upon the Governor Sir Thomas Munro, and stated the object I had in view, mentioning, at the same time, the promised assistance, and the information I had obtained, from Sir Stamford Raffles. As I had expected, Sir Thomas Munro was pleased to enter most fully into the objects of the plan; and I immediately addressed a public letter to the government upon the subject, of which the subjoined are extracts.

"It is known to the Honourable the Governor in Council, that some of the principal governments in Europe have lately employed eminent scientific men in determining the length of the pendulum, for establishing a standard-measure, and for ascertaining the figure of the earth; and in this interesting inquiry, that Great Britain has stood in the foremost rank, sparing no expense that appeared in any the remotest degree necessary, for obtaining the most accurate results.

"In a former communication, I had the honour to state, that

I had written to the gentleman employed by the British government to superintend the operations in England, who was a friend of mine, to procure for me, and send out, an apparatus for measuring the length of the pendulum, similar to that used by himself; and that he had most readily done so; the use which has been made of this apparatus here is fully known to the Honourable the Governor in Council.

"In order to compare and combine the observations for the length of the pendulum taken in different latitudes, in a manner to render such observations of the greatest value, it would be most desirable to have accurate observations taken at the Equator, and a very favourable opportunity for obtaining such observations now presents itself.

"A part of the island of Sumatra, which is under the influence of the Honourable Company's government, is crossed by the Equator, and offers a most eligible station for these operations; added to which, Sir Stamford Raffles, the Lieutenant-Governor of Bencoolen, is anxious, as he informs me, to afford his most 'zealous co-operation and assistance.'

"I therefore beg to submit for the consideration of the Honourable the Governor in Council, that persons to be properly instructed in the use of the pendulum apparatus, and qualified to make all the necessary observations, may be sent to Sumatra, under my direction, to collect, and transmit to the Observatory as collected, the valuable and interesting data requisite for finding the length of the pendulum at the Equator, the more difficult part of the calculations may be made, and the conclusions drawn here, by myself.

"Messrs. Peter Lawrence and John Robinson, who were brought up at the Honourable Company's Surveying School, formerly under my superintendence, would be able, with some instruction, to perform this duty; the former was many years under Colonel Lambton, the superintendent of the grand trigonometrical operations carrying on in this part of the world; the latter was originally qualified to act as assistant at the Observatory, and teacher at the Surveying School; but during my absence in England, he was detached to the districts, and is now I understand employed in the Tank department at Chittoor; but as another Surveyor is there, I have little doubt he can be spared for this service, which may be considered only of a temporary nature. The monthly salary of these assistants might not exceed 175 rupees. A trusty and steady conductor of stores to look after the property, and a sub-conductor of the same character under him, with a proportion of good Lascars, would be necessary; together with tents, including one for an Observatory.

"The pendulum apparatus we have; a clock can be spared for a time from the Observatory for this occasion, with one chronometer; the loan of two other chronometers may possibly be obtained, or two may be purchased. Sextants and artificial horizons may also be taken from the Observatory, with a telescope and a portable transit instrument. A barometer and some smaller articles must be purchased, and levelling and measuring rods be made up. Also the loan of such surveying instruments as may be required, can be obtained from one or more of the public offices, where such are in store. By this arrangement, the expense will be as limited as possible; and I have no doubt, that future and not inconsiderable expense will be saved, by the geographical information which will be collected during the operations, and which, being extremely required by Sir T. S. Raffles, the assistants may be instructed to omit no opportunity of obtaining: points must necessarily be established, and that in the most accurate manner, from which those employed in the ordinary operations of surveying, may, at all times, take a departure. Sir T. S. Raffles, in a communication to me, observes, 'I lament to say that our geographical knowledge, even of the coast, is most defective; with the exception of Acheen Head, and Bencoolen, and, perhaps, Flat Point to the south, neither the latitude or longitude of any place along the whole line of coast, is laid down correctly. I am endeavouring to supply the deficiency, but I am in great want of scientific assistance, and get on but slowly. The Poggy Islands and Pulo Nias appear to be laid down not less than 40 miles to the westward of their true situation; and no two charts agree in the longitude of the main island to the northward of Bencoolen. I mention this in order that your observations may commence at Madras, and be brought on direct to Bencoolen.' The sooner, Sir S. Raffles observes, the party are at Bencoolen 'after January the better;' and opportunities, it is probable, may occur, for sending them in a vessel bound to that place.

"Should the Honourable the Governor in Council be pleased to direct these operations to be performed, I can prepare the assistants for the execution of the duty, and frame detailed instructions for their guidance. A list of the articles required for the expedition can also be submitted for approval."

This letter was dated the 6th of November, 1821, and a most gratifying reply was received to it on the 16th of the same month, of which the subjoined are extracts.

"I am directed to acknowledge the receipt of your letter of the 8th inst. and to state, that the Honourable the Governor in Council, fully sensible of the importance which is to be attached to the observations you are desirous should be made at the Equator, will be happy to afford every possible assistance in the prosecution of researches which are calculated to prove so eminently valuable to general science.

"Instructions will accordingly be immediately issued for the two persons, Messrs. John Robinson and P. Lawrence, whom you consider to be calculated to conduct the desired inquiry, to be placed under your superintendence, in order that they might receive that instruction which it is your intention to give them, to insure the correctness of the observations which they will be intrusted to make. Orders will also be given for the selection of a conductor and sub-conductor, whose characters for steadiness render them fit to take charge of the property; and when you shall have stated the number of Lascars, and the extent and description of the establishment of tents that it may be necessary to assign to the party, directions will also be communicated for their being furnished with them.

"With respect to the number and nature of the instruments and other articles that it may be necessary to provide on the occasion, the Honourable the Governor in Council will be prepared to sanction your indent for all such as are to be procured from the public stores, and feeling confident that you will confine the expense to be incurred within as narrow limits as may be practicable, he will authorise any expenditure which you may make in the provision of those which can only be supplied by purchase.

"The Marine Board will be instructed to report, whenever an opportunity may offer for the departure of the party for Bencoolen."

The preparations were immediately commenced at the Observatory, and completed about the close of the year; the additional instruments were obtained, either from offices where such were in store, or purchased; the difficulty and trouble in a country like this, attendant upon fitting out an expedition of this description, and in having delicate instruments properly packed, may readily be imagined: * these difficulties, however, were successfully overcome; and the two assistants, Messrs. Robinson and Lawrence, were instructed and practised in the duty they had to perform. I expected that an officer with a guard would have been ordered by Sir Stamford Raffles to go with the party from Bencoolen; but it having been suggested to me, that it might be better to dispatch an officer from hence, and concurring in this suggestion, I addressed the Secretary to Government in the Public Department, upon the subject, under date the 28th of Dec. stating,

That it having been suggested to me, if an officer were sent with the party going to the Equator, it might be of service to him, and that difficulties might possibly occur, which persons, merely scientific, could not so readily overcome, I begged he would be pleased to submit for the consideration of the Honour-

* The correspondence which this involved, and which was of some bulk, has been omitted, as tending unnecessarily to increase the length of the Report.

able the Governor in Council, whether it might not be desirable to have this addition to the party.—That we had as much science engaged, I imagined, as could be required; it was, therefore, not *essential* that any gentleman who might go, should have had a regular scientific education—a general knowledge of the object of the expedition merely, with the habit of overcoming difficulties which occur on service in the field, and a power of command to keep all proceeding with it in due order, would be sufficient for the purpose."

The Honourable the Governor in Council was pleased to adopt this suggestion, and to appoint Captain Crisp, of the army of this Presidency, for the service. That officer, after he was appointed, also attending at the Observatory, to obtain information as to the mode of using the pendulum apparatus.

The following is a list of the instruments and articles sent with the Expedition:—

One astronomical clock, gridiron-pendulum.

One pendulum of experiment, with frame for suspending it; arc of vibration, and small telescope.

Two thermometers for ditto.

Three pocket chronometers, viz. Nos. 391 and 397, by Arnold; and a gold one, by Earnshaw.

Three sextants, one with a stand.

Two artificial horizons with glass covers; quicksilver for the same.

One large telescope by Dollond, with stand, for eclipses of the satellites of Jupiter.

One portable transit instrument, with circular wooden stand.

One theodolite and stand; one circumferentor and stand; one azimuth compass; quilted wax-cloth covers for these instruments.

One hand telescope, by Dollond.

One barometer.

One large perambulator.

One brass chain, 100 feet.

One hygrometer.

Two levelling staves, 10 feet each, with vanes.

One plank for fixing the clock, six feet seven inches long, by one foot four inches broad, and two and a half inches thick, eight plugs and screws for fixing the same to the pillar.

Ten circular plugs for fixing the frame for suspending the pendulum of experiment.

One triangular stand for the barometer.

One stand for the arc for measuring the vibrations.

One stand for the small telescope.

A canvass Observatory—camp-table, and stools.

Two magnifying glasses.

One set of (four) stained glasses.
One case of mathematical instruments.
One parallel ruler.
One gunter's scale.
One colour box, indian ink, and brushes.
Nautical Almanacs for 1822 and 1823.
Two Madras Almanacs for 1823.
Book of requisite tables.
Norie's Nautical Tables.
Refined oil for the clock and pendulum.
Bamboos and flags for signals.
Steps to mount to the frame of the pendulum.
Black paper for the ball of the clock-pendulum.
Hair powder for paste.
Two chamois-leather skins for cleaning the glasses of the instruments.

One office tent,
Two subalterns' tents,
Two necessary tents,
Two baggage tents,
One Lascar's tent,
} With storm-ropes, pins, &c. complete.

One large magazine tarpaulin.
With hatchets, bill-hooks, pickaxes, plumb-lines, rope and line of different descriptions, cleets, nails, screw-drivers, drills, and sundry smaller tools and articles.
Stationary, an ample supply, including blank books.
Ten dozen of wax-candles.
Forty measures of oil; candlesticks, lamps, and lanthorns.
Also a proper supply of medicines, and directions for using the same.

These instruments and articles were carefully packed in 25 cases, and arrived quite uninjured at the ultimate destination of the Expedition.

The following instructions, approved by the government, were furnished by me for the guidance of the party. I was the more particular in these instructions, as the service was new to those going upon it, and to prevent, as far as possible, any interruption, from a want of the requisite information.

Instructions.

"The primary object of the Expedition is to make the requisite experiments and observations for ascertaining the length of the pendulum at the Equator, to combine with observations which have been made at Madras, and in other parts of the earth: with this, other objects are not to be allowed to interfere; though opportunities will doubtless occur on the passage to Bencoolen, and from that place to the Equator, and on the return voyage to Madras, for obtaining much valuable nautical information; and no such opportunities should be allowed to escape.

"The party will first proceed to Bencoolen, where Sir Stamford Raffles has promised to furnish any additional assistance that may be required—a vessel to proceed to the Equator, a guard, materials (bricks and lime), and a bricklayer or two to build a pillar, to which to fix the clock; two carpenters; and provisions; comprise the principal assistance wanted. A watch-maker to put up the clock may be required; but it will be better, if possible, to do without one.

"On arriving at Bencoolen, observations should be taken for the time, for the purpose of obtaining the difference of longitude between Madras and that place: these observations should be continued for the rates of the chronometers, and a fresh departure taken from Bencoolen, for the longitude of the place where the experiments with the pendulum are to be made. Observations should also be taken for the latitude of Bencoolen, the variation of the compass, and, if possible, the rise and fall of the tide, with the time of high-water at the full and change.

"In selecting a station at the Equator, care should be taken that it be as far as possible out of the sphere of the attraction of mountains, and close to the sea—a small healthy island, at a moderate distance from the main land, would be a most desirable place for a station. There is an island (*Saka*) on the Equator; this, however, is a long way out; and unless there be a post on Po. Pinjin, it may be at an inconvenient distance from the Sumatran shore: some healthy island may probably be met with nearer the coast.

"Having found the Equator by observation, the pillar is to be erected upon it.* The face should be perfectly flat and upright, and should front the opposite part of the horizon to that the wind of the season blows from; if it be the south-west monsoon, for example, the face of the pillar should be to the north-east; while the pillar is building, the plugs of wood for fixing the frame, which is to support the pendulum of experiment, should be inserted, exactly in the proper places, as well as the plugs for fixing the plank to which the clock is to be attached: small pieces of wood should also be placed in the projecting part of the pillar on which the clock, and the stand for the arc for measuring the vibrations of the pendulum of experiment, are to rest; to those pieces of wood is to be screwed the stand just mentioned. When the pillar shall have properly settled, and

* The pillar to be commenced as early as practicable, and to be kept from rain by a tarpaulin, the object being to have it as dry as possible before the clock and frame are put up. It is not to be plastered, as that would keep it wet a very long time—strong cement that will soon set, to be made use of, if to be obtained—as little water as possible to be used in mixing the chunam, and laying the bricks.

become sufficiently dry, the canvass Observatory may be placed over it, the door fronting the face of the pillar, and the face of the pillar about two feet behind the ridge pole; the whole of the pillar to be nearer the left side of the tent than the right side. The plank should be properly secured, and perpendicular to the horizon. The plank being firm, the clock may be screwed to it, which should be done in the most secure manner. The pendulum to be kept every way as at the Observatory, and the works of the clock the same; the arc for measuring the vibration will be a guide in placing the pendulum, the point of which should stand 1 division to the right of 0, and 0·25 of an inch in front of the arc: by having the face of the pillar upright and flat in the first instance, the plugs so placed in it that the plank shall also be upright and firm against the face of the pillar, the clock will be easily and firmly screwed in the position mentioned. The projecting part of the pillar on which the bottom of the clock-case stands should be perfectly level. The pendulum of the clock is first to be put up, and then the works.* If the black paper and disc should have been rubbed off, the ball of the pendulum is to be covered again, and a fresh disc put upon the centre of the ball. The clock should have been going 24 hours at least before you begin to ascertain its rate; get it to as small a rate as you can without losing too much time.

"The frame for supporting the pendulum of experiment may now be secured. At the Observatory, I was obliged to let the back of the frame 3½ inches in the wall; the surface of the plugs having been let in beyond the surface of the wall exactly that quantity; but the clock was then placed against the wall; here, there will be a plank, which is exactly 2¼ inches in thickness, so that the surface of the plugs should be let in only 1¼ inch, and the front of the pillar, where the frame is to be placed, cut away to that depth. Care should be taken that the frame is put up at first, so that the top of it shall be as nearly level as possible; and at such an height, that when the small frame with the agates is also put up, the point of the pendulum shall just come to the divisions of the arc for measuring the vibrations; and also, that the middle of the black slip at the lower part of the pendulum shall be as nearly as possible opposite the centre of the disc.

"The large frame being levelled † by the spirit levels, put up the smaller frame containing the agates. Let this be most accurately levelled; screw up the Ys, and put up the pendulum of experiment, with the knife-edges in the Ys, having first wiped

* Minute written instructions were also furnished for putting up the works and pendulum of the clock, to make the party independent of assistance in that part of the operations.

† Lead is placed between the parts of this frame; so that with the purchase of the lever, it may be flattened, and the frame thus brought into level; it should, however, be made as nearly level as possible by the workmen when putting it up.

the knife-edges with a piece of cloth, or flannel, saturated with fine oil. This should be done before the observations are commenced, and after finishing them, the knife-edges are always to rest in the Ys, when the pendulum is not used, but to be let down on the agates, when the experiments are making.

"The small telescope is to be placed at such a distance from the pendulum, that the sides of the black slip at the bottom of the pendulum shall just be embraced by the sides of the perpendicular opening in the diaphragm; large pickets with flat tops may be driven into the ground at the proper distance, and the bottom of the triangular stand supporting the telescope be screwed to these, the telescope being kept at the same height as it is in the Observatory, or a brick foundation may be laid with pickets let in: the telescope must be slid horizontally, until the left edge of the diaphragm becomes a tangent to the edge of the disc on the same side; and a mark must be made to show the position of the telescope to the left; move the telescope to the right, until the other side of the diaphragm is a tangent to the part of the disc on the right; here make a mark also, bisect the distance of the two marks, and place the telescope half way between.

"The following is the mode of making the experiments:—The barometer being up, and the thermometer hung near the middle of the pendulum, lower the knife-edges gently upon the agates; adjust the perpendicular sides of the diaphragm most accurately to the edges of the black slip, by the screws on the brass part of the support; then bring the 0 on the arc, to coincide exactly with the angular point of the pendulum. With the forefinger of the right hand upon the edge of the black slip of the pendulum, bring the point to about 1·3 upon the arc for measuring the vibrations, and on the left of 0; *an instant* before the pendulum of the clock comes to its greatest height on the same side, withdraw the finger horizontally; take the height of the thermometer and barometer, which register; look through the telescope, an assistant counting the clock, note the second and parts of a second, when the disc is completely hid behind the black slip of the pendulum; also the instant when it re-appears; let the exact time, minutes, seconds, and tenths, of both be registered, and the mean taken. Immediately after the second observation, (the re-appearance of the edge of the disc) take the arc of vibration very carefully, by seeing how much the pendulum vibrates on each side of 0, which also register. In about eleven or twelve minutes, the disc will again coincide with the pendulum; the times of the same appearances as above must be noted and registered; and thus proceed until five mean coincidences are obtained (as in the form). The thermometer being observed at the beginning, at the third and fifth coincidences, and the barometer at the beginning and end of each set. The pendulum should now be gently stopped, by putting the hands at the sides of the weight, and a new set of experiments commenced as above; perhaps eight sets, four in the morning, and four in the afternoon, may be taken in a day: the names of the observers to be noted; and each person's observations to be kept distinct. The forenoon sets being done, screw up the Ys, so that the knife-edges may rest on them; lift the pendulum gently up, and wipe the knife-edges with the oiled cloth or flannel, and place it again with great care in the Ys, covering the whole with a cloth to keep off dust and damp. Before the afternoon's observations, wipe the knife-edges, and lower them upon the agates very carefully; should there not be light enough in the Observatory-tent, the top may be opened; but great care should be taken to exclude wind from the pendulum during the time of making the experiments; on this account, the face of the pillar and the opening are placed on the side opposite to the prevailing wind.

"Three distinct sets of observations, of 120 or 130 observations in each set, should be taken if possible. The frame with the agates to be examined and levelled between each set, and the other points of adjustment also to be examined.

"During the time of making the experiments, the rate of the clock must be correctly ascertained, both by the stars and the sun, and all the observations carefully registered. A small pillar may be built for the transit instrument, sufficiently to the right in the Observatory, not to obstruct the view of the pendulum through the telescope, but so as to take advantage of the opening in the top of the Observatory for taking the transits.

"Meridian altitudes of the sun, if not too high for the artificial horizon, and of stars on each side of the zenith, should be observed; the more numerous these observations are the better, as a result to the nearest second is required.

"The longitudes must be found by the chronometers, and eclipses of the satellites of Jupiter.

"The nearer the place of observation is to the sea the better; the height of the pendulum above the sea must be found by levelling; also the rise and fall of the tide; the time of high water on full and change; the variation of the compass; the bearings and distances of the different mountains, the extent and elevation nearly, and if known, to note what they are composed of; also the bearings and distances of islands, and parts of the coast; and particularly the nature of the soil, of the place on which the experiments are made; whether sandy, or mould, or rocky; if either of the two former, whether to any depth.

"Every opportunity is to be taken for finding the bearings and distances of places and points along the coasts which may be in sight, with the difference of latitude and difference of longitude; also for obtaining soundings, as well as the set of the currents, as shown by the chronometers and meridian observations, to lay the foundation for a nautical survey, and for the improvement of nautical science; the variation of the compass should also be found as often as possible; the log-line should be accurately marked, so as to give correct distances for bases, and for finding the strength of the currents.

" The details of all the observations, experiments, and results, to be sent to the Observatory, to Mr. Goldingham the astronomer, by every opportunity.

" Journals noting every circumstance that may be thought useful or interesting should be kept: both Robinson and Lawrence may each also be directed to keep a journal.

" *Memorandum of the Points to be attended to.*

" The frame with the agates to be accurately levelled. In taking the pendulum of experiment out of the case, the greatest care should be observed not to bend it; the knife-edges to be wiped with an oiled cloth at the beginning and at the end of the experiments; the knife-edges always to rest in the Ys, when the pendulum is not in use, but to be let down on the agates previously to making the observations.

" The point of the pendulum of experiment not to be placed beyond 1·35 on the arc of vibration before the pendulum is set going; so that the arc of vibration at the first time may not be much more, nor much less, than 1·3.

" The clock to be firmly secured, and quite upright: the frame for suspending the pendulum of experiment also to be firmly secured; marks have been made by means of the arc on the back of the clock-case to show the position the pendulum ought to be brought to.

" Should the clock go too slow, screw up, *or shorten*, the pendulum as many divisions of the nut at the bottom of it as the clock loses seconds in 24 hours.* Should it go too fast, *lengthen* the pendulum, as above, by means of the nut.

" The clock to be wound up once a week.

" The chronometers to be wound up at the same hour every day; one person to be specially appointed to do this.

" In finding the latitude, take stars on each side of the zenith.

" Equal altitudes the best mode of finding the time, though in uncertain weather, it will be as well to take single altitudes also, before nine o'clock, a. m. or after three, p. m.

" On the days of experiment, it is absolutely necessary to have the rate of the clock; this may be found both by the sun, and with the transit instrument by the stars.

" In taking bearings on ship board, use the azimuth compass: allow for currents in making a base of the ship's run by the log for a given time.

* The value of these divisions is not exactly a second, perhaps; but it will be found after two or three trials.

" In taking the meridian altitude of a star, compute the exact time by the chronometer when the star passes; set the index to the double meridian altitude nearly; and take the same at the instant, when, by the chronometer, the star is on the meridian.

" Of the sextant telescopes, always use that with the largest magnifying power, placing the wires parallel to the face of the instrument, and observing in the centre of the telescope. Screw the telescope well in towards the face of the instrument for observations of the stars; for the sun, the middle of the object-glass of the telescope should be placed opposite the middle of the unsilvered part of the horizon-glass. Find the error of sextant at the times when it is used. The horizontal diameter of the sun should be taken for this purpose, if the sun be low.

Form for registering the Experiments with the Pendulum.

Apr. 2, a. m.—Rate of the clock. Hygrometer. Barometer. Inches.

By the sun. By the stars. 14·8 dry. Beginning 30·126 End ..... 30·134

Temperature.	Disc hid.		Disc re-appeared.		Arc of vibration.	Remarks.
81·95	41′	16·5″	41′	25″	1·38	
	53	12·5	53	21	1·29	
82·5	5	10	5	19·5	1·18	
	17	10	17	9·5	1·08	
82·47	29	8·5	22	22·5	1·00	

The above is from the experiments made at the Observatory, before the Expedition sailed.

Nothing was denied that a liberal and enlightened government in this distant quarter of the globe could cause to be supplied; and the Expedition now only waited the arrival of a Company's cruiser, two of which had been placed for a particular service, under the orders of his Excellency Sir Henry Blackwood, the Admiral of the Station, and one of these was to have been spared, for a short time, to carry the party to Bencoolen.

About the beginning of March, finding the cruiser did not arrive,* and that a good opportunity offered for sending the Expedition on the ship Morning Star, I addressed government, stating, that as the season for going to the northward from Bencoolen had commenced, it would be most desirable if the party could proceed to that place without further delay, and as there was now some doubt whether the expected cruiser would call here in time, or even at all, I proposed for the consideration of the Honourable the Governor in Council, that the Expedition should proceed on the ship before-mentioned—this was ordered

* It is fortunate we did not wait longer for her, as she proceeded direct to Bengal.

immediately, and the party* embarked on the Morning Star on the 13th of March, the instruments and baggage having been sent on board the day before.

The following brief Abstract of the Proceedings is founded upon the Diaries kept at the Time.

After a passage of thirty-four days, the Morning Star arrived in Bencoolen Roads. On the 18th of April, the party landed at Fort Malbro' with the instruments and baggage. Capt. Crisp and his family were invited by Sir Stamford Raffles to reside at the Government-House. The observers Messrs. Robinson and Lawrence, and the conductor and sub-conductor, were accommodated with rooms in the lower story of the old Government-House until the 1st of May, when they removed into tents about 200 feet north-east of the house. The Lascars obtained their provisions from the public stores, paying for the same; and the Europeans their's from the Bazar. On the 20th of April, Messrs. Robinson and Lawrence commenced the observations, according to the instructions, which were continued during the stay of the party at Bencoolen. On the 5th of May, at about ten minutes past two, p. m. a violent and alarming shock of an earthquake was felt; it lasted some time, the motion was considerable, attended by a noise, like that of the rushing of a strong wind; the old Government-House shook extremely, but does not appear to have sustained much damage: some other shocks were felt in the course of the afternoon, but not so violent; the weather was clear, except to the northward over the mountains, and a fresh north-west wind prevailed at the time. On the 16th of May, Capt. Crisp, with Messrs. Robinson and Lawrence, and the conductors, proceeded to Rat Island, with the view of ascertaining its position. In the afternoon, the party returned to Bencoolen, with the exception of Messrs. R. and L. who remained, and took some meridian altitudes of stars in the night, and bearings and angles of prominent points next morning. In the afternoon they returned to Fort Marlbro', and on the following day recommenced their observations. On the 24th of May, from a station on the turret of Fort Marlbro', about 50 feet above the level of the sea, Capt. Crisp and Mr. Lawrence took bearings and angles of remarkable points on the lofty ranges of mountains in the interior of Sumatra. On the 31st, Captain Crisp and the observers proceeded to Pulo Bay, nine or ten miles south-east of Fort Marlbro', where they arrived in the afternoon; here a base was measured, and some bearings and angles of objects in the interior and prominent points of the range of mountains taken; in the evening of the 1st of June, they returned to Bencoolen.

* The party consisted of Capt. Crisp, in command; Messrs. John Robinson and Peter Lawrence, observers; Mr. Hamilton, conductor of stores; Mr. Flannigan, sub-conductor; one second tindal, and nine Lascars.

On the 12th of June, Captain Crisp ordered the party to be in readiness to embark on the brig Eleanor, the property of the master-attendant of Bencoolen, but engaged, it was understood, for the expedition—being too small for the party, another but a smaller vessel, belonging to the same owner, was also engaged. On the 13th, Messrs. Robinson and Lawrence, with two Lascars, embarked on the Eleanor, taking some of the smaller instruments, and having in view to discover an island at the equator suited to the purpose of making the experiments upon; the conductor and sub-conductor also embarked on the smaller vessel. The cabin of the Eleanor was found in possession of an officer and his family proceeding, it was understood, to Nattal, and the hold was nearly filled with bricks and lime. On the 17th of June, the vessels sailed from Bencoolen roads for Nattal; on the 20th a. m. they passed Pulo Brinjen, and another small island south-west of the Poggy Islands. At night a very hard and continued squall came on, accompanied by thunder, lightning, and heavy rain; the party were obliged to take refuge in the hold, where, with the hatches on, they were in danger of suffocation—the wind adverse, and extremely violent, drove the vessel so far to leeward, that they were obliged to return to Bencoolen, where they arrived on the 23d, and on the following day, the smaller vessel returned with the conductors, who had suffered a good deal by the violence of the weather, in consequence of the state of their health, and had the greater part of their baggage rendered useless. On the 28th, Captain Crisp came on board, and ordered the instruments and baggage to be landed, and taken to Fort Marlbro'. 29th, Messrs. Robinson and Lawrence returned to their encampment, and recommenced observing—having chronometers belonging to Sir S. Raffles and Captain Patterson of the Honourable Company's ship Canning—besides those with the expedition, to ascertain the rates of. Most of the party were attacked with fever during their stay at Bencoolen.

On the 21st of July, the whole of the party, with the instruments, tents, baggage, &c. embarked on the Honourable Company's ship Canning, Captain Patterson; and on the 23d, they left Bencoolen roads for Tappanooly, where the ship arrived on the 8th of August, and the party landed on the 9th, to the great relief of the observors and conductors of stores, who, the diaries state, had fared on board in a way they had by no means expected.

The tents were pitched on the island of Tappanooly, near the house of the Resident, Mr. Prince. On the 11th, the observations were commenced on a small rocky height at the southern extremity of the island, near the Flag-staff. On the 15th, Mr. Robinson and the two conductors, with Lascars, embarked on the Eleanor, (which had reached Tappanooly before the Canning) for Pulo Panjong, where they arrived on the 21st. On the 29th of August, there was an earthquake on shore, which was felt by those of the party who were afloat—the vessel appearing to receive a severe shock. September 5th, Captain Crisp, with Mr. Lawrence, proceeded to the island of Pulo Bauka, near the north-east extremity of Tappanooly bay, and afterwards to the remarkable island, called the Sugar-loaf Peak; sights for the longitude, and bearings, and angles were taken; and they returned to Tappanooly, about sun-set. 9th, Mr. Lawrence proceeded for Battoo Barroor Point, for the purpose of laying down its position, and the prominent points of the Mansellar Islands, together with some points of Tappanooly bay. The brig Eleanor returned on the 11th from Pulo Panjong. 12th, all the baggage was sent on board the brig, and the remainder of the party embarked at sun-set for Pulo Panjong, where they arrived on the 16th: observations were commenced by Mr. Lawrence on the following day at a station near the Resident's house. On the 18th, the Eleanor sailed for Bencoolen for another supply of materials for the pillar. September 19th, Mr. Lawrence proceeded, with Captain Crisp, to Pulo Telloor, where observations for the latitude and longitude were taken, and also angles of the adjacent islands, and that part on the coast between points Kurboyee and Lubwaun Looloo—returned late at night. Another excursion made on the 23d, to Pulo Pahgaugo, for the purpose of laying down its position; and it was intended to have proceeded thence to the other islands near the equator, but the winds and weather having been very unfavourable, they were obliged to return on the the 25th to Pulo Panjong. On the 26th, they ventured out again for Nattal, but returned the following day. On the 29th, at night, Captain Crisp in one boat, and Mr. Lawrence in another, sailed for Nattal, and proceeded together as far as Pulo Tamang, where, early on the 2d of October, the boats separated, the one with Captain Crisp proceeding round the west side of the island, and the other taking a direct course for Nattal—the weather and winds being unfavourable, the latter strong with a heavy sea—the boat with Mr. Lawrence returned to Pulo Tamang—and on the 3d sailed again, but was obliged to proceed to the Sumatran shore for provisions, and anchored in the river at Patamm.—Next day, the 4th, having heard no intelligence of Captain Crisp, Mr. Lawrence wrote to the Resident, and received a reply late on the 5th, stating that Captain Crisp had not arrived at Nattal, and advising Mr. Lawrence to return to Pulo Panjong—the weather very unfavourable at the time for vessels of any description to proceed to the northward. At sun-rise on the 6th, they sailed, however, for Pulo Panjong, and fortunately arrived in the afternoon, Captain Crisp having also returned. On the 4th, at 10 a. m. the weather being cloudy at the time, a shock of an earthquake was felt, which is stated to have given the earth an undulatory motion for some minutes—another (but a slighter) shock was felt shortly afterwards; these shocks appeared to come from the east, in the direction of Mount Ophir. On the 6th, at night, two vessels, one with Captain Crisp, and the other with Mr. Lawrence on board, sailed for Nattal, and arrived late at night on the 7th. On the 8th and 9th, observations made. On the 10th, in the morning, the position of Nattal Hill was laid down. In the evening they left Nattal on the packet-boat for Pulo Pinnee, arrived at sun-set on the 11th, and anchored close to a coral-reef, the water on which being too shallow, they proceeded to the land, the south-east part of the island, in the small boat, where observations were taken for the latitude and longitude; they returned in the evening to the packet-boat, and sailed for Pulo Panjong, where they arrived on the 15th of October. Early on the 23d, the conductor and sub-conductor, with a great part of the baggage, embarked on the packet-boat, and Mr. Lawrence on the small boat, for Pulo Pinnee; but, owing to adverse winds, and bad weather, were obliged to return on the 26th to Pulo Panjong. The Padres, a sect of Musselman fanatics, being expected to attack our possessions on this part of the coast, Captain Crisp was appointed to a command by Sir Stamford Raffles. On the 31st, that officer and his family embarked on a brig for Nattal. Observations for the rates of chronometers, and for the latitude and longitude, were commenced, and sketches of the coast made. The Eleanor returned from Bencoolen on the 5th of November. On the 17th of November, Captain Crisp arrived in a small boat from Nattal. On the 19th, Captain Crisp in one boat, and Mr. Lawrence and the conductors in another, sailed for Pulo Pinnee; the night having been very dark, the boats parted company, and the latter went on past Pulo Pahgaugo as far as Tooleechemanah Point—the wind being strong, with a heavy sea, they were obliged to make best of their way back to Pulo Panjong. At sun-rise, made another attempt to proceed to Pulo Pinnee, and arrived at the south-east extremity on the 23d—landed and commenced cutting down trees, and clearing a place for the tents. On the 26th, the brig Eleanor, having left Pulo Panjong on the 21st, arrived with Captain Crisp and Mr. Robinson, the greater part of the instruments, tents, and baggage; the brig anchored about five miles off shore. In the course of the day, Captain Crisp, accompanied by the sub-conductor, came on shore in a boat, and afterwards returned to the brig, with the intention of sending the instruments, and all the other articles, on shore in the morning; but there having been very heavy squalls from the north-west at night, during which the only two boats they had were lost, it became impossible to land any thing, and the Eleanor was, therefore, got under weigh for Pulo Panjong, and did not return until the 7th of December, having touched at Nattal. On the 8th, Mr. Robinson and the sub-conductor landed with a part of the baggage; and by the 9th, in the evening, all the articles were on shore, and the tents up. On the 10th, Captain Crisp came on shore; they observed for the latitude, and found the south-east point of the island nearly five minutes north of the Equator. On the 11th, Captain Crisp sailed for Pulo Panjong, leaving directions for Mr. Lawrence and the conductor to proceed to a small island to the south-east, and to examine if it was suited to the purpose we required.* Early on the 16th, Mr. Lawrence proceeded to the island, with the requisite instruments, and a baggage-tent, also three Lascars, and landed about three o'clock in the afternoon. The conductor having been sick was not able to accompany the party. On the 17th, they examined the island, found it 11 feet above the level of the sea, composed of sand seated upon a foundation of coral, in length 365 feet, and breadth 200 feet, distant from the main land about 10 leagues, with a good landing-place, and suitable in every respect for making the experiments upon; having dug a hole to the depth of seven feet, they found very good water, and in abundance—there being no other islands in the vicinity nearer to the Equator than this. On the 18th, Mr. Lawrence returned off Pulo Pinnee, and on the following day, Mr. Robinson proceeded, with a part of the baggage, for the small island, which is called Gaunsah Lout, where he landed; Mr. Lawrence and the commander of the brig sailing at the same time in a smaller boat, which, having been found too deeply laden, and bad weather coming on, they returned, and did not reach the small island until the following day. The instruments and baggage were got to the island by the 7th of January, and the Observatory was put up.

(To be continued.)

Article VI.

On the Consolidation of the Strata of the Earth. By Sir James Hall, Bart. F.R.S. Lond. and Edin.†

The public attention, animated by scientific controversy, has of late years been much directed to Geological subjects; and

* It appears from a letter of Captain Crisp's, which was despatched by a circuitous route, and which I did not receive until the end of last September, [1823] although written many months before, that, at this time, his health had suffered very materially by the labour and exposure attendant upon going about, and in an open boat, in search of a proper station for making the experiments upon; some of his family were also ill at the time at Nattal.

† From the Edinburgh Philosophical Transactions, vol. x. part ii.

the certainty of many important facts has in consequence been ascertained beyond dispute, which were formerly unknown, or at least involved in such obscurity, that no person could have ventured to assert them, without being charged with extravagance. But though, no doubt, many branches of this science still remain to be investigated, such inquiries may now be said to have acquired a considerable degree of consistency and interest, from the substantial basis upon which they have been found to rest.

Thus, in the present day, it is universally admitted, that a great part, I believe, in point of bulk, by far the greatest part, of the solid rock which constitutes the external mass of our globe, is stratified: that these strata, or at least a considerable portion of them, have at one period consisted of a loose assemblage of sand and gravel, broken from rocks of still higher antiquity: that these fragments are infinitely various in quality, in bulk, and in form; some retaining their original sharpness, others rounded and polished by agitation in the water: that these beds alternate with others of limestone, composed, in a great measure, of the shells of sea-fish, which shells are also occasionally scattered through the other strata. So that on the whole, it seems to be sscertained to the satisfaction of all parties in geology, that the strata,—those, at least, of later formation, have once constituted collections of incoherent parts. And it is further admitted, that these beds have undergone various remarkable changes, some chemical, some mechanical.

The chemical changes consist in the consolidation of these loose assemblages into their present state of rock, passing, in that transition, through boundless varieties, in point of flexibility and toughness, and occasional brittleness. The mechanical revolutions are no less remarkable, principally in the change of the strata to their present contorted shape, and elevated position, often many thousand feet above the surface of the sea; though there is full reason to believe that they all once lay in a horizontal position at its bottom.

I have said that the greatest part of the crust of our habitable globe seems unquestionably to be stratified, and produced from detritus or fragmented materials. The other portion, though probably the least in bulk, is, generally, the most conspicuous, owing to its durability, elevation, and picturesque beauty. This kind of rock is contrasted with the former class, particularly in its negative qualities; in being, according to some geologists, altogether devoid of stratification in the general mass, and entirely free from component fragments; the whole being made up of crystalline forms, moulded upon each other, in obedience to certain chemical laws.

This crystalline rock, as the Society are well aware, abounds in the neighbourhood of Edinburgh, in Arthur's Seat, Salisbury

Craigs, and in Corstorphine Hill. It is decidedly posterior to the stratified class, of which it penetrates the crevices at all angles, in the form of dykes or veins, like stucco cast in a mould; frequently also lodging between the strata in vast shapeless masses.

As the rock in question never fails to preserve this quality of universal and perfect crystallization, I heartily concur with Dr. Hope in bestowing upon it the general name of *Crystallite*, under which are comprehended all substances of this kind, including not only Whinstone and Basalt, but also Porphyry, Granite, and Sienite of every description.

The solid mass of our globe, then, in so far as it is naturally exposed to our view, or has been penetrated by the labours of the miner, would appear, (with the exception of some streams which have flowed from Vesuvius, Lipari, and other volcanoes, in which the rock possesses a glassy structure), to be comprehended under these two classes, Aggregates and Crystallites.

The whole of these rocks, of both classes, furnish, at every turn, proofs of their having undergone revolutions of the utmost magnitude; and much ingenuity has been exerted, in endeavouring to trace these changes to some consistent and rational system. But of all the active powers of nature, one only has occurred to me as capable of affording a solution, in any degree satisfactory of the phænomena,—I mean the power of internal heat, which, in all ages, and in various countries, has made its appearance at the surface of the earth, not unfrequently from under the ocean, and which still, in our own days, gives occasional proofs of its unabated activity.

To ascertain the reality and sufficiency of this agent, and to trace the volcanic fire to its source, with tolerable probability, is, doubtless, an object of great interest and curiosity; but it has always appeared to me, that the progress of geology was retarded by a premature anxiety to enter into such investigations.

Taking it for granted, however, as, indeed, no one can dispute, that there frequently do arise violent exertions of heat from under the bed of our ocean, Dr. Hutton held that this might furnish a rational and sufficient theory of the earth, without entering into any inquiry as to the origin of that heat; and admitting that there are many geological facts which cannot be accounted for by such a fire as that of Vesuvius, now acting at the surface, in free communication with the air, he contended that the case may be very different, where that same cause acts at the bottom of a deep sea, and under various modifying circumstances, by which its operation could not fail to be influenced.

This, indeed, constitutes the essence of the Huttonian Theory, which I learned principally in conversation with its illustrious author; and which, since his death, I have taken every means of submitting to a variety of chemical tests; being for ever on the watch for such natural scenes as might illustrate these principles, as well as for opportunities of making experiments, to determine whether such modifications on the action of heat were, or were not, sufficient to justify the expectations of Dr. Hutton.

It was in prosecution of these views that I formerly undertook a set of experiments, proving, I believe to the satisfaction of the scientific world, the identity of Whinstone and Lava, of which a full detail is given in your Transactions. In farther illustration of the same topic, my experiments on Carbonate of Lime were formerly undertaken, by which it was shown, that calcareous matters, exposed to heat under pressure, might be fused; and, on cooling, would crystallise, so as in every respect to resemble marble. To these I beg leave likewise to refer the Society.

The immediate object of the paper I have now the honour of submitting to the Society—the consolidation of the strata—has been pursued in a similar spirit, and with similar views to those formerly announced. In making efforts to trace the modifications which the action of heat would undergo, when compelled to act under the influence of compression, or of other circumstances, all of which, in company, I have always been willing to distinguish by the name of *Plutonic*, (although the term was originally suggested, ironically, by one of our keenest antagonists, the late celebrated Dr. Kirwan), I was led to the particular topic of this paper, by an unexpected scene which presented itself in my own neighbourhood, in the country.

It had often been urged, and apparently with good reason, against this branch of the Huttonian Theory, that no amount of heat applied to loose sand, gravel, or shingle, would occasion the parts to consolidate into a compact stone. And as all my experience led to the same conclusion, I saw that, unless, along with heat, some flux were introduced amongst the materials, no agglutination of the particles would take place. The striking circumstance above alluded to, as occurring near Dunglass, and which will be particularly described presently, having suggested to me the idea that the salt of the ocean might possibly have been the agent in causing the requisite degree of fusion, I instituted a series of experiments, the details of which I am about to bring before the Society. By these, I conceive it will be shown, that this material, under various modifications, is fully adequate to explain the consolidation of the strata, and many other effects which we see on the surface of the Earth.

My success, from the first, was such as to promise the most satisfactory result, though it is only within the last year that I have been able to command the repetition of the experiments in a manner fit to be laid before this Society. This must be my apology to those who hear me, and to such of my friends as take an interest in these investigations, for having so long delayed the publication of a set of facts, some of which had presented themselves to my view many years ago.

Whoever, indeed, has had any experience in the prosecution of new subjects of experimental inquiry, knows that, owing to his ignorance of the requisite adjustment of the proportions of the ingredients, and of other similar arrangements, he must depend, in a great degree, upon chance for the success of his first results, and that he must often submit to spend much time and labour upon a subject, even after it has been made out to his own satisfaction, before he has acquired sufficient command over its details to answer for the result of any particular experiment, so as to be able to produce it with confidence to the public.

It may be interesting, in the first place, to describe, in a general way, the geological structure of the country, in the neighbourhood of the singular scene which gave rise to these speculations.

On different occasions I have laid before this Society observations made on the rugged shore which occupies the southern entrance of our estuary the Firth of Forth, which, from being frequently washed by a very boisterous ocean, presents to view a distinct exhibition of its internal structure. The eastern part is occupied by the promontory of Fastcastle, composed entirely of the elder quality of strata, called by the Germans Grey Wacke. Further to the west it consists of cliffs formed of Sandstone, nearly in a horizontal position. These two meeting in the crag called the Siccar Point, afford the most distinct view we any where have of the peculiar relation and mutual history of these two rocks.

More inland, on the borders of Lammermuir, a set of horizontal beds occur, consisting of a loose assemblage of rounded stones, intermixed with sand and gravel, which bear every appearance of having been deposited by water, and which, as to their general history, seem to have undergone no change since the overwhelming, though transient, agitations of water, of which I have frequently had occasion to speak in this Society.

In the summer of 1812, as I was returning from visiting the granitic range which occurs in the water of Fasnet, in the hills of Lammermuir, and riding down the little valley of Aikengaw, which deeply indents this loose collection of gravel and shingle, about two miles above the village of Oldhamstocks, and at the distance of eight or ten miles from the sea, I was struck with astonishment on seeing one of these gravel banks, formed, as above described, of perfectly loose materials, traversed vertically by a dyke, which, in its middle, consisted of whinstone, and was flanked by solid conglomerate; but this solidity abated gradually till the conglutination of the rounded masses diminishing by degrees, the state of loose shingle and gravel was entirely restored on both sides. The agglutinated mass adjacent to the dyke bore no resemblance to the result of calcareous petrifaction; scarcely ever gave effervescence with acid; and, by its gradual termination, differed from any whinstone-dyke I have seen to penetrate the strata; for, in the ordinary case, the termination of the crystallite against the adjoining aggregate through which it passes, is almost always quite abrupt.

About a hundred yards higher up the valley of Aikengaw, there occurs an agglutination similar to the last, though without any whin-dyke, and sufficiently strong to resist the elements, by which the surrounding matters had been washed away, leaving the pudding-stone, or agglutinated shingle, to stand up by itself, in a manner remarkable enough to have attracted the notice of the peasantry as something supernatural, since they have bestowed upon it the name of the Fairy's Castle.

Farther up the stream, other agglutinations occur frequently, as we could see in little narrow glens cutting through the mass; and higher still, they are so numerous as to meet and convert the whole into one unbroken mass of pudding-stone, occupying all that is exposed to view.

These very remarkable, and, to me at least, novel appearances, were the first which suggested the idea, that the consolidation not only of this class of conglomerates, but of sandstone in general, had been occasioned by the influence of some substance in a gaseous or aëriform state, driven by heat into the interstices between the loose particles of sand and gravel, where it had acted as a flux on the contiguous parts. On considering what this penetrating substance might be, and from whence it could have come, the following circumstance presented itself to my recollection at the moment, and promised to afford some assistance to these conjectures.

A few miles lower down the valley in which the above facts were observed, at the distance of more than a mile from the sea, and between two and three hundred feet perpendicularly above it, there occurs a crag of sandstone, in which a numerous succession of strata are distinctly visible. Several of these beds have yielded much to the action of the air, and, in dry weather, exhibit a considerable white efflorescence, which has completely the taste of common salt; and so remarkable is this circumstance, that the rock has acquired, in the country, the name of Salt-Heugh.

Here, then, it immediately occurred to me, was probably the source of an abundant supply of the elastic substance or fumigator, whose action as a flux had been pointed out by the agglutinations in Aikengaw above described.

I conceived, that, if there were at the bottom of the sea a bed of sand and gravel, drenched with brine of full saturation, and that heat were applied to it from beneath according to Dr. Hutton's hypothesis, the first effect would be, to drive the water from the lowest portion of the sand, and to convert the salt which remained amongst it, together with the sand, into a dry cake. During this operation, or until the cake became quite dry, the absorption of latent heat would prevent the temperature from surpassing the boiling point of brine. But no sooner was this dryness accomplished, than, I imagined, the temperature of the mass would begin to rise above that pitch; the portion of it next the fire would gradually acquire a red-heat; that then the salt, being made by the heat in part to assume an elastic form, would be sent in fumes through the dry cake just described, and thus, by partially melting the contiguous particles, produce an agglutination.

Such being my theoretical views, no time was lost in submitting them to the test of experiment. Taking it for granted that a quantity of sea-salt must frequently be formed and deposited, along with sand and gravel, at the bottom of the ocean (in the manner I shall have occasion to describe at another stage of this paper), where the water has been collected by its superior specific gravity, in the form of brine, I proceeded to make the following experiments.

Dry salt was placed along with sand, sometimes in a separate layer, at the bottom of the crucible, and sometimes mixed throughout the experiment: the whole was then exposed to heat from below. I found that the salt was invariably sent in fumes through the loose mass, and by its action produced solid stone in a manner completely satisfactory, as illustrative of the facts in Aikengaw; and so as to give a good explanation of the production of sand-stone in general.

These artificial stones are of various degrees of durability and hardness;—some of them do not stand exposure to the elements, and crumble when immersed in water;—some resist exposure for years;—others are so soft as not to preserve their form for any length of time;—while some bear to be dressed by the chisel; and, it may be remarked generally, that, as far as the results of my experiments have been compared with natural sandstone, the same boundless variety exists in both cases. A striking instance of this resemblance occurs in the case of the Salt-Heugh, the sandstone of which, when immersed in water, crumbles down, exactly in the same manner as those results of my experiments which taste much of salt.

The fumes of the salt, no doubt, act, in all these cases, as a flux on the siliceous matter, and thus cement the adjacent particles together. The Society are, doubtless, well aware of the power of salt fumes in glazing pottery; and the analogy, I conceive, is complete. It is the application alone that is new.

So far the results were satisfactory. But it next occurred, that it might be plausibly objected, that the presence of the superincumbent cool ocean would interfere with the process, on the principles of latent heat. To put this to the test, I proceeded to expose a quantity of sand, covered to the depth of several inches with common salt-water, to the heat of a furnace, and, as the liquid boiled away, replenished it from time to time by additions from the sea. Of course it gradually approached to a state of brine. But this proved a very tedious operation, requiring a continued ebullition, during three weeks without ceasing, before it became sufficiently saturated with salt by the discharge of the fresh-water; and I thought it much easier, and no less satisfactory, to employ brine from the first, formed at once by loading the water with as much salt as it could dissolve, amounting to about one-third of its weight.

The vessels employed in these early experiments were the large black-lead crucibles used by the brass-founders. I filled the vessel, which was 18 inches high and 10 broad, nearly to the brim with brine of full saturation, the lower portion being occupied, to the depth of about 15 inches, with loose sand from the sea-shore, and thoroughly drenched with the brine. In order to have a view of the progress of the experiment, I placed an earthen-ware tube, about the size and shape of a gun-barrel, closed at bottom, and open at the top, in a vertical position, having its lower extremity immersed in the sand, and reaching to within about an inch of the bottom of the pot, while the other end rose a foot above the surface of the brine, and could be looked into without inconvenience.

After a great number of experiments, furnishing an unbounded variety of results, I at length obtained a confirmation of the main object in view. I observed that the bottom of the porcelain barrel, and of course the sand in which it rested, became red-hot, whilst the brine, which, during the experiment, had been constantly replenished from a separate vessel, continued merely in a state of ebullition: the upper portion of the sand, drenched with the liquid, remained permanently quite loose, but the lower portion of the sand had formed itself into a solid cake.

On allowing the whole to cool, after it had been exposed to a high heat for many hours, and breaking up the mass, I was delighted to find the result, occupying the lower part of the pot, possessed of all the qualities of a perfect sandstone, as may be seen in the specimens now presented to the Society. Whenever the heat was not maintained so long, the sandstone which resulted was less perfect in its structure, tasted strongly of salt, and sometimes crumbled to sand when placed in water.

Many of these early experiments were accomplished with tolerable success. But still the result was somewhat precarious, and could not be announced with the confidence that I felt in presenting my former experiments to this Society.

The cause of this uncertainty I traced to the chemical operation of the salt, acting as a flux upon the porcelain vessels employed. This very action, I was well aware, was the main agent and cause of our success, when kept within proper bounds; but, on being allowed to pass those limits, and to act on the containing vessel as well as on the experiment, it destroyed the vessel, and converted the whole into a confused mass of slag.

After numberless unsuccessful attempts, and after returning again and again to the charge, with an interval sometimes of years, I at last met with a quality in some of the materials to me altogether unlooked for, by means of which may be obtained successful results, with scarcely any risk of failure.

I found that the action of the salt upon the substances of the crucibles of clay, did not exert itself in the same manner upon iron; but that a large vessel of cast-iron, 18 inches deep by 10 wide, and a common gun-barrel welded up at the breech, and open at the top, enabled me to work with the heat of melting gold, without injuring the vessels, and at any time to produce a perfect freestone; thus satisfying our theoretical expectations.

Similar results, in all respects, were produced by exposing pure pounded quartz to the action of the salt fumes,—and also when gravel, or any other mass of loose materials, was used instead of sand.

Having now shown, in a satisfactory manner, that salt, whether in a dry state mixed along with loose materials, or driven in fumes through them, or applied in the state of brine, and exposed to heat, is a sufficient agent to produce a consolidation, such as we see in natural sandstones and other stratified rocks, it remains to be investigated, whether an adequate supply of this flux may be reckoned upon in nature.

It is well known that great diversity exists in the degree of saturation of the sea by salt, at different places; and Buffon has been at much pains in collecting examples of this diversity in his geological volumes, introductory to his Natural History. It is known that, in many of the communications between sea and sea, a constant current sets one way, indicating that the evaporation from the sea, to which this stream flows, surpasses in quantity its supply of fresh-water from the rivers, rains, and springs. This is remarkably the case with the Mediterranean, into which a perpetual stream sets from the ocean, at the Gut of

Gibraltar. We have reason, then, to conclude, both that the surface of the Mediterranean is lower than that of the ocean, and likewise that the quantity of salt in the former is perpetually on the increase; so that the specific gravity of the waters, and the intensity of their saturation, must be perpetually advancing to a state of brine. I am well aware, that an attempt has been made to render such a conclusion unnecessary, by the supposition of a counter-current flowing at the bottom, out of this great basin; but such suppositions are, in my opinion, altogether gratuitous.

What is here said of the Mediterranean, will apply no less to other seas, and even to the great oceans. And wherever a basin occurs, in which a bottom of great depth is surrounded by a ridge comparatively shallow, we may expect to find the lower portion, at least, of the water in a state approaching to brine.

Without any such theoretical explanation of the manner in which a supply of salt is supposed to be formed, it may perhaps be considered sufficient for my purpose, to recal to the recollection of the Society, that there are in almost every part of the world vast districts of rock-salt, and in some countries extensive salt lakes and salt rivers; and in our own country we have many instances of brine springs, besides rock-salt in abundance.

Here then it seems to me, we are plentifully furnished with the means of accounting, in the manner experimentally shown, for the agglutinations of such gravel as that of Aikengaw and for the strata of the Salt-Heugh, which, by an easy analogy, may be transferred to sandstone in general, and, perhaps, to stratified rocks of every description.

A member of this Society, however, well known by his scientific acuteness, alleged, first in his public lectures, and afterwards, upon my requesting an explanation of his objection, again repeated, that I was not justified in such theoretical conclusions, respecting the influence of heat at the bottom of the sea, since the neighbourhood of the cool water would necessarily counteract that influence.

In answer to this difficulty, I must beg leave to remark, that, in all my experiments above alluded to, the sand (viewed by means of the gun-barrel) was seen to become red-hot during the process of consolidation, while the superincumbent brine remained boiling above; and it was even found easy, by supplying cool brine in sufficient quantity, to maintain the temperature of the fluid permanently such, that the hand could be plunged into it at top, without injury, the sandstone below remaining all the while at a full red heat. But whenever I repeated this experiment, with every circumstance the same, both as to duration and temperature, as in the example above detailed, but in which, instead of brine, *fresh* water was used, the result was very different. The lower part of the gun-barrel, immersed in the sand, and in which gold had melted in the brine experiment just mentioned, now remained permanently black and cold; and the whole of the sand in the pot, when removed from the furnace, fell out loose by its own weight; not the least trace of consolidation having taken place.

We may thus, I trust, presume to have added one more new and important modifying circumstance of heat, to those already advanced in support of the Huttonian doctrines; for, since it has been experimentally shown, that heat, under the modifications produced by the presence of salt, as above-described, is fully adequate to the consolidation of loose materials, exposed to its action, it may fairly be presumed, that salt has performed a part, and a very important part, in the consolidation of the strata of the globe.

I should be doing injustice to the subject, were I not to state, that, besides the views developed in the foregoing paper, and supported by actual experiment, many others have occurred to me, respecting the agency of salt under various modifications, and all bearing more or less directly upon the Huttonian Theory of the Earth. Some of these views have been submitted to the test of experiment, and the results, as far as they have yet been carried, give me great hopes of ultimate success. Others are still in the shape of mere conjecture; and none of them are yet in a state to lay before the Society in detail. A simple allusion to one or two of the most important of these views may probably be received with indulgence; and I shall be very happy if gentlemen possessed of adequate leisure shall be induced to follow up, by actual experiment, what I have thrown out as mere matter of speculation.

I conceive that salt, in the state of fumes, and urged by a powerful heat, possibly also modified by pressure, or perhaps combined with other substances, may have penetrated a great variety of rocks, acting as a flux on some, as in basalt, granite, &c.; agglutinating others, as in the case of sandstone, pudding-stone, &c.; softening others, as in the case of contorted strata of greywacke. In many cases, too, I conceive that these fumes may have had the power of carrying along with them various other materials, such as metals in a sublimed state, which would in this way be introduced into rents, veins, and cavities, or may even have entered into the solid mass of the rocks, which I imagine these fumes may have had power to penetrate. I have already tried some experiments in pursuit of these ideas. Salt, for instance, has been mixed with oxide of iron, reduced to fine powder, and then exposed to heat along with quartzose sand. The iron, I found, was borne up along with the salt fumes. The sandstone, formed in this way, was deeply stained with iron, and other most curious appearances presented themselves.

Every one who has seen a sandstone quarry must have noticed evident traces of iron, the rock being stained in a great variety of ways; sometimes in parallel layers,—sometimes in concentric circles, or rather in portions of concentric spheres, like the coats of an onion,—and, generally speaking, disposed in a way not accountable by deposition from water. All these appearances I would account for, by supposing the rock, either at the moment of its agglutination into sandstone, or at some subsequent period, to have been penetrated by the fumes of salt, charged with iron, also in a state of vapour.

I may mention one very curious result of my experiments with salt and iron, acting upon sand, namely, that, upon breaking up the specimen of artificial sandstone, an appearance often presents itself of incipient crystallization, if I may use this term; a number of large, shining, parallel faces pervade the whole mass, and, by holding the specimen at the proper angle to the light, this appearance becomes very obvious. What the nature of these crystals is, I have not investigated; but as they very much resemble what we see in different kinds of sandstone, I am of opinion that they hold out a fair expectation, of our being able to produce many of the crystalline appearances with which we are familiar in nature.

Common sea-salt, such as I have used, as is well known, is not pure muriate of soda; and, in my experiments, I have mixed various other substances with it. In Nature, we must suppose that various contaminating substances would in like manner occur, to diversify the phænomena; and, accordingly, we do find a boundless variety, in the aspect not only of sandstone, but of almost every kind of rock; and I am by no means without expectation, that, in the course of time, we shall be able to imitate in our laboratory as many of these varieties as we choose to exhibit.

I have long been engaged also in a series of experiments on the formation of *Crystallites,* the name by which, as I have before stated, every crystallized rock might, perhaps, be usefully distinguished in contradistinction to *Aggregates,* or those formed of fragments. This great object in experimental geology, I hope to accomplish by means of an instrument which I have long had in use, for the regulation of high heats, a description of which may probably soon be laid before the Society, together with some further results in support of the Huttonian Theory of the Earth.

Article VII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Muride, a supposed new elementary Substance.*

On the 3d of July last, M. Berard read to the Parisian Academy of Sciences a memoir by M. Ballart, of Montpellier, in which he announces the discovery of a new elementary substance, which he denominates muride. The memoir was referred for examination to M. M. Vauquelin, Thenard, and Gay-Lussac; but the following is a sketch of its contents.

In its uncombined state, muride is a reddish liquid, with an odour resembling that of chlorine; its specific gravity is 2·966, it is volatile, and boils at 117° Fahr. At a pressure of 76 cent. it gives a red vapour resembling nitrous gas. It remains fluid at 14° Fah.; is soluble in water, alcohol, and ether; is not altered by a red heat, or by the electric current, extinguishes burning bodies which are immersed in its vapour, decolorizes indigo, and combines with most of the simple bodies, forming compounds analogous to those produced by chlorine and iodine under the same circumstances. It combines with nascent hydrogen, losing its colour, and acquiring the properties of an acid, termed by M. Ballart hydromuridic acid. This acid is readily decomposed by chlorine, which unites with the hydrogen, and liberates the muride in the form of red vapours. It is not decomposed by iodine, but on the contrary decomposes hydriodic gas, and separates the iodine. Hydromuridic acid is not decomposed by oxygen. Potassium, zinc, iron, tin, and other metals, decompose it, disengaging the hydrogen, and uniting with the muride; the resulting compounds are in every respect analogous to chlorides. The *muridure* of potassium has the cubic form, like the iodide and the chloride of this metal.

M. Ballart mentions several processes for obtaining this substance. One of them is as follows: He passes a certain quantity of chlorine into the mother-water of salt-pans, which decomposes the combined hydromuridic acid it contains, the muride in solution giving the fluid a red colour. This solution is then agitated with ether, which dissolves the muride, and by the addition of caustic potash, a *muridure* of potassium is formed, which is easily separated in the solid form: this combination is decomposed in the sequel by means of sulphuric acid and oxide of manganese. The operation is performed in a glass retort, to which is adapted a horizontal tube containing chloride of calcium, and to this tube is adapted a smaller one immersed in a receiver properly cooled. The muride is to be kept in a ground-stoppered bottle, with a small quantity of common sulphuric acid, which, being lighter than the muride, floats upon it, and prevents its evaporation.—(Journal de Pharmicie.)

2. *Spontaneous Combustion of Chlorine and Olefiant Gas.*

It has long been known that chlorine and hydrogen in mixture are liable to explode, when struck by the direct rays of the sun, and an instance is related in the American Journal, (vol. iii. p. 341,) in which these two gases exploded, even in the diffuse light of a cloudy and snowy day. I have not met with any account of a similar action on the part of chlorine and olefiant or heavy carburetted hydrogen. It is well known that when mingled, in about equal volumes, they combine quietly, and become condensed into the peculiar aromatic, oily-looking substance, since called chloric ether. This effect I had so often witnessed, and had never seen any material variation in the result, that I was not prepared to look for any thing else. But in an experiment of this kind, (January 5, 1826,) happening to mingle the chlorine with the olefiant gas in such a manner, that the latter gas was uppermost, the combination went on more slowly than when the reverse order was observed; and the oily matter was gradually precipitated, but was less abundant in quantity than usual. Repeating the experiment, in the same manner, the gases had remained in contact a few minutes, apparently without mingling much except at their surfaces—the chlorine preserving its peculiar colour and the other gas its colourless transparency, when, suddenly, a bright flash pervaded the bell-glass, which was of the capacity of five or six quarts; it was raised out of the water with a slight report—a dense deposit of charcoal lined the glass and floated on the water of the cistern, and the chlorine disappeared. The appearances were much like those which are exhibited when a rag dipped in oil of turpentine is placed in a jar of chlorine gas.

Reflecting on the circumstances, I was led to believe that the peculiar effect, in this case, arose from the fact, that, owing to the great difference in the specific gravity of the two gases, the action took place principally at the two surfaces of contact, and thus the chlorine acting upon a comparatively thin stratum of inflammable gas, the two became so heated, as to pass into vivid combustion. Every new occurrence in practical chemistry, which may involve danger, ought to be exactly stated, that we may be aware of contingencies not otherwise anticipated.—(American Journal of Science.)

Mineralogy.

3. *Thenardite.*

This substance was discovered in the Salt Works of Espertines, about five leagues from Madrid, by M. Rodas, a Spanish manufacturer. The crystalline form is described by M. Cordier, d the analysis is by M. Casaseca, Professor of Chemistry at

Madrid, and a pupil of M. Thenard, in honour of whom he has named it.

The forms of the crystals are easily ascertainable, but the planes are too uneven to admit of accurate measurement; the planes obtained by fracture are, however, even, and the primary form of the crystal is determinable with considerable accuracy; the cleavage is threefold, and in one direction the laminæ are perfectly smooth and brilliant. The primary form (fig. 1), indi-

Fig. 1.

Fig. 2.

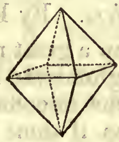
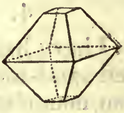


Fig. 3.



cated by cleavage, is a right prism with a rhombic base, the angles of which are nearly 125° and 55°; taking the mean of several measurements, the height of the lateral planes is to that of the terminal as 13 to 15; the cleavage is most distinct in the direction of the base.

There are two varieties of the crystal; first, the octahedron, (fig. 2). It is formed by a decrement of two rows of molecules in height, on the edges of the bases of the primary prism. The octahedron is symmetrical, and very flat in the direction of the small diagonal of the bases of the primary prism. Its vertical section in the direction of the greater diagonal of the base is a slightly acute rhomb, the smaller angle of which coincides with the summit of the crystal.

The second variety (fig. 3) is the preceding crystal, with the summits replaced by a rhombic plane parallel to the bases of the primary form.

The crystals would probably be doubly refractive, but they are not sufficiently transparent to admit of this point being determined. The specific gravity is nearly the same as that of glauberite, viz. about 2·73.

The chemical characters of the crystals are, that, when exposed to the air, they become opaque, and the surface is covered with a powder which is readily removed. According, however, to M. Casaseca, this is not owing to the loss of water, but to the absorption of a small quantity, for the salt is perfectly anhydrous, losing scarcely any weight by exposure to a strong heat; and this little is probably derived from the slight efflorescence at the surface already noticed. Thenardite is perfectly soluble in water; the solution when saturated is slightly alkaline. When put into dilute sulphuric acid, it effervesces, owing to the evolution of carbonic acid gas. Examined by the usual re-agents, it appeared to contain only sulphate and carbonate of soda, and in the following proportions :

Sulphate of soda	99·78
Carbonate of soda.	0·22
	100·00

(Journal de Pharmacie.)

Miscellaneous.

4. *Remarks on Bowlders.* By Peter Dobson.

I have had occasion to dig up a great number of bowlders, of red sandstone, and of the conglomerate kind, in erecting a cotton manufactory; and it was not uncommon to find them worn smooth on the under side, as if done by their having been dragged over rocks and gravelly earth, in one steady position. On examination, they exhibit scratches and furrows on the abraded part; and if among the minerals composing the rock, there happened to be pebbles of felspar, or quartz, (which was not uncommon,) they usually appeared not to be worn so much as the rest of the stone, preserving their more tender parts in a ridge, extending some inches. When several of these pebbles happen to be in one block, the preserved ridges were on the same side of the pebbles, so that it is easy to determine which part of the stone moved forward, in the act of wearing.

I have caused blocks, with the above appearances, and weighing 15 tons, to be split up; and there are now a number of good specimens about the place, that will weigh from 10 to 50 cwt., dug out of the earth 200 feet above the stream of water in the vicinity.

These bowlders are found, not only on the surface, but I have discovered them a number of feet deep, in the earth, in the hard compound of clay, sand, and gravel.

One block of more than 30 cwt., marked and worn as above described, was dug out of a well, at the depth of 24 feet; a part of which is still to be seen.

Bowlders, with these marks upon them, I have observed, not only in this town, but in Manchester, Ellington, and Wilbraham.

I think we cannot account for these appearances, unless we call in the aid of ice along with water, and that they have been worn by being suspended and carried in ice, over rocks and earth, under water.

It is stated in the Edinburgh Encyclopedia, vol. xiii. p. 426, that " fields of ice sometimes rise from the bottom, and bring with them masses of rock, of several hundred tons weight. These masses of stone are imbedded in the ice, they are carried along with the ice, and deposited on shores at a great distance from their original situation."

Similar ideas are expressed in the same work, vol. xi. p. 70.

I mention these appearances on bowlders of sandstone in this vicinity, in order that in other places, if similar appearances exist, they may be noticed. Such observations may lead to probable conclusions respecting the transportation of bowlders, and the formation of banks of earth.—(American Journal of Science.)

5. *New Species of Salamander, (inhabiting Pennsylvania.)* By Richard Harlan, M.D. Prof. of Comp. Anat. to the Phil. Mus.

S. flavissima.

Char. Brownish, yellow above; clear bright yellow beneath; beak marked with three black lines; tail compressed, longer than the body.

Dimensions. Total length three inches two tenths; length of the tail one inch nine tenths; of the body, head inclusive, one inch three tenths.

Description. A long and slender animal, head broader than the body, rather depressed; eyes prominent, iris gilt yellow; a broad black line on each side of the spine extending from the eye to the end of the tail; a narrow depressed black line extending along the spine from the occiput to the base of the tail; all the under parts of the animal of a deep yellow; head separated from the neck by a transverse line under the throat; tail compressed, much longer than the body and head.

Note. I have caught several of these animals beneath the stones in moist places, or on the borders of brooks in shady situations; it is a very active species and sometimes attains to three inches in total length; the black line in the dorsal furrow is sometimes wanting, in which case the back is mottled with black—placed in spirits the yellow colour is destroyed. This species will occupy an intermediate station between the *S. bislineata* and *S. rubriventris.* A specimen is in the cabinet of the Acad. of Nat. Sc. of Phil.—(American Journal of Science.)

6. *On the Semi-decussation of the Optic Nerves.*
By Dr. Crawford.

Mrs. B. at 65, had a slight hemiplegic attack of the left side, 9th Dec. 1816. She regained in great measure the use of her limbs; but the following affection of the sight continued from the time of her seizure till her death, about five years afterwards.

When she looked at any object, she could only see one-half of it distinctly, the other being very obscure. For example, in looking at a person's face, she could only see distinctly that side of it which was to her right hand. This was equally the case whether she looked with both eyes, or only with the right one;

but when she looked with her left eye only, the obscurity was greater. When four fingers were held before her, she could see two of them distinctly; the third she could distinguish, but could not see plainly; the fourth she could not see at all. When she looked at three fingers, she could see two of them pretty plainly; the first, however, more than the second, and the third she could not distinguish at all. When she looked at two fingers, she could only see one distinctly.

After she had recovered so as to be able to get out of bed, it was discovered that, although she could only see one-half of an object plainly, when held directly before her, yet, if it was moved to her right hand, and she continued to look straight forward, she could see the whole of it distinctly. On the contrary, if it was moved to her left hand, keeping her eyes fixed as before, she could not perceive any of the object at all.

Dr. Wollaston does not notice this last circumstance in the instances he relates; but it appears to me an additional confirmation of his opinion. The defective vision was owing to the insensibility of one-half of the retina of each eye; and that was occasioned in this instance, probably, by pressure on the right thalamus nervi optici, where the nervous fibres thence proceeding (on the supposition of this *semi-decussation* with those from the left thalamus), finally expand into the right half of the retina of each eye. This part of the retina being insensible, the rays of light which passed to it from objects on the left of the centre of vision, produced no sensation. But when the eyes were kept fixed, and the object to be looked at moved to the right of the centre of vision, so that all the rays from it passed to the left and sound half of the retina, then the whole of the object became visible.

It should be remarked, that, although the sensation of only the *left* side of the body was impaired, yet it was the *right* half of each retina which was insensible.—(London Med. and Phys. Journ.)

Winchester, *Nov.* 5, 1824.

Article VIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Mr. W. Phillips will shortly publish a new and improved Edition of his Outlines of Mineralogy and Geology, for the use of young persons.

The Fluxional Calculus; an Elementary Treatise designed for Students of the Universities, and for all those who desire to be acquainted with the principles of analysis; by T. Jephson, BD.

Lectures on Astronomy, accompanied and illustrated by the Astronomicon, or a Series of moveable Diagrams. By W. H. Prior.

Travels of the Russian Mission through Mongolia to China. By G. Timkowski; with notes by M. J. Klaproth. 2 vols. 8vo.

JUST PUBLISHED.

Koecker's Principles of Dental Surgery. 8vo. 14*s.*
Scratchley's London Dissector. 6*s.*
Fife's Manual of Chemistry. 7*s.*
Samouelle's Directions for preserving Insects. 5*s.*
Morison's Outlines of Lectures on Mental Diseases. 10*s.* 6*d.*
Rough Notes across the Pampas and Andes; by Capt. J. R. Head. 9*s.* 6*d.*

Article IX.

NEW PATENTS.

Count A. Eugene de Rosen, of Princes'-street, Cavendish-square, for a new engine for communicating power to answer the purposes of a steam-engine.—Aug. 1.

J. B. Wilks, Tandridge Hall, Surrey, for improvements in producing steam for steam-engines, and other purposes.—Aug. 2.

L. W. Wright, Borough-road, engineer, for improvements in the construction of trucks or carriages, applicable to useful purposes.—Aug. 2.

J. Williams, ironmonger and ships'-hearth manufacturer, and J. Doyle, mechanist, Commercial-road, for an apparatus and process for separating salt from sea-water, and thereby rendering it fresh and fit for use.—Aug. 4.

E. Hazard, Norfolk-street, engineer, for methods of preparing explosive mixtures, and employing them as a moving power for machinery.—Aug. 12.

J. T. Thompson, Long Acre, camp-equipage-maker, for improvements in making or manufacturing metallic tubes, whereby strength and lightness are obtained, and for applying them, with various other improvements, to the constructing of the metallic tube and other bedsteads.—Aug. 17.

J. C. Schwieso, Regent-street, musical instrument-maker, for improvements on certain stringed musical-instruments.—Aug. 22.

T. Burstall, Leith, and J. Hill, Bath, engineers, for improvements in the machinery for propelling locomotive carriages.—Aug. 22.

J. Yandall, Cross-street, Surrey, for improvements on apparatus for cooling and heating fluids.—Aug. 24.

F. Halliday, Ham, Surrey, for improvements in raising or forcing water.—Aug. 25.

W. Downe, Exeter, plumber and brass-founder, for improvements on water-closets.—Aug. 25.

R. Busk, and W. K. Westley, of Leeds, flax-spinners, for improvements in machinery for heckling or dressing, and for breaking, scutching, or clearing hemp, flax, or other fibrous substances.—Aug. 29.

W. Day, Strand, trunk and camp-equipage-maker, for improvements on bedsteads, which are also applicable to other purposes.—Aug. 31.

T. R. Williams, Norfolk-street, Strand, for a machine for separating burs or other substances from wool, hair, or fur.—Sept. 18.

Article X.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
Aug. 23	29·52	29·50	29·510	68	58	63·0		SW	Showers; cloudy.
24	29·52	29·50	29·510	70	58	64·0	0·54	SW	Cloudy; showers.
25	29·49	29·48	29·485	68	58	63·0		SE	Rainy.
26	29·52	29·52	29·520	68	56	62·0		SW	Cloudy; showers.
27	29·72	29·70	29·710	70	56	63·0		SW	Cloudy; showers.
28	29·70	29·68	29·690	72	54	63·0		SW	Cloudy; showers.
29	29·56	29·50	29·530	70	56	63·0	0·15	SW	Showers.
30	29·54	29·50	29·520	70	58	64·0		SW	Showers.
31	29·56	29·56	29·560	68	56	62·0		SW	Showers.
Sept. 1	29·66	29·60	29·630	68	56	62·0		NE	Showers.
2	29·72	29·70	29·710	66	55	60·5		NE	Clear; showers.
3	29·76	29·74	29·750	68	54	61·0		NE	Clear.
4	29·82	29·70	29·760	68	56	62·0		W	Clear.
5	29·70	29·60	29·650	66	58	62·0	0·15	W	Rain.
6	29·30	29·20	29·250	62	58	60·0	0·25	NW	Rain.
7	29·62	29·60	29·610	64	54	59·0	0·50	SW	Clear.
8	29·52	29·50	29·510	64	54	59·0		SW	Rain.
9	29·70	29·64	29·670	64	56	60·0	0·30	SW	Clear.
10	29·90	29·84	29·870	64	52	58·0		SW	Clear.
11	30·02	30·00	30·010	63	52	57·5		W	Clear.
12	30·12	30·10	30·110	64	52	58·0		W	Clear; cloudy.
13	29·96	29·80	29·880	65	53	59·0		SW	Clear; cloudy.
14	29·80	29·76	29·780	64	52	58·0		N	Showers.
15	29·98	29·96	29·970	64	53	58·5		NE	Cloudy.
16	30·00	30·00	30·000	64	54	59·0		SE	Cloudy.
17	29·90	29·86	29·880	66	55	60·5		SE	Rain, thun., lightn.
18	29·50	29·48	29·490	67	55	61·0	0·72	SE	Cloudy.
19	29·72	29·70	29·710	65	56	60·5		SE	Cloudy.
20	29·80	29·76	29·780	66	55	60·5		E	Clear; cloudy; shw.
21	29·92	29·90	29·910	64	57	60·5		E	Clear; showers.
22	29·92	29·90	29·910	64	54	59·0		SE	Cloudy.
	30·12	29·48	29·710	72	52	61·0	2·610	SW	

RESULTS.

Barometer, mean height .... 29·710
Register Thermometer, ditto .... 61·0°
Rain, No. 1, 2·610, No. 2, 4·265.
Prevailing wind, SW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, Sept. 23, 1826. EDWARD C. GIDDY.

Article XI.

METEOROLOGICAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
7th Mon.							
July 1	N W	30·34	30·28	81	56	—	
2	N W	30·35	30·34	88	52	—	
3	E	30·34	30·21	89	52	—	
4	E	30·21	30·04	86	60	—	
5	W	30·07	30·04	87	58	·98	
6	W	30·04	29·99	87	66	—	
7	S W	29·99	29·87	85	62	—	
8	S W	29·87	29·86	83	64	—	11
9	S W	29·92	29·86	83	54	—	
10	S W	30.00	29·92	80	56	·90	—
11	W	30·04	30·00	78	62	—	
12	S W	30·04	29·88	78	64	—	
13	S W	29·90	29·88	75	55	—	10
14	W	30·01	29·90	78	60	—	
15	S W	30·03	29·97	75	54	—	04
16	S W	30·11	30·03	73	49	·85	01
17	N W	30·11	30·10	79	51	—	
18	N W	30·10	30·08	78	58	—	
19	N W	30·16	30·08	74	54	—	
20	W	30·16	29·79	72	59	—	14
21	S W	29·92	29·79	73	49	·76	—
22	N E	30·10	29·92	74	50	—	1·83
23	N E	30·18	30·10	67	54	—	36
24	N E	30·26	30·18	70	55	—	
25	N E	30·38	30·26	78	47	—	
26	N E	30·44	30·38	77	44	—	
27	N E	30·44	30·37	74	50	—	
28	N E	30·37	30·24	78	46	—	
29	S W	30·24	30·20	79	49	·95	
30	E	30·20	30·12	85	52	—	
31	S	30·18	30·09	89	59	·48	02
		30·44	29·79	89	44	4·92	2·61

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

REMARKS.

Seventh Month.—1—7. Fine. 8. Fine day: evening showery. 9—11. Fine. 12—14. Cloudy. 15. Fine. 16. Morning cloudy: afternoon fine. 17—19. Fine. 20, 21. Showery. 22. Very rainy night. 23. Rainy day. 24—29. Fine. 30. Sultry. 31. A thunder storm at ten, a. m.

RESULTS.

Winds: NE, 7; E, 3; S, 1; SW, 10; W, 5; NW, 5.

Barometer: Mean height

For the month.. 30·101 inches.

Thermometer: Mean height

For the month.. 67°

Evaporation.. 4·92 in.

Rain.. 2·61

Laboratory, Stratford, Eighth Month, 30, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1826.

Article I.

*Biographical Account of Alexander Wilson, MD. late Professor of Practical Astronomy in Glasgow.** By the late Patrick Wilson, AM. Professor of Practical Astronomy in the University of Glasgow.†

Alexander Wilson, MD. late Professor of Practical Astronomy in Glasgow College, was a younger son of Patrick Wilson, town-clerk of St. Andrew's, and was born there in 1714. He was very young when his father died, and was afterwards brought up by the care of his mother, Clara Fairfoul, a person much respected for her prudence, virtue, and piety.

Having received the usual education at the different schools, he entered to the College of St. Andrew's, where he made great proficiency in literature and the sciences, and, after completing a regular course of studies, was admitted to the degree of Master of Arts in his 19th year.

Before the expiration of his academical course, his inclination led him to prefer the study of Natural Philosophy, and particularly those branches of it which relate to Optics and Astronomy. From his earliest years he discovered a strong propensity to several ingenious arts, among which may be mentioned drawing, modelling of figures, and engraving upon copper-plate. Even when a boy, he often devoted his leisure to such employments, and though in all of them he was almost entirely self-directed and self-taught, yet, from time to time, he produced specimens of ingenuity which drew upon him a general attention, and which, by real judges, were considered as indications of uncommon natural talents.

* From the Transactions of the Royal Society of Edinburgh, vol. x. part ii.

† This Memoir of Dr. Wilson, after being read at the Royal Society, was withdrawn by its author, for the purpose of making some alterations upon it; and was never returned for publication. It was found, however, among the papers of Mr. Patrick Wilson, and is now printed with the consent of his family.

Upon his leaving the College, he was put as an apprentice to a surgeon and apothecary in his native city, with a view of following that profession. At this period he became more particularly known to Dr. Thomas Simson, Professor of Medicine in the University, who ever after treated him with much kindness and friendship. About the same time he had also the good fortune to find a patron in Dr. George Martine, a physician in the town. In those days the construction and graduation of thermometers was little attended to or understood in Britain, and Dr. Martine, from a just conception of the importance of this instrument, in many philosophical pursuits, was then employed in composing those Essays on the subject of Heat which have rendered his name so justly celebrated. The author, besides illustrating so well the theory of the thermometer, was further very desirous of bringing accurate thermometers into general use; and, with this view, he turned the attention of his friend Mr. Wilson to the art of working in glass. Though this was to him entirely a new attempt, depending upon many trials, and much mechanical address, yet he very soon acquired an admirable dexterity in forming the different parts of the instrument by the lamp and blowpipe, and in constructing and graduating the scales with accuracy and elegance; an employment which, for a long time, Mr. Wilson continued to be fond of at convenient seasons, and in which it is well known he greatly excelled.

Possessing naturally much activity of mind, and employing most of his leisure in some ingenious attempt or other, it was about this time that, in making certain optical experiments, he discovered the principles of the Solar Microscope, so far as to exhibit to several of his friends, in a dark chamber, the images of small objects enormously magnified, by the sun's rays entering at a hole in the window-shutter, and after several refractions falling upon a white ground within. But Mr. Wilson as yet was too far separated from the great world, and had too little experience, for bringing forward to the notice of the public any novelty of this kind; and, soon after, a similar combination of glasses, with additional improvements, occurred to Mr. Lieberkuhn, and was at length received as a very curious enlargement of the optical apparatus.

It was also, whilst employing himself in such researches, that Mr. Wilson proposed to many of his philosophical friends the idea of burning at a great distance, by means of plane mirrors, so situated as to throw the rays of the sun upon the same area, without the smallest knowledge of such a thing ever having been imagined by any person before him. But wanting the means of providing himself with any costly apparatus, the matter was pursued no further; and it is well known that M. de Buffon, some years afterwards, when equally uninformed of what Kircher had thought of, hit upon the same conception. In 1747, by a magnificent construction far beyond the reach of Mr. Wilson's finances, the French philosopher showed what might be done in this way, and with such effect, as to render the famous secret imputed to Archimedes, of setting on fire the Roman galleys, much less apocryphal than it had ever been considered before his time.

In 1737 Mr. Wilson departed from St. Andrew's, and, by the advice of his friends, went to London, in order to seek for employment as a young person who had been bred to the medical profession. Soon after his arrival there, he engaged himself with a French refugee, a surgeon and apothecary of good character, who received him into his family, giving him the charge of his shop and of some of his patients, with a small annual salary. About twelve months after he had been fixed in this new situation, Mr. David Gregory, Professor of Mathematics at St. Andrew's, coming to London, introduced him to Dr. Charles Stewart, physician to Archibald Duke of Argyle, then Lord Isla. Dr. Stewart received him with great kindness, and, not long after, made him known to Lord Isla, who, very soon, was pleased to bestow upon him marks of his attention and favour. In his interviews with this nobleman, Mr. Wilson had his curiosity much gratified by some valuable astronomical and physical apparatus which his Lordship had got constructed for himself, and had placed in his library. On the other hand, Mr. Wilson was happy in being able to contribute in some degree to the amusement of his patron, by constructing thermometers of different kinds for him and his friends, with more perfection and elegance than had been hitherto done at London.

Near eighteen months elapsed in this way, during which time he conciliated the good-will and esteem of his master, by a faithful and regular discharge of whatever business was committed to his care; and, in return, he found himself now and then indulged in opportunities of keeping up his connexions with persons of a philosophical cast, when his attendance upon the shop or patients could be conveniently dispensed with. Mr. Wilson has been often heard to speak of the satisfaction he enjoyed even at this period, and of his perfect contentment with every thing which had then fallen to his lot. But a serenity of temper, and a felicity of disposition, were qualities which eminently distinguished him throughout his whole life.

While he thus passed his time in what he considered as a comfortable settlement at his first entering upon the world, a circumstance of a very accidental nature occurred, which gave a new direction to his genius, and which, in the end, led him to an entire change of his profession. This was a transient visit which he happened one day to make to a letter-foundery, along with a friend who wanted to purchase some printing-types. In the course of seeing the common operations of the workmen usually shown to strangers, he was much captivated with the curious contrivances made use of in that business. Some short while afterwards, when reflecting upon what had been shown him in the letter-foundery, he was led to imagine that a certain great improvement of the art might possibly be effected, and of a kind too that, if successfully acomplished, promised to reward the inventor with considerable emolument. His ideas upon that subject he presently imparted to a friend a little older than himself, who had also come from St. Andrew's, and who was possessed of a considerable share of ingenuity, constancy, and enterprise. The consequence of this was, a resolution on the part of both these young adventurers to relinquish, as soon as it could be done with propriety, all other pursuits, and unite their exertions in prosecuting the business of letter-founding upon an improved plan.

It was not long ere they were enabled to carry into effect this resolution, and they first established a small type-foundery at St. Andrew's, and one on a larger scale, two years afterwards, at Camlachie, a village near Glasgow.

In this situation Mr. Wilson had contracted habits of intimacy and friendship with several persons of the most respectable character, particularly with the Professors belonging to the University of Glasgow, and with Messrs. Robert and Andrew Foulis, University-printers. The growing reputation of the University-Press, conducted by these gentlemen, gave additional scope to Mr. Wilson to exert his abilities in constructing their types, and being now left entirely to follow his own judgment and taste, his talents as an artist became every year more conspicuous. When the design was formed by the gentlemen of the University, together with Messrs. Foulis, to print splendid editions of the Greek classics, he, with great alacrity, undertook to execute new types, upon a model highly improved. This he accomplished, at an expence of time and labour which could not be recompensed by any profits arising from the sale of the types themselves. Such disinterested zeal for the honour of the University-Press was, however, upon this occasion, so well understood, as to induce the University, in the preface to the folio Homer, to mention Mr. Wilson in terms as honourable to him as they were just.

Though he thus continued to prosecute letter-founding as his chief business, yet, from his great temperance, domestic habits, and activity, he was enabled now and then to command intervals of leisure, which he never failed to fill up by some useful or ingenious employment. One of these, in which he took great delight, was the constructing of reflecting telescopes, an art which he cultivated with unwearied attention, and in the end with much success.

Among the more advanced students, who, in the years 1748 and 1749, attended the lectures on Divinity in the University, was Mr. Thomas Melvill, so well known by his mathematical talents, and by those fine specimens of genius which are to be found in his posthumous papers, published in the second volume of the Edinburgh Essays, Physical and Literary. With this young person Mr. Wilson then lived in the closest intimacy. Of several philosophical schemes which occurred to them in their social hours, Mr. Wilson proposed one, which was to explore the temperature of the atmosphere in the higher regions, by raising a number of paper kites, one above another, upon the same line, with thermometers appended to those that were to be most elevated. Though they expected, in general, that kites thus connected might be raised to an unusual height, still they were somewhat uncertain how far the thing might succeed upon trial. But the thought being quite new to them, and the purpose to be gained of some importance, they began to prepare for the experiment in the spring of 1749.*

Mr. Wilson's house at Camlachie was the scene of all the little bustle which now became necessary; and both Mr. Melvill and he, alike dexterous in the use of their hands, found much amusement in going through the preliminary work, till, at last, they finished half-a-dozen large paper-kites, from four to seven feet in height, upon the strongest, and, at the same time, upon the slightest construction the materials would admit of. They had also been careful, in giving orders, early, for a very considerable quantity of line, to be spun of such different sizes and strength, as they judged would best answer their purpose: so that one fine day, about the middle of July, when favoured by a gentle steady breeze, they brought out their whole apparatus into an adjoining field, amidst a numerous company, consisting of their friends and others, whom the rumour of this new and ingenious project had drawn from the town.

They began with raising the smallest kite, which, being exactly balanced, soon mounted steadily to its utmost limit, carrying up a line very slender, but of a strength sufficient to command it. In the mean time, the second kite was made ready. Two assistants supported it between them in a sloping direction, with its breast to the wind, and with its tail laid out evenly upon the ground behind, whilst a third person, holding part of its line tight in his hand, stood at a good distance directly in front. Things being so ordered, the extremity of the line belonging to the kite already in the air, was hooked to a loop at the back of the second, which, being now let go, mounted very superbly, and in a little time also took up as much line as could be supported with advantage; thereby allowing its companion to soar to an elevation proportionally higher.

* As no public notice has hitherto been taken of this matter, though Mr. Wilson had always some thoughts of doing so, it is hoped the following detail will not prove unacceptable or tedious to the reader.

Upon launching these kites according to the method which had been projected, and affording them abundance of proper line, the uppermost one ascended to an amazing height, disappearing at times among the white summer-clouds, whilst all the rest, in a series, formed with it, in the air below, such a lofty scale, and that, too, affected by such regular and conspiring motions, as at once changed a boyish pastime into a spectacle which greatly interested every beholder. The pressure of the breeze upon so many surfaces communicating with one another, was found too powerful for a single person to withstand, when contending with the undermost strong line, and it became, therefore, necessary to keep the mastery over the kites by other means.

This species of aërial machinery answering so well, Mr. Wilson and Mr. Melvill employed it several times during that and the following summer, in pursuing those atmospherical experiments for which the kites had been originally intended. To obtain the information they wanted, they contrived that thermometers, properly secured, and having bushy tassels of paper tied to them, should be let fall at stated periods from some of the higher kites; which was accomplished by the gradual singeing of a match-line.

When engaged in these experiments, though now and then they communicated immediately with the clouds, yet, as this happened always in fine dry weather, no symptoms whatever of an electrical nature came under their observation. The sublime analysis of the thunder-bolt, and of the electricity of the atmosphere, lay yet entirely undiscovered, and was reserved two years longer for the sagacity of the celebrated Dr. Franklin. In a letter from Mr. Melvill to Mr. Wilson, dated at Geneva, 21st April, 1753, we find, among several other particulars, his curiosity highly excited by the fame of the Philadelphian experiment; and a great ardour expressed for prosecuting such researches by the advantage of their combined kites. But, in the December following, this beloved companion of Mr. Wilson was removed by death,—to the vast loss of science, and to the unspeakable regret of all who knew him.

In the year 1752, Mr. Wilson, who had married Jean Sharp, daughter of William Sharp, a reputable merchant at St. Andrew's, brought his family to Glasgow. About five years afterwards he invented the Hydrostatical Glass-bubbles, for determining the strength of spirituous liquors of all kinds, which long experience, especially among the distillers and merchants in the West Indies, has now shown to be more accurate and more commodious than the instruments formerly used. From the minutes of a Philosophical and Literary Society, composed of the Professors and some of their friends, whose meetings were held weekly within the College, it appears that these hydrostatical bubbles made the subject of a discourse delivered by Mr. Wilson in the winter of 1757. At this time he also showed how a single glass bubble may serve for estimating very small differences of specific gravity of fluids of the same kind, such as water taken from different springs, or the like. This he did by varying the temperature of such fluids, till the same bubble, when immersed, became stationary at every trial, and then expressing the differences of their specific gravity, by degrees of the thermometer, the value of which can be computed and stated in the usual manner.

In the year 1758 he read another discourse to the same society, upon the motion of pendulums. On this occasion he exhibited a spring-clock of a small compass, which beat seconds by means of a new pendulum he had contrived, upon the principle of the balance, whose centres of oscillation and motion were very near to one another. At one of the trials, it performed so well as not to vary more than a second in about forty hours, when compared with a very exact astronomical clock near to which it was placed. It was some view of rendering much more simple and cheap the machinery of ordinary movements, by the slow vibrations of such a pendulum, which induced Mr. Wilson to prosecute these experiments.

Not long after this, he also put in execution a remarkable improvement of the thermometer, which consists in having the capillary bore drawn very much of an elliptical form, instead of being round. By this means the thread of quicksilver upon the scale presents itself broad, and much more visible than it does in a cylindrical bore of the same capacity. The difficulty of constructing thermometers of this kind had nearly hindered him from completing his invention, as the thread of quicksilver was found extremely liable to disunite, when descending suddenly in so strait a channel. But, by his long experience, joined to further investigation, and more trials, he at last discovered a method of blowing and filling thermometers with flattened bores, which freed them entirely from this defect.

About the same time also, he conceived the design of converting a thermometer graduated for the heat of boiling-water, into a Marine Barometer, in consequence of the well-known difference of temperature which water, when boiling, acquires under the variable pressure of the atmosphere. This he effected by making a boiling-water thermometer, about a foot in length, with a pretty large ball, and having a thread of quicksilver as broad and visible as was consistent with a very perceptible run upon small alterations of temperature. The stem of this thermometer he fortified, by inclosing it in a cylindrical case of white iron, having soldered to it, at its lower end, a socket of brass for receiving half of the ball, which afterwards became entirely defended, by screwing to the socket a hemispherical cap. At the other end of the case which environed the stem, there was soldered a tube of brass, wide enough to admit a scale of proper dimensions, before which there was an opening in the tube, defended by glass.

The utmost range of the scale he determined by the points, where the thermometer was found to be stationary when the ball and a certain part of the stem were immersed in water, boiling under the greatest variations of pressure which the climate afforded. The interval so found, he subdivided by other observations into degrees, which corresponded to *inches* of the barometer, and which were so denominated upon the scale.

In the year 1756, the College of Glasgow, upon the death of Dr. Alexander Macfarlane, of Jamaica, a great lover of, and proficient in, the sciences, received a legacy of a valuable collection of astronomical instruments, which that gentleman had got constructed at London by the best artists, and had carried out with him to Jamaica, with a view of cultivating astronomy in that island. The College upon this soon built an Observatory for their reception, which, by medals placed under the foundation, was called by the name of their generous benefactor; and Mr. Wilson was immediately thought of by the members of the Faculty, as a proper person for taking charge of it, and making the astronomical observations. At this juncture, his Grace Archibald, Duke of Argyle, who had all along continued his patronage to Mr. Wilson, more especially since he had brought the art of letter-founding into Scotland, used his influence with government, and procured his Majesty's presentation, nominating and appointing him Professor of Practical Astronomy and Observer in the College, with an annual salary of fifty pounds, payable out of the Exchequer; and, accordingly, in 1760, he was admitted to this new office by the unanimous and most cordial welcome of all the members of the faculty.

His two eldest sons, who had by this time entered upon a course of liberal education, not long after took upon them the further enlargement and improvement of the letter-foundery; and, before dismissing this topic, it deserves to be mentioned, that Mr. Wilson lived to such an advanced age, as to enjoy in the most feeling manner the reward of his early diligence and excellent example, in seeing the business rising in their hands to the highest reputation, not only in these kingdoms, but in foreign countries.

In 1763, when upon a visit at St. Andrew's, an honorary degree in medicine was conferred upon him by his Alma Mater.

Among the objects which now occupied him in the Observatory, his former labours towards improving the reflecting telescope were resumed, and pursued for a considerable length of time, with a view of obtaining some certain method of giving the parabolic figure to the great speculum. These trials were made upon a variety of metals, comparatively of a small diameter, and focal distance; but he regarded them only as preliminary ones, and had always in contemplation to engage with apertures of much greater dimensions. He was often heard to regret, that no crowned head, or wealthy association, ever thought of patronising an attempt to construct some vast telescope, to be employed in making discoveries in the moon or planets, or in exploring the heavens; and it is more than probable, that if his own means had been less circumscribed, he would of himself have attempted something of this kind. The more recent labours and brilliant success of the excellent Dr. Herschel have fully shown that such suggestions were by no means romantic; and the writer of this account, who has had the happiness of being well acquainted with both these men, has often remarked a striking resemblance in their character and turn of mind.

In 1769, Dr. Wilson made that discovery concerning the solar spots, of which he has treated in the Philosophical Transactions of London for 1774. Not long after he entered upon this new field, the nature of the solar spots was announced by the Royal Society of Copenhagen, as the subject of a prize essay. This induced him to transmit thither a paper written in the Latin language, containing an account of his observations, and of the conclusions drawn from them. In return, he obtained the honourable distinction of a gold medal of near sixteen guineas intrinsic value, having, on its reverse, the figure of Truth pendent in the air, holding a wreath in one hand, and in the other a perspective glass, and the motto, *Veritati luciferæ.*

As an astronomical observer, he was remarkable for a sharp and clear eye, devoid of all blemish, and which, too, without being liable to fatigue, had long been inured to examine and to judge of small objects in their nicest proportions; a circumstance which must have proved of great advantage to him, when employing his sight upon celestial appearances by means of the telescope; and it required only to know him, to have the fullest assurance of his fidelity in rendering an account of his observations.

His discovery in regard to the solar spots, though it be gaining ground more and more among those most conversant in astronomy, yet, like many other new discoveries, has not escaped its share of opposition. This gave him occasion to publish, in the London Philosophical Transactions for 1783, the second paper upon that subject, after a silence of near ten years, wherein, upon the authority of many more observations made in that interval, he obviates objections, and maintains the reality of his discovery, with an entire conviction. The amount of it is, "That the spots are *cavities* or *depressions* in that immensely resplendent substance which invests the body of the sun to a certain depth; that the dark nucleus of the spot is at the bottom of this excavation, which commonly extends downwards to a space equal to the semidiameter of our globe; that the shady or dusky zone which surrounds the nucleus, is nothing but the sloping sides of the excavation reaching from the sun's general surface downward to the nucleus or bottom." All this he has demonstrated by a strict induction drawn from the following phases of the spots, as they traverse the sun's disk.

When a large well-formed spot, consisting of a dark nucleus, and its surrounding umbra or dusky zone, is seen upon the middle of the sun's disc, the zone is generally equally broad all around; but when the same spot verges near to the limb, that side of the dusky zone which lies next to the centre of the disk begins much sooner than the side diametrically opposite to turn narrower, and at last disappears, while the other still remains dilated and visible. And, in like manner, when a spot enters the disc, by the sun's rotation, we see first the nucleus, and the upper and under sides of the shady zone or umbra, together with that side of it nearest to the limb, whilst the side opposite is still wholly invisible. But as the spot advances farther upon the disc, that side of its dusky zone which lately was invisible, now shows itself, and continues to enlarge more and more, till it becomes as broad as any other part surrounding the nucleus.

These phases, which he found so very palpable when observing carefully the great solar spot in November, 1769, and so very frequent, though less obvious, in numberless other spots of a smaller size, which for several years afterwards he examined, prove in the clearest manner that the spots themselves are depressions in the luminous matter of the sun, and lead to many new and interesting ideas concerning the nature and constitution of that stupendous body.

But though he was the first astronomer to whose lot it fell to remark these phænomena of the solar spots which have been just now described, and to draw such important conclusions from them, it appears that the celebrated Mr. Flamstead, so far back as the year 1676, had very nearly anticipated this discovery. For one day when observing a spot of considerable size near the sun's limb, he actually beheld this appearance of the dusky zone which belongs to the nucleus, finding it almost wholly deficient on that side which respected the centre of the disc; and this too when the distance of the spot from the limb corresponded very nearly with that which Dr. Wilson found to be so constant in his observations. Mr. Flamstead was then indeed viewing his spot in peculiar circumstances, and the most favourable of all to perfect vision of the sun, as, by the intervention of a mist, he was enabled to use his telescope without the help of tinged glass put before his eye. The following is his account of this remarkable observation, in which, by the word *macula*, Mr. Flamstead evidently means the nucleus of the spot, and by *nubicula* the dusky zone which surrounds it.

"1676, Nov. 9. Deinde densi adeò vapores excepere solem, ut per ipsos licuit illum nudis oculis intueri. Adhibito tum longiore tubo absque vitro rubro, (quo oculum adversus ejus splendorem munire soleo) maculum contemplatus sum: distincta valdè videbatur, ejusque figuræ quæ in schemate adpingitur: 'Nubecula ipsi circumducta elliptica omninò; *sed, quod valdè miratus sum*, admodum dilatata à parte limbum respiciente; ab altera vero versus centrum, maculæ fere cohærere videbatur.'

"Observavi dein maculæ a limbo proximo distantium 1′ 13″." *Hist. Cælest. Flamsteadii*, vol. prim. p. 363.

When Dr. Wilson saw the great spot on the 23d November, 1769, it had nearly the same situation upon the disk, and the same aspect as the one here described. But, at that time, like Mr. Flamstead, he had no conception of what was signified by such an appearance. It was not till next day, after remarking certain striking alterations of the form both of the nucleus and umbra, that the suggestion first arose in his mind of the spot being an *excavation* or *depression* on the luminous matter of the sun; which idea, the subsequent observations of the same spot most evidently confirmed.

Not long before his death, in turning over at more leisure the pages of this admirable astronomer, Dr. Wilson, for the first time, met with the above passage, and was pleased at finding so remarkable a coincidence as to the leading fact upon which his discovery rests.

Among his papers there were found many letters he had received from Dr. Maskelyne, upon whose correspondence Dr. Wilson set a very high value. All his papers, published in the London Philosphical Transactions, were communicated by that friend. Among these, we find a short one in the volume for 1774, wherein he proposes to diminish the diameter of the finest wires, used in the focus of the astronomical telescope, by flattening them according to a method there described; an idea which, though very simple, seems extremely worthy of attention.

In the month of January, 1777, when conversing, as he often did in the evenings, with his son, who had now made some proficiency in the sciences, their attention was somehow turned to the following query, proposed by Sir Isaac Newton, among many others, at the end of his Optics, namely, "What hinders the fixed stars from falling upon one another?"

In reflecting upon this matter, they readily came to be of opinion, that, if a similar question had been put in respect of the component parts of the solar system, it would have admitted of a very easy solution, on account of *periodical motion* appearing to them as the great means employed by nature for counteracting the power of gravity, and for maintaining the sun and the whole retinue of planets, primary as well as secondary, and of comets, at commodious distances from one another.

In like manner, Dr. Wilson thought it not unreasonable to suppose, that the same principle might have assigned to it a dominion incomparably wider in extent, and that the order and stability, even of a *universe,* and of every individual system comprehended in it, might depend upon *periodical motion* round some grand centre of general gravitation. This conception, besides appearing to them warranted by every view they could take of the nature of gravity, seemed moreover to receive some support from the discoveries which, since the time of the great Halley, have been made of what has been called the "Proper motions of the fixed stars," and particularly from the opinion entertained by that excellent astronomer Dr. Maskelyne, "That, probably, all the stars are continually changing their places by some slow and peculiar motions throughout the mundane space."

Soon after this view had arisen, out of the familiar conversation above-mentioned, it was published in a very short anonymous tract, entitled, "Thoughts on general Gravitation, and Views thence arising as to the State of the Universe." The chief inducement to so early a publication was the hope of drawing immediate attention to so interesting a point, which might possibly lead to the discovery of some way by which the matter might be brought to the test of observation.

It is quite obvious, that the foregoing suggestions necessarily imply a motion of the solar system, as one of that immense host, which, for what we yet know, may be subjected to the laws of periodical revolution. Accordingly, it early occurred, that, perhaps, the most advantageous way of advancing in this investigation, might be to try to find out, if possible, symptoms of such a law as affecting that system to which we ourselves belong.

It sometimes struck him, when looking over the progress of philosophical discovery, that many things of high moment appear to have lain long wrapped up in embryo, by our not employing ourselves more frequently in what may be called "a *direct search,*" and in filling up with more attention and boldness the list of desiderata. Between this last step, and the accomplishment of a profound discovery, he conceived that the transition might sometimes be made with no great effort of invention, by only sifting carefully such principles as are already known and familiar to us, and availing ourselves of them in their full extent.

It was by proceeding in this way, and, when considering the manner by which the motion of light would be affected by reflecting and refracting media, themselves moving with great velocity (a most interesting field in Optics, then wholly uncultivated), that two principles came into view, either of which may possibly serve us in detecting a general motion belonging to the solar system, relatively to the surrounding fixed stars, or in proving a negative with regard to it. Of these, a very summary account has been given in the historical part of the Edinburgh Philosophical Transactions, vol. i. But should they be successful in discovering such a concealed motion, the same principles cannot fail of determining the velocity and direction of it; and, in process of time, whether such a translation of the whole system be in a straight line or a curve, and if in a curve, whether it be of such a kind as may indicate a periodical revolution. And it needs scarce be mentioned, that if such a thing should actually be made out, besides enriching astronomy with that knowledge which depends upon measurable parallaxes in the sphere of the starry firmament; it would also bestow a very high authority upon Dr. Wilson's suggestions, of what possibly may be the plan of Nature in upholding the universe.

At the time of the last-mentioned publication, he was sixty-three years old, but still continued to enjoy the blessings of an uninterrupted state of good health. In the year 1784, at the recommendation of the University, his Majesty was graciously pleased to nominate and appoint Patrick Wilson, AM. Dr. Wilson's second son, to be assistant and successor to his father, as Professor of Practical Astronomy and Observer; a circumstance which heightened the consolations he enjoyed during the evening of life.

In March and April, 1786, when he had nearly completed his seventy-second year, it became apparent to his family and friends that his constitution and strength were fast declining. After a gradual and easy decay, which lasted throughout the whole of that summer and autumn, and which he bore with the utmost composure and resignation, amidst the tender solicitudes of his surrounding family, he at last expired in their arms, on the 16th day of October.

The private character of Dr. Wilson was amiable to an uncommon degree. From his early youth to venerable age he was actuated by a rational and steadfast piety, enlivened by those gracious assurances which carry our hopes and prospects beyond the grave, and sweeten the lot of human life. The cast of his temper, though uniformly cheerful and serene, was yet meek and humble, and his affections flowed in the warmest current immediately from the heart. His looks, as well as his conversation and demeanour, constantly indicated a soul full of innocence and benignity, in harmony with itself, and aspiring to be so with all around it.

Article II.

Incidental Formation of the Compound of Hyponitrous and Sulphuric Acids, lately examined by Dr. Henry. By Mr. Scanlan.

(To the Editors of the *Annals of Philosophy*.)

Gentlemen, *Laboratory, Shaw-street, Sept.* 16, 1826.

A few days since, while preparing nitric acid, from one atom nitre, and two atoms oil of vitriol, sp. gr. 1·812, a compound was formed, which I believe to be the same as that found in the Manchester vitriol chamber, and examined by Dr. Henry.

The distillation was performed in a cast-iron pot, with a stoneware head and connecting pipe, to which was adapted a glass receiver. When about nine-tenths of the acid had distilled over in a continuous stream,* the receiver was changed; it now began to drop very slowly, and was quite green;† the fire was then urged, and suddenly the receiver became lined with a white substance, which, at first, I mistook for the boiling over of the fused bisulphate of potash—an accident which has more than once occurred with me; but on examining more closely, I found the substance to be translucent and crystalline, resembling ice, as it forms on the pane of a window, and I observed, when it came in contact with the liquid acid, it effervesced violently, and the acid did not become muddy.

Although I could not collect enough of the substance to prove its identity with that described by Dr. Henry, yet, I think, there is little doubt that they are the same. A small portion crystallized in the bent tube, connecting the receiver with a Woulfe's bottle; when distilled water was introduced into this tube, nitrous gas was evolved, the instant the water came in contact with the substance, with brisk effervescence, first becoming blue-green at the point of contact; and the resulting solution, which was transparent and colourless, abundantly precipitated solution of nitrate of barytes.

I conceive that the production of this compound, in the present instance, can only be accounted for, by supposing that when the nitric acid had distilled over, the atom of sulphuric acid, constituting a bisulphate, began to act upon the iron, producing sulphurous acid gas, which, coming in contact with nitrous acid, in the atmosphere of the apparatus, gave rise to this substance. The production of sulphurous acid gas would also account for the boiling over before-mentioned; for I find bisulphate of potash to fuse quietly in a glass retort, and to bear

* Sp. gr. 1·455 did not disturb solution of nitrate of barytes.

† Sp. gr. 1·237 contained a good deal of sulphuric acid.

a much higher heat than I have ever applied, in making nitric acid, without parting with any of its acid.

If, with Dr. Henry, we suppose this compound to consist of 5 atoms sulphuric acid + 1 atom of hyponitrous acid, and that, for the decomposition of this atom of hyponitrous acid, the presence of a "contiguous atom of hyponitrous acid" be necessary, we must, of course, suppose the decomposition of two atoms of the compound, which would give 10 atoms sulphuric acid, instead of 5, the number of atoms calculated from the result of analysis. Viewing the constitution of the solid, as Dr. Henry does, we must suppose, when an atom of it is thrown into water, that the atom of hyponitrous acid resolves itself into half an atom of nitrous gas, and half an atom of nitrous acid 15 + 23 = 38, an atom of hyponitrous acid.

M. Scanlan.

Article III.

An Answer to Dr. Christison's "Reply." By R. Phillips, FRS. L. and E. &c.*

Sir,

It is with great reluctance that I feel myself again compelled to address you, but having the choice of submitting to your misrepresentations, or of refuting them, I prefer the latter.

I shall, however, first acknowledge my error in supposing I had stated in my first paper on arsenic that certain fluids could not be decolorized by animal charcoal; it appears I was more careful than I had imagined, not even hinting the poison that might be so disguised as to increase the difficulty of detection, and I certainly did not expect that a Professor of Medical Jurisprudence would supply my deficiency in this respect, but this act was reserved for you; and it seems to me to be what, on another occasion, you term treating the subject "too much as a chemist, and too little as a medical jurist."

As excuses for *boiling* the coloured fluid with the animal charcoal, instead of merely *mixing* them as I had directed, you state that I have used "in one place the ambiguous term digest;" and afterwards alluding to the error which I have now acknowledged, you say, "the readers of the *Annals* will not wonder at my imagining I saw directions to *boil* in the author's injunctions to *mix* and *digest*, when he himself finds a statement of facts where there exists not even a shadow of them."

Now I confess I do not feel the force of this reasoning: I do not see how my having committed a blunder on a certain subject,

* See *Annals*, N. S. vol. vii. p. 30, vol. x. p. 298, vol. xii. p. 23.

is to account for your having months *previously* made two blunders on another.

But to facts.—I deny that I have used the words *mix* and *digest* ambiguously. Alluding in my first paper to the use of animal charcoal, my words are, "I, therefore, mixed some of it with a coloured solution of arsenious acid, &c.; and afterwards supposing it might be suspected that the phosphate of lime of the animal charcoal might have produced by its solution an appearance of the presence of arsenious acid, I state that "I found, however, that water or wine which was merely digested on the animal charcoal produced no effect with nitrate of silver, &c." Now, Sir, observe the charcoal was *mixed* with arsenical fluid, and *only* mixed, that the arsenious acid might not be separated by it. The animal charcoal was *digested* in water and wine, in order to dissolve the phosphate of lime if possible. Had you quoted this passage, you would not have ventured to assert that I had directed animal charcoal to be *digested* in a fluid containing arsenious acid, for none was present, and the object in digesting was totally different.

Again, you represent me as having employed a decolorized solution containing a grain of arsenious acid per ounce, when I have distinctly stated that "the silver test readily detected the arsenic when so far diluted as to form only $\frac{1}{10000}$ of the solution." Nor is this all: you insinuate that I could not distinguish between the green colour produced by adventitious matter from that obtained by the combination of arsenious acid and oxide of copper; but if you had attended to the precaution which I recommend of first precipitating the hydrate of copper, and then adding the solution of arsenic, you would, I think, have seen that I could not have made this mistake.

I have already admitted my error in supposing that I had used a precaution which I did not employ; an imperfect recollection of what I had written is, however, a mistake which I trust you will be inclined readily to pardon, for it is one into which you have yourself fallen. You say in your reply, that with respect to authors who treat of medical jurisprudence, you "unfortunately wrote *most* authors instead of *some* authors." The construction of your sentence is such, however, as to show that the error was of a more deliberate nature, and not a mere slip of the pen. You "unfortunately," for the latter supposition, wrote "almost all authors."

In my remarks upon your paper, I have noticed your assertion, that "the charcoal of the black flux is not necessary in the process [of reduction], and subcarbonate of potash might, therefore, answer as well, but it is seldom so dry."

In your reply, you say, "I meant the process *in the text*,—the process for reducing the sulphuret; and I distinctly remember that my reason for introducing the clause was, that if I did not, some critic might accuse me of thinking charcoal necessary for the process." My criticism takes another direction than that which you apprehended; for I assert, although you are unacquainted with the fact, that charcoal is as necessary in reducing the sulphuret of arsenic as the oxide; and the experiment upon which this conclusion is founded is this:—I mixed one part of orpiment with about two parts of carbonate of soda; the mixture was subjected to a red heat until all fumes ceased. On examining the residual mass, I found it to consist of carbonate, sulphate, and arseniate of soda, and consequently the whole of the arsenic cannot be procured by means of an alkali without the assistance of charcoal, whether we act upon the oxide or sulphuret.

In concluding, I again readily acquit you of any intentional misrepresentation; for the carelessness you have repeatedly evinced is quite sufficient to account for the errors you have committed. Your obedient servant, R. PHILLIPS.

ARTICLE IV.

Telescopical Observations on the Moon. By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

THE moon was one of the first objects examined by the telescope. Galileo perceived so striking a resemblance to that appearance which the earth may be supposed to have, if viewed from a great distance, as to conclude, according to Anaxagoras, Pythagoras, and other ancient philosophers, "Lunam scilicet esse quasi tellurem alteram, ejus pars lucidior terrenem superficiem, obscurior vero aqueam congruè repræsentet." (Sidereus Nuncius, p. 17.) Galileo not only observed mountains, but measured the altitudes of some; and assigns a reason why he could not perceive any upon the limb (p. 22, 23); had his telescope been more powerful, they would have been distinctly visible. Since this time, the speculations of philosophers have been very various; Hevelius retained the ancient opinion, and supported it by many careful observations; but at the present day, astronomers usually maintain that the moon is destitute both of water and of an atmosphere; some making its surface one mass of volcanic craters (Robison's Mechanical Philosophy, p. 562); whilst others, in order to explain the origin of meteoric stones, fancy that the moon is composed of the bases of alkalies and earths in their metallic state; by some unaccountable process, masses of lunar matter are some how projected beyond the point where the centripetal force of the earth is equal to that of the moon; continually descending, they reach our atmosphere, inflame and descend to the earth; these productions we mortals call meteoric stones, and are reminded of the illustrious Baron Munchausen's origin of volcanic eruptions. All might fit together very well, were the data good. That there is an atmosphere appears highly probable, because volcanic eruptions have been frequently observed; or at least, phænomena have been seen which cannot be explained upon any other hypothesis; the late Sir W. Herschel observed three such phænomena, April 19, 1787; also May 4, 1783; a similar, but more remarkable appearance presented itself March 7, 1794; April 13, 1793, and Feb. 5, 1794, Mr. Piazzi observed a similar phænomenon; since this time, others have been observed, one by Capt. Kater, and one by myself, Jan. 22, 1825, in the same place as those observed by Piazzi, near the spot called Aristarchus by Cassini, and Mons Porphyrites, by Hevelius.

In reasoning upon inaccessible objects, we must argue from what we know; and since, on the earth, fire cannot be maintained without air, we have a right to make the same assumption respecting the moon: hence then these appearances render the existence of an atmosphere highly probable. Mr. Schroeter endeavoured to establish the existence of a lunar atmosphere, by the prolongation of the cusps, when the moon is two or three days old. At an occultation of Jupiter's satellites, the third became indistinct, 1^s or 2^s before it disappeared: the fourth became invisible when near the limb. (Phil. Trans. 1792.)

Some astronomers have found that stars disappeared suddenly behind the moon, without undergoing any previous change; whilst others observed a sensible diminution of brilliancy. Now if the subject be fairly examined, we shall find that every occultation is not suitable for the purpose; for, first, except near the change, the moon's light is so great that small changes in the brilliancy of a star cannot be easily observed: secondly, the moon's atmosphere must be of small extent, and very rare; for, according to Dr. Wollaston (Phil. Trans. 1822), were the earth's atmosphere of infinite extent, the density of the atmosphere of each planet would be equal to that of the earth's, at distances which are proportional to the square roots of their quantities of matter: hence in this extreme case, the lunar atmosphere must be very rare; but its density is far less than even this quantity: thirdly, for both these reasons, if a star pass nearly perpendicularly into the atmosphere; i. e. if it be very nearly in the path of the moon's centre, the trifling effect that may reasonably be expected cannot continue longer than one or two seconds of time: on the contrary, if the star be very nearly the moon's semidiameter on either side of the path of the centre, the effect of the atmosphere will be chiefly visible in elevating the star, or in keeping it in view for some seconds after it has been seen to touch the limb. In a number of the *Annals* for the beginning of the year 1819, I described a very satisfactory observation. On May 12, 1826, the star 5 ♋ was similarly eclipsed: the conclusions were less satisfactory, because the star was eclipsed during a considerably longer time; it touched the dark part of the globe near the north horn; before it was quite in contact, it was elongated in a direction perpendicular to a tangent at the point of contact; it lost a sensible part of its brilliancy before it disappeared. Should a total eclipse happen, when the moon is near the meridian, and in the milky way, or any crowded part of the heavens, occultations of every sort, and very close appulses, might be observed, whereby the question may be decided.

Results of a rather satisfactory nature were obtained from the occultation of Saturn, Oct. 30, 1825. At the immersion, the moon's altitude being very low, the wind high, and the air very tremulous, a good observation could not be made. The emersion happened under better circumstances; it took place from behind the dark part of the disc; and when a very minute part of the Wansa first appeared, it was very considerably agitated; whilst some minute insulated specks on the dark part of the moon were quite steady, and in the field of view at the same time. As more of the ring came into view, the part which was very near the moon was indistinct; and when the globe of the planet had emerged, and a vacant space of two or three seconds of a degree between it and the moon ought to have been seen, the globe of Saturn was extended to the moon's limb, and indistinct on part of that hemisphere; and as the remainder of the ring emerged, the part near the moon was confused and indistinct, whilst the remote parts were distinct and well defined. The observation was made with a six inch Newtonian reflector, power 130; a higher could not be used on account of the unfavourable state of the air. Saturn's light being inflected by passing by the moon's surface, some part of the appearance must have resulted therefrom; and although it is difficult, or rather impossible to calculate the effect due to inflexion, yet, reasoning from analogy, the effects appear too considerable to be the effect of inflection alone.

We may now examine how far observation supports the idea that there are seas in the moon. There is certainly one great difficulty, although some of those parts which are supposed to be seas are favourably situated, the reflected image of the sun has never been observed. However if the moon be similarly constituted to the earth, the surface of the sea will rarely be smooth; and whether the trifling quantity of light reflected from the surface of water, even if smooth, would be visible at so great a distance as 240,000 miles, is doubtful; and if rough, undoubtedly its dissipation must be so great that the sun's image cannot be visible on the earth. If parts of the moon be covered with water, they will appear darker than the other parts of the surface, except in that point which reflects an image of the sun, if such there be; for, excepting at that point which must be very small, none of the reflected rays can reach the eye of the observer: on the earth, the waters of the ocean are perfectly clear; the bed of the sea is illuminated by the sun; but of the light which falls upon water, part is reflected; of the rays which pass below the surface, many are absorbed; and of those which are reflected from the bed of the sea, many are absorbed by passing through a great extent of water: therefore the bed of the sea will receive and reflect fewer rays than the surface of the moon; hence seas will appear darker than the land. Again, when the line which separates the illuminated from the dark hemisphere, passes over the bright parts of the moon, it is rugged and broken; the shadows of elevated parts are very black and sharply defined; but when it crosses the dark parts, the phænomena are very different; the line is in general a smooth elliptical curve; it is shaded off very gradually, whereby it almost imperceptibly terminates in perfect darkness. There are irregularities in it, but they are of a different form to those which present themselves on the bright part of the disc; these irregularities are of the following sorts: 1. Long, narrow, dusky ridges; smoothly and softly defined; not broken angularly; windings always rounded; they have often branches; sometimes one side has a whitish brilliancy; in this case, a shadow is projected by the other, which shadow is very gradually softened at its margin. These ridges are very distinct and numerous in Mare Mediterraneum, when the moon is at the ages represented in maps 13, 14, 15, of Hevelius's Selenographia. 2. Bright spots which project considerable shadows. If these be carefully examined, they will be found to be very considerable elevations: the shadows are remarkably soft, and destitute of the sharpness of those on the brighter parts: if these elevations have cavities within them, the shadow of the margin, which is projected into the cavity, is sometimes very intense and sharply defined; the altitude of such spots is great. These may be seen at the ages represented in the following maps of the Selenographia; between 7 and 8 in Pontus Euxinus; in 12, 13, 14, these phænomena may be observed in M. Christi, J. Ebissus, J. Minorca; in 16 in Corsica and Sardinia, and in many other parts. 3. Spots which have less brilliancy than the part of the moon, called land, by Hevelius, and greater than that which he supposes to be water. Some such appear in Palus Mœotis; Erichtini Scopuli in Pontus Euxinus are remarkable; at the age, map 14 to 17, many such may be seen in Pontus Euxinus, Mare Mediterraneum, Mare Hyperboreorum, Mare Pamphilium, &c.: most of these project the soft shadows before named. A tolerably good idea of the difference between the effect of the bright and dark parts of the lunar disc may be formed by examining Palus Mœotis in map 2; Pontus Euxinus and S. Extremus Ponti in 6; Pontus Euxinus in 7, 8, 9; Mare Hyperboreorum, Mare Mediterraneum, and Mare Adriaticum in 12; Mare Mediterraneum in 15, 32, 34, 35, 36; but if the moon be examined with a good and powerful telescope, the appearances here noticed can scarcely fail to convey the idea, that the earth, viewed from a great distance, would present the same general aspect. If an observer stand about half way up a very high cliff, whose shadow is projected upon the sea, he will be sheltered from the sun's rays: the water being clear, the bed of the sea will be illuminated, but less so than the neighbouring land; the shadows of elevated places have the soft appearance observed in some parts of the moon; whilst the sea appears dark. There is no doubt, or at least I have none, from the observations I have made, but that the ridges and other supposed submarine objects are sometimes more distinct and more conspicuous than at others, the air being quite clear, and the moon at the same age: this I have especially noticed in Palus Mœotis, a part of the moon well adapted to observations of this sort. As an additional confirmation, the surface of each of the dark parts must be smooth and spherical; else how can the terminator appear smooth and elliptical in passing over them?

There are parts of the moon which have the appearance of rivers and small lakes; rivers, if there be any, cannot be traced through their whole length; because, at least on the earth, at a great distance from the sea, their breadth is too small to be seen at so great a distance as that of the moon. I am aware that some of your readers may be inclined to regard this statement as visionary; I only beg such to examine Palus Mœotis with a large telescope, if possible, one which bears a power of 400 or 500, with sufficient light; the telescope should be so firmly mounted as not to be affected by tremors, and the air should be clear; the best time is when the moon is about eight or nine days old; on the north margin will be found a conspicuous narrow line, running nearly parallel with a small part of the north-west, the whole of the north, and part of the north-east boundary of Mœotis, and joined to it by four similar lines which increase in breadth as they approach it; to the east is another such line, whose south extremity divides into two short branches, one of which touches a mountain which stands at the point where Palus Byces joins L. Corocondametis; on the south-west, from the end of the projection joining Pr. Agarum, another rises, joined by a second, a little to the south; after the junction, the branch takes a direction little west of the south; its extremity is divided into four branches which run towards Paludes. I had a very distinct view of these parts, June 13, 1826, as well as at other times before and since; they are seen to great advantage with the six inch reflector, and powers from 200 to 400, and when the atmosphere is in a very fine state with 800: I have seen them very well with my 40 feet aerial, power 200. A drawing will be sent for insertion in the *Annals*, when I shall have had opportunity to make observations during one or two more lunations. If astronomers were to divide the lunar disc into a number of small portions, and each to observe his own part regularly, using large and powerful instruments, I am certain, and I speak from a long continued series of observations with large telescopes, that much valuable knowledge may be obtained. I should have great pleasure in taking a portion of the moon along with other astronomers.

J. B. Emmett.

Article V.

Report of the Length of the Pendulum at the Equator. By John Goldingham, Esq. FRS. *From Experiments and Observations made on an Expedition fitted out under his Direction, from the Observatory at Madras: by Order of the Madras Government in the Year* 1821. *Together with a Deduction of the Figure of the Earth, by combining the Equator, Madras, and London Experiments. Also the Geographical Situation of different Places seen on the Expedition.*

(*Concluded from* p. 299.)

On the 22d of December, the observations, having more immediate reference to the primary object of the expedition, may be said to have commenced. At the island Gaunsah Lout, a slight shock of an earthquake was felt on the 30th of December, which lasted about half a minute. On the 10th of January, Captain Crisp arrived from Nattal, finding the pillar commenced, and the observations for the latitude, time, and longitude going on. On the 21st, Captain Crisp arrived again from Nattal, and on the 23d proceeded to Ayr Bongy: the conductor, whose health rendered it necessary, left the small island for Pulo Panjong. The pillar, having the plugs of teak-wood inserted according to the instructions, was finished on the 27th; there not having been bricks sufficient, the foundation and back-part of the pillar were formed of pieces of the coral-rock, which was found to be very hard, and at least equal to brick for the purpose. On the 31st of January, the plank and frame for supporting the pendulum of experiment were firmly secured in their places; the clock-case was then screwed to the plank. On the 2d of February, Captain Crisp arrivel from Nattal; the works of the clock were put up, and on the 5th, observations taken for finding its rate. On the 7th, Captain Crisp returned to Nattal in the brig Eleanor, having seen that every thing was in a proper train for commencing the experiments with the pendulum. On the 11th, three of the savage inhabitants of these parts, who came here under the pretence, or for the purpose, of fishing, got into the large tent, and took from thence the transit instrument, azimuth compass, circumferentor, and a small box belonging to Captain Crisp, thinking, as it would appear, that the brass parts of the instruments were gold.

On the 13th of February, Messrs. Robinson and Lawrence commenced the experiments with the pendulum, each taking separate sets. On the 17th, a boat, with provisions* for the party, arrived from Nattal: some of the islands adjacent to Gaunsah Lout were now visited by a few of the savage inhabitants under the pretence of collecting turtle—the party, which consisted of Messrs. Robinson and Lawrence, the sub-conductor, and five Lascars, two of these ill of fever, thinking themselves a good deal exposed, added to what had before occurred, deemed it prudent to keep a look-out during the night, and to establish an armed watch, who proceeded regularly round the island after dark until day-light; Captain Crisp having been made acquainted with the loss of the instruments, sepoys were despatched to the different islands and places, and search made for them, but without success. The loss of the transit is much to be regretted, as although every precaution was taken to obtain a correct rate by the sun, still the verification of this rate by the stars would have been very satisfactory.

The experiments with the pendulum were completed on the 20th of March, and, as will be seen, were very numerous, as well as the observations for the latitude; the other observations, as directed in the instructions, were also taken. On the 20th, Captain Crisp arrived from Nattal, and on the 21st ordered the clock and pendulum of experiment to be taken down, and replaced in their cases. On the 23d A. M. the instruments, tents, and baggage, having been sent on board the Eleanor, Messrs. Robinson and Lawrence, with the sub-conductor and Lascars, embarked on that vessel, and sailed for Pulo Pinnee, where they anchored in the former birth. On the 24th, Captain Crisp, who had been to Pulo Batooa, arrived, and the Eleanor sailed for Nattal—Mr. Lawrence and the sub-conductor proceeded to Nattal on the small boat, where they arrived the same night. On the 28th, the instruments and some of the baggage were transhipped to the brig Sophia belonging to Mr. Prince; some observations were made during their stay here. On the 29th, Captain Crisp and family came on board the Sophia, where also was Mr. Lawrence; and about seven P. M. the vessel was got under weigh, and sailed for Bencoolen; the Eleanor, with the remainder of the party, sailed, at the same time, for Pulo Panjong and Padang, at the former place to take up the conductor, Mr. Hamilton, and to lay down the geographical position of the latter; the Sophia reached Bencoolen on the 9th of April, when the instruments and baggage were landed; the brig Eleanor

* The food of the party consisted chiefly of salt-fish and rice, with now and then a fowl; they had no means of fishing, and most of them, indeed, no time.

arrived four days afterwards: the requisite observations for the time were taken; and, after waiting about 30 days for a passage, the whole party embarked in the ship Eleanor for Madras, and arrived in the roads on the 4th of June.

I shall now proceed to give the observations and experiments.

It may be necessary to premise, that the following tables contain only the observations which were considered to be the most accurate. The method of proving them having been by calculating the whole of the observations, then taking the mean —and any of the results which differed more than a given quantity from that mean, were not retained.* Of the experiments with the pendulum, I have not deemed it proper to admit those made on the days when the clock was wound up; as it appears, that the machinery for keeping the works in motion while the clock is winding did not act, and that at such times the second hand was also liable to go back; it is true the clock was compared both before winding and after, with a chronometer; but in an inquiry of this nature, where it was required to have the rate of the clock to a very small fraction of a second, I thought it better to reject altogether the experiments made on the days mentioned, particularly as the remaining observations were so very numerous. The whole of the original observations and experiments are preserved, however, in the books at the Observatory. None of the pendulum experiments have been rejected, except those just noticed, as having been taken on the days when the clock was wound up; this was once a week.†

The foregoing are all the observations from which the conclusions will be drawn; these I have endeavoured should be arranged in as clear a manner as possible; in order that any person so disposed may be enabled to examine the foundation on which the conclusions rest, and to go over the calculations.

We now proceed to the computation of the experiments and observations. The results only of the different operations are contained in the tables which follow: for the performance of these operations the Honourable the Governor in Council was pleased to allow me a continuance of the aid of the two observers, Robinson and Lawrence. The rules for reducing the experiments having been furnished them step by step as they proceeded, each calculating the experiments and observations himself had made, and the results were not entered, until the operations had been twice gone over, and then examined; the

* Of the observations for the latitude of Po. Gaunsah Lout, where the experiments for ascertaining the length of the pendulum were made, I have not admitted those which differed more than 15 seconds from the mean of the whole. Nearly all the observations taken there by Assistant Lawrence stood this test, and very few were rejected. At other places where the observations were less numerous, I have admitted all, not differing more than 25, and in some cases 30 seconds from the mean.

† The tables here given in the original report, occupying 86 folio pages, are necessarily omitted. *Edit.*

details of the calculations are of course not inserted, as these alone would occupy a very large volume. The calculations for finding the specific gravity of the pendulum for the correction on account of the buoyancy of the atmosphere, for the height of the pendulum above the level of the sea, for finding the length of the pendulum at the equator, and for the ellipticity, have also been gone over by E. Lake, Esq. of the Madras Engineers, who was so good as to undertake this service; and as in nice and difficult computations of this nature, the more calculators there are, independent of each other, the better, I was much obliged, and I may add gratified, by his having done so.

I have commenced with the pendulum experiments as being the most important; the first table shows the rate of the clock while those experiments were making, the same which is used in the deductions. The series of each observer I have divided into four sets; between the second and third sets, the apparatus was purposely put completely out of adjustment, and set right again; between the other sets, the apparatus was examined from time to time, and any trifling adjustment made that was required.*

Remark.—Great changes in the atmosphere took place, it will be seen, during the time these experiments were making. In the course of a day, we find the thermometer at the commencement of the experiments in the morning at 79° or 80°; the hygrometer from 24° to 35° moist; † and in the afternoon of the same day, while the experiments were going on, the heat increased to 96° or 97°, the hygrometer at the same time showing from 30° to 48° dry; and considerable variations, though not quite to so great an extent as these, are common; the nights being generally cold with dew sometimes, and sometimes rain falling: rain also fell on some of the days.

Although the Observatory was of treble tent-cloth, and the part above the apparatus covered with a large tarpaulin, it was to be expected that the pendulum must have felt such changes as these, and in a degree not shown, I imagine, by the instruments used for reducing the experiments. The only corrections now required are for the buoyancy of the atmosphere, and for the height of the pendulum, above the level of the sea; that for the latter will be the same for all the sets, and the heights of the barometer used for the former of these corrections, vary but little in the four sets; but it will be seen, almost invariably, when the hygrometer showed the atmosphere to be damp, the vibrations in 24 hours were greater than when it was in a dry

* The tables here referred to, which occupy 72 pages, we also omit. *Edit.*

† It is to be regretted that Mr. Goldingham has not stated the nature of the hygrometer he employed; as little or no use can be made of the hygrometrical observations, on that account. A similar omission renders it impossible to deduce from Mr. Goldingham's experiments on the velocity of sound, given in a former number of the *Annals*, from the Phil. Trans., the influence of the humidity of the atmosphere on sound; for which it is the only datum omitted in the paper. *Edit.*

state. The observations commenced early in the day, after the pendulum had been under the influence of the chill and damp of the night, and I am led to think retained a much greater degree of cold, than was exhibited by the thermometer, particularly during the time the moisture it might have collected was evaporating by the influence of the sun. Hence the greater number of vibrations at such times. Again, great heat in the middle of the day might be retained by the metal of which the pendulum is composed, in a greater degree, and longer than the air. Hence the greater the expansion of the pendulum, and the consequent fewer number of vibrations in 24 hours, than would have been shown under a more equable state of the atmosphere. The differences, exclusive of such parts as are attributable to the different heights of the barometer, though very obvious, are not large; and if the changes in the atmosphere which we have noticed give in one case too small a number of vibrations, and in the other a number too large, the mean will probably show as correct a result as could be obtained under the most favourable circumstances. Before closing this remark, I shall take the opportunity to state, that the barometer sent with the expedition, though the best to be procured here, and a good one, as far as relates to the rise and fall of the mercury; yet from some general defect (in the scale probably) showed the height less than it ought; and by comparisons with the Observatory barometer, both before the expedition left Madras, and after its return, the exact difference was 0·103 inch: this correction has accordingly been applied to all the heights of the barometer registered with the experiments and observations.*

We shall now proceed to apply the remaining corrections. The specific gravity of the pendulum was ascertained for the Madras experiments in the manner stated in my paper published in the Philosophical Transactions; † I was anxious to have this verified, and, if possible, by a more accurate apparatus than I was able to submit the pendulum to before; this it occurred to me might probably be done at the Mint; and I accordingly addressed the Honourable the Governor in Council upon the subject; an order was immediately sent to the Assay-Master, Dr. Aitken, to have the operation performed; this was most carefully done, and the mean result of three trials, on different days, was as follows:—Thermometer 91°; barometer 29·9551; specific gravity 7·966.

Distilled water was used on the first two days, and rain water on the third day; the results however differed very little. The

* Some experiments which I have since made at the Observatory confirm the opinion above given; during the time, however, of making the experiments, although great changes took place in the thermometer, there were none in the hygrometer; and it appears to have been when there were considerable changes in the latter, that the greatest effect of the atmosphere was felt by the pendulum at the island.

† A notice of this paper will be found in the *Annals*, N. S. vol. iv. p. 382.

specific gravity of the pendulum formerly found, the thermometer having been 88° and the barometer 30·064, was 8·1085; the difference, therefore, is not very considerable; but I shall of course use the Mint specific gravity in the following computations, and also by it re-calculate the correction for the buoyancy of the atmosphere, applied to the experiments for finding the length of the pendulum at Madras.

The weight of water to that of air at 53° of the thermometer, and 29·27 inches of the barometer, is as 836 to 1, the expansion of air for each degree of the thermometer being $\frac{1}{480}$ of its bulk. The specific gravity of the pendulum at that height of the thermometer and barometer will, therefore, be 7·5414.

Corrections for the Buoyancy of the Atmosphere.

First set (L).

Therm.	Barom.	Vibrations in 24 hours.
88·63	30·151	86158·674
53·0	29·270	
35·63		

Then 1·7417 × 35·63 = ... 62·0568
30·151 : 836 :: 29·270 : .... 811·5724

873·6292

836 : 7·5414 :: 873·6292 : 7·8808 specific gravity. And 873·6292 × 7·8808 = 6884·8970 the pendulum heavier than air.

Square of the number of vibrations 86158·674 = 7423317105, 4383 divided by 6884·8970 = 1078203·073 × the square of the number of vibrations in 24 hours, and the square root of the same, gives the number of vibrations in 24 hours, corrected for the buoyancy of the atmosphere = 86164·931 or + 6·257 vibrations for the correction.

In like manner, the corrections for the buoyancy of the atmosphere were found for the other sets (L), and are for the second set + 6·254; for the third + 6·333; and for the fourth + 6·269.

The corrected number of vibrations in 24 hours will, therefore, be; first set 86164·931; second 86165·966; third 86167·111; fourth 86167·233.

Following the same process with the results (R), we shall have the correction on account of the buoyancy of the atmosphere for the first set 6·2892; for the second 6·2685; for the third 6·3486; and for the fourth 6·2759.

And the corrected number of vibrations; first set 86165·0772; second 86165·9115; third 86167·3836; and the fourth 86167·3659.

It now only remains to ascertain and apply the correction for the height of the pendulum above the level of the sea. The height of the pendulum above the level of the sea at low water was 12½ feet, or 0·00236742 of a mile; the radius of the earth at the equator being allowed 3967·5 miles. Hence the correction for the height of the pendulum above the level of the sea will be + 0·0514 of a vibration.

The true number of vibrations in 24 hours will, therefore, be:

First set (L)	86164·9822
Second	86166·0177
Third	86167·1624
Fourth	86167·2848
First set (R)	86165·1286
Second	86165·9629
Third	86167·4350
Fourth	86167·4173

In London, latitude 51° 31′ 8·4″, the pendulum of experiment made 86293·14 vibrations in 24 hours, the thermometer being 67·6, and the barometer 29·97 inches; the height above the level of the sea 83 feet. Hence the correction for the buoyancy of the atmosphere, allowing the Mint specific gravity, will be + 6·7478; and that for the height above the sea + 0·22: the true number of vibrations in 24 hours will, therefore, be 86300·1078, the square of which is 7447708606·2916.

The length of the seconds pendulum in London at the temperature of 70°, deduced from Capt. Kater's experiments, is 39·142434 inches.

Now 86300·1078^2 : 86400^2 :: 39·142434 : 39·23310101 inches, the length of the pendulum of experiment.

Then by the first set (L) 86400^2 : 86164·9822^2 :: 39·23310101 : 39·0199543268 the length of the pendulum at Gaunsah Lout.

By the same operation, the length of the pendulum at Gaunsah Lout by the second set (L) is 39·0208915108; by the third 39·0219289661; by the fourth 39·0220398271 inches.

Repeating the operation, the first set (R) gives the length of the pendulum at Gaunsah Lout 39·0200869218; the second 39·0208425551; the third 39·0221758674; and the fourth set 39·02215983605 inches.

The mean of these will be:

Sets (L)	First	39·0199543268	
	Second	39·0208915108	
		Mean	39·0204229188
	Third	39·0219289661	
	Fourth	39·0220398271	
			39·0219843966
		Mean of the four sets	39·0212036577

Also,

Sets (R)	First	39·0200869218	
	Second. ..	39·0208425551	
		Mean.	39·020464738
	Third.	39·0221758674	
	Fourth	39·02215983605	
		Mean.	39·0221678517
		Mean of the four sets (R)	39·0213162951
		Mean of the four sets (L)	39·0212036577

Length of the pendulum at Pulo Gaunsah Lout by the mean of all the experiments, 39·0212599764 inches.

The latitude of Pulo Gaunsah Lout, according to the mean of the meridian observations, as hereafter deduced, is 0° 1′ 48·78″ north.

Then by combining the London observations and those at Gaunsah Lout, we have the length of the pendulum at the Equator 39·02125994 inches.

We now proceed to deduce the length of the pendulum at Madras by the specific gravity found at the Mint.

During the time of taking the first series of experiments at Madras, the mean height of the thermometer was 83·48°, of the barometer 30·121, the number of vibrations in 24 hours 86166·108. Hence we find that the correction for the buoyancy of the atmosphere is + 6·376. The mean height of the thermometer during the time of taking the second series was 85·49°, of the barometer 30·258, the number of vibrations in 24 hours 86166·048; and the correction now deduced for the buoyancy of the atmosphere, + 6·378. The correction for the height above the level of the sea, as formerly found, is + 0·095. These corrections being applied, we shall find the true number of vibrations in 24 hours by the first series 86172·579; and by the second series 86172·521; the mean being 86172·550.

Hence the length of the pendulum at the Madras Observatory, in latitude 13° 4′ 9·1″ north, by the first series, will be 39·0268350769; and by the second series 39·0267825415; the mean of both being 39·0268088092 inches.

Then by combining the London with the Madras experiments, and taking the length of the pendulum at the Equator, deduced from the Gaunsah Lout experiments,* we find the diminution of gravity from the Pole to the Equator to be ·0052756159; and the ellipticity of the earth $\frac{1}{296 \cdot 61}$.

* The length of the pendulum at the Equator by computation, with the data here given, will be 39·01628254 inches; differing from the measurement 0·00497740 of an inch; the diminution of gravity from the Pole to the Equator, using the computed length of the pendulum will be ·00527629, and the ellipticity of the earth $\frac{1}{296 \cdot 57}$.

The Latitude and Longitude of some of the Places on the West Coast of Sumatra, and the Islands in the Vicinity of Nattal, deduced from the Station of Pulo Gaunsah Lout, in Latitude 0° 01′ 49″ N. and Longitude 98° 50′ 06″ E.

Station at	Names of places.	Bearings referred to the meridian.	Distances direct.	Distances on the Meridian.	Distances on the Perpendicular.	Latitude.	Longitude from Pulo Gaunsah Lout.	Longitude from Greenwich.
			Feet.	Feet.	Feet.			
Pulo Gaunsah Lout.	Pulo Pinnee..........	57° 37′ 02″ NW	32163	17226 N	27162 W	0° 04′ 40″ N	0° 04′ 28″ W	98° 47′ 17″ E
	Pulo Oolur...........	30 34 03	23588	20310	11996	0 05 11	0 01 58	98 48 09
	Pulo Gaunsah	1 24 58 NE	6609	6607	163 E	0 02 55	0 0 02 E	98 50 09
	Nellang Hill..........	32 47 58	134020	112653	72599	0 20 27	0 11 56	99 02 03
	Oojong Twan	44 09 58	110979	79608	77324	0 14 59	0 12 42	99 02 49
	Lubwaun Looloo......	49 34 58	114334	74128	87048	0 14 05	0 14 18	99 04 25
	Toolechemanah.......	57 44 58	128109	68362	108345	0 13 07	0 17 48	99 07 55
	Pungalauren	63 22 28	148005	66330	132310	0 12 47	0 21 44	99 11 51
	Kurboyee Hill.... ...	74 33 28	189341	50415	182506	0 10 0	0 29 59	99 20 06
	Mount Ophir.	87 28 07	432663	19109	432241	0 04 59	1 10 52	100 00 59
	Pk. NE of Ar. Bonghy	58 32 58	318650	166260	271838	0 29 17	0 44 40	99 34 47

The Latitude and Longitude of Places in the Vicinity of Bencoolen, deduced from Fort Marlborough Turret, in Latitude 3° 47′ 31 S. and Longitude 102° 15′ 44″ E.

Stations at	Names of places.	Bearings referred to the meridian of Fort Marlborough turret.	Distances.	Distance on the Meridian.	Distance on the Perpendicular.	Latitude.	Longitude from Fort Marlborough turret.	Longitude from Greenwich.
			Feet.	Feet.	Feet.			
Fort Marlborough.	Rat Island.	55° 0′ 15″ SW	31202	17895 S	25560 W	3° 50′ 29″ S	0° 4′ 12″ W	102° 11′ 32″ E
	N. Peak, or Lye Peak. .	1 11 30 NW	197808	197766 N	4114 W	3 14 48	0 0 41	102 15 03
	Goonum Pallee	13 9 15 NE	148863	144858	34298 E	3 23 33	0 5 39 E	102 21 23
	Goonum Benko	41 19 30	104656	78594	69107 E	3 34 31	0 11 22	102 27 06
	Lion's Rump	68 45 0 SE	69558	25210 S	64829 E	3 51 41	0 10 39	102 26 23
	Mr. Presgrave's Tree . .	32 14 15 SW	2381	2014	1270 W	3 47 51	0 0 13 W	102 15 31
Rat Island.	W. end of the base	52 28 59 SE	42389	25815	33622 E	3 54 45 S	0 1 20 E	102 17 04 E
	E. end of the base.	56 37 46 SE	43930	24164	36627 E	3 54 28 S	0 1 50	102 17 34
W. end of the base.	Hamilton's Tomb.	3 30 14 NW	37424	37354	2287 W	3 48 34 S	0 0 57	102 16 41
	Argyle Hill.	50 26 01 NE	35256	22457	27179 E	3 51 02 S	0 5 47	102 21 31

The Latitude and Longitude of Places on the West Coast of Sumatra, about Ayer Bonghy and the adjacent Islands, deduced from the Station at Pulo Panjong, in Latitude 0° 11′ 22″ N. and Longitude 99° 17′ 10″ E.

Station at	Names of places.	Bearings referred to the meridian.	Distances direct.	Distances on the Meridian.	Distances on the Perpendicular.	Latitude.	Longitude from Pulo Panjong.	Longitude from Greenwich.
			Feet.	Feet.	Feet.			
Pulo Panjong.	Pulo Telloor..........	37° 34′ 55″ SE	31284	24792 S	19080 E	0° 07′ 16″ N	0° 03′ 15″ E	99° 20′ 25″ E
	Pulo Pahgaugo	68 17 35 SW	16262	6014	15109 W	0 10 22	0 02 26 W	99 15 32
	Ayer Bonghy Hill	89 08 35 NE	23718	355 N	23715 E	0 11 26	0 03 54 E	99 21 04
	Lubwaun Hill	60 03 05	23356	11660	20237	0 13 18	0 03 19	99 20 29
	Goonum Allyee.	66 41 05	30257	11975	27786	0 13 21	0 04 34	99 21 44
	Mount Ophir.........	81 37 05 SE	264974	38600 S	262147	0 04 58	0 43 05	100 00 15
	High Peak on the Range	49 04 05 NE	170022	111392 N	128450	0 29 48	0 21 06	99 38 16
	Ayer Bonghy	85 06 05	23888	2040	23801	0 11 42	0 03 55	99 21 05

Table of Latitudes and Longitudes of some of the remarkable Places on the West Coast of Sumatra, of prominent Points on the Ranges of Mountains, and of some of the Islands about the Equator.

Names of places.	Latitude.	Longitude from Madras Observatory.	Longitude from Greenwich.	Remarks.
Bencoolen	3° 47′ 31″ S	21° 58′ 23″ E	102° 15′ 44″ E	Fort Marlborough turret.
Rat Island	3 50 29	21 54 11	102 11 32	Station near the wharf.
Conspicuous Hill	3 23 33	22 04 02	102 21 23	Centre.
Sugar Loaf Peak	3 34 31	22 09 45	102 27 06	
Lye Peak	3 14 48	21 57 42	102 15 03	
Lion's Rump	3 51 41	22 09 02	102 26 23	Centre.
Mr. Presgrave's Garden	3 47 51	21 58 10	102 15 31	Staff on a tree.
Hamilton's Tomb	3 48 34	21 59 43	102 17 04	Tomb.
Argyle Hill	3 51 02	22 04 10	102 21 31	Mark.
Tappanooly Island	1 43 47 N	18 23 56	98 41 17	Staff on the height.
Sugar Loaf Peak (Island)	1 34 54	18 20 26	98 37 47	
Natal	0 33 26	18 44 20	99 01 41	Station NE of the Fort.
Pulo Panjong	0 11 22	18 59 49	99 17 10	Station east of the Bungalow.
Pulo Pahgaugo	0 10 22	18 57 23	99 14 44	Centre of the island.
Pulo Telloor	0 07 16	19 03 04	99 20 25	Station on the island.
Ayer Bonghy Hill	0 11 26	19 03 43	99 21 04	Mark.
Lubwaun Hill	0 13 18	19 03 08	99 20 29	Bush.
Goonum Allyee	0 13 21	19 04 23	99 21 44	Highest part of the range.
Mount Ophir	0 04 58	19 43 27	100 00 48	Remarkable peak.
High Peak on the Range	0 29 48	19 20 55	99 38 16	
Ayer Bonghy	0 11 42	19 03 44	99 21 05	Station north of the staff.
Pungalauren	0 12 47	18 54 39	99 11 51	Highest peak.
Pulo Pinnee	0 04 40	18 29 56	98 47 17	South-east point of the island.
Pulo Oolur	0 05 11	18 30 48	98 48 09	Centre of the island.
Nellang Hill	0 20 27	18 44 42	99 02 03	Centre of the peak.
Oojong Tuan	0 14 59	18 45 28	99 02 49	Centre of the hill.
Lubwaun Looloo	0 14 05	18 47 04	99 04 25	Centre of the hill.
Toolechemanah	0 13 07	18 50 34	99 07 55	Highest part of the hill.
Pulo Gaunsah	0 02 55	18 32 48	98 50 09	Centre of the island.
Pulo Gaunsah Lout	0 01 49	18 32 45·5	98 50 06·5	Pillar on the island.

Referring to the extracts from Sir Stamford Raffles' letter before given, we find only the geographical situation of Bencoolen, and that of two or three other points to the southward, are supposed to be correctly known; and even Bencoolen, according to the latest and best authorities, appears to be placed upwards of twelve miles more to the eastward than it really is, supposing the longitude here deduced to be correct, which there seems little reason to doubt. Proceeding to the northward, we find Natal is considered to be in longitude 98° 40′ E; whereas, according to the foregoing deductions, it is in longitude 99° $1\frac{3}{4}$′; so that between Bencoolen and Natal, in a difference of latitude of little more than four degrees, there appears to be an error in longitude of about 34 minutes. Mount Ophir, according to a chart of Arrowsmith's, published ten or eleven years ago, is placed in about longitude 100° 12′, which is more than eleven miles too far to the eastward; Natal is in 99° 24′, or 44 east of the authorities before alluded to, and about 22 miles east of the longitude deduced from the observations given in this report.

From all which it will appear, I imagine, that the geographical situations here deduced are of considerable value; and, as was intended, will furnish points of departure for a survey of the coast and islands, as well as for navigators, who, with good chronometers on board, may do much towards filling up with accuracy between the points now given, the greatest difficulty being to establish a position to take a departure from. Bencoolen and Mount Ophir are two excellent points; the other principal places here given are not much inferior. Pulo Gaunsah Lout, if navigators could discover it, would also be among the best places for taking a departure from, its position being also so well laid down.

Madras Observatory. J. GOLDINGHAM, Astron.

ARTICLE VI.

On Cafein. By M. Pelletier.*

IT is well known that a white volatile crystallizable substance exists in coffee: the discovery was made by M. Robiquet, and stated in his analysis of coffee, read in 1821 to the Society of Pharmacy. At the same period M. Caventou and I, without any knowledge of the labours of M. Robiquet, also found this substance in ascertaining whether coffee did not contain quina or some analogous substance, coffee being similar to cinchona in the natural method. M. Robiquet has stated this fact in his memoir.

* Journal de Pharmacie.

Hitherto but little is known about cafein, M. Robiquet not having published his memoir upon it, and we were waiting its publication in order to state some observations which had been made by us only.*

Having some time since had occasion to prepare a quantity of cafein, I employed a process which appeared to combine several advantages, and this, I think it proper to publish, more especially because I had, rather from analogy than certain proof, hazarded the opinion, in opposition to that of M. Robiquet's, that cafein is alkaline; and I have since satisfied myself that with acids it acts similarly to those substances which are electro-chemically indifferent, and that when it dissolves in acids, the solution takes place like that of narcotin for example, that is, without saturating the acid, and very differently from morphia, &c.; and I, therefore, take advantage of this opportunity to state what appears to me to be fact in this respect.

M. Robiquet obtains cafein by treating raw coffee with cold distilled water; the brownish liquors are evaporated after being treated with calcined magnesia; and then being suffered to remain, the cafein crystallizes in nearly colourless semitransparent arborescent crystals. It is purified by solution in alcohol or boiling water, from which it separates on cooling in silky crystals resembling amianthus. The process adopted by M. Caventou and myself is rather different: we exhaust the raw coffee by alcohol, the spirituous extract is afterwards treated by cold water which separates a fatty matter; the solution of the extractive matter is heated with the addition of caustic magnesia; the magnesian precipitate collected in a filter is slightly washed and treated with alcohol to separate the cafein, which is separated by evaporating the alcohol.

A difficulty occurs in the evaporation if the magnesian precipitate is insufficiently washed, and the cafein extracted by the alcohol is rendered impure by the presence of adventitious colouring substances resembling syrup; and from these it is very difficult to purify it without incurring great loss; on the other hand, if the precipitate is well washed, very little cafein is obtained, it being carried off by the washing water. It is indeed true, that by concentrating the washings and putting them in a cold place, crystals of cafein may be procured; but this does not always succeed, especially in summer, owing to the fermentation which takes place in the liquors. It is to avoid this inconvenience that I have sought for an expeditious method of separating the cafein from the washing water. I wash the magnesian precipitate perfectly to dissolve the whole of the

* An extract of M. Robiquet's memoir will be found in the Dictionnaire Technologique, Article Cafe; and of ours in the Dictionnaire de Medicine.

cafein, and then evaporate all the liquors to obtain a dry extract, using a salt-water bath towards the end of the evaporation; I then treat the extract with alcohol of sp. gr. 817; this dissolves the cafein without taking up any sensible quantity of the saccharine and gummy colouring matter. In order to extract the whole of the cafein, the extract must be treated five or six times with alcohol, taking care to dry it by steam, or a salt-water bath, before each addition of fresh alcohol.

The spirituous solutions filtered through purified animal charcoal are to be concentrated by distillation to a certain point, and then very fine crystals of cafein are obtained.

Having procured by this process a very considerable quantity of cafein, even from some much damaged coffee which my colleague, M. Henry, had sent me, I had an opportunity of determining whether cafein saturated acids, and whether, as I at first thought, it ought to be regarded as a salifiable organic base. I was soon convinced that this substance possessed no electro-chemical property. Acids increase its solubility but little in cold water, in which it is but slightly soluble, but by hot water it is readily taken up. The circumstance which led me into an error was this; I obtained some crystals, in the form of long transparent prisms from the acid liquors, whilst from pure water I procured only a confused crystallization of opaque silky threads; whether acid be present or not, weak solutions give long, acicular, transparent, and slightly flexible crystals; but whatever quantity of acid and cafein be mixed, the acidity of the liquor depends upon the relative proportions of water and acid, and it is not sensibly diminished by the cafein.

I shall not now state the other properties of cafein, referring for them to the Dictionnaires de Technologie et de Medicine, already mentioned, I have only noticed the action of acids to correct an error which I had committed.

Returning to the process for extracting cafein, it may be inquired, why, as this substance is not alkaline, magnesia is employed to obtain it? The magnesia appears to me to favour the operation on account of its affinity for colouring matter. Having several times endeavoured to do without it, I have indeed obtained cafein, but in much smaller quantity, and as it was very impure, much of it was lost by purifying it. I have no doubt, but that by employing acetate of lead, or any other substance, which would separate the colouring matter, the same results would be obtained; but the process which I have detailed appears to me to be more simple than those which it would be necessary to follow in making use of metallic salts.

According to the analysis which M. Dumas and I have made of cafein, it is of all vegetable products that which contains most azote,* and more than animal bodies; nevertheless *it* does not, under any circumstances, undergo the putrefactive fermentation, which seems to indicate that the difference which exists between azotized vegetable and animal matter, the putrefying property which the latter possesses does not depend upon the greater quantity of azote, but upon a peculiar arrangement of the compound molecules; crystalline force alone might suffice to preserve this stability of the elements in cafein and some other azotized products of the vegetable kingdom. Even in animal substances, it may be observed that those which crystallize, such as urea and uric acid, though much azotized, are the least susceptible of putrifying.

Article VII.

Account of an improved Electro-magnetic Apparatus. By Mr. W. Sturgeon.†

The science of electro-magnetism, although so generally interesting, yet (comparatively speaking) appears to be very little understood. This latter circumstance is probably, in a great measure, owing to the difficulty of making the experiments, and the great expense attending the process; for, besides the first price of a large battery, considerable expense in acid must always attend its excitation, whenever an experiment is attempted. Large batteries are always attended with difficulty of management, and the great quantity of hydrogen evolved during the process renders the use of them extremely inconvenient to the operator. These are evidently great obstacles to the experiments being often repeated; and to the science being generally known. Another, and perhaps no less obstacle to the advancement of this interesting science, is, that the experiments being hitherto exhibited on so small a scale, are by no means calculated to illustrate the subject in public lectures; for when the experimenter succeeds even to his wishes (which is not frequently the case), the experiment can only be seen by a very near observer, and the more distant part of the auditory are obliged to take for granted what they hear reported (from

* Cafein is composed of

Carbon..........	46·51	Albumen contains of azote	15·705
Azote...........	21·54	Gelatine...............	16·998
Hydrogen.......	4·81	Fibrin.................	19·934
Oxygen.........	27·14	Urea...................	43·400
	100·00		

Cafein then contains less azote than urea only, and urea putrifies less readily than fibrin, &c.

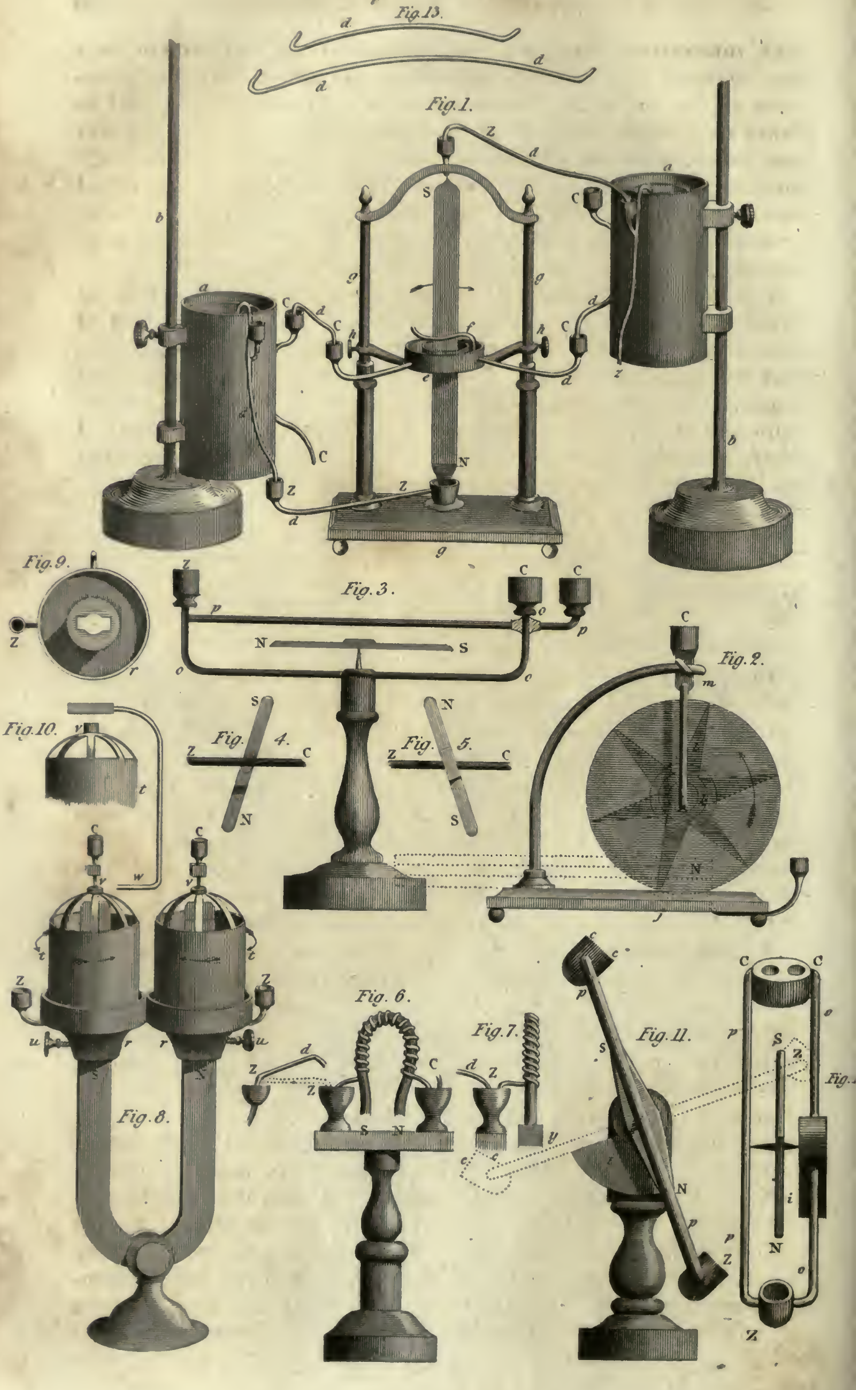
† Abstracted from the Transactions of the Society for the Encouragement of Arts, &c.

those persons who are more favourably situated), of some of the most interesting facts, which they, from their distance, are unable to witness.

With a view of removing, in some measure, these apparently formidable obstacles in the progress of this infant science, I have devoted a considerable portion of time, labour, and expense, in repeating several of the experiments, under various circumstances, and with various forms and sizes of batteries. I have likewise instituted a series of experiments, for the purpose of discovering, if possible, if any particular ratio of galvanic and magnetic power was absolutely necessary to be observed in the process of electro-magnetism. If no particular proportion of those two powers was essential, then it appeared highly probable that an increase of magnetic power might compensate for a deficiency of the galvanic, and thereby render the use of large galvanic batteries quite unnecessary, an object which I considered both interesting in its nature, and, by reducing the expense, and facilitating the process, exceedingly desirable to the experimenter; and I am happy to state, that my labours were no ways abortive, for instead of electro-magnetic phenomena depending on powerful galvanic, and feeble magnetic force, as had till then been practised, I found, during that inquiry, that the galvanic force may be reduced to almost any degree, provided the magnetic be sufficiently powerful. This discovery led me to the use of powerful magnets, and small galvanic batteries, for with small magnets the experiments can never be made on a large scale, although the galvanic force be ever so powerful; and as minute and delicate experiments are not calculated for sufficiently conspicuous illustration in public lectures, I considered that an apparatus for exhibiting the experiments on a large scale, and with easy management, would not only be well adapted to the lecture room, but absolutely valuable to the advancement of the science. Upon this principle I have constructed a complete set of instruments, which, from their superior magnitude, and peculiar arrangement, are, in my humble opinion, and by the certificates I have been honoured with, are, in the opinion of gentlemen whose judgment I presume will ever be held in the highest estimation, well adapted for the illustration of the subject, either in the private study or public lecture room.

It will be understood from what I have already stated, as well as from an inspection of the instruments, that the mode which I have taken for the production of electro-magnetic phenomena is more simple in its management, less expensive in the process, better calculated for the illustration of the subject, and the reverse of that which has hitherto been used, and which, by its almost entire dependence on the tedious and expensive process of galvanism, has considerably retarded and obscured this new

Mr. W. Sturgeon's Electro-Magnetic Apparatus.



J. Shury Sculp.

Engraved for the Annals of Philosophy. Published by Baldwin, Cradock and Joy, November 1st 1826.

and interesting science; for whenever an experiment was not attended with the anticipated success, the failure was generally attributed to an insufficiency of galvanic power; and in order to increase the effect, it appears that the experimenter had no other means of accomplishing his object, than by augmenting the power of his battery, or by reducing the size and increasing the delicacy of his other apparatus, the magnetic power being either entirely lost sight of, or regardlessly neglected, as if no ways materially concerned in the process.

I have found, however, by the above-mentioned course of experiments, that the magnetic force is as essential as that of galvanism to the development of electro-magnetic phenomena; and the apparatus which I now submit to the attention and impartial consideration of your valuable Society, acting on the principle of powerful magnetism and feeble galvanism, will, I trust, be found more eligible and efficient than any other that has yet been brought before the public.

Reference to the Engraving of Mr. W. Sturgeon's Electro-magnetic Apparatus. (Plate XLI.)

Plate XLI. fig. 1. A perspective view of an apparatus to show the revolution of a magnet round its own axis. *a a* the two galvanic apparatuses on their stands *b b,* they are acting on the magnet N S, by means of the connecting wires *d d d d*; both their copper poles C C are applied to the equator *e* of the magnet, while the zinc pole Z of one is applied to the north pole N, and the zinc pole Z of the other is applied to the south pole S of the magnet. A wire *f* is soldered on to the magnet, and bent down at one end to dip into the circular trough *e* to form the equatorial connexion: and as all the connexions are made by mercury and amalgamated wires, the end of this wire is amalgamated, and mercury put into the trough: all the little cups Z and C are also amalgamated at the bottom, and contain mercury; the bottom wires of the zinc and copper poles are likewise amalgamated to dip in connecting cups when wanted. The magnet has brass wire centers on which it turns, that at the north pole stands in a cup Z with mercury; and the other at the south pole enters the amalgamated hollow in the screwed end of the upper connecting cup Z. When the connections are made, as above described, on pouring dilute nitric acid into the troughs *a a,* the magnet will revolve in the way shown by the arrow; but on changing the connexions, by applying the copper wires to the poles, and the zinc ones to the equator, it will revolve the contrary way; here the magnet only forms the connexion between the electric poles, and revolves around, or with the current which is conducted by it. *g g g* is the stand which supports the magnet; the equatorial trough *e* is made moveable on the pillars *g g*, and is fixed by the screws *h h*.

Fig. 2. A view of a circular metal disk, made to revolve between the poles of a horse-shoe magnet; the disc is amalgamated round its edge, and dips into a little mercury contained in a hollow *j* of the stand, the centers *k k* on which it turns, and the hollows that receive them in the forked support *l l* are amalgamated; the screw *m* allows the disc to be adjusted, and fixed so as only just to touch the surface of the mercury. A horse-shoe magnet N or N S shown by dotted lines, is laid on the stand, then one of the troughs *a* of fig. 1 is to be adjusted on its stand *b*, till its bottom wire z dips into the connecting cup z, forming the zinc communication, and a connecting wire *d* with bent ends is to dip into the copper connecting cup c of the trough, and into the cup *c* of the disc; the communication of the poles being thus made (the current passes from z, through the mercury *j*, into the edge of the disc, and through its centers *k k* into the fork *l l*, and up to the cup *c*) the disc will then revolve as shown by the arrow. By reversing either the poles of the magnet, or the electric poles, the revolution of the wheel is reversed; but if both are reversed, the revolution will continue in the same way as at first. The six rays are painted on the disc, merely to render the revolution visible at a greater distance.

Fig. 3. A stand supporting a needle between two conducting wires *o o* and *p p* to show the different effect of electricity on the needle when passing above or below it; the cup z is common to both, but the other ends have each a separate cup c c: when the electric current passes along the upper wire, *p p* the needle takes the position as shown in fig. 4; but on lifting the connecting wire out of the cup *p* c, and putting it into the cup *o* c, the current passes through the under wire *o o*, and the needle immediately goes round to the position indicated in fig. 5; then if you watch the motion of the needle, and keep alternately transferring the wire out of one cup into the other, keeping time with the needle, you may bring it into the most rapid revolution that you can possibly keep time with.

Figs. 6 and 7. A front and side view of a stand with two connecting cups z and c made of wood, in which the bent iron wire wound round with copper wire is supported by the two copper wire ends. On making the galvanic connexion through the copper wire, the iron wire becomes a strong horse-shoe magnet, and will support a heavy bar of iron as *y* fig. 7; but on lifting the connecting wire *d*, fig. 6, out of the cup z, the weight immediately drops, and on restoring the connexion, the power is restored; then if you change z for c, it will change N for S, or if you only wrap the copper wire about the iron wire, as a right threaded screw instead of a left one, as in the Plate, it will change N for S. This is explained by what takes place in figs 3, 4, and 5.

Fig. 8, a horse-shoe magnet, mounted with two mercurial troughs *r r*, (fig. 9 shows one separate) *t t* two cylinders suspended on the ends of the magnets, by points within their crowns under the cups *v v*; their bottom edges are filed away, leaving only four points (as fig. 10) to touch the mercury, by which means the friction is much lessened. The troughs are adjusted by the screws *u u* so as to bring the mercury just in contact with the points of the cylinder; the screw points of the upper cups c c just touch the mercury in the cups *v*. Upon making the communications as before with the cups z z and c c, the cylinders will revolve as shown by the arrows.

Figs 11 and 12 show a front and back view of a dipping needle, mounted between two wires, *o* and *p*; they are here placed in the direction of the dip, but the quadrant *i* allows them to move one quarter round, or to the equator of the magnet, as shown by dotted lines. In their present position the needle will deviate, as figs 4 and 5; and it will be seen the needle cannot take a position quite at right angles to the wire, owing to the terrestrial magnetism drawing it on one side; but when the wires are carried round to the dotted position fig. 11, the needle remaining as it was, so as to be at right angles to each other, then on passing the current from z through the wire *o o*, no effect will appear to take place, the needle is only more confirmed to its position, but on passing it through *p p*, the needle goes round, and dips with its south pole. The wire passes through the wooden cup z, but the two ends of it *p* and *o* only just enter their respective wooden cups c c; these wooden cups are placed at an angle of 45° to the horizon, so that in either position they are similar, and will hold mercury enough to make the contact.

Fig. 13 shows two of the connecting wires separate, three or four pairs of each of these are required.

These figures are nearly one-fifth of the real size, and it will be seen that the magnetic power is very great in proportion to the galvanic power.

Article VIII.

Further Observations on the Genus Hinnites, with the addition of another recent Species, indigenous to Great Britain. By J. E. Gray, Esq. FGS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Paris, Oct.* 16, 1826.

In the number for August last, p. 103, I described a recent species of the genus Hinnites of Mr. De France, which I had discovered in the collection of the British Museum. A few days since, on looking over the beautiful and extensive collection of recent and fossil shells belonging to my friend Dr. Deshayes, who is at present engaged in describing and publishing the fossil shells found in the environs of Paris, I observed that he had placed the Pecten sinuosus of *Lamarck*, the Ostrea sinuosa of *Gmelin*, which is not uncommon on the British coast, as a recent species of the above-named genus, which he has called *Hinnites sinuosus*, and on examining the shell, I am convinced of the propriety of the situation assigned to it by Dr. Deshayes.

On account of the worn and shattered state of the other recent species in the Museum not showing very distinctly the mark of attachment, one or two of my friends have been induced to doubt of its being attached; and, therefore, I am the more glad to add this species to the genus, as it is well known to most British conchologists, that the Pecten sinuosus is always attached to rocks, and is generally found in their holes and crevices; but the fact does not appear to have been known to Lamarck, who observes, that this species is "very singular from its deformities," which are evidently produced by the irregular surface of the rock to which they are attached; and two of the specimens in my possession have the upper valve very similarly marked to the specimen described in the former paper, and the marks are doubtless occasioned by the same causes. Sometimes the shells are scarcely distorted, and they are usually furnished with elongated or long lamellar spines, by which they are attached.

The fact of the English species having been so long kept in the genus Pecten, shows the great affinity which the genus *Hinnites** must have to them; it appears indeed to be an osculant or intermediate genus between the Pectines and the Spondyli, it being provided with the groove for the passage of the Byssus, like the true Pectines, and with the attached shell of the true Spondyli; and at the same time it is separated from the former by the shell being attached by the lamellar processes, and from the latter by the hinge being destitute of any appearance of teeth, although this character loses much of its importance when the hinges of the Pectines are carefully examined; for many of them are provided with distinct teeth on the hinge-margin, as well as with the curiously-shaped tubercular lateral teeth placed below the ears.

* The name of this genus must be changed to Hinnus; for whilst this paper was going through the press, a classical friend has pointed out to me, that the above word is the proper derivitive of Hinnites, and not Hinnita, as I had before inadvertently called it, in my former paper.

Article IX.

Alcohol derived from the Fermentation of Bread. By Mr. Thomas Graham.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Edinburgh, Sept.* 25, 1826.

Two facts of considerable importance in determining the nature of the *panary* fermentation, have been made known by your ingenious correspondent upon the art of baking bread. He has shown that the fermentation depends upon the saccharine ingredient of the flour, by renewing it when exhausted by the addition of sugar; and provided for the little alteration in the proportion of sugar existing in the flour, before and after fermentation, by exhibiting the influence of the baking in converting a portion of the starch into sugar. From the known laws of the decomposition of sugar, it is presumed, with considerable reason, that the fermentation is the vinous. The production of alcohol in the course of the fermentation of bread in baking, which we have found to take place, and rendered appreciable, is, perhaps, a most irrefragable proof of which this theory is susceptible.

To avoid the use of yeast, which might introduce alcohol, a small quantity of flour was kneaded, and allowed to ferment in the usual way, to serve as leaven. By means of the leaven a considerable quantity of flour was fermented; and, when the fermentation had arrived at the proper point, formed into a loaf. The loaf was carefully inclosed in a distillatory apparatus, and subjected for a considerable time to the baking temperature. Upon examining the condensed liquid, the taste and smell of alcohol were quite perceptible, and by repeatedly rectifying it a small quantity of alcohol was obtained of strength sufficient to burn, and to ignite gunpowder by its combustion.

The experiment was frequently repeated, and in different bakings the amount of alcohol obtained, of the above strength, found to vary from 0·3 to 1 per cent. by weight of the flour employed. When the fermented flour was allowed to sour before baking, the amount of alcohol rapidly diminished; and in all cases, the disagreeable empyreuma completely disguised the peculiar smell of the alcohol, when in its first diluted state, and in vapour. I am, Gentlemen, with great respect,

Your most obedient servant,

Thomas Graham.

Article X.

On the Tungstate of Lead. By A. Levy, Esq. FGS. &c.

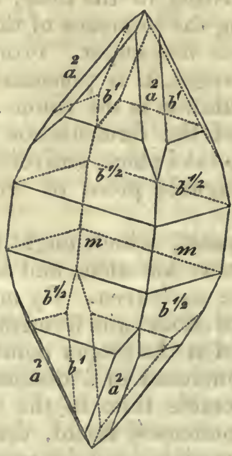
(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

If you think the following short notice respecting tungstate of lead worth insertion in the *Annals of Philosophy,* you will oblige me by sparing a little room for it in the ensuing number.

One of the specimens of tungstate of lead in Mr. Turner's collection offered me very distinct crystals of the form represented by fig. 1. They are much lengthened in the direction of

Fig. 1.



the axis, whitish and translucent; they cleave easily parallel to the planes marked b', as well as in a direction perpendicular to the axis. The incidences, easily obtained by means of the reflective goniometer, either on natural planes, or planes of cleavage, were as follow:

$b_2', b' = 99° 43'$	$b', b' = 131° 30'$	$m, b' = 155° 45'$
$a, a^2 = 106\ 47$	$a^2, a^2 = 65$	$m, a^2 = 126\ 37$
$b^{\frac{1}{2}}, b^{\frac{1}{2}} = 92\ 46$	$b^{\frac{1}{2}}, b^{\frac{1}{2}} = 154\ 36$	$m, b^{\frac{1}{2}} = 167\ 18$

In some of the crystals, the planes of the modification $b^{\frac{1}{2}}$ are wanting; whilst others are composed of this modification alone, and present the form of very acute octahedrons, most of which are cuneiform. I also found that the very small white crystals, mentioned by Bournon, and which sometimes accompany the molybdate of lead, present one of the preceding forms; and as far as I could ascertain, measure very nearly the same angles: they are, therefore, most likely tungstate of lead.

I was immediately struck with the almost identity of these measurements with those offered by molybdate of lead, and the only difference I could notice with respect to cleavage was, that in the tungstate of lead, the cleavage perpendicular to the axis appeared to me more easily obtained than in the molybdate. In consequence of this great similarity of crystallographical characters, I begged Mr. Children, about two years ago, to examine chemically a small quantity of the substance, to ascertain if it had not been wrongly named, and whether it was not simply molybdate. The quantity he had to operate upon, however, was so small, that no decisive result could be obtained, and in consequence of the following considerations, I have placed the specimens in the collection apart from the molybdate, and under the name of tungstate of lead.

I had then noticed that the measurements of molybdate of lead were very nearly the same as those of tungstate of lime; it appeared besides from the great analogy of forms, macles, and cleavages, as well as the near equality of angles of carbonate of lead and arragonite, as well as of phosphate of lead and phosphate of lime, that (to use the language of Prof. Mitscherlich, which now it is well known how to understand), lead and lime were isomorphous bases. It was, therefore, to be expected that tungstate of lead would measure nearly the same angles as tungstate of lime; and consequently nearly the same as molybdate of lead. Another inference to be drawn from what precedes is the isomorphism of molybdic and tungstic acids; that is, the same analogy between them as has been proved to exist between phosphoric and arsenic acids.* With the desire to establish this result upon more facts, I endeavoured last year to procure some artificial molybdates and tungstates; but Mr. Faraday, to whom I applied, told me they were very difficult to obtain in a crystallized state. I think, however, that the measurements of tungstate of lead which Mr. Brooke was so good as to show me a few days ago, and which he had taken upon small crystals of that substance (a few specimens of which were lately received by Mr. G. Sowerby), but without noticing their near equality to those of molybdate of lead and tungstate of lime, confirm the results I had previously obtained, and justify the inference I had drawn from it.

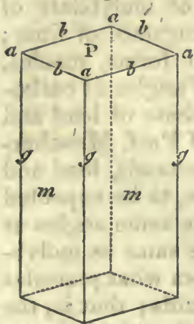
Besides this new example of isomorphism, I have been some time engaged in the examination of a class of substances which present a remarkable analogy of forms, and near equality of measurements,

* These two acids present a case analogous to the one under consideration. Their combinations with two isomorphous bases producing isomorphous crystals, those of arseniate of cobalt and of phosphate of iron.

among which I shall mention cymophane, peridot, humite, fosterite, crystallized serpentine, tantalite, &c. the results of which I shall publish when completed ; and I hope that as the number of examples of isomorphisms will increase, the attention of mineralogists and chemists may be directed to this interesting subject, which require the help of both.

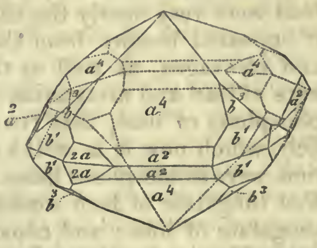
I shall also avail myself of this opportunity to give the description of a beautiful crystal of tungstate of lime, which belongs to the collection of the Right Honourable the Dowager Countess of Aylesford, and which her ladyship has allowed me to examine. By comparing its measurements with those precedingly given for tungstate of lead, the analogy between these two substances will appear. The form is represented by fig. 2, and the crystal is nearly of the size of the figure, and perfect. I have assumed for primitive the square prism represented by fig. 3, in which the side b of the base is to the height as 1 is to 2·098.

Fig. 2.



$b : g :: 1 : 2{\cdot}098.$

Fig. 3.



P, m = 90°. m, m = 90°. b', b' = 100° 40′. b', b' = 129° 2′. b', b' = 150° 27′. b^3, b^3 = 69° 56′. $b^3 : b^3$ = 132° 11′. a^2, a^2 = 112° 2′. $a^2 : a^2$ = 108° 12′. a^2, a^4 = 160° 33′. a^4, a^4 = 73° 8′. $a^4 : a^4$ = 130° 10′. ${}_2a, {}_2a$ = 155° 56′. ${}^2a, b'$ = 151° 33′. ${}_2a, a^2$ = 152° 21′. ${}_2a, a^4$ = 136° 12′.

This magnificent crystal is of a pale yellowish colour, and transparent. Mr. Heuland, on seeing the specimen to which it is attached, had no doubt that its locality was Breitenbrunn, in Saxony.

Article XI.

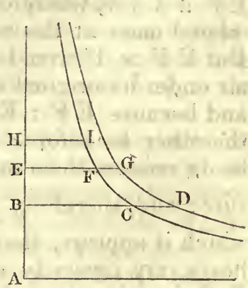
On the Law of Temperature. By Mr. Meikle.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Edinburgh, Sept.* 30, 1826.

In the last number of the Edinburgh New Philosophical Journal, I had occasion to point out some very remarkable inconsistencies in the opinions which are entertained by most of the principal authorities of the present day, regarding the law of temperature. The subject is one of great difficulty, and those who have given it sufficient attention are aware, that there is abundant room for falling into mistakes. In such researches, as is well known, it is no easy task to avoid confounding variations in the quantity of heat with the variations on our common scales of temperature; and it is curious that though new terms have been coined for the express purpose of overcoming this source of fallacy, those who were thus accoutred have not thereby been protected from their former mistakes; from which it would appear, that the difficulty was owing to something else than a want of words; and that the nature of things was not, in this instance at least, changed by a new name.

In the paper referred to, I have attempted both to point out some of the chief misconceptions which exist on this subject, and also to show what law of temperature is alone consistent with admitted principles. No new hypothesis is introduced. But from reconsidering the subject, I find that the law of temperature admits of being investigated in a somewhat simpler form, so as entirely to avoid the differential equation, and the determination of the requisite form of its integral, which led those great mathematicians who have preceded me in this inquiry so far astray.



Let t be the temperature, or rather the indication on the common scale of an air thermometer, p the pressure, and ϱ the density of a mass of air; then a and b being constants, we have from the law of Boyle,

$$p = b\varrho\,(1 + a\,t) \quad \ldots\ldots\ldots \quad (A)$$

Now it is obvious, that the specific heat of air under a constant pressure will be to its specific heat under a constant volume, in the inverse ratio of the variations of temperature produced in these two different cases by equal variations in the quantities of heat; so that the following expressions respectively contain all the variables which enter into these specific heats, relatively to the ordinary graduation, viz.

$$\frac{1}{d\,t} = -\frac{1}{d\,\varrho}\cdot\frac{a\,\varrho}{1 + a\,t}, \text{ and } \frac{1}{d'\,t} = \frac{1}{dp}\cdot\frac{a\,p}{1 + a\,t}\,*$$

* The specific heats are $\frac{d\,q}{d\,t} \times 1^{\circ}$ and $\frac{d\,q}{d'\,t} \times 1^{\circ}$. But $d\,q$ the differential of the quantity of heat, being constant, and the same in both terms, is here omitted, as also the constant linear degree of the common scale.

which are obtained from Equation (A) by making p and ϱ alternately to vary with t, whilst the other is constant.

Let the point A represent — 448° F. or — 266·7° cent. and let the temperature be reckoned on the straight line A B as on the common scale of an air thermometer. Also let C I be a line of such a nature, that any ordinate as B C, E F, H I, &c. may be respectively proportional to the specific heat of a mass of air under a constant volume at the temperatures B, E, H, &c. so that the intercepted areas will denote the corresponding variations in the quantity of heat, under a constant volume. But MM. Gay-Lussac and Welter have ascertained by experiment, that the specific heat of air under a constant pressure, exceeds that under a constant volume, in a constant ratio, which call that of $k : 1$; wherefore, if these ordinates be every where increased in that ratio, another line G D passing through their extremities must be of the same nature with C I, whatever that may be, and the intercepted areas, of course, to the former as k to 1.

Let B D × 1° be the specific heat of a mass of air under a constant pressure, and let its temperature be raised from B to E, under the same pressure: then area B D G E will denote the increase in the quantity of heat, and E G × 1°, the specific heat under a constant pressure at the temperature E. Now E G : E F :: k : 1, wherefore E F × 1° will be the specific heat of the dilated mass at the temperature E, under a constant volume. But E F × 1° would still have been the specific heat had the air under its original volume been raised to the temperature E; and because E F : E G :: 1 : k, E G × 1° would have been the other as before. Hence the *constant* ratio of the specific heats renders them independent of the actual density or pressure; and, therefore, $\frac{\varrho}{d\varrho}$ and $\frac{p}{dp}$ are constant quantities. From which it appears, that the algebraic expressions for the specific heats vary inversely as $1 + a\,t$; or that any ordinate B C, or B D, is inversely as A B, which is the well-known property of a hyperbola; and, therefore, C I and D G are both hyperbolas having A for their centre, and A H for an asymptote.*

Hence, as before, the variations of volume, under a constant pressure, or the variations of temperature on the common scale,

* The quantities $\frac{d\,q}{d\,t}$ and $\frac{d\,q}{d'\,t}$ are only linear expressions, such as B D and B C. To be complete, they must be multiplied by the particular linear degree of the scale to which they belong.

The result which I have obtained is the only one which can make the algebraic and geometrical values of the specific heats agree together. The hypothesis of M. Laplace gives two inconsistent values to both quantities, even algebraically, and a third different from these, but the same with mine geometrically. (See Annales de Chimie et de Physique, xxiii 339, and Mecanique Celeste, livre xii. 98.)

form a geometrical progression; whilst the variations in the quantity of heat are uniform. From this the other conclusions are easily deduced regarding the relation which subsists between p, ϱ and t, when the quantity of heat is constant, as may be seen in the paper referred to.

I am, Gentlemen, your very obedient servant,

HENRY MEIKLE.

ARTICLE XII.

Suggestions for the Improvement of the British System of Chemical Instruction. By Edward B. Stephens, Chemical Assistant to the Royal Dublin Society.

Dec. 20, 1825.

THE method by which the science of chemistry has hitherto been taught in public lectures throughout the united kingdom appears to me susceptible of a variety of improvements. The suggestions which I now offer in the hope of effecting these, are not founded upon my judgment alone. Though not aware of any similar observations in print, yet I am happy to state that many individuals whose scientific acquirements, experience, and rank in society give them an undoubted right to judge, coincide with me in opinion. As their communications on the subject have continually tended to guide and form my judgment, it is but justice to state my obligations to them, and forego the credit of originality for the weight of authority.

The regular analytic course is certainly well calculated to improve those who have previously acquired elementary and experimental knowledge of the subject, and who wish to review and systematically arrange in their memory, the facts already stored in it; but it is morally impossible for those not previously possessed of elementary knowledge to derive similar advantages from attendance on such a course of instruction. The series of lectures at present in fashion in these kingdoms presupposes considerable information already attained. It is evident from the refined style and learned tenor of the usual course, that a chemical lecturer addresses his auditors both as critics and pupils: he assumes that they are imbued with the various literary and scientific knowledge requisite to a clear conception of his plan and language: that he need only allude to the sister sciences of mineralogy, electricity, meteorology, and pneumatics, to be perfectly understood; and that explanation and repetition at every step, would be alike tiresome and useless.

Now we all know from our own experience, that few indeed of those who attend a course of chemical lectures for the first time, are possessed of general knowledge sufficient for a correct comprehension of the subject; and that with respect to the majority, the lecturer is proceeding to build before the foundation is really laid. The consequence is, that even the most attentive and well inclined amongst his youthful auditory are unable to follow him in his deductions, and are often thrown into partial despair by the apparent difficulties of the study. An instance or two will explain their peculiar embarrassments. The first time they hear of specific gravity at a lecture, its universal relation to solids, fluids, and gases, will perhaps appear incomprehensible to them; and all the necessary calculations and corrections respecting temperature, atmospheric pressure, and hygrometric moisture, tend to place the matter in greater obscurity. But is the subject naturally obscure? Certainly not: the error lies in the mode of instruction adopted by the lecturer, who brings forward barometers, thermometers, and hygrometers, and applies them at once to his subject, taking for granted that his auditors are all sufficiently informed of their construction and use by a previous study of natural philosophy; which certainly ought to be the case, and certainly is not, as education is generally managed.

Again, when a lecturer treats of precipitation at the commencement of his course as usual, he proceeds to exemplify it in a way that must inevitably create a confusion of ideas in an uninformed mind. For instance, he pours a solution of muriate of barytes into another of sulphate of soda, points to a white cloud appearing in the mixture as an evident precipitate, and informs his class that it is produced by the double decomposition which has taken place between the two salts, as the result of their compound elective attraction, and that two new substances are thereby formed—sulphate of barytes and muriate of soda. In this short explanation a pupil is introduced to a variety of new matters, ideas, and terms. He hears the word "precipitate" used for the first time as a noun, whereas he had usually understood it as a verb active "to throw downwards," and is not a little confused to hear of precipitates forming clouds, or rising to the surfaces of liquids. He may never before have heard of the four salts concerned in the experiment, and the only comment on the matter afforded him at the time generally is—that what is commonly termed muriate of soda, is properly a chloride of sodium!—all of which remain to be explained to him in future lectures. A hundred similar instances might be adduced.

Thus every explanation of the laws which influence matter, contains (according to the present system of chemical instruction) something that a pupil who attends for the first time is not prepared to understand, and frequent allusions to substances he has not before heard of, but which he is told he will hear of again. In consequence he is induced to postpone thought and reflection to a more convenient opportunity, and thus acquires the bad habit of listening passively to what conveys no definite notions to his mind.

If, on the contrary, a lecturer with partial consistency exhibit the phenomenon of precipitation, without informing his class what the agents are which he employs, a young pupil, who is exerting his faculties to learn the name and character of every substance brought forward, feels particularly disappointed and confused if he be stinted in this manner in the information proper to an analytic course.

Instead then of seeking to effect the double object of instructing the learned and unlearned at the same time, (which necessarily produces one or both of the evils above mentioned) a lecturer who has studied "the conduct of the human understanding" will endeavour to accomplish a separation of these two classes, that he may prepare for each suitable information. The instruction of the advanced student is sufficiently provided for by the usual routine; but to really benefit those commencing the study, he will perceive the necessity of constructing a series of lectures on a very different plan. His grand object in these should be,—to convey elementary instruction in a style and manner comprehensible to the plainest capacity,—excluding every idea of display and artificial system which might interfere with so desirable an end:—his surest way to attain a correct view of what such a course of instruction ought to be, is, to suppose himself a pupil, and consider what kind of lectures he would require in similar circumstances.

Keeping this in view, he may readily frame a course of preparatory lectures that shall proceed with ease and satisfaction to all concerned. On the first day, each element may be displayed in succession, and its distinguishing properties familiarly stated; (avoiding its combinations, for it is impossible a lecturer can be understood, if he speak of them at first;)—and by the time all the simple substances in nature that we are acquainted with are thus reviewed and classified, a pupil will be astonished to find how few they are, and what an easy science chemistry appears when clearly entered on. The elements (as exhibited) may remain in their order on the table during succeeding lectures, in which their primary combinations should be experimentally explained, and completely gone through before the secondary combinations, or salts, are brought forward. The latter must be described in their turn before the complex animal and vegetable products can be consistently treated of. Whenever a compound is introduced, its elements may be again referred to with effect, for here repetition is truly judicious. If pupils have constant opportunities of turning their attention to explanatory diagrams and groupes of the simpler substances which together form the compound that the lecturer is speaking of, they will have comparatively little difficulty in recollecting the composition of bodies. No actual preparation of the elementary substances should be attempted before a class of beginners, for they are not prepared to understand the *rationale* of their production. This exhibition should be reserved for another course, and for another class who are sufficiently advanced to comprehend the means by which the lecturer obtains them, and are in no danger of supposing that he is making hydrogen or oxygen gas, when he is only liberating it.

In fact, this science is so extensive, and so peculiarly liable to misconception in its language and objects, that two distinct courses are indispensibly requisite to avoid inconsistency and confusion, and enable a teacher to do justice to his class. The first,—simple, explanatory, synthetic,—ascending from elements to compounds, and so arranged that the class may arrive at a knowledge of the laws of nature as the result of direct experiments. Here the lecturer should address his auditors as persons totally ignorant of chemistry. This course ought to be preparatory to the second, wherein he might proceed on the present system; that is,—first promulgating general laws, and then illustrating them by exhibitions and experiments; and so far as the previous lectures had made his class acquainted with the substances experimented upon, so far will they really understand and profit by these.

The latter may be termed the descending, the analytic lectures, in which the decomposition of substances might be experimentally pushed to the utmost limits of our skill, the methods of detection and separation thoroughly explained, and the application of chemistry to the arts and manufactures; to pharmacy, metallurgy, agriculture, &c. demonstrated with all the eloquence and collateral information which can be brought to bear upon the subject. The refinements of electricity, magnetism, may now be displayed with consistency and effect; and a discussion of the principal conflicting speculations which divide the chemical world may also be intelligibly entered on. By these arrangements, all who are prepared to enjoy the heights of science may be gratified without the loss of time they have hitherto sustained in listening to a repetition of well known facts, (which the present mixed system renders unavoidable); and those who now sit year after year endeavouring to understand the subject, will also be enabled (by the preparatory course) to derive enjoyment and instruction from elevated themes, which would otherwise be to them the essence of perplexity.

In the elementary lectures, the various substances should be arranged as much as possible into the distinct and familiar groups which the pupil's mind would naturally place them in.

His memory is aided as much by their classification into metals, earths, gases, combustibles, acids, alkalies, &c. as that of the botanist is by the Linnæan arrangement of plants; and though both are in some respects artificial, yet both are decidedly useful. In fact, the comparative ease with which the sciences are studied in latter years arises from this facility of arrangement, which enables the student to refer many hundred (in some cases many thousand) individuals to a very few classes; each possessed of a common character or family likeness. In chemistry, unfortunately, the electro-positive and negative classification (though scientifically correct,) requires so much previous knowledge for its proper comprehension, that a pupil is altogether debarred from its aid in making his first experimental acquaintance with the science. It would, therefore, perhaps be as well not to press it on his attention, till he had learned sufficient to fully understand and appreciate it.

A junior class cannot see too much of the practical operations of the laboratory as soon as it can comprehend them. Whenever the preparation of a substance will not distract attention from the lecturer, as he proceeds to new matter, and whenever his time permits, it will be wise to bring his furnaces before his pupils, and convert the mysteries of the laboratory into engaging illustrations, and welcome aids to memory.

In the instruction of this junior class, a lecturer's object should not be to teach all that is known of the science, but to lay a solid foundation of general facts in the youthful mind, and create in it an ability and a desire to work out its own instruction, while he unfolds the means, and gradually implants in it the habit of industrious investigation.

Proceeding in this spirit, he will not dwell on the minor salts of the vegetable, animal, and mineral world, unless he can associate them with some agricultural, mining, physiological, or medical comments, which are always valuable as practical illustrations of the use of chemistry. The merits of rival hypotheses (or unproved theories) are matters interesting only to more advanced students, and may safely be omitted for the present. I am also of opinion, that, however consistent it may be to commence an advanced course with separate lectures devoted to the explanation of the laws of chemical action, and what are termed the canons of chemistry,—yet they may with great propriety be omitted in an elementary one, inasmuch as a pupil cannot then apply them: he may remember them by great exertion, but such knowledge hangs a dead weight on his memory till he is afforded an opportunity of deducing them from his own practice, or from observations on the experiments exhibited at lectures. A great evil is done when the mind of a youth is bewildered by having more information pressed upon him than he can receive at once. Confusion induces despondency, and the mischief is increased by his loss of that part of the lecture which often passes unheeded during the continuance of his perplexity, leaving an opening for fresh misunderstandings that he may never afterwards have opportunity or leisure to remove. Carelessness and dislike frequently follow in the train of consequences, for no one can really love a science that is apparently above his comprehension.

Now, the doctrines of aggregation, affinity, decomposition, combustion, absolute, specific, and latent caloric, &c. &c. which, with their definitions and illustrations, form the substance of several lectures at the outset of a scientific course of chemistry, under the present arrangement, are poured into a young pupil's ear long before he can possibly understand to what they relate; ignorant as he must be at first of the substances which he sees employed in illustration. In the select elementary course, this inconsistency should be carefully avoided, and these doctrines introduced only when experiments directly illustrative of the properties of each substance afford opportunities of explaining the accompanying appearances, and consistently entering on the *rationale* of the matter. If no more be said at one time than refers to experiments and exhibitions already made, the progress of pupils will be real, rapid, and satisfactory. They are always inclined to generalize and draw conclusions from analogy (when they understand a subject,) as quickly as a prudent teacher should wish: his chief care on this point will therefore be to correct the notions they instinctively form, (or most likely sanction them) by declaring the true state of the case at the end of each series of experiments. It is judicious at all times to give them opportunities and habits of exerting their reasoning powers. The mind is pleased with its own labours and discoveries, and memory fondly retains such little triumphs long after the dogmas impressed by the voice of authority have faded from recollection.

Definitions without examples are, to the generality of young persons, very perplexing and unprofitable. To say the truth, they are matters rather of curiosity than utility in the present improving and changeable state of our science; and on weak minds are often found to act like fetters which cannot easily be shaken off when new lights are shining to stimulate them to mental exertion. Since the discovery of the intimate connection of voltaic electricity and magnetism with chemistry, for instance, all our old definitions of the science must in a great measure go for nothing.

In adherence to the principles advocated throughout, the history of the science should be deferred till the end of the first course, or better still, till the beginning of the second. By this time the lecturer will be intelligible when he adverts to the energetic labours and brilliant inventions of his predecessors, and may fearlessly assume a loftier style, suitable to the dignity of his subject. But if he enter on a critical enumeration of the discoveries of Scheele, Higgins, Richter, Lavoisier, Berthollet, and Davy, before his class is well grounded, nay, far advanced in knowledge of the facts of Chemistry,—the labour will be as much misplaced, and of as little service, as a display of the refined methods of La Place's Mecanique Celeste would be to one who is labouring through the Elements of Euclid.

A judicious recapitulation of the most important facts at the conclusion of each lecture, and a brief summary of them at the commencement of the succeeding one, will be found eminently useful throughout the preparatory course. Almost every young person understands a subject better by having it twice explained, and some are in absolute need of repetition, where the doctrine is entirely new to them. Those necessarily absent from a particular lecture, are at the next in evident want of a summary of what they have lost, to enable them if possible to recover it by reading, and profit by the remainder of the course, which is, (or ought to be) an exhibition of particular facts, and general reasonings on them, so arranged that the first parts of the series shall form natural and easy stepping-stones to enable a constant audience to arrive at the last.

An hour's lecture is perhaps too much for a very young audience. Few youths of either sex have learned to command their serious attention so long, and it is of particular consequence to avoid fatiguing it. It is perhaps of equal importance to avoid distracting it by entering upon unconnected topics in the same day. The convenience of both parties must of course determine the times and duration of the sittings, but it will be invariably found that the oftener they can meet, and the less in proportion the lecturer attempts to impress on his pupils' memory at each meeting, the greater effect will his instructions have.

A lecturer to a junior class should not only speak slowly and distinctly, but if possible avoid reading to them. The preparation of manuscript lectures necessarily consumes a large portion of time; and, after all, his pupils will yield far more deference to passable extemporaneous, than to the best written communications. This is quite natural, and a teacher who speaks from the fullness of his subject (using his bottles and glasses and specimens for memoranda as he proceeds), not only commands greater attention, but really acquires a more intelligent and convincing style than the best reader can hope to attain. Eloquence is not indispensibly necessary to an instructor in this science: high-wrought mystical language, and all attempts at display, are foreign and injurious to the end proposed, particularly before an elementary class. If the lecturer be really possessed of the desire to teach, and set about it earnestly and unaffectedly

—like a gentleman called on to explain what he knows on an interesting subject to a company of his friends, he may be almost certain of success.

In imparting a knowledge of this science, a speaker has vast advantages over an author. The illustrations of books are limited to drawings; whilst in conversation a teacher can add to these experimental exhibitions and specimens which enable all the senses to aid in fixing an association of ideas in the memory. He can employ emphasis or gesture, repeat or omit particular facts, refine or lower his style as may appear proper. His mere glance awakens attention. He can explain, if it appear he has been previously misunderstood, bring forward the latest and most interesting facts applicable to his subject, and make even accident the ground of pleasing and instructive remark. It therefore requires no great depth of observation to enable a lecturer to determine which of the two systems, writing or speaking, he should incline to, and cultivate.

One of the greatest advantages which a chemical lecturer may avail himself of, is that of conversing with his class after lecture; and fortunately the benefits are reciprocal. He is allowed an opportunity of coming at their difficulties, answering objections, and removing misunderstandings by simplifying the subject. A single misconception at the beginning of a lecture is often sufficient to prevent a pupil comprehending what follows, yet it is impossible at the time for a lecturer to penetrate and remove the cause of his embarrassment. He ought therefore to invite regular explanatory conversations with the class for his own sake as well as theirs, for it cannot be creditable to a teacher to be incomprehensible to any of his pupils.

When the children of operatives form a portion of his class, communication on both sides is still more necessary. As it reveals to him their peculiar deficiencies, he will perceive the propriety (indeed the necessity) of explaining more than strictly belongs to his own science to render it fully understood. They generally hear local names and terms of art widely different from the scientific phraseology, but which nevertheless a lecturer must make himself master of, to become intelligible: in fact, he must translate his words into theirs as he goes on. When he speaks of elements, their thoughts are perhaps fixed on fire, air, earth, and water, which still maintain that rank in some of their school-books: salt conveys only the idea of the culinary variety, and salts (in their conception of the term) is restricted to the pharmaceutical preparations of Cheltenham, Rochelle, Epsom, &c. Gas will most likely be limited to our common source of illumination; spirit is generally used to designate any acid employed in the arts; metal is understood in different senses by the iron-founder, the brazier, the glass-blower, and the road-maker; and tin is particularly applied by mechanics to designate that useful household ware the principal ingredient of which is iron. Our scientific verbs, in many instances, convey no distinct notions to their understanding; and a lecturer who uses them without explanation, might as well speak in a foreign language. He says "the metal is oxidized," where they would only understand the term rusted. He describes an acid as saturated by an alkali, iron as oxidized in the smith's fire, and lime as neutralized by carbonic acid gas; whereas many of his class are perhaps only accustomed to hear, "the acid is killed," "the iron is poisoned," "the lime is dead."

If the conversational system be adopted, all such misunderstandings will quickly be removed, and a sensible lecturer will gladly avail himself of it, as the best means to discover his own defects in teaching. If the impartial questions and lively suggestions of pupils were freely permitted and generally encouraged, various important discoveries would naturally ensue, and many of the systematic errors and absurdities which have hitherto disgraced all sciences, would have been quickly discovered and banished, or perhaps never adoped.

In laying a sound foundation for scientific acquirement in the youthful mind, it is very unwise to encourage (by example or commendation) a taste for hypotheses and speculations. With such an imaginative habit in early life, nothing is easier than to be mistaken. The rage for systematizing is perhaps the greatest bar to the attainment of truth in every branch of knowledge: in the science of chemistry it has been actively mischievous. In the last century, Phlogiston, like a pagan deity, occupied all minds to the exclusion of every important truth which interfered with its ideal existence. Since its downfall, oxygen was maintained to be the sole acidifying principle, and the attraction of the mass upheld as destructive of definite proportions, as strenuously as if their champions could not err. A brief essay of this nature would not have space to enumerate all the chemical hypotheses that were downright errors: the words are now looked on as nearly synonymous. In the interesting sciences of geology, electricity, and meteorology also, this unfortunate taste for generalizing favourite facts has hitherto been very prevalent, and in consequence, the authors have been universally suspected; insomuch that an experienced reader learns to distrust their opinions and inferences as completely as if they proceeded from professional advocates on a point of law. A novice in science, however, too often acquires corresponding bad habits of speculating and taking things for granted, and frequently loses valuable time in the study of authors who dogmatize on the laws and operations of nature, as confidently as if they were in her secrets.

Young students are frequently deceived by this plausible and dictatorial style, and forego the right of thinking for themselves, till they are roused from their false security by reading contradictory accounts, assigning different causes for the same effect, and thus fortunately discover that both parties have been unblushingly stating their guesses as matters of fact.

Perhaps the greatest improvement in education, effected in modern times, is the system of mutual instruction which has been so happily applied to the diffusion of chemical knowledge in the national institutions of France. On this plan, each of a numerous class of pupils undertakes in turn to deliver, or assist at a lecture on a given portion of the science, (on carbon, for instance,) under the direction of the professors. This creates a necessity that each shall thoroughly uuderstand the part to which he applies himself, to enable him to instruct others with credit and effect. He must be prepared to make experiments, to answer questions, and explain the difficulties of the subject to his companions; and it is invariably found that in these circumstances they learn with quickness and satisfaction. Whether this proceeds from sympathy and purity of reasoning on their part, or from the absence of all display and repulsive pretension on his, whether their attention is rivetted by the novel sight of their companions successively appearing as lecturers, or that the science is simplified by the familiar and modest language which is naturally employed on these occasions, it is certain the effect is most beneficial, and the plan consequently worthy of earnest attention.

In a public laboratory where the system of mutual instruction and investigation is adopted, students soon perceive the value of each other's company. Almost every one is possessed of some peculiar character or turn of mind, from which his companions may derive a benefit they could not separately have attained. For instance, the talent of one is to originate ideas, to invent; of another, to seize on and apply the invention to purposes of utility; of a third, to follow up these notions by a patient experimental reseach conferring satisfaction and certainty by every step in his progress; the taste of a fourth lies in reading, and he brings valuable collateral information to bear on the subject under examination; while a fifth amuses himself in talking about the matter to every one, and collecting various opinions and advice for the benefit of his more studious companions. The attribute of a sixth is foresight, of that peculiar species, which conjures up objections of every probable and possible shape, and thereby ensures to his more sanguine friends the advantages of experience, without the loss of time and labour usually paid for it. In addition to these may be observed an embryo critic, possessed of that happy talent which guides some minds almost instinctively to the clear perception of a lurking error, as something vitally noxious, employed in separating facts from mistakes, detecting the "beggings of the question" which his eager companions are unconsciously indulging in, and good humouredly proving their profound ignorance of all that is not established on the certain basis of experiment.

Wherever this system is applicable, similar happy results await its introduction. An experienced pupil and a novice, placed together at a table or a furnace, will proceed to acquire knowledge with double efficacy. Two pairs of hands are necessary in many operations, and two heads are always better than one. The younger is continually asking questions which induce the elder to search into the stock of knowledge actually in his possession, to arrange, and to state it intelligibly in answer. The younger acquires by example a facility of operating, and as a matter of course, day after day, receives a detail of the experience of the elder in familiar language at the instant he has need of it, and an opportunity of applying it to use. This practical instruction, when associated with his own experimental proof of its correctness and value, becomes indelible. Again, one can refer to books, while the other attends to the work; one may consult the professors on difficult points, while the other watches the progress of an experiment, and records the necessary observations. Both co-operate in emergencies, and sympathize in success and disappointments.

Where such an arrangement for the diffusion of chemical science is adopted by a public institution in these kingdoms, its resident lecturer may be relieved from much of the toil of private instruction generally allotted to him under the present system. His cares for his working pupils may then be limited (as in France) to directing their studies, allotting proper portions of the science for their own lectures, suggesting appropriate subjects for their investigation, explaining the rationale of new phenomena, and giving a word of assistance to all as they require it while engaged in working out their own information.

He may commit to their zeal and activity the determination of all matters of minor interest, or of mere curiosity, which would otherwise seriously encroach on his valuable time, but which may be made excellent assays for their practice. Working pupils may soon be made tolerable assistants; and by a little arrangement to apportion experimental labours to their several degrees of skill and knowledge, the lecturer may superintend a dozen investigations and analyses in operation at once around him, and effect more in one season by their judiciously combined efforts, than he could unaided, in ten.

His proper sphere would then be superintendance. Like the captain of a ship, he would generally be able to effect more by directing others in their operations, than by working himself;

inasmuch as the labour of the head is more valuable than that of the hands.

The only inconvenience which in France has been found to result from a pursuance of this system of "mutual instruction" is, that the pupils by their combined efforts sometimes outstrip their teachers, if the latter neglect to go hand in hand with them in the race of knowledge: however the success of Pestallozzi's mode of education (whose tutors in many sciences learn with, or only one lesson before, their scholars) has almost overthrown the necessity of instructors assuming scientific omniscience and infallibility as indispensible attributes.

It is delightful to observe what progress is made in any science by social communication. In chemistry it is particularly conspicuous. For example, Robert mentions a fact of which Richard was ignorant, and which completely enables the latter to understand another analogous case that was previously incomprehensible to him for want of it. He joyfully announces the new light which has burst upon him, and the enlarged views it opens. Robert's fact is now illuminated by Richard's commentary, and thus, returning with interest to the former, reciprocally extends his sphere of mental vision. In this way both cheerfully climb the heights of discovery, alternately pulling or pushing each other upwards, till they gain an elevation far beyond the power of either separately to attain: now confirming each other's views by agreement; then rendering assurance doubly sure by a difference of opinion which leads to a closer investigation: frequently discovering and freeing each other from long-cherished errors, which had hitherto like fetters impeded their ascent; and which they would have borne, perhaps through life, had they travelled singly and selfishly.

Thus both reciprocally enjoy the double advantages attaching to the characters of tutors and pupils. As comrades they assist, and as councillors they advise. Their pursuits and intimacy constitute them excellent judges of each other's notions, proceedings, and general character, and insure to each in turn the benefit of the sagacity of both. These advantages seldom terminate with their common labours. The associates have become warmly interested in each other's cares and amusements, sorrows and joys. Continually engaged in acquiring a similarity of knowledge and identity of opinions, enjoying the pleasure of assisting, as well as the benefit of assistance, and observing in each other the various estimable qualities induced by their peculiar course of study, their intimacy naturally improves into a friendship built on the best foundations; participation of elevating knowledge which never satiates, interchange of good offices, and respect for the talents or industry they severally evince, and have constant occasion to appreciate.

Article XIII.

Memoir on a peculiar Substance contained in Sea Water. By M. Balard, Apothecary and Chemist to the Faculty of Sciences, at Montpelier.*

I had repeatedly observed, upon treating the washings of the ashes of the fucus which contain iodine, with an aqueous solution of chlorine, that after having added a solution of starch, there was not only a blue colour, occasioned by the iodine, but also a little above it, a yellowish colour of considerable intensity.

This orange yellow colour was also apparent when the mother water of our salt-works was treated in the same manner; and the tint was strong in proportion to the concentration of the liquid. The production of this colour is accompanied with a peculiar penetrating smell.

I examined the nature of this colouring principle, and my first attempts led me to the following observations:

1. The mother water of the salt works, treated with chlorine, loses its colour and characteristic odour when it is exposed for a day or two to the air, and chlorine does not afterwards reproduce the same phenomenon.

2. If it be treated with the alkalies or their sub-carbonates, the smell and colour are also lost.

3. The same effects are produced when any reagent is added to the coloured fluid, which yields it hydrogen, either directly or by the intervention of water.

These effects are produced by sulphurous acid, ammonia, sulphuretted hydrogen, the hydrosulphurets, but especially by a mixture of zinc and sulphuric acid, which presents nascent hydrogen to the fluid.

4. When the fluid has been decolorized by the alkalies or bodies containing hydrogen, the addition of chlorine restores the original colour.

Two explanations naturally present themselves to account for these various phenomena; in the first place, it may be supposed that the yellow matter is a compound of chlorine with some substance contained in the mother water of the salt-works; in the second place, it may be imagined that the colouring matter had been evolved from some of its combinations, by the chlorine, and that this had taken its place.

To determine which opinion to adopt, it was requisite to obtain the colouring matter in a separate state; its volatility afforded some hope that distillation would be sufficient to separate it from the liquid, and I had recourse to this process.

* From the Annales de Chemie and de Physique, xxxii. p. 337.

The salt water possessing its yellow tint, when subjected to distillation, does in fact evolve, almost as soon as it boils, very thick vapours that are condensed by cooling into a liquid, which I found to possess the greater number of the properties of the coloured liquor; but they were not so distinctly marked. This liquid was of a reddish yellow colour, its smell somewhat resembled oxide of chlorine, it was not acid, lost its colour by the action of the alkalies of sulphurous acid and sulphuretted hydrogen, &c. and, in fact, by all the re-agents which decolorized the water of the salt-works itself after the action of chlorine. It cannot hence be doubted that this first product of the distillation contained the substance in question, especially as the remainder of the liquid had lost in this respect all its original properties. Its colour had disappeared: instead of its penetrating smell, there remained only an ethereal smell, which I shall again mention. Chlorine had not the power of restoring the yellow colour.

In order to obtain this substance in a pure state, it remained only to separate it from the water which was volatilized with it. With this intention I passed the orange vapours over chloride of calcium. They were condensed of a deep red colour in small drops, which were very volatile, filling the small vessel in which they were contained with vapours in colour resembling nitrous vapour. I believed that I had thus obtained the colouring matter in a state of purity, but the process was unproductive. I reckoned that an operation had succeeded when it gave me one drop of the liquid. Such minute quantities of matter allowed only experiments almost microscopic. I owe to them, nevertheless, the first essays which I made upon the nature of this substance, and the researches which I afterwards made on a larger scale confirmed them.

I was at first induced to take this substance as a chloride of iodine, different indeed from those compounds which are already known to chemists; it was in vain that all my trials were directed to this end. It gave no blue colour with solution of starch, nor with solution of sublimate; and as it gave a white precipitate with protonitrate of mercury, and also with nitrate of lead, &c. it was evident that it contained no iodine.

On the other hand, I had repeatedly subjected this substance to the influence of the voltaic pile, and also to a high temperature, but it did not in either case exhibit the slightest appearance of decomposition. Such resistance could not fail to suggest the idea, that I had to do with a simple body, or one which acted in the same manner as simple bodies; and this opinion has been strengthened by every trial to which I have subjected it. I imagined it to be a simple substance, possessing in its chemical relations, the greatest resemblance to chlorine and iodine, and forming analogous combinations; but always presenting physical and chemical properties which furnished the strongest reasons for distinguishing it from them.

M. Anglada advised me to call this substance *Brôme,** deriving this name from the Greek βρωμος (*fœtor.*)

Two processes may be adopted for the extraction of brôme: the first has already been mentioned; it consists in distilling the mother water after the action of chlorine, and condensing the orange vapours which come over at the moment of ebullition.

By this process, which is a slow one, only a small quantity of impure brôme is obtained. I satisfied myself that it occurs always mixed with a ternary combination of hydrogen, carbon, and brôme, analogous in the nature of its properties to hydrocarburet of chlorine. On these accounts I abandoned this process when I had discovered another, more easy of execution, and giving purer brôme and in larger proportion. This process consists in treating the mother water with chlorine, and I then pour some ether upon the surface of the liquid, and I fill the vessel entirely; by strongly agitating these two liquids afterwards so as to mix them, and then leaving them some moments to allow of their separation, the ether floats, having assumed a fine hyacinthine red colour, whilst the mother water becomes colourless, and instead of the penetrating and irritating smell of brôme, it has merely that of the ether which it holds in solution.

The coloured ether, which is a true ethereal solution of brôme, when agitated with an alkaline substance, and especially with caustic potash, loses its colour and disagreeable smell. The potash absorbs the brôme, and by successively agitating the yellow mother water with ether, and the coloured ether with potash, I succeed in combining all the brôme of a great quantity of the mother water with a small proportion of alkali. The potash gradually loses all its alkaline properties, and is converted into a saline matter which is soluble in water, and crystallizes in cubes by the evaporation of the liquid: it is these cubic crystals that I successfully employ for the preparation of brôme; I mix these crystals, after pulverizing them, with purified peroxide of manganese, and upon this mixture, put into a small distilling apparatus, I pour sulphuric acid diluted with half its weight of water. This acid, if it were mixed with the crystals alone, would extricate white vapours and very little brôme, and the same effect is produced if it be used with the mixture of salt and manganese, in a more concentrated state, but employed as directed it produces orange vapours that condense into small drops of brôme, and which may be collected by immersing the end of the retort into the bottom of a small receiver filled with cold water; the vapours of brôme dissolve in the water; that which

* A notice of the discovery of this substance, under the name of *Muride,* was given in last month's *Annals.*—*Edit.*

condenses in the neck of the retort precipitates to the bottom of the vessel on account of its great specific gravity. Whatever may be the affinity which water possesses for this body, the stratum of liquid which surrounds it is very soon saturated, and this surrounding the brôme, it secures it from the solvent power of the superior strata; to obtain it in a state of great purity it is afterwards necessary only to separate it and to deprive it of the water which it may retain, by distilling it from chloride of calcium.

The properties of brôme are, that, when examined in mass by reflected light, it is a blackish-red fluid, but when a thin stratum is placed between the light and the eye, it is of a hyacinthine red colour.

Its disagreeable smell reminds one of that of the oxides of chlorine, but it is much less intense; its taste is extremely strong; it acts upon organic substances, upon wood, cork, &c.; it corrodes the skin especially, giving it a deep yellow colour; this colour, which is less intense than that produced by iodine, like it, disappears after some time; and if it have remained in contact with the skin for some time, the colour disappears only when the epidermis is destroyed.

It acts strongly upon animals, a single drop put into the bill of a bird killed it; its specific gravity, as nearly as I could ascertain it with the small quantities of the substance, was 2·966, and when exposed to a temperature of 18° centig. it is not rendered solid; it is readily volatilized, which is a great contrast to its specific gravity; when a drop of brôme is put into any vessel, it is immediately filled with a deep orange red vapour, which by its colour might be mistaken for nitrous acid, if it were not distinguishable from it by numerous properties. It boils at 47° centig. heat, which thus varies the physical state of brôme, has no action at all upon its chemical nature. I did not find any decomposition, by passing its vapour through a luted glass tube heated strongly red: it is not a conductor of voltaic electricity; I ascertained this by preventing the decomposition of water, by interposing a portion of brôme three or four lines in length in one of the conductors: neither does electricity appear capable of decomposing brôme; this substance, when submitted to the action of a pile, strong enough to decompose water and saline solutions, suffered no apparent diminution of volume, no evolution of gas, nor any deposit of matter upon the ends of the platina conducting wires. In a word, it gives no indication of decomposition.

The vapour of brôme does not support combustion; a lighted taper when immersed in it is soon extinguished, but before it goes out, it burns for an instant with a flame which is green at the base, and red in the upper part, just as it does in chlorine gas.

Brôme is soluble in water and alcohol, and especially in ether, it is but very slightly soluble in sulphuric acid; olive oil acts slowly upon it; it does not redden tincture of tournsol, but decolorizes it rapidly, very much like chlorine. Solution of indigo in sulphuric acid is also decolorized by it.

The great analogy which I had remarked between the action of brôme and that of chlorine upon vegetable colours, made me think that it existed also between the causes of these phenomena; and that brôme, having affinity for hydrogen, probably took it as chlorine does from organic bodies which are put in contact with it. This was the motive which directed my experiments in my researches after a combination of hydrogen with brôme.

I first tried to make them act directly upon each other, but without success. My trials were more fortunate when I put brôme in action with several gaseous compounds of hydrogen. I obtained by this method a colourless gas, strongly acid, which when absorbed by potash reproduced the cubic crystals which I had already obtained, by agitating the alkali with ether containing brôme.

I afterwards tried to procure from these crystals the gaseous matter which they seemed to contain. When treated with concentrated sulphuric acid, they evolved an acid gas which I recognised as hydrobromic acid, when I had found that chlorine decomposed it, disengaging vapours of brôme, and that certain metals, by taking this substance from it left only pure hydrogen. This acid may be prepared by several processes:

1. I exposed during some time hydrogen mixed with the vapour of brôme to the solar rays, without observing any sensible combination; but I found that hydrobromic acid gas was produced, by exposing the mixture to the flame of a taper, or still better, by introducing an ignited iron wire into the receiver which contained it.

In all these cases, the action is not propagated throughout the whole mass, as occurs with chlorine and hydrogen; the combination is produced only around the hot body which occasions it. It probably would not have so happened, if I had been able to collect and measure the vapours of brôme, and to have mixed them with determinate proportions of hydrogen.

2. Hydriodic acid gas, and sulphuretted and phosphuretted hydrogen gases are decomposed by brôme, which is changed into hydrobromic acid, by separating the vapours of iodine, sulphur, and phosphorus; this decomposition is always effected with the disengagement of heat.

The volume of gas does not sensibly alter when hydriodic acid gas is decomposed by brôme; and it increases, when the decomposition of sulphuretted and phosphuretted hydrogen is effected by it. Brôme acts in the same way upon these compounds of hydrogen when they are dissolved in water, and hydrobromic acid is formed at their expense.

3. Hydrobromic acid may be procured by decomposing the cubic crystals of brôme and potash with sulphuric acid, but the gas so obtained is often mixed with a small quantity of sulphurous and muriatic acid gases, which prevents the employment of this method when the hydrobromic acid is wanted perfectly pure.

4. To obtain this acid in a state of purity, I had recourse to a process, borrowed to a certain extent from that which is employed for the preparation of hydriodic acid gas. Brôme and phosphorus when put together and moistened with a few drops of water, give out an abundance of hydrobromic acid gas, which may be received over mercury.

Hydrobromic acid gas is colourless, its taste is quite acid. When exposed to the air it exhales white vapours, which are denser than those produced in the same way from muriatic acid. These vapours have a very penetrating smell, and occasion violent coughing.

Hydrobromic acid is not decomposed when it is passed through a red hot tube of glass; nor does it suffer decomposition if previously mixed with oxygen, and then passed through the red hot tube, nor is any effect produced by putting a taper in the mixture.

On the other hand, brôme does not appear to be capable of decomposing water, as chlorine does. I did not find either that oxygen was disengaged, or hydrobromic acid formed, by passing brôme and the vapour of water through a red hot glass tube.

Hydrobromic acid is decomposable by chlorine, which, uniting with its hydrogen, produces immediately abundant orange red vapours, and a deposition of small drops of brôme. In operating over mercury these drops are soon absorbed by the metal, and the gaseous matter which remains after the action possesses all the characters of muriatic acid. Certain metals also decompose hydrobromic acid gas. It appeared that when it was pure, mercury did not occasion any alteration; but tin and potassium decomposed it entirely, the first at a moderately high, and the latter at the usual temperature. A fragment of potassium passed into a graduated tube full of this gas loses its metallic brilliancy in a few seconds, and is converted into a white matter which gives out brôme by the action of chlorine; the volume of the gas is exactly reduced to one half in this experiment, and the residual gas is hydrogen; according to this experiment hydrobromic acid gas is similarly constituted to hydriodic and muriatic acid gases; that is to say, it is formed of equal volumes of hydrogen gas and the vapour of brôme, without either increase or diminution of volume.

Hydrobromic acid gas is very soluble in water: the solution may be prepared, either by treating a solution of sulphuretted hydrogen with brôme, or by causing the acid gas disengaged by any of the processes described to pass into water; the solution becomes hot, increases in volume, acquires great density, and the property of exhaling white vapours when exposed to the air: when properly prepared, this solution is colourless; but if the hydrobromic acid gas is mixed with vapour of brôme, the solution becomes of a deep orange-red colour. It may have this colour imparted to it by shaking the solution with brôme, and it dissolves more of it than equal volumes of water. This solution may be termed *bromated hydrobromic acid;* if it be heated, vapours of brôme and hydrobromic acid are both evolved, and a solution of acid remains which is nearly colourless, but less concentrated.

Chlorine immediately decomposes solution of hydrobromic acid, and gives it a tint of uncombined brôme; nitric acid acts upon hydrobromic acid less suddenly, but with more energy as soon as re-action has commenced; much brôme is produced, and probably water and nitrous acid; a fluid is obtained analogous to aqua regia, and which dissolves gold and platina. Sulphuric acid possesses to a certain extent the power of decomposing hydrobromic acid; so that it is not uncommon, when this gas is evolved by means of sulphuric acid, to see vapours of brôme and sulphurous acid formed by a re-action, the cause of which will be easily understood.

(*To be continued.*)

Article XIV.

Proceedings of Philosophical Societies.

MEDICO-BOTANICAL SOCIETY OF LONDON.

The First General Meeting of this Society was holden on Friday, the 13th of October, at eight o'clock, p.m. Sir James M'Gregor, Director General of the Army Medical Board, President, in the Chair.

After the usual business of the Society had been gone through, a letter from his Royal Highness the Duke of Clarence was read, desiring his name to be added as an Honorary Patron, and regretting that his residence at Bushey Park excluded the possibility of his attendance that evening.

The Director (Mr. Frost) then delivered his Oration, in which he congratulated the Society on the rapid advance it had made during the last Session, and the great benefit it had derived from the unwearied zeal which many of its members exerted in its behalf. He also informed the meeting that their distinguished President had lately ordered "that no person shall be admitted to an examination to qualify him to practise in the medical department of the army without having attended, amongst other branches of science, lectures on botany for six months;" the salutary effects of which regulation would, in a few years, demonstrate its utility.

Sir James rose to address the meeting, and assured them that he was but performing his duty in enforcing the regulation just mentioned, or any other of a similar kind, which might, in any degree, be conducive to the extension of practical and useful knowledge in that department with the direction of which he had been entrusted, and concluded by moving that the thanks of the Society be given to the Director, and that he be requested to make his excellent Oration more public.

The Chevalier Castillo, Consul-General in London for Spain, was introduced, and admitted as a Foreign Member of the Society.

Several letters from distinguished foreigners were read, among whom were Baron Humboldt, Baron Ferussac, Mr. Wyttenbach; and Mr. Jacquin, whose diploma of Honorary Member was intrusted by the President to Mr. Vivenot, of Vienna.

A communication from his Majesty's Vice-Consul for Guatimala, Mr. Schenly, was read, and the meeting adjourned to Friday the 10th day of Nov. 1826.

Article XV.

SCIENTIFIC NOTICES.

Chemistry.

1. *On the Confinement of Dry Gases over Mercury.*

The results of an experiment made by Mr. Faraday, and quoted as such, having been deemed of sufficient interest to be doubted, he has been induced to repeat it, and though the original experiment was not published by him, he is inclined to put the latter and more careful one upon record, because of the strong illustration it affords of the difficulty of confining dry gases over mercury alone. Two volumes of hydrogen gas were mixed with one volume of oxygen gas, in a jar over the mercurial trough, and fused chloride of lime introduced, for the purpose of removing hygrometric water. Three glass-bottles, of about three ounces capacity each, were selected for the accuracy with which their glass stoppers had been ground into them; they were well cleaned and dried, no grease being allowed upon the stopper. The mixture of gases was transferred into these bottles over the mercurial trough, until they were about four-fifths full, the rest of the space being occupied by the mercury. The stoppers were then replaced as tightly as could be, the bottles put into glasses in an inverted position, and mercury poured round the stoppers and necks, until it rose considerably above them, though not quite so high as the level of the mercury within. Thus arranged they were put into a cupboard, which happened to be dark, and were sealed up. This was done on June 28, 1825, and on September 15, 1826, after a lapse of fifteen months, they were examined. The seals were unbroken, and the bottles found exactly as they were left, the mercury still being higher on the inside than the outside. One of them was taken to the mercurial trough, and part of its gaseous contents transferred; upon examination it proved to be common air, no traces of the original mixture of oxygen and hydrogen remaining in the bottle. A second was examined in the same manner; it proved to contain an explosive mixture. A portion of the gas introduced into a tube with a piece of spongy platina caused dull ignition of the platina; no explosion took place, but a diminution to rather less than one-half. The residue supported combustion a little better than common air. It would appear, therefore, that nearly a half of the mixture of oxygen and hydrogen had escaped from it, and been replaced by common air. The third bottle, examined in a similar manner, yielded also an explosive mixture, and upon trial was found to contain nearly two-fifths of a mixture of oxygen and hydrogen, the rest being a very little better in oxygen than common air.

There is no good reason for supposing that this capability of escape between glass and mercury is confined to the mixture here experimented with; probably every other gas, having no action on the mercury or the glass, would have made its way out in the same manner. There is every reason for believing that a small quantity of grease round the stoppers would have made them perfectly tight.—(Journ. of Science.)

2. *Cafein.*

M. Garot adopts the following method of preparing this substance:—A quantity of bruised raw coffee was twice infused in boiling water: the brown liquors, when cold, were mixed; on the addition of a solution of acetate of lead, a very abundant precipitate of a pistacio green colour was obtained. The liquor after filtration was yellowish, but after separating the excess of acetate of lead by means of sulphuretted hydrogen, it became nearly colourless; the free acid remaining in solution was saturated by ammonia, and by careful evaporation, crystals of cafein are obtained, which, by purification, were procured in colourless silky crystals. It appears by these experiments, that the colouring and extractive matter of the coffee are precipitated by the oxide of lead, while the cafein, not combining with it, crystallizes afterwards from the filtered infusion.—(Journal de Pharm.)

3. *Iodine found in the Mineral Spring of Bonnington, near Leith.* (Extract of a Letter to Prof. Jameson from Dr. Tucker.)

.....The Bonnington mineral water, in addition to the other substances hitherto discovered in it, contains iodine, which may be readily detected by the following method:—Evaporate a pint of the water to dryness; take up the soluble parts in a drachm or two of a diluted solution of starch quite cold, and add a few drops of concentrated sulphuric acid; the characteristic blue colour will then make its appearance. I prefer the use of sulphuric to nitric acid or chlorine for decomposing the hydriodic acid, for it effects that object with certainty, and does not decompose the iodole of starch, or prevent its formation, as the last two are apt to do.

The greater part of the iron in the Bonnington water is under the form of the carbonate of iron, which is held in solution by carbonic acid. It also contains the muriatic and sulphuric acids in combination with lime, magnesia, and soda; the last of which is the predominating base. Potash is also present, and forms the hydriodate of potash with the hydriodic acid. Its quantity, however, is more than sufficient for saturating that acid; for the residual salts still contain it after the hydriodate of potash has been removed by alcohol.

I have examined portions of water, the springs of Harrowgate, Moffat, and Pitcaithly, but could discover in them no trace of iodine...... (Edin. New Phil. Journ.)

4. *Fluidity of Sulphur at common Temperatures.*

Mr. Faraday having placed a Florence flask containing sulphur upon a hot sand-bath, it was left to itself. Next morning the bath being cold, it was found that the flask had broken, and in consequence of the sulphur running out, nearly the whole of it had disappeared. The flask being broken open, was examined, and was found lined with a sulphur dew, consisting of large and small globules intermixed. The greater number of these, perhaps two thirds, were in the usual opaque solid state; the remainder were fluid, although the temperature had been for some hours that of the atmosphere. On touching one of these drops, it immediately became solid, crystalline, and opaque, assuming the ordinary state of sulphur, and perfectly resembling the others in appearance. This took place very rapidly, so that it was hardly possible to apply a wire or other body to the drops quick enough to derange the form before solidity had been acquired: by quick motion, however, it might be effected, and by passing the finger over them, a sort of smear could be produced. Whether touched by metal, glass, wood, or the skin, the change seemed equally rapid; but it appeared to require actual contact; no vibration of the glass on which the globules lay rendered them solid, and many of them were retained for a week in their fluid state. This state of the sulphur appears evidently to be analogous to that of water cooled in a quiescent state below its freezing point; and the same property is also exhibited by some other bodies, but I believe no instance is known where the difference between the usual point of fluidity and that which could thus be obtained is so great: it, in the present instance, amounts to 130°, and it might probably have been rendered greater if artificial cold had been applied.—(Journal of Science.)

5. *Detection of Arsenic.*

The following "elegant test of the precise nature of the metallic crust (viz. that obtained by Dr. Christison's method of detecting arsenic) when its quantity is too minute for its physical characters to be unequivocally ascertained, was communicated to Dr. Christison by Dr. Turner, Lecturer on Chemistry in Edinburgh. It consists in chasing the crust up and down the tube by heat till it is all oxidated; *when it assumes the appearance of sparkling crystals, which may be ascertained by a microscope of four powers to be octohedra.*"—(Extract from Edin. New Phil. Journ.)

Mineralogy.

6. *Analysis of Halloyite.*

This mineral has been analysed by M. Berthier, it is found at Angleure, near Liège; it occurs in kidney form, or tubercular masses larger than the fist, among the ores of iron, zinc, and lead, which occupy the cavities of the transition limestone of the north, and which are especially so common in the provinces of Liège and Namur. M. Omalius d'Halloy is the first who noticed it some years since; mineralogists will therefore undoubtedly approve the name given to this substance, as that of a philosopher who has so greatly contributed to the study of geology.

Halloylite is compact, its fracture is the waxy conchoidal; it may be indented by the nail and polished by rubbing with the finger; its colour is pure white or white slightly shaded with greyish blue; it is transparent at the edges, and adheres strongly to the tongue. When small pieces are put into water it becomes transparent like hydrophane; air is given out, and its weight is increased about one-fifth. By calcination it loses 0·265 to 0·280 of water, becomes very hard and milk white.

If it be powdered and exposed for some time to a temperature of nearly 212°, it loses water, for after that it does not lose more than 0·16 by calcination. The powder dried, but not calcined, rapidly absorbs water when put into it, or when it is left exposed to moist air. Sulphuric acid readily acts upon it, even cold; it separates gelatinous silica, which dissolves perfectly in the alkalies. By analysis after drying in a stove it yielded

Silica	44·94
Alumina	39·06
Water	16·00
	100·00

The alumina contained a little iron, which renders it probable that the blue tint of the mineral may be owing to the presence of phosphate of iron.—(Annales de Chimie.)

7. *Cold produced by Combination of Metals.*

According to M. Döbereiner, when 118 parts of tin, 207 of lead, 284 of bismuth, and 1617 of mercury were mixed at the temperature of about 65° Fahr. it immediately fell to 14°.—(Ibid.)

Zoology.

8. *Notice on the Digestive Organs of the Genus Comatula of Lamarck, and on the Crinoidea of Miller.* By J. E. Gray, Esq. FGS.

Having lately occasion to examine a specimen of Comatula preserved in spirits, I was struck by observing that the proboscis-like tube described by Peron, Lamarck, Miller, &c. as the mouth of this animal, was not situated in the centre of the body, but at an intermediate distance between the centre and the margin, on the smooth place intermediate between the arms. On examining the centre, I found a distinct, rather large, aperture, which certainly led on to the intestinal cavity. Now from the situation of the latter opening, which is similar to the mouth of Asterias, and from its form, I am inclined to consider it as the mouth. This hole could only have escaped the view of the above-quoted authors by their having examined dried specimens only. This central aperture is pentagonal, surrounded by a fringe which diverges at its angles, sending out towards the margin, and dividing before it reaches the edge so as to give a double fringe-line to each arm, which extends up its centre, and sends off a process to the inner edge of each of the fingers, so as to ciliate their inner edge. This continuation of the abdominal integuments is doubtlessly intended for the motion of the arms. I was not enabled to examine the internal structure of the specimen, so that I cannot speak with certainty with regard to the uses of this aperture, but the central one did not appear to be provided with any teeth. The tubular process is contracted at its end, and furnished with 10 small short filiform tentacula; and upon blowing with a tube into the central one, the cavity of the abdomen was dilated, and the air came out of the tube.

Mr. Miller, in his description of the genus Comatula, appears to have only examined a dried species of the genus, and to have taken his account of the mouth from Lamarck, as he compares it with the mouth of the *Crinoidea,* which evidently appears to belong to the same group; but he describes the mouth of that family as being "able to be protruded into the form of an elongated proboscis."

On consulting the abdominal integuments of the specimen formerly belonging to Mr. Tobin, now in the British Museum, of *Pentacrinus Asteria, Koenig,* (the *P. Caput Medusæ* of Muller, figured by Muller, in his *Crustacea,* Plate 2, f. 8, it very nearly agrees with the abdominal surface of the *Comatulæ,* and the part marked *x* in the figure is the true mouth, but I could not discover any traces of the tubular vent; and on examining the pelvis of the specimens of the fossil species of Crinoidea in the same collection, I was equally at a loss to discover any traces of the latter part; and the central or subcentral hole (mouth) in several of the specimens, appears to be produced into a kind of proboscis; but sometimes that part itself is difficult to discover, so that I am not, from the apparent absence of the part, perfectly convinced that it does not exist in the recent animal. I may also observe, that there is no trace in the fossil species, of the radiating muscular lines which surround the mouth of the Comatulæ.

The Comatulæ, on account of their possessing a double aperture to their digestive organs, should certainly be separated from the *Asteroida* or the *Asteriæ* of Linné; and as they so greatly resemble the only recent species of *Crinoidea* at present known, I should certainly place them in the latter family, till specimens of the latter can be observed alive, or which have been kept in spirits, so that the absence or presence of the second aperture may be distinctly traced.

The foregoing observation was made on a specimen of *Comatula Mediterranea* of Lamarck, which appears to be the *C. fimbriata* of Miller, which is certainly distinct from the *C. fimbriata* of Lamarck; but since that time, having had an opportunity of examining *C. carinata,* Lamarck, or a very closely allied species, I find nearly the same structure, but that the tubular proboscis is bent down towards the centre as if by a suture, so that the openings are very close together, and the muscular ridges are stronger both on the abdominal integument and on the fingers; and it appears to be this part which forms the fringe of them: I may also add, that from the examination of a very mutilated specimen of this genus, the abdominal cavity is extended to the internal concavity of the crustaceous plate, to which the dorsal arms are attached, in the same manner as it is produced down the stem of the *Encrinus*, so that there appears every reason to believe that this genus, and the *Crinoidea* of Miller, should form one family, to which the name of *Encrinidæ* might be attached, on the right of priority.

While on the subject of the star-like animals, I may remark, that after examining numerous specimens of *Ophiura* and *Euryale*, preserved in spirits, I have not been able to find any provided with the *corpus spongiosus* of Spix, and this, together with their want of *Ambulacra* on the lower surface of the rays, for the passage of the sucker, incline me to form them together into a family under the name of *Ophiuridæ*, and to leave only the genus *Asterias* of Lamarck, which is capable of being separated into two or three groups, as the family *Asteriadæ*, whose habits and manners of living differ greatly from the former family.

9. *Splendid Collection of Shells.*

We are happy to announce that a further opportunity of becoming possessed of specimens from the splendid collection of shells, formed by the late Earl of Tankerville, is about to be offered to the public. This matchless collection, together with a large addition from several other collections, consisting in the whole of about 4000 species, and more than 40,000 specimens, will soon be brought forward for sale, by public auction, by Mr. G. B. Sowerby.

The collections may be seen at 156, Regent-street, where also may be obtained copies of a plan for the disposal of them, affording peculiar advantages to naturalists and scientific collectors.

Article XVI.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Elements of Logic, containing the substance of the article on that subject, in the Encyclopædia Metropolitana; by the Rev. R. Whately, DD.

A Treatise on the Steam-Engine, Historical, Practical, and Descriptive; by John Farey, Engineer. Illustrated by numerous engravings, by the late Mr. Lowry.

A Personal Narrative of a Journey from India to England, by Bassorah, Bagdad, the Ruins of Babylon, &c. in the Year 1824. By Capt. the Hon. G. Keppel.

A Sequel to the Diversions of Purley; containing an Essay on English Verbs, with Remarks on Mr. Tooke's Work, and on some Terms employed to denote Soul or Spirit; by John Barclay.

On Galvanism, with Observations on its Chymical Properties and Medical Efficacy in Chronic Diseases, with Practical Illustrations. Also Remarks on some Auxiliary Remedies, with Plates; by M. La Beaume.

JUST PUBLISHED.

An Essay upon the War Galleys of the Ancients; by John Howell. 8vo. With 11 Plates. *5s.*

Mathematics practically applied to the Useful and Fine Arts; by Baron C. Dupin: adapted to the State of the Arts in England, by George Birkbeck, MD. No. I. 1*s.*

Lardner's Trigonometry. 8vo. 12*s.*

Areas of Circles. 12mo. 3*s.*

Hooper on the Brain. 4to. *2l.* 12*s.* 6*d.*

Collections from the unpublished Writings of the late C. H. Parry, MD. 2 vols. royal 8vo. 1*l.* 12*s.*

Plain Advice for all Classes of Deaf Persons, the Deaf and Dumb, &c. *5s.*

Article XVII.

NEW PATENTS.

T. R. Williams, Norfolk-street, Strand, for an improved method of manufacturing hats and caps with the assistance of machinery.—Sept. 18.

J. R. Chard, Somersetshire, lace-manufacturer, for improvements in machinery for making net, commonly called bobbin or twist-net.—Oct. 4.

F. Halliday, Ham, for certain improvements or apparatus used in drawing boots on and off.—Oct. 4.

T. Jones, Coleman-street, accountant, for an improvement on wheels for carriages.—Oct. 11.

W. Mills, Hazelhouse, Bisley, Gloucestershire, for an improvement in fire-arms.—Oct. 18.

W. Church, Birmingham, for improvements in printing.—Oct. 18.

S. Pratt, New Bond-street, camp equipage manufacturer, for improvements on beds, bedsteads, couches, seats, and other articles of furniture.—Oct. 18.

W. Busk, Broad-street, for improvements in propelling boats and ships, or other vessels, or floating bodies.—Oct. 18.

J. Piney, Shanklen, Isle of Wight, and G. Pocock, Bristol, for improvements in the construction of carts or other carriages, and the application of a power hitherto unused for that purpose to draw the same, which power is also applicable to the drawing of ships and other vessels, and for raising weights, and for other useful purposes.—Oct. 18.

Article XVIII.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
Sept. 23	29·86	29·60	29·730	65	58	61·5		SE	Cloudy; showers.
24	29·48	29·44	29·460	64	58	61·0		S	Showers.
25	29·40	29·30	29·350	66	58	62·0	0·150	S	Rain.
26	29·58	29·50	29·540	65	58	61·5		SW	Fair.
27	29·40	29·30	29·350	66	57	61·5	0·200	SW	Rain.
28	29·98	29·66	29·820	65	56	60·5		SW	Cloudy.
29	29·60	29·44	29·520	66	55	60·5		SW	Showers.
30	29·55	29·50	29·525	66	58	62·0	0·060	SW	Fair.
Oct. 1	29·64	29·60	29·620	60	52	56·0		N	Clear.
2	29·78	29·76	29·770	62	54	58·0		NW	Clear.
3	29·70	29·68	29·690	63	51	57·0		NW	Clear; showers.
4	29·70	29·68	29·690	62	52	57·0		NW	Clear.
5	29·76	29·70	29·730	62	53	57·5		W	Clear; showers.
6	29·86	29·84	29·850	61	48	54·5		NW	Fair.
7	29·80	29·79	29·795	61	52	56·5	0·220	SW	Fair; showers.
8	29·73	29·66	29·695	62	51	56·5		SW	Showers.
9	29·66	29·66	29·660	62	52	57·0		SW	Showers.
10	29·70	29·66	29·680	63	52	57·5		W	Showers.
11	29·90	29·90	29·900	64	52	58·0	0·300	W	Showers.
12	29·90	29·90	29·900	65	58	61·5		W	Fair; showers.
13	29·88	29·88	29·880	64	58	61·0		SW	Misty rain.
14	29·88	29·88	29·880	64	58	61·0		SW	Fair.
15	29·60	29·50	29·550	62	58	60·0		S	Fair.
16	29·50	29·33	29·415	60	54	57·0		SW	Fair; clear.
17	29·60	29·60	29·600	60	50	55·0		SE	Fair.
18	29·58	29·58	29·580	62	48	55·0		SE	Fair; showers.
19	29·56	29·54	29·550	63	54	58·5		SE	Fair; clear.
20	29·56	29·54	29·550	64	56	60·0		SE	Fair.
21	29·56	29·55	29·555	64	56	60·0		S	Fair.
22	29·60	29·60	29·600	64	57	60·5	0·100	S	Clear.
	29·98	29·30	29·650	66	48	59·0	1·03	SW	

RESULTS.

Barometer, mean height .... 29·650
Register Thermometer, ditto .... 59·0°
Rain, No. 1, 1·030, No. 2, 2·950.
Prevailing wind, SW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, Oct. 23, 1826. EDWARD C. GIDDY.

Article XIX.

METEOROLOGICAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
8th Mon.							
Aug. 1	N E	30·18	30·12	85	62	—	—
2	N E	30·12	30·06	81	61	—	—
3	N W	30·09	30·06	81	60	—	92
4	N E	30·14	30·09	77	59	—	32
5	N E	30·19	30·14	74	50	—	
6	N W	30·28	30·19	77	56	—	
7	N W	30·28	30·23	78	59	—	
8	S W	30·23	30·13	85	55	—	
9	N W	30·13	30·10	81	59	—	—
10	N E	30·10	30·04	80	55	—	—
11	S W	30·15	30·03	66	48	—	35
12	N W	30·33	30·15	71	46	—	
13	S E	30·33	30·11	78	46	1·39	
14	S W	30·20	30·11	80	50	—	
15	S W	30·20	30·10	75	52	—	—
16	N W	30·23	30·11	73	51	—	
17	N W	30·42	30·23	74	60	—	
18	S W	30·43	30·42	81	52	—	
19	E	30·43	30·18	84	52	·94	
20	E	30·18	30·09	88	57	—	
21	N E	30·09	30·06	77	57	—	
22	S E	30·06	29·97	79	57	—	
23	S W	29·97	29·85	76	58	—	—
24	W	29·89	29·86	76	61	—	
25	S W	29·90	29·89	80	60	—	20
26	S W	30·08	29·90	74	54	·92	
27	W	30·14	30·08	73	50	—	
28	S E	30·14	30·08	78	64	—	
29	N W	30·08	29·88	80	58	—	
30	S E	29·93	29·88	78	56	—	
31	S W	29·93	29·92	75	57	·89	08
		30·43	29·85	88	46	4·14	1·87

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

REMARKS.

Eighth Month.—1. Sultry. 2. Fine. 3. Overcast: a heavy storm about midnight. 4. Rainy night. 5—8. Fine. 9. Fine day: some rain at night. 10. Fine. 11. Rainy. 12—19. Fine. 20. Sultry. 21, 22. Fine. 23. Cloudy. 24. Fine. 25. Fine day: the sky became suddenly overcast about seven, p. m. and a violent storm followed, accompanied with incessant lightning for two or three hours. 26—31. Fine.

RESULTS.

Winds: NE, 6; E, 2; SE, 4; SW, 9; W, 2; NW, 8.

Barometer: Mean height

For the month.. 30·109 inches

Thermometer: Mean height

For the month.. 66·725°

Evaporation.. 4·14 in.

Rain.. 1·87

Laboratory, Stratford, Tenth Month, 24, 1826. R. HOWARD.

METEOROLOGIGAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
9th Mon.							
Sept. 1	N W	29·92	29·86	76	57	—	09
2	S W	29·25	29·86	65	60	—	30
3	E	30·06	29·95	70	51	—	
4	S	30·06	30·05	76	50	—	15
5	N W	30·05	29·65	68	50	—	53
6	S E	29·65	29·32	68	53	—	42
7	S E	29·78	29·65	58	49	—	19
8	S W	29·90	29·78	71	51	—	49
9	E	30·15	29·90	62	57	—	
10	S W	30·33	30·15	64	54	—	
11	N W	30·35	30·33	64	44	—	
12	N W	30·35	30·20	68	46	—	
13	W	30·20	30·05	67	48	·87	
14	W	30·36	30·05	68	44	—	13
15	N E	30·41	30·36	68	46	—	
16	E	30·41	30·11	68	40	—	
17	E	30·11	29·98	73	52	—	14
18	N E	30·02	29·98	68	60	—	34
19	S	30·02	29·98	72	59	—	
20	N E	30·16	30·02	65	48	—	
21	N E	30·28	30·16	66	40	—	
22	E	30·28	30·20	61	32	—	
23	E	30·20	30·00	74	48	—	
24	S E	30·00	29·84	70	44	·90	13
25	S	29·95	29·84	70	39	—	15
26	S	30·04	29·95	70	63	—	35
27	N W	30·28	30·04	68	46	—	
28	S W	30·28	30·24	72	52	—	
29	S E	30·24	29·93	72	55	—	
30	S	29·96	29·91	72	52	·78	02
		30·41	29·32	76	32	2·55	3·43

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

REMARKS.

Ninth Month.—1. Fine. 2. Rainy. 3. Cloudy. 4. Showery evening. 5. A shower at twelve, a. m. 6—8. Rainy. 9—13. Fine. 14. Rainy afternoon. 15—17. Fine. 18. Rainy. 19—23. Fine. 24—26. Mornings rainy: afternoons fine. 27. Fine. 29. Foggy morning: fine day. 30. Cloudy and fine.

RESULTS.

Winds: NE, 4; E, 6; SE, 4; S, 5; SW, 4; W, 2; NW, 5.

Barometer: Mean height

—For the month.................................... 30·051 inches.

Thermometer: Mean height

—For the month.................................... 59·066°

Evaporation .... 2·55 in.

Rain.. 3·43

Laboratory, Stratford, Tenth Month, 24, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

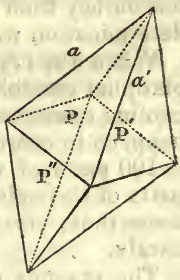
DECEMBER, 1826.

Article I.

On Anhydrous Sulphate of Soda. By Thomas Thomson, MD. FRS. Professor of Chemistry, Glasgow.

There is a manufactory of carbonate of soda near Glasgow, belonging to Mr. Wilson, jun. of Hurlet. The process consists in mutually decomposing protosulphate of iron and common salt. The sulphate of soda thus produced is decomposed and converted into carbonate of soda in the usual manner. Sometime ago they were in the habit of boiling their saturated leys; during which part of the process large crystals were observed to form in the inside of the boilers. Mr. William Wilson, brother of the proprietor of the work, got a number of these crystals, and from the circumstances of their formation, he concluded that they must be anhydrous sulphate of soda. He was so obliging as to bring me a few of them that their real nature might be ascertained in my laboratory.

The crystals were octahedrons with a rhombic base of a very large size; many of them measuring about 1·8 inch in length, and 0·8 inch in breadth. They were translucent, but not quite transparent, and the faces were too rough on the surface to admit of measuring the angles by the reflective goniometer. By a number of measurements with the common goniometer, the inclination of



P on P′ is 75°
P on P″ 140

The first of these is the average of 16 measurements never deviating from each other more than 1°; but the second is the result of one measurement only; for I found only a single crystal in which a part of the lower pyramid was visible. In general, it was totally wanting from the way in which the crystals had grouped together, or rather its faces, in consequence of that circumstance, were rendered so irregular that nothing could be ascertained respecting the inclination of the faces of the two pyramids on each other. The large angle of the pyramid constituting the edge *a* was also measured; but the measurements deviated so much from each other that no conclusion could be drawn. I rely much more upon the measurement of the edge *a′*, because all the different measurements agreed nearly with each other. In some crystals, the face P was much larger than the face P′ making the summit of the pyramid terminate not in a point but a ridge. In some crystals, a four-sided oblique prism was interposed between the two pyramids; but none of these admitted of measurement.

The crystals were firm and solid, and had a glassy appearance. When exposed to an incipient red heat, they underwent no change. When kept about two months in a damp press, they had obviously imbibed moisture; for there was an efflorescence on their surface. When these crystals were heated to redness, they lost about one-third of an atom of water (9·36 grains lost 0·36 grain), the effloresced portion became soft and loose, and could easily be detached from the crystal, leaving the crystalline nucleus as perfect as ever.

The specific gravity of these crystals was 2·645, determined by weighing them in alcohol. In a paper printed in the *Annals of Philosophy* for December, 1825 (vol. x. p. 441), I give the specific gravity of anhydrous sulphate of soda 2·640. This does not deviate much from the present determination. The specific gravity now given has the greatest chance of being correct, as the crystal weighed was hard and compact, and less liable to any inaccuracy than the anhydrous powder, from which the former determination was obtained.

When the crystals were exposed to a strong red heat in a platinum crucible, they underwent the igneous fusion, and on cooling concreted into a foliated brittle saline mass, exactly as happens to common sulphate of soda in the same circumstances.

100 parts of water at the temperature of 57° dissolve 10·58 parts of this salt. When the saturated solution is set aside for some time, crystals of common glauber salt shoot in it abundantly.

The crystals do not affect vegetable blues. Nine grains of them being dissolved in water, and mixed with a solution of 13·25 grains of chloride of barium, abundance of sulphate of barytes fell down, and the residual liquid was neither rendered muddy by sulphate of soda, nor muriate of barytes. This demonstrates that the acid in this salt is precisely the same, and in the same proportion as in glauber salt. Finally, when this salt is dissolved in water and crystallized, it yields common crystals of glauber salt to the very last drop.

These experiments leave no doubt that the salt is really an anhydrous sulphate of soda, as it had been thought to be by Mr. W. Wilson; so that the only difference between it and glauber salt is the absence of all water.

Thus it appears that sulphuric acid and soda are capable of combining and crystallizing without water as well as sulphuric acid and potash. Three distinct species of sulphate of soda are now known to exist.

1. Anhydrous sulphate, crystallizing in a boiling solution, and crystallizing in octahedrons with rhomboidal bases.

2. Common sulphate of soda, containing 10 atoms water, crystallizing in a cold solution, and forming crystals which have the shape of doubly oblique four-sided prisms.

3. Sulphate of soda crystallizing in a supersaturated solution of sulphate of soda made in a high temperature, and set aside for some days in a well-corked phial. The crystals are opaque, white, four-sided prisms, and contain eight atoms of water instead of ten. The first account of this variety was published by Mr. Faraday. There is a description and analysis of it made some years ago by myself in one of my common-place-books. I had forgot the circumstance, till it was brought to my recollection by Mr. Faraday's paper, which I saw for the first time about two months ago in a German journal.

Article II.

On the Reaction of Sulphate of Magnesia and Bicarbonate of Soda. By M. Planche.*

It is known that the bicarbonate of soda and the sulphate of magnesia, in a state of aqueous solution, exercise no reciprocal action in the cold, and that it is only when a certain quantity of carbonic acid has been disengaged by heat, or, in other words, when the alkaline bicarbonate has passed into the state of subcarbonate, that the sulphuric acid prevails over the soda, and leaves the magnesia to the carbonic acid. But I have nowhere seen it mentioned that the two salts mixed together, in a dry state, and in the form of powder, react upon each other. This must at least be the case with regard to their immediate and instantaneous mixture, since in this state they dissolve in water without affecting its transparency, and consequently without any decomposition taking place, or at least any apparent decompo-

* From the Journal de Pharmacie.

sition. Presuming upon this property of the two salts, a physician prescribed several years ago to M. de Sommariva, a mixture of powdered sulphate of magnesia and saturated carbonate of soda. He gave alternately either this mixture alone or bicarbonate of soda. Being charged with the preparation of both these medicines for a journey of three months, which M. de Sommariva made annually to Italy, I always had the precaution of placing the mixture in a very dry state, and divided into parcels in tin canisters, to preserve it from humidity. I used the same precaution with regard to the carbonate of soda. I observed that the sulphate of magnesia was free of hydrochlorate.

During three years M. de S. a man very careful of his health, and besides a good observer, never perceived that cold water became turbid when he dissolved the two salts together in it; but in 1822, having been obliged to prolong his journey beyond the usual time, he laid up a store for a year. Toward the end of the fifth month, M. de S. remarked that the same water which he ordinarily used became slightly milky, and that the change which he rightly attributed, though without being able to explain the cause, to the alteration of the powder, went on increasing as the time advanced. At length, by the seventh month, the precipitate which formed in the water became so considerable, that M. de S. deemed it proper to intermit the use of the powder, and sent for some more, promising to inform me, on his return from Italy, of what, according to his expression, had happened. M. de S. returned at the end of six months, and sent me back the powder in question, which I submitted to the following experiments.

1. This powder put into a quantity of cold water, double that which is necessary for dissolving the two salts, rendered it milky.

2. Dissolved in a large quantity of water it deposited a white powder, which, on being washed several times and dried, was found to be subcarbonate of magnesia.

The liquor in which this deposit was formed was limpid after being filtered, and was not rendered turbid, either cold or hot, by the soluble alkaline subcarbonates. All the acids stronger than the carbonic disengaged this latter from it. Lastly, when suitably evaporated, sulphate and carbonate of soda were obtained, part of the latter of which was in the form of subcarbonate. To explain here the presence of the carbonate of soda it requires to be known that the quantity of bicarbonate mixed with the sulphate of magnesia, was more than sufficient to decompose this latter salt.

There results from this observation that the sufficiently prolonged contact of sulphate of magnesia and bicarbonate of soda in a dry state, determines a chemical action similar to that which the concurrence of water and heat would produce, affording a new example of the inaccuracy of the old chemical axiom, Corpora non agunt nisi soluta.*

Article III.

Remarks on certain Observations made by MM. Leuret and Lassaigne, and Professors Tiedeman and Gmelin, in their Works on Digestion recently published; particularly with respect to the Presence of free Muriatic Acid in the Stomachs of Animals. By W. Prout, MD. FRS.

(To the Editors of the *Annals of Philosophy.*)

Gentlemen,

In the year 1823 the Royal Academy of Sciences proposed, as the subject of their prize essay, an inquiry into the nature of the digestive process in the different classes of animals. Of the candidates that offered themselves no one was deemed to have entirely satisfied the views of the Academy, though two of them were considered worthy of honourable mention. "The authors (says the report) have made a great number of experiments, and have obtained remarkable results. For this reason, and in consideration of the expensive nature of the researches in which they engaged, the Academy have adjudged to each the sum of 1500 francs." The authors of one of the essays were MM. Leuret and Lassaigne, who, I presume, accepted the offer of the Academy; of the other, Professors Tiedeman and Gmelin, of Heidelburgh. The latter gentlemen seem to have been offended with the decision of the Academy, refused the above offer, and published their essay themselves.† That of Leuret and Lassaigne has been also published; ‡ and the object of the present communication is to make some remarks on certain passages in these works, in which I am myself more immediately concerned; and first of MM. Leuret and Lassaigne.

In page 114 of their work, these gentlemen refer to my paper published in the Transactions of the Royal Society, on the free

* Had not the salts *got damp* in the course of the seven months?—*Ed.*

† Recherches Experimentales Physiologiques et Chemiques sur la Digestion, considérée dans les quatre Classes d'Animaux vertébrés. Par Fred. Tiedeman et Léop. Gmelin, Professeurs a l'Université de Heidelberg. Traduites de l'Allemand, par A. J. L. Jourdan. A Paris, 1826. Of this work only the first volume, containing the mammiferous animals, is yet published. I have quoted from the French Translation.

‡ Recherches Physiologiques et Chimiques pour servir a l'Histoire de la Digestion. Par MM. Leuret et Lassaigne: Ouvrage mentionné honorablement par l'Academie Royale des Sciences, dans sa Séance publique du 20 Juin, 1825. A Paris, 1825.

muriatic acid met with in the stomachs of animals;* and after briefly describing the experiments there related, which they say they have repeated with nearly similar results, they make the following remarks:—" M. Prout, après avoir supersaturé avec de la potasse pure la portion du liquide dans lequel il cherche a determiner toute la quantité d'acide muriatique libre et combiné, l'evapore à siccité et le calcine pour détruire la matière organique. En dissolvant alors le résidu dans l'eau distillée, il estime par le nitrate d'argent la proportion d'acide muriatique qu'il obtient dans cette expérience. Ici l'auteur n'a point observé que cette methode vicieuse devait necessairement l'induire en erreur, car l'exces de potasse qu'il emploie réagit sur les matières azotées pendant la calcination au rouge obscur, et il se ferme du cyanure de potassium et du sous-carbonate de potasse, qui constituent en partie avec les muriates le residu salin; or, comme le cyanure de potassium et le sous-carbonate de potasse précipitent le nitrate d'argent, le précipité que forme ce réactif dans cette circonstance n'est point du chlorure d'argent pur; et c'est cependant d'après son poids que M. Prout a calculé celui de l'acide muriatique qu'il soupçonnait exister à l'etat de liberté. Les conclusions du travail de ce chemiste sont donc inexactes." I confess these remarks surprised me not a little, as I conceived the merest tyro knew how to obviate the sources of error here pointed out. I wish these gentlemen, therefore, to know (what every *chemist* might have taken for granted when it was stated that the experiments "were made in the *usual* manner,") that *the excess of potash was always supersaturated with nitric acid before the nitrate of silver was employed.*

I now proceed to make a few remarks on certain passages in the work of Prof. Tiedeman and Gmelin. These gentlemen in their preface, after alluding to some other discoveries in which they had been anticipated by M. Chevreul, proceed to say, " Il en est de même pour ce qui regarde l'acide hydrochlorique libre (free muriatic acid) trouvé dans le suc gastrique des animaux. L'honneur de la première decouverte appartient à M. Prout. *Mais nous l'avons faite egalement, sans connaître ses recherches, au mois de fevrier* 1824, en distillant diverses liqueurs stomacales, et ce fut seulement un mois après que le memoire de M. Prout sur ce objet nous parvint." While these gentlemen thus confirm the discovery of free muriatic acid in the stomachs of animals, they also state that they have found two other acids in the same organ, viz. the *acetic* acid and the *butyric* acid; and what I have chiefly to observe upon, is, that they seem to wish their readers to believe that I denied the presence of *any* other acid except the muriatic in the stomach. Now this is by no means

* On the Nature of the Acid and Saline Matters usually existing in the Stomachs of Animals. Philos. Trans. 1824. Part I, p. 45.

the case. My object in publishing the paper in question was simply to establish that one important fact, and nothing more; though I confess I then believed, and do still, that the muriatic acid occurs *naturally* more frequently, and in greater abundance, in that organ than any other acid; for when I have met with combustible acids (as I have, since my paper was published, in a few instances), these seemed to be rather derived from the food than from the stomach itself, in most instances. So far, however, am I from denying the existence of any other acid in the stomach except the muriatic, that on the contrary I think it exceedingly probable, were the contents of the human stomach in particular examined, under all the circumstances of diet and derangement to which it is liable, that many other acids besides the acetic and butyric (of which latter, by the bye, I know nothing) would be found in it. With respect to the *lactic* acid which has so long figured as an important ingredient in animal fluids, chiefly on the authority of Berzelius, I always doubted its existence, and am not therefore at all surprised that it has proved to be a nonentity. MM. Tiedeman and Gmelin inform us,* that Berzelius himself now admits that he was mistaken, and that in fact what he considered as lactic acid is only disguised acetic acid. I long to see the grounds on which this justly celebrated chemist has changed his opinion.

MM. Tiedeman and Gmelin make some remarks on the method I employed for determining the nature and quantity of acid in the stomach which require to be noticed. This method, to a certain extent, they give very accurately, but *omit entirely the point of most importance, and which was designed as a check upon the whole;* and then proceed to say that the method is inexact and imperfect. To render this obvious, it will be necessary to repeat the method here, which was as follows: The fluid collected from the stomach was divided into four portions. "1. The first of these portions was evaporated to dryness in its natural state, and the residuum burnt in a platinum vessel; the saline matter left was then dissolved in water, and the quantity of muriatic acid present determined by nitrate of silver in the usual manner; the proportion of muriatic acid in union with a *fixed* alkali was thus determined. 2. Another portion of the original fluid was supersaturated with potash, then evaporated to dryness and burnt, and the muriatic acid contained in the saline residuum determined as before. In this manner the *total* quantity of muriatic acid present in the fluid was ascertained. 3. A third portion was exactly neutralized with a solution of potash of known strength, and the quantity required for that purpose accurately noticed. This gave the proportion of *free* acid present; and by adding this to the quantity in union with a

* Page 167.

fixed alkali as determined above, and subtracting the sum from the *total* quantity of muriatic acid present, the proportion of acid in union with *ammonia* was estimated. *But as a check to this result, the third neutralized portion above-mentioned was evaporated to dryness, and the muriate of ammonia expelled by heat, and collected. The quantity of muriatic acid this contained was then determined as before, and was always found to represent nearly the quantity of muriate of ammonia as before estimated, thus proving the general accuracy of the whole experiments beyond a doubt.* 4. The remaining fourth portion of the original fluid was reserved for miscellaneous experiments." Now, in their account of my method, MM. Tiedeman and Gmelin have totally omitted the part in italics, without which, when ammonia is present (but not otherwise), the process would be unsatisfactory, as I well knew, but which entirely removed the only objection that could be made to it. I may also here mention that there was another circumstance which operated as a check to my results, and which, by some accident, was omitted in my paper, viz. that the third neutralized portion above-mentioned *remained neutral after combustion,* which could not have been the case had the free acid present been of a combustible nature. This was a point always particularly attended to; and on reference to my notes, I find that, at the time my paper was written, no instance of the contrary had occurred to me. Since that time, however, as has been already mentioned, I have met with a few instances of the presence of combustible acids in the stomachs of animals. And here, perhaps, it may not be amiss to make a few remarks on the method in question, which seems in general not to have been duly appreciated by chemists. The mere determination of the existence of a principle in any compound, without its quantity be at the same time ascertained, is often unsatisfactory; at least the determination of the latter point corroborates the former in no small degree; for before the quantity of a substance can be ascertained, it must be obtained *per se,* or in some well-determined state of combination, circumstances necessarily implying a much more complete and satisfactory investigation than that by mere tests only. My object, therefore, was to contrive a method, by which both these points might be determined with precision at the same time. After trying a great variety, (for those related are by no means to be considered as the *only* experiments made on the subject) the one above mentioned was chosen as the best suited to my purpose; and so completely did it seem to answer the end in view, that had I detailed *all* that was done besides, which would have half filled the volume of the Transactions, I do not think that the point in question would have been a whit better established. I may, however, mention here, that among other means, *distillation,* as subsequently employed by Mr.

Children and Messrs. Tiedeman and Gmelin, was tried, and with the same results; that is to say, the *existence* of free muriatic acid was indicated, but its *quantity* could obviously not be thus determined, at least with any thing like accuracy.

I have yet one or two other points on which I shall make a few remarks, and on which the German philosophers have by some means misrepresented me in an extraordinary manner. In a paper first published by me nine or ten years ago, and subsequently, in 1819, with some revisions, in the *Annals of Philosophy*,* on the digestive process, I have said that "the contents of the stomachs of animals feeding on *vegetable substances*, even when fully digested, and about to pass the pylorus, exhibit no traces of an albuminous principle; but the moment they enter the duodenum, "they undergo remarkable changes, not only in their appearances, but in their properties. These changes appear to be chiefly induced by the action of two secreted fluids, with which they there come in contact, and are intimately mixed. These are the bile and pancreatic juice, on the nature of which we shall make a few remarks. The bile consists chiefly, according to the accurate observations of Berzelius, which agree with my own, of a large proportion of water holding in solution a peculiar bitter substance, named the biliary principle, of the mucus of the gall bladder, and of the usual salts contained in the blood, and in all the fluids secreted from it. The properties of the pancreatic juice I never could satisfactorily ascertain, but it has usually been considered as analogous to the saliva; and if this opinion be correct, it may be safely considered as containing no albumen. The changes produced in the digested alimentary matters by these fluids are evidently of a chemical nature. A gaseous product is usually evolved; a distinct precipitation of the biliary principle, in apparent union with some others, chiefly of an excrementitious nature, takes place, the mixture becomes neutral, *and an albuminous principle is formed*, at least traces of this principle appear, which, however, become much more distinctly visible at some distance from the pylorus." And this is *all* I have stated on the subject; but Messrs. Tiedeman and Gmelin represent me as asserting in general terms, what I never dreamt of, that *albumen is solely formed in the duodenum, and cannot exist in the stomach even if placed there* (for without this latter supposition the following remarks are unintelligible). "Mais il resulte evidemment (they proceed) de nos experiences faites sur des chiens, des chevaux, et des ruminans, que quand la nourriture consiste *en blanc d'œuf liquide*, ou *quand les alimens contiennent d'albumine*, cette substance se trouve dissoute par le suc gastrique et versée dans le duodenum avec le chyme, sans eprouver aucun changement. Nous ne pouvons donc pas admettre avec Prout que l'albumine *se forme dans*

* Vol. xiii. (O. S.) p. 12 and 265.

le duodenum seulment, aux depens des alimens qui ont été dissous dans l'estomac, et par l'effet de leur melange, tant avec la bile, qu'avec le suc pancreatique." I read over these passages several times with the view of discovering what connexion they had with the point in question, or how they bore on it, but have been unable to discover. I simply asserted that in animals feeding on vegetable substances, the albuminous principle is not developed till the digested matters come in contact with the bile and pancreatic juice, and nothing more—a fact, by the bye, which is confirmed by Messrs. Tiedeman and Gmelin; but with respect to the introduction of albuminous matter into the stomach, this is *quite a different case;* and I can assure these gentlemen that I never doubted for a moment if I had examined the stomach of a dog, or any other animal, after feeding it on albumen, that I should have found traces of this principle, not indeed entirely unchanged, but possessing most of its original properties.

MM. Tiedeman and Gmelin go on to observe with respect to the albumen in the duodenum, " S'il s'en trouve beaucoup plus dans le contenu de l'intestin grêle que dans celui de l'estomac, cette circonstance depend du melange de la masse chymeuse avec le suc pancreatique, dont Prout ne connaissait pas la composition, et dans lequel nous avons trouvé une grande quantité d'albumine chez le chien, comme chez la brebis et le cheval." When my paper was published in 1819, I did not know the composition of the pancreatic juice, as was then stated, and I regret that I do not know so much about it yet as I could wish. I believe, however, that it contains albumen, and consequently admit that *some* of the albumen found in the duodenum may be derived from this source, though it is still my decided belief that by far the greater proportion found there under the circumstances I have mentioned is derived from the food, and is actually developed on the spot during the series of changes that there take place, and in which the bile and pancreatic juice play an important part.

Notwithstanding these little inaccuracies, which were probably mere oversights, I cannot close the present remarks without expressing my high opinion of the value of MM. Tiedeman and Gmelin's volume. Having gone over most of the ground traversed by these gentlemen, I am well aware of the labour and difficulty of the march; and though we may differ in some minor particulars, which is not to be wondered at, I am satisfied, as far as we go together, with the general accuracy of their observations. With respect to MM. Leuret and Lassaigne's book, I am sorry that I cannot express the same sentiments; indeed as a work it does not appear to me to be at all comparable with that of the German philosophers.

I am, Gentlemen, your most obedient servant,

W. Prout.

Article IV.

Memoir on a peculiar Substance contained in Sea Water. By M. Balard, Apothecary and Chemist to the Faculty of Sciences, at Montpelier.

(*Concluded from* p. 387.)

Certain metals act upon liquid hydrobromic acid; iron, zinc, and tin, are dissolved by it with the evolution of hydrogen; metallic oxides when put into this acid act differently upon it, the greater part of them, the alkalies, the earths, the oxides of iron, and the peroxide of copper and mercury, form fluid combinations which may be regarded as hydrobromates; there are some oxides with which the hydrobromic acid undergoes double decomposition, and produces water and metallic hydrobromurets; such are the protoxide of lead and the oxide of silver.

Those oxides which contain much oxygen, and have no affinity for hydrobromic acid, or which cannot, by decomposing it, form corresponding bromurets at this high degree of oxidation, lose a certain quantity of their oxygen, which determines the decomposition of a part of the hydrobromic acid, and consequently there is a disengagement of brome. The less oxygenated oxide afterwards forms with the acid which escapes decomposition an hydrobromate, or a metallic bromuret. It is an action of this kind which is exerted by the peroxides of lead, antimony, and manganese. The last compound may be employed with hydrobromic acid to prepare brome; this method, which resembles that for the preparation of chlorine gas, is easier than that which I have previously described.

It will be observed that brome has less affinity for hydrogen than chlorine has, but it is greater than that of iodine; hydrogen combines readily with chlorine, but it is more difficult to unite it directly with iodine and brome. Chlorine decomposes water at a high temperature; brome and iodine do not decompose it under similar circumstances. Hydrobromic acid is decomposed by chlorine, but brome in its turn also decomposes hydriodic acid. The action of metals upon these hydracids leads to the same consequences. The hydriodic acid is decomposed by the action of mercury; pure hydrobromic acid may, on the contrary, be long kept on this metal without undergoing any sensible alteration; but at a moderate temperature, it begins to be decomposed by tin, which would have exerted no action upon muriatic acid.

It results from this unequal affinity, that the properties of hydrobromic acid are in some degree intermediate between those of muriatic and hydriodic acids. If it resemble the first by the difficulty with which it is decomposed, by the united influence of oxygen and heat, it resembles, on the other hand, the latter by the property which it possesses of being altered, to a certain extent, by sulphuric acid, and by its property of dissolving an excess of brome.

The action of brome upon the metals greatly resembles that which chlorine exerts upon the same bodies. Antimony and tin burn by contact with brome. Potassium evolves so much heat and light by uniting with it, and so violent a detonation ensues as to break vessels of glass in which the union is effected, and to project the result of the combination to a distance.

The bromurets, which are formed directly with these bodies, and especially the bromuret of potassium, resemble in their appearance and properties those which are obtained when the metallic oxides are treated with hydrobromic acid, either in the dry or moist way, after having evaporated the solutions, or caused them to crystallize. Their aqueous solutions have all the properties of their respective hydrobromates. These facts render it very probable that metallic bromurets, like chlorides and iodides, are converted into hydrobromates by solution in water, and reciprocally the hydrobomates are changed into bromurets in passing to the solid state: the study of these two orders of compounds cannot be separated without inconvenience.

As I have prepared only a few hydrobromates and bromurets, I cannot give their general history. It is sufficient to state that the hydrobromates will be easily recognized by the property which they possess of imparting a yellow colour, and emitting brome, when made to act upon bodies which attract hydrogen strongly. Such are the chloric and nitric acids, and especially chlorine, which explains the use of this last substance in the separation of brome; as to the bromurets, they are all decomposed by chlorine with the disengagement of brome.

Bromuret of Potassium.

I employed several processes for the preparation of bromuret of potassium. 1. I obtained it by immersing the metal in the vapour of brome; 2. By decomposing hydrobromic acid by it; 3. By directly combining this acid with potash, evaporating the solution, and drying the residuum; 4. The cubic crystals obtained by saturating the ethereal solution of brome with potash may be considered either as hydrobromate of potash, or bromuret of potassium. They always contain small portions of muriate of potash or soda; in whatever mode the bromuret of potassium is obtained, its properties are always similar, and if it be dissolved in water and recrystallized, it usually assumes a cubic form, and sometimes that of a long rectangular parallelopiped; its taste is sharp; when heated, it decrepitates and undergoes igneous fusion, without suffering any change; chlorine decomposes it at a high temperature, brome is evolved, and chloride of potassium is formed. Iodine does not act upon it, even at a high temperature; on the contrary, when brome is made to pass over fused iodide of potassium, abundant violet vapours are disengaged; boracic acid does not decompose it at a red heat, unless the vapour of water be passed over the mixture, and then hydrobromic acid is given out.

When bromuret of potassium is put into water, it is converted into hydrobromate of potash; it is more soluble in hot water than cold, and produces a sensible diminution of temperature during solution; it is soluble also in alcohol, though sparingly so; the solution of hydrobromate of potash does not dissolve more brome than pure water; the solution is decomposed by sulphuric acid, which disengages the vapour of hydrobromic acid, and of brome; 1·27 gramme of brome, treated in this manner, left a residuum of 0·973 of sulphate of potash; this quantity of salt contains 0·52668 of potash, consisting of 0·08927 of oxygen and 0·43741 of potassium: according to this experiment, bromuret of potassium is composed of

Brome	65·56
Potassium	34·44
	100·00

Supposing this compound to be formed of an atom of brome and an atom of potassium, the atomic weight of brome will be 93·26, the atom of oxygen being 10.

The metallic bromurets are converted into neutral hydrobromates by solution in water, which, being decomposed, the two volumes of hydrogen combine with the brome, and the volume of oxygen unites with the metal. As hydrobromic acid is composed of equal volumes of hydrogen and the vapour of brome, it follows that the two volumes of hydrogen should produce four volumes of hydrobromic acid; from which it is to be concluded that the metallic hydrobromates contain in volume four times as much hydrobromic acid as of oxygen in their oxides; then as 0·08927 gramme of oxygen occupies a volume of 0·0624 lit. 1·270 gramme of bromuret of potassium ought to yield 0·2496 lit. of hydrobromic acid. The specific gravity of the vapour of brome according to these data ought to be 5·1354, and that of bromic acid 2·6021. I have not yet verified these theoretical results by experiment.

Hydrobromate of Ammonia.

Hydrobromic acid gas combines with an equal volume of ammoniacal gas; the result is a saline compound which may also be obtained by combining hydrobromic acid with liquid ammonia. I have already prepared hydrobromate of ammonia by decomposing ammoniacal gas, or liquid ammonia with brome; the results of this action are extrication of heat without light, the evolution of azote, and the formation of hydrobromate of ammonia; I did not observe in any of these cases that a compound was formed analogous to chloride of azote.

Hydrobromate of ammonia is solid and colourless; when moistened and exposed to the air, it becomes yellowish, and acquires the property of turning turmeric paper red. It crystallizes in the form of long prisms, upon which smaller ones are placed at a right angle; by heat it is volatilized.

Hydrobromate of Barytes.

I obtained this salt by shaking the ethereal solution of brome with hydrate of barytes, or by directly combining barytes with hydrobromic acid. Hydrobromate of barytes fuses when exposed to heat; it is very soluble in water, and also dissolves in alcohol; the crystals are opaque and mammillated, and bear no resemblance to those of muriate of barytes.

Hydrobromate of Magnesia.

This salt is uncrystallizeable, deliquescent, and, like the muriate of magnesia, is decomposed at a high temperature.

Bromuret of Lead.

When an aqueous solution of an hydrobromate is dropped into a solution of lead, a white crystalline precipitate is formed resembling chloride of lead in appearance; this precipitate, when strongly heated, fuses into a red fluid which exhales very weak white vapours, which concrete by cooling into a fine yellow matter.

Bromuret of lead in a moist state is decomposable by the nitric and sulphuric acid, brome being disengaged in the first case, and brome and hydrobromic acid in the second; the great cohesion which it acquires by fusion prevents nitric acid from acting upon it; and it can be decomposed only by boiling sulphuric acid.

Deutobromuret of Tin.

I have already remarked that tin is dissolved by hydrobromic acid, with the evolution of hydrogen. The hydrobromate which results when dried is converted into a proto-bromuret, which I have only slightly examined, but which I have ascertained to be very different from the compound obtained when brome is made to act directly upon tin, and it is evidently a deutobromuret.

Tin burns when put in contact with brome, and it is converted into a solid white substance, which has a crystalline appearance, and is very fusible, and readily volatilized; it exhales only traces of white vapour when exposed to the air; it dissolves in water without any sensible extrication of heat, and is converted into an acid deutobromate. When put into hot liquid sulphuric acid, this compound liquefies, and remains at the bottom of the acid, having the appearance of oily drops, and without suffering any sensible alteration. Nitric acid, on the contrary, soon occasions a rapid disengagement of brome; the deutobromuret of tin, analogous to the fuming liquor of Libavius, it will be observed, possesses but few of the properties of this latter compound.

Bromurets of Mercury.

Mercury combines with brome in several proportions. A solution of an alkaline hydrobromate, acting upon protonitrate of mercury, occasions the formation of a white precipitate, resembling protochloride of mercury, and which appears to be a protobromuret of the metal.

Brome attacks mercury strongly; the combination is effected with the extrication of heat unaccompanied by light. The result is a white substance, which sublimes when heated, and which is soluble in water, alcohol, and especially in ether, it is precipitable red and yellow by the alkalies, and offers many analogies with corrosive sublimate. It is distinguished by the red vapours of brome which it yields, when treated with nitric acid, and still better by the sulphuric acid; the advantage of using the latter acid appears to me to depend upon the possibility of employing a higher temperature.

Bromuret of Silver.

Nitrate of silver produces a curdy precipitate of bromuret of silver in the solutions of the hydrobromates. This compound is of a canary yellow colour when dried in the dark; but if exposed while moist to the light, it blackens, but not so quickly as the chloride of silver; like this substance also it is insoluble in water, and soluble in ammonia. Nitric acid produces no effect upon it even when boiling, but sulphuric acid disengages vapour of brome when boiling. Nasent hydrogen decomposes it, and there are produced metallic silver and hydrobromic acid, and I employed this method to analyze the bromuret of silver; I introduced a certain quantity of it into a mixture of pure granulated zinc and dilute sulphuric acid; the silver was reduced, and I weighed the silver after ascertaining that the zinc had been completely dissolved; the mean of two experiments, which differed but very little from each other, gave us the composition

Silver	589
Brome	411
	1000

This gives 94·29 as the atomic weight of brome, which does not differ much from that deduced from the analysis of the bromuret of potassium.

Bromuret of Gold.

Brome and its aqueous solution are capable of dissolving small portions of gold; and a yellow bromuret is obtained, staining animal substances of a yellow colour, and decomposable by heat into brome and metallic gold.

Bromuret of Platina.

Platina is not acted upon by brome at common temperatures; but it is dissolved by bromo-nitric acid, and forms a compound of a yellow colour, which is decomposed by heat, and which, like the chloride of platina, produces sparingly soluble yellow precipitates in the solutions of potash and ammonia.

On the Action of Brome upon Metallic Oxides.

Brome acts upon metallic oxides under two different circumstances; when they are dry and strongly heated, and at usual temperatures with the presence of water.

If the vapour of brome be passed over potash, soda, barytes, or lime, at a red heat, vivid ignition takes place; oxygen gas is liberated, and the bromurets of potassium, sodium, &c. are found in the tube. I was unable to decompose magnesia and zirconia in the same manner; the brome circulated round these ignited earths without either liberating oxygen, or combining with them. Sublimed oxide of zinc underwent no alteration by the action of brome at a high temperature.

The metallic oxides which brome is capable of decomposing do not appear susceptible of this alteration when they are combined with a powerful acid; so that I attempted in vain to liberate oxygen by passing brome over red-hot sulphate of potash. When an acid has but little affinity for the metallic oxide, the case is different; the alkaline carbonates being completely decomposable by brome, which evolves gas consisting of two volumes of carbonic acid and one volume of oxygen.

The phenomena are very different when brome is made to act upon the alkalies or earths, already mentioned: when they are dissolved in or mixed with a considerable proportion of water, no liberation of oxygen gas is observed, the smell and colour of the brome disappear, but a compound is formed from which brome is evolved by weak acids, such as the acetic acid, and which possesses the property of quickly decolorizing tincture of tournsole. According to these experiments, brome appears to be capable of forming bromurets of oxides, analogous to the chlorides of lime, soda, &c.

When brome is mixed with a very concentrated solution of potash, the solution after evaporation yields not only cubic crystals of hydrobromate of potash, but acicular crystals, which appear to be bromate of potash. Barytes and lime act in a similar manner, but magnesia does not appear to possess the same property. Analogy leads to the conclusion that these two sorts of salt are connected with the decomposition of water.

The decomposition of water which is so readily effected with the assistance of the alkalies also occurs, but in a less complete manner when brome acts upon it, with the assistance of the sun's rays. An aqueous solution of brome which I had for a long time exposed to the rays of the sun, gave sensible indications of the presence of bromic and hydrobromic acids, the formation of which can scarcely be explained but by supposing that water has been decomposed.

It appears to me from the facts stated in the last and preceding paragraphs, that brome acts less strongly upon the metals than chlorine does, but with more energy than iodine; the evolution of light and heat which accompanies combination of the former with these bodies, shows that brome has more resemblance to the action of iodine under similar circumstances. Although tin combines with brome with the disengagement of light, which it does not with chlorine, it is, perhaps, dependant upon the circumstance of the brome being fluid, which allows of the combination of greater masses.

The iodides are decomposed by brome, and the bromurets by chlorine. Iodine, which decomposes potash and soda very readily at a high temperature, does not so act upon barytes, but combines with it to form an iodide of an oxide; brome, on the contrary, effects the decomposition of this base, and also of lime, but it does not act so efficaciously upon magnesia, while chlorine exerts its decomposing action upon this oxide.

Of the Bromic Acid and its Combinations.

When brome is shaken with a sufficiently strong solution of potash, there are formed, as I have already mentioned, two very different compounds; the hydrobromate of potash is obtained in solution, and a white crystalline precipitate settles at the bottom of the vessel, which appears to be bromate of potash, for it fuses upon burning coals like nitre, and is converted into bromuret of potassium by heat, giving out oxygen gas.

Bromate of potash is very slightly soluble in alcohol; it dissolves in considerable quantity in boiling water, from which it separates by cooling in the form of needles grouped together. When it is made to crystallize by evaporation, it is deposited in laminæ of a dull aspect; it is decomposed by heat, deflagrates upon red-hot coals, and when mixed with sulphur, it detonates by percussion.

The solution of bromate of potash forms a precipitate in one of nitrate of silver; this precipitate is white and pulverulent, scarcely becoming black by exposure to light, and is thus distinguished from bromuret of silver, which is yellow, curdy, and readily altered by the solar rays. Bromate of potash does not precipitate the salts of lead, whereas the hydrobromate of potash occasions a very abundant crystalline precipitate in them; with protonitrate of mercury the bromate of potash forms a yellowish white precipitate which is soluble in nitric acid. Bromate of potash possesses a property of which the chlorates are destitute, but which exists to a great extent in the iodates; its acid is decomposed by the influence of hydrogenating causes, as if it were uncombined; thus sulphurous acid, sulphuretted hydrogen, hydrobromic acid and muriatic acid, react upon the bromate of potash, and produce in the three first cases a disengagement of brome, and in the last instance a compound of brome and chlorine.

I tried, but in vain, to obtain an oxide of brome by the decomposition of bromate of potash; it is true indeed that the failure may be owing to the small quantity of the substances upon which I am able to make my researches.

Hydrobromic acid, when diluted with water, evolves brome by agitation with bromate of potash. Diluted sulphuric acid produces at 212° Fahr. a disengagement of gas, which I attempted to collect over water, mercury, and oil. I always procured brome and oxygen gas, which seems to show, either that brome cannot form oxides, or that the compounds, if they could be procured, are less permanent than the oxides of chlorine.

Bromate of potash may be obtained by a different process from that which I have described. It is sufficient to combine brome and chlorine, and to mix potash with the aqueous solution of the compound, and instantly there is produced the decomposition of water, a bromate and muriate of potash; these salts, on account of their different solubility, are easily separated. I employed this process for the preparation of bromate of barytes, which I obtained in the form of acicular crystals, soluble in boiling water, slightly soluble in cold water, and burning with a green flame upon red-hot charcoal.

When dilute sulphuric acid is poured into an aqueous solution of bromate of barytes so as to precipitate all the base, the remaining solution is one of bromic acid, from which the greater part of the water may be separated by slow evaporation; it then acquires the consistence of a syrup: if the heat be raised so as completely to expel the water, a portion of the acid is vaporized, and another portion is decomposed into oxygen and brome; similar effects appear to be produced by evaporating the fluid in vacuo with sulphuric acid; water, therefore, appears to be requisite to the constitution of bromic acid.

Bromic acid reddens tournsol paper strongly, and soon after decolorizes it; it has scarcely any smell, its taste is very acid, but not at all caustic. Nitric and sulphuric acids have no chemical action upon it; the latter indeed, when it is much concentrated, liberates brome, and produces an effervescence, which is probably occasioned by the disengagement of oxygen. These effects, however, seem to be attributable to the high temperature which sulphuric acid produces by combining with the water of the bromic acid, for they are not produced by dilute sulphuric acid.

The hydracids, and those acids which are not saturated with oxygen, act with great energy upon bromic acid; the sulphurous and hydrobromic acids and sulphuretted hydrogen decompose it, and so also do the muriatic and hydriodic acids. In the latter case, compounds of brome with chlorine and iodine are obtained, and these acids, when combined with bases, act similarly upon bromic acid.

Bromic acid occasions a white pulverulent precipitate in the salts of silver, which appears to be a metallic bromate. It also precipitates the concentrated solutions of the salts of lead, but the compound obtained is dissolved by the addition of a small quantity of water, and it is distinguished by this solubility from that which the hydrobromates form in the solutions of the same metallic salts; it also gives a white precipitate, as the bromate of potash does, with protonitrate of mercury.

The properties of bromic acid strongly resemble the analogous compounds of chlorine and iodine; but the impossibility of depriving it of water perfectly, and of boiling it without partial decomposition, most resembles chloric acid, and shows that the oxygen is less strongly retained than in the iodic acid.

The proportions of the principles which constitute bromic acid show that it is subject to the same laws of composition as the chloric, iodic, and nitric acids; 1·128 of bromate of potash is reduced by calcination to 0·790 of bromuret of potassium. The loss of weight derived from the disengagement of oxygen was consequently 0·338; 0·790 of bromuret of potassium contain, according to the analysis already stated, 0·27255 of potassium and 0·51745 of brome; this quantity of potassium requires 0·05563 of oxygen to convert it into potash, which, subtracted from 0·338, leave 0·28237 as the oxygen combined with 0·51745 of brome: bromic acid, according to this experiment, will consist of

Brome	64·69
Oxygen	35·31
	100·00

In representing the atom of brome by 93·28, derived from the analysis of bromuret of potassium, and supposing the bromic acid to consist of five atoms of oxygen and one atom of brome, 100 parts of bromic acid should consist of

Brome	65·10
Oxygen..........................	34·90
	100·00

These numbers differ so little from those deduced from direct analysis, that the supposition from which they are derived may, it appears to me, be regarded as true.

On the Combination of Brome with Chlorine and Iodine.

Brome combines with chlorine at common temperatures; this combination may be effected by passing a current of chlorine through brome, and condensing the disengaged vapours by means of a freezing mixture. Chloride of brome is a reddish yellow coloured fluid, much less intense than the brome itself; it has a penetrating smell, and it causes the eyes to water; its taste is extremely disagreeable; it is very fluid and volatile. Its vapour is of a deep yellow colour, similar to the oxides of chlorine, but not at all resembling the orange vapour of the brome itself; it causes the metals to burn, and probably forms with them metallic chlorides and bromurets; chloride of brome is soluble in water; the solution resembles it in smell and colour, and, like it, rapidly decolorizes tournsol paper, without reddening it: consequently it dissolves in water without undergoing any alteration of properties; but by the influence of the alkalies, it decomposes water. Potash, soda, and barytes, poured into a solution of chloride of brome, produce muriates and bromates of these bases, a property which exists in chloride of iodine, and which proves that chlorine possesses greater affinity for hydrogen than brome does.

Bromuret of Iodine.

Iodine appears to be capable of forming two compounds with brome; if these bodies be made to act upon each other in certain proportions, a solid compound is obtained, which, when heated, yields a reddish brown vapour, condensible into small crystals of the same colour resembling fern leaves in form. An additional quantity of brome converts these crystals into a fluid, resembling in appearance hydriodic acid containing much iodine; this compound is soluble in water, to which it imparts the power of decolorizing tournsole paper without reddening it; when the alkalies are poured into it, they form hydrobromates and iodates, as may be supposed from analogy.

Bromuret of Phosphorus.

Phosphorus and brome put in contact in a flask containing carbonic acid act suddenly upon each other, with the evolution of heat and light; the result of the combination separates into two portions; one of them is solid, sublimes and crystallizes in the upper part of the flask; the other is fluid, and remains in the lower part; this appears to contain less brome than the crystalline compound; in fact it may be made to crystallize by adding a sufficient quantity of brome to it. The fluid compound I shall call the protobromuret of phosphorus, and the solid deutobromuret.

The protobromuret of phosphorus is fluid even at 12° centig. It reddens tournsol paper faintly; it probably even owes this property to the imperfect dryness of the materials from which I prepared it. It vaporizes readily, and by exposure to the air emits penetrating vapours; like the protochloride it is susceptible of dissolving an excess of phosphorus, and then acquires the property of inflaming combustible bodies, when put into contact with it: it acts very energetically upon water with the extrication of much heat, and produces hydrobromic acid, which may be collected in the state of gas, when a few drops only of water are added to it; but it is dissolved if a large quantity of water be used: this acid solution, when evaporated, leaves a residuum which burns slightly when it is dried, and is converted into phosphoric acid.

The deutobromuret of phosphorus is solid, of a yellow colour; when moderately heated, it is first rendered fluid, the colour of which is red, and by increasing the heat, it is converted into vapour of the same colour.

When the deutobromuret of phosphorus is cooled after fusion, or when its vapour is condensed, it yields rhombic crystals in the first case, and in the second, the crystals are needle-formed, and placed upon each other; the metals decompose it, and there are probably produced metallic bromurets and phosphurets; when exposed to the air, it emits dense penetrating vapours; it decomposes water with the extrication of heat, and produces hydrobromic and phosphoric acids.

When chlorine is made to act upon either of the bromurets of phosphorus, red vapours of brome are disengaged, and chloride of phosphorus is obtained. Iodine does not decompose these compounds; on the contrary, when brome is made to act upon iodide of phosphorus, violet vapours and a bromuret are produced.

Bromuret of Sulphur.

This compound may be obtained by pouring brome upon sublimed sulphur; it is converted into a fluid of an oily appearance and a reddish tint, which is much deeper than that of chloride of sulphur, and which, like that compound, is capable of emitting white vapours when exposed to the air, and of a somewhat similar smell.

Bromuret of sulphur reddens tournsol paper faintly, but with water it reddens it strongly. Cold water acts slowly upon bromuret of sulphur, but at a boiling heat it produces a slight detonation, and there are formed hydrobromic and sulphuric acids and sulphuretted hydrogen; whereas chloride of sulphur, under similar circumstances, would have yielded muriatic, sulphurous and sulphuric acids, without detonation.

Bromuret of sulphur is decomposed by chlorine with the evolution of brome and the production of chloride of sulphur.

Hydrocarburet of Brome.

I have observed no appearance either of decomposition or combination by exposing carbon to various temperatures in contact with brome; but I easily united it with bicarburetted hydrogen. If a drop of brome be poured into a flask of this gas, it is instantly converted into a substance of an oily appearance, and heavier than water, and colourless; and instead of the penetrating smell of brome, it has an ethereal smell, which is sweeter than that of the hydrocarburet of chlorine.

The hydrocarburet of brome is readily volatized; it is decomposed by passing through a red-hot tube. I obtained in this experiment a deposit of carbon, and hydrobromic acid gas. It burns when presented to an inflamed body, and produces very acid vapours, and a thick smoke formed by very finely-divided carbon. I tried in vain to obtain bromuret of carbon, by exposing a mixture of this hydrocarburet of brome to the sun's rays.

A compound, similar to that now described, may be obtained by distilling the mother-water of the salt works rendered yellow by chlorine. The brome obtained in this mode is often mixed with hydrocarburet of brome, from which it is separated by water. It sometimes even happens, that in performing this operation, all the brome is converted into this triple compound. This effect is probably produced by the action of the brome upon a small quantity of organic matter which the salt-water contains, and which imparts to the residuum of evaporation the property of blackening when strongly heated.

Action of Brome upon some Organic Bodies.

The great affinity which brome possesses for hydrogen indicates beforehand its mode of action upon organic bodies. It decomposes the greater number of them, always forming hydrobromic acid, and sometimes separating carbon.

Brome readily dissolves in acetic acid, upon which it acts slowly; it is very soluble in ether and alcohol. The coloured solutions which are formed lose their tint after some days, and hydrobromic acid is found in the liquor. The fat oils produce effects of this nature very slowly, but they occur instantaneously when brome is put into essential oils; when a few drops of this substance are mixed with oil of turpentine or aniseed, heat is extricated, attended with the production of the vapour of hydrobromic acid, and the essential oil is changed into a resinous substance of a yellow colour resembling turpentine. Resin acts in the same way with brome; camphor dissolves perfectly well in this fluid, losing almost entirely its smell and volatility by the combination. The compound of brome and camphor solidifies and crystallizes by exposure to cold.

The most permanent colouring substances are entirely changed by the action of brome, which takes away their tint, and, like chlorine, converts into a peculiar yellow substance.

I did not perceive any remarkable action between brome and sugar, starch, morphia, margaric acid, &c.

The small quantity of brome which I could spare prevented me from examining how it would act with other organic bodies.

Natural History of Brome.

Brome occurs in very small quantity in sea water; even the mother-water of the salt works contains but very little, although it is much diminished in volume by the evaporation which occasions the separation of common salt, and which does not contain a sensible quantity of it. The nature of the methods by which brome is separated seems to indicate that it exists in the state of hydrobromic acid, and some circumstances induce me to suppose that this acid is combined with magnesia; for if the residuum obtained by evaporating the water of salt works be strongly calcined, it loses the property of disengaging brome by mixture with chlorine; recollecting that the hydrobromates which I have examined are not decomposable by heat excepting that of magnesia; it leads to the supposition that the water of the salt springs contains this compound.

Marine plants and animals also contain brome. The ashes of the plants which grow in the Mediterranean all give a yellow tint when the soluble part is treated with chlorine. I have also seen the same colour produced by causing chlorine to act upon the solution of the ashes of the *Ianthina violacea,* a testaceous mollusca, for which I am indebted to M. Auguste Berard, and which this distinguished officer brought from St. Helena in his second voyage round the world. I have obtained a considerable quantity of brome from the mother-water of barilla employed for the preparation of iodine.

To conclude, it appeared that the residuum obtained by evaporating a mineral water from the eastern Pyrenees, which was strongly saline, became yellow by mixture with chlorine. If brome really exists in a water of this kind, we may expect to meet with it in salt springs, properly so called, and especially in the mother-water of rock salt; I had not the means requisite for ascertaining this point. What has been stated renders it extremely probable that brome will be found in a great number of marine productions, or of submarine origin.

If I have not been deceived as to facts which I have related, they fully authorise, as it seems to me, the opinion which I have stated as to the nature of brome, and which has served me in explaining its combinations.

Any substance which, in its separate state, so effectually resists, as brome does, all attempts to decompose it; which is expelled by chlorine from all compounds of which it forms a part, and always appearing with its original qualities; which, acting upon the combinations of iodine, always replaces that substance, to produce corresponding effects in the new compounds; which, notwithstanding this opposition of chemical action, is associated with chlorine and iodine by well supported analogies, seems, on these accounts, to possess the same claim to be considered as a simple body.

If this opinion should acquire confirmation by the subsequent examinations to which chemists may subject brome, the rank which it ought to occupy among simple bodies is between chlorine and iodine; it will not be uninteresting to see two substances so nearly allied as chlorine and iodine, admitting a new body between them, and serving, by more intimate relations, to connect a group of agents, the first two of which are already so remarkable. Such an approximation of properties and chemical relations between these three simple bodies will acquire additional importance on account of their common origin.

Whilst at the commencement of my researches, in examining the several combinations of brome, I almost always found in them the strongest points of resemblance with the analogous compounds of chlorine; I confess I felt some scruples in admitting brome to be a peculiar substance; but these scruples have not been able to withstand the power with which chlorine separates it from its compounds, while brome separates iodine from its combinations.

I shall not conceal how much the materials which I have been able to collect in order to trace the history of brome have left still to be desired. I should indeed very willingly have deferred their publication until more numerous researches had allowed of my leaving fewer blanks, if I had not thought it would be more useful in this important object of research to direct the attention of those chemists to it, who possess more power clearly to elucidate the subjects of which they treat.

I do not for my own part intend to discontinue my attention to this substance, as soon as the waters of our salt springs are sufficiently concentrated to admit of my conveniently separating the brome from them, especially if this sketch should be so fortunate as to interest the Academy; and if fresh efforts should yield me results of sufficient importance, I shall hasten to submit them to it, and shall do it with the most perfect confidence.

M. Gay-Lussac's Report upon the Memoir of M. Balard respecting a new Substance; extracted from the Procès-verbal of Monday, Aug. 14, 1826.

MM. Vauquelin, Thenard, and myself, have been commissioned by the Academy to report our opinion respecting a memoir of M. Balard's, the object of which is to describe the properties of a new substance which he has found in sea water; and we have performed this commission.

M. Balard has given this substance the name of *muride;* but this denomination being liable to several objections, we have, with the consent of the author, called it *Brome,* from βρωμος, a *bad smell.*

Brome is fluid at the average temperature of the atmosphere, and even at 18° below 0° centig. In quantity its colour is of a deep reddish-brown; in small quantity it is of a hyacinthine red; the colour of its vapour exactly similar to that of nitrous acid; it is very volatile, and is converted into vapour at 47° centig. Its smell is very strong, and much resembles that of chlorine; its density is about 3.

Brome destroys colours in the same manner as chlorine; it is soluble in water, alcohol, and ether. M. Balard has combined it with a great number of simple substances, and obtained very remarkable compounds. Chlorine is more powerful than brome, but in its turn it is stronger than iodine. This property renders it very probable that brome is only a compound of chlorine and iodine, as may be suspected from the affinity which it has for these two bodies. To form an exact idea of the properties of brome, it must be composed with chlorine; with hydrogen it forms an hydracid, hydrobromic acid, and with oxygen, bromic acid, the compounds of which, with basis, have the strongest analogy with the chlorates. When heated, it decomposes like chlorine all the soluble alkaline oxides, and evolves oxygen; when cold, it combines with these oxides, and forms bromurets, which are readily decomposable by heat and the weak acids. It combines also with bicarburetted hydrogen gas, and produces an oleaginous fluid of a very sweet ethereal smell.

The weight of its atom is 9·328, that of oxygen being unity. M. Balard, when sending his memoir to the Academy, accompanied it with some small portions of brome, and of some of its combinations, with which we have made some experiments. We have also obtained brome by the process described by M. Balard, by treating the mother-waters of the salt marshes of the plain of Aven, which was sent to us by our colleague M. d'Arcet.

If the few experiments which we have been able to perform has not afforded us that certainty of the existence of brome as a very simple body, which, in the present day, is properly required, we consider it at least very probable that it is so. The memoir of M. Balard is extremely well drawn up, and the numerous results which he relates would not fail to excite great interest, even if it should be proved that brome is not a simple body.

The discovery of brome is a very important acquisition to chemistry, and gives M. Balard honourable rank in the career of the sciences. We are of opinion that this young chemist is every way worthy of the encouragement of the Academy; and we have the honour to propose that his memoir should be printed in the *Recueil des Savans Etrangers*.

Signed Vauquelin, Thenard, Gay-Lussac, Reporter.

The Academy adopts the conclusions of this Report.

Article V.

On Combustion. By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Great Ouseburn, Nov.* 15, 1826.

The fact has long been established, that when condensation takes place during chemical combination, heat is evolved;* sometimes the quantity is so small as to be barely sensible; at others, intense heat is excited, accompanied with emission of light. The old chemists, supposing atmospheric air to be an elementary substance, and finding its presence essential to what is ordinarily termed combustion, gave the name of inflammable bodies to those substances which emit both light and heat when exposed to the air at an elevated temperature; and although cases where both light and heat are abundantly united during combination, where air is excluded, and is indeed unessential, are very numerous, yet they limited the signification of the term combustion to those cases which can take place only by the agency of air,† which, in the language of modern chemistry, was supposed to be the only "supporter of combustion." When the science of chemistry was reformed by the French philosophers, since the atmosphere was then known to be a mixture‡ of two gaseous bodies, of which, that which consti-

* If any volume of water be mixed with an equal one of the sulphuric acid of commerce, a great degree of heat is excited; when the mixture becomes cool, it will not occupy the measure of two volumes. Nitric acid excites less heat, and the union is attended with less considerable condensation. When water and alcohol are mixed, both effects are witnessed in a smaller degree. The law extends to all classes of bodies.

† This limitation is strictly proper in the ordinary affairs of life; for inasmuch as both life and fire are supported solely by the atmosphere, it is the sensible and obvious supporter of combustion.

‡ A mixture, because by mixing the proper proportions of oxygen and azote, a compound resembling atmospheric air is produced (except that the small proportion of carbonic acid gas, aqueous vapour, and minute quantities of some other gaseous bodies, are wanting), yet neither expansion, compression, heat, cold, or any other symptom of

tutes about four-fifths is almost inert, serving only to moderate the energy of the other, which consists of one-fifth part; they ascribed to this one-fifth part, which they denominated oxygen, all the properties formerly attributed to the whole atmosphere, which it possesses in a high degree. They followed up the old plan, limiting the cases of combustion to those which cannot take place without the presence of oxygen, which now began to be considered as the only "supporter." On examining the products of combustion, Lavoisier was the first to determine with rigid accuracy, that oxygen becomes united to the inflammable body, that the sum of the weights of the oxygen which disappears, and of the combustible consumed, is precisely equal to the weight of the product. Hence he inferred that at a certain temperature, any one of the class of inflammables attracts the particles of oxygen so powerfully as to separate it from the caloric with which in its gaseous state it is combined: hence this caloric is liberated. Hence he supposed all the light and heat that are evolved to be separated from the oxygen gas.* The old doctrine which recognized in oxygen the only supporter of combustion, continued to prevail, until chlorine, iodine, and perhaps fluorine, were added to the list. Now all these are termed the "supporters of combustion." Are the interests of science promoted by making this distinction between supporters of combustion and inflammables? In the common affairs of life the distinction is useful; for the atmosphere is the only gaseous matter which comes under universal notice, and popularly it is with reference to it that substances are inflammable or uninflammable. But extend the distinction to the four "supporters," and we find that carbon is inflammable, oxygen being the supporter; but it is uninflammable in chlorine and (if I recollect facts distinctly) in iodine. Boron is in the same state: other examples might be quoted; but since they will be well known to your chemical readers, I shall not quote any others, particularly since these show that a body is inflammable when exposed to one supporter, but inert when another is applied. Hence each supporter must have its own class of inflammables.

But I extend the application of the query. Were our atmosphere composed of hydrogen gas, then oxygen gas and chlorine

chemical action, presents itself: therefore it is merely a mixture. Besides, the properties of oxygen gas are in nowise changed; they are only moderated; and the azote produces no positive effect, except that of moderating the otherwise too powerful energy of the oxygen.

* When it is considered that all bodies absorb much caloric during their conversion into the gaseous state, which they evolve again during condensation, it is difficult to see how this most eminent philosopher could have made any other supposition; for the science was just beginning to be framed; he had no means of ascertaining the quantity of caloric remaining in the product of combustion. Hence, reasoning in a truly philosophical manner, he concluded that, in the case of phosphorus and iron especially, the heat evolved is that which is required for the conversion of oxygen into a gaseous state; and surely he made a right assumption, when the contrary could not be proved.

gas would be inflammables.* Were it vapour of sulphur, then iron, copper,† and many other metals, would be inflammables, and sulphur the supporter, while zinc‡ and charcoal would be uninflammable. Were aqueous vapour our atmosphere, potassium would be, perhaps, almost the only combustible.§ Hence it appears that the terms (I speak of their philosophical application only) supporter of combustion, and inflammable substance, are merely relative; that which is a supporter in one case is inflammable in another; and *vice versâ.* But again, in cases of combustion, why is one body a supporter of combustion rather than the other? When light and heat accompany chemical combination, the phenomenon is properly and truly styled combustion: each of the substances contains a definite portion of caloric; during combination condensation takes place; therefore, the sum of the content of the interstices between the particles of the bodies combined is diminished, and consequently heat is excited. If this be accompanied with light, why is the term combustion to be confined to those cases which require the presence of oxygen, chlorine, or iodine? Is not the action which takes place between filings of copper, or iron and sulphur, as truly combustion? The action is the same; the phenomena are the same. However, if it be desired to place under one general head those substances which resemble oxygen in their general properties, this is to make a truly scientific arrangement; but why limit the term combustion to those cases which require their presence, whilst multitudes of examples, answering every requirement, can be produced, where they need not be present?

There is one class of phenomena which extends through, and exhibits itself in, every part of an extensive scale: whenever bodies enter into chemical union, a change of temperature accompanies the change which takes place in the properties of the substances. Metals become oxides; sulphur, phosphorus, carbon, and some other bodies, become acids; potassium, sodium, &c. alkalies, if oxygen be concerned as the supporter of combustion. In other cases, as the union of carbonic acid gas to hydrate of lime, a neutral salt results; yet in all cases of this order, condensation takes place, heat is evolved in various degrees. Now since under the influence of oxygen, combustion

* If a stream of oxygen be admitted into a vessel of hydrogen gas, and an electric spark be passed just above the orifice, a cone of flame will immediately appear, and appearances will be the same as when the stream is hydrogen, and the surrounding medium oxygen.

† A piece of copper foil inflames spontaneously in vapour of sulphur: if iron or copper filings be mixed with sulphur, on applying a moderate heat, violent ignition instantly takes place.

‡ Zinc cannot be made to unite directly with sulphur.

§ This may, perhaps, be controverted, since charcoal and iron, when ignited, decompose water; but in this case, external heat is applied; nor do I recollect to have seen any experimental proof that aqueous vapour alone can support the combustion of charcoal or iron.

is that combination which is attended with the evolution of light and heat, may we not properly consider every similar act of combination in the same light? If we do, the scale is unbroken; heat is excited by every such combination; when it becomes so intense as to be accompanied with light, it is an example of combustion: the light and heat are emitted by both or all the substances jointly; no one in particular is the supporter. Considering the subject in this light, we have a number of substances placed in regular order, whether galvanic, that of real chemical attraction, that of visible chemical attraction, which makes no corrections for the effects of cohesion, temperature, elasticity, solubility, &c. or any other, and the combustibility of substances combined, or the ratio of the quantity of heat excited during their combination, will be reciprocally as their distance from the extremes of the scale. If it could be proved that the light and heat arise and are derived from one body only, that would properly be termed a supporter; but since this is not the case, neither is a supporter, neither is an inflammable (except popular language be used); but the union of the two produces both light and heat. If the term combustion be applied to all cases where a temperature not less than that of ignition is excited, ultimately some force or forces will be developed, whose operation may be seen in every case where condensation takes place, and to which it is to be ascribed.

If we examine the nature of the phenomenon, the rationality of what I have stated will be more apparent. The law seems to be universal, that condensation or reduction of volume excites heat.* If then the density of a compound exceed the mean density of its component parts, heat must be excited. Let $m, n,$ be the weights of two substances; a and b their specific gravities: the volume of the compound $= \frac{m\,b + n\,a}{a\,b}$; its spec. grav. $\left(= \frac{\text{weight}}{\text{volume}}\right) = \frac{\overline{m + n} \,.\, a\,b}{m\,b + n\,a}$:† if then its specific gravity exceed $\frac{\overline{m + n} \,.\, a\,b}{m\,b + n\,a}$, heat is evolved during the process of combination, because condensation is accompanied by the evolution of heat. If the condensation be such that the temperature of ignition, at least, be produced, it is a case of true combustion. Why then arbitrarily limit it to the cases where the presence of one of the received "supporters" is requisite? It is surely more scientific to recognize one operation from the production of the most intense cold to the excitation of the most powerful heat, applicable to every operation and combination of "undecompounded

* The end of a rod of soft iron may be made red-hot by hammering; if a gas be suddenly compressed, heat is evolved; stamping heats metals, &c.

† This excellent formula is taken from the Retrospect, and does not materially differ from Newton's.

substances," than to propose limitations, which make a number of laws of action out of one; and then we must consider every case of chemical combination which excites the temperature of ignition, as a case of combustion; whether the resultant be an oxide, chloride, ioduret, sulphuret, phosphuret, or whatever else it may be. It is also universally allowed that the caloric results from, or is evolved in consequence of the condensation which takes place;* therefore its origin is not to be traced to either substance singly, but to a diminution of the specific heat which is produced by the condensation; and hence the specific † heat of the product of combustion is less than the sum of the specific heats of its component parts. The capacity for heat is generally less than the sum of the capacities; and the difference between the sum of the specific heats before combustion and that of the product, whether oxide, chloride, sulphuret, &c. is a measure of the heat evolved during the combustion; yet in the present state of chemical science, the change which takes place in the capacity cannot be made a measure of the intensity of the force, nor of the quantity of heat evolved: for let a be any very small interval of temperature; at the first interval from the true zero, the temperature will be a, and let the capacity or quantity of heat absorbed in elevating the body to that temperature be c; when the temperature is $a + a$, or $2\,a$, let the capacity be c'; at temperature $2\,a + a = 3\,a$, let the capacity be c'',‡ and so forth, to any assumed temperature. Now at any given temperature, $n\,a$, the specific heat is the sum of the quantities which have been absorbed, i. e. the sum of all the capacities $= c + c' + c'' + c''' + \&c. + c^{\overline{\prime n-1}}$; whilst the capacity at that temperature is simply $c^{\overline{\prime n-1}}$. If then we make the temperature the abscissa of a curve,

* I am aware that the phenomena of gunpowder, of oxymuriate of potash (I use the old-fashioned words, not being able to discover any advantage of potassa over potash, platinum over platina) mixed with sulphur or other inflammables, and of similar compounds are apparently irreconcilable with this statement. But we must remember that the same gas, as it exists in different solid compounds, has not a constant density (as will appear towards the conclusion of this paper); and the quantity of heat remaining will be the greatest where the gas is least condensed, i. e. when united with bodies which have least attraction for it generally. Now in the manufacture of the oxymuriate, or hyperoxymuriate, or chlorate of potash, little heat is evolved; hence the gas retains much of its elastic force, or is but slightly condensed; hence it retains much of its native energy. Similarly, nitrate of potash, or of copper, or silver, retains much of the energy of oxygen; but when united to iron or zinc, little remains. Here we recognise one law of action: when a gas, which is a supporter of combustion, enters into combination with solid matter, and is not highly condensed, it must retain much of its peculiar energy; and hence, much gas, containing but little specific heat, may be evolved, even by the combustion of a mixture of solid matters. Nitre must contain much specific heat; for azote is not inflammable: its union with oxygen requires the aid of external heat: the union of the gaseous compounds of azote with oxygen evolve little heat; the gas is not highly condensed; therefore the acid in nitre retains much of its energy.

† By specific heat I mean the whole quantity of caloric or latent heat which is contained in a body. I mention this, because by many writers, specific heat and capacity for heat are made synonimous.

‡ c, c', c'', $c^{\overline{\prime n-1}}$ are supposed to increase or decrease according to some definite law.

its fluxion will be a; make the area equal to the specific heat; then the capacity for heat, i. e. fluxion of the abscissa × ordinate, will be the fluxion of the area: hence the capacity is the fluxion of the specific heat. When then the ratio of at least three capacities of a body shall be experimentally found, the nature of the curve will be known, since it is $f\,y\,\dot{x}$. In all cases the capacities of weights, proportional to the atomic weights, and which may be called atomic capacities, must be registered, as also the specific heats of the same; these will represent the capacities and specific heats of the atoms themselves. Thus, if the specific heats of two atomic weights A, B, be S, s, the capacities will be $\dot{S}$ and $\dot{s}$: during combination, if no change take place, the specific heat of the compound will be $S + s$; and the capacity $\dot{S} + \dot{s}$; but if a change take place in the capacity, the corresponding change in the specific heat cannot be known until $f\,y\,\dot{x}$ is known. If we examine a number of cases, we shall find that during combustion the capacity is generally diminished. Using Dalton's table of capacities of equal weights, the atomic capacity (1 atom) of oxygen 38·00 + atomic capacity of hydrogen (1 atom) 21·4 = 59·4; the capacity of aqueous vapour = 13·95; hence the diminution of capacity, measured by the same scale, is 45·45. 1 atomic capacity of charcoal 1·56 + 2 atoms capacity oxygen 76·00 = 77·56; that of 1 atom of carbonic acid = 23·1: hence the diminution is 54·46. The capacity is always diminished when heat is evolved during combination; but the change of capacity is not in the direct ratio of the heat evolved, except the capacity be proportioned to the specific heat, in which case the specific heat may be represented by the logarithmic curve.

In all cases of combustion in oxygen, or of combination with gaseous matter, an equal condensation of the gas does not take place. By the above-mentioned formula, having given the relative weights of the elements of a binary compound, the specific gravity of one of the substances, as well as that of the compound, that of the other may be found; for c being the sp. gr. of the compound, $b = \frac{n \,.\, a \,.\, c}{m + n \,.\, a - m \,.\, c}$. The oxygen being b, its specific gravity, as it exists in phosphoric acid, = 5·1; in black oxide of manganese 3·1, red lead 3·2, iron mica 2·28, glass of antimony 2·21, red copper ore 1·47, oxide of arsenic 1·4. From these examples, oxygen seems to be most condensed by those bodies which have the greatest attraction for it, i. e. by those whose electric state is most remote from that of oxygen, or which have the least force of gravity; and these are the bodies which evolve most heat during combustion. If chlorine be the supporter, the inflammables follow a different order; another with iodine; and another for sulphur, &c. according to their respective order in the galvanic series, or in the order of the force of gravity.

The order of inflammability with regard to oxygen is nearly inversely as the atomic capacity: the capacity of gold is 10·0, silver 8·8, copper 7·04, lead 4·16, tin 4·06, iron 3·64, zinc 3·4, charcoal 1·56, and oxygen being the supporter, those bodies which have the least atomic capacity have the most powerful attraction for it, and evolve most heat during combustion; whilst oxygen seems to have the greatest atomic capacity of all known bodies.

Since heat is evolved whenever condensation takes place, and since when the combination of two or more bodies is attended with condensation, heat is evolved, if we consider every such case as an example of combustion, provided heat and light are both evolved, we recognise only one general law applicable to every case of chemical union, one extreme of which is termed combustion; i. e. that where their electric states or forces of attraction are most remote from each other; or rather when they differ most from some one substance, on either side, positive or negative, whose force with each is to be compared according to the electro-chemical law of Sir H. Davy.

Some cases of combustion are accompanied with flame, as in the example of sulphur, phosphorus, potassium in oxygen; others are not, as that of iron wire in oxygen gas; bismuth or antimony powder in chlorine. Here again we may trace the operation of a general law. When any part of the combustible is volatilized, the combustion of its vapour produces flame; if not, flame is wanting. If a large flame be made by burning pitch or resin, a large reddish-white flame is produced; if a large flame of olefiant gas be examined, the flame is whiter, yet red at the top; if the common carburetted hydrogen gas be used, the flame is less brilliant; if pure hydrogen, yet paler, and paler in proportion to the purity of the gases; so that if the oxyhydrogen blowpipe be used, the flame is faint and small. Newton (who, as was said of him, could see more clearly through a mist than others through a microscope) said, "Flame is vapour heated red-hot." Now if we examine the matter fairly, we shall find that he was not far from the truth. If much of the inflammable body, if solid at a high temperature, as carbon, be volatilized, as in the case of resin or pitch, the flame is brilliant, owing to the combustion of the volatilized particles; but owing to their excess (for much soot is deposited) the flame is red. In a large flame of olefiant gas, less soot is produced, and the flame is whiter. In the case of common carburetted hydrogen gas, little or no carbon is deposited, and the flame is whiter: below this limit, the flame becomes paler; so that when very pure oxygen and very pure hydrogen gas are used, the flame is very pale; and the purer the gases, the paler the flame. Hence then it appears, that when a solid which evolves much light during combustion is volatilized, the vapour (if it may be so called) evolves light, most probably in proportion to its quantity. When this is diminished, the quantity of light is reduced, until arriving at a state of purity in oxygen and hydrogen gases, the light and heat approach so nearly to the radiant state (owing to the want of a sufficient quantity of matter in the flame) that very little light is evolved. Hence we may infer, that the quantity of the light depends upon the quantity of solid matter in the flame; for if there be no solid matter, heat sufficient to melt platina, and to produce most vivid light, may pass through a transparent medium without producing any sensible effect; yet if solid matter be introduced, it will be most vividly ignited. Hence a platina wire may slowly consume a mixture of oxygen and inflammable gas. The combustion is too slow to produce flame, yet it is sufficient to ignite the wire: this is analogous to an experiment of Wedgwood, and also to the ignition of spongy platina by a stream of hydrogen, which seems to be owing to the large surface of action and the great density of the metal. Although the quantity of light emitted by gases of the same nature be nearly in proportion to the quantity of oxygen consumed, the law does not obtain with regard to different inflammables; for hydrogen, which consumes the greatest quantity, evolves the least light: the light is in proportion to the quantity of oxygen consumed, the intensity of the heat, the quantity of unconsumed solid raised in vapour, its power of emitting light during combustion directly, and the quantity which escapes combustion in the flame, together with its power of obstructing light, inversely.

From the phenomena of combustion is derived a powerful argument in favour of the materiality of heat or caloric. The smallest spark will set fire to phosphorus, and some other substances: from this combustion other combustibles may be inflamed without limit. If then heat be only a vibratory motion excited among the particles, since the cause is equal to the effect; or since the quantity of motion in the exciting body is equal to the whole elapsed; the quantity of motion in the smallest spark is equal to that in the greatest conflagration. Therefore a body of given magnitude can communicate its own force, quantity of force and velocity, to any assignable quantity of matter, which is absurd.

Article VI.

On Gaseous Bodies. By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

In a former paper I committed a considerable error in deducing the law of force in gases from Newtoni Princip. lib. 1, prop. 90, which I wish now to correct. Instead of taking that view of the subject, it ought to have been investigated by prop. 90, lib. 2; for since the pressure upon the whole surface of a sphere is equal to that of a cylinder of equal density with the atmosphere of the sphere at all equal distances, whose base is equal to the surface of the sphere; and the force upon an hemisphere is equal to that of a similar cylinder whose base is equal to the area of a great circle. Hence the elastic force of a gas will be nearly in proportion to its density. Many curious results relating to the properties of gases follow from the proportion; but since I have not leisure to make an arrangement ready for the next number of the *Annals*, I shall content myself at present with giving notice of the discovery of the error; and shall communicate the proposition, and trace some of its consequences, by the earliest possible opportunity.

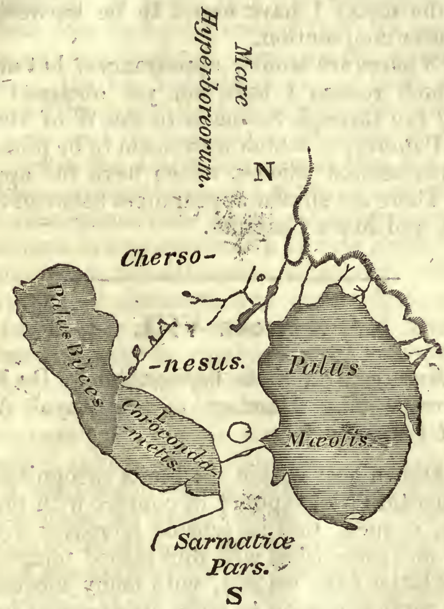
Article VII.

Telescopical Observations on the Moon.
By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

In the figure (see next page) your readers will find an outline of certain appearances of the moon's surface, which I named in my last paper. The figure is not presented as a finished or even accurate drawing; it merely represents the general appearance of those parts on the N boundary of Palus Mœotis, of Hevelius, Mare Crisium of Cassini, which have the appearance of rivers. I have not measured the parts with the micrometer, because I wish to trace the lines to their full extent; to obtain a more correct outline of the very numerous similar objects on the S part than I now possess. To free them from illusions, arising from the shadows of ridges and other objects of similar nature, will require observations continued for a considerable time before the micrometer need be used. The figure here given



is the result of a considerable number of observations. I first obtained a view of the most prominent parts with my short aërial, using a power of about 60. A similar view to what I then obtained is given in Hevelius's Map, entitled, "Tabula Selenographica Phasium Generalis," (Selenographia, p. 202). The different views obtained at various ages of the moon are to be found in Maps Nos. 8 to 20, in some of which, parts of the broad line which runs almost parallel to the margin of Mœotis are most conspicuous; in others, some of the lines which connect it with Mœotis are seen. In Cassini's Map, given in Smith's Optics (the only representation given by that astronomer which I possess), similar portions are to be seen.

On applying my reflector with powers of 130 to 400, the appearance is as in the figure. A scientific friend of mine lately observed a part of what is here delineated with an excellent five foot achromatic; but having less power and light than my reflector, he did not see the finer lines; and from occasional glimpses I have had, I have not been able to trace them to their full extent, although, on one occasion, I used a power of 800, which is very distinct, and has abundance of light. To see the appearances, the air should be in such a state that good and steady discs of the stars may be obtained; the telescope must have abundance of light, a high power, and be very steadily mounted. Under these circumstances, it frequently happens, that the whole cannot be distinctly traced at one view. The best age of the moon I have found to be between eight and twelve days after conjunction.

About the S parts are similar appearances, but more complicated, for which reason I have not yet obtained a complete outline; they run through Sarmatia to the W of Mare Caspium and towards Paludes, to which they seem to be joined; forming in their course several spaces, which have the appearance of small lakes. There are similar appearances between S Extremus, Ponti Euxini, and Mare Caspium.

Article VIII.

On the Existence of a Limit to Vaporization. By M. Faraday, FRS. Corresponding Member of the Royal Academy of Sciences at Paris, &c.*

It is well known that within the limits recognised by experiment, the constitution of vapour† in contact with the body from which it rises, is such, that its tension increases with increased temperature, and diminishes with diminished temperature; and, though in the latter case we can, with many substances, so far attenuate the vapour as soon to make its presence inappreciable to our tests, yet an opinion is very prevalent, and I believe general,‡ that still small portions are produced; the tension being correspondent to the comparatively low temperature of the substance. Upon this view, it has been supposed that every substance in vacuo, or surrounded by vapour or gas, having no chemical action upon it, has an atmosphere of its own around it; and that our atmosphere must contain, diffused through it, minute portions of the vapours of all those substances with which it is in contact, even down to the earths and metals. I believe that a theory of meteorites has been formed upon this opinion.

Perhaps the point has never been distinctly considered; and it may therefore not be uninteresting to urge two or three reasons, in part dependant upon experimental proof, why this should not be the case. The object, therefore, which I shall hold in view in the following pages, is to show that a *limit* exists to the production of vapour of any tension by bodies placed in vacuo, or in elastic media, beneath which limit they are perfectly fixed.

* From the Philosophical Transactions for 1826.

† By the term vapour, I mean throughout this paper that state of a body in which it is permanently and indefinitely elastic.

‡ See Sir H. Davy's paper on Electrical Phenomena exhibited in Vacuo. Philos. Trans. 1822, p. 70.

Dr. Wollaston, by a beautiful train of argument and observation, has gone far to prove that our atmosphere is of finite extent, its boundary being dependant upon the opposing powers of elasticity and gravitation.* On passing upwards from the earth's surface, the air becomes more and more attenuated, in consequence of the gradually diminishing pressure of the superincumbent part, and its tension or elasticity is proportionally diminished; when the diminution is such that the elasticity is a force, not more powerful than the attraction of gravity, then a limit to the atmosphere must occur. The particles of the atmosphere there tend to separate with a certain force; but this force is not greater than the attraction of gravity, which tends to make them approach the earth and each other; and as expansion would necessarily give rise to diminished tension, the force of gravity would then be strongest, and consequently would cause contraction, until the powers were balanced as before.

Assuming this state of things as proved, the air at the limit of the atmosphere has a certain degree of elasticity or tension; and, although it cannot there exist of smaller tension, yet, if portions of it were removed to a farther distance from the earth, or if the force of gravity over it could in any other way be diminished, then it would expand, and exist of a lower tension; upon the renewal of the gravitating force, either by approximation to the earth's surface or otherwise, the particles would approach each other, until the elasticity of the whole was again equal to the force of gravity.

Inasmuch as gases and vapours undergo no change by mere expansion or attenuation, which can at all disturb the analogy existing between them in their permanent state under ordinary circumstances, all the phenomena which have been assumed as occurring with the air at the limit of our atmosphere may, with equal propriety, be admitted with respect to vapour in general in similar circumstances; for we have no reason for supposing that the particles of one vapour more than another are *free* from the influence of gravity, although the force may, and without doubt does, vary, with the weight and elasticity of the particles of each particular substance.

It will be evident also that similar effects would be produced by the force of gravity upon air or vapour of the extreme tenuity and feeble tension referred to, whatever be the means taken to bring it into that state; and it is not necessary to imagine the portion of air operated upon, as taken from the extremity of our atmosphere, for a portion of that at the earth's surface, if it could be expanded to the same degree by an air pump, would undergo the same changes: when of a certain rarity it would just balance the attraction of gravitation, and fill the receiver

* Phil. Trans. 1822, p. 89.

with vapour; but then, if half were taken out of the receiver, the remaining portion, in place of filling the vessel, would submit to the force of gravity, would contract into the lower half of the receiver, until, by the approximation of their particles, the vapour there existing should have an elasticity equal to the force of gravity to which it was subject. This is a necessary consequence of Dr. Wollaston's argument.

There is yet another method of diminishing the elasticity of vapour, namely, by diminution of temperature. With respect to the most elastic substances, as air, and many gases, the comparatively small range which we can command beneath common temperatures does nothing more at the earth's surface than diminish in a slight degree their elasticity, though two or three of them, as sulphurous acid and chlorine, have been in part condensed into liquids. But with respect to innumerable bodies, their tendency to form vapour is so small, that at common temperatures the vapour produced approximates in rarity to the air upon the limits of our atmosphere; and with these, the power we possess of lessening tension by diminution of temperature may be quite sufficient to render it a smaller force than its opponent gravity; in which case it will be easy to comprehend that the vapour would give way to the latter, and be entirely condensed. The metal, silver, for instance, when violently heated, as on charcoal urged by a jet of oxygen, or by the oxy-hydrogen, or oxy-alcohol flame, is converted into vapour; lower the temperature, and before the metal falls beneath a white heat, the tension of the vapour is so far diminished, that its existence becomes inappreciable by the most delicate tests. Suppose, however, that portions are formed, and that vapour of a certain tension is produced at that temperature, it must be astonishingly diminished by the time the metal has sunk to a mere red heat; and we can hardly conceive it possible, I think, that the silver should have descended to common temperatures, before its accompanying vapour will, by its gradual diminution in tension, if uninfluenced by other circumstances, have had an elastic force far inferior to the force of gravity; in which case, that moment at which the two forces had become equal, would be the last moment in which vapour could exist around it; the metal at every lower temperature being perfectly fixed.

I have illustrated this case by silver, because, from the high temperature required to make any vapour appreciable, there can be little doubt that the equality of the gravitating and elastic forces must take place much above common temperatures, and therefore within the range which we can command. But there is, I think, reason to believe, that the equality in these forces, at or above ordinary temperatures, may take place with bodies far more volatile than silver; with substances indeed which boil under common circumstances at 600° or 700° F.

If, as I have formerly shown,* some clean mercury be put at the bottom of a clean dry bottle, a piece of gold leaf attached to the under part of the stopper by which it is closed, and the whole left for some months at a temperature of from 60° to 80°, the gold leaf will be found whitened by amalgamation, in consequence of the vapour which rises from the mercury beneath; but upon making the experiment in the winter of 1824–5, I was unable to obtain the effect, however near the gold leaf was brought to the surface of the mercury; and I am now inclined to believe, because the elastic force of any vapour which the mercury could have produced at that temperature was less than the force of gravity upon it, and that consequently the mercury was then perfectly fixed.

Sir Humphry Davy, in his experiments on the electrical phenomena exhibited in vacuo, found, that when the temperature of the vacuum above mercury was lowered to 20° F. no further diminution, even down to − 20° F. was able to effect any change, as to the power of transmitting electricity, or in the luminous appearances; and that these phenomena were then nearly of the same intensity as in the vacuum made over tin.† Hence, in conjunction with the preceding reasoning, I am led to conclude, that they were then produced independent of any vapour of the metals, and that under the circumstances described; no vapour of mercury existed at temperatures beneath 20° F.

Concentrated sulphuric acid boils at about 600° F. but as the temperature is lowered, the tension of its vapour is rapidly diminished. Signor Bellani ‡ placed a thin plate of zinc at the upper part of a closed bottle, at the bottom of which was some concentrated sulphuric acid. No action had taken place at the end of two years, the zinc then remaining as bright as at first; and this fact is very properly adduced in illustration of the fixedness of sulphuric acid at common temperatures. Here I should again presume, that the elastic force which tended to form vapour was surpassed by the force of gravity.

Whether it be admitted or not, that in these experiments the limit of volatilization, according to the principle of the balance of forces before stated had been obtained, I think, we can hardly doubt that such is the case at common temperatures, with respect to the silver, and with all bodies which bear a high temperature without appreciable loss by volatilization, as platina, gold, iron, nickel, silica, alumina, charcoal, &c.; and consequently that, at common temperatures, no portion of vapour rises from these bodies or surrounds them; that they are really and truly fixed; and that none of them can exist in the atmosphere in the state of vapour.

* Quarterly Journal of Science, x. 354.

† Phil. Trans. 1822, p. 71.

‡ Giornale di Fisica, v. 197.

But there is another force, independent of that of gravity, at least of the general gravity of the earth, which appears to me sufficient to overcome a certain degree of vaporous elasticity, and consequently competent to the condensation of vapour of inferior tension, even though gravity should be suspended; I mean the force of homogeneous attraction.

Into a clean glass tube, about half an inch in diameter, introduce a piece of camphor: contract the tube at the lamp about four inches from the extremity; then exhaust it, and seal it hermetically at the contracted part; collect the camphor to one end of the tube; and then, having placed the tube in a convenient position, cool the other end slightly, as by covering it with a piece of bibulous paper preserved in a moist state by a basin of water and thread of cotton; in this way, a difference in temperature of a few degrees will be occasioned between the ends of the tube, and after some days, or a week or two, crystals of camphor will be deposited in the cooled part; there will not, however, be more than three or four of them, and these will continue to increase in size as long as the experiment is undisturbed, without the formation of any new crystals, unless the difference of temperature be considerable.

A little consideration will, I think, satisfy us, that, after the first formation of the crystals in the cooled part, they have the power of diminishing the tension of the vapour of camphor, below that point at which it could have remained unchanged in contact with the glass, or in space: for the vapour of the camphor is of a certain tension in the cooled end of the tube, which it can retain in contact with the glass, and therefore it remains unchanged; but which it cannot retain in contact with the crystal of camphor, for there it is condensed, and continually adds to its mass. Now this can only be in consequence of a positive power in the crystal of camphor of attracting other particles to it; and the phenomena of the experiment are such as to show, that the force is able to overcome a certain degree of elasticity in the surrounding vapour. There is, therefore, no difficulty in conceiving, that by diminishing the temperature of a body and its atmosphere of vapour, the tension of the latter may be so far decreased, as at last to be inferior to the force with which the solid portion, by the attraction of aggregation, draws the particles to it, in which case it would immediately cause the entire condensation of the vapour.

The preceding experiment may be made with iodine, and many other substances; and indeed there is no case of distinct crystallization by sublimation* which does not equally afford evidence of the power of the solid matter to overcome a positive degree of tension in the vapour from which the crystals are

* Calomel, corrosive sublimate, oxide of antimony, naphthaline, oxalic acid, &c. &c.

formed. The same power, or the force of aggregation, is also illustrated in crystallizing solutions; where the solution has a tendency to deposit upon a crystal, when it has not the same tendency to deposit elsewhere.

It may be imagined that crystallization would scarcely go on from these attenuated vapours, as it does in the denser states of the vapours experimented upon. There is, however, no good reason for supposing any difference in the force of aggregation of a solid body, dependant upon changes in the tension of the vapour about it; and indeed, generally speaking, the method I have assumed for diminishing the tension of the vapour, namely, by diminishing temperature, would cause increase in the force of aggregation.

Such are the principal reasons which have induced me to believe in the existence of a limit to the tension of vapour. If I am correct, then there are at least two causes, each of which is sufficient to overcome and destroy vapour when reduced to a certain tension; and both of which are acting effectually with numerous substances upon the surface of the earth, and retaining them in a state of perfect fixity. I have given reasons for supposing that the two bodies named, which boil at about 600° F. are perfectly fixed within limits of low temperature which we can command; and I have no doubt, that nearly all the present recognised metals, the earths, carbon, and many of the metallic oxides, besides the greater number of their compounds, are perfectly fixed bodies at common temperatures. The smell emitted by various metals when rubbed may be objected to these conclusions, but the circumstances under which these odours are produced are such as not to leave any serious objections on my mind to the opinions above advanced.

I refrain from extending these views, as might easily be done, to the atomic theory, being rather desirous that they should first obtain the sanction or correction of scientific men. I should have been glad to have quoted more experiments upon the subject, and especially relative to such bodies as acquire their fixed point at, or somewhat below, common temperatures. Captain Franklin has kindly undertaken to make certain experiments for me in the cold regions to which he has gone, and probably when he returns from his arduous undertaking, he may have some contributions towards this subject.

Article IX.

Account of some Volcanic Eruptions, &c. in the Islands of Japan.

Professor Daubeny observes, in his Description of active and extinct volcanos lately reviewed in the Annals,* that "in the islands of Japan, ten volcanos have been enumerated, but little is known concerning them;" and he only mentions in particular three volcanos in Jesso, the northernmost of the Japanese islands, with two diminutive cones on islands near its south-western extremity, and one volcano in Satzuma Bay in the most southern island called Kiou-siou. In this deficiency of information respecting so considerable a portion of the volcanic chain extending from the peninsula of Kamtschatka through the Kurule islands and those of Japan to the group of Loo Choo, and thence to Formosa, and the Phillipines, the subjoined particulars of volcanic eruptions in Niphon, the central and largest island of the Japan group, and also in Kiou-siou, extracted from Titsingh's Illustrations of Japan, may be acceptable to such of our readers as are interested in the subject. They have been derived from some collections relative to the phænomena of volcanos, made whilst engaged in studying the history of Meteorites, with the immediate view of preparing the means for investigating the question, whether any important coincidences in point of time could be detected between the descents of meteorites and the eruptions of volcanos,† and with the ulterior design of laying the foundation of a treatise on volcanic phænomena; this design, however, has been altogether superseded by the publication of Dr. Daubeny's excellent work, which comprises nearly all the information on volcanos that the present writer had been able to collect. But in a few instances, as in the present one, the collections alluded to afford particulars unnoticed by Dr. D. and these it may now be useful occasionally to give to the public in this Journal.

The following extracts from M. Titsingh's work ‡ are written in a confused, and in some places in an obscure manner, and exhibit perhaps a degree of oriental hyperbole, or at least of

* See the present vol. p. 215.

† Although the idea of the projection of meteorites from volcanos has been discarded by the best-informed writers on their nature and origin, yet there are many facts respecting the meteors from which they descend, and the mineralogical constitution of the substances themselves, which render it necessary in the thorough investigation of their history, to keep in view all the phænomena of volcanos. That meteorites cannot be emitted, *as such*, from volcanos, appears certain; but that the causes of volcanic phænomena are intimately allied to those of the phænomena of meteorites, if they are not actually the same with them, the present writer hopes to prove, in a work exhibiting the present state of our knowledge regarding meteorites, which will be published in the ensuing spring.

‡ Illustrations of Japan; by M. Titsingh, translated from the French by Frederic Shoberl. London, 1822, 4to.

exaggeration arising from the fears and sufferings of the narrators. They have also, it is probable, undergone some alteration in character, from their successive translation from the Japanese language into Dutch, and thence (perhaps through the French) into English. Although they do not present any novel features in the history of volcanos, they may serve to show that those of Japan are among the most active on the globe; and the swallowing-up of the twenty-seven villages in the province of Sinano would appear to be a parallel case to the phænomenon exhibited by the mountain Papandayang in Java, described by Dr. Horsfield.*

It is not easy to decide whether the convulsions recorded of the Mountains Unsen and Miyiyama in the island of Kiou-siou, were really of a volcanic nature or not: if they were, the descent of the water produced, as at Vesuvius, by the condensation of the vapours emitted by the crater, or, as in the Andes, by the melting of the snow upon their summits, caused by the eruptions, has doubtless been mistaken in the narrative, for the ejection of water from the volcanos; an error which has more than once been committed by describers of volcanic phænomena in Europe. E. W. B.

In the beginning of the month of Sept. 1783, M. Titsingh received from Yedo the following particulars of the dreadful ravages occasioned by the eruption of the volcano, Asama-ga-daki, in the districts of Djozou and Zinzou.

On the 28th of the sixth month of the third year *Ten-mio* (July 27, 1783), at eight o'clock in the morning, there arose in the province of Sinano,† a very strong east wind, accompanied with a dull noise like that of an earthquake, which increased daily, and foreboded the most disastrous consequences.

On the 4th of the seventh month (Aug. 1), there was a tremendous noise, and a shock of an earthquake; the walls of the houses cracked, and seemed ready to tumble; each successive shock was more violent, till the flames burst forth, with a terrific uproar from the summit of the mountain, followed by a tremendous eruption of sand and stones: though it was broad day, every thing was enveloped in profound darkness through which the flames alone threw at times a lurid light. Till the 4th of August, the mountain never ceased to cast up sand and stones.

The large village of Sakamoto, and several others situated at the foot of the volcano, were soon reduced to ashes by the ignited matter which it projected, and by the flames which burst from the earth. The inhabitants fled; but the chasms every-where formed by the opening of the ground prevented

* See Daubeny's Volcanos, p. 317.

† An extensive central province of the island of Nifon, to the north-west of Kai and of Mousasi, in which Yedo is situated.

their escape, and in a moment a great number of persons were swallowed up, or consumed by the flames; violent shocks continued to be felt till the 8th of the seventh month, and were perceptible to the distance of twenty or thirty leagues: enormous stones and clouds of sand were carried by the wind towards the east and north.

The water of the rivers Yoko-gawa and Karousawa boiled; the course of the Yone-gawa, one of the largest rivers of Japan, was obstructed, and the boiling water inundated the adjacent country, doing incredible mischief. The bears, hyænas, and other beasts of prey, fled from the mountains, and flocked to the neighbouring villages, where they devoured the inhabitants, or mangled them in a horrible manner. The number of dead bodies floating upon the rivers was incalculable.

About the middle of the same month, a more circumstantial account of this phænomena was transmitted to me from Yedo. It is in substance as follows:

From the 4th of the seventh month (Aug. 1), there was heard night and day a rumbling like that of very loud thunder, which gradually increased in violence. On the 5th, a shower of sand and ashes fell on all sides; and on the 6th, the volcano projected at Ouye-wake an immense quantity of stones, some of which were so large, that two persons were not able to carry them. Twenty-seven villages were swallowed up, and four only escaped, namely, Matsyeda, Yasouye, Takasakie, and Fousie-oka. At the last of these places, there fell a shower of red-hot stones, each weighing four or five ounces. At two o'clock the same day, the mountain Asama cast forth a torrent of flames and balls of fire; the earth shook in a frightful manner; the whole country was enveloped in darkness, and, though mid-day, it was not to be distinguished from the darkest night. The thunder was so tremendous that the inhabitants were paralyzed with terror to such a degree as to appear inanimate. About ten o'clock there fell small stones mixed with sand and ashes at Fousic-oka, the ground was covered with them to the depth of eight or nine inches; at Yasouye they were fourteen or fifteen inches, and at Matsyeda three feet deep. All the growing crops were totally destroyed.

On the 7th, about one o'clock, several rivers became dry; at two a thick vapour was seen at Asouma over the river Tane-gawa, the black muddy water of which boiled up violently. An immense quantity of red-hot stones floating on the surface gave it the appearance of a torrent of fire. Mokou, one of the life-guards, and a great number of men and horses, were swept away by the current, and cast on shore at Nakanose, or carried along by the river Zin-meï-gawa.

On the 8th, at ten in the morning, a torrent of sulphur, mixed with rocks, large stones and mud, rushing from the mountain, precipitated itself into the river Asouma-gawa, in the districts of Djozou and Gemba-kori, and swelled it so prodigiously that it overflowed, carried away houses, and laid waste the whole country. The number of persons who perished was immense.

At Zinya-tchekou, on the road to Naya-kama, there were incessant and violent shocks from the 6th to the 8th.

At Sakamoto-tchekou, there was a continued shower of red-hot stones from the 5th to the 6th.

At Fonsio-tchekou, gravel fell in an incessant torrent.

At Kouraye-sawa, there fell such a prodigious quantity of red-hot stones, that all the inhabitants perished in the flames, with the exception of the chief magistrate: the exact number of the dead is not known.

On the 9th, about one o'clock, large trees and timbers of houses began to be seen floating on the river of Yedo, which was soon afterwards completely covered with the mangled carcasses of men and beasts. In the country of Zinzou, the devastation extended over a tract of thirty leagues.

At Siomio, Asouma-kori, and Kamawara-moura, at the foot of Mount Asama, all the inhabitants perished except seventeen.

Half of the village of Daïzen-moura was carried away by the lava.

The villages of Nisikoubo-moura, Nakaï-moura, Fao-moura, Kousaki-faramoura, and Matski-moura, totally disappeared.

At the village of Tsoubou-moura, the warehouse of Soukisayemon was preserved; all the other houses, with the inhabitants, were swept away by the fiery deluge.

The villages of Tsoutchewara-moura, Yokokabe-moura, Koto-moura, Kawato-moura, Fa-moura, Kawafarayou-moura, and Farada-moura were likewise swept away.

Fifty-seven houses of the village of Misima-moura were swallowed up, and sixteen persons carried away by the torrent, which every where left a sediment of sand of the depth of ten feet.

At Gounba-kori, Kawasima and Fara-moura, out of 153 houses, six only were left; the others were carried away.

The whole village of Obasi-moura disappeared.

The village of Ono-moura and the guard-house of Mokou were swept away by a torrent of boiling mud.

The village of Yemaye-moura was completely buried by sand.

Many other villages, besides those here named, either partly disappeared with their inhabitants, or were swept away. It was impossible to determine the number of the dead, and the devastation was incalculable. P. 97—100.

On the 18th of the first month of the fifth year *Kouansei* (1793), about five o'clock in the afternoon, the whole summit of the mountain of Unsen fell in, and the cavity thus formed was so deep, that it was impossible to hear the noise made in falling by the stones that were thrown into it. Torrents of boiling water gushed from all parts, and the vapour which rose from it resembled a thick smoke. The latter phænomenon ceased in a few days.

On the 6th of the second month, there was an eruption of the volcano of Bivo-no-koubi, about half a league from its summit. The flames ascended to a great height; the lava which ran down spread with rapidity at the foot of the mountain, and in a few days the whole country, for several miles round, was in flames. The fire consumed all the trees on the neighbouring heights, and the valley, in which it made the greatest havoc, was soon covered with relics of burnt matter, and filled with stones and ashes. The fire was not like ordinary fire; it was sparkling and of a reddish colour, interrupted from time to time by brown blazes. On the 1st of the third month, at ten o'clock at night, a tremendous earthquake was felt throughout the whole island of Kiou-siou,* but particularly in the province of Simabara. The first shock was so violent that people could scarcely keep on their legs; they were seized at the same time with a complete stupefaction, so that they had scarcely presence of mind to provide for their personal safety. Immense rocks were precipitated from the mountain; the earth opened, the houses were shaken with such force, that the inhabitants durst not stay in them for fear of being crushed in the ruins. Neither could they venture to stop any where, from apprehension of the inundation which usually follows a violent earthquake; and the recollection of what had happened some years before in Sinano, as already related in the proper place, heightened the terror of the inhabitants. Carrying the sick and the children in their arms, they set out in troops in quest of some place of refuge from a similar calamity. Nothing was to be heard but cries, lamentations, and fervent prayers imploring the protection of heaven. The shocks having ceased, in a few hours they returned to their homes. Some houses were demolished, and their inmates buried in the ruins; but fortunately the mischief was not so great as had been feared.

The mountain meanwhile continued burning, and the lava spread obliquely towards the castle; but being stopped in its course by a great number of rocks, it turned slowly to the north. The inhabitants were in terrible alarm, because the shocks were incessantly recurring, though with less violence than at first.

On the 1st of the fourth month, about noon, when every body was at dinner, a fresh shock was felt with a motion which lasted upwards of an hour and a half, and became more and more violent, threatening all around with instant destruction. It was not long before several houses beyond the castle were ingulphed with their inhabitants, which seemed to be the signal for the most dreadful disasters. The cries of men and animals aggra-

* Kiou-siou, or Kidjo (the nine provinces) is thus named on account of its division into nine provinces. It is the second in extent and the westernmost of the islands composing the empire of Japan.

vated the horrors of the catastrophe. Prodigious rocks rolling from the mountain overthrew and crushed every thing that happened to be in their way.

A tremendous noise, resembling loud and repeated discharges of artillery, was heard under ground and in the air: at length, when the danger was supposed to be over, a horrible eruption of Mount Miyiyama took place: the greatest part of it was exploded into the air, fell into the sea, and by its fall raised the water to such a height as to inundate both the town and country. At the same time, an enormous quantity of water issuing from the clefts of the mountain, met the sea-water in the streets, and produced whirlpools, which, in some places, washed away the very foundations of the houses, so as to leave not a vestige of habitations. The castle alone remained uninjured, because the water could not penetrate its strong massive walls: several houses near it were so completely destroyed, that not one stone was left upon one another. Men and beasts were drowned by the flood. Some were found suspended from trees, others standing upright, others kneeling, and others again on their heads in the mud; and the streets were strewed with dead bodies. Out of all those who fled for the purpose of seeking refuge in the castle, a very small number effected their escape, and all these had received more or less injury. The cries of those who were still alive beneath the ruins pierced the heart, and yet no assistance could be rendered them.

At length recourse was had to the expedient of sending fifty criminals from the castle to remove the rubbish, for the purpose of extricating such of the miserable wretches as were still living, and of interring the dead. Of those who were taken out of the ruins, some had their legs, others their arms, or other members, fractured. The tubs which are used in Japan, instead of coffins, for burying the dead, were uncovered in the cemeteries, or broken, the large stones laid over them having been carried away by the torrent. Thus the whole country was all at once transformed into a desert; but the province of Figo, opposite to Simabara, is reduced to a still more deplorable state. Its form seems to have been entirely changed; not the least trace of what it was formerly is now to be discovered. A great number of vessels which lay at anchor in the neighbourhood went to the bottom; and an incredible number of carcases of men and beasts and other wrecks, were brought down by the current, so that the ships could scarcely force a passage through them. The wretchedness that every where prevails is inexpressible, and fills the spectator with horror. The number of those who are known to have perished exceeds 53,000; and it is impossible to describe the consternation produced by this catastrophe.

P. 109—112.

Article X.

*Altitudes of the Stations, and of several other Remarkable Hills in England and Wales, in English Feet above the level of the Sea, with Occasional Notices of their constituent Rocks, computed from the Observations made in the Course of the Trigonometrical Survey.**

		Feet
Agnes Beacon (St.) *Cornwall*	Clay-slate	621
Allington Knoll, *Kent*	Weald clay	329
Allport Heights, *Derbyshire*	Millstone grit	980
Alnwick Moor, *Northumberland*		808
Ann's Hill (St.) *Surrey*	Upper Marine	240
Arbury Hill, *Northamptonshire*	Oolite	804
Arran Fowddy, *Merionethshire*	Greywacke slate	2955
Arrenig, *Merionethshire*	Greywacke slate	2809
Ash Beacon, *Somerset*		655
Ashley Heath, *Staffordshire*		801
Axedge, *Derbyshire*		1753
Bagborough, *Somerset*	Greywacke slate	1270
Bagshot Heath, *Surrey*	Upper Marine	463
Banstead, *Surrey*	Plastic clay	576
Bar Beacon, *Staffordshire*	New red sandstone,	653
Bardon Hill, *Leicestershire*	Greywacke	853
Barnaby Moor, *Yorkshire*		784
Beacon Hill, *Wiltshire*		690
Beacons of Brecknock, *Brecknock*	Old red sandstone,	2862
Beachy Head, *Sussex*	Chalk	564
Beeston Castle (Top of) *Cheshire*		556
Belle-field Hill, *Cheshire*		401
Beryl Hill, *Lancashire*		128
Billing Beacon, *Lancashire*		633
Bindown, *Cornwall*		658
Black Comb, *Cumberland*	Clay-slate	1919
Black Down, *Dorsetshire*	Chalk	817
Black Hambleton Down, *Yorkshire*	Oolite	1246
Blackheddon, *Northumberland*		646
Bleasdale Forest, *Lancashire*	Millstone grit	1709
Bodmin Down, *Cornwall*		645
Bolt Head, *Devonshire*		630
Boulsworth Hill, *Lancashire*	Millstone grit	1689
Botley Hill, *Surrey*	Chalk	880
Botton Head, *Yorkshire*	Oolits	1485

* Outlines of Mineralogy and Geology, comprehending the Elements of those Sciences; intended principally for the Use of Young Persons. Fourth Edition, 1826. By William Phillips, F.L.S. F.G.S. &c.

		Feet
Bow Brickill, *Bucks*	Iron sand	683
Bow Fell, *Cumberland*	Clay-slate	2911
Bow Hill, *Sussex*	Chalk	702
Bradfield Point, *Yorkshire*		1246
Bradley Knoll, *Somersetshie*		973
Brandon Mount, *Durham*		875
Brenin Fawr, *Pembrokeshire*		1285
Brightling Down, *Sussex*		646
Broadway Beacon, *Gloucestershire*	Oolite	1086
Brown Clee Hill, *Shropshire*	Trap	1805
Brown Willy, *Cornwall*	Granite	1368
Bull Barrow, *Dorsetshire*		927
Burian (St.), *Cornwall*		415
Burleigh Moor, *Yorkshire*		553
Butser Hill, *Hampshire*	Chalk	917
Butterton Hill, *Devonshire*	Granite	1203
Bwlch Mawr, *Caernarvonshire*	Greywacke slate	1673
Cader Ferwyn, *Merionethshire*	Greywacke slate	2563
Cader Idris, *Merionethshire*	Green st. and slates	2914
Cadon, Barrow, *Cornwall*	Clay slate	1011
Caermarthen Vau, *Caermarthenshire*		2596
Calf Hill, *Westmoreland*		2188
Capellante, *Brecknockshire*	Old red sandstone,	2394
Capel Kynon, *Caernarvonshire*		1046
Carn Bonellis, *Cornwall*	Granite	822
Carn Minnis, *Cornwall*		805
Carnedd David, *Caernarvonshire*	Greywacke	3427
Carnedd Llewellyn, *Caernarvonshire*	Greywacke	3469
Carraton Hill, *Cornwall*		1208
Castle Ring, *Staffordshire*	New red sandstone,	715
Cawsand Beacon, *Devonshire*	Granite	1792
Cefn Bryn, *Glamorganshire*	Old red sandstone,	583
Chanctonbury Hill, *Sussex*	Chalk	814
Charton Common, *Dorset*		582
Cheviot, *Northumberland*	Greenstone?	2658
Clifton Beacon, *Yorkshire*		417
Cleave Down, *Gloucestershire*	Oolite	1134
Collier Law, *Durham*	Millstone grit	1678
Coniston Fell, *Lancashire*	Greywacke	2577
Cradle Mountain, *Brecknockshire*	Old red sandstone,	2545
Cross Fell, *Cumberland*	Coal measures	2901
Crowborough Beacon, *Sussex*	Iron sand	804
Cern y Brain Mountain, *Denbighshire*,	Mountain limest.?	1857
Danby Beacon, *Yorkshire*	Oolite	966
Deadman, *Cornwall*		379

		Feet
Dean Hill, *Hampshire*	Plastic clay	539
Delamere Forest, *Cheshire*	New red sandstone,	596
Dent Hill, *Cumberland*	Greywacke	1115
Ditchling Beacon, *Sussex*	Chalk	858
Dover Castle, *Kent*	Chalk	469
Dumpdon Hill, *Dorset*		879
Dundon Beacon, *Somerset*		360
Dunkery Beacon, *Somerset*	Greywacke	1668
Dundry Beacon, *Somerset*	Oolite	790
Dunnose, *Isle of Wight*	Iron sand	792
Dwggan, near Builth, *Brecknockshire*	Old red sandstone,	2071
Easington Heights, *Yorkshire*	Lias	681
Epwell Hill, *Oxford*	Oolite	836
Fairlight Down, *Sussex*	Chalk	599
Farley Down (near Bath) *Gloucestersh.*	Oolite	700
Firle Beacon, *Sussex*	Chalk	820
Folkstone Turnpike, *Kent*	Chalk	575
Frant Steeple (Top) *Sussex*	Iron sand	659
Furland (near Dartmouth) *Devon*	Greywacke	589
Garreg Mountain, *Flintshire*		835
Garth (The) *Glamorganshire*		981
Gerwyn Goch, *Caernarvonshire*	Greywacke	1723
Go Hill, *Lancashire*		304
Goudhurst, *Kent*		497
Grassmere Fell, *Cumberland*		2756
Greenwich Observatory, *Kent*	Plastic clay	214
Gringley on the Hill, *Yorkshire*		235
Gwaunysgaer Down, *Flintshire*	M. limestone ?	732
Haldon (Little), *Devonshire*	Greensand	818
Hanger Hill (Tower), *Middlesex*		251
Hathersedge, *Derbyshire*	Millstone grit	1377
Hawkeston Obelisk (Top) *Shropshire*		812
Hedgehope, *Northumberland*	Greenstone ?	2347
Helvellin, *Cumberland*	Clay slate	3055
Hensbarrow Beacon, *Cornwall*	Granite	1034
Heswell Hill, *Cheshire*	New red sandstone,	475
Highbeech, *Essex*	London clay	750
Highclere Beacon, *Hampshire*	Chalk	900
Highgate Down, *Pembrokeshire*	Old red sandstone,	294
High Nook, near Dimchurch, *Kent*		28
High Pike, *Cumberland*	Clay slate	2101
Hind Head, *Surrey*	Green sand	923
Holland Hill, *Nottinghamshire*	New red sandstone,	487

		Feet
Holme Moss, *Derbyshire*	Coal measures	1859
Hollingborn Hill, *Kent*	Chalk	616
Holy-Head Mountain, *Anglesea*	Clay slate ?	709
Hundred Acres, *Surrey*	Plastic clay	443
Hunsley Beacon, *Yorkshire*	Chalk	531
Ingleborough Hill, *Yorkshire*	Mountain limest.	2361
Inkpin Beacon, *Hampshire*	Chalk	1011
Kensworth, *Hertfordshire*	Chalk	904
Kilhope Law, *Durham*	Mountain limest.	2196
King's Arbour, *Middlesex*	London clay	132
Kit Hill, *Cornwall*	Granite	1067
Lansdown, *Somersetshire*	Oolite	813
Ledstone Beacon, *Yorkshire*		278
Leith Hill, *Surrey*	Greensand	993
Lillyhoe, *Hertfordshire*	Chalk	664
Llandinam Mountain, *Montgomerysh*	Greywacke	1898
Llanelian Mountain, *Denbighshire*	Transition limest.	1110
Llangeinor Mountain, *Glamorganshire*,	Coal measures	1859
Llannon, *Caermarthenshire*	Greywacke slate	912
Llwydiart Mountain, *Anglesea*	Clay slate ?	523
Long Mount Forest, *Shropshire*	Greywacke	1674
Long Mountain, *Montgomeryshire*	Greywacke	1330
Loosehoe, *Yorkshire*	Oolite	1404
Lord's Seat, *Yorkshire*	Millstone grit	1715
Maker Heights, *Cornwall*	Greywacke ?	402
Malvern Hills, *Worcestershire*	Hornblende rock	1444
Marros Beacon, *Caermarthenshire*		514
Margam Down, *Glamorganshire*	Coal measures	1099
May Hill, *Gloucestershire*	Transition limest.	965
Moel Fammau, *Denbighshire*	Greywacke	1845
Moel Morwith, *Denbighshire*	Greywacke	1767
Moel Issa, *Denbighshire*	Monntain limest.	1037
Moel Rhyddlad, *Anglesea*	Clay slate ?	465
Moor Lynch (Windmill) *Somerset*	Lias	330
Motteston Down, *Isle of Wight*	Chalk	698
Mow Copt, *Cheshire*	Coal measures	1091
Muzzle Hill, *Bucks*		744
Nettlebed (Windmill) *Oxfordshire*		820
New Inn Hill, *Caermarthenshire*		1168
Newton Down, *Pembrokeshire*	Old red sandstone	322
Nine Barrow Down, *Dorsetshire*	Chalk	642
Nine Standards, *Westmoreland*	Millstone grit	2136

		Feet
North Berule, *Isle of Man*	Greywacke	1804
Norwood, *Surrey*	London clay	389
Nuffield Common, *Oxfordshire*	Chalk	757
Ogmoor Down, *Glamorganshire*		292
Paddlesworth, *Kent*	Chalk	642
Pen Hill, *Yorkshire*		2245
Pendle Hill, *Lancashire*	Millstone grit	1803
Pengarn, *Merionethshire*	Greywacke ?	1510
Penmaen Maur, *Caernarvonshire*		1540
Pennigant Hill, *Yorkshire*	Mountain limest.	2270
Pertinney, *Cornwall*		689
Pillar, *Cumberland*		2893
Pilsdon Hill, *Dorsetshire*	Green sand ?	934
Plumstone Down, *Pembrokeshire*	Greenstone	573
Plynlimmon, *Cardiganshire*	Greywacke	2463
Pontop Pike, *Durham*	Coal measures	1018
Portsdown Hill, *Hampshire*	Chalk	447
Precellx Top, *Pembrokeshire*		1754
Radnor Forest, *Radnorshire*	Greywacke	2163
Rhiw Mountain, *Caernarvonshire*	Greywacke ?	1013
Rippin Tor, *Devon*	Granite	1549
Rivel Mountain, *Caernarvonshire*	Greywacke ?	1866
Rivington Hill, *Lancashire*	Millstone grit	1545
Rodney's Pillar (Base of) *Montgom.*	Trap	1199
Rook's Hill, *Sussex*	Chalk	702
Roseberry Topping, *Yorkshire*	Oolite	1022
Rufflaw, *Northumberland*		595
Rumbles Moor, *Yorkshire*	Millstone grit	1308
Saddleback, *Cumberland*	Greywacke ?	2787
Sarum (Old), *Wilts*	Chalk	339
Sca Fell (Low Point), *Cumberland*	Clay slate	3092
Sca Fell (High Point), *Cumberland*	Clay slate	3166
Scilly Bank, *Cumberland*		500
Scutchamfly Beacon, *Berks*		853
Sennen, *Cornwall*	Granite	387
Sherwood Forest, *Nottinghamshire*	New red sandstone,	600
Shooters Hill, *Kent*	Plastic clay	446
Shunnor Fell, *Yorkshire*	Millstone grit	2329
Simonside Hill, *Northumberland*	Coal measures	1407
Skiddaw, *Cumberland*	Greywacke ?	3022
Snea Fell		2004
Snowdon, *Caernarvonshire*	Greywacke	3571
Staincross Heights, *Yorkshire*		514

		Feet
Stathern Point, *Leicestershire*	Oolite	490
Stockbridge Hill, *Hants*		620
St. Stephen's Down, *Cornwall*	Granite	605
Stow Hill, *Herefordshire*		1417
Stow on the Wold, *Gloucestershire*		883
Swingfield Steeple (Top), *Kent*		530
Symond's Hill, *Gloucestershire*		795
Talsarn, *Cardiganshire*	Greywacke	1143
Tenterden Steeple, *Kent*		322
Thorney Down, *Somerset*	Mountain limest.	610
Tregarron Down, *Cardiganshire*	Greywacke	1747
Trelleg Beacon, *Monmouthshire*		1011
Trevose Head, *Cornwall*		274
Water Cragg, *Yorkshire*		2186
Weaver Hill, *Staffordshire*	Millstone grit	1154
Wendover Down, *Buckinghamshire*		905
Westbury Down, *Wilts*		775
Whernside (in Ingleton Fells) *Yorksh.*	Mountain limest.	2384
Whernside (in Kettlewell Dale) *Yorksh.*	Millstone grit	2263
White Horse Hill, *Berkshire*	Chalk	893
Whiteham Hill, *Berkshire*		576
Wilton Beacon, *Yorkshire*		809
Wingreen Hill, *Dorsetshire*		941
Wittle Hill, *Lancashire*		1614
Wordeslow Hill, *Durham*		632
Wrekin, *Shropshire*		1320
Ynaliog Mountain, *Caernarvonshire*		584

Article XI.

Analyses of Books.

*Memoirs of the Astronomical Society of London, Vol. II. Part II.**

The contents of this part of the Memoirs of the Astronomical Society are as follows :—

XX. *On the Latitude of the Royal Observatory of Greenwich.* By John Pond, Esq. Astronomer Royal.

XXI. *On the Determination of Latitudes by Observations of Azimuths and Altitudes alone.* By M. Littrow, Director of the Imperial Observatory at Vienna, Assoc. Ast. Soc. &c. (Translated by the Foreign Secretary.)

XXII. *Mémoire sur différens Points relatifs à la Théorie des*

* For the contents of vol. ii. Part 1. of these Memoirs, see *Annals* for April last.

Perturbations des Planètes exposée dans la Mécanique Céleste. Par M. Plana.

XXIII. *Mr. John Ramage's Description of his large Reflecting Telescopes.* (With Plates.)

XXIV. *On Parallaxes.* By J. J. Littrow, Director of the Imperial Observatory, and Professor of Astronomy at Vienna, Associate of the Astronomical Society of London, &c.

XXV. *On the Co-latitude of the Observatory of Stephen Groombridge, Esq. at Blackheath; determined by his own Observations of Circumpolar Stars, reduced by the Constant of Refraction* 58″, 133 *at* 45°.

XXVI. *Observations of the Eclipses of Jupiter's Satellites, made at Futty Ghur, on the Ganges* (*N. Lat.* 27° 21′ 35″) *in* 1824–5. By Major J. A. Hodgson, Revenue Surveyor General. Communicated in a Letter to the Secretary of the Astronomical Society.

XXVII. *A Comparison of Observations made on Double Stars.* By Professor Struve. Communicated in a Letter to J. F. W. Herschel, Esq. Foreign Secretary of this Society.

XXVIII. *Observations of the Occultation of Saturn on the* 30*th October*, 1825. By R. Comfield, Esq. and J. Wallis, Esq. Communicated in a Letter from the former to Dr. Gregory, Secretary of the Society.

XXIX. *Account of some Observations made with a* 20-*feet Reflecting Telescope.* By J. F. W. Herschel, Esq. For. Sec. Ast. Soc: comprehending,

1. *Descriptions and approximate Places of* 321 *new Double and Triple Stars.*
2. *Observations of the second Comet of* 1825.
3. *An Account of the actual State of the Great Nebula in Orion compared with those of former Astronomers.*
4. *Observations of the Nebula in the Girdle of Andromeda.*

XXX. *Explanation of the Method of observing with the two Mural Circles, as practised at present at the Royal Observatory of Greenwich.* By John Pond, Esq. Astronomer Royal.

XXXI. *Extracts from three Letters, addressed by M. Gambart, Director of the Royal Observatory at Marseilles, to James South, Esq. MAS. respecting the Discovery, and Elements of the Orbit, of a Comet, which appears to be the same with that of* 1772 *and* 1805. Communicated by James South, Esq.

XXXII. *Report of the Committee appointed by the Council of the Astronomical Society of London, for the purpose of examining the Telescope constructed by Mr. Tulley, by Order of the Council.**

XXXIII. *Micrometrical Observations of the Planet Saturn, made with Fraunhofer's large Refractor, at Dorpat.* By Prof. Struve, Associate of the Astronomical Society of London.

XXXIV. *Summary of the Observations made for the Deter-*

* See *Annals*, N. S. vol. xi. p. 440.

mination of the Latitude of the Observatory of Wilna. By J. Slawinsky, Associate of the Astronomical Society of London.

XXXV. *Supplement to a former Paper "On the Latitude of the Royal Observatory of Greenwich."* By John Pond, Esq. Astronomer Royal.

*Report of the Council of the Society to the Sixth Annual General Meeting.**

Addresses of Francis Baily, Esq. FRS. President of the Astronomical Society of London, on presenting the Honorary Medals of the Society to the several Persons to whom they had been awarded.†

The remainder of the General Catalogue of the principal Stars

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New Tables for facilitating the computation of Precession, Aberration, and Nutation of 2881 *principal fixed Stars, together with a Catalogue of the same reduced to January* 1, 1830*; computed at the expence and under the direction of the Astronomical Society of London; to which is prefixed an Introduction explanatory of their Construction and Application.* By Francis Baily, Esq. President of the Society.

From this work we extract the following, as the severest test of its merit.

"The preceding Catalogue having been finished, it became desirable to ascertain how far the mean places of the stars, (which had been brought up from the observations of Bradley and Piazzi by means of the proposed formula) could be depended upon. With this view a comparison was made with the places of the 36 Greenwich stars that have been observed and reduced at different times by Messrs. Bessel, Brinkley, and Pond: and which is inserted at the end of the work.

"There are two Catalogues of the Right Ascension of the 36 Greenwich stars published by M. Bessel in the Konigsberg Observations: one (which may be considered as Dr. Maskelyne's catalogue of 1805) reduced to 1815, and the other (depending on M. Bessel's own observations) reduced to 1825. Both these catalogues were brought up to 1830 by means of the annual variations attached to the catalogue of 1825. The catalogue of the same stars by Dr. Brinkley was taken from M. Schumacher's *Astronomische Nachrichten*, No. 78: it is there reduced to 1824, but was brought up to 1830 by means of the annual variations annexed thereto. The *first* catalogue, in AR, of Mr. Pond, was taken from that (reduced to 1819) which is inserted in the Nautical Almanac for 1822; and was the last that was published prior to his important alteration of the position of the equinoctial points by the addition of $0^{s}{\cdot}31$ to all the stars. The *second* catalogue of Mr. Pond was that (reduced to 1825) which is published in the Nautical Almanac for 1829, and contains the latest corrections, to August 1826. Both

* See *Annals*, N. S. vol. xi. p. 295. † See Ibid. p. 454.

these catalogues were brought up to 1830 by means of the annual variations annexed to the *latter* catalogue.

"On a comparison of these several catalogues it appears that, as to the *Right Ascensions*, the catalogue of the Astronomical Society falls far within the limits of the errors of observation: since more than two-thirds of the stars there compared are *between* the mean places as severally given by these eminent observers: and in those instances where this is not the case, the position does not differ so much from that of some one of the observers, as those observers do from each other, and from themselves.

"With respect to the *North Polar Distances* recourse was had to the two catalogues of Mr. Pond: one reduced to 1818, (being the last correction of his Standard Catalogue of 1812-13, prior to the derangement of the mural circle) and published in the Nautical Almanac for 1820: the other reduced to 1825, and taken from the Nautical Almanac for 1829, above mentioned. These were brought up to 1830 by means of the annual variations annexed to the latter catalogue: and which differ, in some instances very considerably, from the values annexed to the catalogue of 1818. Out of the 70 comparisons made, it will be found that in nearly one half of them the difference is below one second; that in 16 others the difference is below two seconds; and that in 7 others the difference is below three seconds: whilst the difference in the remainder (which in five cases, only, exceeds four seconds) may be considerably reduced by the adoption of the annual variations annexed to the catalogue of 1818; the difference of which will in fact, in many of the comparisons, amount to a quantity equal to the whole of the difference in question. Indeed, a difference in the *mode of reduction* will frequently account for differences, as great as any that occur in these comparisons."

The *mean difference* of each catalogue from that of the Astronomical Society is inserted at the bottom of the respective columns: and will be found as follows:

Bessel,	1815 =	− 0s·004	Right Ascension.
——	1825 =	+ 0 ·151	
Brinkley,	1824 =	+ 0 ·017	
Pond,	1819 =	+ 0 ·023	
——	1825 =	+ 0 ·351	
Pond,	1818 =	+ 0″·64	North Polar Distance.
——	1825 =	+ 1 ·64	

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The meetings of the Royal Society commenced for the present Session on the 16th of November; when the following business was transacted:

The President announced that his Majesty had presented to the Society the suite of apartments in Somerset House, lately occupied by the Commissioners of the Lottery: he also announced the resignation of Mr. Brande, as one of the Secretaries of the Society.

Lieut.-Col. D. Denham, Capt. W. H. Smyth, RN. and Nicholas Brown, Esq. were respectively admitted Fellows of the Society.

The Croonian Lecture, On the Generation of the Common Oyster, and the River Muscle; by Sir E. Home, Bart. VPRS. was read; and a paper was begun, On a Percussion Shell, to be fired from a common Gun; by Lieut.-Col. Millar: communicated by R. I. Murchison, Esq. FRS.

Nov. 23.—Charles Bell, Esq. was admitted a Fellow, and MM. Bouvard, Chevreul, and Dulong, were respectively *elected* Foreign Members of the Society; and the reading of Col. Millar's paper was concluded.

LINNÆAN SOCIETY.

The meetings of this Society were resumed on Nov. 7, when the reading of Dr. Hamilton's Commentary on the fourth part of the *Hortus Malabaricus* was continued.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Thirteenth Annual Report of the Council.

In performing this annual duty, the Council have again the pleasure of announcing the continued prosperity of this Institution. Its progress is indeed slow and unattended by any brilliant transactions; but it is silently and unostentatiously advancing in the acquisition of a geological knowledge of our county. More considerable and valuable additions to our cabinet of native minerals might, from time to time be made, and much useful information might be obtained by minutely exploring various interesting localities, but the limited state of the Society's funds opposes an insuperable obstacle to the speedy completion of these important undertakings. If therefore this unscientific, but real difficulty, be taken into consideration, the wonder will then be, not how little, but how much has been accomplished.

Another year has again elapsed, but the school of mines is not established: and there appears at present little probability of the then proposed plan being carried into execution. The Council regret its failure, since the hope of the speedy removal of a national reproach has been thereby postponed; but they will not yet despair of the eventual accomplishment of a measure, first suggested by this Institution, and which it has never ceased to recommend. This Society, enrolling amongst its members a great portion of the rank and wealth of the county, should be foremost in promoting an Institution of the first importance, both in an economical and scientific point of view; and for the establishment of which "*one and all*" should unite, who are interested in the honour and prosperity of Cornwall.

The Museum, which in the foremost place commands the Council's attention, has been enlarged by the addition of another Cabinet extending the whole length of one of the galleries: this has afforded sufficient space for the exhibition of several series of Foreign specimens, which have been arranged by the Curator with his usual neatness and judgment. The attainment of this object will afford but little interest to the scientific stranger; but to our native students it is of great importance, as displaying examples of the various and dissimilar mineral productions of our globe. To our constant and liberal correspondent William Maclure, Esq. of Philadelphia, we are indebted for another donation of minerals from the United States of America; consisting of various modifications of Serpentine, of Augite, Maclurite, Franklinite, Red Oxide of Zinc, and other interesting specimens. Doctor Jer. Van Rensselaer, of New York, has presented to the Society several earthy and metallic minerals, which are particularly specified in the Curator's Report of donations.

Captain Wallis, of Bodmin, has also presented a series of specimens illustrative of the geology of the country between Hydrabad and Madras, accompanied by a descriptive account of the same, together with a map of this part of the peninsula of India.

To the kind contributions of several members, in compliance with the request made at the last annual meeting, the geological department has been recently indebted for many specimens of granite; but the varieties of this interesting formation are so numerous, that many more would be equally acceptable.

The department of simple minerals has also been enriched by the purchase in Cumberland of a series of splendid minerals, consisting of variously crystallized galena, blende, fluor spar, sulphate of barytes, arragonite, and other substances.

Before concluding, the Council take this opportunity of soliciting further donations from those members who have private collections: many may have refrained from presenting their spare duplicates on the ground that our Museum already possesses better specimens of the same kind: these however would be very acceptable, and would enable the Society to comply with the repeated applications that have been made by institutions, both at home and abroad, for an exchange of minerals.

"Lastly, the Council propose for the consideration of the general meeting the propriety of expending a portion of their income in the collection of geological specimens: thereby promoting the Society by enlarging the knowledge of our rock-formations and collecting several pieces of the same rocks for the purpose of exchanges. Simple crystalline minerals cannot be obtained to any great extent, but at prices very far exceeding our funds, and indeed such Cornish minerals may be found in the collections of all extensive mineral dealers. Rock-specimens, however, are not to be met with in such places; but they can be collected with comparatively little expence; and a series of granite, slate, and serpentine rocks would be the most valuable return the Society could make in their exchanges with scientific institutions. Moreover such intercourse cannot fail of being highly advantageous to both parties, and may be instrumental in the advancement of the science of geology.

By order, H. S. Boase, Secretary.

October 13, 1826.

The following Papers have been read since the last Report:—

On the Granite of the West of Cornwall. By Joseph Carne, Esq. FRS. &c. Member of the Society.

On the Sand Banks of the Northern Shores of Mount's Bay. By H. S. Boase, MD. Secretary of the Society.

Some Account of certain Ancient Circles and Barrows on the Summit of Botrea Hill, in the Parish of Sancreed. By T. F. Barham, MD. Librarian of the Society.

On the Changes which appear to have taken place in the primitive Form of the Cornish Peninsula. By John Hawkins, Esq. FRS. &c. Honorary Member of the Society.

On the Temperature of Mines. By Henry Boase, Esq. Treasurer of the Society.

An Account of some Circumstances connected with the Heave of a Copper Lode by a Flucan Vein in the Consolidated Mines, in the Parish of Gwenap. By Mr. W. J. Henwood, Member of the Society.

On the Geology of the Coast from Sennen Cove to the Land's End. By Joseph Carne, Esq. FRS. &c.

On a singular Exudation of Gas in the Union Mines, in the Parish of Gwenap. By Mr. W. J. Henwood.

Observations on the Suspension of the Stannary Courts. By Henry Boase, Esq.

On the Importance of a Deep Adit from the Northern Coast. By R. Edmonds, Esq. Member of the Society.

A Notice relative to a new Fusee for the Blasting of Rocks. By R. Collins, Esq. Member of the Society.

An account of the Quantity of Tin produced in Cornwall in the Year ending with the Midsummer Quarter 1826. By Joseph Carne, Esq. FRS.

An account of the Produce of the Copper Mines of Cornwall, in Ore Copper, and Money, in the Year ending the 30th June, 1826. By Mr. Alfred Jenkyns.

Among the presents is the following :—

A metallic Pan and Cover, about 15 inches in diameter, found at the depth of 12 feet in an old Stream Work, near St Columb. The metal is very good tin nearly equal to grain. This vessel was probably used for culinary purposes, and at a period previous to the introduction in this country of the alloys of tin with copper and with lead. By H. S. Boase, MD.

At the Anniversary Meeting held on the 13th of Oct. 1826, Davies Gilbert, Esq. MP. VPRS. &c. President, in the Chair; the Report of the Council being read, it was resolved :—

That it be printed and circulated amongst the Members:

That the thanks of the Society be presented:

1. To the Authors of the various Papers.
2. To the Donors of Minerals, Books, &c.
3. To the Officers of the Society:

Lastly,—That another Volume of Transactions be forthwith published, and that the new Council be requested to take immediate measures for that purpose.

The following members were then elected Officers and Council for the year ensuing :—

President.—Davies Gilbert, Esq. MP. &c. &c.

Vice-Presidents.—Sir John St. Aubyn, Bart.; Sir Charles Lemon, Bart.; William T. Praed, Esq.; Rev. Uriah Tonkin.

Secretary.—Henry S. Boase, MD.

Treasurer.—Henry Boase, Esq.

Librarian.—T. F. Barham, MD.

Curator.—Edward C. Giddy, Esq.

Assistant Secretary.—Richard Moyle, Esq.

Council.—George S. Borlase, Esq.; Joseph Carne, Esq.; Stephen Davey, Esq.; Richard Edmonds, Esq.; William M. Tweedy, Esq.; Robert W. Fox, Esq.; George Grenfell, Esq.; Michael Williams, Esq.; Humphry Grylls, Esq.; George D. John, Esq.

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Method of purifying Crystals.* By M. Robinet.

Every practical chemist knows how difficult it often is, particularly in the analysis of organic substances, to clear away from crystalline products the mother water, and other heterogeneous matters, which collect in their interstices. When the crystals are very fine, and still more when they are soluble in the ordinary menstrua, it is sometimes impossible to clear them, although perfectly pure, by any other method than repeated crystallization and digestion with animal charcoal; both of which processes are troublesome, and occasion considerable loss. M. Robinet has proposed a new and very simple method, which was suggested to him, in consequence of observing, that when a parcel of crystals came into contact with the mouth of the *pipette* during the act of suction, they were instantly and perfectly cleaned. The process depends on the transmission of a current of air through the crystals. He has suggested various forms of apparatus for the purpose. The simplest consists of a double-mouthed bottle, with a funnel in one mouth, and a bent tube in the other; the lower opening of the funnel being obstructed by a ball of cotton-wool, and the crystals placed above the cotton. On sucking the air through the crystals by a bent tube, they are cleaned in a few seconds; and, if necessary, the operation may be repeated after previously introducing a little water into the funnel. A convenient way of constructing the apparatus so as to work of itself, is to make the second tube reach the bottom of the bottle with one limb, and with the other a vessel of water situated on a lower level. The whole bottle and tube being filled with water, the funnel is to be introduced, and the water then allowed to run off by the syphon. On the large scale a more suitable apparatus will be a tube from a steam-boiler, by which the bottle may be filled with steam from time to time.

The steam communication being shut off, and the steam in the bottle condensed, the stream of air will immediately carry through with it the whole of the mother-water from the most silky crystals.—(Journal de Chimie Medicale.)

2. *On the Decomposition of Cyanate of Silver, by Sulphuretted Hydrogen.* By Dr. Liebig.

M. Gay Lussac and I have already shown, observes Dr. Liebig, that sulphuretted hydrogen decomposes cyanate of silver; the cyanic acid is not, however, separated in a pure state; one part of it combines with the sulphuretted hydrogen, and peculiar compounds are formed which have in general the characters of acids. When cyanate of silver is decomposed by hydriodic and muriatic acid, hydrocyanic acid is evolved, and new acids are obtained which contain iodine and chlorine, and which have the property of giving deep red coloured solutions with the salts of peroxide of iron, after having been neutralized with a base. With sulphuretted hydrogen no hydrocyanic acid is obtained, and the new acid immediately produces a deep red coloured fluid with the persalts of iron, without previous neutralization with a base.

It was also shown in the memoir above alluded to that cyanic acid is composed of an atom of cyanogen, and one of oxygen, and it appeared to us that in the acid obtained with the cyanate of silver and sulphuretted hydrogen, that oxygen was exactly replaced by sulphur, and that we had obtained a compound of sulphur and cyanogen. Such are the results which have been stated in the memoir; sometime since I undertook some experiments to determine with more precision the nature of the acid obtained by decomposing cyanate of silver with sulphuretted hydrogen. The results of these observations are slightly different from those which we had previously obtained, but we have not paid minute attention to these several acids.

If sulphuretted hydrogen gas be passed through water, holding cyanate of silver in suspension, and if the liquor be strongly agitated before the cyanate is totally decomposed, an extremely penetrating smell is perceived, and ammonia, when exposed to the mixture, produces a white cloud. As soon as the salt is entirely decomposed, which happens when the fluid becomes clear, this smell is no longer perceptible. The liquid separated by the filter from the sulphuret of silver has an acrid taste, and reddens turnsol; when mixed with lime ammonia is expelled, it precipitates muriate of barytes after being heated with nitric acid, gives a bulky precipitate with nitrate of silver, and changes the persalts of iron to a deep red colour; it appears from this that the cyanic acid, decomposed by the sulphuretted, is converted into a cyanite (?) of ammonia, and a peculiar acid containing sulphur, but different from the sulpho-cyanic acid. The fluid, when left for some time exposed to the air, deposits a yellow powder, and then the smell of hydrocyanic acid is perceived; the solution by spontaneous evaporation yields a deliquescent salt, which, with acid, gives the penetrating smell of sulpho-cyanic acid.

As it is extremely probable that the formation of ammonia had been determined by the affinity of the acid, I made another experiment to decompose the cyanate of silver with sulphuret of barium, obtained by decomposing sulphate of barytes with lamp black; the sulphuret of barium was gradually added to the cyanate suspended in boiling water, as long as sulphuret of silver was formed; the filtered liquor was very alkaline, but it gave no sulphuretted hydrogen on the addition of an acid; nitrate of silver gave a yellow precipitate, which became black on drying. A current of carbonic acid gas passed through the solution, produced only a small quantity of carbonate, and by evaporation a yellow salt was procured, which heated to 100° cent. burnt without giving light, till it lost all its moisture, and it became grey: it was then treated with water, which dissolved a sulpho-cyanuret of barium, and carbonate of barytes remained: the acids separate sulpho-cyanic and carbonic acids, and lime evolves ammonia. When heated in glass tubes after being dried, it fuses, gives carbonate of ammonia and cyanogen, and sulphuret of barium remains.

When nitrate of silver is precipitated by the freshly prepared barytic salt, a bulky precipitate is obtained, which, after being well washed and heated in boiling water, is converted into sulphuret of silver, and produces carbonate of ammonia. It appears from this that the acid which is united with these oxides must contain oxygen, besides carbon, hydrogen, and azote.

When the barytic salt is decomposed with sulphuric acid, a fluid acid is obtained which very readily decomposes. If the salt be pure then no other product is obtained, but if it contain even a trace of silver, hydrocyanic acid is formed. If the liquid in which cyanate of silver has been decomposed by sulphuret of barium be filtered, before all the cyanate is decomposed, cyanate of silver and of barytes crystallizes upon cooling; this shows that the cyanate of silver loses half of its oxide, before the cyanic acid itself undergoes any change.

If the cyanic acid gives half of its oxygen to the sulphuret of barium, and takes in exchange an equivalent portion of sulphur, the new acid would be formed of two atoms of cyanogen, one of sulphur, and one of oxygen, and the salt of silver, when decomposing at a boiling heat, with six atoms of water would produce one atom of sulphuret of silver, four atoms of carbonic acid, and two of ammonia.

Although these results do not possess all the certainty that is desirable, they prove at any rate, that, during the decomposition of cyanate of silver by sulphuretted hydrogen, or sulphuret of barium, different products are formed from those which had been supposed, and that the red colour assumed by a solution of a persalt of iron, is not a sufficient proof of the existence of sulpho-cyanic acid, since there are several other substances, differing totally from this acid, which possess the same property.—(Ann. de Chimie et de Physique.)

3. *Analysis of Tymp Cinder.*

In the blast furnaces of the iron works at Merthyr Tydvil, a scoriaceous matter is produced below the opening of the pipes, which is rich in alkali, and is collected by the workmen, and is used instead of soap. This substance has been analysed by M. Berthier, who states that it consists of small scoriaceous particles, which are black and magnetic, and occasionally interspersed with grains of mamellated scoria; all these are enveloped in a very deliquescent alkaline substance; this matter, treated with water, gives:

Soluble salts	0·385
Insoluble matter	0·615
	1·000

The soluble salts were found to consist of

Carbonate of potash	0·63
Sulphate of potash	0·37
Silica	a trace
	1·00

There was no muriatic nor phosphoric acid. The insoluble matter gave by analysis:

Silica	0·343
Protoxide of iron	0·260
Alumina	0·040
Lime	0·052
Potash	0·205
Mamellated scoria	0·100
	1·000

The alkali is undoubtedly derived from the earthy matter, with which the carbonate of iron occurring with coal is always intimately mixed.—(Annales des Mines.)

4. *Spontaneously Inflammable Metallic Powders.*

M. Magnus has observed that when the pure oxides of iron, cobalt, or nickel, are reduced by hydrogen gas at temperatures but very little above that of boiling mercury, metals are obtained, which, when allowed to cool in the hydrogen gas, inflame spontaneously by exposure to the atmosphere. If the reduction has been effected at a red heat, this does not take place.

When the same oxides are mixed with a little alumine, the metals obtained as before inflame spontaneously in the atmosphere, even though the heat used has been that of redness, and yet from the quantity of oxygen disengaged, it has been evident that the alumine has not been de-oxidized.

When a metal, thus competent to inflame in the air, is heated in carbonic acid gas, it loses its peculiar property, but re-assumes it upon being re-heated in hydrogen gas, and allowed to cool as before.

Nevertheless, the hydrogen is not the cause of this inflammability; for when oxalate of iron is heated in a vessel with a narrow neck, so that the acid may be decomposed and the whole allowed to cool, the metallic iron-powder obtained inflames spontaneously in the atmosphere. No other metal but the three mentioned have presented this phenomena.

It results from these experiments, that when the difficulty-fusible metals are in a state of extreme division, and have not aggregated either from adhesion or softness, they have the property of inflaming in the air. This effect is probably due to a power possessed by these metals of condensing such quantity of oxygen on their extended surface, as to occasion the conditions necessary to the oxidation of the metal.

The inflammability of Homberg's pyrophorus, prepared by heating to redness a mixture of alum and flour, depends probably on the same cause; for the inflammation does not take place, except when the heat has been so moderated as to be insufficient for the fusion of the sulphuret of potassium.

These phenomena are analogous to those observed by M. Dobereiner, as belonging to platina, and with the faculty which silicium and zirconium have of oxidating under certain circumstances, as M. Berzelius has shown. Perhaps, also, they may assist in discovering the causes of the formation of nitric acid in artificial nitre-beds.—(Annales des Mines, xii. 210.)

With these effects should be ranged the remarkable one observed by Dr. Gobel, as produced by the residue left upon igniting the tartrate of lead in close vessels. See vol. xvi. p. 385, of this Journal.—Ed.—(Institution Journal.)

5. *Precipitation of a Metal from Solution by other Metals.*

Professor Fischer, of Breslau, remarks that the reduction of a metallic oxide from its solution in an acid or alkali by a metal, depends upon the following causes:—

1. Principally the relative affinity of the two metals for oxygen.

2. The affinity of the oxide of the reducing metal for the acid, or alkali:—for this reason tin, bismuth, and iron only reduce a small number of salts, whilst zinc reduces a great number. It is for the same reason that there are but few alkaline solutions reduced by metals: silver dissolved in ammonia is not reduced by zinc; copper dissolved in ammonia is not reduced by tin, antimony, bismuth, or iron.

3. The electric tension which may exist between the precipitating and precipitated metal.

4. The affinity of the metals for each other:—copper is precipitated by iron in a state of purity; precipitated by zinc, a kind of brass is formed.

5. The state of saturation and concentration of the solution:—the precipitation is more rapid as the fluid is more acid and concentrated. If the metal is required in fine dendritical crystals, the solution should be dilute. Many metallic salts, reducible by certain metals when dissolved in water, are not so when dissolved in alcohol.

6. The tendency of the precipitated metals to assume the granular or crystalline state:—lead, silver, and tin are precipitated from their solutions more readily than gold or platina.

7. The position of the reducing metal, relative to the solution:—when the metal is in plates or wires offering a large sur-

face, the precipitation is rapid; but when it is placed at the surface of the liquor, touching it only by a point, it requires a long time. Two equal quantities of solution of silver were taken: a plate of copper was plunged into one, and a bar of copper made merely to touch the surface of the other; the first was entirely freed from silver in one hour, the latter not in three months.

When a zinc rod is put into a solution of acetate, or nitrate of lead, the first large crystals of lead after a time fall off, and are replaced by smaller, which in turn fall, and are succeeded by others, and this alternate production continues a long time. The cause of this effect appears to be in the power which most metallic solutions have, when saturated with base, of dissolving small quantities of other metals: thus the saturated nitrate, muriate, and acetate of zinc slowly dissolve a notable quantity of lead; the muriates of zinc and tin and the acetate of lead dissolve a little copper, and the nitrate of copper dissolves finely-divided silver. When, therefore, a piece of zinc is placed at the surface of a solution of lead, the latter metal falling to the bottom of the fluid is in part dissolved by the salt of zinc formed; and the solution of lead thus formed spreading through the liquid, thus places the zinc in constant contact with it, and hence the successive precipitation of the lead which is found to take place.

If finely-divided silver is put at the bottom of a narrow tube, and about two inches in depth of a solution of copper saturated with oxide be poured upon it, then a copper-wire plunged to the depth of a couple of lines will soon become covered with silver, and in three or four weeks fine crystals of the metal will appear, which will be larger as the tube is narrower. It appears, that in all these circumstances double subsalts are formed; crystals of subnitrate of copper almost always appears mixed with those of silver.—Ann. des Mines, xii. 197.—(Institution Journal.)

Mineralogy.

6. *Bitberg Meteoric Iron.*

According to Stromeyer, it contains iron 81·8; nickel 11·9; cobalt 1·0; manganese 0·2; sulphur 5·1 = 100·0. Stromeyer did not examine it for chrome, but intended to do so.—(Edin. New Philos. Journ.)

Zoology.

7. *On Female Pheasants assuming the Male Plumage.*

The Editor of "The Edinburgh New Philosophical Journal," has annexed the following interesting note to a translation of M. Isidore Geoffroy Saint Hilaire's memoir "On Female Pheasants assuming the Male Plumage," published in the present number of that valuable journal.

"The interesting fact of female birds assuming the plumage of the male was in modern times first attended to by the celebrated I. Hunter, who, in a memoir on this subject in the Philosophical Transactions of London, describes a hen pheasant and pea hen which had in old age assumed the male plumage. Mr. G. St. Hilaire, in the preceding memoir, says, that of the many pea hens in the menagerie in Paris, no instance occurred of the pea hen assuming the male plumage, a fact which shows such a change is rarely met with in the peacock. In the Museum of this University, there is a fine specimen of the pea hen with the male plumage, presented to the Museum by the Duchess of Buccleugh. In the note accompanying the gift, it is said the change was effected during the course of a few years. The following description will convey an idea of the degree of change experienced in this individual. The head and neck have assumed the same green and blue tints which characterise the male; the breast and belly also have the same deep colour. As in the male, the primaries are pale-brown, and a patch upon the wing bright green. The dorsal feathers, however, are still more or less mottled with grey; and the green which they have partially assumed is lighter than in the male, and not blended with the coppery hue which in his plumage extends from the middle of the back to the rump. The rump feathers are elongated, some of them to the length of 18 inches, but the train formed by them is scanty, and the ocellar spots are neither so large nor so varied as in the male. The ordinary tubercles on the tarsi of the female have been developed into thick, regular conical spurs, about half the length of those of the male. In short, the change is so much advanced, that after another month, it would probably have been complete.

In the museum of the University, there is a specimen of the female pheasant with the male plumage, presented some years ago by Dr. Hope.* The only differences which the plumage of this individual exhibits, when contrasted with the male bird, are the following: first, the tail feathers are shorter than those of an adult male, although considerably longer than those of an ordinary female; secondly, the lustre of the colours in general is not quite so vivid as in the male, especially on the back of the wings. There is no appearance of spurs.

Sometimes the same sort of apparent change of sex is observed among domestic poultry. Mr. Neill, at Canonmills, had a black hen, of what is called the French breed, which in her twelfth year ceased to lay eggs, and gradually assumed somewhat the appearance, and, to a considerable degree, the manners of a cock. The principal change of plumage consisted

* In the British Museum are several similar specimens, particularly two remarkably fine ones, lately shot in Kent, by Thomas Law Hodges, Esq. of Hempsted Place, near Cranbrook, and by him presented to the Museum.—C.

in the tuft on the head becoming thinner, and showing some upright stray feathers, and in a single elongated feather projecting from the tail. The spurs were larger than usual in hens, but these had probably been increasing for some years. The change of manner of the bird was quite remarkable: she strutted about in an overbearing way, with a firm pace and raised tail. She formed a party among the fowls, which she led separate from the cock; and she roosted apart from him. She became very voracious; and when food was set down (losing all resemblance, in this instance, to the generous male), she beat off the other hens: when, in these cases, she came in contact with the cock, she stared at him, but without making any attack. She soon became very fat, and died within a few months seemingly of over fatness. Her cry was altered, but had little resemblance to the crowing of the cock; less indeed than is sometimes noticed in young hens.

In a valuable paper, by Dr. Butler, of Plymouth, in the third volume of the Memoirs of the Wernerian Society, there are many interesting facts on this subject, and from which we extract the following table:

Table of such Birds as have, in advanced Life, assumed the Plumage of the Male, with the Names of those Authors who have noticed the Fact.

Order 4.—Gallinæ.—Domestic Birds.

Gen. 1. Pavo, Pea-hen	Hunter.
2. Meleagris, Turkey ...	Bechstein.
3. Phasianus colchicus ..	Pheasant common: Hunter.
——— pictus	——— golden: Blumenbach.
——— gallus ..	Fowl domestic: Aristotle, Tucker, Butler.
4. Tetrao Perdix, Partridge	Montagu.
5. Columba, Pigeon	Tiedemann.

Order 5.—Grallæ.—Waders.

2. Family Pressirostres	*Gen.* 1. Otis Busand. Tiedemann.
3. ——— Cultrirostres	3 Tribe, Gen. 4. Platalea, Pelican of American: Catesby.

Order 6.—Palmipeda.—Web-footed.

2. Family, Lamellirostres, soft skin on the beak.
1. Anas, Duck, common and wild: Tiedemann.

Miscellaneous.

8. *Remarkable Rainbow.*

On the 18th May of this year (1826), at six o'clock, p.m. lightning appeared in the eastern part of the heavens, and a little rain fell. There, where it was darker, I, and many of the inhabitants of Lengsfeldst, in Eisenach, observed a very remarkable rainbow. We saw not only, as is commonly the case, the feebly coloured interior rainbow, and the darker coloured exterior rainbow, with all their transition of colours, but among these also the following threefold repetition of them in the following order: Most exterior rainbow, violet, blue, green, yellow, and red; under a dark layer, and below those with diminished intensity of colour, first the common interior bow, with red, orange, yellow, green, blue, violet; then the following three; purple, orange, green, violet; purple, orange, green, violet; purple, orange, and finally, a dull green arched stripe. *Karsten Archiv.*—(Edin. New Phil. Journ.)

9. *The Moon and its Inhabitants.*

Olbers considers it as very probable that the moon is inhabited by rational creatures, and that its surface is more or less covered with a vegetation not very dissimilar to that of our own earth. Gruithuisen maintains that he has discovered, by means of his telescope, great artificial works in the moon, erected by the Lunarians; and very lately another observer maintains, from actual observation, that great edifices do exist in the moon. Noggerath, the geologist, does not deny the accuracy of the descriptions published by Gruithuisen, but maintains that all these appearances are owing to vast whin dykes, or trap veins, rising above the general lunar surface.

Gruithuisen, in a conversation with the great astronomer Gauss, after describing the regular figures he had discovered in the moon, spoke of the possibility of a correspondence with the inhabitants of the moon. He brought, he says, to Gauss's recollection the idea he had communicated many years ago to Zimmerman. Gauss answered, that the plan of erecting a geometrical figure on the plains of Siberia corresponded with his opinion, because, according to his view, a correspondence with the inhabitants of the moon could only be begun by means of such mathematical contemplations and ideas, which we and they must have in common. The vast circular hollows in the moon have been by some considered as evidences of volcanic action, but they differ so much in form and structure from volcanic craters, that many are now of opinion, and with reason, that they are vast circular valleys.—(Edin. New Phil. Journ.)

Is the preceding extraordinary piece of *Scientific Intelligence* (under which head it appears as above) a quiz, or are Messrs. *Gruithuisen, another Observer,* and *Noggerath,* downright lunatics? As to the alleged conversation between MM. Gruithuisen and Gauss, the latter must, we conclude, have intended to laugh in his sleeve at the strange speculations of the former, whilst he seemed to enter into his wild, extravagant views.—*Ed.*

10. *Transmission of Sound.*

The extreme facility with which sounds are heard at a considerable distance in severely cold weather, has often been a subject of remark; but a circumstance occurred at Port Bowen, which deserves to be noticed, as affording a sort of measure of this facility. Lieutenant Foster having occasion to send a man from the Observatory to the opposite shore of the harbour, a measured distance of 6696 feet, or about one mile and two-tenths, in order to fix a meridian mark, had placed a person half-way between to repeat his directions; but he found on trial that this precaution was unnecessary, as he could, without difficulty, keep up a conversation with the man at the distant station.—(Parry's Voyage.)

11. *Unprecedented Cold.*

Plattsburgh, Feb. 22, 1826.—On Tuesday and Wednesday of last week, was the coldest weather probably ever experienced in the United States. We did not ascertain how low the thermometer sunk in this place; but at Fort Covington, fourteen miles distant, a thermometer sunk to 40° below zero, and the mercury froze! How much lower an alcohol thermometer would have sunk is not known; probably, however, not more than one or two degrees, as mercury exposed at the same time was a long time in congealing. A degree of cold sufficient to freeze mercury was never before noticed in the United States, and probably never in so low a latitude as 45°. The coldest weather that we recollect to have heard of in this country was 32° below zero.—(Intelligencer.)

12. *The Heat of July,* 1825.

The heat of July, 1825, seems to have been as oppressive in England and France as in this country, and to have been attended in some instances with the same fatal effects, as a number of sudden deaths are mentioned in the papers. The thermometer stood at Bath on the 19th, in the shade, at 89°; and the number of horses that had died is supposed to be greater than at any former period. The effects of continued hot weather were seriously felt. Brooks and ponds were become quite dry, and vegetation was suffering from the scorching heat of the sun. The weather in Paris was most intensely hot, and such a season has scarcely ever been remembered there. Nearly a period of twelve weeks elapsed without a single drop of rain, and the papers represent the country as absolutely burnt up. The thermometer of Fahrenheit was daily as high as 90°, even in cool parts of the city, and was in many places between 90° and 100° throughout the day. The waters of the river Seine were extremely low indeed.—(American Journal of Science.)

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Early in December will be published the Zoological Journal, No. IX. consisting of papers in various departments of Zoology, with some Account of the Life, and Writings, and Contributions to Science, of the late Sir T. S. Raffles, Knt. FRS. &c. President of the Zoological Society.

Mr. Faraday has in the press an octavo volume, entitled, "Chemical Manipulation," containing Instructions to Students in Chemistry relative to the Methods of performing Experiments.

Mathematical and Astronomical Tables, for the Use of Students in Mathematics, Surveyors, Engineers, Navigators, &c.: by W. Galbraith, MA.

JUST PUBLISHED.

Geological and Historical Observations on the Eastern Valleys of Norfolk; by J. G. Robberds, jun. 8vo. 4*s.*

Dewhurst's Dictionary of Anatomy. 8vo. Part I. 5*s.* 6*d.*

Hamilton's Outlines of Midwifery. 8vo. 7*s.* 6*d.*

Burrow's Conchology. 8vo. 16*s.* plain, or 1*l.* 11*s.* 6*d.* coloured.

Lamarck's Conchology illustrated. Part IV. 1*l.* 11*s.* 6*d.* plain, or 3*l.* 3*s.* coloured.

Illustrations in Ornithology; by Sir W. Jardine, Bart. FRSE. &c. and Prideaux John Selby, FLS. &c. Part I.

Daubeny's Description of Volcanos. 8vo. 16*s.*

Phillips's Outlines of Mineralogy and Geology. 8vo. 8*s.* 6*d.*

Memoirs of the London Astronomical Society, Vol. II. Part II. 30*s.*

Article XV.

NEW PATENTS.

B. Newmarch, Cheltenham, for improvements on fire-arms.—Nov. 7.

E. Thomason, Birmingham, goldsmith and silversmith, for improvements in the construction of medals, tokens, and coins.—Nov. 9.

H. C. Lacy, Manchester, coach-master, for an apparatus on which to suspend carriage bodies.—Nov. 18.

B. Woodcroft, Manchester, silk manufacturer, for his improvements in wheels and paddles for propelling boats and vessels.—Nov. 18.

Article XVI.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
Oct. 23	29·62	29·60	29·610	63	57	60·0		SW	Clear.
24	29·64	29·62	29·630	64	53	58·5		SW	Showers: rain at nt.
25	29·30	29·28	29·290	56	52	54·0		W	Showers.
26	29·50	29·43	29·465	60	50	55·0		NW	Showers.
27	29·55	29·52	29·535	60	50	55·0		NW	Showers.
28	29·98	29·98	29·980	56	50	53·0	0·60	NW	Fair.
29	29·98	29·97	29·975	60	50	55·0		NW	Fair.
30	29·96	29·90	29·930	58	53	55·5		W	Misty rain.
31	29·98	29·96	29·970	55	48	51·5		NW	Fair; showers.
Nov. 1	29·71	29·70	29·705	50	57	53·5		NW	Showers.
2	29·78	29·74	29·760	52	47	49·5		N	Fair.
3	29·76	29·76	29·760	55	44	49·5		NE	Fair.
4	29·76	29·68	29·720	54	48	51·0		NE	Fair; showers.
5	29·71	29·70	29·705	54	48	51·0		NE	Fair; showers.
6	29·72	29·70	29·710	54	48	51·0		N	Fair; showers.
7	29·92	29·80	29·860	48	36	42·0		N	Showers.
8	29·96	29·94	29·950	51	36	43·5		NW	Fair.
9	29·99	29·98	29·985	52	38	45·0		NW	Fair; showers.
10	30·00	29·98	29·990	54	38	46·0		NW	Fair; showers.
11	29·70	29·68	29·690	54	40	47·0		W	Rain.
12	29·49	29·41	29·450	54	47	50·5	0·15	W	Showers.
13	29·10	28·70	28·900	55	46	50·5		SW	Rain.
14	29·50	29·10	29·300	52	44	48·0		N	Hail showers.
15	29·70	29·62	29·660	52	46	49·0	0·20	NW	Showers.
16	29·68	29·66	29·670	51	38	44·5		SW	Rain.
17	29·74	29·73	29·735	50	44	47·0		W	Showers.
18	29·92	29·91	29·915	50	38	44·0	0·08	W	Showers.
19	30·00	29·92	29·960	50	42	46·0		NE	Clear; fair.
20	30·15	30·14	30·145	49	43	46·0		NE	Fair.
21	30·22	30·20	30·210	48	43	45·5		NE	Fair.
22	30·20	30·15	30·175	48	38	43·0		NE	Fair.
	30·22	28·70	29·753	64	36	50·0	1·130	NW	

RESULTS.

Barometer, mean height .... 29·753
Register Thermometer, ditto .... 50·0°
Rain, No. 1, 4·130, No. 2, 3·020.
Prevailing wind, NW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, Nov. 24, 1826. EDWARD C. GIDDY.

Article XVII.

METEOROLOGICAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
10th Mon.							
Oct. 1	Var.	30·09	29·96	68	39	—	04
2	E	30·09	30·04	68	45	—	
3	W	30·04	29·93	62	45	—	
4	N W	29·95	29·93	58	35	—	
5	N W	30·27	29·95	55	29	—	
6	S W	30·27	30·26	55	28	—	
7	S W	30·27	30·08	64	44	—	—
8	S W	30·07	29·97	63	50	—	16
9	W	29·97	29·86	57	43	—	27
10	S W	30·18	29·86	65	55	—	07
11	S W	30·23	30·18	66	59	—	
12	S W	30·23	30·17	66	—	—	
13	S W	30·20	29·95	—	—	—	
14	S	29·95	29·63	—	—	—	
15	S E	30·17	29·78	67	40	—	
16	S E	30·14	30·79	67	34	·90	17
17	S E	30·14	30·11	61	42	—	
18	E	30·13	30·11	62	57	—	—
19	S E	30·13	30·11	62	58	—	
20	S E	30·11	30·10	62	55	—	
21	S E	30·10	30·09	71	51	—	
22	S E	30·09	30·05	65	53	—	53
23	S E	30·07	30·05	65	50	—	20
24	S W	30·07	29·60	62	53	—	29
25	S W	29·60	29·60	59	42	—	05
26	N W	29·77	29·60	51	37	—	—
27	S W	30·25	29·77	54	40	—	25
28	N W	30·32	30·25	54	41	—	
29	N W	30·32	30·28	53	49	—	02
30	S E	30·28	30·24	55	47	—	
31	N W	30·24	29·94	53	40	·65	
		30·32	29·60	71	28	1·55	2·05

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

REMARKS.

Tenth Month.—1. A heavy shower about two, p. m. 2—5. Fine. 6. Foggy morning: day fine. 7. Day fine: rain at night. 8. Cloudy: rain at night. 9. Cloudy and fine: night rainy. 10. Morning rainy: afternoon cloudy. 11. Cloudy. 12. Cloudy. 14, 15. Fine, 16. A heavy shower of rain, with thunder, about two p.m. 17—19. Cloudy. 20, 21. Fine. 22. Rainy. 23. A thunder storm about one, p. m.: lightning in the evening. 24. Fine day: rain at night. 25. Cloudy and fine. 26. Fine. 27. Rainy. 28. Cloudy. 29, 30. Cloudy. 31. Fine.

RESULTS.

Winds: E, 2; S, 1; SE, 9; SW, 10; W, 2; NW, 6; Var. 1.

Barometer: Mean height

For the month.. 30·064 inches.

Thermometer: Mean height

For the month.. 53·105°

Evaporation.. 1·55 in.

Rain.. 2·05

Laboratory, Stratford, Eleventh Month, 24, 1826. R. HOWARD.

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H.

I.

K.

L.

END OF VOL. XII.

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