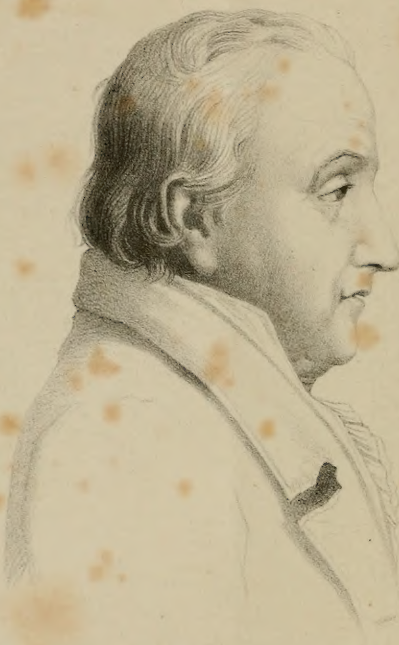

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Editor of the "Astronomical Correspondence."

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AND

RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1836.

I. *Facts relating to Optical Science. No.* III. *By* H. F. Talbot, *Esq., F.R.S.**

Optical Properties of the Iodide of Mercury.

CHEMICAL writers have observed that this substance exhibits remarkable changes of colour. It is orange-red when cold, but becomes yellow when moderately heated. As it grows cold again, the red tint reappears.

Sometimes, however, the yellow exhibits more permanency, as has been remarked by Dr. Inglis in his Essay on Iodine†. He says "that the yellow crystals of the biniodide retain that colour *for a considerable time*, unless suddenly cooled or agitated, when the characteristic crimson tint of the biniodide again appears."

Wishing to examine into the cause of these facts, I placed a small portion of the red iodide between two plates of glass, and warmed it over a spirit-lamp. It immediately sublimed into a yellow powder composed of minute crystals. As it cooled, blood-red spots appeared upon the surface of the yellow mass and gradually spread themselves over the whole, with the exception of some portions around the circumference which usually remained yellow. When examined with a microscope this powder presented the curious appearance of

* Communicated by the Author. Nos. I. and II. of these "Facts" will be found in Lond. and Edinb. Phil. Mag., vol. iv. pp. 112, 289.

† See the Number for January last, (vol. viii.) p. 18.

orange-red crystals lying interspersed among yellow ones, which resembled them in size and shape, and were different in no respect but in colour.

Upon repeating this experiment I found that by continuing the heat a little longer I could obtain much larger crystals, such that the field of view of the microscope would only contain a few of them at a time; and with these it became possible to see the phænomenon much more distinctly and advantageously. These large crystals have the shape of thin flat lozenges or oblique-angled parallelograms of a pale yellow colour. They are very transparent, act strongly upon light, and form a very pleasing and convenient object for the polarizing microscope. But the most important and singular phænomenon which they exhibit, is the sudden change of colour which they are capable of undergoing, and to which I do not think the science of optics has hitherto furnished any parallel. The change takes place in some of the crystals during the process of cooling, in others shortly afterwards, while in others the yellow tint remains permanent for many hours, or even days. In general, a crystal which is about to change colour, is known by the appearance of a red streak along one of its sides or edges.

If then the observer selects one of these and fixes his attention upon it, he will shortly afterwards see it change colour from pale yellow to a fine and deep orange-red. The change generally occupies only a few seconds, and the red tint advances uniformly across the crystal, *i.e.* the boundary of the red and yellow is a straight line *parallel to two sides of the rhomboid*, and its motion is across the crystal from one of these sides to the opposite one.

The change of colour is accompanied by a visible internal motion in the crystal, like a sinking or giving way of successive ranks of particles, one consequence of which is, that the crystal after the change is generally less transparent than it was before. This phænomenon is, I think, the most evident proof which we yet possess of the dependency of colour upon internal molecular arrangement.

As this substance sublimes very readily, I tried what might be the effect of placing a hot piece of glass over the crystals while they were under examination with the microscope. Immediately each crystal became surrounded with a cloud of little particles which appeared to me to be rhomboids like the larger ones from which they were derived.

The cracking of the larger crystals from heat must be an effect of unequal expansion, and it appears not unlikely from this experiment that the sublimation of the substance is produced by this cause acting more energetically at a high temperature.

On Prismatic Spectra.

It is much to be desired that an extensive course of experiments should be made on the spectra of chemical flames, accompanied with accurate measurements of the relative position of the bright and dark lines, or *maxima* and *minima* of light which are generally seen in them. The definite rays emitted by certain substances, as, for example, the yellow rays of the salts of soda, possess a fixed and invariable character, which is analogous in some measure to the fixed proportion in which all bodies combine, according to the atomic theory. It may be expected, therefore, that optical researches, carefully conducted, may throw some additional light upon chemistry. Some experiments which I formerly made upon this subject will be found in Brewster's Journal for 1826*. In addition to the substances there enumerated as giving a peculiar optical character to flame, I have found that the salts of copper are exceedingly remarkable. They give spectra so covered throughout with dark lines as to resemble in that respect the solar spectrum. The flames of boracic acid and nitrate of barytes also possess somewhat of a similar character. The most convenient way of obtaining brilliant spectra of these substances is to deflagrate them with chlorate of potash, but this is attended with the inconvenience that the spectrum produced by the chlorate is seen in conjunction with the other, and an allowance must be made accordingly. Another good method is to sprinkle the substance in powder on the wick of a spirit-lamp, and direct a current of oxygen upon it. With regard to the *accurate* measurement of the lines, it requires the use of very superior apparatus. I have sometimes made approximate measurements by fixing a divided scale transversely to the linear aperture through which the light of the burning body was observed. This aperture was then expanded by the prism into a spectrum parallel to the scale, by means of which it could then be measured. An objection may, perhaps, occur to the reader, that the scale would thus be as much refracted as the light itself, and therefore could not serve to measure it. But this difficulty was avoided by a simple contrivance, viz. by illuminating the scale with homogeneous light.

Spectra of various Galvanic Flames.

Silver leaf deflagrated by galvanism gave a spectrum with

* Vol. v. p. 77. See also the present Journal, vol. iv. p. 114.

several definite rays, among which two green rays appeared to me to possess nearly the same tint, although differing in refrangibility.

Gold-leaf, and copper-leaf each afforded a fine spectrum exhibiting peculiar definite rays. The effect of zinc was still more interesting; I observed in this instance a strong red ray, three blue rays, besides several more of other colours. These experiments were made in the laboratory of the Royal Institution in June 1834.

Errata. In the memoir on Light, vol. v. p. 326, line 22, *for* "jointed" *read* "joined."

P. 328, line 3 from the bottom, *for* "observations" *read* "obscurations".

The conclusion of the article in vol. iv. p. 290, is inaccurately expressed; it should stand thus: "But when the obliquity reaches a certain point one of the images suffers total internal reflexion before the other does. This would equally happen whether the balsam were employed or not: but its use is attended with advantage because this effect then takes place at a much greater angle of obliquity, and consequently the separation of the images is more perfectly and conveniently obtained."

II. *Remarks on* Mr. Hopkins's "*Researches in Physical Geology.*" *By* Henry S. Boase, *M.D., &c., Secretary to the Royal Geological Society of Cornwall.**

THE vast accumulation of facts during the present century concerning the structure of the earth, has established the title of geology to rank among the more important branches of human knowledge: and the attempts which have been lately made to solve geological problems by mathematical analysis, lead us to expect that it may one day attain to a more exalted position among the sciences. In Germany, this application was made many years ago, more particularly by Schmidt and Zimmermann, in illustration of the phænomena of veins. And in France, Elie de Beaumont and Dufrenoy have called in its aid to support the doctrine of elevation-craters. But to Mr. Hopkins we are greatly indebted for vindicating the character of the science in this country by making a more extended application of this agent, and for the important results at which he has arrived†.

* Communicated by the Author.

† Mr. Hopkins's Abstract of his Memoir on Physical Geology appeared in our last volume, p. 227, *et seq.*

I am not duly qualified to offer an opinion concerning the mathematical portion of Mr. Hopkins's work: but, admitting the great merit of the application of his *propositions* in pointing out what state of things is, or is not, compatible with the action of an elevatory force, I would venture to offer some remarks respecting the accuracy of the data on which his reasoning is founded. And I the more readily enter on this examination, as he has stated that his "speculations are thrown out with the hope of indicating some of the more critical points of inquiry on which the ultimate determination of this question must turn, and which are generally best indicated in such cases by theoretical discussion."

I have, in my "Treatise on Primary Geology," expressed an opinion that many supposed cases of elevation may be explained by the lines of structure, and by depositions on uneven surfaces; but waiving this at present, I will at once proceed to consider the nature of the elevatory force and its modes of action, adopted by Mr. Hopkins as the basis of his investigations.

The elevatory force which acted upon the lower surface of the uplifted mass is supposed to have been some expansive fluid. Now until the mass be ruptured this may be a satisfactory cause; but is it not probable that immediately the resistance has been overcome by the production of fissures, an explosion would follow, which would be as much more terrific in its devastations than modern volcanos, as the vent, extending across whole continents, would vastly exceed the dimensions of a crater? Can the present state of things—strata dipping on either side of mountain ranges traversed and intersected by regular systems of veins and dykes—be supposed to have resulted from such tremendous convulsions? And again, the internal pressure being removed by the rupture of the elevated mass, and the support consequently withdrawn, ought not the strata to have fallen down and have obliterated the spaces in which the veins and ranges of granite are said to have been subsequently formed?

This objection might be, in some measure, got over by supposing the uplifted and fissured mass to have been acted on whilst a solid layer was interposed between it and the moving power, whereby the fractured strata would be supported; but such an expedient, even if admissible, would only be an imaginary solution of the difficulty, and would be at variance with Mr. Hopkins's deduction that the fissures must necessarily commence in the *under* part of the mass.

Granting, however, the probability of this *modus operandi*, and the accuracy of the *first approximation*, we will proceed to the modifying circumstances, the possible existence of which Mr. Hopkins has admitted, and stated that they would render it very difficult to calculate with any *precision* the resulting phænomena. One of the most influential of these circumstances is the *jointed structure* in the mass subjected to the action of the elevatory power. Mr. Hopkins, duly impressed with its importance, has made many interesting remarks thereon, more particularly concerning the coincidence of lines of structure and of fissure.

The structure of rocks is a subject which has engaged my attention for many years; and in the seventh volume of this Journal (p. 376 *et seq.*) I endeavoured to show that the joints have not been mechanically produced, as Prof. Sedgwick supposes, but have resulted from an arrangement of the particles by the attraction of cohesion during the process of consolidation, being in its nature similar to crystallization. The Professor has, indeed, referred to Mr. Hopkins's "Researches" in support of his views, but it will be seen by the following passage that Mr. Hopkins, on *physical* grounds, has arrived at the same conclusion as myself. "It has been shown that extraneous forces could only tend to produce systems of fissures crossing each other at right angles, whereas regular systems of joints appear to meet each other frequently at acute angles, and consequently must necessarily have been owing to some different cause. I do not therefore conceive that any general tension of the mass produced by extension from elevation, or contraction in the course of solidification, can have had any material effect on the formation of joints. It is probably, I think, to be referred entirely to some kind of internal molecular action."

That the lines of structure are perfectly independent of any elevatory power would seem to be implied by their occurring even inclined in opposite directions entirely within horizontal strata; and also by their existence in solid rocks between beds of incoherent substances, as, for instance, in the oolite of Buckland Point, in the parish of Mells, which Mr. Townsend has described as rhombohedral beds dipping at an angle of 40°, and confined between two horizontal beds of clay*.

If then solid rocks have necessarily a jointed structure, one of the data on which Mr. Hopkins's calculations are founded is invalidated, in as much as the elevatory force can never have acted on a solid mass without the interference of this modifying circumstance. The abstract consideration of the question I can easily conceive to be required in order to ar-

* Greenough's Geological Essays, p. 13.

rive at a just estimation of the individual value of the moving power; but it would seem to be almost impracticable to calculate the result of such an action on a mass traversed by joints at *acute* angles to each other,—a condition found to be very prevalent in existing rocks. I am aware that it is not generally admitted that *all solid rocks have a jointed structure*; but Mr. Hopkins ought not to object to it, as he regards such structure to have proceeded from an "internal molecular action."

Now, if rocks having a jointed structure be acted on by an expansive fluid so as to overcome their continuity, it might be expected, as I have elsewhere stated, that the dislocations would take place on the lines of structure, as being the directions of least resistance. Mr. Hopkins also adds an excellent practical remark, "that the accuracy of coincidence, between the lines of structure and of fissure, is essential to the theory which would assign the latter phænomena to the prior existence of the former. A difference of a few degrees in the angular position of the above lines would, if clearly established, be fatal to this theory: because, as I have already explained, although a fissure produced by an elevatory force would cross a line of less resistance under a certain condition without change of direction, that condition cannot be generally satisfied when the angle between the fissure and line of less resistance is small; in such a case the fissure will be propagated exactly along the latter line." Such an occurrence, it appears to me, might rather suggest a doubt concerning the supposed action of an elevatory force, since our daily experience shows that all solid rocks do now possess a jointed structure. Mr. Hopkins, however, is of opinion that should this coincidence be established, it is more probable that the lines of structure have been influenced by the dislocation of the mass than *vice versâ*. This result of the mathematical analysis seems to have reduced the argument in favour of an elevatory force *ad absurdum*; for undisturbed horizontal strata have a jointed structure, and in rocks having a rhomboidal structure the joints meet at acute angles, which Mr. Hopkins himself states must necessarily have been owing to some different cause than that of extraneous forces, such as extension from elevation or contraction during solidification.

Mr. Hopkins has visited Cornwall, and found it, like others, a stumbling-block. He does not view its perplexing phænomena as indicative of a defect in the theory of geology, but regards them as exceptions to the general rule. "In this mining district," he says, "it appears most necessary to recognise the influence of a previously veined or jointed structure on the direction of its dislocations." And yet two or three pages afterwards we find a different opinion, viz. that the great system of metalliferous veins was formed in open joints *superinduced* after the great dislocations which accompanied the injection of the granite. I am glad to find that his "hasty inspection" of Cornwall has confirmed my more lengthened observation on the coincidence of the directions of veins with those of joints; but he surely jumps too hastily to the conclusion that this circumstance destroys the *contemporaneous hypothesis* which I have advocated. Although the direction of veins may correspond with lines of structure, they are not identical therewith. The joints pass indiscriminately through both veins and rock, whether granite or slate, or both conjointly; so that individual concretions or blocks, formed by the intersection of parallel systems of joints, contain more or less of these, according as the vein contracts or expands in its dimensions:—just as the lines of structure in sedimentary rocks pass through concretions and organic remains, so do they traverse the granite, slate, and veins, showing in both cases that the things intersected must have existed previous to the consolidation of the mass.

Mr. Hopkins regards the contemporaneous formation as "an inconceivable process," more especially for those who consider the slate as a sedimentary rock. Such a notion, however, is not perhaps so absurd as it at first sight appears to be, but is consistent with the prevailing theory; for if the primary slates be metamorphic rocks, why not the granite also? Vast districts in the North of Europe and elsewhere consist of gneiss, which some have called granite, and others granitic gneiss. Why was it not as easy for the central fire to change fossiliferous strata into granite, as into granitic gneiss? And if so, then the primary slates and granite might be contemporaneous; though in this case formed by a superinduced or secondary action, whilst in the one which I advocate they have resulted from the original fusion of the earth. Mr. Hopkins acknowledges that "the perfect continuity of the veins of Cornwall in passing from the killas to the granite forms a curious feature in the geology of that district if we are to regard the former as a sedimentary deposit." It certainly places the prevailing theory in a most perplexing dilemma, which however it appears to me is satisfactorily solved by admitting the contemporaneous nature of the granite and slate.

As the metalliferous veins of Cornwall do not seem to be referrible to the same cause as those of Flintshire and Derbyshire, Mr. Hopkins has sought for some other mode of obtaining fissures in which they might be formed; and he thinks that such may be found in the structural joints of the granite and slate produced since the elevatory injection of the granite. But before admitting this, let us inquire whether such a formation in joints would be analogous to the metalliferous veins of Cornwall. Existing joints removed from atmospheric agency are merely linear*, whilst the veins are of all dimensions from a line to even thirty feet in thickness. How few joints comparatively have veins; and how frequently both the rock and vein possess structural joints in common! Again, what proof is there that the Cornish slate was not constituted like all other solid rocks which we now find endowed with joints, whether horizontal or inclined? Even Mr. Hopkins admits that these joints have arisen from molecular action, and are consequently independent of the origin or the position of the unconsolidated rock.

Before concluding I embrace this opportunity, as immediately connected with the foregoing subject, to notice a passage in Mr. Lyell's address to the Geological Society concerning my comments on Professor Sedgwick's paper on the structure of rocks. He observes that I consider certain passages in this paper inconsistent with each other, and he confesses that at first they struck him in the same light, but that the Professor has explained the apparent inconsistency. Now the tendency of this remark of the President of the Geological Society is to imply that I am equally mistaken, and though this is only one of many points which I have discussed, yet all may fall under the same imputation. The haste consequent on preparing for an annual meeting may account for the oversight, but the fact is, that the Professor's reply to Mr. Lyell is no *explanation*, but merely a *repetition* of the statement which is as clearly expressed in the paper itself, and which I have endeavoured to show cannot be maintained. This being so, and the examples brought forward by the President being also opposed to the Professor's hypothesis, perhaps he may be induced to give the case a more careful consideration.

* Since writing the above I have learnt that Mr. Robert Were Fox has advanced a new opinion concerning the formation of veins—on the supposition of a gradual contraction of the mass producing a progressive enlargement of joints.

III. *Remarks on Microscopic Chemistry. By the Rev.* Edward Craig, *M.A., F.R.S.E.**

IT has been suggested to me that a short notice of some modes which I have adopted for examining under the microscope the phænomena attendant on chemical action, might tend in some degree to facilitate the researches of other and abler men in this department of inquiry. Not that the subject can be regarded as altogether new: after the lengthened and accurate observations of Leeuwenhoeck, Hooke, and others, on minute substances, it is impossible that the particular point to which my attention has been turned can altogether have escaped notice; yet the microscopic investigation of chemical agents in a state of reaction has certainly not occupied attention as much as it seems to merit.

M. Raspail published recently in Paris, a work which he entitles *Nouvel Système de Chimie organique, fondé sur des méthodes nouvelles d'Observation.* I have met with this work since the origination of my own modes of observation; and it fully justifies the idea, that notwithstanding the long time that the microscope has been in use, it has never been effectively applied to the object in consideration.

M. Raspail's work is a detailed account of the examination of organic structures; and the mode of observation for this purpose on which he lays the most stress, as having a claim to novelty, is the microscopic investigation of the visible effect produced on such structures by chemical agents. "I carry," he says, "the laboratory of the chemist on to the port-objects of the microscope." His very elaborate and valuable treatise closes with a note to the appendix, vindicating the novelty and originality of his modes of observation.

My own observations, conducted by one so much a novice in matters of scientific inquiry, are in themselves necessarily unimportant; they have, however, been carried on independently of any knowledge of M. Raspail's work; and they have a different object, in as much as M. Raspail confined himself to means for observing a particular class of phænomena. The particular object to which my attention was turned was the adoption of arrangements by which chemical action generally in the minutest visible quantities of substances might be examined.

Some months ago I was led by the statement of Dr. Brown, the eminent scientific botanist, to examine the motion of small molecules of matter floating in water, and excluded from the

* Read before the Royal Society of Edinburgh in December 1834; and now communicated by the Author.

pressure of the atmosphere: this suggested a further inspection into the form and character of the smallest discoverable particles of any pure chemical precipitates, many of which I found to be circular in the form of their molecular particles. From this the step was made naturally and almost necessarily to the observation of chemical action. The plan adopted for placing very small particles of matter in a floating state of thin film under the microscope, presented also an ample means for submitting to equally accurate scrutiny equally small portions of substances in a state of reaction: a few sentences will detail the plan. The phænomena may in a few cases be described, and then the interminable field of such inquiry will be fully open to any one who delights in the novelty of new modes of scientific research. The veriest tyro and the most advanced chemist will be alike interested in the wonders that open to his view.

The microscope which I use was made by Chevallier of Paris, and is peculiarly well adapted for the purpose. The port-object is a broad brass tablet perforated in the middle to receive reflected light from below, and fitted also with a powerful convex lens to throw light from above. It is steady and firm, which is very desirable. The whole additional apparatus necessary is a number of small plates of thin and very flat glass, filed at the edges to prevent them from cutting the finger, and a few glass rods with small rounded ends for taking up a drop of liquid. The glasses should be free from flaws and fitted to lie closely with even pressure upon one another.

When I wish to examine the action of two substances I lay a plate of glass on the port-object, and put on it a very minute portion of one substance; I adjust this to the focus of the lens in use, and ascertain the form and character of the substance in a quiescent state. I then lengthen the distance of the object-glass from the object a little to prepare for the subsequent observation; for I find that in the use of high-powered lenses the introduction of a second plate of glass alters the focus: and this must be provided for, as one instant lost, after the two chemical agents are in contact, is of importance. I then draw the glass a little aside, and holding in my hand another plate of glass, I put on it the other substance, say a drop of acid. I spread both to an equal extent on their respective glasses, and then quietly and carefully turning down the one upon the other, I push them gently forward, by the application of a fine point to the edge of the lower glass only, to their proper place on the port-object. The upper glass should never be touched after it is turned down, as it will disturb the action. It should therefore be somewhat smaller in dimension then the lower glass, to diminish the risk of its being moved accidentally. The pressure of the one glass upon the other spreads the whole compound into one nearly uniform film upon one level, and consequently the whole action going on may be examined by gently moving the glass in different directions.

The few experiments which I describe are those of combinations and decompositions with respect to which the results are universally known. They are detailed, therefore, only to exemplify the very great facility with which the most minute phænomena attending chemical action can be seen by means of the apparatus described, and which are lost sight of in operating with larger quantities, in the retort or the test-tube.

For instance: put on the lower glass a very small portion of carbonate of copper, and on the upper glass a drop of nitric acid, and bring them together. The carbonic acid is seen to evolve in beautiful globules with more or less violence, which gradually merge into each other: the round particles of the carbonate gradually disappear as the solution of the nitrate of copper is formed. A few massy crystals of a deep blue colour appear amongst the remains of the carbonate, whilst in the clear solution multitudes of small rhombic tabular crystals are deposited in all their possible varieties of proportion. Raise the upper glass gently and add a drop of ammonia; the crystals of the nitrate instantly dissolve and disappear. The nitrate of ammonia is spread over the glass in a great variety of forms, and this is interspersed with groups of prisms of the deep violet-coloured ammoniuret of copper.

The phænomena attendant on the production of the chloro-chromic acid are equally beautiful. The action of the sulphuric acid on the chloride of sodium for the elimination of the muriatic acid is at first very violent. The whole field becomes turbid with green and red particles rushing onward in different currents. At length it begins to clear. Often from one brighter spot a series of globules begin to arise, increase, and rush forward in one continued course, attended on each side by a multitude of small drops of the chloro-chromic acid; and finally, as the action subsides, a field of clear amber-coloured liquid appears filled with varied crystals of sulphate of soda and sulphate of potash, sprinkled irregularly with the blood-red drops of the acid, and occasionally mixed with cubes of the chloride of sodium and cubes of the bichromate of potash that yet remain undecomposed.

On bringing together the ferrocyanate of potash and sulphate of iron, the process of crystallization is very distinctly observed. Sulphate of potash in solution is at first formed, in which the small particles of the deep Prussian blue float. The course of these coloured particles floating in the colourless fluid exhibits the currents that run in it, and soon the crystallization of the sulphate of potash commences in a variety of characteristic forms, the particles of the pigment indicating the flowing of the tide in which they float, as the solution gradually forms itself into masses of crystallization along the line.

One of the most remarkable changes in the character of crystallization is seen if sulphuric acid be added to carbonate of copper: crystals speedily appear in the form of six-sided tabular prisms. Add a little ammonia; the form of crystallization is changed entirely to long rectangular prisms with the angles replaced. Add a little more ammonia and the form changes to several varieties of the rhombic octohedron: a little nitric acid restores again the form of the rectangular prism. And in all these successive changes it is not that a few crystals of another form have been superadded, but each time the metamorphosis is seen to take place in the whole mass.

The microscopic history of iodine in the exhibition to it of a variety of agents is most interesting. Its effect as a colouring matter on the oval molecules of starch is very pretty. It is equally curious to see the application of a portion of nitric acid swell those coloured globules from their centre till they are torn by the internal force and dissipated. If solution of iodine be added to sulphate of soda in solution, the result is very beautiful. The alcohol takes up a portion of the water from the sulphate of soda, which consequently crystallizes in long prisms. The iodine deprived of the alcohol appears in cherry-red drops, and in dark rhombic metallic-looking crystals.

It would be easy to multiply instances, but these must suffice as a specimen of the interesting objects that await experiment as the reward of the observer. The range of experiments may easily be extended to those processes which require heat: longer glasses must then be used which reach beyond the edge of the port-object, and a small spirit-lamp applied under the projecting portion of the glass will give any degree of heat required either for evaporation or boiling.

Some curious results also have been observed on the application of the galvanic pile to chemical substances under the microscope. For this purpose it is desirable to use a lens of somewhat smaller power. The observation cannot well be carried on except on a single plate of glass; and the lens used, therefore, should be of such a focus as to allow of its being above the reach of the vapours that arise, and that would otherwise condense on it and obstruct the view. Some little tact and experience also are required to manage the wires of the battery, because under the compound microscope they appear reversed. I will give an instance or two. When the copper wires were put into a drop of ammonia, a very beautiful greenish foliated or dendritic structure started from the positive pole, and rushed towards the other. On withdrawing the wires a little from each other, a new growth arose out of the extremity of the previous formation, and generally on coming within a certain distance of the negative pole it assumed the metallic lustre.

Galvanic action has been long known to coagulate albumen: on examining this action under the microscope this thicker or white portion showed itself to be a vesicular structure which shrunk up in folds separated in several directions by stronger integuments; while the thin liquid which previously filled them, and gave them in a state of distension complete transparency, spread over the glass dividing and drying in compartments like those of a dragon-fly's wing.

These statements will be ample to put any one in possession of the mode for making similar observations. Further illustrations are unnecessary. The object of this notice is merely to describe a simple arrangement for approaching more nearly the phænomena of chemical action. The microscope will thus open to the chemist new and interminable fields of fascinating inquiry, which cannot but have their use; for although in this mode of operating the several substances must meet each other in unweighed and indefinite proportions, yet the plan seems to hold out some advantages, at least to facilitate the incipient processes of analysis, and to serve as a guide to subsequent experiments of a more measured kind and on a larger scale. If the observation of results is recorded and classified, they must at last lead the practised observer to more than conjectural conclusions as to what he sees; the visible effects under the action of certain agents will become daily more accurately known, till at length the microscopic examination of any substance will go very far to establish its real character.

N.B. Since this paper was read its author has applied a micrometer to the microscope for measuring the angles of the minutest crystals that appear on the field. A short notice of this appeared in Jameson's Journal.

IV. *Notice of the Harvest-Bug.* *By* A Correspondent.*

Acarus autumnalis, Shaw.
Acarus Ricinus, Latreille.

IF powers of annoyance form a claim to attention, there is none superior to that possessed by the minute insect known in England as the *Harvest-bug*, and on the Continent, where, according to Latreille's *personal* experience, its effects are equally serious, as *la Louvette.*

No good description of it is however extant, and the engraving in Shaw's work bears a very slight resemblance to nature, owing doubtless to the extreme difficulty of obtaining specimens of an insect so nearly invisible to the naked eye. Having been so fortunate as to procure several uninjured harvest-bugs, and having submitted them to a highly powerful magnifier, so as to make a drawing, which underwent many comparisons with the living subject, and is as correct as it is possible to render it, an engraving from it is annexed, for the examination of the curious, together with such particulars as differ from the account of established authorities;—not from any wish to cavil or find fault with those who have done so much for entomology, but with an anxiety, laudable it is hoped, to add (without punning) a *mite* to truth.

The Acarus in question then is a hexapod, of a brilliant scarlet colour: its motion is very swift, and the only way in which the observer can satisfactorily contemplate it is by immersing the insect in a drop of water, in which it swims vigorously†, and from which it cannot escape.

The body is oval, sprinkled with stiff hairs, and sixteen very strong ones fringe the hinder part; the legs are horny, like those of a beetle: each foot is furnished with two, and some-

* Communicated by Thomas John Hussey, D.D., Rector of Hayes, Kent.
† One specimen was still swimming after a lapse of seven hours.

times three, strong claws, with which it works so rapidly, mole-fashion, that it inserts itself beneath the skin in a few seconds. Shaw states that it "adheres to the skin by means of two strong hooks attached to the fore part of the body," but these I have never been able to see: he appears not to have been aware of its *burying itself beneath the surface*, in which case it is no longer possible to extract it; a small tumour then forms, the itching of which is intolerable. Patience, the panacea universally recommended, is as universally neglected. Serious consequences often arise in an irritable constitution, from broken sleep, and the skin being torn in frantic endeavours to procure relief. External applications are of little avail, the creature being safe beneath the skin; sal volatile, seldom had recourse to till the nails have failed, will change the itching to a pungent smart. As however is the case with all similar scourges, there are individuals perfectly exempt from its attacks.

Shaw, Latreille, and White of Selborne all state that this insect is located upon corn, kidney-beans, and various other vegetables; this they probably adopted from each other, the original foundation being popular belief: but having been assured in the course of my researches on this subject that* Daddy Long-legs (*Phalangium Opilio*) was the father of Harvest-bugs, and the common red garden spider their prolific mother, and having heard a regular war determined against *them* as the origin of all the suffering, I may be excused for doubting the value of *popular* opinions; and let us hope that these absurd fancies of persons who ought to have known better will vanish before the light shed by the *popular* study of entomology.

The evidence then appears strongly to favour the opinion that the habitat of the harvest-bug is upon, or close to, the ground. White says that "upon the chalk-downs the warrener's nets are sometimes coloured red by them†;" and incredible as this may appear to one engaged in contemplating a single specimen, there is no doubt of the truth of the statement, even though it had rested on meaner authority, for the hem of many a Hampshire petticoat has been similarly discoloured, the wearer of which by throwing it off in time prevented the ravages of the insect being extended to the upper part of the person. Experienced sportsmen well know that

* Probably from his being frequently covered with another parasitic Acarus, *Acarus ocypete*.

† Chalk is the favourite soil; and perhaps their abounding in corn-fields is owing to the earth being so dry among the ripe straw, and so warm also.

on the moors they escape the enemy by wearing a close boot. After walking some time upon gravel, far removed from any plant whatever, the stocking will be found sprinkled with them, when, running rapidly upwards, they ensconce themselves wherever the dress is most closely confined to the body. Animals, particularly horses, suffer dreadfully from this cause, the tender skin of the lips and nose being frequently covered with nests of harvest-bugs, which have fixed there during grazing, but which probably cannot bury themselves as in the human being from the toughness of the skin. The cat's whiskers have a scarlet spot at the insertion of each hair *, and she *bites* her paws all day, yet does not relinquish her favourite bask on the warm gravel, which probably is the cause of her annoyance, because the rabbits, shut up in a building, though fed even on the freshest of kidney-bean plants, are not aware that harvest-bugs exist.

If it be asked where was the embryo harvest-bug,—where was the insect whose life, beginning as it would seem with the greatest heat of summer, ended with the first cold of autumn,—during the intermediate nine months? we may reply, Probably buried in embryo in the soil. But research would afford no information on this subject, from the minuteness of the insect.

Rectory, Hayes, Kent, March 1836. A. M. H.

V. *On the Divergence of Plants, and its Analogy to the Irritability of Animals. By* Henry Johnson, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In No. 33† of your valuable Journal you have done me the favour to insert a short communication on the subject of a newly discovered property in plants supposed to be analogous to animal irritability, and which communication has been honoured by the notice of two "friends," whose inquiries and remarks are appended to my paper.

I take great shame to myself for having allowed a whole year to elapse without any attention having been paid, or at least any answer returned, to these very valuable and obliging notices, for which, whilst I apologise for my apparent neglect of them, allow me to offer through you my best thanks to your ingenious and able correspondents.

* They probably do not breed in the animal skin any more than in that of their superior prey, because though visible in large groups, each individual seems equally mature.

† Lond. & Edinb. Phil. Mag., March 1835, vol. vi. p. 164.

At the foot of page 166, your botanical friend asks me if I have ever tried the effect of division on *Dirca palustris*, or on any plant of the natural order *Thymeleæ.*

The *Dirca palustris* is an exotic, and, I believe, a rare plant, which never having seen, I have of course not had an opportunity of making it the subject of experiment.

Belonging to the natural order *Thymeleæ* there is one genus only found in England*, the genus *Daphne*, and of this I have, during the present month, had an opportunity of examining two species, the *Daphne Mezereum* and *Daph. Laureola.* On dividing the recent green shoot of this year I found it in both decidedly divergent. They form, therefore, no exception to anything which I have stated in my paper.

At page 169, your medical friend remarks, that the phænomena described in my paper most closely resemble the contraction of the ligamentum nuchæ by which the head of animals is retracted after death, and which Bichât attributes to vital contractility.

Not having, at present, access to the works of Bichât, I am unable to learn the evidence on which he grounds this opinion. Whether true or not, however, I do not see that it affords an objection to anything which I have advanced.

If this contraction of the ligamentum nuchæ be an instance of vital contractility, and susceptible of excitation by stimuli, it would appear to me to be identical with the irritability or contractility of muscular parts, and analogous to *divergence*: and therefore not a "distinct property."

If stimulants do not excite contraction in the ligamentum nuchæ, the property, whether vital or not, on which its motions depend, differs in this essential particular from divergence. I am, Gentlemen, yours, &c.

Shrewsbury, May 28, 1836. HENRY JOHNSON, M.D.

VI. *On the Theory of Vanishing Fractions, in Reply to* Professor Young. *By* Mr. W. S. B. WOOLHOUSE.†

WHEN Professor Young's first letter on the theory of vanishing fractions made its appearance in the April Number of the Philosophical Magazine, the anomalous objections that were urged against my general principles with such apparent confidence were accounted for in my mind by the belief that he had been carried away by a partial and very imperfect perusal of the contents of my essay. Professor

* Gray's Natural Arrangement of British Plants.

† Communicated by the Author.

Young seemed to imagine that I had contented myself with testing the accuracy of those principles by particular examples, and appeared to be unacquanted with the circumstance that my especial object in writing that essay was to show the impropriety of multiplying and dividing by zero in analytical reasonings, and to establish the applications of the differential and integral calculus in a more consistent and logical manner. But having already given in my former communication what I consider to be a complete demonstration of the general principles alluded to, it is indeed strange that Prof. Young, in his present letter, should have followed the same course of objection and evinced the same inattention to my repeated denial of the logical accuracy of processes in which multiplications or divisions by zero are concerned. At page 396, I have distinctly proved the fallacy of such processes by showing that in the one case we may pass from a condition that determines a particular value to another of an indeterminate character, and that in the other we may pass from a condition that is satisfied by any value to another that would limit the unknown to a particular value. But even after this Professor Young proceeds entirely on the incorrect supposition that not only those operations, but that analytical processes under all circumstances must necessarily lead to justifiable results; and this he does without condescending to adduce a single argument in favour of so objectionable a supposition.

Prof. Young writes rather largely in reply to my observations on what I designated in my former letter "the theory of analytical results," and endeavours to make out that I have fallen into a mistake. I introduce the subject by first observing, that "I never before heard of the incompetency of an analytical result to afford any positive information that an investigation could admit of." The mathematical readers of the Journal who have read the paragraph containing my entire explanation of this point, will readily perceive that Prof. Young through the whole of his observations has misinterpreted the meaning of this isolated sentence, which I think he would not have done had he taken "a more enlarged view" of the analysis. As a matter of course, I allude to analytical results arrived at by a process of general reasoning, not a solution obtained by imperfect means. In every analytical investigation I consider that the generality of the process requires that each step shall hold good both *directly* and *conversely*, and consequently that each successive equation shall be fulfilled by neither more nor less than the several roots that are contained in the equations that have preceded. As an instance, suppose an equation with all its terms collected to one side should resolve itself into two factors, and become of the form

$$F(x,y)f(x,y) = 0 \quad (\alpha)$$

Its complete solution will evidently comprehend the whole of the solutions that can be obtained from the two separate equations

$$F(x,y) = 0 \quad (\beta)$$
$$f(x,y) = 0 \quad (\gamma)$$

each of which will determine a separate system of values. It is hence clear that if we had to resolve only one of the two equations (β), (γ), that we should arrive at only a *part* of the solutions to (α), and therefore that neither (β) nor (γ), separately considered, can be regarded as a complete deduction from the preceding equation. This principle, which applies to any number of factors, is so well understood by mathematicians that it would be a waste of time to discuss it at length. When an investigation is conducted in the general manner, just described, it is obvious that the final result must furnish every value capable of satisfying the original analytical conditions. Prof. Young must have overlooked altogether the general nature of complete analytical processes when he mentions "*singular solutions,*" that occur in the integration of differential equations, as a case in opposition to my remarks. He must be aware that a direct process of integration of a differential equation, that admits of a singular solution, will always determine that solution at the same time with the general solution. The operation will lead to an equation consisting of two factors, similar to the foregoing equation (α), and when decomposed into the two separate equations (β), (γ), the one will give the general while the other will give the singular solution, and both solutions taken together will form the *complete* solution. I should have thought that Professor Young would have better examined the matter before he favoured me with the gratuitous compliment that I had done myself a "wanton injustice." We might similarly refer to the case of a general solution of a functional equation, usually obtained by the assumption of a particular solution: with very few exceptions the general solution so elicited does not comprehend all the forms, but only represents one of an infinite variety of classes of solutions that will satisfy the proposed equation; and it cannot, therefore, be recognised as a perfect result. It would, however, be wrong to adduce instances of any kind in which the results are arrived at by indirect and defective means; for in that case they would no more be the results of investigation than the mere anticipation of a value by trial in the original equations: we cannot be assured in such cases that we obtain a complete solution as we necessarily do in perfect processes. The process in the ellipse question is founded on general reasoning as far as the vanishing fractions, and those fractions ought therefore to supply all the values that can satisfy the original equations. I still hold myself justified, therefore, in maintaining that Prof. Young not only involves himself in a "palpable inconsistency," but that he also takes an imperfect view of the "theory of analytical results," when he denies the competency of the results of the ellipse question to furnish the requisite values, and at the same time agrees to receive them from the original analytical conditions.

With respect to the quadratic equation, advanced at page 519, the common process of resolution is perfectly general when it is considered that the radical surd is capable of sustaining either the positive or the negative sign. The roots 4, $\frac{8}{3}$, presented by the involved quadratic, rigidly satisfy the original *analytical* condition; for the radical term may, *analytically*, be interpreted either as being + or −, and so far as the analysis is concerned it is perfectly immaterial which interpretation is adopted. Thus the result 4 satisfies the condition $2x + \sqrt{x^2-7} = 5$ with the same analytical strictness that it satisfies $2x - \sqrt{x^2-7} = 5$; the only difference is that in the one case the + and in the other the − interpretation of the root is not complied with. It may be observed, however, that the nature of a problem originating such an equation may exclude all − interpretations, and therefore limit the calculation to the + value of the root. In such a case the positive information, obtained from the analytical result, would be that the numbers 4, $\frac{8}{3}$ furnish every solution to the original *analytical* condition; the rejective information, suggested by the nature of the problem, would be the exclusion of both roots as not satisfying the implied condition of + interpretation; and the final conclusion would be that the problem did not admit of any solution whatever. How my ingenious friend can for a moment imagine that this example, or indeed that any of the "cluster of instances" contained in Mr. Horner's paper, is either consistent with his view of the matter or inconsistent with mine is to me very extraordinary. For my part I am convinced that any impartial person who gives the slightest attention to the case will be led to an exactly opposite conclusion.

I have before observed that Prof. Young throughout the whole of his letter goes entirely on the wrong hypothesis that analytical processes under all circumstances must necessarily lead to justifiable results. I beg again to advance the contrary doctrine, that a result can be received as general only so far as the reasoning employed in deducing it is fairly applicable to the particular instances. In the present case consider the general equations given in my former letter, viz.

$$0 = (x-a)^{\alpha}\, \theta x - y(x-a)^{\beta}\, \phi x \; \ldots \; (p)$$

$$0 = (x-a)^{\alpha-\beta}\, \theta x - y\, \phi x \; \ldots\ldots \; (q)$$

If we proceed from the first of these equations to the second, we divide by the factor $(x-a)^{\beta}$; and if we proceed from the second equation to the first, we multiply by the same factor. The logical accuracy of either of these processes must necessarily fail for the particular case in which x is equal to a, as I have shown in my former letter; for in this case the first equation is evidently satisfied by *any value* of y while the latter limits it to a *particular* value. The complete solution of the first equation (p) must comprehend both systems of values obtained from the two separate equations indicated by the two factors, viz.

$$0 = x-a \; \ldots\ldots\ldots\ldots\ldots\ldots\ldots \; (r)$$

$$0 = (x-a)^{\alpha-\beta}\, \theta x - y\, \phi x \; \ldots \; (s)$$

and as the former does not contain the quantity y, that variable may obviously possess any value whatever in the first system when x fulfills the equation $o = x-a$. The system of values given by the condition (s) will determine the curve M N described at page 28 of my essay, while the former system will determine the indefinite straight line R S; and both the curve and straight line will complete the geometrical representation of the equation (p), while the curve alone is the representation of (q). It would be just as improper to reject the system of values furnished by (r) in the present case, as it would be to reject the system of values furnished by the equation (β) in page 20, in resolving the equation (α). If in the equation (α) we were to adopt the hypothesis that the quantities must not fulfill the condition (β), the whole of the solutions would be comprised in the result obtained by the general resolution of (γ); but if this hypothesis were removed the result so obtained would evidently fail to represent all the solutions to the proposed equation. In the same manner, if in the equation (p) we were to adopt the supposition that the condition (r) must not be satisfied, or that x must not $= a$, the whole of the solutions would be comprised in the resolution of (s) alone; but the result would necessarily fail to be complete when the supposition becomes removed*.

* The celebrated Bishop Berkeley, in his able work entitled the "Analyst," has, with singularly acute and convincing arguments, amply elucidated this fallacy of shifting the hypothesis.

In speaking of dividing by zero, it must be understood that in deducing the equation

$$y = \frac{(x-a)^{\alpha}\,\theta x}{(x-a)^{\beta}\,\phi x} \;\cdots\cdots\cdots\; (t)$$

from (p), we do not in this case actually perform the operation of division so long as the fraction is not reduced to lower dimensions. The equation (t) in its present shape is synonymous with the condition (p), being merely the same condition differently written down. I have had occasion to say before that the mere placing of a quantity in the denominator of a fraction cannot be strictly recognised as an actual performance of division.

From the foregoing observations the effect of the fault committed in dividing by *zero* is very evident. We proceed from the condition (p), which contains an *infinite* number of values of y, to another that contains only one value. The fault is similar to that which would be committed in dividing the equation (α) in page 20, by $F(x,y)$, and then taking only the resulting equation (γ) for the solution of (α), which would necessarily exclude from the results the solutions contained in (β). But the error in the present case would be greater as we should exclude from the result the infinite number of values contained in (r). If we were permitted to multiply and divide by zero we should be at liberty to play many curious tricks with analytical equations. As an instance, if any equation were proposed for solution, of the form $\phi(x,y) = 0$, we might first multiply by any arbitrary function $\theta(x,y)$ which would give $\phi(x,y)\,\theta(x,y) = 0$; we might next divide by $\phi(x,y)$, which would give $\theta(x,y) = 0$, that is, we should be able to make any function whatever of the same quantities equal to *zero*, which is a manifest absurdity.

On the whole we may remark, that when the value of a symbol y which ought to be determinate comes out in a vanishing fraction (t), the fault alluded to has occurred in the process. The symbol y and its corresponding vanishing fraction as they appear in the equation (t) cannot in this case represent the solution of the original condition, but a solution of the equation which is antecedent to the final result and which possesses an infinite number of values; and consequently the symbol given in the final result is not strictly synonymous with the same symbol as it appears in the original equation. It is therefore vain to refer to the original condition for the interpretation of this vanishing fraction, for in that case we interpret the root of the first condition, whereas the fraction represents the root of another condition that has been improperly derived from it.

At page 516, Prof. Young states that I have only discussed the *converse* of the Proposition III. in my former letter. This, however, is not the case, for in that letter I distinctly go into the demonstration of each of the four propositions extracted from my essay. Indeed, immediately after, at page 518, Professor Young himself admits that I have done so!! In allusion to my having rejected as illogical the processes in which multiplications or divisions by zero occur, he there states that "if only results obtained under such restrictions as these are admitted to come under the second and third principles, then the *generality* of those principles is, of course, at once given up, and my friend and I are thus far agreed." Now these restrictions are no more than the necessary exclusion of results obtained by imperfect and fallacious reasoning, and cannot fairly be considered as limitations to the generality of the propositions. It is now distinctly avowed by Prof. Young that the generality of the propositions objected to is established for all cases in which these restrictions are attended to, or in which the fallacious reasoning does not enter. Proceeding on this admission, it necessarily follows that in every case in which the final result fails to conform with the propositions the fallacious reasoning must have been introduced somewhere in the process of solution. In every possible case, therefore, of nonconformity with the Proposition II. or III. we observe that the fact itself is a sure indication that a multiplication or division by zero has occurred in the investigation, and therefore that the case must be rejected at once as not offering a legitimate result. It would be utterly useless, therefore, to enter into Prof. Young's observations at page 517, on the expression for the radius of curvature*, since neither this nor any other particular example of nonconformity can have the least weight unless my friend can show the operations of multiplying and dividing by zero to be strictly logical.

Prof. Young is quite out in supposing that the restrictions to correct reasoning will limit the application of the principles

* I would, however, here remark that the conditions $P = 0$, $Q = 0$, will not *necessarily* cause a value of dr to be zero, as Prof. Young alleges. Such a doctrine would imply that any vanishing fraction $\frac{P}{Q}$ would necessarily attain its greatest or least value when its numerator and denominator both vanish, which is not the case. The conditions $P = 0$, $Q = 0$ will not, therefore, determine the points of higher contact referred to by Professor Young. In his example of the parabola the determination of the true result is purely accidental.

to "comparatively few cases." On the contrary, the false cases very seldom occur, and they can generally be corrected by a slight reference to Proposition IV. This is, in fact, the very way in which I dismiss Prof. Young's query respecting the sum of the geometrical series. I refer to Proposition IV. not to *interpret* but to *correct* the resulting expression for the particular case in which the reasoning has failed. Had the expression been a just deduction, Proposition III. would have applied to it, and the sum in that case *would* have been *anything*. How Professor Young could have misconceived me I am quite at a loss to explain, as I have distinctly pointed out the fault that occurs in deducing it.

In Prof. Young's first letter, at page 298, he states that "when we are operating with equations of the first degree containing several unknown quantities, the symbol $\frac{0}{0}$ is, in fact, the very form which the result usually takes when the proposed equations involve incompatible conditions." I feel assured that Prof. Young, after a little reflection, will not venture again to assert the truth of this hasty and erroneous statement. At page 520, however, he has attempted to refute my observation that $\frac{0}{0}$ can never be the symbol of absurdity in the result of an investigation logically conducted; but it will be seen that my friend's remarks are founded on the same erroneous hypothesis, that the result is justifiable in whatever way it may have been deduced. When it is the result of a logical process it is obvious, since the antecedent equation is satisfied by any value, that all the preceding equations, and consequently the original condition, must likewise be satisfied by any value. The symbol $\frac{0}{0}$, whenever it is the result of a strict investigation, may therefore possess any value, and cannot possibly be the "symbol of absurdity," however much Prof. Young may be "surprised" at the statement.

In thus candidly replying to my friend's letter, I am disposed to give him every credit for his own opinions. I think, however, that he ought not, for his own sake, to have proceeded to such a length with his remarks, without having, in the first place, made some attempt in support of his objectionable premises. Should Prof. Young still entertain the same opinions I shall make no further attempt to change them, though I may be allowed to maintain my own. It will not be necessary to enter into any further details at present, as I have doubtless said quite sufficient for the mathematical readers of the Journal. At least I am well satisfied with having shown, by concise, general, and undeniable reasoning, that my friend might have advantageously spared himself the trouble of offering his numerous observations, had he paid more regard to the logical strictness of his assumptions.

London, June 4, 1836.

VII. *On the Water of Crystallization of Soda-alum. By* Thomas Graham, *F.R.S.E., Professor of Chemistry in the Andersonian University, Glasgow; Corr. Member of the Royal Academy of Sciences of Berlin, &c.**

THE double sulphate of alumina and soda crystallizes in the form of the regular octohedron, like the sulphate of alumina and potash, while the former salt is supposed to contain twenty-six atoms of water, and the latter contains only twenty-four. The coincidence in form of these two salts is most interesting, for in no other corresponding salts of potash and soda has such a relation been observed from which any inference in respect to isomorphism could properly be drawn. Yet if the soda-salt contains two atoms more of water than the potash-salt, the conclusion which follows is, not that soda and potash are isomorphous bodies, but that soda plus two atoms water is isomorphous with potash, as ammonia plus one atom of water is isomorphous with the same body. But the last analogy is superficial and likely to prove illusory.

The exact determination of the water of crystallization of a salt is often a problem of no inconsiderable difficulty, as many precautions must be taken which are by no means obvious. To have alumina free from potash or ammonia, it was precipitated from pure potash-alum by means of carbonate of soda. A solution of sulphate of alumina was formed by dissolving the precipitated alumina in the proper quantity of sulphuric acid, and the requisite proportion of sulphate of soda was added. A considerable crop of crystals of soda-alum were obtained from the above solution allowed to evaporate spontaneously in air.

Like most very soluble salts the crystals of soda-alum, when newly prepared, retain hygrometrically a notable quantity of the saline liquor in which they have been formed. But the crystals of this salt cannot be dried easily, as after they lose their hygrometric water they are nearly as efflorescent as sulphate of soda itself. Before being submitted to analysis the crystals had been kept for five months of cold weather in a large phial stopt by a cork. Their surfaces remained per-

* Communicated by the Author.

fectly bright, and not in the slightest degree effloresced; but the crystals had lost, from the escape of their hygrometric water, that extreme and watery clearness which we have in the crystal newly removed from its mother-liquor. From my experience in respect to such salts, I had reason to believe that the crystals of soda-alum were now in a most suitable condition for analysis.

Upon a very damp day the crystals were reduced to powder and pressed in blotting-paper. A large crystal exposed to the air at the same time lost nothing. 20·35 grs. of the salt so prepared were exposed on a sand-bath to a heat, which was gradually raised so as to effloresce the salt without melting it or causing vesicular swelling. In eight hours the salt had been heated above the melting point of tin, and had lost 8·98 grains. It was thereafter heated, in a gradual and cautious manner, to low redness by the spirit-lamp, and the loss became 9·65 grains. By a continued exposure to the same heat for half an hour more, the salt lost only one hundredth of a grain additional. Supposing it now to have lost all its water, the salt will consist of

			Theory of 24 atoms of water.
Sulphate of alumina and soda	10·69	100·	100·
Water	9·66	90·37	88·9
	20·35	190·37	188·9

The calcined salt dissolved slowly but completely in boiling water. By precipitation with muriate of barytes it afforded 21·22 grains sulphate of barytes, equivalent to 7·37 grains sulphuric acid. Or the crystallized salt contains 34·73 per cent. of sulphuric acid, while the theory of twenty-four atoms of water supposes it to contain 34·93 per cent. sulphuric acid.

It follows from this analysis that soda-alum contains twenty-four atoms of water and not twenty-six.

There is no reason to question the perfect accuracy of the analysis of potash-alum by Berzelius, which gives to it likewise twenty-four atoms of water. Dried in the manner described for soda-alum, I found it to consist of

		Theory of 24 atoms of water.
Sulphate of alumina and potash	100·	100·
Water	84·8	83·4
	184·8	183·4

In such analyses, there is imminent danger of the water carrying off a little acid with it, unless it is expelled in the most slow and cautious manner. It is probably from this cause that the water has come to be overestimated in the case of the alums. But they stand a low red heat without decomposition, if first made quite anhydrous.

VIII. *Second Theorem of Algebraic Elimination, connected with the Question of the Possibility of resolving, in finite Terms, Equations of the Fifth Degree. By Professor Sir* William Rowan Hamilton, *Astronomer Royal of Ireland.*

(In continuation of a Communication in the last volume, p. 538.)

Theorem II. If x be eliminated between a proposed equation of the fifth degree,

$$x^5+Ax^4+Bx^3+Cx^2+Dx+E=0, \quad \ldots\ldots \quad (55.)$$

and an assumed equation, of the form

$$y=Qx+f(x), \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

in which $f(x)$ denotes any rational function of x,

$$f(x)=\frac{M'x^{\mu'}+M''x^{\mu''}+\ldots}{K'x^{\varkappa'}+K''x^{\varkappa''}+\ldots}; \quad \ldots\ldots\ldots\ldots \quad (3.)$$

and if the constants of this function be such as to reduce the result of the elimination to the form

$$y^5+B'y^3+D'y+E'=0, \quad \ldots\ldots\ldots\ldots\ldots \quad (56.)$$

independently of Q: then not only must we have

$$A=0, \quad C=0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (57.)$$

so that the proposed equation of the fifth degree must be of the form

$$x^5+Bx^3+Dx+E=0, \quad \ldots\ldots\ldots\ldots \quad (58.)$$

but also the function $f(x)$ must be of the form

$$f(x)=qx+(x^5+Bx^3+Dx+E)\,.\,\phi(x), \quad \ldots \quad (59.)$$

q being some constant multiplier, and $\phi(x)$ some rational function of x, which does not contain the polynome x^5+Bx^3+Dx+E as one of the factors of its denominator; unless we have either, first,

$$E=0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (60.)$$

or else, secondly,

$$5D=B^2, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (61.)$$

or, as the third and only remaining case of exception,

$$5^5E^4+2^2BE^3(2^2\,5^3D^2-3^2\,5^2B^2D+3^3B^4)$$
$$+2^4D^3(2^4D^2-2^3B^2D+B^4)=0. \quad \ldots\ldots \quad (62.)$$

Demonstration.—If we denote by $x_1\, x_2\, x_3\, x_4\, x_5$ the five roots of the proposed equation of the fifth degree, and put, as is permitted,

$$\left.\begin{array}{l} f(x_1) = h_1 + q\,x_1,\ f(x_2) = h_2 + q\,x_2,\ f(x_3) = h_3 + q\,x_3, \\ f(x_4) = h_4 + q\,x_4,\ f(x_5) = q\,x_5,\ \dots\dots\dots \end{array}\right\} \quad (9.)$$

and
$$Q + q = Q', \quad \dots\dots\dots \quad (8.)$$

the result of the elimination of x between the two equations (55.) and (2.) may be denoted thus,

$$(y - Q'x_1 - h_1)\,(y - Q'x_2 - h_2)\,(y - Q'x_3 - h_3)\,(y - Q'x_4 - h_4) \times (y - Q'x_5) = 0; \quad \dots\dots \quad (10.)$$

and if this result is to be of the form (56.), independently of Q, and therefore also of Q′, we must have the six following relations:

$$x_1 + x_2 + x_3 + x_4 + x_5 = 0, \quad \dots\dots\dots \quad (11.)$$

$$x_1 x_2 x_3 + x_2 x_3 x_4 + x_3 x_4 x_5 + x_4 x_5 x_1 + x_5 x_1 x_2 + x_1 x_3 x_4 + x_2 x_4 x_5 + x_3 x_5 x_1 + x_4 x_1 x_2 + x_5 x_2 x_3 = 0, \quad (13.)$$

$$h_1 + h_2 + h_3 + h_4 = 0, \quad \dots\dots\dots \quad (16.)$$

$$h_1\,(x_2 x_3 + x_2 x_4 + x_2 x_5 + x_3 x_4 + x_3 x_5 + x_4 x_5) + h_2\,(\&c.) + h_3\,(\&c.) + h_4\,(\&c.) = 0, \quad \dots\dots\dots \quad (63.)$$

$$h_1 h_2\,(x_3 + x_4 + x_5) + h_1 h_3\,(\&c.) + h_1 h_4\,(\&c.) + h_2 h_3\,(\&c.) + h_2 h_4\,(\&c.) + h_3 h_4\,(\&c.) = 0, \quad \dots\dots \quad (64.)$$

$$h_1 h_2 h_3 + h_1 h_2 h_4 + h_1 h_3 h_4 + h_2 h_3 h_4 = 0; \quad \dots\dots \quad (19.)$$

of which the two first give

$$A = 0,\ \ C = 0, \quad \dots\dots\dots \quad (57.)$$

and the three last may, by attending to the first and third, and by eliminating h_4, be written thus:

$$h_1\,(x_1^2 - x_4^2) + h_2\,(x_2^2 - x_4^2) + h_3\,(x_3^2 - x_4^2) = 0, \quad (20.)$$

$$h_1^2\,x_1 + h_2^2\,x_2 + h_3^2\,x_3 + (h_1 + h_2 + h_3)^2\,x_4 = 0, \quad (21.)$$

$$(h_2 + h_3)\,(h_3 + h_1)\,(h_1 + h_2) = 0. \quad \dots\dots\dots \quad (22.)$$

Selecting, as we are at liberty to do, the first of the three factors of (22.), namely,

$$h_2 + h_3 = 0, \quad \dots\dots\dots \quad (23.)$$

and eliminating h_3 by this, we reduce the two conditions (20.) and (21.) to the two following:

$$h_1\,(x_1^2 - x_4^2) + h_2\,(x_2^2 - x_3^2) = 0, \quad \dots\dots \quad (24.)$$

$$h_1^2\,(x_1 + x_4) + h_2^2\,(x_2 + x_3) = 0, \quad \dots\dots \quad (25.)$$

which give, by elimination of h_2,

$$h_1^2 (x_1 + x_4)\{(x_1 + x_4)(x_1 - x_4)^2 + (x_2 + x_3)(x_2 - x_3)^2\} = 0. \quad (26.)$$

And from these equations (of which some occurred in the investigation of the former theorem, but are now for greater clearness repeated,) we see that we must have

$$h_1 = 0, \quad h_2 = 0, \quad h_3 = 0, \quad h_4 = 0, \qquad (29.)$$

and therefore, by (9.),

$$f(x_1) = q\,x_1, \quad f(x_2) = q\,x_2, \quad f(x_3) = q\,x_3,$$
$$f(x_4) = q\,x_4, f(x_5) = q\,x_5, \;\ldots\ldots \qquad (45.)$$

unless we have either

$$x_1 + x_4 = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \qquad (65.)$$

or else

$$(x_1 + x_4)\,(x_1 - x_4)^2 + (x_2 + x_3)\,(x_2 - x_3)^2 = 0, \qquad (30.)$$

or at least some one of those other relations into which (65.) and (30.) may be changed, by changing the arrangement of the roots of the proposed equation of the fifth degree.

The alternative (65.), combined with (57.), gives evidently

$$E = 0; \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \qquad (60.)$$

but the meaning of the alternative (30.) is a little less easy to examine, now that we do not suppose the coefficient B to vanish, as we did in the investigation of the former theorem. However, the following process is tolerably simple. We may conceive that $x_1\,x_2\,x_3\,x_4$ are roots of a certain biquadratic equation,

$$x^4 + a\,x^3 + b\,x^2 + c\,x + d = 0, \quad \ldots\ldots\ldots \qquad (66.)$$

and may express, by means of its coefficients $a\,b\,c\,d$, the symmetric functions of $x_1\,x_2\,x_3\,x_4$ which enter into the development of the product formed by multiplying together the condition (30.), and these two other similar conditions,

$$(x_1 + x_3)\,(x_1 - x_3)^2 + (x_2 + x_4)\,(x_2 - x_4)^2 = 0, \quad \ldots \qquad (67.)$$
$$(x_1 + x_2)\,(x_1 - x_2)^2 + (x_3 + x_4)\,(x_3 - x_4)^2 = 0. \quad \ldots \qquad (68.)$$

If we put, for abridgement,

$$x_1^3 + x_2^3 + x_3^3 + x_4^3 = \mathrm{f}, \quad \ldots\ldots\ldots\ldots\ldots \qquad (69.)$$

$$-x_1\,x_2\,(x_1 + x_2) - x_3\,x_4\,(x_3 + x_4) = \mathrm{g}, \quad \ldots \qquad (70.)$$

$$-x_1\,x_3\,(x_1 + x_3) - x_2\,x_4\,(x_2 + x_4)$$
$$-x_1\,x_4\,(x_1 + x_4) - x_2\,x_3\,(x_2 + x_3) = \mathrm{h}, \quad \ldots\ldots\ldots\ldots \qquad (71.)$$

$$\{x_1\,x_3\,(x_1 + x_3) + x_2\,x_4\,(x_2 + x_4)\}$$
$$\{x_1\,x_4\,(x_1 + x_4) + x_2\,x_3\,(x_2 + x_3)\} = \mathrm{i}, \quad \ldots \qquad (72.)$$

the condition (68.) will become

$$\mathrm{f} + \mathrm{g} = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \qquad (73.)$$

and the product of the two other conditions (67.) and (66.) will become

$$f^2 + h f + i = 0, \quad \ldots\ldots\ldots\ldots\ldots \quad (74.)$$

so that the product of all the three conditions becomes

$$f^3 + (g + h) f^2 + (g h + i) f + g i = 0; \qquad (75.)$$

and the symmetric functions f, $g + h$, $g h + i$, $g i$, may be expressed as follows,

$$f = -a^3 + 3 a b - 3 c, \quad \ldots\ldots\ldots\ldots\ldots \quad (76.)$$

$$g + h = a b - 3 c, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (77.)$$

$$g h + i = a^3 c - 4 a^2 d - 2 a b c + 3 c^2, \quad \ldots\ldots \quad (78.)$$

$$g i = a^5 d - 4 a^3 b d + 4 a^2 c d + a b c^2 - c^3. \qquad (79.)$$

Again, the proposed equation of the fifth degree,

$$x^5 + B x^3 + D x + E = 0, \quad \ldots\ldots \quad (58.)$$

must be exactly divisible by the biquadratic equation (66.), because all the roots of the latter are also roots of the former; and therefore we must have

$$B = b - a^2, \; D = d - a^2 b, \; E = -a d, \qquad (80.)$$

and

$$c = a b. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (81.)$$

This relation $c = a b$ reduces the expressions (76.)...(79.) to the following,

$$\left.\begin{aligned} f &= -a^3, \\ g + h &= -2 a b, \\ g h + i &= a^4 b + a^2 b^2 - 4 a^2 d, \\ g i &= a^5 d; \quad \ldots\ldots\ldots\ldots \end{aligned}\right\} \quad \ldots\ldots \quad (82.)$$

and thereby reduces the condition (75.), that is, the product of the three conditions (66.) (67.) (68.), to the form

$$-a^9 - 3 a^7 b - a^5 b^2 + 5 a^5 d = 0, \quad \ldots\ldots \quad (83.)$$

which gives either

$$a = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (84.)$$

or else

$$a^4 + 3 a^2 b + b^2 = 5 d, \quad \ldots\ldots\ldots\ldots\ldots \quad (85.)$$

and therefore, by (80.), either

$$E = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (60.)$$

or else

$$5 D = B^2. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (61.)$$

Thus, when we set aside these two particular cases, we see by (45.), that under the circumstances supposed in the enunciation of the theorem, the function $f(x) - q x$ vanishes, for every value of x which makes the polynome $x^5 + B x^3 + D x + E$ vanish; and that therefore if we set aside the third and only remaining case of exception, namely, the case in which the proposed equation of the fifth degree has two equal roots, and in which consequently the condition (62.) is satisfied, the function $f(x)$ must be of the form (59.); which was the thing to be proved.

Corollary.—Setting aside the three excepted cases (60.) (61.) (62.), the coefficients of the equation (50.) of the fifth degree in y will be expressed as follows,

$$B' = Q'^2 B, \quad D' = Q'^4 D, \quad E' = Q'^5 E; \ldots \quad (86.)$$

and if we attempt to reduce it to De Moivre's solvible form, by making

$$D' = \tfrac{1}{5} B'^2, \quad \ldots\ldots\ldots\ldots\ldots \quad (50.)$$

we find

$$Q'^4 = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (51.)$$

that is,

$$Q' = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (52.)$$

so that the relation between y and x reduces itself to the form

$$y = (x^5 + B x^3 + D x + E) \,.\, \phi(x), \quad \ldots\ldots\ldots \quad (87.)$$

which can give no assistance towards resolving the proposed equation (58.) of the fifth degree in x.

Observatory, Dublin, June 11, 1836.

IX. *Observations on the Fossil Genera* Pseudammonites *and* Ichthyosiagonites *of the Solenhofen Limestone, contained in a Letter to* R. I. Murchison, *Esq., V.P.R.S., &c.* *By* D. E. Rüppell, *M.D., of Frankfort.**

Dear Sir,

I SEND you herewith the few words you requested me to draw up concerning the fossils I wish to exhibit at the Geological Society.

In a paper which I published in 1829, I ventured to express my opinion upon the generic character of two fossils, fragments of which are commonly met with at Solenhofen, and in other calcareous strata of formations of similar age. Having met with some of these fossils in what I considered to be a more than usually perfect condition, I was led to adopt

* Communicated on the part of the Author by Mr. Murchison, who takes this method of laying before the public the notice, which would have been read before the Geological Society, at its last meeting of this session, had not the letter of Dr. Rüppell been missent. The fossils alluded to were exhibited. Mr. Murchison is convinced that this small fragment will be read with interest as coming from the pen of the distinguished traveller whose researches have thrown so much light on the physical geography and natural history of Nubia and Abyssinia.

my present ideas of the animals to which they belonged. I perceived that the forms which naturalists had united in one genus under the name of *Trigonellites*, *Tellinites*, *Ichthyosiagonites*, and *Lepadites*, (words which are all synonymous,) belonged really to two distinct genera. One of these fossils is not unfrequently found with an Ammonite-like shell, but which has only an apparent likeness to the true Ammonite, for it has no internal septa. In many of these Ammonite-like shells there are found, near to their opening, two calcareous plates resembling in appearance a bivalve shell. These must in my opinion have belonged to the animal which inhabited the Ammonite-like shell, and may have served as a kind of operculum to it, or perhaps as an organ for mastication. I formed this opinion by observing that whenever these two fossils, apparently so dissimilar, are found near one another, the right-hand side of the bivalve-like shell is uniformly of the same length as the largest diameter of the external whorl of the Ammonite-shaped fossil. Besides, I perceived that the two parts which form this kind of operculum are in some other points perfectly distinct in structure from any living bivalve shells, namely, the valves are not connected with ligaments, and have a sharp edge on the side where they unite; the other margin, opposite to this sharp edge, is a thick calcareous mass. The laminæ which are on the convex side of these opercula have not, like other bivalves, a central point round which they increase, but are placed somewhat in a diagonal position, a circumstance which is never met with in a real bivalve shell.

Since I wrote the paper alluded to, I have observed a considerable number of these fossils, all of which confirmed the constant proportion of the diameters of the bivalve and ammonite-like shell when found together. I remain, therefore, confident that they belonged to one animal, forming quite a new type in the series of *Mollusca*, for which I have proposed the name of *Pseudammonites*.

Some naturalists have expressed the idea that the finding both these fossils so frequently together was a consequence of the animal of the Ammonite-like shell having eaten the other. But if so, how does it happen that there is so constantly a fixed proportion in their relative sizes, and why are more than one pair never found in each shell? Besides, had the one served as food for the other, why are the apparent bivalve shells always in a fine state of preservation, lying parallel to each other?

The other fossil, in shape somewhat similar to these opercula, I consider to be an internal shell met with in a large elliptic muscular mass. I have exhibited before the Geological Society one of the specimens which suggested to me this idea, and the sight of it illustrates those facts which I have explained in my work better than a more lengthened description. That a bivalve may have been an interior shell is rather an unexpected exception to all known rules of conchology. It is, however, not a theory of mine, but is borne out by plain facts. To this singular genus I gave the name of *Ichthyosiagones* (1829). It has been changed since to *Aptychus*, I know not why; and again, both the types of *Pseudammonites* and *Ichythyosiagones* have been mixed together as species belonging to the same genus, more particularly in a paper printed last year in the memoirs of the Linnæan Society of Normandy, by Mr. Eudes de Longchamp, who proposed a new name for the genus "*Munsteria*"!

The object I have had in presenting to the meeting of the Geological Society some of the fossils I have spoken of, is to call the attention of English geologists to these shells, that a new investigation may be made by some conchologist, in order to reexamine the question whether these fossils are to form the types of two distinct genera, according to my observations; or if they are to be considered as of one genus, but specifically differing from each other, which seems to be the opinion of those persons, who are influenced in drawing their conclusion from the apparent similarity of the external form of these shells.

Yours very truly,

D. E. Rüppell.

X. *On the former Extent of the Persian Gulf, and on the Non-identity of Babylon and Babel; in Reply to* Mr. Carter. *By* C. T. Beke, *Esq., F.S.A.*

[Continued from page 515, and concluded.]

On the subject of the unlikelihood that Babel would, in the earliest post-diluvian ages, have been built in the lowlands of the Euphrates (supposing them to have existed), Mr. Carter observes that "no allusion is made to his answer that the cities and settlements of a hot climate, *and more particularly those of an early people*, are of necessity fixed in such places." In a former paper of mine* I stated that a portion of Mr. Carter's arguments, which I then combated, was "founded upon the assumption that society in the time of Noah existed in a state of infancy as regarded its culture and knowledge;" he having remarked in the paper to which that was a reply†,

* Lond. and Edinb. Phil. Mag., vol. iv. p. 281. † *Ibid.*, p. 182.

that "navigation *in early society* is usually performed in boats made of a single tree." In his answer*, however, that gentleman says, "Mr. Beke has here mistaken my meaning. I have not expressed any opinion respecting the general culture and knowledge of mankind at that period. My remark was confined to their navigation only." Under these circumstances I was desirous of avoiding, if possible, anything which might bring us into discussion respecting the state of "early society;" especially as it was manifest, from more than one expression in Mr. Carter's second paper, that his opinion and mine were not at all likely to coincide. But as it is necessary that I should refer to the subject, I must be permitted to say that I do not consider the instance adduced from the evidence of Col. Chesney of the villages of the half-savage residents on the banks of the Euphrates, which are frequently washed away by the stream, as at all analogous to the *cities*—not merely "inclosed lands," or "small villages," or "little settlements," —of mankind in the post-diluvian ages. As I have in the third chapter of my *Origines Biblicæ* explained at length my views in connexion with this point†, I will merely remark that the nearest analogy to the earliest post-diluvians is probably to be found in the European settlers in the New World, both people having sprung from a previous civilized and artificial state of society. Now we see that even in the tropical portions of the Americas, where they have quite as great a "notion of the value of water" as the inhabitants of the East can possibly have, although for the purposes of *foreign* commerce (which the earliest post-diluvians had not,) they have in some cases fixed upon situations like that of the deadly New Orleans, yet these offsets from the Old World have not, "heedless of all the good reasons to the contrary, chosen for their settlements every such impracticable spot they could find."

But is there not an entire fallacy in the argument as to the alleged value of water in hot climates? The value of water in such climates is, properly speaking, principally of an *artificial* character, arising from the *scarcity* of that element. In those cases in which it is to be procured in plenty, the general habits of the people show that the *real* value is not so great as it is in more temperate regions. Of course it is not intended to be denied that water has a considerable real value in the former countries also; but if it be *on account of that value* (for so I understand Mr. Carter's argument,) that the people dwelling near

* Lond. and Edinb. Phil. Mag., vol. v. p. 244.

† See also a paper entitled "Views in Ethnography," &c., published in Jameson's New Edinburgh Philosophical Journal, vol. xviii. pp. 285—296, in which some of my opinions on this topic are yet further elucidated.

the Euphrates are induced to erect their habitations in "impracticable spots," where they are constantly liable to be washed away, should we not find the inhabitants of Mesopotamia generally settled on the banks of the two rivers instead of being dispersed over the interior, and ought we not likewise to see the people of Arabia flocking to the shores of the Euphrates, and employed in "following their villages afloat to arrest the materials of their dwellings," instead of remaining in their unwatered deserts? As to the mere erection of cities in plains, by the sides of rivers, and even in marshy situations, this is no peculiarity whatever of hot climates, but is equally prevalent even in the coldest. When opportunities occur, settlements are, in all countries, usually made on the banks of rivers; but then they are not in the first instance founded from choice in *impracticable* spots, but are placed on the higher grounds, where they can enjoy all the benefits of the supply of water without being liable to the tides and ordinary floods; it being afterwards only, as population increases, that the buildings are extended over the lower lands, from which the waters have been banked out.

Mr. Carter asserts, however, that "the earliest settlements on record were, in fact, fixed in such places;" and he instances the cities of Egypt, those of the vale of Siddim, and also Nineveh in particular; which last city, he says, was built "in the lowlands of the Tigris, a valley eight or ten miles broad, and where the floods were so great that of old it was like a pool of water," referring to Nahum, ii. 8. as an authority for the fact. I regret to be obliged to observe that he has altogether mistaken the meaning of this text. In our authorized version (which alone he would seem to have consulted,) it is unquestionably said, "But Nineveh is of old *like a pool* of water;" but by this expression our venerable translators evidently intended, not a natural, but *an artificial pool*, as in 2 Kings, xx. 20. Neh. iii. 15. Eccl. ii. 6. and many other places, where the same expression occurs: and this, in fact,—or *a fish-pond* in particular, as in Cant. vii. 4.—is the real meaning of the word בְּרֵכָה*, which is used by the prophet with reference to the immense population of Nineveh:—"that great city, wherein were more than six score thousand persons that could not discern between their right hand and their left hand†." The comparison of

* The Arabic بِرْكَةٌ has the same signification. In its primitive meaning it is, a reservoir of water at which the camels *kneel* (בָּרַךְ), *badach*, to drink: literally, therefore, a *kneeling-place*.

† Jonah iv. 11.

the city may be either to a *pool* (reservoir) *full of water*, or to a *fish-pond swarming with fish*: the context shows that the general idea of *fulness* cannot be mistaken:—"But Nineveh is of old [full or swarming] like a pool of water; *yet they shall flee away*: stand, stand; but none shall look back......*she is empty, and void, and waste**."

As regards the "cities of the plain," it may be perfectly true that Jordan "overflowed all his banks in the time of harvest," without its thence following that those cities were affected by the inundation any more than Jericho, and the other cities which were, and still are, erected along the valley of that river.

On the subject of Egypt, the opinion is already expressed at length in my *Origines Biblicæ*, that the Mitzraim of Scripture was nowhere within the valley of the Nile: it becomes unnecessary, therefore, that I should here say anything upon this particular point. But leaving this quite out of the question, we find from the earliest writer of profane history, that even within Egypt itself, the natives—a totally distinct people (I may just remark,) from the Mitzrites of *the north*, in the neighbourhood of Canaan,—came originally from the higher lands of *the south*, and that it was only "as their country became more extensive that some remained in their primitive places of residence, *whilst others migrated to a lower situation; whence it was that the Thebaid went formerly under the name of Egypt* †."

Independently therefore of "all the good reasons to the contrary", the opinion is unfounded,—for what evidence we have is directly opposed to such an opinion,—that "the earliest people on record chose for their settlements every such impracticable spot they could find."

On separate grounds it is yet contended, that the Babel of Genesis was actually built in the lowlands of the Euphrates, for that "Isaiah xxiii. 13. seems distinctly to identify Nimrod's Babel and Babylon," from which text it is inferred that "the Assyrian Nimrod founded Babel, formed into a social community the remnant of the people scattered and broken at the Dispersion, and the Assyrian of later days set up the towers and raised up the palaces thereof;" and it is subsequently argued as follows: "As Shinar must have embraced no very extensive range, and Nimrod's Babel (or Babylon) and the

* Diodati's Italian translation represents the idea of the original far better than our English version: "Or Nineve è stata, dal tempo che è in essere, come un *vivaio d' acque*: ora fuggono essi: fermatevi, fermatevi; ma niuno si rivolge.:...Ella è votata, e spogliata, e desolata."

† Euterpe 15.

Babylon of the prophets were in it, that we have here two places of the same name,—both moreover built in the very infancy of society, and both by 'the Assyrian,' is an inference not sanctioned by true historical construction." But upon what construction, I would venture to inquire, is the former part of this text of Isaiah, "the Assyrian founded it," to be referred to "the Assyrian *Nimrod*," and the latter portion of the same text, "they set up the towers thereof," to be applied to "the Assyrian *of later days*"? And again, upon what construction is the epithet "the Assyrian" given to Nimrod, who, according to Mr. Carter's own interpretation of Gen. x. 10, was *no Assyrian at all*, but a foreigner who founded an intrusive monarchy in the land of Asshur?

In citing this text, "Behold the land of the Chaldeans: this people was not, till the Assyrian founded it for them that dwell in the wilderness: they set up the towers thereof, they raised the palaces thereof, *and he brought it to ruin*," (This last portion of the verse is omitted to be quoted,) Mr. Carter says, "No one can doubt that this refers to the celebrated Babylon." Unfortunately, it happens that a very great many do doubt it, among whom it will be sufficient to name R. Jonathan ben Uzriel, the translator of the Peshito, Jerome, Theodosion, Saadias, Jarchi, and Kimchi, without enumerating a whole host of modern translators and commentators. In fact, Gesenius says that the last three portions of this verse are, almost unanimously, referred *to the destruction of Tyre by the Chaldeans**. As rendered in our authorized version (agreeing with Diodati) the meaning is not all obvious; but our translators could certainly never have intended to refer it (as Mr. Carter reads their words,) *merely* to the foundation of Babylon, or else what is the meaning of the last portion of it, "and he brought it to ruin"? Mr. Carter does not, indeed, cite these words, but it is impossible to detach them from those which precede them; and I apprehend that gentleman would find a difficulty in showing their applicability to *the founder of Babylon*, whether that founder were Nimrod "in the infancy of society," or "the Assyrian of later days †."

* "Die drei letzten Versglieder beziehen sich fast ohne Widerspruch auf die Zerstörung von Tyrus durch die Chaldäer."—*Commentar. u. d. Jesaias; in loc.*

† In explanation of this passage it may be allowed me to remark, that the "towers" have generally been understood to be the *war-towers* of the besieging Chaldeans, and the "palaces" to be *those of Tyre* which were, either (according to Saadias and Theodosion,) "raised (or roused) up" with affright or tumult through the siege, or (according to the Targum of Jonathan, the Vulgate, Jarchi, and Kimchi,) demolished by the enemy. I have for several years past had this very difficult and still unintelligible text

The greatest weight of authority unquestionably is for referring the former half of this verse to the foundation of the Chaldean Babel or Babylon by the Assyrians;—not, however, by any one individual, "the Assyrian," but אַשּׁוּר (*Asshúr*), that is, the people of Asshur, or *the Assyrian nation generally*. Yet this is so far from aiding Mr. Carter's opinion as to that city's having been "a settlement of the earliest antiquity," "made in the very infancy of society," that it proves directly the contrary; for if the Babel of Genesis "was not till the Assyrians founded it," it is manifest that the Assyrians must already have existed as a nation, and that some time—probably a considerable time: "...this people *was not till* the Assyrians founded it,"—had first elapsed. And not merely so, but it would also seem completely to establish the non-identity of Babel and Babylon; for (as Mr. Carter himself observes,) "why was Babel" said in Gen. x. 10. to be "only *the beginning* of his kingdom, if we are not to understand that it was Nimrod who also builded Nineveh"? But if so, it follows that Babel must have been erected *before* he "went out into Assyria"; whilst, on the other hand, the prophet tells us that the Chaldean Babel "was not till the Assyrians founded it"—in other words, that it did not exist until *after* the establishment of the Assyrian Empire by Nimrod.

Without however placing entire dependence upon this argument, on account of the darkness of the text of Isaiah, I must most distinctly assert, that this text does not (nor indeed does any other throughout the sacred volume,) in the slightest manner connect the two cities: the whole that can be inferred from it with respect to the age of Babylon is, that that capital was already in the prophet's time a great flourishing city. It is from the Jews of the Captivity alone that we derive the prevalent erroneous notion with respect to the identity of the Babel of Genesis with the far more recent Babylon (Babel). From the similarity of the names of the two cities and the existence in the latter of the famed tower of Belus, they (perhaps not unnaturally,) fell into the error of imagining them to be the same; and hence arose all the fables respecting the tower of Babel, for which not the slightest ground exists in the pages of Scripture. But that Babylon could not have been founded near the time of Nimrod—that in fact it did not exist until a comparatively late period, must assuredly be the only

under my consideration, and I am willing to regard it, *on the whole*, as an authority in favour of my hypothesis as to the late foundation of Babylon, although I must, at the same time, confess that I am far from being satisfied with any particular interpretation of it which has yet been given.

inference which is to be drawn from the circumstance that—whilst Assyria (Asshur) is mentioned in the very earliest portions of the particular history of the Israelites and their immediate progenitors*, and is brought into direct connexion with the history of the kingdom of Israel in or before the time of Jeroboam II. †, when Nineveh had "of old" been a great and populous city,—Babylon itself, a city nearer to Canaan than Nineveh, and indeed almost in the road between them, is not even mentioned until some time afterwards, although *Shinar* was known to the Israelites from the earliest period ‡. And it is most worthy of remark that even when Babylon is first referred to in the scriptural history, it is merely as one of the places from which *the king of Assyria* brought inhabitants to repeople the country of the captive Israelites §, and as the city where the same monarch carried Manasseh king of Judah into captivity ||. It is true that Babylon is mentioned a short time previously to the latter of these two events, in the time of Manasseh's father ¶, as having had its own ruler; but the comparison of the whole scriptural history evinces that the kings of Babylon were *Assyrian viceroys*, and not independent sovereigns:—that this was actually the case is expressly recorded by Alexander Polyhistor **.

Upon the hypothesis of the distinction between Babel and Babylon, and of the late foundation of the latter city, the scriptural history, as connected therewith, becomes quite intelligible; which otherwise it certainly is not. We can also fully understand how Herodotus, in mentioning the Assyrian empire, should describe Babylon merely as *one* of its great cities, which only "became the royal residence after the destruction of Nineveh ††." In entire accordance with the same hypothesis is that historian's statement that the *Assyrian* Semiramis (whom he makes to have preceded Nitocris only five generations,) "raised certain mounds [at Babylon]...*till when* the whole plain was subject to inundations from the river ‡‡;" and yet more particularly so is that of Megasthenes, who tells us that "from the beginning *all things were water, called the*

* See Gen. xxv. 18. Numb. xxiv. 22.

† Compare Jonah iii. 2. *et seq.*, and 2 Kings xiv. 25.

‡ See Josh. vii. 21: the expression, which in our authorized version is rendered "a *Babylonish* garment," is in the original אַדֶּרֶת שִׁנְעָר (*addéreth Shinhár*), "a garment (mantle) *of Shinar*."

§ See 2 Kings xvii. 24.

|| 2 Chr. xxxiii. 11.

¶ Isa. xxxix. 1.

** *Euseb. Arm. Chron.* 42, in Cory's *Ancient Fragments,* 2nd Edit. pp. 61, 62.

†† Clio, 178.

‡‡ Clio, 184.

sea, and that Belus caused this state of things to cease, and appointed to each its proper place, and surrounded Babylon with a wall*."

As to the supposition that "Shinar must have embraced no very extensive range," there is not the slightest ground for it. Like *Asia*—which name was gradually extended from a small portion of Lydia, first to Asia *Minor*, and then to an entire quarter of the globe,—and many other names of countries, it may well have had very different applications at different times; and in the latter portions of the Scriptural history the same name was probably given to the whole country beyond the Euphrates, of the north-western portion of which the early Shinar was only a small division.

I have at present to add but little on the geological portion of the subject†. Were the "power of the Euphrates, Tigris, and neighbouring streams to form new lands and expel the ocean," a solitary case and opposed to the usual course of nature, Mr. Carter might justly characterize it as "extraordinary," and might even be excused for imagining it to be "supposed"; but seeing that *all rivers on the face of the earth* under similar, or even yet less advantageous circumstances, do actually possess this power‡, it would indeed be *extraordinary* if these rivers alone were to be excepted from the rule. Even an instance of the formation of *rock* within the limits of the most recent fluviatile deposits is furnished us

* Πάντα μὲν ἐξ ἀρχῆς ὕδωρ εἶναι, θάλασσαν καλεομένην. Βῆλον δὲ σφέα παῦσαι, χώρην ἑκάστῳ ἀπονείμαντα, καὶ Βαβυλῶνα τείχει περιβαλεῖν. *Euseb. Præp. Evan.* lib. 10. in Cory's *Anc. Frag.* p. 45. It is deserving of attention that Megasthenes further states that when Nebuchadnezzar rebuilt Babylon "he constructed dykes against the irruptions of the Persian Gulf:" ἐπετείχισι δὲ καὶ τῆς Ἐρυθρῆς θαλάσσης τὴν ἐπίκλυσιν. (*ibid.*),—a precaution which would seem to have been rather needless and extraordinary, unless the sea then approached much nearer to the city than it does at present. The lakes and marshes of the Euphrates would, doubtless, at that period have extended very far northward, and might well have been "called the sea."

† I must remark that my paper in the Number of your Journal for July last, (1835,) was (as, indeed, you are well aware,) not written for the pages of this Journal, it having been destined by me for an entirely different purpose; and it was consequently not meant as an answer to Mr. Carter's arguments. In consequence, however, of my subsequently requesting you to give it insertion "as a continuation of my former paper," that gentleman was, I allow, entirely warranted in regarding it in the light of a further answer. This explanation will account for the want of connexion which exists between that paper and my former communications,—a want of connexion which is commented upon by my opponent.

‡ See the many instances adduced by Mr. Lyell in his *Principles of Geology*, vol. i. ch. 13, 14.

by Mr. Lyell *; so that the discovery even of rock by Alexander, would prove literally nothing against the fact of the increase of land in the locality in dispute.

Far as it is from being my wish to dogmatize upon a subject which is unquestionably attended with many difficulties; I am even willing to admit that the gain of land to the extent originally contended for by me, although far from being disproved by Mr. Carter, is also far from being *proved* by me. But this is not the point principally in dispute, which (independently of the grounds upon which the identity of Babel and Babylon is denied by me,) must, in the first instance at least, confine itself to the question,—Has or has not a change of such importance taken place as *materially* to affect the geography of the localities in question, and such, therefore, as to render the descriptions of ancient writers inapplicable to the present state of the country? I am willing to believe that, upon further consideration, Mr. Carter himself will see reason to admit this to be the case. For myself I only wait for sufficient evidence, or even reasonable arguments, to relinquish any portion of my hypothesis:—which hypothesis I am wedded to in as much only as I believe it to approach the truth, and which, therefore, I shall most cheerfully abandon so far as it can be shown to be incorrect. I am, Gentlemen, yours, &c.

Bremen, Feb. 3, 1836. CHARLES T. BEKE.

XI. *Letter from* Baron von Humboldt *to His Royal Highness the Duke of* Sussex, K.G., *President of the Royal Society of London, on the Advancement of the Knowledge of Terrestrial Magnetism, by the Establishment of Magnetic Stations and corresponding Observations.*†

SIR,

THE generous interest taken by Your Royal Highness in the advancement of human knowledge, encourages me to hope for the favourable reception of the request which with respectful confidence, I now venture to address to you. I take the liberty of soliciting your attention to the labours requisite for the investigation, by precise means, almost constantly employed, of the variations of *terrestrial magnetism*. By obtaining the cooperation of a great number of zealous observers, provided with instruments of similar construction, M. Arago,

* "That a great proportion, at least, of the new deposit in the delta of the Rhone consists of *rock*, and not of loose incoherent matter, is perfectly ascertained. In the museum at Montpellier is a cannon taken up from the sea near the mouth of the river, imbedded in a crystalline calcareous rock." —*Principles of Geology*, vol. i. p. 234, 1st edit.

† We translate this letter from Schumacher's *Astronomische Nachrichten*, No. 306, which has been kindly communicated to us for the purpose.

Mr. Kupffer, and myself have succeeded in the last eight years in extending these researches over a very considerable part of the northern hemisphere. Permanent *magnetic stations* being now established from Paris to China, following towards the east the parallels from 40° to 60°, I feel myself justified in soliciting, through the intervention of Your Royal Highness, the powerful cooperation of the Royal Society of London, to sanction this enterprise, and also to promote its success by the establishment of new stations, as well in the vicinity of the magnetic equator as in the temperate part of the southern hemisphere.

An object which is equally important whether it be considered in connexion with the physics of the earth or the improvement of nautical science, has a double claim upon the attention of a Society, which has from its commencement, with constantly increasing success, cultivated the vast field of the exact sciences. Our information respecting the progressive development of the knowledge which we possess of *terrestrial magnetism* must be indeed imperfect, if we are ignorant of the numerous valuable observations which have been made at different epochs, and are still being made, in the British isles, and in various parts of the equinoctial zone subject to the same empire. Our present object is to render these observations more useful, that is, better adapted to manifest great physical laws, by coordinating them according to a uniform plan, and connecting them with the observations now in progress upon the continent of Europe and Northern Asia.

Having been much occupied during my travels in the equinoctial regions of America, during the years 1799—1804, with the phænomena of the intensity of the magnetic forces, and the inclination and declination of the magnetic needle, on my return to my own country I conceived the design of examining the progress of the *horary variations of the declination*, and the *perturbations* to which it is liable, by employing a method which, I believe, has never yet been followed upon an extensive scale. In a large garden at Berlin, during the years 1806 and 1807, particularly at the period of the equinoxes and solstices, I measured the angular alterations of the magnetic meridian, at intervals of an hour, often of half an hour, without interruption during four, five, and six days, and as many nights. Mr. Oltmanns, whose numerous calculations of geographical positions have recommended him to the notice of astronomers, kindly shared with me the fatigues of these labours. The instrument which we employed was a magnetic telescope (*lunette aimantée*) of Prony, capable of being reversed upon its axis, suspended according to the method of Coulomb, placed in a glass frame, and directed towards a very distant meridian mark, the divisions of which, illuminated during the night, indicated even six or seven seconds of horary variation. In verifying the habitual regularity of a *nocturnal period*, I was struck with the frequency of the perturbations, especially of oscillations the amplitude of which extended beyond all the divisions of the scale, and which occurred repeatedly at the same hours before sunrise, and the violent and accelerated movements of which could not be attributed to any accidental mechanical cause. These vagaries of the needle, the almost periodical return of which has recently been confirmed by Mr. Kupffer in the narration of his Travels in the Caucasus, appeared to me the effect of a reaction of the interior of the earth towards the surface; I should venture to say, of *magnetic storms*, which indicate a rapid change of tension. From that time it has been my desire to establish on the east and west of the meridian of Berlin apparatus similar to my own, in order to obtain corresponding observations made at great distances and at the same hours; but the political tempest of Germany, and my hasty departure for France, whither I was sent by the Government, delayed for a length of time the execution of this project. Fortunately my illustrious friend M. Arago, after his return from the coasts of Africa and the prisons of Spain, undertook, I think about the year 1818, a series of observations upon magnetic declinations at the Observatory of Paris, which, made daily at intervals uniformly fixed, and continued upon the same plan to the present day, are considered, with regard to their number and mutual connexion, superior to everything that has been attempted in this kind of physical investigations. Gambey's apparatus, which is employed, is of perfect execution. Provided with micrometers and microscopes, it may be employed with more certainty and convenience than Prony's instrument, which is attached to a strong magnetized bar of $20\frac{1}{4}$ inches in length.

During the progress of these observations M. Arago has discovered, and proved by numerous examples, a phænomenon which differs essentially from the observation made by Prof. Hiorter at Upsal in 1741. He has discovered not only that the *Auroræ boreales* disturb the regular progress of the horary declinations there when they are not visible, but also that early in the morning, often ten or twelve hours before the luminous phænomenon is developed in a very distant place, its appearance is announced by the particular form presented by the curve of the diurnal variations, that is, by the value of the *maxima* of elongation of the morning and night. Another new fact was manifested in the perturbations. Mr. Kupffer having established at Cazan, nearly the eastern limit of Europe, one of Gambey's compasses, exactly similar to that employed by M. Arago at Paris, the two observers were convinced by a certain number of corresponding measures of horary declination, that, notwithstanding a difference of longitude of more than 47°, the perturbations were isochronous. They were like signals which from the interior of the earth simultaneously arrived at its surface on the borders of the Seine and the Wolga.

When in 1827 I again fixed my residence at Berlin, my first care was to renew the series of observations which I had made at short intervals during the days and nights of the years 1806 and 1807. I endeavoured at the same time to generalize the means of simultaneous observations, the accidental employment of which had just produced results so important. One of Gambey's compasses was placed in the *magnetic pavilion*, in which no portion of iron was introduced, which had been erected in the middle of a garden. Regular observations could not commence till the autumn of 1828. Being called, in the spring of 1829, by His Majesty the Emperor of Russia, to undertake a mineralogical tour in the North of Asia and on the Caspian Sea, I had an opportunity rapidly to extend the line of stations towards the east. At my request the Imperial Academy and the Curator of the University of Cazan erected *magnetic houses* at St. Petersburgh and Cazan. In a committee of the Imperial Academy, at which I had the honour of presiding, a discussion took place on the immense advantages, with regard to our knowledge of the laws of terrestrial magnetism, presented by the vast extent of country limited on one side by the curve without declination of Doskino, (between Moscow and Cazan, or with more precision, according to M. Adolphe Erman, between Osablikowo and Doskino, in lat. 56° 0′, and long. 40° 36′ east of Paris,) and on the other, by the curve without declination of Arsentchewa near Lake Baikal, which is believed to be identical with that of Doskino, with a difference of meridians of 63° 21′. The Imperial department for Mines having generously concurred in the same object, *magnetic stations* have been successively established at Moscow, Barnaoul, the astronomical position of which I find to be at the foot of Altai, in lat. 53° 19′ 21″, long. 5^h 27′ 20″ east of Paris, and at Nertschinsk. The Academy of St. Petersburgh has done still more, and has sent a courageous and clever astronomer, M. George Fuss, the brother of its perpetual secretary, to Pekin, and has procured the erection there of a *magnetic pavilion*, in the convent garden of the monks of the Greek church. This undertaking cannot be mentioned without recalling the fact, that, according to the *Penthsaoyani*, a medical natural history composed under the Soung dynasty, nearly four hundred years before Christopher Columbus and the natives of Europe had the least idea of magnetic declination, the Chinese suspended the needle by means of a thread, to allow it perfect freedom of motion; and that they knew that when thus suspended, according to the method of Coulomb, (as in the Jesuit Lana's apparatus in the seventeenth century,) the needle declined to the south-east, and never rested at the true south point. Since the return of M. Fuss, M. Kowanko, a young officer of mines, whom I had the pleasure to meet in the Oural, continues the observations of horary declination, corresponding to those of Germany, St. Petersburgh, Cazan, and Nicolajeff in the Crimea, where Admiral Greigh has established one of Gambey's compasses, the care of which is confided to the director of the Observatory, Mr. Knorre. I have also obtained the establishment of a magnetic apparatus at the depth of thirty-five fathoms in an adit in the mines of Freiberg in Saxony, where Mr. Reich to whom we are indebted for his valuable labours upon the mean temperature of the earth at different depths, is assiduously engaged in making observations at regulated intervals. M. Boussingault, who neglects nothing which is calculated to advance the progress of the physics of the earth, has sent us from South America observations of horary declination made at Marmato, in the province of Antioquia, in north lat. 5° 27′, in a place where the declination is eastern, as at Cazan and Barnaoul in Asia; while on the north-western coasts of the new continent, at Sitka in the Russian settlements, Baron von Wrangel, also provided with one of Gambey's compasses, has taken part in the simultaneous observations made at the time of the solstices and equinoxes. A Spanish admiral, M. de Laborde, having been informed of a request that I had made to the Patriotic Society of the Havannah, had the kindness, unsolicited, to desire me to send him instruments proper for determining with precision the inclination, the absolute declination, and the horary variation of declination and intensity of the magnetic forces. The valuable instruments desired, exactly similar to those in the possession of the Observatory of Paris, arrived in safety in the island of Cuba; but the alteration in the maritime command at the Havannah, and other local circumstances, have hitherto prevented the employment of them, and the establishment of a magnetic station under the tropic of Cancer. The same has also occurred up to the present time with regard to one of Gambey's compasses which M. Arago had caused to be erected, at his own expense, to obtain observations in the interior of Mexico, where the soil is elevated six thousand feet above the level of the sea. Lastly, during my last residence in Paris, I had the honour of proposing to Admiral Duperré, Minister for marine affairs, the establishment of a magnetic station in Iceland. The proposal was received with the utmost eagerness, and the instrument, which is already ordered, will be deposited during the present summer at the port of Reikiawig, when the expedition which has been sent to the north in search of M. de Blosseville and his companions in misfortune returns to Iceland to continue its scientific labours. There cannot be any doubt that the Danish Government, which protects with generous ardour astronomy and the advancement of nautical science, will favour the establishment of a magnetic station in one of its provinces bordering on the polar circle. At Chili also M. Gay has made a great number of corresponding horary observations, according to the instructions of M. Arago.

I have entered upon this long and minute historical detail, to show how far I have hitherto succeeded, in conjunction with my friends, in extending the number of simultaneous observations. After my return from Siberia, Mr. Dove and I published, in 1830, a graphic delineation of the curves of horary declination of Berlin, Freiberg, Petersburgh, and Nicolajeff in the Crimea, to show the parallelism of these lines, notwithstanding the distance of the stations and the influence of extraordinary perturbations. In the comparison of the observations of St. Petersburgh and Nicolajeff, use has been made of observations taken at the very small intervals of twenty minutes. It must not, however, be imagined that this parallelism of inflections always exists in the horary curves. We have found that even in places very near to each other,—for instance, at Berlin and in the mines of Freiberg,—the magnetic reactions from the interior to the surface of the earth are not always simultaneous; that one of the needles presents considerable perturbations, while the other preserves that regularity, which under each meridian is the function of the true time of the place. In the memoir published in 1830, I proposed the following periods for simultaneous observations at all the stations.

March 20th and 21st. May 4th and 5th. June 21st and 22nd. Aug. 6th and 7th. Sept. 23rd and 24th. Nov. 5th and 6th. Dec. 21st and 22nd.	From four o'clock in the morning of the first day, to midnight of the second day. The observations to be continued at each *magnetic station* during the day and night, at intervals not exceeding one hour.

As several observers situated upon the line of the stations have found these periods too near to each other, it has been thought advisable to insist in preference upon the time of the solstices and equinoxes.

England, from the time of William Gilbert, Graham, and Halley to that of the more recent exertions of Messrs. Gilpin, Beaufoy (at Bushy), Barlow, and Christie, has produced a rich collection of materials applicable to the discovery of the physical laws which regulate the variation of the magnetic declination, either in one place according to the different hours and seasons, or at various distances from the magnetic equator and the lines without declination. Mr. Gilpin made observations during twelve hours every day for more than seven months. The numerous observations of Colonel Beaufoy were regularly published in Thomson's Annals. The memorable expeditions to the most inhospitable regions of the North have furnished Messrs. Sabine, Franklin, Hood, Parry, Henry Foster, Beechey, and James Clarke Ross with a rich harvest of important observations. Physical geography is indebted for a considerable increase of knowledge respecting terrestrial magnetism and meteorology to the attempts which have recently been made to determine the form of the north-west passage or strait; and to the perilous explorations of the frozen coasts of Asia by Captains Wrangel, Lütke, and Anjou. During the progress of these noble efforts, an unexpected impulse has been given to the physical sciences by the light thrown upon them by a branch of natural philosophy the theoretical progress of which for two centuries had been extremely slow. Such has been the effect of the grand discoveries of Oersted, Arago, Ampère, Seebeck, and Faraday upon the nature of electro-magnetic forces. Excited by the talents and ingenious exertions of learned travellers cooperating for the promotion of one object, Messrs. Hansteen, Due, and Adolphus Erman, by the fortunate union of very precise astronomical and physical means, have explored, throughout the immense extent of Northern Asia, the isoclinal, isogonal, and isodynamic curves for very nearly the same epoch. When speaking of this great project, long since conceived and proposed by Mr. Hansteen, I ought, perhaps, to pass over in silence the observations upon magnetic inclination which I made upon the rarely-visited frontier of Chinese Dzoungarie and on the coasts of the Caspian Sea, published in the second volume of my *Fragmens Asiatiques*. My learned countryman Mr. Adolphus Erman, who embarked at Kamtschatka and returned to Europe by Cape Horn, had the rare advantage of continuing throughout a long voyage the measure of the three manifestations of terrestrial magnetism at the surface of the globe. He employed the same instruments and the same methods which he had made use of from Berlin to the mouth of the Oby, and thence to the Sea of Okhotsk.

That which characterizes our epoch, at a time distinguished by grand discoveries in optics, electricity, and magnetism, is the possibility of connecting phænomena by the generalization of empirical laws, and the mutual aid afforded by sciences which had long remained isolated. At the present day simple observations upon horary declination or magnetic intensity, made simultaneously in situations very distant from each other, reveal, so to speak, what passes at profound depths in the interior of our planet, and in the superior regions of the atmosphere. The luminous emanations, the polar explosions which accompany the magnetic storm, appear to follow great changes in the habitual or mean tension of terrestrial magnetism.

It would tend greatly to promote the advancement of the mathematical and physical sciences if, under the Presidency and auspices of Your Royal Highness, the Royal Society of London, to which I make it my boast to have belonged for twenty years, would exert its powerful influence to extend the *line of simultaneous observations*, and to establish *permanent magnetic stations*, either in the region of the tropics, on each side of the magnetic equator, the proximity of which necessarily diminishes the amplitude of the horary declinations, or in the high latitudes of the southern hemisphere and in Canada. I venture to propose this latter point, because observations upon horary declination made in the vast extent of the United States are still very rare. Those, however, of Salem, in 1810, calculated by Mr. Bowditch, and compared by Arago with the observations of Cassini, Gilpin, and Beaufoy, merit great praise, and might serve as a guide to observers in Canada in investigating whether the declination there does not diminish between the vernal equinox and the summer solstice, contrary to what occurs in Western Europe. In a memoir that I published five years ago, I suggested as magnetic stations extremely favourable to the progress of our knowledge, New Holland, Ceylon, the Mauritius, the Cape of Good Hope (rendered illustrious by the labours of Sir John Herschel), St. Helena, and some point on the eastern coast of America to the south of Quebec. In the last century, in the years 1794 and 1796, an English traveller, Mr. Macdonald, made some new and important observations upon the diurnal motion of the needle at Sumatra and St. Helena, which have since been confirmed and extended upon a large scale in the scientific expeditions of Captains Freycinet and Duperrey;

the former having the command of the sloop Uranie from 1817 to 1820, and the latter, who has six times crossed the magnetic equator, commanding the sloop Coquille from 1822 to 1825. To promote the rapid advancement of the theory of terrestrial magnetism, or at least to establish with more precision empirical laws, it is necessary at the same time to prolong and to vary the lines of *corresponding observations*; also to distinguish in observations of horary variations, what arises from the influence of the seasons, of serene and cloudy weather and of abundant rains, of the hours of day and night, and of the true time at each place, that is, from the influence of the sun, and of all isochronous influences at the different meridians. To these observations of horary declination must be united those of the annual movement of the *absolute declination*, of the *inclination of the needle*, and of the *intensity of the magnetic forces*, the increase of which from the magnetic equator to the poles is unequal in the Western American and the Eastern Asiatic hemispheres. All these *data*, indispensable bases for the future theory, can only acquire certainty and importance by the means of establishments which shall remain permanent for a great number of years, of Physical Observatories in which the investigation of numerical elements may be repeated at settled intervals of time, and with similar instruments. Travellers who cross a country in but one direction and at one epoch merely prepare the way for an undertaking which should embrace the complete delineation of the lines without declination at intervals equally distant; the progressive removal of the points of intersection of the terrestrial and magnetic equators; the changes of form in the isogonal and isodynamic lines; and the influence upon the slow or accelerated movement of the curves, which indubitably arises from the configuration and articulation of the continents. It must be considered fortunate if the isolated labours of travellers, whose cause it is my office to plead, have contributed to give animation to a species of investigation which is the work of centuries, and which requiries the concurrence of numerous observers, distributed according to a plan arranged after mature consideration, under the direction of several of the great scientific centres of Europe. The directors should not always confine themselves to the narrow limits of the same instructions, but they should vary them freely in adaptation to the progressive state of physical science, and the improvement of instruments and methods of observation.

When soliciting Your Royal Highness to condescend to communicate this letter to the illustrious Society over which you preside, it is not in any degree my office to inquire, which are the magnetic stations that merit preference at the present time, or that local circumstances may admit of establishing. To have solicited the concurrence of the Royal Society of London will be sufficient to give new life to a useful enterprise in which I have been engaged for very many years. I venture simply to express the wish that, should my proposition be received with indulgence, the Royal Society would enter into direct communication with the Royal Society of Göttingen, the Royal Institute of France, and the Imperial Academy of Russia, in order to adopt measures the best adapted for the combination of what it may be proposed to establish with what already exists upon a *very* considerable extent of surface. Perhaps also measures might be previously concerted for the publication of *partial observations*, and also (if the calculation would not require too much time, and too much retard the communications,) of the *mean results*. One of the happy effects of civilization and the progress of reason is, that when addressing learned societies, their willing concurrence may be relied upon if the object for which it is solicited tends to promote the advancement of the sciences or the intellectual development of humanity.

Labours of astonishing precision have been performed, within the last few years, with instruments of extraordinary power, in a magnetic pavilion of the Observatory of Göttingen, which are well worthy of the attention of philosophers, as they offer a more precise method of measuring the horary variations. The magnetized bar is of much larger dimensions than even the bar of Prony's magnetic telescope; and the extremity is furnished with a mirror, in which are reflected the divisions of a scale which is more or less distant, according to the angular value desired to be given to these divisions. By the employment of this improved method the necessity for the observer's approaching the magnetized bar is obviated, and by preventing the currents of air produced by the proximity of the human body, or during the night, of a lamp, observations may be made in the smallest intervals of time. The great geometrician Mr. Gauss,—to whom we owe this mode of making observations, as well as the means of reducing the intensity of the magnetic force in any part of the earth to an absolute proportion, and the ingenious invention of a *magnetometer* put into motion by a *multiplier of induction*,—published in the years 1834 and 1835 several series of simultaneous observations made with similar apparatus, and at intervals of five or ten minutes, at Göttingen, Copenhagen, Altona, Brunswick, Leipzig, Berlin (where Mr. Encke has already established a very spacious magnetic house, near the New Royal Observatory), Milan, and Rome. Mr. Schumacher's

German Ephemeris (*Jahrbuch für* 1836) proves graphically, and by the parallelism of the smallest inflections of the horary curves, the simultaneity of the perturbations at Milan and Copenhagen, two cities having a difference of latitude of 10° 13′.

Mr. Gauss first made observations at the times which I proposed in 1830, but with the intention of referring the angular dimensions of magnetic declination to the smallest intervals of time. (On the 7th of February 1834, alterations of six minutes of the arc corresponded to a single minute of time.) Mr. Gauss reduced the forty-four hours of simultaneous observations to twenty-four hours; and appointed six [seven ?] periods of the year, viz. the last Saturday of each month consisting of an uneven number of days, for the stations which are provided with his new apparatus. The small magnetized bars which he employs as magnetometers are of four pounds weight, and the large ones of twenty-five pounds. The curious *apparatus of induction* proper to render sensible and measurable the oscillatory movements predicted by a theory founded upon Mr. Faraday's admirable discovery, consists of two bars fastened together, each of twenty-five pounds weight. I thought it proper to mention the valuable labours of Mr. Gauss, in order that those members of the Royal Society of London who have rendered most service to the study of terrestrial magnetism, and who know the localities of the colonial establishments, may take into consideration whether bars of great weight, provided with a mirror, and suspended in a pavilion carefully closed, should be employed in the new stations to be established; or whether Gambey's compass, hitherto uniformly used in our present stations in Europe and Asia, should still be employed. In discussing this question the advantages will undoubtedly be estimated which, in the apparatus of Mr. Gauss, arise from the smaller mobility of the bars by currents of air, as well as from the facility and rapidity with which the angular divisions may be read in very short intervals of time. My desire is only to see the line of magnetic stations extended, whatever be the means by which the precision of the corresponding observations may be attained. I ought also to mention that two accomplished travellers, Messrs. Sartorius and Listing, provided with very portable instruments of small dimensions, have very successfully employed the method of the great geometrician of Göttingen in their excursions to Naples and in Sicily.*

Your Royal Highness will, I hope, excuse the length of this

* An abstract of a memoir by Prof. Gauss in which his apparatus and method of observation are fully described will be found in Lond. & Edinb. Phil. Mag., vol. ii. p. 291, *et seq.*—Edit.

communication; but I thought that it would be useful to unite under one point of view what has been done or proposed in different countries towards the attainment of extensive simultaneous observations upon the laws of terrestrial magnetism.

Accept, Sir, the acknowledgement of the profound respect with which I have the honour of being,

Your Royal Highness's, &c. &c.,

Berlin, April, 1836. ALEXANDER VON HUMBOLDT.

XII. *On a peculiar Voltaic Condition of Iron, by Professor* SCHOENBEIN, *of Bâle; in a Letter to* Mr. Faraday: *with further Experiments on the same Subject, by Mr.* FARADAY, *communicated in a Letter to* Mr. Phillips.

To Michael Faraday, Esq., D.C.L., F.R.S., &c.

SIR,

AS our Continental and particularly German periodicals are rather slow in publishing scientific papers, and as I am anxious to make you as soon as possible acquainted with some new electro-chemical phænomena lately observed by me, I take the liberty to state them to you by writing. Being tempted to do so only by scientific motives, I entertain the flattering hope that the contents of my letter will be received by you with kindness. The facts I am about laying before you seem to me not only to be new, but at the same time deserving the attention of chemical philosophers. *Les voici.*

If one of the ends of an iron wire be made red hot, and af ter cooling be immersed in nitric acid, sp. gr. 1·35, neither the end in question nor any other part of the wire will be affected, whilst the acid of the said strength is well known to act rather violently upon common iron. To see how far the influence of the oxidized end of the wire goes, I took an iron wire of 50 in length and $0'''{\cdot}5$ in thickness, heated one of its ends about $3''$ in length, immersed it in the acid of the strength above men tioned, and afterwards put the other end into the same fluid. No action of the acid upon the iron took place. From a similar experiment made upon a cylindrical iron bar of $16'$ in length and $4'''$ diameter the same result was obtained. The limits of this protecting influence of oxide of iron with regard to quantities I have not yet ascertained; but as to the influence of heat, I found that above the temperature of about 75° the acid acts in the common way upon iron, and in the same manner also, at common temperatures, when the said acid contains water beyond a certain quantity, for instance, 1, 10, 100, and even 1000 times its volume. By immersing an iron wire in nitric acid of sp. gr. 1·5 it becomes likewise indifferent to the same acid of 1·35.

But by far the most curious fact observed by me is, that any number of iron wires may be made indifferent to nitric acid by the following means. An iron wire with one of its ends oxidized is made to touch another common iron wire; both are then introduced into nitric acid of sp. gr. 1·35, so as to immerse the oxidized end of the one wire first into the fluid, and to have part of both wires above the level of the acid. Under these circumstances no chemical action upon the wires will take place, for the second wire is, of course, but a continuation of that provided with an oxidized end. But no action occurs, even after the wires have been separated from each other. If the second wire having become indifferent be now taken out of the acid and made to touch at any of its parts not having been immersed a third wire, and both again introduced into the acid so as to make that part of the second wire which had previously been in the fluid enter first, either of the wires will be acted upon either during their contact or after their separation. In this manner the third wire can make indifferent or passive a fourth one, and so on.

Another fact, which has as yet, as far as I know, not been observed, is the following one. A wire made indifferent by any of the means before mentioned is immersed in nitric acid of sp. gr. 1·35, so as to have a considerable part of it remaining out of the fluid; another common wire is put into the same acid, likewise having one of its ends rising above the level of the fluid. The part immersed of this wire will, of course, be acted upon in a lively manner. If the ends of the wires which are out of the acid be now made to touch one another, the indifferent wire will instantly be turned into an active one, whatever may be the lengths of the parts of the wires not immersed. (If there is any instance of chemical affinity being transmitted in the form of a current by means of conducting bodies, I think the fact just stated may be considered as such.) It is a matter of course that direct contact between the two wires in question is not an indispensably necessary condition for communicating chemical activity from the active wire to the passive one; for any metal connecting the two ends of the wires renders the same service.

Before passing to another subject, I must mention a fact, which seems to be one of some importance. An iron wire curved into a fork is made to touch at its bend, a wire provided with an oxidized end; in this state of contact both are introduced into nitric acid of sp. gr. 1·35 and 30°, so as first to immerse in the acid the oxidized end; the fork will, of course, not be affected. If now a common iron wire be put into the acid, and one of the ends of the fork touched by it, this end will immediately be acted upon, whilst the other end remains passive; but as soon as the iron wire with the oxidized end is put out of contact with the bend of the fork, its second end is also turned active. If the parts of the fork rising above the level of the acid be touched by an iron wire, part of which is immersed and active in the acid, no communication of chemical activity will take place, and both ends of the fork remain passive; but by the removal of the iron wire (with the oxidized end) from the bend of the fork this will be thrown into chemical action.

As all the phænomena spoken of in the preceding lines are, no doubt, in some way or other dependent upon a peculiar electrical state of the wires, I was very curious to see in what manner iron would be acted upon by nitric acid when used as an electrode. For this purpose I made use of that form of the pile called the *couronne des tasses*, consisting of fifteen pairs of zinc and copper. A platina wire was connected with (what we call) the negative pole of the pile, an iron wire with the positive one. The free end of the platina wire was first plunged into nitric acid sp. gr. 1·35, and by the free end of the iron wire the circuit closed. Under these circumstances the iron was not in the least affected by the acid; and it remained indifferent to the fluid not only as long as the current was passing through it, but even after it had ceased to perform the function of the positive electrode. The iron wire proved, in fact, to be possessed of all the properties of what we have called a passive one. If such a wire is made to touch the negative electrode, it instantaneously becomes an active one and a nitrate of iron is formed; whether it be separate from the positive pole or still connected with it, and the acid be strong or weak.

But another phænomenon is dependent upon the passive state of the iron, which phænomenon is in direct contradiction with all the assertions hitherto made by philosophical experimenters. The oxygen at the anode arising from the decomposition of water contained in the acid, does not combine with the iron serving as the electrode, but is evolved at it, just in the same manner as if it were platina, and to such a volume as to bear the ratio of 1 : 2 to the quantity of hydrogen evolved at the cathode. To obtain this result I made use of an acid containing 20 times its volume of water; I found, however, that an acid containing 400 times its volume of water still shows the phænomenon in a very obvious manner. But I must repeat it, the indispensable condition for causing the evolution of the oxygen at the iron wire is to close the circuit exactly in the same manner as above mentioned. For if, *exempli gratiâ*, the circuit be closed with the negative platina wire, not one single bubble of oxygen gas makes its appearance at the positive iron; neither is oxygen given out at it, when the circuit is closed, by plunging first one end of the iron wire into the nitric acid, and by afterward sputting its other end in connexion with the positive pole of the pile. In both cases a nitrate of iron is formed, even in an acid containing 400 times its volume of water; which salt may be easily observed descending from the iron wire in the shape of brownish-yellow-coloured streaks.

I have still to state the remarkable fact, that if the evolution of oxygen at the anode be ever so rapidly going on, and the iron wire made to touch the negative electrode within the acid, the disengagement of oxygen is discontinued, not only during the time of contact of the wires, but after the electrodes have been separated from each other. A few moments holding the iron wire out of the acid is, however, sufficient to recommunicate to it the property of letting oxygen gas evolve at its surface. By the same method the wire acquires its evolving power again, whatever may have been the cause of its loss. The evolution of oxygen also takes place in dilute sulphuric and phosphoric acids, provided, however, the circuit be closed in the manner above described. It is worthy of remark, that the disengagement of oxygen at the iron in the last-named acids is much easier stopt, and much more difficult to be caused again, than is the case in nitric acid. In an aqueous solution of caustic potash, oxygen is evolved at the positive iron, in whatever manner the circuit may be closed, but no such disengagement takes place in aqueous solutions of hydracids, chlorides, bromides, iodides, fluorides. The oxygen, resulting in these cases from the decomposition of water, and the anion (chlorine, bromine, &c.) of the other electrolyte decomposed combine at the same time with the iron.

To generalize these facts, it may be said, that independently of the manner of closing the circuit, oxygen is always disengaged at the positive iron, provided the aqueous fluid in which it is immersed do not (in a sensible manner) chemically act upon it; and that no evolution of oxygen at the anode in contact with iron under any circumstances takes place, if besides oxygen another anion is set free possessed of a strong affinity for iron. This metal having once had oxygen evolved at itself, proves always to be indifferent to nitric acid of a certain strength, whatever may be the chemical nature of the fluid in which the phænomenon has taken place.

I have made a series of experiments upon silver, copper, tin, lead, cadmium, bismuth, zinc, mercury, but none showed any resemblance to iron, for all of them were oxidized when serving as positive electrodes. Having at this present moment neither cobalt nor nickel at my command, I could not try these magnetic metals, which I strongly suspect to act in the same manner as iron does.

It appears from what I have just stated that the anomalous bearing of the iron has nothing to do with its degree of affinity for oxygen, but must be founded upon something else. Your sagacity, which has already penetrated into so many mysteries of nature, will easily put away the veil which as yet covers the phænomenon stated in my letter, in case you should think it worth while to make it the object of your researches.

Before I finish I must beg of you the favour of overlooking with indulgence the many faults I have, no doubt, committed in my letter. Formerly I was tolerably well acquainted with your native tongue; but now, having been out of practice in writing or speaking it, it is rather hard work to me to express myself in English.

It is hardly necessary to say that you may privately or publicly make any use of the contents of this letter.

I am, Sir, your most obedient Servant,

C. T. Schoenbein,

Bâle, May 17, 1836. Prof. of Chem. in the University of Bâle.

Dear Phillips,

The preceding letter from Professor Schoenbein, which I received a week or two ago, contains facts of such interest in relation to the first principles of chemical electricity, that I think you will be glad to publish it in your Philosophical Magazine. I send it to you unaltered, except in a word or two here and there; but am encouraged by what I consider the Professor's permission (or rather the request with which he has honoured me,) to add a few results in confirmation of the effects described, and illustrative of some conclusions that may be drawn from the facts.

The influence of the oxidized iron wire, the transference of the inactive state from wire to wire, and the destruction of that state, are the facts I have principally verified; but they are so well described by Professor Schoenbein that I will not add a word to what he has said on these points, but go at once to other results.

Iron wire, as M. Schoenbein has stated, when put *alone* into strong nitric acid, either wholly or partly immersed, acquires the peculiar inactive state. This I find takes place best in a long narrow close vessel, such as a tube, rather than in a flat broad open one like a dish. When thus rendered quiescent by itself, it has the same properties and relations as that to which the power has been communicated from other wires.

If a piece of ordinary iron wire be plunged wholly or in part into nitric acid of about specific gravity 1·3 or 1·35, and after action has commenced it be touched by a piece of platina wire, also dipping into the acid, the action between the acid and the iron wire is instantly stopped. The immersed portion of the iron becomes quite bright, and *remains* so, and is in fact in the same state, and can be used in the same manner as the iron rendered inactive by the means already described. This protecting power of platina with respect to iron is very constant and distinct, and is the more striking as being an effect the very reverse of that which might have been anticipated prior to the knowledge of M. Schoenbein's results. It is equally exerted if the communication between it and the iron is not immediate, but made by other metals; as, for instance, the wire of a galvanometer; and if circumstances be favourable, a small surface of platina will reduce and nullify the action of the acid upon a large surface of iron.

This effect is the more striking if it be contrasted with that produced by zinc; for the latter metal, instead of protecting the iron, throws it into violent action with the nitric acid, and determines its quick and complete solution. The phænomena are well observed by putting the iron wire into nitric acid of the given strength, and touching it in the acid alternately by pieces of platina and zinc: it becomes active or inactive accordingly; being preserved by association with the platina, and corroded by association with the zinc. So also, as M. Schoenbein has stated, if iron be made the negative electrode of a battery containing from two to ten or more pairs of plates in such acid, it is violently acted upon; but when rendered the positive electrode, although oxidized and dissolved, the process, comparatively, is extremely slow.

Gold has the same power over iron immersed in the nitric acid that platina has. Even silver has a similar action; but from its relation to the acid, the effect is attended with peculiar and changeable results, which I will refer to hereafter.

A piece of box-wood charcoal, and also charcoal from other sources, has this power of preserving iron, and bringing it into the inactive state. Plumbago, as might be expected, has the same power.

When a piece of bright steel was first connected with a piece of platina, then the platina dipped into the acid, and lastly the steel immersed, according to the order directed in the former cases by Professor Schoenbein, the steel was preserved by the platina, and remained clear and bright in the acid, even after the platina was separated from it, having, in fact, the properties of the inactive iron. When immersed of itself, there was at first action of the usual kind, which, being followed by the appearance of the black carbonaceous crust, known so well in the common process of examining steel, the action immediately ceased, and the steel was preserved, not only at the part immersed, but upon introducing a further portion, it also remained clean and bright, being actually protected by association with the carbon evolved on the part first immersed.

When the iron is in this peculiar inactive state, as M. Schoenbein has stated, there is not the least action between it and the nitric acid. I have retained such iron in nitric acid, both alone and in association with platina wire for 30 days, without change; the metal has remained perfectly bright, and not a particle has been dissolved.

A piece of iron wire in connexion with platina wire was entirely immersed in nitric acid of the given strength, and the latter gradually heated. No change took place until the acid was nearly at the boiling-point, when it and the iron suddenly entered into action, and the latter was instantly dissolved.

As an illustration of the extent and influence of this state, I may mention, that with a little management it can be shown that the iron has lost, when in the peculiar state, even its power of precipitating copper and other metals. A mixture of about equal parts of a solution of nitrate of copper and nitric acid was made. Iron in the ordinary, or even in the peculiar state, when put into this solution, acted, and copper was precipitated; but if the inactive iron was first connected with a piece of platina dipping into the solution, and then its own prepared surface immersed, after a few seconds the platina might be removed, and the iron would remain pure and bright for some time. At last it usually started into activity, and began to precipitate copper, being itself rapidly corroded. When silver is the metal in solution, the effect is still more striking, and will be referred to immediately.

I then used a galvanometer as the means of connexion between the iron and other metals thus associated together in nitric acid, for the purpose of ascertaining, by the electric currents produced, in what relative condition the metals stood to each other; and I will, in the few results I may have to describe, use the relations of platina and zinc to each other as the terms under of comparison by which to indicate the states of these metals various circumstances.

The oxidized iron wire of Professor Schoenbein is, when in association with platina, exactly as another piece of platina would be. There is no chemical action, nor any electric current. The iron wire rendered inactive either by association with the oxidized wire or in any other way, is also as platina to the platina, and produces no current.

When ordinary iron and platina in connexion by means of the galvanometer are dipped into the acid, (it matters not which first,) there is action at the first moment on the iron, and a very strong electric current, the iron being as zinc to the platina. The action on the iron is, however, soon stopped by the influence of the platina, and then the current instantly ceases, the iron now acting as platina to the platina. If the iron be lifted into the air for a moment until action recommences on it, and be then reimmersed, it again produces a current, acting as zinc to the platina; but as before, the moment the action stops, the current is stopt also.

If an active or ordinary, and an inactive or peculiar iron wire be both immersed in the nitric acid separately, and then connected either directly or through the galvanometer, the second does not render the first inactive, but is itself thrown into action by it. At the first moment of contact, however, a strong electric current is formed, the first iron acting as zinc, and the second as platina. Immediately that the chemical action is reestablished at the second as well as the first, all current ceases, and both pieces act like zinc. On touching either of them in the acid with a piece of platina, both are protected, and cease to act; but there is no current through the galvanometer, for both change together.

When iron was associated with gold or charcoal, the phænomena were the same. Using steel instead of iron, like effects ensued.

One of the most valuable results in the present state of this branch of science which these experiments afford, is the additional proof that *voltaic electricity is due to chemical action, and not to contact.* The proof is equally striking and decisive with that which I was able to give in the Eighth Series of my Experimental Researches (par. 880)*. What indeed can show more evidently that the current of electricity is due to chemical action rather than to contact, than the fact, that though the contact is continued, yet when the chemical action ceases, the current ceases also?

It might at first be supposed, that in consequence of the peculiar state of the iron, there was some obstacle, not merely to the *formation* of a current, but to the *passage* of one; and that, therefore, the current which metallic contact tended to produce could not circulate in the system. This supposition was, however, negatived by removing the platina wire into a

* See Lond. and Edinb. Phil. Mag., vol. vi. p. 36.—Edit.

second cup of nitric acid, and then connecting the two cups by a compound platina and iron wire, putting the platina into the first vessel, and the iron attached to it into the second. The second wire acted at the first moment, producing its corresponding current, which passed through the first cup, and consequently through the first and inactive wire, and affected the galvanometer in the usual way. As soon as the second iron was brought into the *peculiar* condition, the current of course ceased; but that very cessation showed that the electric current was not stopped by a want of conducting power, or a want of metallic contact, for both remained unchanged, but by the absence of chemical action. These experiments, in which the current ceases whilst contact is continued, combined with those I formerly gave, in which the current is produced though contact does not exist, form together a perfect body of evidence in respect to this elementary principle of voltaic action.

With respect to the state of the iron when inactive in the nitric acid, it must not be confounded with the inactive state of amalgamated or pure zinc in dilute sulphuric acid. The distinction is easily made by the contact of platina with either in the respective acids, for with the iron such association does nothing, whereas with the zinc it develops the full force of that metal and generates a powerful electric current. The iron is in fact as if it had no attraction for oxygen, and therefore could not act on the electrolyte present, and consequently could produce no current. My strong impression is that the surface of the iron is oxidized, or that the superficial particles of the metal are in such relation to the oxygen of the electrolyte as to be equivalent to an oxidation; and that having thus their affinity for oxygen satisfied, and not being dissolved by the acid under the circumstances, there is no renewal of the metallic surface no reiteration of the attraction of successive particles of the iron on the elements of successive portions of the electrolyte, and therefore not those successive chemical actions by which the electric current (which is definite in its production as well as in its action) can be continued.

In support of this view I may observe, that in the first experiment described by Professor Schoenbein, it cannot be doubted that the formation of a coat of oxide over the iron when heated is the cause of its peculiar and inactive state: the coat of oxide is visible by its colour. In the next place all the forms of experiment by which this iron, or platina, or charcoal, or other voltaic arrangements are used to bring ordinary iron into the peculiar state, are accompanied by a determination of oxygen to the surface of the iron; this is shown by the electric current produced at the first moment, and which in such cases always precedes the change of the iron from the common to the peculiar state. That the coat of oxide produced by common means might be so thin as not to be sensible and yet be effectual, was shown by heating a piece of iron an inch or two from the end, so that though blue at the heated part, the end did not seem in the slightest degree affected, and yet that end was in the peculiar state. Again, whether the iron be oxidized in the flame much or only to the very slight degree just described, or be brought into the peculiar state by voltaic association with other pieces or with platina, &c., still if a part of its surface were removed even in the smallest degree and then the new surface put into contact with the nitric acid, that part was at the first moment as common iron; the state being abundantly evident by the electrical current produced at the instant of immersion.

Why the superficial film of oxide which I suppose to be formed when the iron is brought into the peculiar state by voltaic association, or occasionally by immersion alone into nitric acid, is not dissolved by the acid, is I presume dependent upon the peculiarities of this oxide and of nitric acid of the strength required for these experiments; but as a matter of fact it is well known that the oxide produced upon the surface of iron by heat, and showing itself by thin films of various colours, is scarcely touched by nitric acid of the given strength though left in contact with it for days together. That this does not depend upon the film having any great thickness, but upon its peculiar condition, is rendered probable from the fact that iron oxidized by heat, only in that slight degree as to offer no difference to the eye, has been left in nitric acid of the given strength for weeks together without any change. And that this mode of superficial oxidation, or this kind of oxide, may occur in the voltaic cases, is rendered probable by the results of the oxidation of iron in nitrate of silver. When nitrate of silver is fused and common iron dipped into it, so as to be thoroughly wetted, being either alone or in association with platina, the iron does not commence a violent action on the nitrate and throw down silver, but it is gradually oxidized on the surface with exactly the same appearances of colour, uniformity of surface, &c., as if it were slowly oxidized by heat in the air.

Professor Schoenbein has stated the case of iron when acting as the positive electrode of a *couronne des tasses*. If that instrument be in strong action, or if an ordinary battery be used containing from two to ten or more plates, the positive iron instantly becomes covered in the nitric acid with a coat of oxide, which though it does not adhere closely still is not readily dissolved by the acid when the connexion with the battery is broken, but remains for many hours on the iron, which itself is in the peculiar inactive state. If the power of the voltaic apparatus be very weak, the coat of oxide on the iron in the nitric acid often assumes a blue tint like that of the oxide formed by heat. A part of the iron is however always dissolved in these cases.

If it be allowed that the surface particles of the iron are associated with oxygen, are in fact oxidized, then all the other actions of it in combination with common iron and other metals will be consistent; and the cause of its platina-like action, of its forming a strong voltaic current with common iron in the first instance, and then being thrown into action by it, will be explained by considering it as having the power of determining and disposing of a certain portion of hydrogen from the electrolyte at the first moment and being at the same time brought into a free metallic condition on the surface so as to act afterwards as ordinary iron.

I need scarcely refer here to the probable existence of a very close connexion between the phænomena which Professor Schoenbein has thus pointed out with regard to iron, and those which have been observed by others, as Ritter and Marianini, with regard to secondary piles, and A. De la Rive with respect to peculiar affections of platina surfaces.

In my Experimental Researches (par. 476.) I have recorded a case of voltaic excitement, which very much surprised me at the time, but which I can now explain. I refer to the fact stated, that when platina and iron wire were connected voltaically in association with fused nitrate or chloride of silver, there was an electric current produced, but in the reverse direction to that expected. On repeating the experiment I found that when iron was associated with platina or silver in fused nitrate or chloride of silver, there was occasionally no current, and when a current did occur it was almost constantly as if the iron was as platina, the silver or platina used being as zinc. In all such cases, however, it was a thermo-electric current which existed. The volta-electric current could not be obtained, or lasted only for a moment.

When iron in the peculiar inactive state was associated with silver in nitric acid sp. gr. 1·35, there was an electric current, the iron acting as platina; the silver gradually became tarnished and the current continued for some time. When ordinary iron and silver were used in the nitric acid there was immediate action and a current, the iron being as zinc, to the silver as platina. In a few moments the current was reversed, and the relation of the metals was also reversed, the iron being as platina, to the silver as zinc; then another inversion took place, and then another, and thus the changes went on sometimes eight or nine times together, ending at last generally in a current constant in its direction, the iron being as zinc, to the silver as platina: occasionally the reverse was the case, the predominant current being as if the silver acted as zinc.

This relation of iron to silver, which was before referred to page 58, produces some curious results as to the precipitation of one metal by another. If a piece of clean iron is put into an aqueous solution of nitrate of silver, there is no immediate apparent change of any kind. After several days the iron will become slightly discoloured, and small irregular crystals of silver will appear; but the action is so slow as to require time and care for its observation. When a solution of nitrate of silver to which a little nitric acid had been added was used, there was still no sensible immediate action on the iron. When the solution was rendered very acid, then there was direct immediate action on the iron; it became covered with a coat of precipitated silver: the action then suddenly ceased, the silver was immediately redissolved, and the iron left perfectly clear, in the peculiar condition, and unable to cause any further precipitation of the silver from the solution. It is a remarkable thing in this experiment to see the silver rapidly dissolve away in a solution which cannot touch the iron, and to see the iron in a clean metallic state unable to precipitate the silver.

Iron and platina in an aqueous solution of nitrate of silver produce no electric current; both act as platina. When the solution is rendered a little acid by nitric acid, there is a very feeble current for a moment, the iron being as zinc. When still more acid is added so as to cause the iron to precipitate silver, there is a strong current whilst that action lasts, but when it ceases the current ceases, and then it is that the silver is redissolved. The association of the platina with the iron evidently helps much to stop the action.

When iron is associated with mercury, copper, lead, tin, zinc, and some other metals, in an aqueous solution of nitrate of silver, it produces a constant electric current, but always acts the part of *platinum*. This is perhaps most striking with mercury and copper, because of the marked contrast it affords to the effects produced in dilute sulphuric acid and most ordinary solutions. The constancy of the current even causes crystals of silver to form on the iron as the negative electrode. It might at first seem surprising that the power which tends to reduce silver on the iron negative electrode did not also bring back the iron from its peculiar state, whether that be a state of oxidation or not. But it must be remembered that the moment a particle of silver is reduced on the iron, it not only tends to keep the iron in the peculiar state according to the facts before described, but also acts as the negative electrode, and there is no doubt that the current of electricity which continues to circulate through the solution passes essentially between it and the silver, and not between it and the iron, the latter metal being merely the conductor interposed between the silver and the copper extremities of the metallic arrangement.

I am afraid you will think I have pursued this matter to a greater length than it deserves; but I have been exceedingly interested by M. Schoenbein's researches, and cannot help thinking that the peculiar condition of iron which he has pointed out will (whatever it may depend upon) enable us hereafter more closely to examine the surface-action of the metals and electrolytes when they are associated in voltaic combinations, and so give us a just knowledge of the nature of the two modes of action by which particles under the influence of the same power can produce either local effects of combination or current affinity*.

I am, my dear Phillips, very truly yours,

Royal Institution, June 16, 1836. M. FARADAY.

XIII. *Notice of the Magnetic Action of Manganese at Low Temperatures, asstated by* M. Berthier. *In a letter from* Mr. Faraday.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following fact, stated by M. Berthier, has great interest to me, in consequence of the views I have taken of the general magnetic relations and characters of the metals. As you have done me the favour to publish these views in your Magazine†, perhaps you will think the present note also worth a place in the next Number.

Berthier in his *Traité des Essais par la Voie Sèche*, tome i. p. 532, has the following passage in his account of the physical properties of the metals. "Magnetism.—There are only three metals which are habitually endowed with magnetic force: these are iron, cobalt, and nickel; *but manganese also possesses it beneath a certain degree of temperature much below zero.*" There is no reference to any account of this experi-

* Experimental Researches, Eighth Series, parr. 947. 996. [or Lond. and Edinb. Phil. Mag., vol. vi. pp. 174, 337.—EDIT.]

† See Lond. and Edinb. Phil. Mag., vol. viii. p. 177.—EDIT.

mental result, and it is therefore probable that M. Berthier himself has observed the fact, in which case it cannot be doubted; but the result is so important that any one possessing pure manganese who can verify the result and give an account of the degree of temperature at which the change takes place, will be doing a service to science. The great point will be to secure the *perfect absence* of iron or nickel from the manganese. With respect to cobalt, I have already stated that when pure, I cannot find it to possess magnetic properties at common or low temperatures.

I am, Gentlemen, yours, &c.,

Royal Institution, June 17, 1836. M. FARADAY.

XIV. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

[Continued from vol. viii. page 348.]

Dec. 8, 1835.—SPECIMENS were exhibited of various *Birds* chiefly from the Society's collection, which Mr. Gould regarded as hitherto undescribed. At the request of the Chairman he pointed out the distinguishing peculiarities of the undermentioned species: viz. *Phœnicura plumbea, Pyrgita cinnamomea, Merula castanea, Saurophagus Swainsonii, Brachypus gularis, Merula Nestor, Ianthocincla pectoralis*, and *Ianth. albogularis*; and also the following new genus.

STENORHYNCHUS.

Rostrum capite longius, gracile, compressum, subfornicatum; mandibulâ superiore leviter emarginatâ, culmine in frontem depressissimum producto.

Nares ovales, apertæ.

Alæ breviusculæ, subrotundatæ; remige 1mâ brevissimâ, 4tâ longiore; 5tâ et 6tâ 4tam subæquantibus.

Cauda mediocris, rotundata; rectricibus decem?

Pedes robusti: *acrotarsis* subscutellatis; *halluce ungue*que postico fortibus, tarsum longitudine subæquantibus, digito intermedio brevioribus.

Plumæ molles.

STENORHYNCHUS RUFICAUDA. *Sten. suprà sordidè saturatè brunneus, rufo caudam versus tinctus; caudâ, secundariis, scapularibusque saturatè rufo-brunneis; subtùs brunnescenti-cinereus, in rufo-brunneum ad latera vergens.*

Long. tot. $9\frac{1}{2}$ poll.; *rostri*, $1\frac{3}{4}$; *alæ*, $4\frac{3}{4}$; *caudæ*, $3\frac{1}{2}$; *tarsi*, 1.

Rostrum nigrum; *pedes* brunnei.

Hab.

As only one specimen of this bird has yet been seen, it is doubtful whether it may not possess twelve tail-feathers; but, after a careful examination, Mr. Gould can discover no more than ten.

A paper was read, entitled "Mémoire sur une Nouvelle Espèce de Poisson du Genre Histiophore, de la Mer Rouge: par M. E. Rüppell, M.D., Memb. Ext. Z. S." It was accompanied by a drawing of the fish described in it.

MM. Cuvier and Valenciennes have described, in their 'Histoire Naturelle des Poissons,' three species of *Sword-fishes* of the genus *Histiophorus*; from all of which Dr. Rüppell regards his fish as distinct, although it apparently approaches most nearly to *Hist. Americanus*: it should seem that its occurrence at Djetta, on the coast of Arabia, was only accidental, as the Arab fishermen knew no name for it. The most striking peculiarity of the new species is the uniformity of the colour of its dorsal fin: in all those which were previously known the first dorsal fin is varied with spots; in the one obtained by Dr. Rüppell, the first dorsal fin is black throughout and destitute of spots, on which account its discoverer proposes for it the name of

Histiophorus immaculatus, under which its characters are given in the Society's "Proceedings."

Dr. Rüppell describes the fish in considerable detail. He has not, however, examined it anatomically, on account of his possessing only one specimen, which he had deposited in the Frankfort Museum.

The following notes by Sir Robert Heron, Bart., were read.

"In many books that I have seen some errors are made in the history of the *Kangaroos,* which my long possession of those animals enables me to correct.

"The *great Kangaroo* does not make use of his tail in leaping. He uses it in walking, and still more in standing. When excited, he stands (the male only) on tip-toe and on his tail; and is then of prodigious height. In fighting he does not stand on the tail and one leg, but balances himself for a moment on the tail only, and strikes forward with both hind legs.

"The *bush Kangaroo,* or *Kanguru enfumé* of Cuvier, never uses his legs in fighting. He generally contents himself with threatening with his teeth and a low growl; but I have seen him, when attacked by an *Emu,* jump up at the bird's head. Neither of them, however, has persevered in annoyance.

"When sitting in a state of repose the *great Kangaroo* throws the tail behind him: the lesser one before him, between his legs."

The following note by Sir Robert Heron, Bart., was also read, as giving an account of an extraordinary instance of want of sagacity in a *Dog.*

"A large old white female terrier followed me this autumn from Grantham. She remained perfectly satisfied for three weeks, when, on my again going to attend the petty sessions, she again followed me, I then found that she belonged to one of my colleagues, the Rev. Mr. Ottley; and that she had long been a great favourite in the family, who were greatly distressed at her loss. It happened that Mr. Ottley and I each rode a chestnut pony with a long tail. This had completely deceived the dog, whose unsentimental friendship did not prompt her to ask any further questions."

Dec. 22.—Specimens were exhibited of several *Rodent* animals collected during his survey of the Straits of Magalhaens, by Capt. P. P. King, R.N., Corr. Memb. Z. S., and presented by him to the Society. They were accompanied by some notes by Capt. King, which were read.

In bringing the animals severally under the notice of the Meeting, Mr. Bennett first directed particular attention to one of them, which constituted, in his estimation, a new species in the genus *Ctenomys*, Blainv. To elucidate its relations with the nearly allied genera of *Herbivorous Rodentia*, *Octodon*, Benn., and *Poephagomys*, F. Cuv., a specimen of *Octodon Cumingii* was exhibited and compared with it; and Mr. Bennett stated his intention of entering with some detail into the subject in a paper which he proposed to prepare upon it.

In the structure of its molar teeth, *Octodon* may be regarded as occupying an intermediate station between *Poephagomys* and *Ctenomys*. In *Octodon* the molars of the upper jaw differ remarkably in form from those of the lower. The upper molars have on their inner side a slight fold of enamel, indicating a groove tending in some measure to separate on this aspect the mass of the tooth into two cylinders: on their outer side a similar fold penetrates more deeply, and behind it the crown of the tooth does not project outwardly to so great an extent as it does in front. If each molar tooth of the upper jaw be regarded as composed of two partially united cylinders, slightly compressed from before backwards, and somewhat oblique in their direction, the anterior of these cylinders might be described as entire, and the posterior as being truncated by the removal of its outer half. Of such teeth there are, in the upper jaw of *Octodon*, on each side, four; the hindermost being the smallest, and that in which the peculiar form is least strongly marked. In *Ctenomys*, all the molar teeth, both of the upper and the lower jaw, correspond with the structure that exists in the upper jaw of *Octodon*, excepting that their crowns are slenderer and more obliquely placed, whence the external emargination becomes less sharply defined; and also excepting that the hinder molar in each jaw is so small as to be almost evanescent: as is generally the case, however, the relative position of the teeth is counterchanged, and the deficiency in the outline of the crown of the tooth, which in the upper jaw is external, is, in the lower jaw, internal. In the lower jaw of *Octodon* the crowns of the molars assume a figure very different from those of the upper, dependent chiefly on the prolongation of the hinder portion of the tooth to the same lateral extent as its anterior part: each of them consists of two cylinders, not disjoined in the middle where the bony portion of the crown is continuous, but partially separated by a fold of enamel on either side producing a corresponding notch; placed obliquely with respect to the jaw they resemble, in some measure, a figure of 8 with its elements flattened obliquely, pressed towards each other, and not connected by the transverse middle bars. With the lower molars of *Octodon* those of *Poephagomys*, as figured by M. F. Cuvier, correspond in structure in both jaws. *Octodon* thus exhibits, in its dissimilar molars, the types of two genera: the molars of its upper jaw represent those of both jaws of *Ctenomys*; those of its lower jaw correspond with the molars of both jaws of *Poephagomys*.

The characters distinguishing the new species of *Ctenomys* are chiefly those of colour. The *Cten. Brasiliensis* is described by M. de Blainville as being shining rufous above, and reddish white below. The new species may be characterized as the *Ctenomys Magellanicus*. Captain King states that this "little animal is very timid; feeds upon grass; and is eaten by the Patagonian Indians. It inhabits holes, which it burrows, in the ground: and, from the number of the holes, it would appear to be very abundant."

A second animal exhibited appears, like the preceding, to represent in the more southern latitudes of South America a genus whose type was originally observed in Brasil. Mr. Bennett regarded it as a second species of *Kerodon*, F. Cuv., chiefly distinguishable from the one discovered by Prince Maximilian of Wied by its more uniform colour. Excepting a slight dash of white behind the ear, and a longer line of the same colour marking the edge of each branch of the lower jaw, the animal is entirely grey; the upper surface being distinguished from the under by a greater depth of tint, and by the intermixture of a free grizzling of yellow and black. The crowns of the molar teeth, as in the typical species, consist of bone surrounded by two triangles of enamel, the bases of which are connected together by a short line of enamel passing from the one to the other: all the lines being slender and sharply defined.

For this species Mr. Bennett proposed the name of *Kerodon Kingii*.

The third animal exhibited was remarked on as constituting a new species of *Cavy*, distinct from all those that were previously known, including the two which have recently been described by M. Brandt in the 'Nouveaux Mémoires de l'Académie Impériale de St. Petersbourg.' Mr. Bennett characterized it as the *Cavia Cutleri*, King MSS.

The general form of the animal is probably similar to that of the *restless Cavy, Cavia Cobaya*, Gmel., popularly known as the Guinea-pig. It is covered universally by long, smooth, glossy, black hairs, which are slightly tinged with brown. Its ears are rather large, broadly expanded, and hairy; and between them the hairs are longer than those on the adjoining parts, occasioning a slight appearance of a crest. On the middle of each cheek the hairs radiate as from a centre, almost in a similar manner to that in which they spread from around the crown of the *bonneted Monkeys*, and the skin is consequently left in the middle point almost bare. The dentition is altogether that of the *restless Cavy*, and the incisors, as in it, are white. The skull is rather more expanded laterally, which gives to it an appearance of comparative flatness.

"This animal was known, on the survey, by the name of the *Peruvian Cavy*. The specimen in the Society's collection was presented to one of the officers of the Beagle by an American sailing-master, of Stonington, U.S., a very intelligent person, to whom we were much indebted. The trivial name which I have proposed for it is in recollection of the benefit we derived from his experience and knowledge of the intricate navigation of the south-western coast of Patagonia, which was freely imparted to us on several occasions."—P. P. K.

The collection also contained specimens of a *Mouse*, for which Mr. Bennett proposed the name of *Mus Magellanicus*.

Specimens were exhibited of several *Marsupialia*, on which Mr. Ogilby made the following remarks.

"A small collection of *Marsupial Quadrupeds*, which Mr. Gould lately received from his brother-in-law, Mr. Coxen, contains two or three interesting species, which the usual kindness of Mr. Gould enables me to notice. They were all procured, as I am informed, in the country beyond the Hunter River, about eighty miles north of Sydney in New South Wales. The most remarkable is an undescribed species of *Phalanger*, which I propose to call

Phalangista Canina. It is similar in size and general proportions to *Phal. Vulpina*, and the two allied species described in the 'Proceedings' for 1830–31, page 135, (Phil. Mag. and Annals, N.S., vol. xi. p. 133.) but is easily distinguished from them all by the small size and round form of the ears, as well as by the distribution of the colours. All the upper parts of the body, the head, cheeks, back, sides, and outer face of the arms and thighs are of a uniform grizzled brown; the throat, breast, belly, and interior of the members dirty ashy grey with a slight shade of yellow. The ears are only an inch in length and about the same in breadth, being thus little more than half as long as in *Phal. Vulpina*. They are naked within, but covered with deep coffee-coloured fur on the outside; the nose, and the paws, both before and behind, are dark brown; and the tail is bushy and entirely black to within about 2 inches of its root, which is of the same colour as the back. All these circumstances distinguish the present species from *Phal. Vulpina*, with which alone it can possibly be confounded, and in which the backs of the ears, and the cheeks and paws are yellowish white, whilst the black colour occupies only the latter half of the tail. Both these animals have long black *vibrissæ*, and a tuft of similar stiff hair on the cheek, about an inch below and behind the eye. The whole length from the nose to the root of the tail is 2 feet; the length of the tail 13½ inches.

Phal. Cookii. I notice this species merely to observe that the present specimen is the only certain evidence we possess of this animal being an inhabitant of Continental Australia. Cook observed it in Van Diemen's Land, and I had never been able to ascertain the precise locality from which the various other individuals I had formerly examined, were obtained.

Macropus Eugenii. This specimen agrees with M. Desmarest's description, and is interesting as coming from a very distant part of the country.

Perameles obesula. An adult specimen of the same size as the full-grown *Per. nasuta.* I notice it to mention that the teeth are, in all respects, similar to those of *Per. nasuta,* both in form and number.

The collection contains besides, two very fine specimens of *Petaurus Taguanoides*; one of *Pet. Sciureus;* one of *Hydromys chrysogaster*; and a young *Koala.*"—W. O.

PROCEEDINGS AT THE FRIDAY EVENING MEETINGS OF THE MEMBERS OF THE ROYAL INSTITUTION.

March 25.—Mr. Goadby on Insect Anatomy.

April 15.—Sir James South on Astronomical Observations as carried on in the fixed Observatory.

April 22.—Sir James South. The same (concluded).

April 29.—Mr. Faraday on Plumbago and Pencils.

May 6.—Mr. Daniell on a new and constant Voltaic Battery.

May 13.—Dr. Lardner on Steam communication with India (concluded).

May 20.—Professor Mayo on some of the uses of Sensation.

May 27.—Mr. Pettigrew unrolled an Egyptian Mummy.

June 3.—Mr. Beamish on the present state and prospects of the Thames Tunnel.

Jnne 10.—Mr. Faraday. Considerations respecting the nature of Chemical Elements.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. viii. p. 431.]

A meeting of the Philosophical Society was held on Monday evening, April 18th, Dr. Clark, the President, in the chair. The Astronomer Royal (lately Prof. Airy) read a communication on the intensity of light in the neighbourhood of a caustic. One object of this investigation was to determine what must be the circumstances of the rainbow on the undulatory theory of light. Afterwards Mr. Hopkins gave an account of the agreement between the results of his theory of elevatory geological forces, and the phænomena of faults, as observed by him in the strata of Derbyshire.

A meeting of this Society was held on Monday evening, May 2nd, Dr. Thackeray, Vice-President, in the chair. A memoir was read by S. Earnshaw, Esq., St. John's, "On the Integration of the Equation of Continuity of Fluids in Motion;" also a memoir by Professor Miller on the Measurements of the Axes of Optical Elasticity of certain Crystals. This memoir contained various determinations, from which it appears that the law concerning the connexion of the crystalline and the optical properties of crystals suggested by Professor Neumann, namely, that the optical axes are the axes of crystalline simplicity, is false; but that it is true, in many of the cases hitherto examined, that one of the optical axes coincides with the axis of a principal crystalline zone. Afterwards Mr. Webster, of Trinity College, made some observations on the periodical and occasional changes of the height of the barometer, and on their connexion with the changes of temperature arising from the seasons and from the condensation of aqueous vapour.

A meeting of this Society was also held on Monday evening, May 16th, Dr. Thackeray, V.P., in the chair. A letter from A. De Morgan, Esq., to the Rev. George Peacock, was read, containing a sketch of a method of introducing discontinuous constants into the arithmetical expressions for infinite series. Also a memoir by P. Kelland, Esq., of Queen's College, on the mathematical results of a mixture of elastic fluids (as air and vapour in the atmosphere), and on the theory of heat. With regard to the latter subject, the object was to show that there is a translation backwards or forwards of the calorific particles, consequent on and varying in intensity with the transverse vibration. Mr. Hopkins made some statements respecting experiments recently made on the temperature of mines and the doctrine of central heat. Mr. Airy gave an account of observations of temperature made during the great solar eclipse of May 15th.

XV. *Intelligence and Miscellaneous Articles.*

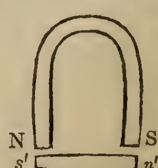
ON THE FEEBLE ATTRACTION OF THE ELECTRO-MAGNET FOR SMALL PARTICLES OF IRON AT SHORT DISTANCES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE enormous sustaining power of the electro-magnet has for some time been exhibited as a matter of great curiosity, but its very feeble attraction for small particles of iron at short distances is not, I believe, very generally known. This fact was first mentioned to me by Mr. Clarke, magnetical instrument-maker, which since then I have frequently noticed myself. I am not aware that any explanation of this seeming anomaly has as yet been given; I have therefore ventured to offer one, which, if considered satisfactory, and of interest sufficient to deserve a place in your valuable Journal, I shall be obliged by its insertion.

It will be necessary first to observe the phænomena which take place when a piece of soft iron is under the influence of the ordinary horseshoe magnet. When the armature $s'n'$ is brought near to the magnet N S, magnetism is induced in $s'n'$; and according to the law of magnetic induction each extremity of $s'n'$ has its state of polarity opposite to that of the adjacent pole of the magnet N S, and a tendency to approach each other immediately takes place, and if the force of attraction be sufficient to overcome the inertia of $s'n'$, contact will instantly follow.



If the armature be sufficiently massive to receive all the magnetism N S is capable of inducing, the magnet will not be able to sustain any more, and consequently a limit to its inducing power must exist. The reaction of the armature upon the magnet will also strengthen the adhesion between them: probably the effect of this reaction will be influenced by the facility with which magnetism permeates the steel of which the magnet is made, and be greater in the softer kinds of steel.

If this view be correct, the sustaining power of a common magnet cannot be considered as an exact measure of its magnetic intensity.

When a piece of soft iron is placed at a short distance from the poles of an electro-magnet, under the influence of a galvanic current, a comparatively trivial effect is produced, showing that the magnet thus induced is but of feeble magnetic intensity. This is owing, probably, to the facility with which soft iron is permeated by magnetism, and consequently any considerable accumulation prevented. But when the iron is in contact with the poles of the electro-magnet, the magnetism, instead of escaping, will induce in the armature polarity, and the armature reacting powerfully on the soft iron of the electro-magnet, and receiving continuous additions of magnetism from the galvanic current, will be attracted by the magnet with increasing force until the attraction between them becomes immense. If the galvanic action be discontinued, the keeper will remain applied to the electro-magnet, though less firmly; and after it has been removed nearly all the magnetism escapes.

If this explanation be correct, it will be obvious that the electro-magnet will not be well adapted for the construction of magneto-electrical machines, in which the armature is made to rotate rapidly in front of the poles of the magnet without actual contact.

I am, &c.

No. 1, Maze Pond, Borough, May 7, 1836. GEORGE RAINEY.

OBSERVATIONS ON THE SOLAR ECLIPSE OF MAY 15, 1836; AND ON THE AURORA BOREALIS OF APRIL 22.

To the Editors of the Philosophical Magazine and Journal of Science.

GENTLEMEN,

I am well aware that you must have received various accounts of observations made during the late eclipse; nevertheless, I beg to trouble you with one or two made by myself at that time upon the possibility of seeing the lunar mountains on the round or unbroken side of the moon, although it may be presumed that an account of the singular appearance at the time of the annular phase has been transmitted to you. I saw the roughness of the moon's edge from the beginning of the eclipse; but at the time of the ring becoming nearly equal on the eastern and western sides its narrowest part was divided directly across in two places, the light of the sun passing between the mountains. This affords an excellent method of calculating the heights of the lunar mountains; for it may be readily known of what breadth the narrowest part of the ring appeared at this place (about 54° 53′ 53″ N. and about 1° 24′ W.). The mountains fully covered it, and I believe were higher than it a little. This observation was made with a 42-inch reflector, (Newtonian) with 5·75 inches aperture. As I did not expect so rare a sight, and there was not time to get the wire or divided eye-piece micrometer after it was seen, I regret to say no measures were taken of the heights.

I may mention another circumstance which was not overlooked, namely, the appearance of the solar spots during the eclipse, which afforded the most favourable opportunity of examining them to advantage. My observations were made with the view of ascertaining whether any difference in shade could be seen similar to the lunar cavities, or whether anything which indicates a rise above the solar surface; but not the least could be observed, or even imagined to be visible. Though I have examined the solar spots regularly for ten or twelve years, I never saw them to greater advantage than on that occasion.

I will close this letter with an observation upon the aurora borealis which was visible at this place on the 22nd of April 1836, at 10^{h} 45^{m} P.M. It appeared directly overhead in the form of a star of great magnitude of not less than 90 degrees diameter, with numerous rays shooting every way, those to the north appearing of a deep red colour. With attentive examination I could not discover the least darting or motion of the rays at one time, which if so, is not easily accounted for by the principles of electricity. A very similar appearance is represented by a plate in the Journal of the Royal Institution. Your humble Servant,

High Barns, near Sunderland, May 26, 1836. W. ETTRICK.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The next Meeting will be held at Bristol during the week commencing on Monday, August 22nd; the Members of the General Committee will assemble on the preceding Saturday.

METEORS OBSERVED IN INDIA IN 1832.

The following notices are derived from "Extracts from a Journal of a Residence, and during several Journeys, in the Province of Behar, in the years 1831 to 1834. By Mr. J. Stephenson," which appear in the Number for December, of the *Journal of the Asiatic Society of Bengal*, vol. iv. p. 713.

Beautiful Meteor observed near Singhea, Tirhut, April 11th, 1832.

At 4 hours 45 minutes A.M. and at daybreak, observed a meteor in the form of a globular ball of fire, which passed through the air, from west to east, in a horizontal direction, and with a motion moderately rapid. Its size appeared to be about a foot in diameter, having a fiery train of the most splendid brilliancy apparently many yards long. It illuminated the country as far as the eye could reach, and remained visible for five seconds, after which it exploded like a rocket throwing off numerous corruscations of intense light; but without any report or noise of any kind. Its apparent elevation inconsiderable.

Another beautiful Meteor observed at the same Village on the 20th of May, 1832.

At 6 hours 40 minutes P.M. a large pear-shaped meteor was observed shooting very rapidly in a horizontal position, and in a direction from north to south. Nothing could exceed the brilliant mixture of green, tinged with blue colours, exhibited during its rapid progress. It left a luminous train of great length behind, and remained visible about three seconds, then disappeared in the southern horizon, without exhibiting any signs of exploding.

ANALYSIS OF PLOMBGOMME. BY M. DUFRESNOY.

The compound of oxide of lead and alumina, called by mineralogists plombgomme, on account of its resemblance to drops of gum which exude from trees, has hitherto been found only in Huelgoat mine in Brittany. It has, however, lately occurred in a lead mine near Beaujeu: it is found in small mammillated masses, with slightly varying textures; some are of a yellowish white colour, externally very shining, and the fracture is both splintery and testaceous, without any trace of crystallization; others are slightly greenish, composed of concentric layers, and possess a radiated structure, like wavellite. When observed with a microscope, the fibres appear to be crystalline, with a rhombic fracture, like some arragonites.

The hardness of plombgomme is intermediate as to that of carbonate and phosphate of lime. Its specific gravity is 4·88: before the blowpipe it decrepitates; on charcoal it swells and yields a scoriaceous white enamel.

By analysis it yielded

Silica	2·11
Alumina	34·23
Deutoxide of lead	43·42
Phosphoric acid	1·89
Water	16·14
Loss	2.21
	100·00

The specimen subjected to analysis contained phosphate of lead: it is very probable that the phosphoric acid found indicates a certain quantity of phosphate of lead mixed with the plombgomme. On this supposition, the analysis should be thus stated:

Silica	2·11
Alumina	34·23
Deutoxide of lead	37·51
Phosphate of lead	7·80
Water	16·14
Loss	2·21
	0.010

Ann. de Chim. et de Phys., tome lix. p. 440.

ON THE ACTION OF IODINE ON ORGANIC SALIFIABLE BASES.

M. Pelletier in a paper on the action of iodine upon strychnia, brucia, cinchonia, quina, and morphia, remarks that being unacquainted with the action that the halogenous bodies, and chiefly iodine, bromine, and chlorine, exert upon the salifiable organic bases, we do not know whether these bodies can combine with the vegetable alkalies without decomposing them or not.

In endeavouring to elucidate this point he has arrived at the following conclusions. That iodine combines with most of the organic bases in atomic proportion: thus, strychnia affords a crystalline and coloured iodide composed of 2 eqs. of iodine and 1 eq. of strychnia; brucia forms two iodides, one composed of 2 eqs. of iodine and 1 eq. of brucia, the other of 4 of iodine, to 1 of base; whilst cinchonia and quina each combine with iodine in the proportion of 1 eq. of iodine to one of base.

That iodic acid combines with the organic bases, forming neutral and acid salts, in which the acid and base exist in the same proportion as their respective iodides.

That hydriodic acid unites with the organic bases, forming salts which have a tendency to an excess of base; thus, the hydriodates of brucia and strychnia are (sesquibasic) subsesquisalts without water of crystallization. The organic hydriodates are decomposed by iodic acid, iodine being liberated, whilst the hydriodate is converted into an iodide.

The action of iodine upon morphia forms a very singular exception to the above, for one part of the iodine combines with hydrogen from the morphia to form hydriodic acid, whilst the other portion of iodine unites with the substance resulting from the morphia. When morphia is acted on by iodic acid, the acid loses its oxygen, which unites with one portion of the morphia, forming a red substance like that resulting from the action of nitric acid on morphia, whilst the iodine evolved acts on the other portion of morphia as it does by direct contact; but the resulting combination is decomposed by a fresh portion of iodic acid, and entirely converted into iodine and the red substance.—*L'Institut,* 2nd March.

ON A NEW MODE OF ANALYSIS OF CLOSELY AGGREGATED MINERALS.

Dr. Abich states that when carbonate of barytes is heated to whiteness, it fuses and is deprived of the whole of its carbonic acid; and this property he has very advantageously employed in the analysis of minerals, its caustic power being so great that it quickly and completely decomposes the aluminates and corundum, bodies which are with the greatest difficulty acted on by pure potash; and cyanite, staurolite, andalusite, cymophane, zircon, and the felspars are also acted upon in the most complete manner. To conduct the analysis successfully, the following precautions are necessary. A furnace by which an extremely high and well-regulated temperature can be obtained in a short time. The mineral reduced to powder, which need not be extremely fine, levigation being useless, must be mixed with from 4 to 6 times its weight of pure carbonate of barytes, and placed in a platinum crucible; this is then inclosed in a Hessian crucible covered and luted, which placed on any convenient support in the furnace must be heated to whiteness, and kept at that temperature for from 15 to 20 minutes: a perfectly fused mass is obtained, which dissolves with facility in diluted muriatic acid.—*Annales de Chimie,* December 1835.

ON SOME NEW COMBINATIONS OF CARBOHYDROGEN OR METHYLENE.

MM. Dumas and Peligot have succeeded in obtaining some new combinations of carbohydrogen. The first, hydrofluate of carbohydrogen, may be obtained by gently heating a mixture of fluoride of potassium and sulphate of carbohydrogen; sulphate of potash is formed, and a gas evolved which collected over water is deprived of all foreign substances, and is then pure hydrofluate of carbohydrogen. It is colourless, of an agreeable æthereal odour, and burns with a flame similar to that of alcohol, but rather more blue. By its combustion hydrofluoric acid is formed, the vapours of which are diffused in the air. It is slightly soluble in water, 100 parts of water at 60° Fahr., dissolving 1·66 of this gas. The analysis of this gas indicates its composition to be

One volume of carbohydrogen .... 0·4904
One volume of hydrofluoric acid ...... 0·6788—1·1692

According to this, hydrofluate of carbohydrogen, like the hydrochlorate of that base, contains as well as hydrochloric æther, 1 vol. of acid and 1 of carburetted hydrogen condensed into one volume.

When a mixture of pyroxylic spirit, nitric acid, and nitrate of silver is boiled in the proportions used in making fulminating silver, or in any other proportion, no action takes place before the evaporation of $\frac{9}{10}$ths of the liquor, unless the nitric acid is very strong and the operation is conducted in a retort, when amongst the volatile products abundance of nitrate of carbohydrogen is found. Towards the end of the operation, when the solution is so concentrated that the nitrate of silver would become solid by cooling, by continuing the ebullition lively action takes place, much hyponitrous acid is liberated, and there is deposited a white powder. This detonates with difficulty by a blow, and deflagrates feebly when placed in contact with hot coals, from which MM. Dumas and Peligot at first considered that it contained fulminating silver; but a more attentive examination showed it to be oxalate of silver.

By adding pyroxylic spirit to the acid nitrate of mercury, a considerable quantity of a yellowish white and resinous-looking substance is immediately deposited, which by boiling for a long time with concentrated nitric acid, produces a white powder which is perfectly pure oxalate of mercury.

On an analysis the substance formed by the immediate action of pyroxylic spirit on nitrate of mercury gave from 1·025, water 0·024 and carbonic acid 0·180. 0·600 gave by protochloride of tin, and hydrochloric acid, 0·452 of mercury. 0·700 afforded 18 cubic centim. of damp azote at 54° and 0·75. These results are equivalent to

Carbon........	4·80	or	4 eqs.	4·53
Hydrogen......	0·45	—	2 eqs.	0·37
Mercury	75·30	—	2 eqs.	74·80
Azote	2·70	—	1 eq.	2·60
Oxygen	16·76	—	6 eqs.	17·70
	100·00			100·00

This leads to so complicated a formula, that they merely give it without insisting on its correctness:

$$2\,(C^4\,H^2\,O^3, Hg\,O) + Az^2\,O^3, Hg^2\,O,$$

which would indicate the existence of a compound of 2 eqs. of formiate of mercury and 1 eq. of nitrate of mercury.

By passing anhydrous sulphuric acid into pyroxylic spirit, diluting the liquor, and supersaturating by barytes, there are obtained sulphate of barytes, which precipitates, and sulphocarbohydrate of barytes, which remains in solution. The solution freed from excess of barytes by carbonic acid, and being concentrated at a low temperature and then suffered to crystallize spontaneously, furnishes truncated prisms very thin and long, which appear to have a rhomboidal base.

In the same circumstances the common sulphocarbohydrate of barytes affords very different crystals; but the composition of the salt produced from the anhydrous acid does not differ from the salt afforded by the common acid. This salt is composed of

	Expt.	Calcul.
Carbon..................	6·95	6·89
Hydrogen................	1·75	1·66
Sulphate of barytes	64·70	65·15

The calculation was founded on the formula

$$Ba\,O, S\,O^3 + C^4\,H^4, S\,O^3, H^2\,O.$$

It appears then that a series of isomeric sulphocarbohydrates exist, resulting from the action of sulphuric acid on pyroxylic spirit.

Tartrocarbohydrate of barytes may be obtained by mixing a solution of tartaric acid in pyroxylic spirit, and a solution of barytes in the same spirit together, and washing the precipitate with anhydrous pyroxylic spirit. It is composed of

	Exp.	Calcul.		
Carbon........	23·9	25·3	or	20 eqs.
Hydrogen	3·0	3·3	—	16 eqs.
Barytes	30·8	31·8	—	1 eq.
Oxygen	41·3	39·6	—	12 eqs.

This salt is in a gelatinous state when formed in the pyroxylic spirit, but when washed with water it becomes granular and is converted into tartrate of barytes.

The oxalic, acetic, and benzoic acids dissolved in pyroxylic spirit, and added to barytes likewise dissolved in spirit, produce merely their respective salts, the two latter anhydrous.

ON THE PERIODIDE OF IRON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Having noticed in our recent chemical works an account of a periodide of iron, and the method adopted for its preparation being to expose the protiodide in solution to the free action of air, I have presumed to offer an opinion, and I do apprehend that the change in this case which takes place is a peroxidation of a part of the iron which falls, and its equivalent of iodine is left *free* in the solution of the yet unchanged protiodide of iron, and a portion remains thus unchanged after a thin stratum has been exposed several weeks, which a solution of potash will indicate by precipitating a protoxide of iron; but there is no periodide of iron in this solution, nor could I form a periodide by a persalt of iron and iodide of potassium, and I believe no such compound to exist.

Whilst upon this subject, it may be perhaps not uninteresting to the medical part of your readers to give a short account of the protiodide. It was first employed in medicine by Dr. A. T. Thomson, and has since kept its character as a valuable tonic: the great inconvenience arising from its tendency to decompose when dissolved in water, is completely obviated by a coil of iron wire traversing the whole column of the solution, which was suggested by me when it came into extensive use as a therapeutic agent, and nearly three years' experience proves it to answer most satisfactorily the object intended: it will preserve it perfectly neutral even if the solution be fully exposed to air and light; it is true, in that case more peroxide is formed, but, filter the solution when you will, it is perfectly colourless, and transparent as distilled water: this is a very important point, and one which the medical profession should be made fully acquainted with, being a safe test for its neutrality and purity. Any colour, however slight the tinge, shows the presence of some iodine in a free state, or some impurities derived probably from one of the materials employed to make it; this no doubt has given rise to the difference of opinion as to its action on the animal œconomy. The colourless neutral compound when diluted has an agreeable flavour, similar to that of a chalybeate spring, whereas any free iodine gives a mawkish taste and is liable to nauseate the stomach.

I am, yours, &c.,

P. SQUIRE.

227, Oxford Street, May 12, 1836.

METEOROLOGICAL OBSERVATIONS FOR MAY 1836.

Chiswick.—May 1, 2. Slightly clouded: stormy. 3. Cold and windy. 4. Cold rain. 5. Rain: cloudy and fine. 6. Light haze: fine. 7—10. Fine. 11—18. Very fine. 19. Cold haze: fine. 20. Very fine: rain at night. 21, 22. Fine. 23. Rain: stormy. 24. Clear and cold. 25—28. Cold and dry. 29—31. Fine.

Boston.—May 1. Fine. 2. Stormy. 3. Stormy: rain P.M. 4. Rain. 5. Stormy: rain early A.M.: rain P.M. 6, 7. Fine. 8—10. Cloudy. 11—18. Fine. 19. Cloudy. 20. Fine: rain P.M. 21, 22. Fine. 23. Stormy. 24, 25. Cloudy. 26—30. Fine. 31. Cloudy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. May.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
☉ 1.	29·794	29·896	29·827	29·38	43·6	35·3	49·0	53	40	48	NW.	E.	calm	...	·06	...	39
M. 2.	29·855	30·069	29·882	29·59	49·5	39·3	52·3	56	40	50	NE. var.	NE.	NE.	·052	·01	...	40
T. 3.	29·940	29·954	29·846	29·63	46·3	40·4	53·3	56	43	47	NE. var.	NE.	NE.	...	·08	...	40
W. 4.	29·679	29·715	29·688	29·25	49·2	43·7	54·6	57	41	47	N.	NE.	NE.	·091	·24	·26	44
Th. 5.	29·699	29·914	29·715	29·27	45·7	42·5	50·6	54	41	48	N.	SE.	E.	·133	·40	·03	43
F. 6.	30·073	30·217	30·082	29·60	50·5	42·3	58·7	61	39	56	E.	E.	calm	·355	...	·03	41
☾ S. 7.	30·295	30·313	30·193	29·83	53·3	40·8	56·6	61	42	53	N.	NE.	E.	...	...	...	42
☉ 8.	30·275	30·299	30·256	29·84	47·7	43·0	58·3	61	41	49·5	N.	NE.	calm	...	...	...	42
M. 9.	30·259	30·262	30·209	29·80	49·7	43·2	57·2	58	40	48	NE.	N.	calm	...	...	...	44
T. 10.	30·212	30·231	30·209	29·74	50·2	42·3	58·6	62	31	51	NE.	NE.	calm	...	...	...	41
W. 11.	30·204	30·217	30·151	29·62	53·6	40·5	64·8	70	38	57	WSW.	SW.	calm	...	...	...	43
Th. 12.	30·216	30·287	30·217	29·55	57·4	46·5	68·6	73	36	60	SW.	SW.	calm	...	...	...	45
F. 13.	30·356	30·392	30·346	29·72	58·5	46·8	67·4	73	42	60	W.	W.	calm	...	...	...	47
S. 14.	30·562	30·480	30·464	29·94	60·2	51·5	67·4	72	39	61	N.	NW.	NE.	...	...	...	53
● ☉ 15.	30·608	30·537	30·497	29·91	57·7	49·9	66·7	74	41	63	E.	S.	calm	...	...	...	50
M. 16.	30·548	30·535	30·510	29·86	60·0	50·9	69·5	73	41	61·5	N.	E.	calm	...	...	...	51
T. 17.	30·564	30·548	30·412	29·93	59·3	49·6	68·5	74	43	62	SE.	E.	calm	...	...	...	52
W. 18.	30·388	30·377	30·292	29·75	57·2	50·5	65·3	70	47	67	E.	E.	calm	...	...	...	53
Th. 19.	30·311	30·281	30·183	29·71	51·4	48·0	58·2	61	40	53	E.	E.	calm	...	...	...	49
F. 20.	30·089	30·104	29·942	29·60	53·5	44·3	66·5	72	45	57·5	E.	SE.	W.	...	·14	...	47
S. 21.	30·016	30·043	29·996	29·55	56·7	49·6	61·7	65	39	57	NE.	E.	E.	·133	...	·15	52
☉ 22.	30·010	29·999	29·878	29·58	52·6	42·6	55·3	59	40	53	E.	SE.	E.	...	·06	...	43
☽ M. 23.	29·846	29·986	29·480	29·55	52·3	44·0	57·3	59	45	53	S. var.	NE.	E.	·044	·01	...	45
T. 24.	30·109	30·237	30·098	29·75	51·8	46·3	59·3	60	39	51	NE. var.	NE.	E.	...	...	...	43
W. 25.	30·265	30·278	30·264	29·85	51·2	44·2	58·2	60	41	51·5	NNE.	NE.	NE.	...	...	...	45
Th. 26.	30·338	30·349	30·332	29·93	53·2	45·7	56·2	63	38	54	E.	E.	E.	...	...	...	44
F. 27.	30·425	30·411	30·383	30·	54·6	41·8	58·9	64	33	56	E.	E.	E.	...	...	...	42
S. 28.	30·396	30·395	30·351	29·94	53·2	42·0	62·7	66	45	57	NE. var.	NE.	calm	...	...	...	44
☉ 29.	30·380	30·371	30·325	29·90	54·7	47·3	63·8	66	36	56	NE.	NE.	E.	...	...	...	45
○ M. 30.	30·301	30·303	30·186	29·82	54·7	43·4	65·0	68	45	58	NNE.	NE.	NE.	...	...	...	46
T. 31.	30·109	30·112	29·952	29·66	55·3	47·4	67·6	70	47	55	N.	NE.	calm	...	·01	...	46
	30·197	30·548	29·480	29·71	53·1	44·7	60·6	74	31	54·8				Sum ·808	1·01	0·47	45·2

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

AUGUST 1836.

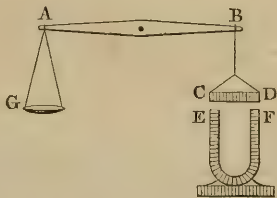
XVI. *On the Cause of the remarkable Difference between the Attractions of a Permanent and of an Electro-Magnet on Soft Iron at a Distance.* *By the Rev.* WILLIAM RITCHIE, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution and in the University of London.**

AS soon as the electro-magnet was constructed and employed to illustrate the immense magnetic power communicated to soft iron, it must have been observed that its attraction for iron filings or pieces of soft iron *at a distance* was much less than that of a permanent magnet of equal *lifting* power. This peculiar property rendered the electro-magnet not well suited for magnetic induction at a distance; and hence, after a few unsuccessful trials to substitute it for the permanent magnet in my apparatus for continued rotation, it was long since abandoned. In a short paper by Mr. Rainey in the last Number of this Journal, p. 72, the fact is stated, and an explanation attempted to be given of this peculiarity; but I am afraid the explanation will not be found in accordance with the present state of the science. This subject having engaged my attention some years ago, I had several times commenced a paper intended for the Philosophical Magazine, but other more pressing subjects prevented me from finishing it. As the fact is a necessary consequence of the properties of

* Communicated by the Author.

electro-magnets which I formerly made public in your Journal*, I take the liberty of sending you the present investigation, which may be regarded as the completion of my former paper.

Experiment 1. Suspend a piece of soft iron, C D, at the extremity of a slender delicate balance of light wood; place a permanent horse-shoe magnet below it, and ascertain its attractive force, by weights put into the scale G, when it is in contact, and also when it is removed to different distances from the soft iron. Remove the permanent magnet, and substitute a very *short* electro-magnet of equal lifting power. Remove it to the same distances as before, and the attractive power will diminish very rapidly compared with that of the permanent steel magnet.



Exp. 2. Instead of the short electro-magnet, substitute a very long one (one of two or three feet long, for example,) and of equal carrying power; remove it to the same distances and ascertain its attractive power, and it will be found that its attraction for the lifter at these distances will *not* diminish so rapidly as that of the short one. The *longer* the electro-magnet becomes, the *more* does it approach to the character of the permanent magnet in all its properties.

Exp. 3. Instead of making the electro-magnet of *soft* iron, make it of *hard* iron or untempered steel; repeat the preceding experiments, and its attractive power at a distance compared with its lifting power will be much greater than in the case of the electro-magnet of soft iron.

These facts, which, as far as I know, have not before been published, will enable us to account for this property on principles previously recognised. The perfect equality of *action* and *reaction* must be found to exist in this case as well as in every other in which *force* of any kind is concerned. The electricity which has been decomposed and arranged in the soft

* [Prof. Ritchie's papers "*On the Power of an Electro-Magnet to retain its Magnetism after the Battery has been removed*," and "*On certain curious Properties of Common and Electro-Magnets*," will be found in Lond. and Edinb. Phil. Mag., vol. iii. pp. 122, 124.—Edit.]

iron in the peculiar manner which constitutes magnetism, cannot decompose and arrange the electricity belonging to the lifter without suffering a corresponding *diminution*, and the more difficult the arrangement in the lifter so much greater will be the diminution of power in the electro-magnet. Again, if the electricity in the electro-magnet be easily arranged by the induction of the voltaic helix, it will be easily forced back to its natural state by the *reaction* of that belonging to the lifter. Hence it follows that when the *inducing* power of the electro-magnet is very great (which it is when the lifter is in contact with its ends) it will possess sufficient power to *vanquish* the *coercitive* force of the lifter, arrange by induction a large portion of the electricity of the lifter, and thus exhibit powerful attraction. When the lifter is removed to a certain distance, one tenth of an inch for example, the power of the electro-magnet being much *diminished* in consequence of the distance, whilst the difficulty of overcoming the coercitive force of the lifter is *increased*, the effect will be very small compared with the former. For if the inducing power be only *equal* to the coercitive force of the lifter, no attraction whatever will take place; and hence the impossibility of magnetizing a large bar of steel tempered as hard as possible, by means of a small permanent magnet with a soft temper.

Now, if the coercitive force of the electro-magnet be increased, which is done either by employing a long magnet, or using hard iron or untempered steel, such a magnet will suffer a *less* diminution by the *reaction* of the lifter in the case of increased difficulty of arrangement in the lifter, than in the case of the short electro-magnet of perfectly soft iron.

In the case of the permanent magnet of tempered steel, the electricity belonging to it was arranged with *difficulty*, and after repeated *touches* of another magnet, and consequently it will easily vanquish the coercitive power of a piece of soft iron, and induce a magnetic state upon it, whilst the peculiar arrangement of its own electricity will remain nearly *unchanged*. Hence its *attractive* powers will diminish *nearly* as the squares of the distances of the soft iron from its poles, or *imaginary* centres of accumulation, a law which cannot exist in the case of the electro-magnet the electricity of which is so easily put in motion round the elementary molecules of the iron by the reaction of the lifter.

In the explanation given by Mr. Rainey the lifter is supposed to *react* powerfully on the electro-magnet so as to increase its power, a supposition which cannot possibly be admitted. For the electro-magnet must, in the first place, give the lifter *all* its magnetic power, consequently the power of the lifter never can *exceed* that of the electro-magnet, and consequently never can *induce* a higher magnetic state in it than what has already been done by the voltaic helix.

XVII. *Remarks on the* Rev. J. H. Pratt's *Demonstration of a Proposition in the* Mécanique Céleste. *By* A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

At page 474 of your Journal for last month (June, vol. viii.) an old controversy is revived. Whatever may have been intended by bringing forward this subject at the present time, it is certain that no new light is thrown upon it.

In order to separate what is clear and undeniably proved from the point in which lies the difficulty, it will be necessary to set out from M. Poisson's statement of the problem, viz.

$$X = \frac{1}{4\pi}\int_0^{\pi}\int_0^{2\pi} \frac{(1-\alpha^2) f(\theta', \psi') \sin\theta' \, d\theta' \, d\psi'}{(1-2\alpha p+\alpha^2)^{\frac{3}{2}}},$$

$$p = \cos\theta \cos\theta' + \sin\theta \sin\theta' \cos(\psi-\psi');$$

the arcs θ and ψ are the initial values of the variable arcs θ' and ψ', which increase from θ and ψ to $\theta+\pi$ and $\psi+2\pi$; and α is a constant which is here supposed less than 1, but may approach to it indefinitely. These things premised, if any value be assigned to α, the symbol X stands for the definite integral taken between the extreme limits of the variable arcs.

1st. Let the expression under the sign of integration be expanded in a series proceeding by the powers of α, then

$$X = \frac{1}{4\pi}\int_0^{\pi}\int_0^{2\pi} f'(\theta', \psi') \sin\theta' \, d\theta' \, d\varpi'$$

$$\{1 + 3\alpha P_1 + 5\alpha^2 P_2 \dots (2i+1) P_i \dots\}.$$

For the sake of simplifying put

$$Y_i = \frac{2i+1}{4\pi}\int_0^{\pi}\int_0^{2\pi} P_i f(\theta', \psi') \sin\theta' \, d\theta' \, d\varpi';$$

then

$$X = Y_0 + \alpha Y_1 + \alpha^2 Y_2 \dots\dots \alpha^i Y_i \dots .$$

All the coefficients, P_i, satisfy a certain equation in partial differentials relatively to the arcs θ and ψ, which equation, as it is well known, need not be here transcribed. This property is owing to the combinations of θ and ψ which enter into the composition of these coefficients; and as the same combinations remain unchanged in the integrals, Y_i, these integrals necessarily satisfy the same equation of partial differentials. Thus the peculiar nature of the integrals Y_i does not depend upon the function $f(\theta', \psi')$, but is derived from the coefficients P_i.

It is obvious that the coefficients P_i are every one susceptible of one form only: and hence the several integrals, Y_i, taken between the extreme limits of the variable arcs, are susceptible of one value only.

When α is less than 1, the foregoing value of X is expressed by a converging series, single in its form. If we put X' for what X becomes when $\alpha = 1$, and further suppose that the integrals, Y_i, form a converging series, we shall have

$$X' = Y_0 + Y_1 + Y_2 \ldots\ldots Y_i \ldots\ldots .$$

The value of X' is therefore a series, single in its form, and such as M. Poisson has determined. But, in what has been said, there is not a word to prove that $X' = f(\theta, \psi)$, or that X' will be found by substituting in $f(\theta', \psi')$, the initial arcs θ, ψ, for the variable arcs θ', ψ'. The proof of this is the second part of M. Poisson's investigation; and it is here that the difficulty lies. Those who hold that his investigation is rigorous, must admit the truth of the resulting theorem; those who are content with results, cannot repose confidence on better authority; those who seek unexceptionable evidence in the mathematics, may find reason to demur at some parts of the proof. As Mr. Pratt has not touched on this point, it is not necessary to notice it further.

2ndly. Let us now make a particular supposition respecting $f(\theta', \psi')$, namely, that it is a rational function in finite terms of the three quantities, $\cos\theta'$, $\sin\theta'$, $\cos\psi'$, $\sin\theta'$, $\sin\psi'$; which will be verified both when $f(\theta', \psi')$ is actually a finite function of the three quantities, and when it is a converging series of such functions. According to what is usually taught $f(\theta', \psi')$ and $f(\theta, \psi)$ may, each, be expanded at least into one determinate series, viz.

$$f(\theta', \psi') = Z_0' + Z_1' + Z_2' \ldots Z_i' \ldots,$$
$$f(\theta, \psi) = Z_0 + Z_1 + Z_2 \ldots Z_i \ldots,$$

all the terms of the developments satisfying the fundamental equation in partial differentials. If we now substitute the series for $f(\theta', \psi')$ in the terms of X′, we shall obtain simply,

$$Y_i = \int_0^{\pi}\int_0^{2\pi} P_i f(\theta', \psi') \sin\theta' \, d\theta' \, d\varpi'$$

$$= \int_0^{\pi}\int_0^{2\pi} P_i Z_i' \sin\theta' \, d\theta' \, d\varpi' = Z_i,$$

because all the parts of $f(\theta', \psi')$, except only Z_i, produce nothing in the integral. It is, therefore, proved, in this particular supposition respecting $f(\theta', \psi')$, that the developments of X′ and $f(\theta, \psi)$ are identical in all their terms: and as the first series has been shown to be susceptible of one form only, the same thing must be true of the other series. In order to arrive at this conclusion it was not even necessary to introduce the expansion of $f(\theta', \psi')$: for, in the case in hand the equation

$$X' = f(\theta, \psi)$$

is easily deduced by direct integration, as was done many years ago.

All that we properly know of this theory, because it is all that is strictly proved, is contained in what has been said. The process which M. Poisson has invented for demonstrating the equation

$$X' = f(\theta, \psi)$$

without restriction, is merely an analytical artifice founded on assumptions: it has not been verified by any rigorous investigation; on the contrary, it is opposed to every such investigation.

Mr. Pratt's observations fall to the ground; since, contrary to what he assumes, every proposed function is susceptible of one development only. The term Y_i in the expansion of X′, or Z_i in the expansion of $f(\theta, \psi)$ in the particular case mentioned, is equal to the integral

$$\int_0^{\pi}\int_0^{2\pi} P_i f(\theta', \psi') \sin\theta' \, d\theta' \, d\varpi',$$

which has one value only; because the arcs θ' and ψ' vary between determinate limits; and, for any assigned values of these arcs, each of the functions P_i and $f(\theta', \psi')$ is single in its form.

It seems to be implied in the language used by Mr. Pratt, that Professor Airy was the first who raised objections to this analytical theory propounded by Laplace, and who placed it in the point of view here given of it: but whether this be correct or not will best appear by citing the Professor's own words:—"I conclude with Mr. Ivory, that the theory (of the third book of the *Mécanique Céleste*) applies only to spheroids in which the elevation of the spheroid above the sphere ($f(\theta, \psi)$) is expressed by a rational function of cos θ, sin θ cos ψ, sin θ sin ψ."*

June 27, 1836. DISJOTA.

XVIII. *On the Hydrates of Barytes and Strontia.* *By Mr.* J. D. SMITH.

To Richard Phillips, Esq., F.R.S., &c.

MY DEAR SIR,

HAVING accidentally obtained from their respective solutions some fine crystals of barytes and strontia, I resolved to submit to experiment the various hydrates of these substances described by Dr. Dalton in his Chemical Philosophy, in order to ascertain whether mistakes, similar to those you pointed out some months since in the composition of the crystallized hydrates, had not occurred with respect to the remainder: in the course of which I endeavoured to verify the results at which you arrived in your paper in the Phil. Mag. of January 1835; but I found that the composition assigned by you to these hydrates did not agree with my experiments, as will presently be seen.

The crystals of barytes were obtained perfectly dry, by placing them in a funnel, suffering them to drain, and then wrapping them in several folds of filtering paper, thus excluding them from the air, by exposure to which they speedily effloresce, and are converted into carbonate of barytes.

Three analyses were made by dissolving the crystals in hot water, precipitating the barytes by dilute sulphuric acid, and collecting and igniting the precipitate. In the first experiment 30 grs. of the crystals gave 22·45 grs. of sulphate = 14·7 grs. of barytes; in the second, from 38 grs. of the crystals 27·89 of sulphate = 18·3 grs. of barytes were obtained; and the third afforded 27·77 grs. of sulphate = 18·2 gr. of barytes from 38·2 grs. of the crystallized hydrate. Now,

$$\begin{array}{lll} & \text{Bar.} & \text{Water.} \\ \text{1st exp.} & 14{\cdot}7 - 30{\cdot} & = 15{\cdot}3 \\ \text{2nd exp.} & 18{\cdot}3 - 38{\cdot} & = 19{\cdot}7 \\ \text{3rd exp.} & 18{\cdot}2 - 38{\cdot}2 & = 20{\cdot}0 \end{array}\left.\vphantom{\begin{array}{l}1\\1\\1\\1\end{array}}\right\} = 51{\cdot}2 \text{ of barytes combined with } 55{\cdot}0 \text{ of water, or } 76 \text{ of barytes, or 1 eq. and } 81{\cdot}6 \text{ or almost exactly 9 eqs. of water.}$$

The quantity of sulphate of barytes obtained in your ex-

* Transactions of the Cambridge Philosophical Society for 1826 (vol. ii. p. 389.).

periments would indicate 84·7 parts or nearly $9\frac{1}{2}$ eqs. of water combined with 76 parts or 1 eq. of barytes; this error is doubtless owing to your not having obtained the crystals quite dry, and not making allowance for the interposed water, which, in that case, they would have contained.

When crystals of barytes are heated to 212°, they fuse in their water of crystallization, and by continuing the heat until no more vapour is expelled, a porous and friable mass remains. 20 grs. of this residue being heated with hot water left 3·03 grs. of insoluble (carb. of barytes), and the solution precipitated by dilute sulphuric acid, gave after ignition 20·64 grs. of sulphate = 13·52 grs. of barytes. This experiment repeated on the same quantity gave 1·28 grs. of insoluble carbonate and 22·97 grs. of sulphate = 15 grs. of barytes. In the first experiment 20 − 3·03 = 16·97 − 13·52 of barytes leaves 3·45 for water; and in the second, 20−1·28 = 18·72−15· barytes leaves for water 3·72, the mean of which, 14·26 of barytes and 3·58 of water, is equivalent to 76 parts, or 1 eq. of barytes combined with 19 parts or a little more than 2 eqs. of water. This is the compound corresponding to the fifth hydrate of Dalton.

By exposing the crystallized hydrate to a red heat in a covered platinum crucible, applying the heat cautiously and gradually, it at first enters into the watery fusion, during which care must be taken to prevent it from boiling over, after which it fuses quietly down, leaving a light brown-coloured mass possessing a crystalline structure.

20 grs. of the crystallized hydrate heated to redness in a platinum crucible left 10·87 grs. −20 = 9·13 grs. of water expelled. 20 : 91·3 : : 157 the eq. of cryst. barytes : : 71·6, or almost exactly 8 eqs. of water, leaving 1 eq. of water in combination with 1 eq. of barytes. 28·52 grs. of the crystals left after ignition 15·47 = 13·05 grs. of water driven off, or 71·83 = 8 eqs. of water from 157, or 1 eq. of the crystallized hydrate. Thus the residual mass obtained by heating the crystallized barytes to redness is evidently a compound of 1 eq. of barytes + 1 eq. of water, and there exist three distinct hydrates of barytes, viz.

The 3rd, deposited from solution in water; the 2nd, obtained by heating the crystals in a water bath; and the 1st, by heating either of the preceding hydrates to redness, without access of air.

1	barytes	+ 9 water	=	157	
1	do.	+ 2 do.	=	94	equivalent numbers.
1	do.	+ 1 do.	=	85	

The crystals of strontia having been rendered perfectly dry in the same manner as the barytes, 30 grs. were dissolved in hot water and precipitated by bicarbonate of potash; the weight of the carbonate of strontia obtained was 17 grs. = 12 of strontia: a second experiment conducted in a similar manner afforded from 31·5 grs. 17·57 of carbonate = 12·34 of strontia.

	Strontia.	Water.
1st exp.	12—30	= 18
2nd exp.	12·34—31·5	= 19·16
	24·34 :	37·16 : : 52 : 80,

or almost exactly 9 eqs. water to 1 eq. of strontia. As the quantity of carbonate of strontia obtained by you would indicate rather more than 10 eqs. of water to 1 of strontia, I should imagine that your adoption of 10 eqs. of water to 1 eq. of strontia as the composition of the crystalline hydrate, may be traced to the same cause as in the case of the crystallized barytes, viz. dampness of the crystals employed.

If crystallized strontia is exposed to the heat of boiling water, it readily parts with a portion of its water of crystallization, without undergoing the watery fusion, and falls to powder. 21·6 grs. of this powder dissolved in water and precipitated by bicarbonate of potash gave 26·06 of carbonate of strontia; the experiment being repeated upon 20 grs., 24·34 grs. of carbonate were obtained: these respectively afford 18·3 and 17·1 grs. of strontia, combined, the first with 3·3 grs. of water = 21·6 grs., and the second with 2·9 of water = 20. The mean of these experiments affords from 20·8 grs. of the powder 17·7 of strontia, and 3·1 of water = 52 or 1 eq. of strontia, and 9·1 or 1 eq. of water.

When the crystallized hydrate of strontia is heated to redness it first falls to powder, then fuses, and finally leaves a white friable mass, which when moistened with water evolves heat similar to lime when slaked.

41·22 grs. of the crystals heated to redness in a covered platinum crucible left 16·58 grs., and this repeated on 64·2 grs. of the crystals left 24·72 grs.; the mean of these two experiments gave 32·1 grs. of water expelled from 52·7 grs. of the crystals; or from 133, the eq. of the crystallized hydrate, 81 or 9 eqs. of water driven off by a red heat. The residue on analysis proved to be anhydrous; for from 23·76 grs. of it, 1·16 grs. of insoluble matter, and 32·3 grs. of carbonate were obtained, which are equal to 22·7 of strontia + 1·16 of insoluble = to the weight originally taken.

These experiments show that by a red heat all the water may be expelled from the hydrates of strontia, of which there are two, viz.

The 9th hydrate, crystallized, composed of 1 eq. strontia + 9 eqs. water = 133; and the 1st hydrate, pulverulent, composed of 1 eq. strontia + 1 eq. water = 61.

On considering the compounds that these two metallic oxides, barytes and strontia, form with water, some singular facts present themselves to our notice: for instance, the extremely large proportion of water they unite with to form the crystallized hydrates, when compared to the hydrated oxides of all the other metals, to which they seem to form exceptions; for the law of combination, except with these two oxides, appears to be, *an equivalent of water to each equivalent of oxygen*; thus, the hydrates of potash, lime, nickel, zinc, &c. &c. are composed of 1 eq. of water and 1 eq. of oxygen to 1 of base; the hydrate of the sesquioxide of iron of 3 eqs. of water, 3 of oxygen, and 2 of metal, and the hydrates of the binoxides of tin, vanadium, &c. of 2 eqs. of water, 2 of oxygen, and 1 of base. Again, the second hydrate of barytes is the only known instance in which the protoxide of a metal unites with 2 equivalents of water to form a compound of 2 eqs. water, 1 oxygen, and 1 base. This earth also resembles the alkalies potash and soda, in retaining water at a red heat; unlike the hydrates of the other earths, lime, strontia, magnesia, &c. which lose the whole of their combined water, and become anhydrous, by exposure to an elevated temperature.

I am, my dear Sir, your obliged Pupil,

St. Thomas's Hospital, June 17, 1836. J. Denham Smith.

XIX. *Notice respecting* Dr. Ehrenberg's *Collections of dried* Infusoria, *and other microscopic Objects.*

To Richard Taylor, Esq., &c. &c.

Dear Sir, British Museum, 21st June 1836.

Dr. EHRENBERG of Berlin, well known for his elaborate work on the *Infusoria**, has recently presented to the British Museum a series of dried microscopic objects, consisting chiefly of infusory animalcules, globules of blood, &c., accompanied by a short notice (too short indeed) of his method of preparing them, and a list of the subjects. Dr. Ehrenberg preserves these most minute and perishable of known organic forms by means of rapid desiccation on little plates of mica, in which manner he informs us that he has suc-

* *Organisation systematik, &c. der Infusionsthierchen*, 3 vols. fol. Berlin, 1830—1834.

ceeded in making a very satisfactory dried collection, not only of nearly 300 species of *Infusoria*, but also of other kinds of microscopic objects. He mounts them between double plates of mica, fixed in the cells of slides, in the usual manner of preparing the scales of butterflies and *Poduræ*, and other transparent microscopic objects; and thus, he says, "I have not only preserved the form and colour of the *shielded* (*cuirassés*) *Rotatoriæ* and *Bacillariæ*, but also the *softest* and *most delicate* of the polygastric *Infusoria*, even those of the genus *Monas*; as well as the tissue of plants; the *Spermatozoa* and *Cercariæ*; the different sorts of globules of blood, with their nuclei; globules of lymph, chyle, and milk; and the nervous tubes, &c., of a great number of animals, and of man."

A power of about 300 (linear) is sufficient for viewing these objects, "but a lower power does not show them satisfactorily, however well they may be illuminated."

I subjoin a list of the subjects presented to the Museum, and remain, Dear Sir, faithfully yours,

John Geo. Children.

Slide, No. 1.

1. Monas viridis.
2. Polysoma uvella, and Monas termo.
3. Spirillum undula, and Vibrio bacillus.
4. Euglenia acus; Eu. viridis; Eu. pyrum.
5. Coleps hirsutus.
6. Volvox globator.

No. 2.

1. Paramecium caudatum.
2. Glaucoma scintillans.
3. Trichoda carnium.
4. Carchesium polypinum.
5. Epistylis nutans.
6. Euplotes Charon.

No. 3.

1. Stentor niger.
2. Paramecium aurelia.
3. Glaucoma scintillans.
4. Stentor polymorphus.
5. Stentor cœruleus.
6. Idem—compressed, to show the testiculi.

No. 4.

1. Nassula ornata.
2. Nassula elegans.
3. Nassula aurea.
4. Idem—crushed, to show the teeth.
5. Chilodon uncinatus.
6. Chlamydomonas pulvisculus.

No. 5.

1. Hydatina senta.
2. Idem—crushed, to show the teeth.
3. Polyarthra trigla.
4. Brachionus pala—with its eggs.
5. Brachionus rubens—ditto.
6. Anuræa aculeata.

No. 6.

1. Globules of blood of the Sheep (Ovis aries).
2. Ditto of the Frog (Rana temporaria).
3. Grains of the Retina of the Eye of the same.
4. Spermatozoa vespertilionis murini.
5. Arhnanthes longipes.
6. Meridion vernale; Fragilaria rhabdosoma; Navicula acus; Na. amphisbæna.

XX. *Some concluding Remarks on the Theory of Vanishing Fractions.* *By* J. R. Young, *Esq., Professor of Mathematics, Belfast College.**

MR. WOOLHOUSE's letter in the preceding Number of this Magazine (p. 18) renders it befitting that I should offer a few additional observations; although I had hoped that the many examples I had given of the entire fallacy of the principles in dispute, would have rendered any further recurrence to the subject on my part quite unnecessary. As far indeed as that letter can be regarded as a "reply" to my arguments, I see nothing, I must confess, to justify me in prolonging the discussion. It clearly, however, establishes the fact that Mr. Woolhouse has all along laboured under a mistake as to what the processes of mathematicians in the doctrine of vanishing fractions really are, for he asserts these processes to involve the absurdity of "multiplying and dividing by zero", and fancies that, by defending "the usual methods," I attempt to justify such operations.

Mr. Woolhouse says that his "especial object in writing the Essay was to show the impropriety of multiplying and dividing by zero," and he urges my attention to the logical inaccuracy of such processes. Now it appears to me that Mr. Woolhouse would have been as profitably employed in writing to show the impropriety of making two and two equal to five; for the fallacy which he denounces is never committed; and if, as indeed his letter shows, he has *imagined* this fallacy in the reasonings of Lacroix and Bourdon, to which I have already referred him, he has been the subject of a rather singular delusion. With this erroneous impression on his mind, however, it is not difficult to conceive how he has been led to make so many remarkable statements in reference to analytical processes and analytical results: he has been combating a doctrine which exists only in his own imagination; and all the "palpable inconsistencies" which he has discovered belong, not to the true theory which mathematicians have laid down, but to the fanciful system which Mr. Woolhouse has set up, unwittingly, in its stead.

Mr. Woolhouse has evidently adopted the old and exploded notion—indulged, I believe, by no other person living—that the elimination of the vanishing factor,—as, for instance, $x-a$, in such a case as $\frac{x^2-a^2}{x-a}$, is a mere artificial contrivance, to bring out, in the hypothesis of $x = a$, *one* only of the innumer-

* Communicated by the Author.

able values which the expression is competent to give, seeing that it then assumes the indeterminate form $\frac{a^2-a^2}{a-a}$. He has altogether overlooked the fact that the questions, What is the value of $\frac{x^2-a^2}{x-a}$ when $x=a$? and, What is the value of $\frac{a^2-a^2}{a-a}$? (questions which have certainly been confounded before,) are perfectly distinct. The *value* of $\frac{x^2-a^2}{x-a}$ when $x=a$ is not $\frac{a^2-a^2}{a-a}$, as Mr. Woolhouse supposes; this is merely the symbolical *form* which $\frac{x^2-a^2}{x-a}$ assumes in that case. The *value* of $\frac{x^2-a^2}{x-a}$ is $x+a$; and this value when $x=a$ is $2a$. The expression $\frac{x^2-a^2}{x-a}$ implies an *operation* to be performed, viz. division, and the value is the *result* of that operation. When the form $\frac{a^2-a^2}{a-a}$ is isolated, or detached from its interpretation, it is of course indeterminate; for the operation indicated requires that we determine such a quantity P, that when it is submitted to the reverse operation, $(a-a)$ P, the result may be a^2-a^2; and it is easily seen that an infinite variety of suitable values for P exist; for it may be generally expressed by

$$P=(a+a)\pm p.$$

But the *result* of the operation $\frac{x^2-a^2}{x-a}$ is definite, and distinctly points out which of the above infinite variety is comprehended among the values of $\frac{x^2-a^2}{x-a}$ in the ultimate state of the hypothesis, to the exclusion of all others.

Mr. Woolhouse fancies that when a vanishing factor, $x-a$, is introduced into an algebraic process an indeterminateness is introduced at the same time. This is contrary to fact, and to the doctrine of all writers on the subject. The introduction of a foreign factor, whether by elimination or otherwise, can *never* affect the analytical limitations which existed before its entrance; and it is the well-known and universal practice of analysis to reject these foreign factors at the close of the process, although they are not always discoverable without an "appeal to the original analytical conditions." In the case, however, of what has been called a *vanishing* factor, the expulsion is always easily effected by the usual method of vanishing fractions, and Mr. Woolhouse will find upon examining *any* modern author that nothing *more* is effected.

Mr. Woolhouse appears to fall into great inaccuracy of reasoning at page 22, when he infers that the introduction of the factor $x-a$ into the equation

$$0 = (x-a)^{\alpha-\beta}\,\theta x - y\,\phi\,x, \text{ ....} \quad (q.)$$

must introduce new values for y; or when he supposes that the result of this introduction, viz.

$$0 = (x-a)\,\{(x-a)^{\alpha-\beta}\,\theta x - y\,\phi\,x\}, \text{.........} \quad (p.)$$

is equivalent to the two independent equations

$$0 = x-a \quad \text{............................} \quad (r.)$$

$$0 = (x-a)^{\alpha-\beta}\,\theta x - y\,\phi\,x \quad \text{.........} \quad (s.)$$

If Mr. Woolhouse regard this as sound logic, the logic of Lacroix must seem to him "palpably inconsistent" indeed. I beg to suggest to Mr. Woolhouse that his equation ($r.$) does *not* exist independently of, but only simultaneously with, the equation ($s.$); and he will not be able to point out any writer who argues otherwise. He is also wrong in affirming that the equation ($p.$), due regard being paid to the circumstance of a foreign factor entering it, becomes indeterminate for $x = a$. That equation, as to number of admissible values, is identical with ($s.$). If this be satisfied for $x = a$, then $x = a$ will be an admissible solution of ($p.$), but not else, seeing that ($p.$) resolves itself, when $x = a$, into the two *simultaneous* equations ($r.$) and ($s.$); and it is distinctly in reference to this circumstance that the solutions of ($p.$) are to be viewed.

Such is, in substance, the doctrine of all modern analysts, but it is very different from that which Mr. Woolhouse has gratuitously condemned. I regret that he did not examine, with more attention, the process of Lacroix, in solving the problem of Clairaut, to which I referred him in my former letter. I think if he had done this, instead of passing over in silence so decided an argument against his own "principles" as that process furnishes, he surely would have spared the truisms with which his "Reply" abounds.

It is no doubt *possible* that Mr. Woolhouse may have met with some obscure work in which his zero processes may occur. I certainly have never seen any such work; and if one exist it must be perfectly unique; and I trust that Mr. Woolhouse will make it known to the public, in order that connoisseurs may possess themselves of so singular a specimen of scientific absurdity.

From what I have now said it will be seen that I deny, *in toto*, the justness of Mr. Woolhouse's charge of bad logic in the common processes of the doctrine of vanishing fractions; and I have moreover very briefly shown how those processes ought to be interpreted. It is only upon this assumed bad logic that Mr. Woolhouse rests the stability of his remarkable "principles"; if then the logic is shown to be sound, but that Mr. Woolhouse has, unconsciously, misinterpreted the steps, what becomes of these said "principles"?

I shall now take my leave of this subject; I have carefully examined my former letters, and do not find a single remark which I wish to recall, nor a single mathematical statement at variance with received and well-established principles. The only alteration I would wish to make is, that the word "may" be substituted for "will" immediately after equation (2.) at page 517 of last volume; the "will" occurring for "may" justifies Mr. Woolhouse's foot-note at page 24.

Ample as are the materials which Mr. Woolhouse's last letter supplies for comment and objection, I shall in conclusion merely notice two points, more immediately concerning myself. At page 21, Mr. Woolhouse says that I "deny the competency of the results of the ellipse question to furnish the requisite values, and at the same time agree to receive them from the original analytical conditions." Mr. Woolhouse is again at fault; let him read what I really do deny, instead of attributing to me his own imaginings. I have said at page 298 (last volume) that "the *fact* of the problem admitting *multiple* solutions is information which the analytical result is incompetent to supply;" this is very different from asserting that these multiple solutions, *if they exist*, could not be furnished by the result; the question is—do they exist or not? and on this question the result supplies no information. Again, Mr. Woolhouse appears to have views different from other people on the subject of singular solutions, or else he uses technical terms in reference to this topic, in an unauthorized sense. It is sufficient for me here, without inquiring into his peculiar notions, to show that my remark, in reference to this subject, is in strict accordance with the received language of analysis. I have said that "singular solutions, though not comprised in the resulting integrals which furnish the general solutions to certain differential equations, have nevertheless the property of satisfying the proposed conditions." Lagrange expresses himself as follows: "La théorie des équations dérivées, porte naturellement à conclure que toute valeur qui peut satisfaire à une équation dérivée donnée doit être renfermée dans son équation primitive, pourvu que celle-ci ait toute la généralité dont elle est susceptible, par les constantes arbitraires qui doivent y entrer. Il y a néanmoins des equations dérivées auxquelles satisfont des valeurs que j'appelle *singulières*, parce qu'elles ne sont pas comprises dans leurs équations primitives*." Again: "On doit conclure de là que, pour que x soit une valeur singulière non comprise dans la valeur générale, il faut," &c.†

I here terminate these observations; nor shall I again trespass on the pages of the Philosophical Magazine, by any further remarks upon a subject which has now been so fully set before its readers.

July 6, 1836.

XXI. *Heights of Whernside, Great Whernside, Rumbles Moor, Pendle Hill, and Boulsworth.* By John Nixon, *Esq.*‡

THE following trigonometrical differences of level, measured at numerous stations for the determination of the above altitudes, having been calculated, under a range of distances of from four to thirty miles, with the *constant* refraction of the formula given in my last (vol. viii. p. 480), are submitted as a severe test of its claims to general accuracy. The details of some of the measurements have been already given, and those of the remainder will appear in my surveys of Wharfdale, Ribblesdale, &c.

For every station each day's observation of the difference of level between the standard hill and the other (calculated in the manner described in pages 437–8 of vol. vi.) is arranged in a separate line.

Trigonometrical Differences of Level.

Whernside above *Ingleborough.*

	Feet.		Feet.
At Great Whernside	45·0	At Ingleborough	41·2
Settronside§	40·5 (a)	———	38·8
———	41·2	———	40·0
Raisegill Hag	41·8	Penygent	42·3
Cosh	40·6	———	40·9
		———	39·8

* *Calcul des Fonctions*, p. 178. † *Ibid.*, p. 234.

‡ Communicated by the Author.

§ The mark (a) denotes that as there was no accompanying observation of the standard hill, its *average* difference of level was used in the calculation.

	Feet.
At Whan Fell	41·3
Dod Fell	43·3
Whernside	42·4
Knoutberry Hill	40·8
Great Whitber	41·9
———	41·5
Moughton Fell	41·7
Shunnor Fell	39·9
Hunt's Cross	42·6
Mean	+41·4
Height of Ingleborough	2384·5*
——— Whernside	2426·0

Ingleborough above *Great Whernside.*

At Great Whernside	68·9
———	69·7
———	61·4
———	65·6
———	66·0
———	65·8
Arncliff Moor†	58·3
Settronside	61·7 (*a*)
———	63·0
———	64·7
Raisegill Hag	56·1 ‡
Ryeloaf	64·2
Rumbles Moor	64·1 (*a*
———	62·4
———	57·5
———	65·3
———	66·0 (*a*)
———	62·6
Symon Seat	64·2
Pendle Hill	53·6
Penygent	61·4
———	65·4
———	64·9
Dod Fell	61·6
Knoutberry Hill ..	67·4
Grisedale Edge	66·1
Ingleborough	66·0
———	66·4
———	65·6
Mean ..	—63·7
Height of Ingleborough	2384·5
———Gt. Whernside	2320·8

Whernside above *Great Whernside.*

	Feet.
At Great Whernside ..	106·4
At Settronside	102·2
———	104·2 (*a*)
Raisegill Hag ‡ ..	97·9
Ingleborough	106·0 (*a*)
———	106·4 (*a*)
———	105·6 (*a*)
Penygent	103·7
———	106·3
———	104·7
Dod Fell	105·0
Knoutberry Hill ..	108·2
Mean ..	—105·3
Height of Whernside	2426·0
——— Gt. Whernside	2320·7
Do. by Ingleborough	2320·8
Mean	2320·8

Ingleborough above *Rumbles Moor.*

At Great Whernside ..	1056·7
——— ..	1052·5 (*a*)
——— ..	1049·7
——— ..	1057·0
Arncliff Moor † ..	1048·3
Settronside	1056·0
Rumbles Moor ..	1061·0
——— ..	1049·7
——— ..	1059·5
——— ..	1049·0
Symon Seat ..	1056·3
Pendle	1047·9
Ingleborough	1055·8
	—1053·8
Height of Ingleborough	2384·5
———Rumbles Moor	1330·7

Great Whernside above *Rumbles Moor.*

At Great Whernside ..	987·8
——— ..	986·3
——— ..	988·3
——— ..	991·2
Arncliff Moor ..	990·0
Settronside	993·8 (*a*)
———	989·4
Howber Hill	991·2
———	986·3
———	991·4 (*a*)
———	989·6 (*a*)
———	988·1
———	987·6

* See Lond. and Edinb. Phil. Mag., vol. vi. p. 440.

† At this station Ingleborough has evidently been measured in defect.

‡ The height of Great Whernside has been observed considerably in defect from Raisegill Hag.

	Feet.
Rumbles Moor ..	990·7
—— ..	998·6
—— ..	992·2
—— ..	994·2
—— ..	988·8
—— ..	992·2
Gt. Almias Cliff ..	1001·7
Symon Seat	993·2
——	992·4
——	992·1
Pendle	994·3
Draughton Moor ..	988·7
—— ..	989·0
—— ..	988·5
Flasby Fell	986·8
Halton Height	985·4
—— ..	985·6
Ingleborough ..	989·8
Mean ..	—990·5
Ht. of Gt. Whernside	2320·8
—— Rumbles Moor	1330·3
Do. by Ingleborough	1330·7
Mean	1330·5

Ingleborough above *Pendle Hill.*

At Great Whernside ..	538·8
—— ..	549·3
—— ..	542·8 (*a*)
—— ..	548·0
Arncliff Moor	542·0
Settronside	551·1
Ryeloaf	540·2
Rumbles Moor ..	543·4 (*a*)
—— ..	551·4 (*a*)
—— ..	554·1
—— ..	554·0 (*a*)
—— ..	549·3
—— ..	560·1
Symon Seat	544·2
Pendle Hill	540·5
Ingleborough	536·7
——	536·2
Penygent	545·3
Knoutberry Hill ..	540·3
Smearsitt	544·1
Mean ..	—545·6
Height of Ingleborough	2384·5
—— Pendle Hill	1839·0

Great Whernside above *Pendle Hill.*

	Feet.
At Great Whernside ..	469·9
—— ..	479·6
—— ..	476·6
—— ..	482·2
Arncliff Moor	474·2
—— ..	483·7
Settronside	488·1 (*a*)
Ryeloaf	476·0
Howber Hill	474·0
——	486·7 (*a*)
——	477·0
Rumbles Moor	481·4 (*a*)
—— ..	487·3
—— ..	491·7 (*a*)
—— ..	492·0
—— ..	491·8
—— ..	494·8
Symon Seat	487·1
——	480·0
Pendle	486·9
Draughton Moor ..	480·0
—— ..	486·8
—— ..	485·8
Flasby Fell	480·4
Kilnsey Moor	478·3
——	476·0
Halton Height	480·9
——	483·6
Ingleborough	470·7 (*a*)
——	470·2
Penygent	483·9
Knoutberry Hill ..	472·9
Mean	481·6
Height of Gt. Whernside	2320·8
—— Pendle Hill ..	1839·2

Pendle Hill above *Rumbles Moor.*

At Great Whernside ..	518·0
—— ..	508·8 (*a*)
—— ..	509·7
—— ..	509·0
Arncliff Moor	506·3
Settronside	505·7
Howber Hill	512·3
——	504·7
——	510·7
Rumbles Moor	511·4
——	503·4
——	506·9
——	500·9
——	500·4
——	499·4
Symon Seat	505·3
——	512·1
Pendle Hill	507·4
Draughton Moor ..	508·8
—— ..	502·2
—— ..	502·7
Lippersley Pike	511·0

	Feet.
At Flasby Fell	506·4
Halton Height	504·5
———	502·0
Ingleborough	519·3
Mean	+507·3
Height of Rumbles Moor	1330·5
——— Pendle Hill	1837·8
Pendle Hill by Inglebro'	1839·0
——— Gt. Whernside	1839·2
——— Rumbles Moor	1837·8
Mean	1838·7*

Ingleborough above *Boulsworth.*

At Great Whernside	674·7
Arncliff Moor	667·0
Rumbles Moor	685·2 (*a*)
———	682·4
———	680·3
Symon Seat	680·0
Pendle	665·5
	−676·5
Height of Ingleborough	2384·5
——— Boulsworth	1708·0

Great Whernside above *Boulsworth.*

At Great Whernside	608·9
Arncliff Moor	608·6
Howber Hill	610·4 (*a*)
———	611·5
Rumbles Moor	621·1
———	625·0
———	615·0
Symon Seat	615·7
Pendle	611·4
Draughton Moor	614·2
———	614·8
Halton Height	611·2
———	608·3
Mean	613·6
Height of Gt. Whernside	2320·8
——— Boulsworth	1707·2

Boulsworth above *Rumbles Moor.*

	Feet.
At Great Whernside	382·3
Arncliff Moor	381·4
Howber Hill	379·2
———	376·1
Rumbles Moor	369·6
———	367·3
———	379·2
The Chevin	377·0
Symon Seat	376·4
Pendle	383·0
Draughton Moor	374·5
———	374·2
Lippersley Pike	380·8
Halton Height	374·2
———	377·3
Mean	+376·8
Height of Rumbles Moor	1330·5
——— Boulsworth	1707·3

Pendle above *Boulsworth.*

At Great Whernside	126·7
Arncliff Moor	125·0
Howber Hill	129·6 (*a*)
———	134·6
Rumbles Moor	133·8
———	133·1
———	120·2
Symon Seat	135·7
Pendle	124·5
Draughton Moor	134·3
———	128·0
Lippersley Pike	130·2
Halton Height	130·3
———	124·7
Mean	−129·3
Height of Pendle	1838·7
——— Boulsworth	1709·4

Boulsworth by Inglebro'	1708·0
——— Great Whernside	1707·2
——— Rumbles Moor	1707·3
——— Pendle	1709·4
Mean	1708·0

It will be seen from the following statement, that Colonel Mudge's measurements of the above altitudes (above mean low water, spring tides,) are invariably less than mine.

* This height is exclusive of the Beacon hillock (about 7 or 8 feet high).

	Mudge.	Nixon.	Diff.
Ingleborough	2361	2384·5	+23·5
Great Whernside	2263	2320·8	+57·8
Whernside (in Ingleton Fells)	2384	2426·0	+42·0
Pendle Hill	1824*	1838·7	+14·7
Boulsworth Hill	1689	1708·0	+19·0
Rumbles Moor..................	1308	1330·5	+22·5

Ilkley, May 24, 1836. JOHN NIXON.

XXII. *On a Property of the Parabola. By* J. W. LUBBOCK, *Esq., F.R.S.*†

IN the 8th volume of Gergonne's *Annales de Mathématiques*, p. 9, M. Poncelet has given the following theorem: "Un triangle étant circonscrit à une parabole, si on lui circonscrit à son tour une circonférence de cercle, elle passera necessairement par le foyer même de la courbe."

See also a paper by M. Steiner in the 19th volume of the same work.

The proofs which have been given of this elegant property of the parabola are indirect, and however ingenious they may be, it seems desirable to show how the theorem in question may be deduced immediately from the equation to the curve. The general methods of analytical geometry may be deemed incomplete and imperfect while they do not embrace questions of this nature, and their great advantage is liable to be overlooked.

Let A B C be a triangle, and let $x_1, y_1, x_2, y_2, x_3, y_3$, be the coordinates of the points A, B, C. I propose to prove that if the lines A B, B C, A C touch a parabola the focus of the parabola is in the circumference of the circumscribing circle A B C.

The equation to any straight line passing through given points (x_1, y_1), (x_2, y_2) is

$$y - y_2 = \frac{y_1 - y_2}{x_1 - x_2}(x_1 - x_2).$$

The equation to the tangent passing through the point (x_1, y_1) and touching a curve in the point (x, y) is

$$y - y_1 = \frac{\mathrm{d}\, y}{\mathrm{d}\, x}(x - x_1).$$

This equation is generally given for rectangular coordinates

* This height is probably *inclusive* of the Beacon hillock.

† Communicated by the Author.

only, but the reasoning by which it is established is equally applicable to coordinates inclined to each other at any angle.

Let $y^2 = 2px$ be the equation to a parabola referred to any coordinuate axes Ox, Oy oblique or rectangular. $y = Vq$, $x = PV$, $p = 2SP$. (See Bridge's Conic Sections, p. 15.)

$$\frac{dy}{dx} = \frac{p}{y} = \frac{y_1 - y_2}{x_1 - x_2},$$

if the tangent passes through (x_1, y_1), (x_2, y_2).

$$y_1 - y_2 = \frac{p}{2} \frac{(x_1 - x_2)^2}{(x_1 y_2 - x_2 y_1)} \qquad (1.)$$

Similarly,

$$y_3 - y_1 = \frac{p}{2} \frac{(x_3 - x_1)^2}{(x_3 y_1 - x_1 y_3)} \qquad (2.)$$

$$y_2 - y_3 = \frac{p}{2} \frac{(x_2 - x_3)^2}{(x_2 y_3 - x_3 y_2)} \qquad (3.)$$

By making the diameter of the parabola Ox pass through the point A, I may hereafter make $y_1 = 0$, without limiting the generality of the question. Subtracting (2.) from (1.);

$$x_1 (y_3 - y_2)(y_3 + y_2) + y_1 (x_1 y_2 - x_2 y_1 - x_1 y_3 + x_3 y_1)$$
$$= \frac{p}{2} (2x_1 - x_2 - x_3)(x_3 - x_2)$$

$$p(x_3 + x_2)(x_3 - x_2)(x_3 y_2 - x_2 y_3 - x_1 y_3 - x_1 y_2)$$
$$= 2y_1 (x_2 y_3 - x_3 y_2)(x_1 y_2 - x_2 y_1 - x_1 y_3 + x_3 y_1) \qquad (4.)$$

Hence if $y_1 = 0$

$$x_3 + x_2 = 0 \quad \text{or} \quad x_3 - x_2 = 0$$
$$\text{or} \quad x_3 y_2 - x_2 y_3 + x_1 y_3 - x_1 y_2 = 0.$$

In the second case $y_2 - y_3 = 0$; and since $x_3 = x_2$, $y_3 = y_2$ the points (x_2, y_2), (x_3, y_3), coincide and are identical; in the third case the points (x_1, y_1), (x_2, y_2), (x_3, y_3) are in the same straight line; it is useless therefore to consider these cases, and it is sufficient to take the first case only, namely, when $y_1 = 0$, and $x_3 + x_2 = 0$.

Let X, Y be the coordinates of the centre and R the radius of the *circumscribing circle* passing through the points (x_1, y_1), (x_2, y_2) (x_3, y_3), then by the equation to the circle,

$$(x_1 - X)^2 + (y_1 - Y)^2 + 2(x_1 - X)(y_1 - Y) \cos xy = R^2$$
$$(x_2 - X)^2 + (y_2 - Y)^2 + 2(x_2 - X)(y_2 - Y) \cos xy = R^2.$$

Subtracting the latter equation from the former, I find an equation which may be written as follows:

$$(x_1+x_2-2X)(x_1-x_2+(y_1-y_2)\cos xy)$$
$$+(y_1+y_2-2Y)(y_1-y_2+(x_1-x_2)\cos xy)=0.$$

Substituting in this equation for y_1-y_2 its value

$$\frac{p}{2}\frac{(x_1-x_2)^2}{(x_1y_2-x_2y_1)}, \quad \text{(See p. 101).}$$

and dividing by x_1-x_2,

$$(x_1+x_2-2X)\left(x_1y_2-x_2y_1+\frac{p}{2}(x_1-x_2)\cos xy\right)$$

$$+(y_1+y_2-2Y)\left(\frac{p}{2}(x_1-x_2)+(x_1y_2-x_2y_1)\cos xy\right)=0,$$

which equation may be written in the form

$$\{x_1+x_2+x_3+\frac{p}{2}-2X$$
$$+(y_1+y_2+y_3-2Y)\cos xy\}\ (x_1y_2-x_2y_1)$$
$$+\frac{p}{2}\left\{\{(x_1+x_2)\cos xy-2Y-2X\cos xy\}(x_1+x_2)+x_1y_1-x_2y_2\right\}$$
$$(x_3+y_3\cos xy)\ (x_1y_2-x_2y_1)=0. \qquad (5.)$$

and also by symmetry, since the three points (x_1, y_1), (x_2, y_2), (x_3, y_3) are similarly related,

$$\{x_2+x_3+x_1+\frac{p}{2}-2X$$
$$+(y_2+y_3+y_1-2Y)\cos xy\}\ (x_2y_3-x_3y_2)$$
$$+\frac{p}{2}\left\{\{(x_2+y_3)\cos xy-2Y-2X\cos xy\}(x_2-x_3)+x_2y_2-x_3y_3\right\}$$
$$-(x_1+y_1\cos xy)\ (x_2y_3-x_3y_2)=0 \qquad (6.)$$

$$\left\{(x_3+x_1+x_2+\frac{p}{2}-2X\right.$$
$$\left.+(y_3+y_1+y_2-2Y)\cos xy\right\}(x_3y_1-x_1y_3)$$
$$+\frac{p}{2}\left\{\{(x_3+x_1)\cos xy-2Y-2X\cos xy\}(x_3-x_1+(x_3y_3-x_1y_1\right\}$$
$$-(x_2+y_2\cos xy)\ (x_3y_1-x_1y_3)=0.$$

Adding together the three last equations, many terms destroy each other, and

$$\left\{x_1+x_2+x_3+\frac{p}{2}-2X+(y_1+y_2+y_3-2Y)\cos xy\right\}$$

$$\{x_1y_2-x_2y_1+x_3y_1-x_1y_3+x_2y_3-x_3y_2\}=0. \quad (7.)$$

Unless the three points (x_1, y_1), (x_2, y_2), (x_3, y_3) are in the same straight line

$$x_1+x_2+x_3+\frac{p}{2}-2X+(y_1+y_2+y_3-2Y)\cos xy=0. \quad (8.)$$

This equation is general, but simplifications result if $y_1=0$; (which supposition does not limit the generality of the solution of the problem proposed;) and in this case by p. 101, line 18, $x_2+x_3=0$, $x_2=-x_3$, equation (8.) becomes

$$x_1+\frac{p}{2}-2X+(y_2+y_3-2Y)\cos xy=0. \quad (9.)$$

and by equation (7.),

$$\left((x_1-\frac{p}{2}\right)(y_2+y_3)+p(Y+X\cos xy)=0. \quad (10.)$$

If $\quad Ax^2+Bxy+Cy^2+Dx+Ey+F=0,$
is the equation to any curve of the second order, the free result generally from the intersection of the lines whose equations are,

$$(B^2-4AC)(x^2-y^2)+(2BE-4DC)x-(2BD-4EA)y$$
$$+E^2-4CF-D^2+4AF=0.$$

$$(B^2-4AC)(x^2-y^2)+(BD-2AC)x+(BD-2DC)y$$
$$+2BF-ED-\{(B^2-4AC)y^2+(2BD-4AE)y$$
$$+D^2-4AE\}\cos xy=0. \quad \text{(See Phil. Mag. Aug. 1831.)}$$

If the equation to the curve is $y^2=2px$, in which case $A=0$, $B=0$, $C=1$, $D=-2p$, $E=0$, $F=0$, it is easy to deduce from the equations given above, or to prove otherwise, that the focus results from the intersection of the straight lines whose equations are $x=\frac{p}{2}$, and $y=-p\cos xy$.

Let x_4, y_4 denote the coordinates of the intersection of the *circumscribing circle* passing through (x_1, y_1), (x_2, y_2), (x_3, y_3) with the line

$$x=\frac{p}{2}, \qquad x_4=\frac{p}{2}.$$

In order to prove that the circle passes through the focus of the parabola it is sufficient to show that $y_4=-p\cos xy$. By the equation to the circle

$$\left(\frac{p}{2}-X\right)^2+(y_4-Y)^2+2\left(\frac{p}{5}-X\right)(y_4-Y)\cos xy=R^2$$

$$(x_1-X)^2+Y^2-2(x_1-X)Y\cos xy \qquad =R^2$$

Since $y_1 = 0$. Subtracting the latter equation from the former

$$\left(x_1 + \frac{p}{2} - 2X\right)\left(x_1 - \frac{p}{2} - y_4 \cos xy\right)$$
$$+ (y_4 - 2Y)\left(-y_4 + \left(x_1 - \frac{p}{2}\right)\cos xy\right) = 0,$$

which equation may be written as follows:

$$\left(x_1 - \frac{p}{2}\right)\left(x_1 + \frac{p}{2} - 2X - 2Y\cos xy\right)$$
$$-p\cos xy\,(Y + X\cos xy)$$
$$-(y_4 + p\cos xy)(y_4 - 2Y - 2X\cos xy) = 0.$$

By equation (9.)

$$x_1 + \frac{p}{2} - 2 + X(y_2 + y_3 - 2Y)\cos xy = 0; \text{ hence}$$

$$\left\{\left(x_1 - \frac{p}{2}\right)(y_2 + y_3) + p(Y + X\cos xy\right\}\cos xy$$
$$-(y_4 + p\cos xy)(y_4 - 2Y - 2X\cos xy) = 0.$$

Also by equation (10.)

$$\left(x_1 - \frac{p}{2}\right)(y_2 + y_3) + p(Y + X\cos xy) = 0; \text{ hence}$$
$$(y_4 + p\cos xy)(y_4 - 2Y - 2X\cos xy) = 0$$
$$y_4 = -p\cos xy, \text{ or } y_4 = 2Y + 2X\cos xy,$$

and it is evident that the *circumscribing circle* passes through the focus of the parabola.

XXIII. *On the Position of the South Magnetic Pole.* By Edward Rudge, *Esq., F.R.S., S.A., L.S. & H.S.**

THE experiments detailed by Captain James Clark Ross, R.N., &c., which led to the important discovery of the north magnetic pole, and which are published in the Philosophical Transactions for the year 1834, suggested to me as an object of interesting inquiry, whether any similar affection of the horizontal magnetic needle had ever been noticed by any former navigator of the *southern* hemisphere, from which an approach to the magnetic *south* pole could be surmised. No such appearances seem to have been observed by Anson, or any one after him; but prior to his circumnavigation of the globe, Captain Abel Tasman, who was appointed for the discovery of southern countries by direction of the Dutch East India Company, sailed from Batavia with two vessels on the

* Read before the Royal Society, Feb. 19, 1835; and now communicated by the Author.

14th of August 1642, in his account of the voyage, gives the following particulars of an observation made on the 22nd of November of the same year, when by a prior and subsequent observation of November the 15th and 24th, he was in about latitude 43° S., and longitude from Paris 160°.

"The needle was in continual motion without resting upon any of the eight points of the compass," which he says, "led him to conjecture that there were some mines of loadstone on that spot."

Tasman's Journal, written in Low Dutch, is now an extremely rare book: a translation of it is given in Dr. Hooke's Philosophical Tracts, p. 179, for the year 1682; in Narborough's and in Correal's Collections of Voyages; and also by Harris, who gives a new translation of it in the second edition of his Collection of Voyages, where, although he notices Dr. Halley's theory of the magnetic poles, which was published in 1683, he does not seem to suspect that Tasman's observation of this very remarkable affection of the magnetic needle was made in the immediate vicinity of the *south magnetic pole*, at that period in that particular situation, ascertained by the horizontal needle only; the dipping-needle, invented by Norman in 1681, being then unknown. Dr. Halley was of opinion that the north magnetic pole was not far from Baffin's Bay, and that the south magnetic pole was in the Indian Ocean, south-west from New Zealand; whether he had availed himself of the observation made by Tasman in forming this opinion, does not appear. Euler places the north magnetic pole for the year 1757 in latitude 76° north, and longitude 96° west from Teneriffe; and the south magnetic pole in latitude 58° south, and longitude 158° west from Teneriffe.

It has been ascertained by observation, that the magnetic poles were on the meridian of the poles of the earth at London in the year 1657, being fifteen years after Tasman's observations, and that it reached its utmost degree of variation west in the year 1818, when it became stationary at 24° 26′ west, and has since in respect of London been retrograding towards the east, completing one quarter of the circle round the poles of the earth in 161 years at the rate of 11 or 12 minutes of a degree in a year; so that, presuming Tasman was on the south magnetic pole on the 22nd of November 1642, it would now be found in or about the forty-third parallel of south latitude to the south-east of the island of Madagascar, a convenient situation, when compared with that of the north magnetic pole for ascertaining the exact position of the south magnetic pole, and where experiments with the horizontal- and dipping-needles to lead to its discovery and determine the comparative intensity of the south magnetic power might with facility be made. In pursuance of this desirable object the progress of the south magnetic pole might be accurately ascertained by annual observations; whether its distance from the south pole of the earth is uniform in its progress and if in an exact opposite direction to the north magnetic pole; to trace the point at which the axis of the magnetic poles crosses that of the earth; and thus by a continued series of observations and experiments a wide field might be opened to enlarge our hitherto imperfect knowledge of this mysterious power, which might be considered of so much importance in guiding and directing the motion of the earth on its axis and in its orbit.

Table of the Observations on the Magnetic Needle made by Captain John Abel Tasman *from the beginning to the termination of his Voyage; extracted from his Journal.*

Time.		Latitude.	Longitude from Paris.	Variation of the Needle.	
1642.					
Oct. 8 to	22.	40°40′ S.	....	23°24°&25°W.	
	22.	49 47	89° 44′	26° 45 W.	
Nov.	6.	49 4	114 56	26	
	15.	44 3	140 32	18 30 W.	
	21.	....	158	4 W.	
	22.	....	....	....	The needle in continual agitation.
	24.	42 25	163 50	....	The needle pointed towards the land, now first discovered and called Van Diemen's Land.
Dec.	1.	43 10	167 55	3 E.	Frederick Henry bay. Van Diemen's Land,
	9.	42 37	176 29	5 E.	New Zealand.
	18.	40 50	191 41	9 E.	
1643.					
January	8.	30 25	192 20	9 E.	
	12.	30 5	195 27	9 30 E.	
	16.	26 29	199 32	8 E.	
	19.	22 35	204 15	7 30 E.	
	21.	21 20	205 29	7 25 E.	
	25.	20 15	206 19	6 20 E.	
March	2.	9 11	192 46	10 E.	
	14.	10 12	186 14	8 45 E.	
	20.	5 15	181 16	9 E.	
	25.	4 35	175 10	9 30 E.	
April	1.	4 30	171 2	8 45 E.	
	12.	3 45	167	10 E.	
	14.	5 27	166 57	9 15 E.	
	20.	5 4	164 27	8 30 E.	
May	12.	0 54	153 17	6 30 E.	
	18.	0 26	147 55	5 30 E.	
	27.	6 12 S.	127 18	Returned to Batavia after 10 months' absence, having sailed round the Australian continent without seeing any part of it but the extremity of Van Diemen's Land.	

XXIV. *On Fluorine.* *By* G. J. Knox, *Esq.*, *and the Rev.* Thomas Knox.*

As far as the existence of a substance which had not hitherto been procured in an independent state could be determined, the experiments and reasoning of Davy and Berzelius are sufficiently conclusive. The only desideratum seems to have been the obtaining a vessel upon which this energetic principle would exert no action. Since fluorine shows no affinity for the negative elements oxygen, chlorine, iodine, and bromine, nor for carbon or nitrogen, it would appear that the vessel to contain it should consist of some solid compound of those substances; but as such vessels would be unable to bear exposure to a high temperature, we considered that though they might be convenient for retaining the gas when once obtained, they would not answer for its production. It was therefore necessary to employ some substance already saturated with the element; and for this purpose fluor spar, from bearing exposure to a high temperature and being easily formed into vessels, appeared best adapted. The most convenient method of obtaining the gas seemed to be by acting upon fluoride of mercury with dry chlorine, by which means, if the absence of moisture could be insured and the formation of a chloride of mercury obtained, fluorine must have been disengaged, and if present would be recognised by appropriate tests.

Placing dry fluoride of mercury in the fluor-spar vessel, we heated it till a glass plate cooled by the evaporation of sulphuret of carbon showed no trace of moisture in the vessel; the chlorine was then passed through a desiccating tube filled with fused chloride of calcium, the tube being bent at an angle, and its extremity drawn capillary, so as to enter the vessel, which, when filled with the gas, had its orifice closed with a plate of fluor spar which was fastened firmly down.

After exposing it to the heat of a spirit-lamp for some time, on removing the fluor spar cover, and replacing it rapidly with one of silica, it showed immediate and powerful action. The inside of the vessel was found on examination to be covered with crystals of bichloride of mercury; both of which results prove the presence of either fluorine or hydrofluoric acid; to determine which, we repeated the experiment, cooling the cover of the vessel so as to condense any hydrofluoric acid which might be present, but none appeared, from

* Communicated by the Authors.

which we inferred that fluorine and not hydrofluoric acid had been present in the vessel, which was also further confirmed by the absence of fumes when the vessel and its contents had been previously dried.

Placing inverted over the orifice of the vessel a clear crystal of fluor spar, with a small perforation in the centre into which a stopper of fluor spar fitted accurately, on the stopper falling into the vessel the tube was filled with a yellowish green gas, the colour of which deepened with heat, and disappeared when cold. On reheating the vessel below, the gas rose again into the crystal above. On removing the crystal while hot to a wet glass plate, it flew to pieces, which prevented us from determining whether the coloured gas was bichloride of mercury under heat and pressure, hydrofluoric acid, or fluorine.

Having procured larger vessels with receivers into which ground stoppers were made to fit accurately, we resumed in the present month the experiments we had tried in the beginning of April.

1st Exp. We heated fluoride of lead with oxygen, and afterwards with dry chlorine without action upon the fluoride. When the receiver (its stopper having fallen into the vessel below) was placed over gold-leaf, a *chloride* of gold was formed.

2nd Exp. Treating hydrofluate of ammonia similarly with chlorine, there was strong action on glass and formation of *chloride* of gold as before.

3rd Exp. Treating fluoride of mercury with chlorine (as we had done in our former experiments), we obtained crystals of bichloride of mercury in the vessel. Leaving the receiver over gold-leaf, there was after a considerable time action on it, producing a yellowish brown appearance. This we placed on a slip of glass, and on adding a few drops of sulphuric acid and evaporating to dryness there was very strong action on the glass where the gold had been, proving that it was a fluoride of gold, and that since gold is not acted on by hydrofluoric acid there must have been fluorine in the receiver. As an additional corroboration there was no hydrogen in the tube, which there would have been had hydrofluoric acid been decomposed by the gold. From these experiments we conclude that fluorine was present in the receiver, but whether a slight trace of hydrofluoric acid (to which the action on glass was due) may not have been present with it, we have not yet determined. We hope on a future occasion to be able to give particulars with regard to the properties of the gas; but we consider that the present results are sufficiently important to justify us in submitting them to the public through the medium of your Magazine. We remain, Gentlemen, yours, &c.

Toomavara, Tipperary, July 1836.

T. Knox,
Geo. J. Knox.

Explanation of Figures.

Fig. 1. The vessel with the receiver in the stand which holds down the receiver by means of spiral springs A.

Fig. 2. Vessel with cover off, showing the orifice and the small depressions in which the gold-leaf, &c. were placed.

Fig. 3. Receiver without stopper.

Fig. 4. The stopper.

Fig. 1.

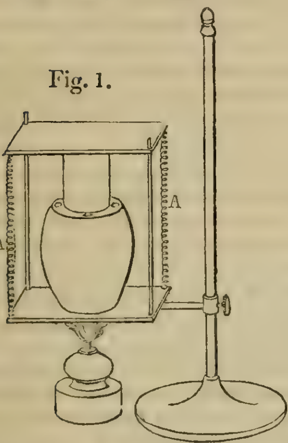


Fig. 2.

Fig. 3.

Fig. 4.

XXV. *On certain new Combinations of Albumen, with an Account of some curious Properties peculiar to that Substance. By* Golding Bird, *F.L.S., F.G.S., Senior Fellow of the Physical Society of Guy's Hospital, &c.**

1. IN the course of the following observations I shall avoid any unnecessary reiteration of facts already well known to chemists, and confine myself to referring to them only when they are required to explain any circumstances connected with those new modifications or combinations of albumen which have fallen under my notice. Our knowledge of the properties of albumen, although more extended than that

* Communicated by the Author.

of most other animal matters, is nevertheless very limited, which limitation arises, in all probability, from its comparatively weak affinity for other bodies, which prevents our becoming acquainted with anything like very prominent or interesting features; I am however convinced that the study of the chemical nature of albumen will reward the investigator with a richer harvest of facts than that of any of the other proximate constituents of the animal frame, as well from its prevalence under some modification or other in every secretion in the body, as from its being the chief constituent of the circulating fluid, and constituting, if I may be allowed the expression, the type of the albuminous principles (*properly* so called) of the blood, and the pabulum from which the different secretions are formed and the waste of the body repaired. Indeed, by a synthetic method, founded to a certain extent upon some of the novel properties of albumen I am about to mention, I trust to be able in a future paper to prove that many, if not all the secretions contain albumen, although its presence has not been suspected, or if suspected not detected, and that they are indebted to the presence of a peculiar combination of this principle for many of their most prominent characters. In the course of my investigations I had frequently occasion to observe that albumen procured from different sources frequently differed slightly in its behaviour to reagents, and an ignorance of this fact led at first to considerable discrepancy in the results of my experiments; thus I may observe that the white of egg and the albuminous secretions of serous surfaces very closely resembled each other, but differed in degree of solubility and many other minor properties from the albumen of serum of blood, which I have generally made the subject of my experiments, after freeing it from fat by agitation with sulphuric æther; and to this form of albumen I shall constantly refer in the course of the following observations.

2. Some serum freed from fat (1.) was mixed with a sufficient quantity of a solution of pure soda to cause it strongly to affect turmeric paper; the heat of a water-bath was applied, the mixture being constantly stirred: in a short time it appeared to solidify, forming a pale yellowish transparent jelly which scarcely at all affected turmeric paper. Distilled water being then added, and heat again applied, a nearly limpid, but somewhat mucilaginous solution of albuminate of soda resulted, which became quite transparent by filtration; it was not at all affected by boiling or the addition of alcohol, but was precipitated by the acids, solutions of chlorine, alum, acetate of lead, bichlorides of iron and mercury, sulphate of copper, ferro-

cyanate of potash (after the addition of acetic acid), and tincture of galls; reactions quite characteristic of solutions of alkaline albuminates.

3. A solution of albuminate of soda (2.) was diluted considerably with cold distilled water and placed in a tall cylindrical glass vessel; through this fluid a current of carbonic acid gas was passed, the tube from which the gas issued being sufficiently long to reach the bottom of the vessel: in a few minutes the fluid, before transparent, became opake, and rapidly deposited a copious precipitate of albumen in the state of a beautifully white, impalpable powder; the acid gas being allowed to pass for some time longer, the precipitate gradually disappeared until the whole was as limpid as before the experiment. The solution thus obtained was acid, and consequently reddened litmus-paper, whereas before the experiment it reddened turmeric; it afforded a copious deposit with those reagents which precipitate solutions of albumen in mere water, or when dissolved by acids, but not with those which affect solutions of albumen in the alkalies only: thus, ebullition caused a considerable deposit, as also did nitric acid, tincture of galls, bichloride of mercury, and alum; whereas the dilute acids and perchloride of iron did not disturb the limpidity of the fluid, although, as before stated, prior to the passage of the acid gas they produced copious precipitates. Heat, I have already stated, caused a considerable deposit of albumen in the same manner as from a mere aqueous solution of albumen which had not undergone coagulation, but differed in requiring a higher temperature, a full boiling-heat being necessary to produce a considerable precipitate. Ammonia when very dilute caused a precipitate also, which readily dissolved in an excess of the precipitant; the action of heat was of course accompanied with a copious evolution of carbonic acid gas. From these facts I was induced to conclude that the albumen previously existing in combination with the soda, had left that alkali to combine with the carbonic acid, thus playing the part of a base or electro-positive element, leaving the soda in the state of bicarbonate, that salt being of course formed by the action of the carbonic acid added: the solution might thus be supposed to contain a mixture of the carbonates of soda and albumen with an excess of carbonic acid. To this explanation it might be objected, that as alkaline bicarbonates are known to dissolve albumen, the acid gas had only converted the alkali into a bicarbonate, which thus held the albumen in solution: if this were true, how can the precipitation of albumen on the first passage of the gas be explained, unless it be supposed that the neutral alkaline carbonate which is first formed,

is incapable of holding in solution so large a quantity of albumen as the free alkali, or its bicarbonate; an assumption directly opposed to fact, as I shall have occasion to show in another place: besides which, the action of reagents ought to differ, and instead of those only which precipitate acid solutions of albumen producing a turbidity, a troubling should be produced by those also which affect its alkaline solution, for surely the solution of an animal matter in a carbonated alkali approaches less to the nature of an acid than to that of an alkaline solution.

4. I next attempted to form a solution of albumen in carbonic acid, excluding the agency of alkali, which if successful would, I considered, at once demonstrate the real nature of the combination; but in this I experienced considerable difficulty, for when a current of carbonic acid gas was passed through an aqueous solution of albumen (1.), no distinct combination was obtained; and on attempting in a similar manner to dissolve albumen previously coagulated by the action of heat or acids, I failed to obtain satisfactory results, from the close state of aggregation in which the albumen was obtained appearing to present a considerable resistance to the solvent action of the acid. I at length succeeded by precipitating albumen from serum of blood by means of alcohol, well washing the precipitate until all traces of alcohol were removed, (the vessel in which the precipitation was performed being immersed in ice-cold water to prevent the action of the evolved heat on the albumen,) carefully avoiding any unnecessary exposure to the air, which, by drying it, might serve to lessen its solubility in the acid. A portion of this finely divided albumen was diffused through cold water, and submitted to the action of a current of carbonic acid gas; after a short time it *entirely dissolved*; but the solution was not perfectly limpid, nor did it become so by filtration. In preparing this solution care must be taken to add a sufficient quantity of water, otherwise a considerable quantity of albumen will be carried mechanically out of the fluid by each bubble of gas, and being deposited on the sides of the vessel, will dry rapidly, and on being returned to the fluid will be found to have lost much of its solubility in the acid; and it is very remarkable how large a quantity of albumen can (by this kind of inverted filtration) be carried beyond the influence of the gas. The finely divided albumen obtained by passing a *limited* quantity of carbonic acid into albuminate of soda (2.), after well washing, may be substituted for that precipitated by alcohol, although it must be observed that it is not quite so readily soluble as that obtained by the latter process, owing to its having undergone

some modification, probably in its state of aggregation, difficult to unravel. If the precipitated albumen is merely digested in an aqueous solution of carbonic acid in a closed flask for some hours, as much appears to be taken up as if a *current* of the gas was used; and hence I have been led to conclude that the solubility of the albumen is not so much owing to the formation of a definite soluble compound (carbonate?) as to its being merely dissolved in the quantity of acid gas which water is capable of holding in solution at ordinary atmospherical pressure and temperature.

5. The solution of albumen in carbonic acid behaves to reagents like a mere aqueous solution,—as dilute serum of blood, with, as far as I know, a single exception, and this is the action of very dilute ammonia, which produces a precipitate of albumen soluble in an excess of the alkali. Heat produces a deposit of albumen with a simultaneous evolution of carbonic acid gas; nitric acid, tincture of galls, acidulated ferrocyanate of potassa, and bichloride of mercury all produce copious precipitates. By exposure to the air it does not very readily become turbid, the carbonic acid being very slowly evolved: after the lapse of a week, however, the albumen is deposited in a white impalpable form. The presence of carbonic acid in these solutions of albumen appears to prevent its ready precipitation by nitric acid, several drops being required to produce a considerable troubling; and on this account I am accustomed to use the nitrohydrochloric acid as a preferable precipitant when I have the detection of albumen in an animal fluid in view, as the action of this acid does not appear to be so liable to be affected by carbonic acid.

6. Wishing to ascertain with greater accuracy whether the albumen precipitated from its solution in soda by carbonic acid, depended for its resolution upon the formation of a compound with that acid, or upon the solvent action of the bicarbonate necessarily formed, I availed myself of Dr. Stevens's adaptation of Prof. Graham's law of the diffusion of gases, by placing a glass vessel filled with the solution (3.) under a large receiver full of hydrogen gas inverted over water. In twelve hours the apparatus was examined, and the fluid subjected to experiment, previously quite limpid, was found to be very turbid from the deposition of its albumen; the carbonic acid having been abstracted by the hydrogen gas. A modification of this experiment was then made by placing over the fluid subjected to the hydrogen gas, a capsule filled with lime-water: the carbonic acid being abstracted as before, was absorbed by the lime-water, causing the precipitation of car-

bonate of lime, which occurring simultaneously with the precipitation of albumen appeared to bear so near a relation to cause and effect that there can, I think, no longer remain a doubt as to the solvent nature of carbonic acid with regard to albumen. These experiments prove moreover another interesting fact, viz. that however energetic a solvent for albumen the uncombined alkalies may be, their carbonates must be regarded as comparatively powerless, contrary to the generally received opinion; for the same quantity of soda must necessarily have existed in the fluid after, as before its being subjected to the action of the carbonic acid, and subsequently of the hydrogen, the only difference being that it had become converted into a carbonate, whereas before the experiment it was pure and uncombined, *quoad* carbonic acid.

7. I was next desirous to ascertain what was the degree of solvent action capable of being exerted on coagulated albumen by alkaline carbonates, and whether this solvent power depended upon any partial decomposition of the salt employed, the acid or base being set free; I therefore precipitated albumen as before from fresh serum by means of alcohol, well washed it with cold distilled water, and divided it into four portions which I placed in as many flasks, the first of which was filled up with a solution of bicarbonate of soda, the second with a solution of the carbonate of soda, the third with water impregnated with carbonic acid, and the fourth with recently boiled distilled water; they were allowed to digest for twelve hours, and then examined after filtration with the following reagents:

Solvent employed.	Ebullition.	Nitric Acid.	Acetic Acid.	Sol. of Alum.	Bichloride of Mercury.
1. Bicarb. soda.	Dense opacity.	Copious precipitate.	Precipitate sol. in excess of acid.	Copious precipitate.	Copious precipitate.
2. Carb. soda.	Opacity.	Do.	Do.	Do.	Do.
3. Sol. of carbonic acid in water.	Copious precipitate.	Do.	—	?	Do.
4. Recently boiled water.	—	—	—	—	—

The water in the fourth flask was used for the purpose of ascertaining whether the precipitated albumen contained any of the soluble form of that principle which might have been mechanically carried down with it, but that this was not the case was satisfactorily proved by the five reagents employed not in the least disturbing the limpidity of the filtered fluid. It will be observed that ebullition produced a copious troubling in the solution of bicarbonate of soda that had been digested on the albumen in the first flask, as if a portion of uncoagulated albumen had been present, which I have just shown was not the case. How then can it be accounted for? Might it not be suggested that the salt employed had been decomposed into a neutral carbonate and free acid, which latter dissolved a portion of albumen, the carbonate being also partially decomposed into free alkali, which by dissolving albumen formed an albuminate of soda, whilst the portion of carbonic acid deserted by its base united to another portion of albumen, and thus the solution might be supposed to consist of undecomposed carbonate of soda, albuminate of soda, and carbonate of albumen (if this expression may be *provisionally* admitted). With regard to the contents of the second flask, in which the neutral carbonate was used, this appears to have undergone an analogous decomposition, for like the contents of the first flask, we find them present the same phænomena with reagents as would be produced by a mixture of solutions of albumen in carbonic acid and albuminate of soda; but experiments are required to clear up the obscurity enveloping this point.

8. An interesting field for investigation thus appeared to present itself in the examination of the action of albumen on the alkaline carbonates, the investigation of which I commenced with great care, and obtained some highly interesting and unexpected results; but not having quite concluded my examination of this part of my subject in consequence of the multitudinous repetition of experiments required to obviate the various sources of fallacy peculiarly incident to investigations in organic chemistry, I am compelled to defer their publicity for some weeks, when I trust to be able to communicate some curious and important facts on this interesting subject.

44 Seymour-street, Euston-square,
July 6, 1836.

XXVI. *Remarks on the Formula for the Dispersion of Light.* *By the Rev.* Baden Powell, *M.A., F.R.S, Savilian Professor of Geometry, Oxford.*

[Continued from vol. viii. p. 309.]

IN a former portion of these papers I have given some account of the methods by which the formula of dispersion, or relation between the length of a wave and the velocity of its propagation, or the refractive index for a given ray and given medium, is made applicable in calculation. I have also illustrated the comparison made by the researches which Sir Wm. R. Hamilton has given me between that formula in its simpler, but consequently only approximate, form, and the exact development. In the present instance, in continuation of the same object of facilitating the study of this highly interesting portion of the science of light, my design is, besides some general remarks, to furnish certain constant elements which enter into all calculations by the exact method; together with one more instance of the comparison of this method with the approximate one from the same source.

The exact method, in fact, consists in this: The relation is expressed by the series of powers of the reciprocal of λ resulting from the division of the development of the sine by the arc, with certain indeterminate coefficients. In the approximate method these are supposed all constant and equal, and are expressed by a common factor. In the exact development this cannot be allowed; but an investigation is given by which they are, in fact, eliminated; and there results a method for obtaining the theoretical index which is equivalent to the enunciation of a law expressing the connexion of the index of any one ray for a given medium, with three others supposed assumed: thus successively each of the four remaining of the seven standard rays have their indices found: or, in another point of view, we may say, that (taking three terms of the series) there are three constants to be found from observations. These depend on the medium; when we eliminate them therefore it is in effect equivalent to assuming three of the indices given by observation.

The very name of an approximate method commonly conveys the idea of a shorter and simpler process. In the present instance, however, the case is quite otherwise. On looking at the analysis contained in my former papers the exact method might appear long and intricate, but in fact the process of calculation is much simpler than might at first sight be imagined, and shorter than in the approximate method. The whole consists in the determination of the two constants log.

a and log. b for each ray, and the differences of the indices of the assumed rays, which constitute the other factors in the equation (26.), (*Lond. & Edinb. Phil. Mag.* for March 1836). That equation in a general form for finding the index of any ray $\mu_{,}$ whose constants are $a_{,}$ and $b_{,}$; supposing the observed indices for B F and H assumed, is as follows:

$$\mu_{,} - \mu_{\mathrm{F}} = a_{,} (\mu_{\mathrm{F}} - \mu_{\mathrm{B}}) + b_{,}(\mu_{\mathrm{H}} - 2\,\mu_{\mathrm{F}} + \mu_{\mathrm{B}})$$

The following values of log. $a_{,}$ and log. $b_{,}$ have been given in the foregoing paper,

$$\log a_{\mathrm{D}} = \bar{1}{\cdot}80441$$

$$\log b_{\mathrm{D}} = \bar{1}{\cdot}06281$$

I have also received the following from the same source,

$$\log a_{\mathrm{G}} = \bar{1}{\cdot}74345$$

$$\log b_{\mathrm{G}} = \bar{1}{\cdot}63384$$

It is easy to determine values of the same constants for the other rays which remain to be found, viz. C and E. They are deduced by the formulas (analogous to those in the former paper, Eq. 15. 27. &c.),

$$t_{,} = \frac{\tau_{,}^{-2} - \tau_{\mathrm{B}}^{-2}}{\tau_{\mathrm{H}}^{-2} - \tau_{\mathrm{B}}^{-2}}$$

$$a_{,} = -\,[1 - 2\,t_{,}]$$

$$b_{,} = -\,t_{,}[1 - 2\,t_{,}].$$

By substituting the values of τ from Fraunhofer's observations, we readily obtain,

$$\log a_{\mathrm{C}} = \bar{1}{\cdot}95433$$

$$\log b_{\mathrm{C}} = \bar{2}{\cdot}65253$$

$$\log a_{\mathrm{E}} = \bar{1}{\cdot}49646$$

$$\log b_{\mathrm{E}} = \bar{1}{\cdot}03196$$

With these logarithms, and those of the other factors in the above equation for $\mu_{,}$ which are derived from the three indices $\mu_{\mathrm{B}}\ \mu_{\mathrm{F}}\ \mu_{\mathrm{H}}$ assumed from observation for the particular medium, we thus get immediately the values of $\mu_{,}$ for the other rays resulting from the exact formula of theory.

It would be easy to give examples of this method; but for the present I shall confine myself to stating the results obtained by it for the ray G in Fraunhofer's media; compared, as before, with the approximate results of my former computation, performed by methods which will now be wholly superseded.

Medium.		μ_G Observed by Fraunhofer.	μ_G Calculated by the exact formula.	μ_G Calculated by the approximate formula.
Flint-glass	13.	1·6603	1·66066	1·6609
Do.	23.	1·6588	1·65910	1·6582
Do.	30.	1·6554	1·65569	1·6551
Do.	3.	1·6308	1·63101	1·6313
Crown-glass	M.	1·5735	1·57368	1·5738
Do.	13.	1·5399	1·54001	1·5399
Do.	9.	1·5416	1·54182	1·5416
Oil of turpentine.		1·4882	1·48833	1·4886
Solution of potash.		1·4126	1·41272	1·4126
Water.		1·3413	1·34140	1·3413

The preceding method enables us to calculate the indices for C, D, E and G, supposing those for B, F, and H assumed. It may, however, be desirable, for rendering the comparison of any case with theory complete and satisfactory, to possess the means of calculating likewise the indices for B, F, or H, assuming three others, or generally any index. For this purpose we must have recourse to the more general form (21.). This will not, in fact, be found more complex or difficult in practice, notwithstanding the simplification obtained by introducing the particular relation subsisting between τ_F τ_B and τ_H on which the process above given depends. I shall therefore here add such a statement of the more general method as may be necessary for the object in view, and likewise furnish those constants which are independent of the particular medium, and necessary for the computation.

The formula (21.) may be expressed, for brevity, thus:

$$0 = (\mu_D - \mu_B)\, k - (\mu_F - \mu_B)\, l + (\mu_H - \mu_B)\, m,$$

and the coefficients $k\ l\ m$ which are independent of the medium are readily found from Fraunhofer's values of $\tau_B\ \tau_D\ \tau_F\ \tau_H$. From these values we have directly

$$\tau_B^{-2} = 15{\cdot}489 \qquad \tau_D^{-2} = 21{\cdot}135$$
$$\tau_F^{-2} = 31{\cdot}075 \qquad \tau_H^{-2} = 46.666,$$

and by means of these we obtain for the coefficients in (21.) the following values:

$$\log k = 3{\cdot}87943$$
$$\log l = 3{\cdot}65263$$
$$\log m = 3{\cdot}94185.$$

In any particular medium then, taking the logarithms of the differences of the three indices, we easily obtain the fourth by the above formula.

For the corresponding relation of the other rays we must take a formula analogous to (21.), which will be as follows:

$$0 = \begin{cases} (\mu_C - \mu_B)\,(\tau_G^{-2} - \tau_B^{-2})\,(\tau_E^{-2} - \tau_B^{-2})\,(\tau_G^{-2} - \tau_E^{-2}) \\ -(\mu_E - \mu_B)\,(\tau_G^{-2} - \tau_B^{-2})\,(\tau_C^{-2} - \tau_B^{-2})\,(\tau_G^{-2} - \tau_C^{-2}) \\ +(\mu_G - \mu_B)\,(\tau_E^{-2} - \tau_B^{-2})\,(\tau_C^{-2} - \tau_B^{-2})\,(\tau_E^{-2} - \tau_C^{-2}) \end{cases}$$

which for brevity may be written as before,

$$0 = (\mu_C - \mu_B)\,k' - (\mu_E - \mu_B)\,l' + (\mu_G - \mu_B)\,m'.$$

The coefficients $k'\,l'\,m'$ may be found exactly as before from Fraunhofer's values of $\tau_B\ \tau_C\ \tau_E\ \tau_G$ which give

$$\tau_C^{-2} = 17{\cdot}045, \quad \tau_E^{-2} = 26{\cdot}437, \quad \tau_G^{-2} = 39{\cdot}705.$$

By means of these we obtain

$$\log k' = 3{\cdot}54623$$
$$\log l' = 2{\cdot}93137$$
$$\log m' = 2{\cdot}28794.$$

I will only add at present that I am now engaged in determining by observation the indices for various media, especially those of a highly dispersive nature: and in the few attempts as yet made to verify the theory in these cases, (in which it is manifestly put to a more severe test than in any of the cases hitherto given,) I have found the method just explained by far the most preferable. The approximate method in any form appears to me at once more troublesome and less satisfactory. The values of the constants applicable *to all media* above given, may be useful to those who may engage in such calculations, or in verifying those already performed by other methods.

Oxford, June 19th, 1836.

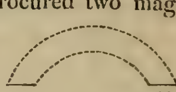
XXVII. *On certain Improvements in the Construction of Magneto-electrical Machines, and on the Use of Caoutchouc for Insulation in Voltaic Batteries.* By Fred. W. Mullins, *Esq., M.P., F.S.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I THINK it important to call the attention of the scientific readers of your valuable Journal, to some improvements recently made by me in the construction of the magneto-electric machine, which go far to demonstrate the still very imperfect state of these instruments, and form a foundation for alterations infinitely more important both in their mode of construction and application.

The machine whose power I had an opportunity of testing was constructed on the most approved principle, and consists of two sets of bar-magnets arranged vertically, each set consisting of a dozen bars, and the upper poles of one set being unconnected with those of the other. I had previously seen and examined horizontal horse-shoe machines, and so far as I was enabled to institute a comparison considered the other mode of construction to be preferable. After trial, however, it struck me that the power of all magneto-electric machines was very imperfectly developed, and that it might be possible to obtain considerably greater effects from the same number of magnetic bars by establishing a *magnetic* connexion between the poles of the latter, and this without much difficulty or increased expense. With this view I procured two magnetized arcs of the shape given in the annexed figure, and of the same width and thickness as the bars of the machine. I then applied them, one to the opposite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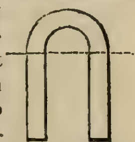
poles of the *outside* pair of bars, and one to those of the *inside*, and on giving the shock to a gentleman who was present, and who had tried the power of the instrument when the poles were *unconnected*, the effect was so much increased that he refused to repeat it, and on trying it on myself I found the power to be fully double what it had previously been. I was aware that connecting pieces of soft iron were sometimes used, but that their utility was said to be very questionable, and having myself tried them, I can safely say that soft iron as a mode of connexion is useless; it is evident, therefore, that the increase of power does *not* depend upon connexion, *unless* when *the substance forming the connexion is in a peculiar state, and thereby capable of exerting a certain influence on the* *poles of each set of magnets*, which influence, it can be shown, does *not* depend upon the *size* of the connecting magnets, for I have tried large horse-shoe magnetic bars, single and in sets, without any increase of power *beyond that* obtained from the small magnetic arcs represented in the figure.

Induction is certainly a cause, but not the *sole cause* of the increased power; there are other causes, as yet unexplained, which I trust may appear satisfactory to those who may peruse a paper which I am now preparing on this highly interesting subject: suffice it here to say, that in the future construction of the instruments in question, magnetic arcs in connexion with *vertical* bar-magnets should decidedly be used in preference to any other form or mode of construction at present known; and I would strongly advise any person who happens to have a machine of the horse-shoe form to cut off the bend as indicated in the annexed figure and reapply the same or other pieces of the same size magnetized, for by so doing it will be found that a vast increase of power will be obtained. I have thrown out these hints in the hope that they may lead to still greater improvements in the mode of developing the powers of combined magnets. In concluding this subject it may be well to observe that with my improved magnetic machine I have charged a Leyden jar, and obtained by the same means various other results similar to those obtained from the action of the common electrical machine.



In conclusion I would add, that in the various experiments I have made in regard to the best modes of developing and sustaining voltaic electricity, I have found that caoutchouc, or Indian-rubber, may be used with great advantage for insulation. I have applied it in place of glass in my intensity-sustaining battery; and as it can be made to adhere to the copper and may be laid on as thin as common letter-paper, a combination of plates or cylinders may be brought so close together as to occupy only a third of the space filled by a similar combination in the batteries at present used. In my intensity-battery, from the advantages derived from bringing the metallic cylinders as close as possible, this mode of insulation is most convenient and satisfactory.

I am, Gentlemen, yours, &c.

House of Commons, July 1, 1836. Fred. W. Mullins.

XXVIII. *Letter from* Mr. Faraday *to* Mr. Brayley *on some former Researches relative to the peculiar Voltaic Condition of Iron reobserved by Professor* Schoenbein, *supplementary to a Letter to* Mr. Phillips, *in the last Number.*

My dear Sir, Royal Institution, July 8, 1836.

I AM greatly your debtor for having pointed out to me Sir John F. W. Herschel's paper on the action of nitric acid on iron in the *Annales de Chimie et de Physique*; I read it at the time of its publication, but it had totally escaped my memory, which is indeed a very bad one now. It renders one half of my letter (supplementary to Professor Schoenbein's) in the last Number of the Philosophical Magazine, p. 57, superfluous; and I regret only that it did not happen to be recalled to my attention in time for me to rearrange my remarks, or at all events to add to them an account of Sir John Herschel's results. However, I hope the Editors of the Phil. Mag. will allow my present letter a place in the next Number; and entertaining that hope I shall include in it a few references to former results bearing upon the extraordinary character of iron to which M. Schoenbein has revived the attention of men of science.

"Bergman relates that upon adding iron to a solution of silver in the nitrous acid no precipitation ensued*."

Keir, who examined this action in the year 1790†, made many excellent experiments upon it. He observed that the iron acquired a *peculiar or altered* state in the solution of silver; that this state was only superficial; that when so altered it was inactive in nitric acid; and that when ordinary iron was put into strong nitric acid there was no action, but the metal assumed the *altered* state.

Westlar, whose results I know only from the *Annales des Mines* for 1832 ‡, observed that iron or steel which had been plunged into a solution of nitrate of silver lost the power of precipitating copper from its solutions; and he attributes the effect to the assumption of a negative electric state by the part immersed, the other part of the iron having assumed the positive state.

Braconnot in 1833§ observed, that filings or even plates of iron in strong nitric acid are not at all affected at common temperatures, and scarcely even at the boiling-point.

Sir John Herschel's observations are in reality the first which refer these phænomena to electric forces; but Westlar's,

* Phil. Trans. 1790, p. 374. † *Ibid.*, pp. 374, 379.

‡ *Annales des Mines*, 1832, vol. ii. p. 322; or *Mag. de Pharm.* 1830.

§ *Annales de Chimie et de Physique*, vol. lii. p. 288.

which do the same, were published before them. The results obtained by the former, extracted from a private journal dated August 1825, were first published in 1833*. He describes the action of nitric acid on iron; the altered state which the metal assumes; the superficial character of the change; the effect of the contact of other metals in bringing the iron back to its first state; the power of platina in assisting to bring on the altered or prepared state; and the habits of steel in nitric acid: he attributes the phænomena to a certain *permanent electric state of the surface of the metal.* I should recommend the republication of this paper in the Philosophical Magazine.

Professor Daniell, in his paper on Voltaic Combinations† (Feb. 1836), found that on associating iron with platina in a battery charged with nitro-sulphuric acid the iron would not act as the generating metal, and that when it was afterwards associated with zinc it acted more powerfully than platina itself. He considers the effect as explicable upon the idea of a force of heterogeneous attraction existing between bodies, and is inclined to believe that association with the platina cleanses the surface of the iron, or possibly causes a difference in the mechanical structure developed in this particular position.

In my letter, therefore, as published in the Philosophical Magazine for the present month (July), what relates to the preserving power of platina on iron ought to be struck out, as having been anticipated by Sir John Herschel, and also much of what relates to the action of silver and iron, as having been formerly recorded by Keir. The facts relating to gold and carbon in association with iron; the experimental results as to the electric currents produced; the argument respecting the chemical source of electricity in the voltaic pile; and my opinion of the cause of the phænomena as due to a relation of the superficial particles of the iron to oxygen, are what remain in the character of contributions to our knowledge of this very beautiful and important case of voltaic condition presented to us by the metal iron.

I am, my dear Sir, yours very truly,

E. W. Brayley, Esq. M. Faraday.
London Institution.

* *Annales de Chimie et de Physique*, 1833, vol. liv. p. 87.
† Phil. Trans. 1836, p. 114.

XXIX. *On the Carboniferous Series of the United States of North America.* By Thomas Weaver, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.**

HAVING in the year 1834 passed through the United States of North America from the Gulf of Mexico to New York, the geology of those vast regions could not fail to arrest my attention. Up to the period of my visit, the geological relations of those States had not received much of my notice beyond what had been published at an earlier day by Mr. Maclure and some others. My mind was therefore the more open to unbiassed impressions. But on my return to England I took great interest in comparing those impressions in particular with the great body of valuable information that has been contributed by different writers concerning the coal-bearing rocks of the United States, especially in Prof. Silliman's American Journal of Science, the Transactions of the Geological Society of Pennsylvania, the separate publications of our countrymen Messrs. G.W. Featherstonhaugh and R. C. Taylor, and the works of Professor Eaton. The general result, as derived from this comparison and combination, I have embodied in a condensed form in a note appended to my Memoir on the South of Ireland, there drawing a parallel between the relations of the carboniferous series in the British Isles and the United States; but as some months may yet elapse before the publication of that Memoir in the Geological Transactions be effected, it has been suggested that it would be useful to make an earlier and somewhat more extended communication on the subject through the medium of a scientific journal of extensive circulation.†

* Communicated by the Author.

† In collateral evidence of the correctness of my views, the following papers on the carboniferous rocks of the United States are particularly deserving of the reader's attention :

In the American Journal of Science.

Vol. 4. Mr. Z. Cist on the range of the anthracite formation in Pennsylvania. (1822.)

6. Mr. James Pierce on the carboniferous rocks of the Catskill mountain chain. (1823.)

12. The same author on the anthracite, bituminous coal, salt and iron of Pennsylvania. (1827.)

18. Professor Silliman on the anthracite region of Lackawanna, and Wyoming on the Susquehanna. (1830.)
Mr. David Thomas, Geological Facts.

19. Professor Silliman on the Mauch Chunk and other anthracite regions of Pennsylvania. (1831.)
Professor Eaton on the coal formations of New York and Pennsylvania.

19. Mr. David Thomas, remarks on Professor Eaton's observations on the coal formation in the State of New York.

My route lay up the Mississippi and Ohio rivers to Pittsburg, and thence overland to Lake Erie, to Buffalo, Niagara, and partly by land and partly by water through the state of New York to the city of that name. In this course, from the first visible rocks of carboniferous limestone and its occasional associates of shale and sandstone adjacent to the Mississippi and Ohio rivers, to the continuous coal measures which come in force near the influx of the great Kanawha into the latter river, extending thence westward into the state of Ohio, and northward through the Virginia, Pennsylvania and New York States to Lake Erie, I was very forcibly struck by the nearly

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Vol. 23. Professor Eaton on the coal beds of Pennsylvania, as being equivalent to the great secondary coal measures of Europe. (1833.) He deprecates the application of the term *transition* either to the carboniferous rocks of the Alleghany or Catskill mountains; and refers to his Geological Text-book, p. 91, second edition (1832); in which see hereon in particular pp. 66, 67, 79, 90, 110, 121, 124, 125, as affording further evidence on the question.

N.B. The relations of the carboniferous series have been much obscured by the peculiar terms employed by this author; the signification of which, however, has been explained by Mr. G. W. Featherstonhaugh in a manner that quite accords with the construction I had myself put on what came under my own observation during the course of my travels. See vol. i. p. 92 of Proceedings of the Geological Society of London (1828), and Phil. Mag. and Annals, vol. v. pp. 138, 139. (1829.); [also vol. vi. p. 75, 76, and Lond. and Edinb. Phil. Mag. vol. vii. p. 515, note.—Edit.]

25. Ten days in Ohio by a naturalist. (1834.)

29. Dr. Hildreth on the bituminous coal deposits of the valley of the Ohio (1835.) This is a particularly valuable memoir for the details it affords.

Mr. G. W. Featherstonhaugh. *Geological Report made to both Houses of Congress.* (1835.)

Mr. R. C. Taylor, *on the north-eastern extremity of the Alleghany mountain range in Pennsylvania*, in Loudon's Magazine of Natural History. (Oct. 1835.)

In the *first volume of the Transactions of the Geological Society of Pennsylvania.* (1835.)

Mr. R. C. Taylor on the relative position of the transition and secondary coal formations in Pennsylvania.

The same author on the coal basin of Blossburg on the Tioga river.

Professor Proost, four papers on the organic remains of the carboniferous limestone of Tenessee, &c.

Mr. E. Miller, Geological description and section of a portion of the Alleghany mountain west of Hollydaysburg.

Dr. Harlan on the fossil vegetable remains in the bituminous coal measures of the Alleghany mountains referred to in Mr. E. Miller's paper.

Mr. T. A. Conrad on some fossil shells presented by Mr. E. Miller from the Alleghany mountains.

Mr. R. C. Taylor, Memoir of a section passing through the bituminous coal field near Richmond, in Virginia.

perfect horizontality of the strata throughout, the prevailing deviation from that position being a slight inclination to the southward, subject however to gentle undulations upon a large scale.

Valuable beds of bituminous coal occur low down in the carboniferous limestone as well as in the higher accumulation of the common coal-bearing measures. Coal is thus obtained in the former adjacent to the Mississippi, near Memphis in Tenessee, and bordering on the confluence of the Missouri with that river, and in Illinois; also near the Ohio river in the vicinage of Owenborough in Kentucky, and in Indiana opposite to Hawesville.

In its western extent into Arkansas the carboniferous limestone appears to come in contact partly with a greywacké tract and partly with the old red sandstone.

From the vicinage of the Great Kanawha river the continuous coal-bearing measures extend to the south-west through Western Virginia and the eastern parts of Kentucky and Tenessee into the northern parts of Alabama, and to the north-east, as above stated, through Pennsylvania into the State of New York.

In the conterminous regions of Ohio, Pennsylvania, and Virginia, the continuous coal-bearing measures include not unfrequently intercalated beds of limestone, which often contain marine animal remains. Some of the coal measures are said also to exhibit in places fossil freshwater shells; and in this general series, beds of sandstone conglomerate are not of uncommon occurrence.

In the State of New York the carboniferous limestone, which underlies these coal measures, reposes partly on old red sandstone, partly on transition rocks. At the northern extremity the old red sandstone which lines the south coast of Lake Ontario from the west of the Niagara to the east of the Oswego river, distinctly underlies the limestone shale and limestone from Queenstown, by the gorge, upward to within two hundred yards of the ferry below the Falls of Niagara, with a gentle dip throughout to the southward. The same relative position is observable west of Lockport, in proceeding east from Lewistown by the line of road that leads to Rochester, also in the Genesee river north of the latter town, and in the course of the Oswego river. To the east of the Oswego the old red sandstone reposes on transition rocks, and being deflected to the south-east, on approaching the Helderberg mountains it appears to be overlapped and concealed by the carboniferous limestone, the latter then coming in contact with the transition rocks on the east, which range from Canada to the southward:

while still more south in Pennsylvania, the coal measures appear to overlap the carboniferous limestone on the east, and to come also in direct contact with the transition rocks. In tracing the order of succession from the carboniferous limestone to the superincumbent coal measures from north to south, the same low angle of inclination to the south is observable from Niagara in the direction of Buffalo, and thence along part of the south coast of lake Erie. The same disposition is likewise to be remarked in passing south by the lakes of Cayuga, Seneca, &c., whose waters find a common vent on the north by the river Oswego. South of those lakes the land rises rapidly by an accumulation of coal measures, forming the northern aspect of the Alleghany mountains, and the southerly dip being still observable. This series contains also red sandstone and conglomerate and beds of limestone, and includes in the higher regions the abundant deposits of coal that occur in the northern confines of Pennsylvania, in the anthracitous coal field of Carbondale and Lackawanna and Wyoming on the Susquehanna, and the bituminous coal fields of Bradford, Tioga, Lycoming, and Clearfield.

In the association of beds of limestone with the coal-bearing measures, we perceive an analogy to like phænomena in parts of the coal tracts of the North of England and of Scotland.

In the immense extent of the carboniferous rocks of the United States, stretching on the one hand from the State of New York on the north-east to that of Alabama on the south-west, and again from that of New York on the east to that of Missouri on the west, the coal is generally bituminous; but great deposits of anthracitous coal occur in the north-eastern and eastern parts of Pennsylvania, in the field of Carbondale, Lackawanna and Wyoming, as above stated, and more south in portions of the regions traversed by the Lehigh and Schuylkill rivers. These anthracitous deposits have been referred by some geologists to the transition epoch; but from all that I have been able to learn of their general characters, position, fossil plants and other organic remains, I conceive them to belong to the great carboniferous order. Mr. Z. Cist of Wilkesbarre has shown that there is a continuity in the anthracitous coal formation, extending from a district lying north of Harrisburg through the regions bordering on the Schuylkill and Lehigh rivers to the valley of the Wyoming on the Susquehanna, and thence up the valley of Lackawanna in the direction of Carbondale. From hence to the bituminous coal fields in Bradford and Tioga counties, situated more west, is a distance of only a few miles, and Professor Eaton conceives these formations to be connected, and to pass into each other as portions of the same geological deposits; a view in which he is supported by these anthracitous and bituminous coal tracts containing fossil plants, similar to those occurring in the bituminous coal field near Zanesville in Ohio, and similar to such as are found also in the great coal fields of Europe, at Newcastle-on-Tyne, at Saarbrück, &c.: and the opinions expressed on this subject by Professor Silliman and Dr. Harlan appear to lean the same way. In further corroboration of this view it may be added, that both the anthracitous and bituminous coal regions in Pennsylvania are productive of large quantities of clay iron stone, the usual concomitant of the coal fields of the great carboniferous order.

On the other hand, Mr. R. C. Taylor has taken a different view, and refers the anthracitous coal of Pennsylvania to the transition æra; but I confess the evidence which he has adduced does not appear to me to disprove the connexion in the north-eastern part of Pennsylvania between the bituminous coal fields of Lycoming, Tioga, and Bradford, with the anthracitous coal fields of Carbondale, Lackawanna and Wyoming. More south, from the two sections which the author has given as roughly traced for the purpose of illustration, the one being drawn from Lycoming county and the other from Clearfield county, but both from the Alleghany mountain range on the north-west to the valley of the Susquehanna on the south-east, it would appear that a wide interval occurs between the nearly horizontal bituminous coal range of the Alleghany mountain on the west and the anthracitous coal range of Schuylkill, &c. on the east; an interval occupied by a series of rocks which, within the transverse distances of 76 and 66 miles respectively, exhibit two anticlinal lines, exposed, it would appear, by abruption, and which are referred by the author to the transition system.

In the Lycoming section (No. 2 of the author) the beds and distances are given in the following order: Miles

(*a.*) From the Alleghany mountain, composed of the coal formation supported by old red sandstone dipping west, and underlaid by shales, limestone, sandstone, to limestone in the Muncey valley (there in anticlinal order) 9

Succeeded by

(*b.*) Greywacké slate, limestone, greywacké shales, with two beds of limestone, greywacké, shales, to sandstone in the valley of Penn's Creek, (there in synclinal order) 18

38

	Miles.
Brought forward...	38
Succeeded by	
(*c*.) Shales, limestone, shales, limestone, to sandstone with conglomerate and a thin bed of *bituminous* caking coal (the accompanying shale of which contains splendid fossil plants) in St. Patrick's hill, (there in anticlinal order, adjoining the Susquehanna valley)	18
Succeeded by	
(*d*.) Red shale across the Susquehanna valley to Blue mountain	20
	76

In the Clearfield section (No. 3 of the author):

(*a*.) From the Alleghany mountain, composed as in the Lycoming section and dipping west, underlaid by greywacké shales, limestone, sandstone, to limestone in the Nuttany valley (there in anticlinal order) ...	10
Succeeded by	
(*b*.) Sandstone with conglomerate, limestone, sandstone and shales, limestone, conglomerate and sandstone, shales, limestone, to conglomerate and sandstone in Juniata valley (there in synclinal order)	25
Succeeded by	
(*c*.) Limestone and shales, grit, limestone to sandstone with conglomerate and a thin bed of *bituminous* caking coal, adjoining Susquehanna valley, (and there in anticlinal order)	15
Succeeded by	
(*d*.) Red shales, sandstone, and conglomerate across the Susquehanna valley to Blue mountain	16
	66

In a third section (No. 1 of the author), still more south, namely, in the county of Bedford, and also drawn originally from the Alleghany bituminous range on the north-west in a south-east direction, but in the portion represented, commencing at Tussey mountain, passing thence over a country with an undulated surface to Allegripus mountain, the Raystown branch of the Juniata, Hopewell ridge, and still further east the dip is throughout, from Tussey mountain to the south-east, at angles varying from 30° to 80°. From Tussey mountain, composed of red sandstone and conglomerate, dipping east, to

Hopewell ridge, occurs the following series of beds: limestone and limestone shale, variously coloured shales and argillaceous sandstones; sandstone and conglomerate in Allegripus ridge, including a bed of bituminous shale with impressions of ferns; red rock and shale in the Raystown branch of the Juniata; conglomerate, various sandstones, shales, and argillaceous beds containing coal beds in the Hopewell ridge and continued still further east. The coal beds thus first appearing in Hopewell ridge, (of which the most western is 20 miles east of the Alleghany mountain,) appear to be of a highly bituminous quality, caking in the fire and forming excellent coke, but of high specific gravity, 1·700, and denominated by the author transition bituminous anthracite. It is to be observed also, that the shale which alternates with the coal measures here is rich in clay ironstone. On this section I shall merely take the liberty to remark, that were we to conceive disruption and denudation to have taken place between the Alleghany bituminous mountain range on the west and the Hopewell ridge on the east, with an original curvature of the beds from west to east; or even to suppose these eastern and western coal tracts to have been originally separate deposits, there appears no very conclusive reason, judging by the evidence given, why the coal of the Hopewell ridge might not be referred to the carboniferous series, as indeed it had been previously in the judgement of the committee of the Senate of Pennsylvania.

In these sections the author represents the eastern base of the Alleghany mountain range as consisting of old red sandstone, with subordinate beds of limestone supporting the coal measures; and the same language is employed when speaking of the north-eastern extremity of the Alleghany mountain range in Pennsylvania; and again when describing the coal basin of Blossburg on the Tioga river; on which it is remarked, "a large portion of these red sandstones and the lower red argillaceous sandstones and shales are crowded with Productæ and crinoidal remains; and occasionally Fucoides and Caryophylliæ, Pectens and Spiriferæ are interspersed." The application of the term old red sandstone does not seem quite correct in these cases, in as much as these beds appear to be an alternating series lying above the great body of the carboniferous limestone that becomes apparent in the northern parts of the State of New York, and which itself reposes there conformably on the extensive formation of old red sandstone subjacent to it, while it supports in a similar manner the alternating beds in question, and these support the more productive coal-bearing measures.

In this distribution of the coal in Pennsylvania, anthracitous on the east and bituminous on the west, we may observe an analogy to the coal deposits in South Wales, where the eastern portions are bituminous and the western anthracitous; and again with those in Ireland, where the northern deposits are bituminous and the southern anthracitous. But both the Welsh and the Irish coal fields belong to the great carboniferous order, and I anticipate that such also will be admitted to be the case with respect to the Pennsylvanian, which point indeed, in reference to the north-eastern parts of that state, appears to me fully established by Professor Eaton. The mere quality of coal, as being anthracitous, cannot alone be considered a decisive criterion of its antiquity, though so conceived by some geologists.

In the extensive tracts of Pennsylvania alone, it must necessarily take time fully to develop all its mineral relations, particularly in districts that may prove of an intricate nature. From the labours of Mr. R. C. Taylor, specially devoted to these researches, we may expect very valuable information, not only in respect of the rocks of the great carboniferous order, but of those of the transition system: and it will be a subject of high interest to have it shown to what extent the rocks of the latter epoch in the United States may correspond with the Silurian or Cambrian divisions of the series so ably developed by the assiduous labours of Mr. Murchison and Professor Sedgwick in England and Wales, or with the transition rocks of Somerset and Devon under the investigation of Mr. De la Beche, or with those of Ireland, which I have endeavoured to describe. In such a comparison it is probable that more than one parallel may be drawn, after a careful examination of the transition tracts of the United States.

As it does not enter into my present view to consider the slight layers of anthracite that are met with in transition clay slate on the river Hudson, or the thicker beds ascribed to that æra that occur in New England, I would refer for detailed information on this subject in particular to the very valuable work of Professor Hitchcock on the Geology of the State of Massachusetts.

I shall conclude with the general remark that if the land of the United States be rich in the vegetable productions of the warm and temperate zones of the earth, it is no less so in the abundance and variety of its mineral stores, of which the principal are its inexhaustible deposits of coal and iron, which may be considered as the mainsprings of arts and manufactures. In the possession of such treasures, combined with the active spirit engendered by free institutions, and the natural and gradually improving facilities of internal and external communication, our American brethren may look forward to the attainment of a state of greatness and prosperity that may not readily lie within the compass of human calculation.

XXX. *On Electro-pulsations and Electro-momentum.* By WILLIAM STURGEON, *Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, Addiscombe, &c.**

IT is very well known to the readers of the Philosophical Magazine, that I have long considered electric currents, when transmitted through inferior conductors between the poles of a voltaic battery, as the effect of a series of distinct discharges, in such rapid succession as not to be individually distinguished by the senses. Such currents I have called electro-pulsatory. See my theory of magnetic electricity in the London and Edinburgh Philosophical Magazine, vol. ii. p. 202.

By following up these views of electro-pulsations, I was about two years ago enabled to dispense with all acid or saline liquids, in the employment of galvanic batteries, for the purpose of galvanizing, as it is called, either to satisfy the curiosity or as a medical process; and my plan, which answers very well, I have found to be productive of a considerable saving in the expense necessarily attendant on the use of voltaic batteries when excited by acid solutions.

It is well known that a Cruickshank battery of about a hundred pairs will, by employing water alone in the cells, charge to a certain degree of intensity almost any extent of coated surface of glass that we please; and that the same degree of charge is given to it by a single contact of the conductors, however short its duration. This being understood, and understanding also that the shock produced by any discharge from a given intensity would be proportional to the quantity of fluid transmitted in a given time, it was easy to foresee that a series of shocks in rapid succession might be produced by some mechanical contrivance, and that the degree of force might be regulated by varying the extent of coated surface.

My first experiments were made with a hundred and fifty pairs of three-inch plates, and about seven feet on each side of coated glass; and my apparatus for producing a rapid succession of shocks was one of Mr. Barlow's stellated electro-

* Communicated by the Author.

magnetic wheels* which was soldered to an iron spindle and put into rotatory motion by a wheel and band; the points of the wheel touching in succession a copper spring in connexion with the positive surface, and thus producing a discharge at every contact of the wheel and copper spring.

When the two surfaces are connected by wires with two basins of salt water, and the hands immersed one in each basin, the effect experienced is precisely that of the discharge of a voltaic battery. The discharges can be made in such rapid succession as to prevent the sensation of distinct shocks; and if the process were to be concealed it would require some experience to distinguish between the effects on the animal œconomy from this apparatus and those from a voltaic battery charged with acid and water.

My views being so far verified, the next attempt was to simplify the apparatus and make it more portable; and as it was readily seen that if one hundred pairs would charge glass of considerable thickness, thinner glass might be charged by fewer pairs; this was done; and eventually the glass entirely dismissed, and its place supplied with well-varnished Bristol-board. These boards answer exceedingly well as a reservoir for low intensities; they may be coated to within an inch of the edge all round, and placed upon their edges either on a piece of glass or on a board properly prepared, and arranged to any required extent like the plates of a voltaic battery, but when considerable intensity is wanted, it is better to use thin glass.

From these facts we learn that metallic surfaces of many acres of extent may possibly be charged to a low intensity in the interior of the earth, by having a thin intervening stratum of inferior conducting matter sufficient to insulate from each other their dissimilar electric surfaces.

It may now be understood that the slightest accident which would suddenly break through the insulation, such as the sinking of a mass of metalline matter from one stratum to the other, would cause a sudden rush of an immense ocean of the electric fluid, which might be productive of subterranean lightenings and tremendous explosions sufficient to shake an extensive range of country on every side.

Connected with the preceding facts there are others which may be conveniently mentioned in this place, and which would lead us to similar explanations of the causes of subterraneous convulsions. Electric currents of considerable magnitude when suddenly checked, or diverted to a new channel, produce a

* [See Phil. Mag., First Series, vol. lix. p. 241.—Edit.]

momentum not very generally understood; but which I will endeavour to explain. A coil of copper wire excited by magnetic action will become a channel for an electric current; and whilst the whole circuit is metallic, the velocity of that current would be considerably greater than if any, even a small part of the circuit were of worse conducting materials: and if the current were suddenly transferred from a channel of the former character to one of the latter, by any contrivance whatever, it would meet a resistance on entering the new channel, which the momentum it had previously required would have to overcome; and a sudden disturbance of the electric fluid, previously at rest, would take place, and a violent rush of the current would as suddenly follow.

It is in this manner that shocks and sparks are produced by magnetic electric machines, where the current, previously in rapid motion, is suddenly transferred to a new channel of inferior conducting character; and all the fluid in the revolving coil rushes through a person properly situated for the new route, and who experiences the electric shock, or else through a thin stratum of air at an interruption in the metallic circuit where the spark is produced.

These, then, are some of the effects of electric currents, or of the momentum of the electric fluid in a state of motion, after the exciting cause is entirely cut off. The shock thus produced may very conveniently be compared to the blow given by Montgolfier's hydraulic ram. Electro-momenta may be produced by any mode of excitation whatever, and the effects will be proportional to the velocity and quantity of the electric fluid first put into motion; and the length of the original channel is also to be taken into account. If then electro-momenta, capable of producing violent shocks and vivid sparks, can be produced by a few hundreds of feet of thin copper wire, what is it that might not be expected from the electro-momenta of nature, arising from currents of many miles in extent, kept in motion either by heat, saline solutions, or by other causes, amongst the metalline strata below the surface of the earth? A sudden disruption in the circuit would insure the blow, and an earthquake might be the result.

Artillery Place, Woolwich, July 4, 1836.

XXXI. *Reviews, and Notices respecting New Books.*

A Practical Treatise on Locomotive Engines upon Railways; with Practical Tables, giving at once the Results of the Formulæ, founded upon a great many new experiments, &c. By the Chev. F. M. G. De Pambour.

IT is only within the last few years that the attention of engineers has been particularly directed to the mechanical capabilities of locomotive engines; and their inquiries have, for the most part, been limited to the vague practical information that is commonly inferred from actual experiments. The vast and splendid projects that now occupy such an important position in the public mind, and that promise such extensive and permanent advantages to society, have created a stronger and more lively interest in the science of railways. An engineer is expected to be, at least, practically acquainted with the theory of locomotive engines; he is supposed to possess that intimacy with the laws of their physical and mechanical action, as to be prepared to estimate pretty nearly, on scientific principles, the speed with which any proposed engine will draw a given load. How far this has really been the case is a question on which it will not here be necessary to offer an opinion; but we may state, that in the first stages of this, as well as of almost every useful branch of science, the practitioner is obliged at first to glean his information from experience alone. The construction of each successive engine suggests new and valuable information as to defects that may in future be avoided and improvements that may be adopted. The working of each engine also furnishes the means of roughly estimating what may be done by any other of a similar construction. Afterwards, however, when the subject becomes to be scientifically discussed, methods are presented by which the capabilities of any intended engine may be previously submitted to an accurate calculation, whatever may be the plan of its construction. We do not think that this object has yet been fully accomplished. The author of the present work, however, treats the subject in a manner that shows him to be well acquainted with the mechanical theory. He first gives a very intelligible description of the locomotive engines employed on the Liverpool and Manchester railway, as well as an account of their dimensions and proportions. He founds the calculations throughout the volume, on a great number of experiments that he has made himself with these engines. In the second chapter he gives rather a tedious discussion relating to the calculations of the true pressure of the steam from the indications of the lever and valve of the boiler: this might be abridged with advantage. The fourth and fifth chapters, which treat on the resistance along the railway and the proportions and effects, form the principal feature of the volume, and they contain a remarkably clear and comprehensive discussion of these most important points. The author very properly considers the friction of the engine separately from that of the load, and shows that in consequence of the additional strain on the machinery, it increases as the load increases. He also shows, that the rate of travelling with a given load does not depend solely on the tractive power of the engine, but that another important element enters into the calculation, viz. the evaporating power of the boiler. An appendix contains a detailed account of expenses, profits, and other valuable particulars of a mercantile and speculative nature, drawn from the documents of the Liverpool and Darlington railways. Altogether, the work is written in a clear and unaffected style; the subjects throughout are treated very philosophically, and with great ability; the typographical execution is also exceedingly creditable, and, judging from the gratification we have experienced on its perusal, we can have no hesitation in pronouncing it an elegant and truly valuable publication, that should be possessed by all persons interested in such pursuits.

XXXII. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

Dec. 22, 1836.—SPECIMENS were exhibited of numerous *Shells* of the genus *Mitra*, Lam., and of one species of *Conœlix*, Swains., forming part of the collection of Mr. Cuming; and an account of them by Mr. Broderip was read, commencing as follows:

"The species of the genus *Mitra*, Lam., which I am about to describe had been sent by Mr. Cuming, in whose cabinet they are, to Mr. Swainson, whose intimate acquaintance with this family renders him so particularly competent to the task of describing them. They were named by him, and he also made notes respecting them before returning them. In the following account of them I have retained Mr. Swainson's name in every instance but one: and whenever he has made any written observations I have quoted them.

Characters, habitats, &c. of the following species were then given, and are printed in the "Proceedings."

Genus MITRA (*Lam.* and *Swains.*). *Mitra nebulosa* (representing *nubila*, Type 5, 1, Sw.), *Swainsonii* (Type 1, 1.), *Ancillides* (5, (2 ?)), *maura* (representing *Tiara foraminata*, Type 1, 4.), *fulvescens* (5, 1.), *testacea* (5, 1. representing *fulva*), *fulva* var. (1, 2. representing *Tiara*), *chrysostoma* (5, 1. representing *ferruginea*), *tristis* (2, 4.), and *effusa* (1, 5.).

Genus TIARA, *Swains.* (MITRA, Lam.) *Tiara foraminata* (representing *Mitra maura*, Type 2, 4.), *muricata, mucronata, catenata* (1, 3.), *multicostata, rosea* (1, 2.), *millecostata* (the close-set longitudinal ribs and cancellated base give this shell, which may not have attained its full growth, the aspect of a *Cancellaria*), *lineata* (5, 1), *nivea* (5, 3.), *aurantia, terebralis, crenata* (5, 3. or 3, 3.), *rubra* (1, 2.), *semiplicata*, and *attenuata* (5, 1).

Mr. Swainson had written on the paper containing *Tiara terebralis*, "Type 4, 4. This is one of the most extraordinary shells in the collection, as it so closely resembles the *Mitra Terebralis* that, but for its possessing the generic characters of *Tiara*, it might pass for the same species."

It is one of the most slender of its genus, and has very much of the general character and form of a *Terebra*; and its resemblance to *Terebra* is increased by the circumstance of its having one spiral groove, more deeply impressed than the others, placed at about one third of the length of each volution before the suture. The points of contact of the decussating with the longitudinal grooves are deeply impressed.

There is a fine specimen in Mr. Broderip's collection.

Mr. Sowerby has furnished me with the account of this species.

Genus Conoelix (*Swains.*). *Conoelix Virgo* (representing *Conus Virgo*).

The following observations by Mr. Swainson elucidate his notes in relation to the *Mitres*, appended to most of the characters of the shells above named:—

"To render my explanation of the notes and references attached to the different species of the *Mitranæ* more intelligible to conchologists, it will be necessary for me to state, in as few words as possible, the result of my investigation of this subfamily, and the principles which have regulated these numerical indications.

"I have already, in another work, characterized the family *Volutidæ*, which appears to be that primary division of the *Carnivorous Gasteropoda* (*Zoophaga*, Lam.), which represents the *Rasorial* type among *Birds*, the *Ungulata* among *Quadrupeds*, and the *Thysanura* among perfect *Insects* (*Ptilota*): these analogies being of course remote, although founded on the structure of the animal, no less than on its testaceous covering. It thus follows that the Lamarckian *Mitræ*, instead of a genus, constitute a subfamily, which appears to be the subtypical group of the circle. The five genera composing this circle I have long ago characterized; and here, for some years, my analysis of the group terminated. The inspection, however, of the numerous species brought home by Mr. Cuming, and the gradually augmented number in my own cabinet, seemed to invite a still further and more minute investigation, for the purpose of ascertaining if any, and what, subgenera were contained in the more crowded groups of *Mitra* and *Tiara*. This investigation was carried on, at intervals, for nearly twelve months; and the result surpassed my most sanguine expectations. It has convinced me that not only does each of the genera of the *Mitranæ* represent analogically the corresponding groups of the *Volutinæ*, but that the same relations can be demonstrated between the minor divisions of the genera *Tiara* and those of *Mitra*: in other words, that these latter represent all the subfamilies and genera of the other *Volutidæ*, while they preserve their own peculiar or generic character. What I have just said on the parallel relations of analogy between the *Mitranæ* and the *Volutidæ*, is strictly applicable, in fact, to the genera *Mitra* and *Tiara*, the primary divisions of each of which can thus be deemonstrated subgenera. Nor is this all: the materials I have been for so many years collecting have enabled me to ascertain, in very many instances, that the variation of the species, in each of these subgenera, is regulated on precisely the same principle. Hence it follows that the two circles of *Mitra* and *Tiara*, like the two divisions of Mr. MacLeay's *Petalocera*, contain species representing each other, so that if their generic character is not attended to, it is almost impossible to discriminate them even as species. Many instances of this extraordinary analogy might be mentioned, independent of that here alluded to, between *Mitra Terebralis* and *Tiara Terebralis*.

"Selecting this shell to illustrate the numbers "Type 4, 4," I may observe, that 'Type 4' signifies that it belongs to the fourth subgenus of *Tiara*, in which group it is the fourth subtype, uniting to *Mitra maura*, which is the fourth subtype of the first or typical subgenus. *Mitra maura*, again, as representing this latter shell, consequently becomes the fourth subtype of the first or typical subgenus, and is therefore marked "Type 1, 4." The first figure always denotes the subgenus, and the last the station which the species appears to hold in its own subgenus.

"I am unacquainted with any group in the animal kingdom which demonstrates more fully than this does the law of representation. It may be mentioned, also, that nearly all the divisions I had long ago characterized, from the formation of the shells alone, have more recently been confirmed by a knowledge of their respective animals: a knowledge for which we are entirely indebted to the able naturalists who accompanied the French expedition on board the Astrolabe."—W. S.

Specimens were exhibited of several hitherto undescribed *Cowries*, most of which have been brought to England within the last few years. They were accompanied by characters and descriptions by J. S. Gaskoin, Esq., which are given in the "Proceedings" under the following names, viz.

Cypræa formosa (Cape of Good Hope), *rubinicolor*, *producta*, *candidula* (Mexico, *Cyp. approximans*, Beck, *Cyp. olorina* Duclos, but first described by Mr. Gaskoin), *acutidentata* (Isle of Muerte, Bay of Guayaquil), *Pediculus*, var. labiosa, *vesicularis* (Cape of Good Hope), and *Beckii*.

There was read an "Extrait du Quatrième Rapport Annuel sur les Travaux de la Société d'Histoire Naturelle de l'Ile Maurice : par M. Julien Desjardins."

The communications relative to the *Mammalia* read before the Natural History Society of the Mauritius in the fourth year of its existence have comprised an account by the secretary, M. Julien Desjardins, of a *Whale* which he regards as the *Physeter macrocephalus*, Linn., that was cast ashore on an adjoining reef: and some observations by the same author on several of the *Mammalia* of the island, and particularly on the hybernation of the *Tenrec, Centenes spinosus*, Ill.; the lethargy of which animal takes place when the thermometer is not lower than 20° Cent., and even when it marks 26°.

In ornithology M. Desjardins has also been the only contributor. He has described, as new, two *Birds* belonging to the island, and has proposed for them the names of *Charadrius Nesogallicus* and *Scolopax elegans*.

M. Liénard, the elder, has, in the course of the year, described many *Fishes*, including a new species of *Plectropoma*, allied to the *Plectr. melanoleuca*, Cuv. & Val., which is of a uniform brown colour, with all its fins of a still deeper brown, except the pectoral which are orange; on this latter character his specific name is founded: a *Holacanthus*, La Cép., from Batavia, remarkable on account of the numerous sinuous silvery lines which occupy principally the middle of the body; and having also on its face two yellow and two black bands, one of which is ocular: a *Cheilinus*, Cuv.: an *Echeneis*, Linn., furnished, on its suctorial disc, with twenty-five pairs of plates: and a *Muræna*, Thunb., the body of which is of an ebony black, and the dorsal fin yellow; the trivial name being indicative of the latter peculiarity. He has also given some account of a collection of *Fishes* obtained from the western coast of Madagascar, and comprising thirteen species, several of which he regards as new. M. Desjardins has described as the *blue-faced Tetrodon*, a species remarkable for two large blue spots on each side of its face, and having the fin-rays as follows; D. 15. A. 12. P. 14. C. 14.; it inhabits the seas adjacent to the Isle of France.

In entomology the only communication made to the Mauritius Society was by M. Goudot, and related to the *Insect* described by Mr. Bennett at the Meeting of the Zoological Society on January 22, 1833, (Proceedings, Part i., p. 12; Lond. and Edinb. Phil. Mag., vol. ii. p. 478,) under the name of *Aphrophora Goudoti*. The communication made to the Zoological Society, of which a full abstract is given at the page quoted, was apparently identical with that read before the Mauritius Society.

The remaining zoological communication related to the *Intestinal Worms*, and was made by the Secretary. It gave some account of the *Distoma hepaticum*, Cuv., as found in the stomach of a cow; and of the *Cysticercus Cellulosæ*, Brems., existing in innumerable quantities over almost the whole of the head, trunk, and extremities of a sow.

An "Extrait du Cinquième Rapport Annuel" of the same Society, by M. Julien Desjardins, Corr. Memb. Z. S., was also read.

In the year of which the present Report gives an account, M. Desjardins has communicated to the Natural History Society of the Mauritius, a list of several species of *Birds* that are occasional visitors of that island; and has also referred particularly to the *Coturnix Sinensis*, Cuv., and the *Nectarinia Borbonica*, Ill., as stationary in the Mauritius.

M. E. Liénard has brought from the Seychelles a species of *Gecko* of considerable size; which he has described in a communication made to the Society: and M. E. Liénard has placed on record the existence in the adjacent seas of the *Sphargis coriaceus*, Merr.

M. Liénard, the elder, has again made numerous contributions to ichthyology. He has given a detailed description of the *Squalus Vulpes*, Linn.: has described as new a *Trichiurus*, Linn., which he had formerly regarded as the *Trich. lepturus*, Ej., but which has the eye much larger, more numerous *striæ* on the *suboperculum*, and a few more rays in the dorsal fin: and has also described two species of *Crenilabrus*, Cuv., which he regards as new; one of them has three longitudinal rose-coloured bands on the white ground of the body, others on the dorsal fin, a large blood-red spot on the ventral fins, and D. 12+10. A. 3+11; the other is banded like the preceding, but is deeply rose-coloured on the back and pale yellow below, has a black circle surrounding the base of the pectoral fin, a large red spot above the *anus*, the dorsal and caudal fins red, the anal and ventrals yellow, the pectorals rose-coloured, and D. 12+9. A. 3+11. He has also given a description of a *Muræna*, Thunb., of a very pale olive yellow towards the front and brown towards the tail, and marked on the back by white ocellated spots bordered with brown.

In the same department M. E. Liénard has contributed descriptions, from recent specimens, of several *Serrani* described by Cuvier and M. Valenciennes in their 'Histoire Naturelle des Poissons'; and has also given a description of a *Blennius*, Linn., destitute of appendages on the head. These fishes were observed in a voyage to the Seychelle Islands, whence M. E. Liénard brought back with him to the Mauritius a *Chætodon* of very varied colours, which M. A. Liénard subsequently described under the name of *Chætodon diversicolor*. M. Desjardins has stated, in a note, that the *Mango fish*, *Polynemus longifilis*, Cuv. & Val., is not found, as had been announced, in the Isle of France. And he adds that he has prepared an alphabetical index to the nine volumes of the 'Histoire Naturelle des Poissons' that had then reached the Mauritius. M. Magon has presented to the Museum of the Society a fragment of a ship's coppered keel pierced by the point of the upper jaw of a *Histiophorus*, Cuv., which still remains infixed in it.

M. Desjardins has contributed the only notices relative to the *Mollusca*, which have consisted of short descriptions of three species belonging to the island: an *Octopus*, *Oct. arenarius*, Desj., found in the shell of a *Dolium*; a *Pupa*, of a red and yellow colour; and a small species of *Helicina*. He has also ascertained the existence at the Mauritius of the *Tornatella flammea*, Auct.

To the same active member the Mauritius Natural History Society is indebted for the only entomological communication made to it in the fifth year of its existence: it is a detailed description of a large species of *Iulus* brought from the Seychelles, and characterized as the *Iulus Seychellarum*, Desj.

Specimens were exhibited of various *Fishes*, forming part of a collection from Mauritius, presented to the Society by M. Julien Desjardins, and forwarded by him at the same time with the "Rapports de la Société d'Histoire Naturelle de l'Ile Maurice." These were severally brought under the notice of the Meeting by Mr. Bennett, who called particular attention to the following, which he regarded as hitherto undescribed, and of which the characters are given in the "Proceedings," viz.

Apogon *tæniopterus*; Acanthurus *Desjardinii, Ruppelii,* and *Blochii*; Labrus *spilonotus*; and Anampses *lineolatus.*

Jan. 12, 1836.—A note addressed to the Secretary by Sir Robert Heron, Bart. M.P., was read. It referred to the writer's success in the breeding of *Curassows* in the last summer at Stubton.

From two individuals in his possession, the male of which is entirely black, and the female of the mottled reddish brown colour which is regarded as characteristic of the *Crax rubra*, Linn., Sir R. Heron has hatched in the last year six young ones in three broods of two eggs each: the eggs were placed under turkeys and common hens. Respecting one of them no notes were made; but the other five were all of the red colour of the female parent. Two of these, which were at two or three weeks old very strong, being still in the flower-garden, were killed in the night by a rat that had eaten its way into the coop in which they were. Two others were sent to the Earl of Derby, who wanted hens. The remaining one is now nearly, if not quite, full grown; and Sir R. Heron proposes to place it with the old pair.

"There is one great peculiarity," Sir R. Heron remarks, "attending the old pair. Their principal food is Indian corn and greens, both which they eat in common: but whenever any biscuit is given to them, as an occasional treat when visitors are here, the male breaks it and takes it in his mouth; waiting, however long, until the hen takes it out of his bill; which she does without the slightest mark of civility, although on excellent terms with him. This proceeding is invariable."

Mr. Yarrell, on behalf of T. C. Heysham, Esq., of Carlisle, exhibited the egg, the young bird of a week old, one of a month old, and the adult female of the *Dottrell, Charadrius Morinellus*, Linn., obtained on Skiddaw in the summer of 1835. Several pairs were breeding in the same locality.

He also stated that a specimen of the *grey Snipe, Macroramphus griseus*, Leach, a young bird of the year, has been obtained near Carlisle in the past year. This is the third recorded instance of the occurrence of the species in England.

Some notes by Mr. Martin of a dissection of a *Vulpine Opossum, Phalangista Vulpina*, Cuv., were read, and are given in the "Proceedings."

A notice by Dr. Rüppell, For. Memb. Z. S., of the existence of canine teeth in an Abyssinian *Antelope, Antilope montana*, Rüpp., was read. It was accompanied by drawings of the structure described in it, which were exhibited.

The following is a translation of Dr. Rüppell's communication.

In several *Mammalia* of the order *Ruminantia* the adult males, and even some females, possess canine teeth, which are more or less developed; to these teeth no other use has been attributed than that of a weapon of defence. The *Camels* (*Camelus*), the *Musk Deer* (*Moschus*), and the *Muntjak* of India (*Cervus Muntjak*), possess these canine teeth in both sexes. In the *red Deer* (*Cervus Elaphus*) and in the *rein Deer* (*Cerv. Tarandus*), the adult males alone are provided with them.

I have just ascertained that there is a species of *Antelope* which possesses these canine teeth; but in which, by a singular anomaly, it is only the young males that are furnished with them. In these too they can only be considered in the light of half-developed germs; for the cartilaginous part which covers the palate and the upper jaw entirely conceals them.

It is the *Ant. montana*, which I discovered in 1824 in the neighbourhood of Sennaar, and of which I published in my 'Zoological Atlas' the figure of an adult male, that is provided, in its youth, with these anomalous canine teeth: the adults of both sexes, and the young females, are destitute of them. I observed, in my last journey in Abyssinia, many individuals of this species in the valleys in the neighbourhood of Gondar: it is far from rare in that locality, but the jungles mingled with thorns, which are its favourite retreat, render the chase of it extremely difficult.

At the time of the publication of my description of this new species, in 1826, I was possessed of only a single adult male, and there were consequently many deficiencies in my account of it. I am now enabled to add to this notice that the females of this species are always destitute of horns; that both sexes have, in the [groins] two rather deep pits covered by a stiff bundle of white hairs; and finally that the species lives in pairs in the valleys of the western part of Abyssinia, where it takes the place of *Ant. Saltiana*, an animal which it exceeds in size by nearly one half. These two species are called by the natives *Madoqua*, by which name the Abyssinians also designate the *Ant. Grimmia*, which equally constitutes a part of the game of that country, so rich in different forms of the *Ruminant* order.—E. R.

A note by Mr. Martin was subsequently read, in which it was stated that it had once occurred to him to observe a rudimentary canine tooth in the female of a species of *Deer* from South America, the body of which had been sent to the Society's house by Sir P. Grey Egerton, for examination. Having noticed an enlargement of the gum of the upper jaw, in the situation in which a canine tooth might possibly be supposed to exist, he cut into it, and found the germ of a canine tooth, about 3 lines in length, imbedded in the gum, and destitute of fang.

Jan. 26.—Specimens were exhibited of numerous *Birds*, chiefly from the Society's collection; and Mr. Gould, at the request of the Chairman, directed the attention of the Meeting to those among them which he regarded as principally interesting either on account of their novelty or for the peculiarity of their form.

They included the following species of the genus *Edolius*, Cuv., which were compared with numerous others placed upon the table for that purpose.

Edolius *grandis, Rangoonensis, Crishna,* and *viridescens*.

Of *Edolius Crishna* a very curious character is furnished by the long, hair-like, black filaments which spring from the head and measure nearly 4 inches in length.

The remaining previously undescribed *Birds* that were exhibited were characterized by Mr. Gould as Orpheus *modulator*, Ixos *leu-*

cotis, Colluricincla *fusca*, Trichophorus *flaveolus*, and Geocichla *Rubecula*.

Mr. Gould subsequently directed the attention of the Meeting to a specimen of the *Turdus macrourus* of Dr. Latham, with the view of explaining the characters which induced him to regard that bird as constituting the type of a new

Genus Kittacincla.

Rostrum caput longitudine æquans, ad apicem emarginatum, rectiusculum, compressiusculum.

Nares basales, plumis brevibus utplurimum tectæ.

Alæ mediocres, rotundatæ: *remige* 1mâ brevissimâ, 4tâ 5tâque subæqualibus, longioribus.

Cauda elongata, gradata.

*Tarsi digiti*que longiusculi, tenues.

Obs. Maribus color suprà utplurimum niger; subtùs brunneus vel albus.

A paper by B. H. Hodgson, Esq., Corr. Memb. Z.S., on some of the *Scolopacidæ* of Nipâl, was read; the copy transmitted by that gentleman to the Society containing various corrections of his memoir which was published at Calcutta in the 'Gleanings of Science' for August, 1831.

Mr. Hodgson's object in the present paper is to bring under the notice of zoologists the various species of the family referred to which occur in Nipâl, on the natural history of which country he has, during a residence of several years, been engaged in making most extensive researches. The result of these it is his intention immediately to publish, accompanied by finished representations of the animals, taken from drawings made in almost every instance from numerous living individuals of the several races.

Mr. Hodgson first describes in detail the *common Woodcock, Scolopax Rusticola*, Linn., as it occurs in Nipâl; where it is, in every respect of form and colour, evidently identical with the European bird. In Nipâl also it seems to be, as it is in Western Europe, of migratory habits: and the periods of its arrival in, and departure from, Nipâl, correspond altogether with the seasons of its appearance and disappearance in England.

He then proceeds to describe in detail the several kinds of *Snipe* which occur in Nipâl.

Two of these are so nearly related to the *common Snipe* of Europe, *Gallinago media*, Ray, that Mr. Hodgson is induced to regard them as being probably specifically identical with that bird: and he accordingly refers them to it as varieties, which are constantly distinguished from each other by the structure of the tail. In one of them the tail-feathers are fourteen or sixteen in number, and are all of the same form: in the other the tail-feathers vary in number from twenty-two to twenty-eight; and the outer ones on either side, to the number of six, eight, or ten, differ remarkably from those of the middle, being narrow, hard, and acuminated. The latter bird may, however, be regarded as the representative of a species to which the name of *Gall. heterura* may be given.

The other two *Snipes* of Nipâl are unquestionably distinct from those of Europe. They are described as the *solitary Snipe, Gall. solitaria,* Hodgs., and the *wood Snipe, Gall. nemoricola,* Ej.

In the *solitary Snipe* the wings are remarkably long; the upper surface, especially on the wings, is minutely dotted, barred, and streaked, with white intermingled with buff and brown; and the *abdomen* is white, barred along the flanks with brown.

The *wood Snipe* has the general colouring of the plumage dark and sombre; the wings short; the *abdomen* and the whole of the under surface thickly barred with transverse lines of dark brown on a dusky white ground; and a tail of sixteen or eighteen, or very rarely twenty, feathers.

Mr. Hodgson describes, with the greatest minuteness, each of these birds, and adverts with the fullest detail to their several habits and distinguishing peculiarities, as well of manners and of seasons as of form and plumage.

Feb. 9.—A letter was read, addressed to the Secretary by M. Thibaut, and dated Malta, January 8, 1836. It communicated various particulars relative to the *Giraffes* belonging to the Society, which have recently been obtained by the writer and which are now in his custody, and may be translated as follows:—

"Having learnt, on my arrival at Malta, that you were desirous of information on the subject of the four *Giraffes* which the Society has entrusted to my care, I regard it as a duty to transmit to you a short statement, by which you will become aware of the difficulties that I encountered in obtaining and preserving for the Society these interesting animals, which are now, I hope, altogether out of danger.

"Instructed by Colonel Campbell, His Majesty's Consul General in the Levant, and desirous of rendering available for the purposes of the Zoological Society the knowledge which I had acquired by twelve years' experience in travelling in the interior of Africa, I quitted Cairo on the 15th of April, 1834. After sailing up the Nile as far as Wadi Halfa (the second cataract), I took camels, and proceeded to Debbat, a province of Dongolah; whence, on the 14th of July, I started for the desert of Kordofan.

"Being perfectly acquainted with the locality, and on friendly terms with the Arabs of the country, I attached them to me still more by the desire of profit. All were desirous of accompanying me in my pursuit of the *Giraffes*, which, up to that time, they had hunted solely for the sake of the flesh, which they eat, and of the skin, from which they make bucklers and sandals. I availed myself of the emulation which prevailed among the Arabs, and as the season was far advanced and favourable, I proceeded immediately to the south-west of Kordofan.

"It was on the 15th of August that I saw the first two *Giraffes*. A rapid chase, on horses accustomed to the fatigues of the desert, put us in possession, at the end of three hours, of the largest of the two: the mother of one of those now in my charge. Unable to take her alive, the Arabs killed her with blows of the sabre, and, cutting her to pieces, carried the meat to the head-quarters which we had established in a wooded situation; an arrangement necessary for our own comforts and to secure pasturage for the camels of both sexes which we had brought with us in aid of the object of our chase. We deferred until the morrow the pursuit of the young *Giraffe*, which my companions assured me they would have no difficulty in again discovering. The Arabs are very fond of the flesh of this animal. I partook of their repast. The live embers were quickly covered with slices of the meat, which I found to be excellent eating.

"On the following day, the 16th of August, the Arabs started at daybreak in search of the young one, of which we had lost sight not far from our camp. The sandy nature of the soil of the desert is well adapted to afford indications to a hunter, and in a very short time we were on the track of the animal which was the object of our pursuit. We followed the traces with rapidity and in silence, cautious to avoid alarming the creature while it was yet at a distance from us. Unwearied myself, and anxious to act in the same manner as the Arabs, I followed them impatiently, and at 9 o'clock in the morning I had the happiness to find myself in possession of the *Giraffe*. A premium was given to the hunter whose horse had first come up with the animal, and this reward is the more merited as the laborious chase is pursued in the midst of brambles and of thorny trees.

"Possessed of this *Giraffe*, it was necessary to rest for three or four days, in order to render it sufficiently tame. During this period an Arab constantly holds it at the end of a long cord. By degrees it becomes accustomed to the presence of man, and takes a little nourishment. To furnish milk for it I had brought with me female camels. It became gradually reconciled to its condition, and was soon willing to follow, in short stages, the route of our caravan.

"This first *Giraffe*, captured at four days' journey to the south-west of Kordofan, will enable us to form some judgement as to its probable age at present; as I have observed its growth and its mode of life. When it first came into my hands, it was necessary to insert a finger into its mouth in order to deceive it into a belief that the nipple of its dam was there: then it sucked freely. According to the opinion of the Arabs, and to the length of time that I have had it, this first *Giraffe* cannot, at the utmost, be more than nineteen months old. Since I have had it, its size has fully doubled.

"The first run of the *Giraffe* is exceedingly rapid. The swiftest horse, if unaccustomed to the desert, could not come up with it unless with extreme difficulty. The Arabs accustom their coursers to hunger and to fatigue; milk generally serves them for food, and gives them power to continue their exertions during a very long run. If the *Giraffe* reaches a mountain, it passes the heights with rapidity: its feet, which are like those of a *Goat*, endow it with the dexterity of that animal; it bounds over ravines with incredible power; horses cannot, in such situations, compete with it.

"The *Giraffe* is fond of a wooded country. The leaves of trees are its principal food. Its conformation allows of its reaching their tops. The one of which I have previously spoken as having been killed by the Arabs measured 21 French feet in height from the ears to the hoofs. Green herbs are also very agreeable to this animal; but its structure does not admit of its feeding on them in the same manner as our domestic animals, such as the *Ox* and the *Horse*. It is obliged to straddle widely; its two fore-feet are gradually stretched widely apart from each other, and its neck being then bent into a semicircular form, the animal is thus enabled to collect the grass. But on the instant that any noise interrupts its repast, the animal raises itself with rapidity, and has recourse to immediate flight.

"The *Giraffe* eats with great delicacy, and takes its food leaf by leaf, collecting them from the trees by means of its long tongue. It rejects the thorns, and in this respect differs from the *Camel*. As the grass on which it is now fed is cut for it, it takes the upper part only, and chews it until it perceives that the stem is too coarse for it. Great care is required for its preservation, and especially great cleanliness.

"It is extremely fond of society and is very sensible. I have observed one of them shed tears when it no longer saw its companions or the persons who were in the habit of attending to it.

"I was so fortunate as to collect five individuals at Kordofan; but the cold weather of December, 1834, killed four of them in the desert on the route to Dongolah, my point of departure for Bebbah. Only one was preserved; this was the first specimen that I obtained, and the one of which I have already spoken. After twenty-two days in the desert, I reached Dongolah on the 6th of January, 1835.

"Unwilling to return to Cairo without being really useful to the Society, and being actually at Dongolah, I determined on resuming the pursuit of *Giraffes*. I remained for three months in the desert, crossing it in all directions. Arabs in whom I could confide accompanied me, and our course was through districts destitute of everything. We had to dread the Arabs of Darfour, of which country I saw the first mountain. We were successful in our researches. I obtained three *Giraffes*, smaller than the one I already possessed. Experience suggested to me the means of preserving them.

"Another trial was reserved for me: that of transporting the animals, by bark, from Wadi Halfa to Cairo, Alexandria, and Malta. Providence has enabled me to surmount all difficulties. The most that they suffered was at sea, during their passage, which lasted twenty-four days, with the weather very tempestuous.

"I arrived at Malta on the 21st of November. We were there detained in quarantine for twenty-five days, after which, through the kind care of Mr. Bourchier, these valuable animals were placed in a good situation, where nothing is wanting for their comfort. With the view of preparing them for the temperature of the country to which they will eventually be removed, I have not thought it advisable that they should be clothed. During the last week the cold has been much greater than they have hitherto experienced; but they have, thanks to the kindness of Mr. Bourchier, everything that can be desired.

"These four *Giraffes*, three males and one female, are so interesting and so beautiful, that I shall exert myself to the utmost to be of use to them. It is possible that they may breed; already I observe in them some tendency towards mutual attachment. They are capable of walking for six hours a day without the slightest fatigue.—G. T."

Mr. Gould, at the request of the Chairman, exhibited a specimen of the *Trogon resplendens*, Gould, and one of the *Trog. pavoninus*, Spix; and stated that he was indebted to the kindness of M. Natterer, who was present at the Meeting, for the opportunity of demonstrating, by the juxtaposition of the *Birds*, the correctness of the determination which he had made in regarding them as distinct species. Mr. Gould directed particular attention to the several characters and distinguishing marks which he had pointed out to the Society on March 10, 1835, and which had subsequently been published in the 'Proceedings,' part iii. p. 29 (Lond. and Edinb. Phil. Mag., vol. vii. p. 226.), and again dwelt especially on the fact that in *Trog. resplendens* the hinder feathers of the back, which are fully 3 feet in length, hang gracefully far away beyond the tail; while in *Trog. pavoninus* the lengthened feathers of the back are rarely equal in length to the tail: in only one instance has M. Natterer known them, in the latter bird, to exceed the tail by so much as a quarter of an inch.

XXXIII. *Intelligence and Miscellaneous Articles.*

EFFECTS OF COMPRESSED AIR ON THE HUMAN BODY.

DR. JUNOD has communicated to the Academy of Sciences the results of his experiments with compressed air.

In order to operate on the whole person, a large spherical copper receiver is employed, which is entered by an opening in the upper part, and which has a cover with three openings; the first for a thermometer, the second for a barometer or manometer, and a third for a tube of communication between the receiver and the pump. The air in the receiver is perpetually renewed by a cock.

When the pressure of the atmosphere is increased one half, the membrane of the tympanum suffers inconvenient pressure, which ceases as gradually as the equilibrium is restored. Respiration is carried on with increased facility; the capacity of the lungs seems to increase; the inspirations are deeper and less frequent. In about 15 minutes an agreeable warmth is felt in the interior of the thorax. The whole œconomy seems to acquire increased strength and vitality.

The increased density of the air appears also to modify the circulation in a remarkable manner: the pulse is more frequent, it is full and is reduced with difficulty; the dimensions of the superficial venous vessels diminish, and they are sometimes completely effaced, so that the blood in its return towards the heart follows the direction of the deep veins. The quantity of venous blood contained in the lungs ought then to diminish, and this explains the increased breathing of air. The blood there is then determined in larger quantity to the arterial system, and especially to the brain. The imagination becomes active, the thoughts are accompanied with a peculiar charm, and some persons are affected with symptoms of intoxication. The power of the muscular system is increased. The weight of the body appears to diminish.

When a person is placed in the receiver and the pressure of the air is diminished one fourth, the membrane of the tympanum is momentarily distended; the respiration is inconvenienced, the inspirations are short and frequent, and in about 15 or 20 minutes there is a true dyspnœa. The pulse is full, compressable and frequent; the superficial vessels are turgid. The eyelids and lips are distended with superabundant fluids, and hæmorrhage and tendency to syncope are sometimes induced; the skin is inconveniently hot, and its functions increased in activity; the salivary and renal glands secrete their fluids less abundantly.—*Journ. de Chim. Méd.*, June, p. 13.

GASTRIC JUICE.

Mons. H. Braconnot considers the gastric juice obtained from dogs to be composed of

1st. Free hydrochloric acid in considerable quantity.

2nd. Hydrochlorate of ammonia.

3rd. Chloride of sodium in large quantity.

4th. Chloride of calcium.

5th. Chloride of iron.

6th. Chloride of potassium, a trace.

7th. Chloride of magnesium.

8th. A colourless and pungent oil.

9th. Animal matter soluble both in water and alcohol, in considerable quantity.

10th. Animal matter soluble in dilute alkalies.

11th. Animal matter soluble in water, but insoluble in alcohol; (the salivary matter of Gmelin).

12th. Mucus.

13th. Phosphate of lime.

M. Blondelot has endeavoured to produce artificial digestions, at the temperature of the human body, by filling glass tubes, some with a mixture of bits of meat and gastric juice, and others with meat and water slightly acidulated with hydrochloric acid: in both cases the flesh preserved its primitive form and fibrous texture whilst quiescent, but by the slightest movement it was converted into a homogeneous mass precisely similar to chyme produced in the stomach.—*Journal de Pharm.*, Feb. 1836.

BIBROMIDE OF MERCURY.

M. Lassaigne informs us that bibromide of mercury is less soluble in water than the bichloride, 105 parts of the former salt requiring 10,000 parts of water at 48° Fahr. to dissolve it, or rather more than 1 of salt to 100 of water; also that albumen forms with the bibromide a compound which remains in solution when diluted with 30 times its weight of water, whilst a solution of the bichloride of the same strength soon becomes troubled and precipitates. This non-precipitation of albumen will not only serve to distinguish the bibromide from the bichloride, but if bichloride of mercury is mixed with from $\frac{1}{5}$th to $\frac{1}{10}$th of its weight of the bromide, it will detect the adulteration.—*Jour. de Chim. Méd.*, April.

FLUORINE.

M. Baudrimont states that he succeeded in isolating fluorine two years since; but he did not announce this discovery because he could not obtain it without a large admixture of oxygen gas. The process by which he first obtained fluorine was by passing fluoride of boron over minium heated to redness, and receiving the gas in a dry vessel. His present method is to treat a mixture of fluoride of calcium and binoxide of manganese with sulphuric acid in a glass tube; but the gas thus obtained is mixed with the vapour of hydrofluoric acid and fluosilicic acid gas; this mixture however does not interfere with the observation of the principal properties of fluorine, which is a gas of a yellowish brown colour, and possesses an odour resembling chlorine and burnt sugar: indigo is bleached by it; it does not act upon glass, but combines directly with gold.—*L'Institut*, 27th April, 1836. (See Messrs. Knox's paper in the present Number.)

ANTIMONIAL COPPER (ÉCLATANT).

Henry Rose analysed this mineral after separating the quartz with which it was mixed, and found its composition to be

Sulphur	26·34
Antimony..............	46·81
Iron	1·39
Copper	24·46
Lead	0·56

which gives the formula $\dot{\mathrm{C}}\mathrm{u} + \dddot{\mathrm{S}}\ 6$, analogous to the composition of zinkenite and miargyrite.—*L'Institut*, May 18, 1836.

ON THE ACTION OF BROMINE UPON ÆTHER.

M. Löwig added bromine to æther in successive small portions until it would not take up any more, and set the mixture aside for about a fortnight, when the æther was completely decomposed, giving rise to the following products, viz.:

1st, Formic acid.
2nd, Hydrobromic acid.
3rd, Hydrobromic æther.
4th, Dense bromic æther (schwerbromæther).
5th, Bromal

To separate these substances the decomposed liquor is to be distilled. The four first come over; and if the operation is not pushed too far, the bromal remains in the retort mixed with a little dense bromic æther and hydrobromic æther. By treating this residue with water, and setting it aside for 24 hours, beautiful crystals of hydrate of bromal are obtained.

Bromal.—Anhydrous bromal is composed of

Carbon	8·64	= 4 eqs.
Hydrogen	0·34	= 1 eq.
Oxygen	6·33	= 2 eqs.
Bromine	84·65	= 3 eqs.
	100·00	

The hydrate of bromal is formed of

1 eq. of bromal	$C^4\ H^1\ O^2\ Br^3$
4 eqs. of water	$H^4\ O^4$
1 eq. of hydrate of bromal	$C^4\ H^5\ O^6\ Br^3$

When the hydrate of bromal is boiled with an alkaline solution, eqs. $C^8\ H^{10}\ O^{12}\ Br^6$ form

2 eqs. of formic acid	=	C^4	H^2	O^6	
2 eqs. of bromoforme	=	C^4	H^2		Br^6
6 eqs. of water	=		H^6	O^6	
		C^8	H^{10}	O^{12}	Br^6

Bromoforme decomposes into bromine and formic acid.

Dense Bromic Æther (schwerbromæther). This is formed in great abundance in the decomposition of æther by bromine. It is very volatile, possesses a very penetrating and agreeable odour, and a sweet taste which remains for a length of time. It is much heavier than water, and even sinks in sulphuric acid. When it is boiled in this acid bromine is liberated, and a colourless fluid distils. Dense bromic æther can be obtained anhydrous by digesting it with caustic potash, and distilling it repeatedly from quick-lime. It is perfectly clear, as limpid as water, and refracts light very powerfully. When passed over red-hot lime it is decomposed, liberating a gas which burns with a clear flame, and forming bromide of calcium. When it is boiled with a solution of potash, bromoforme is evolved, which is decomposed into formic acid and bromine, forming formiate of potash and bromide of potassium. The composition of this ether deduced from three analyses, is

	Ist.	2nd.	3rd.
Carbon	7·80	8·88	9·20
Hydrogen	1·43	1·30	1·36
Oxygen	9·83	8·88	8·50
Bromine	80·94	80·94	80·94
	100·00	100·00	100·00

These results indicate the following atomic constitution :

8 eqs. of Carbon	49·04	8·52
8 eqs. of Hydrogen	8·00	1·39
6 eqs. of Oxygen	48·00	8·37
6 eqs. of Bromine	470·34	81·72
	575·38	100·00

Bromic æther being the heaviest of the fluids resulting from the decomposition of æther by bromine, it is very easy to separate it from the other products. It is still to be decided whether this is a separate compound, or a mixture of various substances.—*Ann. de Chimie*, March, 1836.

ON THE COMPOSITION OF THE CRYSTALLIZED HYDRATE OF POTASH.

M. Walter obtains hydrate of potash in fine crystals by pouring on three or four pounds of fused potash a little water, and when the mixture is cool, adding sufficient hot water to dissolve the remainder of the potash: at the expiration of 12 hours, by decanting the solution, the crystals will be found at the bottom of the vessel. The method of analysis adopted for determining the relative proportions of water and potash, was to neutralize a known weight of the crystals with hydrochloric acid, to evaporate the solution to dryness, and heat the resulting chloride of potassium to redness. 4·065 gram. of crystallized potash afforded 3·207 gram. of chloride = 1·684 gram. of potassium; and as 1·684 gram. of potassium = 2·028 of potash (protoxide of potassium), the crystallized hydrate will be composed of

2·028	potash,
2·037	water,
4·065	

which nearly agrees with the formula of 10 eqs. of water to 1 of potash.

The slight difference between the experimental and the calculated results is evidently owing to a little interposed water, and some slight degree of humidity which the surfaces of the crystals acquired during weighing.

There is also a third hydrate of potash, for 2·462 gram. of the crystals placed under the air-pump lost 0·527 gram., which indicates a compound of 21·4 water and 78·6 potash in 100 parts, or

1 eq. of potash....	48	=	77·71
3 eqs. water......	27	=	22·29
	75	=	100·00

Thus crystallized potash appears to lose 7 eqs. of water in vacuo.—*Journ. de Pharm.*, June, 1836.

Note.—How this crystallized hydrate can be regarded as a compound of 1 eq. of potash and 10 of water I am at a loss to know, as the analysis approximates to 1 eq. of potash and 5 of water; but in any case the analysis does not indicate any atomic combination, for by it 48 of potash will combine with 48·3 of water, the equivalent of potash and water being respectively 48 and 49.—J. S. D.

ON THE COMBINATIONS OF CHROMIUM WITH FLUORINE AND CHLORINE.

Henry Rose has submitted the gaseous perfluoride of chromium of Unverdorben to a rigid examination. He obtained the gas by acting on a mixture of fluoride of calcium and bichromate of potash by sulphuric acid; the gas when passed into water contained in a platinum vessel afforded a solution of the chromic and hydrofluoric acids, from which he obtained 2·16 of fluoride of calcium = 1·031 of fluorine, and 0·339 of chromium. A second analysis afforded 3·02 of fluoride of calcium, and ·729 of oxide of chromium: the mean of these analyses is, chromium 25·57, and fluorine 74·43, in 100 parts. Although the mode of analysis adopted does not admit of absolute certainty, yet it affords an approximation to the true composition utterly at variance with the existence of a perfluoride of chromium, the composition of which, to be analogous to chromic acid, would be

Chromium........	33·4	
Fluorine.........	66·6	——100·0

whilst, according to M. Rose's analysis, the constitution of this substance approaches to a compound of 5, and not of 3 double eqs. of fluorine to 1 eq. of chromium, in which case it would be composed of

Chromium........	23·13	
Fluorine..........	·87	——100·0

If this gaseous body is indeed a fluoride of chromium composed of 5 double eqs. of fluorine to 1 eq. of chromium, the existence of an analogous oxide of chromium containing 5 eqs. of oxygen and 1 eq. of chromium is not improbable.

The chlorochlomic acid of Thomson, prepared by heating a mixture of common salt, bichromate of potash, and sulphuric acid together, on analysis afforded from 1·241 gramme, 2·294 of chloride of silver, and 0·629 of oxide of chromium, which indicate 45·6 of chlorine, 35·53 of chromium, and a loss of 18·87 in 100 parts. By a second analysis of this chloride prepared at another period, 3·33 of chloride of silver, and 0·975 of oxide of chromium were obtained from 1·802 parts, which results are equal to 45·59 of chlorine, and 37·95 of chromium per cent.; and considering the deficiency as oxygen, this chloride according to the first analysis will be a compound of 2 eqs. of chromic acid and 1 eq. of chloride of chromium, which by calculation is equal to

Chromium........	35·38	
Chlorine..........	44·51	
Oxygen..........	20·11	——100·0

This substance is the only known instance of a volatile combination containing chromic acid, and that, a volatile compound formed of an oxide and a chloride. M. Rose dissents from Thomson's opinion in regard to the composition of this body, considering it to be a compound of chromic acid and chloride of chromium, and not a combination of chromic acid and chlorine; for if we consider with Thomson that all the chromium exists as chromic acid, there will be, if we adopt M. Rose's analysis, an excess o 10 per cent., which he attributes to impurity of the carbonate of soda employed by Thomson in his analysis.

In endeavouring to prepare a compound of selenium analogous to the compound of chromium by treating mixtures of the seleniates and chloride of sodium with sulphuric acid, chlorine and chloride of selenium analogous to selenious acid were obtained; and towards the end of the experiment green vapours rose, which condensed into an oily liquid composed of the selenious and sulphuric acids.

Bromide and iodide of potassium mixed with bichromate of potash, and acted on by sulphuric acid, liberate respectively bromine and iodine in a state of purity without the slightest admixture of chromium.—*Ann. de Chimie*, January, 1836.

ON THE ACTION OF SULPHURIC ACID ON OILS.

M. Fremy in examining the kind of saponification which sulphuric acid exerts upon oil, has arrived at several facts in addition to those already ascertained by MM. Chevreul, Braconnot, and Caventou. The oils employed were olive and almond, and the results from both were perfectly similar. When olive oil is treated with half its weight of concentrated sulphuric acid, surrounding the vessel with a freezing mixture to prevent any elevation of temperature and consequent evolution of sulphurous acid, the acid being added very cautiously, after a few minutes the mixture becomes viscid, when the action is finished. Then the mass being treated with water, rather less than six times the bulk of the oil employed, the mixture separates into two strata; the superior is of syrupy consistence, whilst the lower is chiefly composed of water and sulphuric acid; this latter is a sulphoglycerate of lime (?), whilst the superior layer is a mixture of three acids, which he calls sulphostearic, sulphomargaric, and sulpholeic acids.

The aqueous solution of these acids decomposes in a few days, sulphuric acid being formed, and the three fatty acids precipitated.

The sulphostearic and sulphomargaric acids possess little stability, as they always decompose in from 24 to 48 hours at most, which property M. Fremy has availed himself of to separate these two solid acids from the third, which is liquid, and is derived from the decomposition of sulpholeic acid. The two solid acids can be separated by means of alcohol; these he has named hydrostearic and metamargaric acids.

Hydrostearic acid is solid, white, insoluble in water, soluble in both alcohol and æther, from which it crystallizes in mammillated groups; it fuses at about 129° Fahr. Its composition is $C^{35}\ H^{72}\ O^{5}$: it loses $\frac{1}{2}$ an equivalent of water when in combination with bases. It may be volatilized without alteration. All its salts are insoluble in water, except the hydrostearates of soda and ammonia.

Metamargaric acid is white like the preceding; soluble in alcohol; fuses at 120° Fahr. Its composition is given in the formula $C^{35}\ H^{70}\ O^{4\frac{1}{2}}$. It loses $\frac{1}{2}$ an equivalent ($1\frac{1}{2}$?) of water in combining with bases, and becomes $C^{35}\ H^{67}\ O^{3}$, that is, exactly the same composition as common margaric acid.

Hydroleic acid is a slightly coloured liquid at 32° Fahr.; is composed of $C^{35}\ H^{66}\ O^{5\frac{1}{2}}$; loses $\frac{1}{2}$ an equivalent of water by combination, and becomes $C^{35}\ H^{65}\ O^{5}$. When distilled it is almost totally decomposed into carbonic acid, water, and an oil composed almost wholly of two new hydrocarburets. These are liquid at ordinary temperatures; their composition is the same, but the density of their vapours differ, one boiling at 131° Fahr., the other at 226° Fahr. The first he has named oleene, the second elaene. Oleene is a white limpid fluid, burns with a vivid flame, and is composed of carbon 85·95, hydrogen 14·05, or CH^2. Thus this hydrocarburet appears to be isomeric with carbohydrogen, &c. &c.

Elaene is white, less fluid than oleene, boils at 226° Fahr.: its odour is more penetrating than that of oleene, but its elementary composition is the same. It is insoluble in water, but dissolves in alcohol and æther. It combines with chlorine in the proportion of 4 vols. of chlorine to 4 of elaene.

Hydroleic acid treated with concentrated sulphuric acid forms sulpholeic acid, which is soluble both in water and alcohol, and of a slightly acid, and very bitter taste. The sulpholeates of the alkalies are soluble, but have not been crystallized. Sulpholeate of lime is composed of 2 eqs. of hydroleic acid, 1 eq. of sulphuric acid, 1 eq. of lime, and 1 eq. of water. From these experiments it appears that sulphuric acid exerts a kind of saponifying influence on oil; forming glycerine, which combines in its nascent state with sulphuric acid; and fatty acids, which form similar combinations. From the knowledge that fat substances under such different actions always give rise to glycerine and fatty acids, M. Fremy argues that they are educts and not products preexisting in fatty bodies.—*L'Institut*, May 11th.

ON ETHAL.

MM. Dumas and Peligot have analysed ethal with results similar to those of M. Chevreul; they find its composition to be

Carbon	79·2	=	C^{32}
Hydrogen	14·2	=	H^{34}
Oxygen	6·6	=	O

which when compared to alcohol by doubling the equivalents gives the formula

$$C^{64}\ H^{64} + H^4\ O^2.$$

When ethal is distilled with anhydrous phosphoric acid it affords a product composed of

Carbon.......	86·2	=	C^{64}
Hydrogen	14·2	=	H^{64}

which is isomeric with olefiant gas, but exists in a different state of combination; this substance they have named Cetene.

They were not successful in obtaining a compound of cetene corresponding to æther, but have procured a substance analogous to sulphovinic acid. By heating ethal with sulphuric acid and frequently stirring the mixture they combine, forming sulphocetic acid. Sulphocetate of potash is a perfectly white salt, and occurs in pearly spangles; it contains

Sulphate of potash ...	24·0	=	1 eq.
Sulphuric acid	11·7	=	1 eq.
Carbon	53·5	=	64 eqs.
Hydrogen	9·1	=	66 eqs.
Oxygen............	2·1	=	1 eq.

which accords with the formula $SO^3, KO + SO^3, C^{64}H^{64} + H^2O$.

They have also obtained a compound of chlorine and cetene by distilling together ethal and perchloride of phosphorus; the hydrochlorate of cetene is composed of

Carbon......	73·67	=	C^{64}
Hydrogen ..	12·32	=	H^{66}
Chlorine	13·70	=	Ch^2

These chemists consider spermaceti as a compound of margarate and oleate of cetene, in the proportions of

2 eqs. margaric acid,
1 eq. oleic acid,
3 eqs. of cetene,
3 eqs. of water,

and give the following extraordinary formula, founded on an analysis of Chevreul: 472 eqs. of carbon, 445 eqs. of hydrogen, and 14 eqs. of oxygen.—*L'Institut*, May 4th.

VOLATILE OIL OF THE BARK OF THE PRUNUS PADUS.

This oil is a hydruret of benzule analogous to oil of bitter almonds: it affords by analysis

Carbon....	79·34	=	14 eqs.
Hydrogen..	5·68	=	6 eqs.
Oxygen ..	14·98	=	2 eqs.
	100·00		

M. Löwig observes that when this oil is placed in contact with potassium on mercury, the potassium darts rapidly about and soon disappears, the oil becomes deeper-coloured and at last viscid without any visible disengagement of gas.—*Ann. de Chimie*, March, 1836.

ON THE ACTION OF OXALIC ACID ON THE SULPHATES OF IRON AND COPPER.

When a concentrated solution of oxalic acid is poured into one of protosulphate of iron, the liquid assumes a yellow colour, and precipitates after standing for some time. This precipitation does not occur in a solution of the persulphate by the addition of oxalic acid; and in general ferruginous salts are not precipitated either by oxalic acid or by oxalate of ammonia.

These phænomena have already been noticed by M. A. Rose; but M. Vogel of Munich wishing to know whether the decomposition was complete or only partial, instituted a series of experiments, from which he concludes that oxalic acid entirely decomposes the sulphates of iron and copper, setting at liberty the whole of the sulphuric acid, its affinity for these oxides being greater than that of even sulphuric acid. The oxalate of iron obtained is a yellow powder almost insoluble in water, which when heated to redness in a closed vessel leaves a residue of protoxide and carburet of iron. The oxalate of copper is a blue powder insoluble in water, which heated to redness affords metallic and protoxide of copper.—*Jour. de Pharm.*, April, 1836.

LOCALITY OF NATIVE MERCURY.

M. de Bonnard has communicated to the Philomathique Society of Paris, a notice by M. Alluaud, sen. of Limoges, respecting the mercury of Peyrat-le-Chateau, department de la Haute-Vienne.

This metal is found in the native state in a disintegrated granite, which forms the esplanade of the ancient castle of Peyrat, on the side of the royal road from Figeac to Montargis. M. Alluaud describes the nature of the soil of the country, which is entirely formed of various kinds of granite passing into each other, as kaolen and gneiss, &c. On the esplanade of the castle of Peyrat, M. Ranque, in clearing the soil and digging the foundation of a house, found twelve pounds of native mercury, and other persons also found some. M. Alluaud having made several excavations and also examined the places, found the mercury disseminated in a fine-grained granite, which was very quartzose, and the felspar was decomposed. The metal does not exist throughout the rock, but only in parts of it; no bed, vein, or fissure can be perceived. The metal has been found at several distinct places, far from each other and without any communication; this circumstance is unfavourable to the idea of an accidental infiltration from above, for in this case the metal would have occupied a circumscribed situation in some fissure of the rock.

Notwithstanding the singularity of this locality of native mercury in a primary rock which contains no indications of cinnabar, and difficult as it is to draw a conclusion from an isolated observation confined to the narrow space of a few feet, M. Alluaud does not hesitate to pronounce either that the mercury is disseminated in the rock in small masses, irregular both as to form and extent, and in this case that the deposit has been contemporaneous with the formation of the rock; or that it occupies fissures in the rock, which are now imperceptible, into which it was subsequently conveyed by sublimation from the interior of the earth.—*L'Institut*, No. 160.

DONIUM, A NEW METAL CONTAINED IN DAVIDSONITE.

This mineral was discovered by Dr. Davidson of Aberdeen, in a granite quarry in the neighbourhood of that city; it has been examined by Mr. Thomas Richardson, and he concludes that he has obtained from it a metal which differs from any previously known.

"From the alkaline and earthy bases, and from several of the metallic ones, it is eminently distinguished by the green precipitate which it gives with sulpho-hydrate of ammonia; while its solubility in the caustic alkalies, and in carbonate of ammonia, the light brown precipitate thrown down by sulphuretted hydrogen, and the green given by sulpho-hydrate of ammonia, are amply sufficient to distinguish it from all the others.

"If this substance be considered as sufficiently distinct, which, from its characters, I think I am warranted to conclude, I shall propose to give it the name of *Donium*, being a convenient contraction of Aberdonia, the Latin name of Aberdeen, near which place Davidsonite occurs; for the suggestion of which name I am indebted to Dr. Thomson.

"The change of colour which the precipitates of this substance undergo, during the process of washing, appears to be owing to different degrees of oxidation; and with a view to determine, if possible, the characters of the metal itself, as well as its degrees of oxidation, the following experiments were made:

"A. Over a portion of the white oxide strongly heated to redness, in a green glass tube, a current of dry hydrogen gas was passed, for nearly an hour. The whole was converted into a slate-blue mass, while aqueous vapour was evolved at the end of the tube: 100 parts of the white powder, by this means, lost 16·34 of their weight.

"B. A portion of the buff oxide was treated in the same way, and the same slate-blue powder was obtained, with the evolution of aqueous vapour: 100 parts of this oxide lost, by this process, 5·11 of their weight.

"The substance possessing the slate-blue colour exhibited the following characters:

"1. When pounded in dry agate mortar, it appeared to assume a lustre, resembling the metallic.

"2. When heated to redness, it glowed like tinder, and became white.

"3. In dilute muriatic acid, it effervesced, and was converted into white powder.

"4. When placed in a charcoal crucible, properly inclosed, and heated strongly in a forge for half an hour, it was not altered.

"It seems probable, that the slate-blue substance consisted of metallic donium, but in a state of intimate division; while from the experiments made upon the oxides, upon which, however, for many reasons, great confidence cannot be placed, it would appear that the oxides are composed of

1. The Buff . . 94·89 Donium + 5·11 oxygen.
2. The White . 83·66 Donium + 16·34 oxygen.

Or, that the white oxide contains thrice as much oxygen as the buff.

"Although circumstances do not permit of my continuing this investigation, I have reason to believe that it will not be laid aside, but that a more full account of this substance will shortly be given by an individual much more capable of performing the task."—*Records of Science*, June, 1836.

GEOLOGY OF MANCHESTER.

Professor Phillips, in a recent examination of this neighbourhood with reference to the question of the geological age of the limestones and shelly marls, which has lately attracted much attention, has ascertained that the limestone of Ardwick, usually classed with the magnesian limestone, is in truth (as Dr. C. Phillips of Manchester had previously stated to him) a *part of the coal formation of Lancashire.* He has discovered in it the bones of a reptile, perhaps the most ancient yet known in Great Britain. A vast number of parts of fishes (including *Megalichthys Hibberti*), shells, several and many plants of the ordinary coal shales have also been collected by himself, Dr. Phillips, Mr. Looney, Mr. Wm. Williamson, and others. Some of the results of these inquiries, which lead to important inferences as to the possible extension of coal works in the midland counties, will be offered to the British Association at Bristol, August 22, 1836.

EHRENBERG'S NEW DISCOVERY IN PALÆONTOLOGY: TRIPOLI COMPOSED WHOLLY OF INFUSORIAL EXUVIÆ.

At the sitting of the Royal Academy of Sciences of Paris, July 11th, the following letter was communicated, dated Berlin the 3rd of July, from M. Alexander Brongniart:—"I have today become acquainted with a discovery entirely new, for which we are indebted to M. Ehrenberg, and which he has demonstrated to me in the clearest manner; it is that the rocks of homogeneous appearance which are not very hard, friable, even fissile, entirely formed of silex, and which are known by the names of tripoli, more or less solid (*Polierschiefer* of Werner,) are entirely composed of the exuviæ or rather of the perfectly ascertained skeletons of infusorial animals of the family of the *Bacillariæ* and of the genera *Cocconema*, *Gomphonema*, *Synedra*, *Gaillonella*, &c. These remains having perfectly preserved the forms of the siliceous carcases of these infusoria, may be seen with the greatest clearness through the microscope, and may easily be compared with living species, observed and accurately drawn by M. Ehrenberg. In many cases there are no appreciable distinctions.

The species are distinguished by the form, and still more surely by the number of *septa* or transverse lines which divide their small body; and M. Ehrenberg, who has been able to count them by the microscope, has observed the same number of these divisions in living and in fossil species.

They are the tripolis of Bilin in Bohemia, of Santa-Fiora in Tuscany, and of other places which I do not remember with certainty, (of the Isle of France and of Francisbad near Eger, if I am not mistaken,) which have given occasion to these curious observations. The slimy iron ore of marshes is almost wholly composed of *Gaillonella ferruginea*.

The greater part of these species are lacustrine, but there are also some marine, particularly in the tripoli of the Isle of France.—*L'Institut*, No. 166.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The next Meeting will be held at Bristol during the week commencing on Monday, August 22nd; the Members of the General Committee will assemble on the preceding Saturday.

SCIENTIFIC MEMOIRS, Part I.

We have to announce the publication of the First Part of a new periodical work, conducted by Mr. Richard Taylor, assisted by Mr. E. W. Brayley, junr., entitled SCIENTIFIC MEMOIRS, SELECTED FROM THE TRANSACTIONS OF FOREIGN ACADEMIES OF SCIENCE AND LEARNED SOCIETIES, AND FROM FOREIGN JOURNALS. This work may be considered as supplementary to the PHILOSOPHICAL MAGAZINE and other scientific journals; and will give in an œconomical form translations of papers of great interest, but which may exceed the limits of those publications.

The following are the contents of the First Part:

Memoir on the Free Transmission of Radiant Heat through different Solid and Liquid Bodies. By M. Melloni.—New Researches relative to the Immediate Transmission of Radiant Heat through different Solid and Liquid Bodies. By M. Melloni.—Experiments on the Circular Polarization of Light. By H. W. Dove.—Description of an Apparatus for exhibiting the Phænomena of the Rectilinear, Elliptic, and Circular Polarization of Light. By H. W. Dove.—Memoir on Colours in general, and particularly on a new Chromatic Scale, deduced from Metallochromy for Scientific and Practical Purposes. By M. Leopold Nobili.—On the Mathematical Theory of Heat. By M. S. D. Poisson.—Researches on the Elasticity of Bodies which crystallize regularly. By M. Felix Savart.—Experiments on the Oil of the *Spiræa Ulmaria* (Meadow-Sweet). By Professor Löwig.—Researches relative to the Insects, known to the Ancients and Moderns, by which the Vine is infested, and the means of preventing their Ravages. By the Baron Walckenaer.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1836.

Chiswick.—June 1. Overcast. 2. Cloudy and fine: rain at night. 3. Cloudy. 4. Fine. 5. Fine: showery. 6. Very fine. 7. Overcast. 8. Very fine. 9. Fine: slight rain. 10. Overcast: rain. 11—14. Very fine. 15. Hot and dry: lightning at night. 16. Very fine. 17. Fine: thunder showers at noon in heavy drops. 18. Very fine. 19. Fine.: thunder showers. 20. Fine. 21. Cloudy. 22. Rain: overcast and fine. 23. Fine. 24. Fine: thunder storm in afternoon: clear at night. 25. Showery. 26, 27. Very fine. 28—30. Hot and dry.

Boston.—June 1. Cloudy. 2. Cloudy: rain P.M. 3. Cloudy: thunder and lightning with rain P.M. 4, 5. Cloudy: rain P.M. 6. Fine: rain P.M. 7—9. Cloudy. 10. Cloudy and stormy. 11. Cloudy: rain early A.M. 12. Fine: rain P.M. 13, 14. Cloudy. 15. Fine. 16. Fine: rain early A.M. 17. Fine: rain P.M. 18. Cloudy: rain early A.M. 19. Fine: rain P.M. 20. Cloudy. 21. Stormy: rain P.M. 22. Cloudy: rain A.M. 23. Fine. 24. Cloudy: rain P.M. 25. Stormy. 26, 27. Cloudy. 28—30. Fine.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. Thompson at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. Veall at Boston.

Days of Month. 1836. June.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Self-registering. Max.	Max.	Min.								
W. 1.	29·927	29·939	29·888	29·50	56·0	49·2	61·6	63	48	52	NE.	NE.	NE.	·027	...	...	50
Th. 2.	29·736	29·744	29·597	29·31	57·2	51·0	63·7	62	51	55	E.	SE.	calm	...	·14	...	52
F. 3.	29·614	29·604	29·571	29·05	62·3	53·8	66·7	69	54	60	S.	S.	calm	·069	·01	·25	54
S. 4.	29·619	29·733	29·582	29·00	59·7	55·3	67·4	69	47	61	SSW.	S.	W.	·036	·09	·06	54
⊙ 5.	29·780	29·939	29·750	29·13	60·7	50·9	62·6	64	43	59	SW.	NW.	W.	·111	·03	·05	54
☾ M. 6.	30·058	30·034	29·979	29·46	55·9	47·7	66·8	69	52	59	WSW.	S.	W.	·038	·01	·04	50
T. 7.	29·940	29·939	29·700	29·37	58·0	53·9	62·6	64	53	54·5	S.	S.	calm	...	·05	·04	51
W. 8.	29·606	29·647	29·628	29·08	62·6	54·6	68·3	72	52	61	SW.	SW.	calm	...	...	...	53
Th. 9.	29·759	29·824	29·745	29·15	61·8	54·7	66·3	68	57	62	S.	SW.	W.	...	·01	...	55
F. 10.	29·837	29·848	29·806	29·18	61·9	58·0	67·6	69	56	64	S.	SW.	W.	...	·06	...	57
S. 11.	29·756	29·952	29·785	29·14	64·9	59·2	68·6	72	47	64·5	SSE.	S.	calm	·175	...	·09	58
⊙ 12.	30·146	30·262	30·104	29·48	62·3	51·0	65·2	72	46	63	SW.	S.	W.	...	...	...	56
M. 13.	30·317	30·309	30·224	29·70	64·7	53·0	72·3	76	55	62	SW.	W.	W.	...	...	·06	56
● T. 14.	30·269	30·255	30·180	29·62	67·5	59·2	70·7	81	50	70	SW.	S.	calm	...	...	...	60
W. 15.	29·958	29·938	29·815	29·33	69·0	55·3	77·6	86	58	70·5	E.	S.	calm	...	·01	...	60
Th. 16.	29·934	29·910	29·898	29·17	67·4	63·5	73·6	74	52	72	SSE.	S.	NW.	...	...	·04	61
F. 17.	29·907	29·890	29·786	29·20	64·8	58·2	72·3	75	49	69	S.	S.	calm	...	·80	...	59
S. 18.	29·841	29·798	29·683	29·13	64·6	53·9	71·3	73	51	61·5	SSW.	SW.	W.	·075	...	·12	59
⊙ 19.	29·748	29·869	29·718	29·10	63·6	55·2	70·3	72	52	64	SW.	W.	W.	...	·09	...	58
M. 20.	30·035	30·028	29·990	29·35	61·4	55·2	68·6	69	50	59	SW.	W.	W.	·102	...	·21	59
T. 21.	30·033	30·015	29·970	29·42	60·6	53·3	70·3	70	56	58	W.	W.	NW.	...	·03	...	57
☽ W. 22.	29·950	30·029	29·833	29·31	63·2	58·0	65·3	64	57	63	S. var.	SW.	W.	·094	·04	·40	60
Th. 23.	29·825	29·809	29·794	29·21	63·0	59·2	68·2	71	54	64	S. var.	SW.	W.	·016	...	·06	60
F. 24.	29·804	29·901	29·742	29·20	62·2	56·7	68·2	68	50	62	SE. var.	SW.	W.	...	·27	...	58
S. 25.	30·073	30·259	30·081	29·38	62·6	52·4	67·3	69	51	60·5	SW.	SW.	W.	·119	·02	·06	55
⊙ 26.	30·284	30·310	30·255	29·65	64·9	54·6	68·7	74	49	61	SW.	SW.	NW.	·036	...	...	58
M. 27.	30·342	30·325	30·141	29·69	64·2	56·9	72·0	75	50	65	S.	S.	calm	...	...	...	59
○ T. 28.	30·136	30·207	30·069	29·41	71·3	58·9	82·6	85	48	70	SSE.	SW.	SW.	...	...	...	61
W. 29.	30·342	30·307	30·259	29·67	64·5	57·2	72·6	77	44	63	NE.	E.	W.	...	...	...	58
Th. 30.	30·334	30·298	30·186	29·70	67·2	55·3	72·7	85	58	65	E.	S.	E.	...	...	...	56
	29·964	30·325	29·571	29·33	63·0	55·2	69·1	86	43	62·4				Sum 8·98	1·66	1·48	56·6

THE

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PHILOSOPHICAL MAGAZINE

AND

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[THIRD SERIES.]

SEPTEMBER 1836.

XXXIV. *On such Functions as can be expressed by Serieses of periodic Terms.* *By* James Ivory, *Esq., M.A., F.R.S.**

NEVER was any speculation ushered into the mathematical world with more unmeasured praise than the analytical theory of Laplace for the attraction of spheroids approaching nearly to spheres. The very general nature of the processes, and the arriving at results by differentiations merely, without tedious and difficult integrations, called forth the admiration of every geometer. It is not the present intention to inquire whether the expectations raised by so novel a method of investigation, have been fulfilled: but it cannot be denied that the grounds of it are obscure, and have never been demonstrated so as to remove every doubt. The subject having been again brought forward, it is very desirable that it should not now be dismissed without a full elucidation.

The method of Laplace was first published in the Memoirs of the Academy of Sciences in 1782, and afterwards in the third book of the *Mécanique Céleste*. Objections having been made impugning the generality of the investigation, the author returned to the subject in the eleventh book of the same work, which appeared in 1823. He now greatly restricts the demonstration before given in the third book: insomuch that according to the new proofs the method is less general than

* Communicated by the Author.

that subordinate part of the first theory which is well demonstrated, and about which there is no dispute. Thus we have the direct testimony of Laplace that his method was originally extended beyond its just bounds. On the contrary M. Poisson has always contended for the exactness of the theory in its fullest extent. He undertook to clear it from all objections by giving an unexceptionable demonstration of it, first in the nineteenth cahier of the *Journal de l'Ecole Polytechnique*, published in 1823, next in the additions to the *Conn. des Tems* for 1829, and very lately in his Theory of Heat. Prof. Airy also turned his attention to this subject in the Cambridge Transactions for 1826. He thinks that the general theory of Laplace is strictly proved; but he maintains that it can be applied only to such expressions as are susceptible of no more than one development. By means of this modification he arrives at the same conclusions which are stated by Disjota in this Journal for last month (p. 84). But nothing can be more clearly proved than that no function can possibly have more than one development; which sets aside the suggestion of the Professor; who must, therefore, be ranked with those that admit the unrestricted theory of Laplace. Mr. Bowditch, in his excellent Translation, limits the general equation of Laplace, which applies to an attraction proportional to the nth power of the distance, by excluding all negative values of n from -2 to $-\infty$; and, by so doing, he has brought this curious but slippery speculation one step nearer the grasp of the human mind. Confining his attention to the law of attraction that prevails in nature, he attempts to prove the accuracy of Laplace's method in all its generality, drawing his arguments chiefly from geometrical considerations. It will be sufficient to remark here, that the nature of the function expressing the height of the molecule does not depend upon any integral taken between very small limits; but, as Laplace has clearly stated*, upon this, that the differentials shall invariably continue to be infinitely small as the molecule approaches the contact of the two surfaces. It will not be necessary to examine all the demonstrations enumerated; for they all turn upon the sense, more or less extensive, in which one equation is to be understood. It will be sufficient to discuss the investigation of M. Poisson, which is adopted by M. de Pontécoulant in his *Traité du Système du Monde*.

The question is fairly stated by Disjota in the last Number of this Journal. The value of X', which is the limit of the integral X when $\alpha = 1$, is shown to be a series, determinate in its form, all the terms of which satisfy an equation in partial

* *Méc. Cél.*, tom. v. pp. 25 & 26.

differentials; and it is further proved that the characteristic property of the terms of the series is independent of the expression $f(\theta, \psi)$, being derived entirely from the functions P_i produced by the expansion of the radical. The plain inference seems to be, that it will be impossible to deduce generally the value of $f(\theta, \psi)$ from a series the distinguishing character of which is independent of that function: yet this is what M. Poisson undertakes to accomplish by an artifice of calculation.

The formulas of M. Poisson are these*, viz.

$$\left.\begin{aligned} X &= \frac{1}{4\pi}\int_0^{\pi}\int_0^{2\pi} \frac{(1-\alpha^2) f(\theta', \psi') \sin\theta' \, d\theta' \, d\varpi'}{(1-2\alpha p+\alpha^2)^{\frac{3}{2}}}, \\ p &= \cos\theta\cos\theta' - \sin\theta\sin\theta'\cos(\psi-\psi'), \end{aligned}\right\} \quad (1.)$$

α being less than 1, but approaching to it indefinitely: and from this it is proposed to deduce, in the most general manner, the equation

$$X' = f(\theta, \psi), \qquad (2.)$$

X' representing the value of the double integral X when $\alpha = 1$.

If we suppose that $f(\theta', \psi')$ is constant in the small extent for which the increments of X are sensible, the first of the equations (1.) may be thus written:

$$X = f(\theta, \psi) \times \frac{1}{4\pi}\int_0^{\pi}\int_0^{2\pi} \frac{(1-\alpha^2) \sin\theta' \, d\theta' \, d\varpi'}{(1-2\alpha p+\alpha^2)^{\frac{3}{2}}},$$

which will coincide with the equation (2.), because the value of the integral is 1 when $\alpha = 1$, as it is easy to prove in many ways. Now it is presumed that $f(\theta', \psi')$ may be supposed constant, because the numerator of the differential is always small on account of the small multiplier $(1-\alpha^2)$, while the denominator increases rapidly, and quickly becomes so considerable as to make the increments of the integral insensible.

Such is the demonstration of M. Poisson as it appeared in 1823 in the *Journal de l'Ecole Polytechnique*. In the *Conn. des Tems* for 1829, and in the Theory of Heat the author, in order to make the matter plainer, supposes that the arcs θ' and ψ' vary from the initial values θ and ψ to $\theta+y$ and $\psi+z$, y and z being small increments: in consequence

$f(\theta', \psi') = f(\theta+y, \psi+z) = f(\theta, \psi) + \zeta$: and by substituting this value in the first of the equations (1.), we obtain

$$X' = f(\theta, \psi) + X'',$$

$$X'' = \int_0^{y}\int_0^{z} \frac{(1-\alpha^2) \zeta \sin\theta' \, d\theta' \, d\psi'}{(1-2\alpha p+\alpha^2)^{\frac{3}{2}}},$$

* *Théorie de la Chaleur*, p. 213.

supposing that all the quantities under the double sign of integration are expressed in terms of the two variables y and z, and that α is made equal to 1, after the integrations. This transformation is correct; and the introducing of the new integral is important, as it leads to detecting the fault of the investigation. M. Poisson assumes that the differentials of the new integral X'' are all infinitely small, so long as ζ is infinitely small: in which case, X'' being itself infinitely small, it may be rejected, and the same conclusion will be obtained as in the first investigation. Now were the denominators in the successive differentials of X'' always finite quantities, the assumption of M. Poisson would be allowable; but as both the numerators and denominators begin to vary from zero, it is not impossible that, while the first increase to a finite magnitude, and the others to some small quantity B, the quotients may pass through every gradation of quantity; their values may be infinitely small, or finite, or infinitely great. This point must therefore be examined before any just conclusion can be drawn.

Let $u = f(\theta, \psi)$, $u' = f(\theta', \psi')$; then $\zeta = u' - u$: put also $g = 1 - \alpha$; then

$$1 - \alpha^2 = 2g - g^2$$

$$1 - 2\alpha p + \alpha^2 = 2(1-p) - 2g(1-p) + g^2.$$

These values being substituted, the resulting expression of X'' will be,

$$X'' = \int_0^y \int_0^z \frac{(2g - g^2) \cdot \overline{u' - u} \cdot \sin\theta' \, d\theta' \, d\psi'}{(2(1-p) - 2g(1-p) + g^2)^{\frac{3}{2}}}.$$

Now $1-p$ is a small quantity depending upon the values assigned to y and z; and g is a small quantity quite independent of any other; we may therefore suppose that g is equal to $1-p$, or less than it, and even infinitely less than it. Now, c being any positive number less than $\frac{3}{2}$, if we reject quantities of the second order in the last formula, the result may be thus written:

$$X'' = \int_0^y \int_0^z \frac{2g}{(2(1-p))^c} \times \frac{u' - u}{(2(1-p))^{\frac{3}{2} - c}} \times \sin\theta' \, d\theta' \, d\varpi',$$

which is obviously the limit to which the expression of X'' continually tends as values decreasing indefinitely are assigned to y, z, g. Since g may be considered infinitely less than $1-p$, the ultimate value of the factor $\frac{2g}{(2(1-p))^c}$ is always zero: but distinctions must be made with regard to the other factor. When the value of

$$\frac{u'-u}{(2(1-p))^{\frac{3}{2}-c}}$$

is either always finite or infinitely small, all the differentials of X'' will be infinitely small as assumed by M. Poisson, and the equation (2.) will be proved. But the same equation will not be proved if the limit of the same factor be either infinitely great, or if it be a quantity that cannot be generally determined, and of which it cannot be said that it is either finite, or infinitely great.

If $c = 1$, the factor in question will be,

$$\frac{u'-u}{\sqrt{2(1-p)}};$$

which has a finite value when u, or $f(\theta, \psi)$ is a finite function of $\cos\theta$, $\sin\theta \cos\psi$, $\sin\theta \sin\psi$. This readily follows from the usual transformation of such expressions. The same factor will be infinitely small when $u'-u$ is divisible by $(1-p)^n$, n being any positive integer. In all these cases the equation (2.) is demonstrated. If $n = 1$, or if $u'-u$ be divisible by $1-p$, we fall upon the instance particularized by Laplace in the eleventh book of the *Mécan. Céleste.*

But if u, or $f(\theta, \psi)$, be not a finite function of $\cos\theta$, $\sin\theta \cos\psi$, $\sin\theta \sin\psi$, no determinate value can be assigned to the factor

$$\frac{u'-u}{(2(1-p))^{\frac{3}{2}-c}}$$

by any transformation; and in all such cases the equation (2.) is not demonstrated.

Lagrange has considered this subject in the 15th cahier of the *Journal de l'Ecole Polytechnique.* His investigation possesses all the exactness and clearness and elegance which distinguish the writings of this geometer. But the success of his analysis demands that $f(\theta', \psi')$ shall be a finite function of $\cos\theta'$, $\sin\theta' \sin\psi'$, $\sin\theta' \cos\psi'$. For such functions his process leads to a strict demonstration of the equation (2.): for functions of a different description, the algebraic operations fail, and no other conclusion can be drawn except that the equation (2.) is not demonstrated. It is very remarkable that the illustrious author has not noticed a distinction so obvious and necessary.

July 26, 1836. JAMES IVORY.

* *Théorie de la Chaleur*, p. 213.

XXXV. *On the probable Cause of certain Optical Properties observed by* Sir David Brewster *in Crystals of Chabasie. By* James F. W. Johnston, *A.M., F.R.S.E., F.G.S., &c., Professor of Chemistry and Mineralogy in the University of Durham.**

AT the Meeting of the British Association in Edinburgh, Sir David Brewster brought under the consideration of the Chemical section, some very interesting observations on the variations which the doubly refracting power is seen to undergo in different portions of the same crystal of certain varieties of chabasie. An abstract of this paper has since appeared in the Fourth Report of the Association, p. 575; but the leading fact is more precisely stated in the account of the Meeting published in the Literary Gazette (for 1834,) No. 952, p. 690. The double refraction in these crystals, which is positive in the rhomboidal nucleus or centre of the crystal, was seen "to diminish in succeeding layers from a positive state till it disappeared altogether; beyond this neutral line it became negative, and again gradually increased †." This observation was brought forward partly with the view of illustrating, as it does very beautifully, the importance of the optical character of minerals in throwing light upon the structure, composition, and mode of formation of such of them as occur in a crystalline state; but partly also to show that chemical analysis is liable to lead to error by treating as simple minerals what are in reality only aggregates of different substances deposited in successive layers around a common nucleus.

That the layers which exhibit the difference of optical properties mentioned by Sir David, have a different composition, is highly probable. A series of optical changes is produced on some substances, as on *glauberite* and *topaz*, by the elevation of temperature; or, as on unannealed glass, by sudden cooling: it is *possible*, therefore, that the deposition of successive layers of the same chemical constitution under a pressure or temperature constantly varying, *might* produce also phænomena analogous to those observed in the present case. There is not much apparent probability, however, that any such variations actually took place in the circumstances under which the crystals of chabasie were formed. That change of atomic arrangement which gives rise to the interesting phænomenon of dimorphism must necessarily, we may suppose, give to the two forms very different optical properties: but

* Communicated by the Author.

† These phænomena are also detailed, and an explanation attempted, in a paper by Sir David Brewster on another subject, inserted in the Philosophical Transactions for 1830, part i. pp. 93, 94.

here the form of all the layers is the same; so that this mode of accounting for the appearances must also be rejected. Sir David Brewster indeed, speaking of the form of the crystal at the neutral line or line of no double refraction, says: "At this period the form of the crystal would be a cube:" but he speaks here only in reference to the absence of the doubly refracting structure which in a single atom or group of atoms of a simple substance would indicate a cube; but which, in *a mass*, such as the smallest of these layers, is quite compatible with a congeries of rhomboids.

This transition from a positive to a negative state of double refraction may be explained on the principle that the molecules of two substances possessed of opposite optical properties may neutralize each other if the relative positions of the optical axes and the number of molecules of each be rightly adjusted. Let the proper material of a crystal be possessed of a positive double refraction, such as the nucleus in the present case indicates; and let a second substance having a negative index deposit itself during crystallization, either in the form of distinct layers or mixed with the molecules of the first as fluids mix, forming only part of the mass of such layers; let also the quantity so mixed increase, or the distance of the layers decrease, as we depart from the central nucleus, and let the optical axes of the two substances be parallel; we shall have a crystal possessed of properties analogous to those observed in chabasie. The positive energy of the nucleus will gradually decrease till it disappear in a neutral line, and from this neutral line the refraction will again increase with a negative sign.

But the second substance must not only be negatively double-refractive, but also isomorphous with the first or capable of replacing it during crystallization without affecting the form of the crystal. Now among the substances contained by chabasie in large quantity, is silica; and this substance is not only negatively double-refractive, but appears also to be isomorphous with chabasie. The form of this mineral as determined by Mr. William Phillips from cleavage planes, is an obtuse rhomboid of 94° 46′; while that of quartz is a similar rhomboid of 94° 15′. These two forms are as nearly identical as isomorphous bodies generally present themselves in nature: we may therefore consider them as capable of replacing each other.

Suppose then that during the crystallization of the chabasie a slight excess of silica is present in the solution and deposits itself in occasional layers or otherwise as above described, and that the quantity so deposited increases with the size of the crystal; the form of the crystal, its transparency, apparent homogeneity, &c. will be unaffected; its optical properties alone will present a change, and one exactly such as we are considering*.

But if silica be isomorphous with pure chabasie, and be the cause of the phænomena in question, the quantity found in the mineral should be in some degree variable. Granting the positive nucleus of pure chabasie to have a fixed composition associated with a constant form, analysis ought to show that the quantity of silica present in some crystals is greater than in others. Now chemists have recognised two varieties of chabasie; one from Aussig in Bohemia, and from Fassa, analysed by Hoffman, and from Faroe analysed by Arfvredson; represented by the formula,

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

and another from Nova Scotia, analysed by Hoffman, from Gustafsberg, by Berzelius, and from Kilmacolm, by Mr. Connell, represented by the formula,

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

The only difference between these two formulæ is that the latter contains one half more silica in the first member than the former does. This circumstance is entirely in accordance with the view presented in this paper. If the first of the two formulæ be the correct one, the presence of the additional silica in the second should produce a perceptible change in the optical properties. It is impossible to say, however, that even in that formula the quantity of silica indicated may not be greater than the pure mineral may contain. The fact that silica is isomorphous with chabasie renders any formula for this substance exceedingly doubtful. Crystals from the same locality may not always contain the same quantity of silica; it is probably the paucity of analyses only that prevents us from knowing of crystals from one and the same locality to which each formula would apply. Sir David Brewster has not stated from what locality his crystals were obtained †; but the analysis of the actual specimen, and not the locality, would be required to enable us to say which formula would apply to it. Were they

* In the paper already referred to, Phil. Trans. 1830, p. 94, Sir David Brewster supposes the change to be due to the presence of a foreign body.

† One of the specimens I have since learned is from the Giant's Causeway.

even proved to belong to one or other of them, an analysis of the successive layers would still be wanting to demonstrate the absolute reality of the cause above explained.

Whether or not the explanation of the properties in question I have here given, be esteemed correct, there are inquiries of no little interest which spring out of the considerations above suggested. It would be important to know how far the presence of one substance in a compound body in greater or less quantity, the others remaining constant, would affect the optical properties; how far those of the compound may be affected by those of each of its constituents. In regard to form we might in the case of chabasie reason backwards, and inquire how far the whole of the silica might be rejected without change of form; whether $\dot{K} + \dddot{A}L + 6\dot{H}$ should be isomorphous with silica.

I have purposely abstained in the former part of this paper from adverting to the form of alumina, or to the possibility of its having a share in the production of the phænomena to be explained; but if it be isomorphous with silica, then ought $\dot{K} + 6\dot{H}$ to be isomorphous with both and with chabasie. I am not aware of the existence of any hydrated oxide in a crystalline form with six atoms of water; so that we are unable as yet to say how far such an analysis of the forms of crystallized substances containing one or more constituents isomorphous with themselves, may with advantage be pursued.

Note.—The above paper was communicated to the British Association at the Meeting in Dublin in August 1835*. I am now enabled to adduce two other observations which render still more probable the explanation above suggested. For the first I am indebted to Sir David Brewster, who informs me that a specimen of chabasie he has examined *from Faroe*, has a uniform doubly refracting structure throughout its whole mass. Now the Faroe chabasie, if that analysed by Arfvredson is to be considered a type of the whole, belongs to that kind which contains less silica, and throughout which there being supposed no excess of this latter substance above what belongs to the constitution of the mineral, no difference of doubly refracting structure of the kind above adverted to ought to be observable.

2. Among the interesting observations of Biot on the power of certain liquids to cause the plane of polarization to turn to the right or left †, is the remarkable one, that the oils of lemons

[* See Lond. and Edinb. Phil. Mag., vol. vii. p. 399.—Edit.]

[† Translations of the Memoirs by M. Biot in which these observations are detailed, will appear in Part II. and succeeding parts of the *Scientific Memoirs*.—Edit.]

and turpentine produce effects represented by opposite signs, the rotation produced by oil of lemons being +, that of oil of turpentine − and for a given thickness of oil only half that of oil of lemons. The effect produced by one thickness of oil of lemons is almost exactly neutralized by two thicknesses or twice the weight of oil of turpentine: and this whether the two liquids be in separate tubes, *or be previously mixed and presented to the ray of light in one and the same tube.* This is precisely analogous to what, in the above paper, we suppose to take place between the molecules of quartz and those of chabasie.

Portobello, July 22, 1836.

XXXVI. *Observations relative to the preceding Paper. By* Sir David Brewster.

IN examining the optical properties of the different chabasies, I found that the variety from the Giant's Causeway differed so essentially from the ordinary kinds as to entitle it to the distinction of a new mineral. Its *double refraction* was considerably *greater*, and its *ordinary refraction* considerably *less* than that of the common kind. I have since had occasion to examine a most interesting variety of chabasie brought from Faroe, and presented to me by W. C. Trevelyan, Esq. It consists of minute and perfect rhombs sticking loosely together, as it were in stalactites, and these minute crystals are perfectly transparent and much better crystallized than any other specimens which I have seen. Each rhomb, however, was a composite crystal, and the faces of composition coincided with the diagonals of its rhomboidal faces.

The remarkable property which Mr. Johnston has referred to in the preceding ingenious paper, does not exist in these minute crystals from Faroe, and I did not observe it in the specimens from the Giant's Causeway. If it exists, therefore, only in the common chabasie, it will not be difficult to put Mr. Johnston's hypothesis to the test of direct experiment, because this chabasie must, if the hypothesis be true, contain a greater quantity of silex than the other varieties; in order, however, to establish the hypothesis it must also be proved that the outer layers of the rhomb contain more silex than the inner rhomb or nucleus; and if the additional quantity of silex is very small, it may exist as an extraneous ingredient, diminishing its double refraction, not by an opposite double refraction of its own, but merely by separating the particles of chabasie, and diminishing the force of aggregation on which the double refraction of the mineral has been supposed to depend*.

Edinburgh, July 23, 1836.

* See Phil. Trans. 1830, p. 93. [An abstract of the paper here referred to will be found in Phil. Mag. and Annals, N.S. vol. vii. p. 356.—Edit.]

XXXVII. *Reply to* Dr. Boase's "*Remarks on* Mr. Hopkins's '*Researches in Physical Geology*,'" *in the Number for July. By* W. Hopkins, *Esq., M.A., F.G.S., of St. Peter's College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to reply through the medium of your Journal to some remarks by Dr. Boase, contained in your last Number [for July], p. 4, on my memoir on Physical Geology, and to add some additional facts which I have recently had an opportunity of observing.

Dr. Boase appears in the first place to object to the hypothesis on which the whole of my investigations are founded, that of the simultaneous action of an elevatory force on the exterior crust of the globe throughout regions of considerable extent, because he conceives that extensive dislocations produced by such a force would probably be attended with enormous convulsions proportioned to the extent of the rupture. This objection rests entirely on an assumption as to the intensity of the elevatory force. Dr. Boase has not given any reason for supposing that *explosion* must probably accompany dislocation, and without some such reason it is certain that we have no right to make the assumption. It is obvious, in fact, that we can have no means whatever of judging of the intensity of the elevatory force except by the effect produced by it. For anything we can know of its nature independently of inference from observed phænomena, it might be insufficient to produce an earthquake or adequate to produce an almost universal volcano. It may be observed, however, that the extent of simultaneous dislocation would do more than anything else to counteract the explosive tendency of an expansive fluid, because the more extensive the dislocations the more rapidly would the force of expansion be diminished, and the more equable would be the effect on the whole mass. If, on the contrary, a small portion only of the mass should give way, the expansive force would be but little diminished, and its continued action on the yielding part would unquestionably produce much more violent effects on that part than if the mass had yielded generally.

Dr. Boase has rested his objection partly also on the notion, that according to my views the fissures must necessarily begin at the under or *lowest* side of the elevated mass. He will find it carefully stated, however, in my memoir, that they "will not commence at the surface but at *some lower part* of the mass. If the extensibility of the lower part of it be sufficiently increased by its higher temperature, the fissures will commence at some point between the upper and lower surfaces, and will be propagated both upwards and downwards, and may or may not, according to the degree of extension of the mass, reach either the upper or lower surface. This is a point of little consequence as far as regards Dr. Boase's objection above stated, but it may serve to account for the fact that eruptions of fluid matter in some cases have, and in others have not, accompanied dislocations and elevatory movements.

In my investigations I have spoken of *elevatory* forces, the idea of the higher portions of the earth's crust having been elevated being in general, perhaps, more familiar to us than that of the lower having been depressed. I would here observe, however, that so far as relates to the first production of fissures it is immaterial whether we suppose the mass to be bent upwards by a force beneath, or downwards by its own weight, provided the regions thus subsiding simultaneously be of the same extent as those which I have always spoken of as being simultaneously elevated. The secondary phænomena, however, resulting from the fissures produced by the upward, and by the downward movements respectively, would probably present certain characteristic differences; but I shall not now enter into any further investigation of them.

In the abstract (Art. V.) of my memoir which appeared in your Journal (vol. viii. p. 359), I have taken considerable pains to indicate the possible influence of a jointed structure existing in the elevated mass previously to its elevation, and how it might be ascertained *by observation*, whether or not this influence had been considerable. Dr. Boase, however, is disposed to arrive at the determination of this point by the shorter, but, in such matters, most unsatisfactory road of *à priori* reasoning. He observes: "If then solid rocks have necessarily a jointed structure, one of the data on which Mr. Hopkins's calculations are founded is invalidated, in as much as the elevating force can never have acted on a solid mass without the interference of this modifying circumstance." Now, in the first place I would observe that the process of solidification of all rocks must necessarily have been extremely slow, and that probably, therefore, all the modifications which they have undergone during that process must have been the gradual work of lengthened periods of time. It is impossible, then, to say what period might be necessary for any portion of the earth's crust to arrive at that state of its jointed structure which should produce any decided effect on the directions of its dislocations. In my investigations it is unnecessary to suppose any but the lowest degree of solidification in the elevated mass; and therefore it is manifestly quite inadmissible to assume that it could not be dislocated by an elevatory force before its jointed structure had become sufficiently developed to determine the directions of dislocation. Yet the only force which can possibly attach to the objection above quoted depends entirely on this assumption, which, in fact, involves the very point at issue, viz. whether the jointed structure of disturbed masses has been in great measure superinduced *previously* or *subsequently* to their elevation. It is not, however, by this kind of *à priori* reasoning, founded on what we are altogether ignorant of, that the merits of geological theories can be determined; and to attempt to do so is to depart from those principles of inductive philosophy which alone have enabled man to comprehend with clearness and precision so much that is beautiful and wonderful in the laws of nature. I have elsewhere stated that I have not entered into these discussions in the spirit of advocacy of preconceived opinions; and with respect to the two theories, of which one would assign the directions of dislocation principally to the manner in which the elevating force has acted, and the other to the previously jointed state of the mass, I have endeavoured to act with perfect impartiality. I have indicated how their relative claims may probably be decided *by observation*, by which alone, I assert, these claims can be determined, and not by the kind of reasoning on which both the objections above noticed are founded.

With a view to this determination I have lately made some careful observations in the limestone and gritstone district of Derbyshire. In a particular and thick mass of limestone which pervades the greater part of that mining district, the joints are remarkably well developed. They form two systems at right angles (or very nearly so) to each other, which preserve their directions with remarkable accuracy in every part of the district. The other beds also have their principal joints in the same directions wherever they can be distinctly recognised; and such also is the case with the immense mass of gritstone superincumbent on the shale and limestone. One of these directions is a little west of the magnetic north; the other being consequently a little north of magnetic east, while the directions of all the characteristic dislocations of the district are nearly east and west and north and south, thus deviating from those of the joints by an angle of from 20° to 30°, precisely of that magnitude which is too large to be possibly attributed to any error of observation, and too small to admit of the formation of the existing dislocations subsequently to the existence of the joints in the perfection in which they are developed at present.

I have also observed another fact in several places both in this limestone district and the neighbouring coal district, of great importance with respect to these theories, viz. that deviation, in several instances, from the regular parallel directions of the longitudinal and transverse fissures which, as I have shown in my memoir, might be expected to accompany certain *partial elevations*, such as I have observed them to be associated with in the districts above mentioned. These facts are strikingly in accordance with the theory I have investigated, and as directly opposed to that which would assign these dislocations to the previous structure of the mass.

These observations agree also with those of Professor Phillips as recently given in his Geology of Yorkshire. The principal joints in that district have the same direction as in Derbyshire, while the absence in general of any perfect coincidence of lines of dislocation and of joints, affords conclusive proof that the former cannot have been *principally* determined by the previous structure of the mass *.

Among the *dislocations* of Derbyshire I include the great characteristic mineral veins of the district. They may, in fact, as I have elsewhere stated, be regarded as small faults, and uniformly in the vicinity of larger ones. There are also mineral veins, some of which are as manifestly formed in open joints. These differ from the former in no respect except in being of much smaller dimensions. The *vein-stuff* is perfectly similar, and we are led, I think, almost necessarily, on inspection, to the conclusion that it must have been deposited in the same manner in both classes of veins. Now where veins are formed along lines of faults, it surely seems almost impossible not to conclude that it must have been by some subsequent deposition or segregation along the line of dislocation, and consequently, from what I have above advanced, I consider it highly probable that the veins in open joints were formed there in a similar manner after the formation of the joints had commenced, and probably during their gradual enlargement by the contraction of the mass or other causes. The probability that open fissures once existed in the places where these veins are now formed

* The author of the work above referred to has attributed, in his theoretical speculations, more influence to the jointed structure in determining lines of dislocation, than I conceive the facts alleged by him will justify. At the time that part of his work was written, I am not aware that any other physical cause of the laws of these phænomena had been carefully considered.

seems much increased by the fact that partial cavities are frequently found in them, more particularly where the vein is very wide. Large *pipe-veins** are usually filled with sparry substances amongst which the ore is disseminated; but it frequently happens, I believe, that considerable spaces are still left empty, strongly indicating that such was once the case with the whole space occupied by the vein; and it seems highly probable that the usual vertical veins of the district have been filled in the same manner as the pipe-veins, (whatever that process may have been,) because veins of the former kind always communicate with these latter (of which the miner considers them the *feeders*); and I am not aware that any characteristic difference has been observed between the substances which occupy these two descriptions of veins.

Of the formation of the ore of a mineral vein I pretend to offer no conjecture. It seems to present an equal chemical difficulty whatever theory we adopt; but that some process of infiltration might be sufficient for the supply of the vein-stuff is indicated, I think, by the great masses of stalactitic formations met with in some cases, and their comparatively rapid formation in others. The toadstone of Derbyshire also contains in many places numerous veins and insulated small globular portions of calcareous matter to the depth of perhaps 200 feet, the formation of which it seems almost impossible to conceive except by infiltration. To a similar process too may be ascribed, I conceive, the existence in the interior hollows of fossil shells of crystalline substances differing as much from the mass in which the shells are imbedded, as vein-stuff frequently differs from the containing rock. These and similar facts have appeared to me to prove at least the adequacy of the cause assigned to produce the effects which I am at present disposed to attribute to it. I pretend not to offer an opinion as to whether at some antecedent geological period any solvent more powerful than water may have assisted in the process of infiltration. It may, perhaps, not be deemed very impossible†.

[To be continued.]

* Pipe-veins are spaces (frequently of considerable extent) usually existing between the beds of limestone, and filled as mentioned above. The *ore* in these, as in the vertical or *rake vein*, often bears an extremely small proportion to the other substances occupying the vein.

† I am glad to find, from the note in Dr. Boase's communication (p. 9) that Mr. Fox has suggested this same notion about the formation of veins, because he has probably derived it from observation of the Cornish veins, and his opinion is likely to have (and most deservedly) far greater weight with Cornish geologists, than any views emanating from myself.

XXXVIII. *On the Conducting Power of certain Flames and of Heated Air for Electricity. By* THOMAS ANDREWS, *M.D.**

IN some recent memoirs on electricity it has been assumed that the discharge of electricity through flame depends simply upon the temperature to which the air in the flame is elevated. Thus Dr. Ritchie observes that "the flame of a blowpipe is a hollow cone containing highly rarefied air. The electric fluid will therefore glide along such a cone exactly as it does along the interior of a hollow cone of glass partially exhausted of air. We are therefore not to regard flame as a conductor of electricity in the ordinary sense of the term, when the only part it performs in the conduction is that of forming a partial vacuum †." These remarks refer to common electricity; but Mr. Faraday likewise assumes that the relations of flame and heated air to electricity of low tension are the same, in his excellent paper on the "Identity of Electricities derived from different Sources." In order to prove that the latter variety of electricity may be discharged by heated air in the same manner as the former, he performed the following experiment. Having attached fine platina wires to the poles of a galvanic battery of 20 pairs of plates, and brought their extremities very close to each other, but without touching, he found that where they were heated to bright redness by the application of the side of a spirit-lamp flame, the current was freely transmitted. On putting the ends of the wires very close by the side of and parallel to each other, but not touching, the effects were, perhaps, more readily obtained than before. "These effects," he continues, "not hitherto known or expected under this form, are only cases of the discharge which takes place through air between the charcoal terminations of the poles of a powerful (galvanic) battery when they are gradually separated after contact. Here the passage is through heated air, exactly as with common electricity ‡."

On the other hand the celebrated experiments of Erman, which were repeated and extended by Biot, are opposed to this simple view of the subject, and tend to prove that in the power of conducting electricity of feeble tension flames present some remarkable properties which are different in flames of different kinds, and cannot therefore be identical with those of heated air. The experiments of Erman, however, refer only to flames; and as the whole subject was involved in much obscurity, although connected with one of the most remark-

* Communicated by the Author.

† Philosophical Transactions for 1828, p. 376. [An abstract of Dr. Ritchie's paper was given in Phil. Mag. & Annals, N.S. vol. v. p. 223.—EDIT.]

‡ Ibid. 1832, pp. 26, 27. [or Lond. and Edinb. Phil. Mag., vol. iii. p. 165.]

able electrical properties that have yet been discovered, it appeared to be deserving of new investigation.

To detect the passage of an electrical current, the test which I employed in the following experiments was the solution of the iodide of potassium, the extreme delicacy of which was first, I believe, pointed out by Faraday*. A slip of bibulous paper moistened with this solution was placed on a platina plate, supported upon an insulating stand of glass. The negative pole of the battery was brought into contact with the platina plate, while a wire of platina attached to the positive pole rested on the moistened paper. The existence of the current was inferred from the deposition of iodine beneath the positive pole; and when no iodine was deposited I have described the current as being interrupted. By this expression it is only to be understood that if a current did pass it was too feeble to produce any sensible decomposition of the solution of iodide of potassium. The extreme delicacy of this test, and the precautions which must be employed in using it, will be evident from this simple experiment. If the positive pole of a battery composed of a single pair of zinc and platina plates be placed upon the paper of the decomposing apparatus, and the platina plate touched by the moistened finger, no effect will occur, provided the negative pole be insulated; but if that pole be brought into contact with the ground, a deposition of iodine will immediately take place beneath the positive pole. Here the feeble current produced by a single pair of plates after traversing a long extent of imperfect conductors, is still capable of being easily detected by the decomposition of the solution of iodide of potassium.

A battery of 20 pairs of plates charged with common pump, water was carefully insulated, the poles were terminated by platina wires which were introduced into the flame of a spirit lamp, and a decomposing apparatus was interposed in the course of the circuit. The passage of the current was proved by the deposition of iodine at the positive pole.

This experiment was varied by placing the wires in different positions in the flame, but the result was the same, even when they were at the distance of an inch and a half from each other. When very fine wires were employed and brought only into contact with the flame, the effect was diminished, although it was still distinct; but by substituting slips of platina foil for the wires, and augmenting the surfaces of contact, the effect was greatly increased.

A battery consisting of a single pair of plates of platina and amalgamated zinc charged with dilute sulphuric acid was now

[* See Lond. & Edinb. Phil. Mag., vol. iii. p. 255.—Edit.]

employed, and even the current produced by this simple voltaic arrangement was found to be capable of passing through the flame of alcohol, and of decomposing the solution of the iodide of potassium.

It is evident from these results that, although the experiment of Faraday which has been described is perfectly accurate, yet it involves some conditions which are not essential, and others which are unfavourable to its success. The conclusions that are derived from it will on this account require to be modified.

To ascertain whether other flames are capable of transmitting in a similar manner the electrical current, the same arrangement was adopted, and the larger battery charged with water again employed. The flames of coal gas, ether, hydrogen, and charcoal were tried, and the passage of the current through each of them was proved by the occurrence of decomposition. From the quantity of iodine deposited, the current appeared to pass with more facility through the flame of charcoal, and with less facility through the flame of coal gas than it passed through the flame of alcohol. As the flames were in very different states, this circumstance may not afford an exact method of determining the relative conducting powers of these flames; but even with a single pair of plates, so large a quantity of iodine was separated when the current passed through the flame of charcoal as to leave no doubt that its conducting power is greatly superior to that of the other flames which were examined.

The conducting power of the flame of charcoal was further illustrated by obtaining the other effects of electricity from a current passing through it. The poles of the battery of 20 pairs of plates, weakly charged with a mixture of dilute nitric and sulphuric acids, were introduced into the flame of a charcoal fire contained in a small furnace and urged by bellows. The diameter of the flame was about five inches, and the poles were two inches apart from each other, and one inch and a half from the sides of the furnace. The current that passed between the poles thus situated deflected strongly the needle of a galvanometer, rapidly decomposed water, and communicated a slight shock to the tongue. All these effects ceased when the flame was not in contact with the poles.

As the flame of charcoal evidently holds a high rank in the list of imperfect conductors, it became an object of interest to determine whether it might be substituted for the liquid in the cell of a voltaic arrangement; whether, in fact, it possesses the properties of an electrolyte. This did not appear to be the case; for on placing slips of platina and copper vertically opposite to each other in a charcoal flame, and connecting them either with a decomposing apparatus or a galvanometer, no evidence could be obtained of the existence of an electrical current, although the copper was rapidly oxidized.

It is well known that a wire of platina suspended above the flame of an Argand lamp will become heated to bright redness, showing that the air around it has reached at least as high a temperature. It was the conducting power of air heated to redness in this manner that I examined, from the facility of performing experiments upon it. Two platina wires were suspended from insulating supports above the flame of an Argand gas lamp, and connected with the poles of a battery of 20 pairs of plates on Wollaston's construction in vigorous action, but no iodine appeared in the decomposing apparatus. The same negative result was obtained, whether fine platina points approximated as closely as possible to each other, or broad slips of platina foil were employed as poles.

From this experiment it appears to follow that air simply heated to redness does not conduct the current of a battery of 20 pairs of plates, but the singular facts which are now to be described will not admit of so easy an explanation.

The negative pole of a battery of 25 pairs of plates charged with pump water was connected by metallic contact with the brass tube of an Argand gas lamp, at a distance from the orifices through which the gas issued, and a coil of platina wire suspended above but not touching the flame, was attached to the positive pole. When the flame was sufficiently powerful to heat the coil to redness, the current passed freely, although the coil was at least one inch distant from the flame. But when the direction of the current was reversed, the negative pole being connected with the heated coil, and the positive pole with the base of the lamp, the passage of the current could no longer be detected. In the former case the solution of iodide of potassium was decomposed in a few seconds; in the latter case no decomposition occurred, however long the contact was maintained, yet the direction of the current had alone been changed, the other conditions of the experiment remaining the same. Similar effects were obtained, when a piece of well-burned charcoal was substituted for the platina coil in the heated air. Nor were they different when a battery of 83 pairs of plates with double coppers, charged with a solution of common salt, was used. These experiments were frequently repeated, and every source of error carefully avoided.

This property of conducting and interrupting the same voltaic current when flowing in opposite directions is not peculiar to heated air. It also belongs to flames; but in consequence of their higher conducting power, feeble voltaic combinations must be employed to discover it.

One pole of a battery of a single pair of plates, immersed in dilute sulphuric acid, was connected with the brass tube of an Argand gas lamp, and the other pole was attached to a coil of platina wire which rested upon the top of the flame. When the latter pole was positive, the current passed; when negative, it was interrupted. The same battery being employed, one pole was brought into contact with the ignited charcoal of a charcoal fire, and the other with the flame; the current passed, whether the pole in the flame was positive or negative, but much more readily when it was positive.

In the action of the magneto-electrical machine, as it is now constructed, the direction of the current is reversed at every semi-revolution of the soft iron armature; and from this circumstance the elements of compound bodies that are decomposed by it cannot be obtained in a separate state. By substituting this machine for the galvanic battery, any difference in the transmission of two currents perfectly similar, but tending to move in opposite directions, could be observed without altering the arrangement of the apparatus; and thus I expected not only to verify in a striking manner the preceding results, but also to obtain from the magnet the effects of a continuous electrical current flowing in one direction.

The electricity of the machine I employed had sufficient tension to decompose water, to burn metallic leaves, and to cause considerable shocks; but it did not pass sensibly through heated air even when the most favourable arrangement was adopted. By substituting the flame of charcoal for heated air the peculiar property of flame in conducting voltaic electricity was found also to exist in the case of electricity obtained from the magnet.

The points by which the sparks are usually procured from the machine were replaced by a circular disk, and a copper wire was introduced into each of the cups of mercury in which the disks revolved. One of these was connected with a platina wire placed over a charcoal fire at such a distance, that when the fire was urged by bellows it became surrounded by the flame; the other wire had a platina termination, and rested on a slip of paper moistened with the solution of iodide of potassium. The circuit was completed by inserting one extremity of a wire of platina into the side of the furnace so as to bring it into contact with the charcoal, while the other extremity was placed upon the slip of moistened paper. By this arrangement the currents developed by the rotation of the machine would be obliged to pass downwards through the flame to the charcoal and in the reverse direction; and, if equally transmitted, iodine would appear beneath each of the wires placed on the bibulous paper, as actually happens when the circuit is completed by a metallic communication.

On urging the fire till the flame reached the upper pole, and turning at the same time the machine with moderate rapidity, iodine was deposited at one of the wires which rested on the moistened paper, while there was not the slightest discolouration beneath the other wire. From the wire at which the iodine was deposited, it followed that the current was transmitted when the pole in the flame was positive, and interrupted when the same pole was negative. The direction in which the machine was turned did not produce any difference in the result; but by reversing the poles in contact with the flame and with the charcoal, iodine was deposited at the opposite wire. Here, then, was the most distinct proof of a free path being afforded to an electrical current passing in one direction, while a current, differing only in the direction in which it tended to move, was interrupted.

When the machine was turned very rapidly, a slight deposition of iodine took place at the wire, where there was none when the machine was turned more slowly; and at the same time a great quantity of iodine was visible around the other wire. Both poles were now introduced into the charcoal flame, and the machine worked rapidly; iodine was deposited at both wires. When the poles were surrounded to the same extent by the flame, the deposition was apparently similar at each wire; but when one pole was made just to touch the flame, while the other was brought extensively into contact with it, although iodine still appeared at both wires, it was no longer in the same quantity, showing that the current passed more freely in one direction than in the other. When the pole which only touched the flame was positive, the current passed with more facility than when the same pole was negative.

That the current, whose effects disappeared or were diminished, was actually interrupted and not neutralized by an opposite current developed during the combustion, it was easy to prove by connecting wires of platina with the flame and ignited charcoal and completing the circuit through the solution of the iodide of potassium; but no decomposition occurred. The free electricity which Pouillet ascertained to be developed during the process of combustion exists in too small quantity to produce any chemical effects, and cannot therefore influence the results of these experiments.

It is difficult to discover a satisfactory explanation of this property of flame and heated air. That the same current, when moving in opposite directions, will overcome with a different degree of facility any obstacle in its path appears, so far as our present knowledge of this subject extends, to be a general law of electricity. For illustrations of this principle, I may refer to the phænomena presented by the discharge of electricity of high tension across air; to the interesting experiments of Davy, in which different effects were obtained in the discharge of a powerful battery by reversing the terminations of its poles; to those of Peltier on the alterations of the temperature of metallic junctions from the passage of feeble voltaic currents; and, finally, to the observations of Becquerel on the facility with which the positive electricity overcomes an obstacle when the two electricities are separated by the agency of heat in a closed metallic circuit.

But even assuming the accuracy of this principle, we have still to inquire whether its cause can be discovered in particular cases. The unipolar property of the flame of alcohol, which was discovered by Erman, and which Biot has explained with so much precision and accuracy, furnishes an explanation of some of the preceding results, and may, perhaps, be applied to them all. If heated air and the flames of charcoal and alcohol conduct with more facility the positive than the negative electricity, and if the surface of contact of each pole with the heated air or flame be different, it is evident that the current will find the greatest difficulty in passing when it is the negative pole whose contact is least. This conclusion agrees perfectly with one of the experiments which have been described on the flame of charcoal. But it is more difficult to apply the same explanation to the other experiments, unless we assume that the contacts of the flame of coal gas with the metallic aperture from which the gas issues, of the flame of charcoal with the ignited charcoal itself, and of the heated air above an Argand lamp with the flame, are more intimate and perfect than can be obtained between the flame or heated air and a platina wire introduced directly into them. It does not appear to be improbable that this is actually the case.

Although the general conclusions that follow from these experiments agree with those of Erman, yet when they are minutely compared, an apparent discordance will be observed to exist between them. According to Erman, when the poles of a pile charged with a solution of common salt are introduced into the flame of alcohol, the divergence of the leaves of electroscopes connected with each pole did not sensibly diminish, the flame in this case apparently insulating the current. That the insulation, however, was not perfect the experiments which have been before described clearly prove; but I was anxious to establish the same fact from an examination of the state of tension of the poles. To ascertain the tension of the poles I employed the gold-leaf electroscope of Bohnenberger, which presents peculiar advantages in experiments upon voltaic electricity. The pile usually employed consisted of 100 pairs of plates mounted with pump-water; they were arranged in two columns and carefully insulated.

Erman observed that if one pole of a voltaic pile be introduced into an alcohol flame, the other being insulated, and if the flame be touched by a wire in connexion with the ground, the tension of the pole which terminates in the flame will cease, while that of the insulated pole will increase. Here the flame conducts the electricity of the pole with which it is in contact. But when the insulation of the other pole was removed, I found that then the deviation of the leaf of the electroscope attached to the pole which had been introduced into the flame did not apparently diminish when the flame was connected with the ground. This does not arise from the flame insulating under these circumstances the electricity of the pole inserted into it, but from its conducting power being so feeble that it is incapable of removing the tension of that pole as rapidly as it is acquired. If the wire by which the flame is connected with the ground be insulated, and its free extremity placed on the cap of an electroscope, the deviation of the gold-leaf of the instrument will indicate the presence of the same kind of free electricity as that of the pole in the flame. On the same principle a feeble current may actually be conducted by the flame when the opposite poles of a battery are inserted into it without any sensible diminution of the tension of its poles. By obstructing the passage of the positive electricity, so as to reverse the second part of Erman's experiment, the accuracy of this explanation was established.

The surface of contact of the positive pole with the flame was rendered as small as possible by employing a very fine wire of platina, while on the negative side a coil of platina, exposing a far greater extent of surface, was suspended in the flame; but on introducing a wire in communication with the ground into the flame, the tension of the positive pole ceased, while that of the negative pole increased. Fluid conductors were then interposed on the positive side, but the result was the same. The following arrangement was next successfully adopted.

A platina wire was hermetically sealed into one end of a glass tube about $\frac{1}{25}$th of an inch in diameter and 8 inches long, which was filled with common alcohol, and another similar wire was inserted into its open extremity. The positive end of the pile being connected with the first wire, the second wire became the positive pole, so that the column of alcohol formed part of the circuit. When the positive pole was placed in the flame of a spirit-lamp, and the flame touched by a wire in connexion with the ground, the deviation of the leaf of the electroscope attached to that pole ceased, while that of the electroscope connected with the negative pole increased. This showed that the positive electricity was freely transmitted through both the flame and the column of alcohol. When both poles were inserted into the flame, their tensions, as indicated by the electroscopes, did not sensibly diminish; but when the flame was touched by a wire connected with the ground, the deviation of the leaf of the electroscope on the negative side diminished, while that of the electroscope on the positive side increased. The tendency appeared now no longer in favour of the positive but of the negative electricity, which proves that the flame always allowed a small quantity of the latter kind of electricity to pass, and did not perfectly insulate the poles of the voltaic pile.

Although not connected with the subject of this paper, I take the opportunity of observing that by employing a similar contrivance on the negative side the tendency of alkaline soap to conduct negative electricity may be apparently reversed; but for this purpose a column of alcohol $\frac{1}{25}$th of an inch in diameter and $\frac{1}{5}$th of an inch long is sufficient, while in the case of the flame of alcohol a similar column two or three inches long is required.

From the experiments which I have detailed, it is evident that the conducting power of flames for electricity cannot be explained by the diminished elasticity of the gaseous matter which they contain; nor does the conduction of a feeble current of electricity by the flame of alcohol appear to be a particular case of the discharge of a powerful battery between charcoal poles when separated after contact. The flame of alcohol conducts the electricity of a single pair of plates even when the poles are separated by a considerable distance, while with a battery of far greater power no sensible separation of the poles in air can be obtained without altogether interrupting the passage of the current. Electricity of feeble tension passes through flame, because flame is an imperfect conductor; but electricity of high tension forces a passage across heated air, because the particles of the air are unable to resist its powerful repulsive action. In the one case the presence of the flame is essential to the result, in the other case the air presents only an obstacle to be removed, and the experiment will succeed better in a vacuum.

It has only been on the authority of numerous and often-repeated experiments that I have thus ventured to dissent from the conclusion of the eminent philosopher by whose profound and varied researches the science of electricity has of late years been so much extended. Nor is the important question of the identity of common and voltaic electricity affected by the results which I have obtained. The similarity of the arch formed by the discharge of an electrical and galvanic battery between charcoal surfaces shows clearly that there is a perfect analogy in the passage of the voltaic and common electric currents across air.

Belfast, June 22, 1836.

XXXIX. *On a new Method of taking Deep Soundings in the Ocean. By* A Correspondent.*

THE tediousness which attends the taking soundings in the ocean by the common method with the deep-sea lead, and the impossibility of thus ascertaining depths beyond a certain very limited extent, has led to the invention of different instruments which can assist in the operation. These machines descend alone through the water, and on reaching the ground are freed from the weight that dragged them to the bottom; when, rising to the surface, they bring with them an exact account of the depth to which they have gone. It is not difficult to suspend the balance weight so that it shall be detached by the shock against the ground from the rest of the apparatus, and allow it, if itself buoyant, to regain the surface: nor is it less easy to adapt a rotator, whose vanes shall be moved by the current during the descent, and thus cause the revolution of the index which registers on a dial-plate the fathoms of depth. But the great difficulty is to give the machine that buoyancy requisite to bring it back however deep it may have sunk. Mr. Massey, who has practically applied this invention, used a hollow copper globe filled with air: this could not be made very strong consistently with the necessary lightness; and he found that the instrument, which answered well in a depth of two hundred fathoms, when let go in water of unknown deepness never appeared again. On repeating the experiment with a new machine inclosed in a net and attached to a line, by which it was drawn back from four hundred fathoms, it was evident

* Communicated by the Author. See on this subject Lond. and Edinb. Phil. Mag., vol. iii. pp. 82, 352.

that the globe had been unable to support a pressure eighty times that of the atmosphere, unbalanced by any resistance within; it had burst as if by an explosion. Another machine composed entirely of wood also disappeared; and it has been found that most of the solid substances of low specific gravity, such as every species of wood and even cork, however lightly they may float on the surface, will have the water forced into their pores, as well as the mass itself compressed, when exposed to such enormous pressure of the fluid, from which they return no longer capable of floating.

The only resource will therefore be to use some fluid equally incompressible with water, and of less specific gravity; among these oil is the most eligible, as the liquids possess no affinity for each other, and may be allowed to come in contact without any danger of admixture. This should be substituted for the air in the copper receiver, and an aperture made in the lower part to admit the water should a vacuum occur in the inside, but too small to allow the oil to escape if the apparatus be accidentally reverted. In case of any compression of the oil, the water will merely rise in the interior without any danger of fracturing the receiver, whose strength, and consequently weight, may be reduced merely to that necessary to the preservation of its form. The globular form is the best, as it has the greatest capacity with the least surface. The oils vary in their specific gravity from ·85 to more than 1·. If we take 9· as an average attainable degree of lightness, a buoy of one cubic foot capacity, or a sphere of one and a half foot in diameter, will have a floating power of six pounds, which it will retain in every depth unless the compression of the oil proceeds at a much greater ratio than that of sea-water, which doubles its mass, under its own weight, at a depth of ninety miles; but this can only be ascertained by experiment. Laplace estimated the mean depth of the Pacific Ocean at four, and that of the Atlantic at three, miles; this was deduced from his calculations relative to the tides, and he supposed that there was no very extensive variation from this average. At such a depth the quantity of air contained in a buoy of one foot capacity would be compressed into two inches and a half, and a hollow globe, such as Mr. Massey used, would sustain a pressure of more than four tons on each inch of surface.

It may not be impossible to fix a detonating ball so that it shall explode on emerging into the air, and thus announce the return, and indicate the position, of the instrument, which will be exposed to various currents at different depths, and may be floated to a distance, and in a direction, entirely unexpected.

It will be a long process to lay down the soundings of the wide ocean, if we ever possess the power; but when it is accomplished we can hardly yet tell to what discoveries it may lead. We may, probably, be enabled to calculate the gradual approach of what will be hereafter new continents and islands, and to trace the boundaries of those which have once existed; or we may discover in deep chasms and ranges of submarine mountains the causes of those currents which, in spite of the assistance of modern astronomy, are still occasionally so dangerous to the navigator. At all events it must be interesting to unveil the secrets of the abyss, no longer unfathomable, and to send our inanimate messengers to bring us the intelligence, and possibly some of the productions, of those regions where neither the eye of man nor the light of heaven has yet been able to penetrate. J. J.

XL. *On certain Statements relative to* Dr. Apjohn's *Hygrometrical Researches contained in* Dr. Hudson's *Papers inserted in Lond. and Edinb. Phil. Mag., vol.* vii. *and vol.* viii. *By* James Apjohn, *M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Ireland.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a paper by Dr. Hudson of this city, published in the Philosophical Magazine for October 1835, there are some statements relating to certain hygrometrical researches of mine which, though it be contrary to my original intention, I find myself, eventually, in some degree compelled to notice and contradict, and with this view have to request that you will give an early insertion in your Journal to the following brief detail of facts.

In November 1834, I had the honour of reading a memoir to the Royal Irish Academy, an abstract of which was published in the Number of the Philosophical Magazine for the following March. In this was investigated and explained the following expression, which includes the solution of the well-known dew-point problem:

$$f'' = f' - \frac{d}{87} \times \frac{p}{30} \, *.$$

Dr. Hudson, who was present at the reading of this paper,

* d is the difference of the temperature shown by a wet and dry thermometer, f' the force of vapour at the temperature indicated by the former, and f'' its force at the dew-point.

in a few days addressed to me a letter, which was followed by several others, with the professed object of having my opinion upon another method of inferring the dew-point from the indications of a wet and dry thermometer. This method is founded on the following proportion:

$$D : D - d :: f' : f'',$$

in which f', f'', and d have the same significations already assigned to them, while D represents the depression experienced by the wet thermometer in *dry* air of such a temperature that, both for it and the moist air, f' has the same value. To render this proportion applicable in practice to the determination of f'',—the elastic force of vapour at the dew-point,—D must obviously be known as well as d and f'; but Dr. Hudson shows that if D be determined experimentally for any one value of f', it may be deduced for any other value of f' by calculation*.

Upon this method of Dr. Hudson I wish here, *in limine*, to make two remarks. 1. It was proposed after a public exposition of my formula, and by one who was present when my paper was read. 2. It is unsusceptible of practical application without the experimental determination of some one value of D, a thing which has not been attempted by Dr. Hudson.

I shall now, however, go further, and demonstrate that the method proposed by Dr. Hudson is but my method in disguise, or rather but a particular case of my general expression. Thus, since, as I have shown, $f'' = f' - \frac{d}{87} \times \frac{p}{30}$, $d = 87\,(f' - f'') \times \frac{30}{p}$. And, if the air be perfectly dry, in which case $f'' = 0$, $d = 87 f' \times \frac{30}{p}$. Hence, calling the depression in the latter case D, we shall have

$$D : d :: 87 f' \times \frac{30}{p} : 87\,(f' - f'') \times \frac{30}{p},$$

or, when p is constant,

$$D : d :: f' : f' - f'', \text{ and } \textit{dividendo},$$

$$D : D - d :: f' : f'', \text{ the proportion of Dr. Hudson.}$$

This result I communicated to Dr. Hudson in my reply to one of his letters; but perceiving that it was true only on the hypothesis of f' having the same value in the dry and moist

* His *proportion* for this purpose is only exact on the hypothesis of the pressure being constant. The same may be said of his fundamental proportion, $D : D - d :: f' : f''$.

air, I subsequently informed him that, as is obviously the case, the ratio between the depression in dry and the difference between it and the depression in moist air is expressed *generally* by the following proportion:

$$D : D - d : : f^{-} : f^{-} - f' + f'',$$

f^{-} and f' representing the respective forces of vapour at the temperatures shown in each instance by the wet thermometer. I have now arrived at my first ground of complaint against Dr. Hudson. In a postscript to his paper in the Philosophical Magazine for October 1835, he represents me as communicating to him on the occasion just mentioned, the proportion $D : d : : f^{-} - f' + f''$, that is, he omits D from the second term, and then proceeds to comment upon an absurdity created by himself*.

As far then as relates to my first paper, I have merely to recapitulate the following facts. My formula is in itself complete, and immediately applicable in practice; Dr. Hudson's is useless without a knowledge of the value of D. My formula was communicated to the Royal Irish Academy, and explained in his presence a full year before the publication of *his* method. Lastly, his fundamental proportion is but a particular and very obvious case of my general expression, as has been already demonstrated.

If under such circumstances I were to maintain that Dr. Hudson borrowed his method from me, I think it will be admitted I should be able to establish a very probable case against him. Such a charge, however, I have never advanced either by the method of direct assertion or oblique insinuation, and I am even now prepared to admit that Dr. Hudson may have arrived at his proportions previous to any knowledge of my hygrometric formula. Has Dr. Hudson treated me with the same degree of candour? This point the reader will be better able to decide after perusing the following statement.

At a general meeting of the Royal Irish Academy, held in April 1835, I communicated a second paper†, the object of

* If I had to do with any other person I should have no hesitation in concluding that, writing in the hurry of business, and what I never dreamt would meet the public eye, I had myself been the author of the omission in question. The charge, however, I have preferred against Dr. Hudson I cannot abandon without replacing it by one of nearly equal gravity. Dr. Hudson has persisted in attempting to fasten upon me the proportion $D : d : : f^{-} : f^{-} - f' + f''$, and without the slightest allusion to the fact of my having complained of being misrepresented on this very point, and demonstrated step by step in his presence the manner of deducing the correct proportion from my own formula.

† This paper is to be found in the Numbers of the Philosophical Magazine for October, November, and December, 1835.

which was to detail a series of experiments, the results of which established, if I mistake not, the perfect accuracy of my formula for the dew-point. Now, in reference to these experiments Dr. Hudson, in a note to the paper of his already referred to, makes the following observations. "It may not be improper to state here that I communicated the views contained in this paper to my friend Dr. Apjohn two or three days after he communicated his method to the Royal Irish Academy, and I have recently had the pleasure of learning from him that he has employed both the methods of experimenting on dry air, and on air of a known dew-point (heated artificially), with the view of testing and establishing his own formula." It is here undoubtedly insinuated by Dr. Hudson, that I borrowed from him my methods of experiment; but as such was not directly affirmed, I did not at the time think it necessary to publish a formal contradiction. Since that, however, all doubts as to his real meaning have been removed; for both at the Royal Irish Academy, and subsequently in a pamphlet which he has recently circulated on the subject, he has preferred a distinct charge of plagiarism against me, and he has published in support of the charge, without even the formality of asking my permission, some letters of mine written in reply to those which I have already described myself as receiving from him. From this charge, and some others upon which I shall not comment here, as they have not appeared in the pages of any scientific journal, I have, I think I may venture to assert, fully vindicated myself in the judgement of the members of the Royal Irish Academy, and shall now proceed to set myself right with the readers of the Philosophical Magazine.

Having arrived at the solution of a physical question deemed of sufficient importance by the British Association at its meeting in York to be inserted in their list of *Desiderata* in science, it will, I presume, be readily admitted that I felt extremely anxious to test its truth by submitting it to the touchstone of experiment. I was, however, at the time of communicating my formula well aware, for reasons which I have elsewhere stated, that this could not be satisfactorily done by means of Daniell's instrument, and corresponding observations with a wet and dry thermometer; and felt therefore convinced, at a very early period, of the necessity of resorting to the other methods. Now *three such* are immediately pointed to by the formula itself. Thus, 1st, since $f'' = f' - \frac{d}{87} \times \frac{p}{30}$, if $f'' = 0$, *i. e.* if the air be destitute of vapour, $d = 87 f' \times \frac{30}{p}$;

so that if a value of d in perfectly dry air be determined by observation, theory and experiment can be compared.

2. Though f'' be supposed constant, f' and d will vary if the temperature varies. Hence if air having a constant hygrometrical state be raised to different temperatures, and a series of corresponding values of f' and d be determined by observation, the formula, if correct, when applied to each pair, should obviously give the same result, or value of f''.

3. If air be saturated with moisture at a known temperature, such temperature is necessarily its dew-point. Let it now be heated, and values of f' and d then observed in it by means of a wet and dry thermometer. The formula, if correct, when applied to these, should give for the dew-point the temperature at which the air was in the first instance saturated with moisture.

Such are the methods which I employed. They flow immediately from my formula, and I beg to state in the most positive manner that I had resolved to employ them *all* before Dr. Hudson commenced his correspondence with me. Dr. Hudson, however, states that the first and third are his exclusive property, and that I derived them from him. Now what is the evidence he adduces in support of this assertion? Not a tittle, that I can perceive, save a passage in one of my letters in which I recommend him before publishing his proportions relative to the dew-point, to investigate the value of D by some of the experimental methods mentioned by him in a previous letter. From this passage Dr. Hudson infers, by some process of reasoning with which I am unacquainted, but which certainly would not appear to be characterized by much of the *vis consequentiæ*, that I must have been ignorant of these methods before they were mentioned by him; that is, that I must have overlooked the most simple consequences of the expression I had investigated, and felt so anxious to verify by experiment. But it is not necessary to argue the matter further. The following letter from Professor Lloyd has, I presume, satisfied Dr. Hudson himself that he is in error, and that I contemplated my experiments on dry air before I had ever any intercourse with him, direct or indirect, on this or any other subject.

"My dear Apjohn.

"I very well recollect your mentioning on the evening on which you read your first paper on the moist-bulb thermometer to the Academy, that your hygrometric formula suggested an easy method of determining by experiment the specific heats of the gases, viz. by observing the temperatures indicated by the two thermometers in gases deprived of their moisture. I think you stated at the same time that it was your intention to engage in this investigation at some early opportunity; but of this circumstance my remembrance is not so distinct as of the other. Believe me always yours sincerely,

"Trinity College, June 13, 1836. "H. Lloyd."

"*Dr. Apjohn.*"

Having disposed of Dr. Hudson's claims of priority in relation to the suggestion of the hygrometrical experiments performed by me, I shall now briefly advert to another topic.

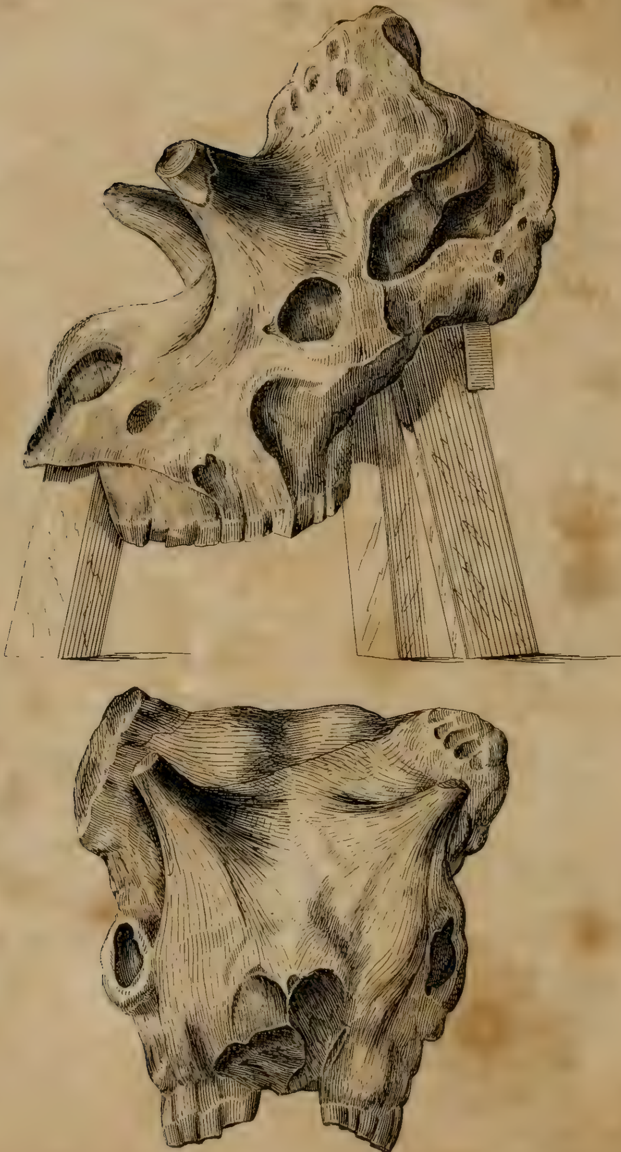
Almost immediately after having fallen on my hygrometric formula I resolved, should I succeed in verifying it by experiment, to apply it to the determination of the specific heats of the gaseous bodies. That it may be employed in such research is obvious, as the specific heat of the gas is a factor in the formula. In fact, my expression in its most extended form, so as to include the various gases, is $f'' = f' - \frac{48\,s\,a\,d}{e} \times \frac{p}{30}$, s being the specific gravity and a the specific heat of the gas, and e the caloric of elasticity of aqueous vapour whose elastic force is f'. From this it is easy to deduce $a = (f' - f'') \times \frac{e}{48\,s\,d} \times \frac{30}{p}$, an equation which, when $f'' = 0$, or the gas is dry, becomes $a = f' \times \frac{e}{48\,s\,d} \times \frac{30}{p}$. If then an observation with a wet and dry thermometer be made in a number of gases, their specific heats, that is, the value of a for each, may be calculated. Such is the principle of my method.

Upon this plan I have performed a number of experiments, and am, I may observe, still occupied with the subject. Having, however, on the Saturday previous to the meeting of the British Association in this city, arrived, with the aid of a more perfect apparatus than I had previously employed, at some, as far as I could judge, very satisfactory results, I set myself down, and in a very hurried manner submitted them to calculation, and gave to the Chemical Section not so much a formal paper on the subject as an account, chiefly verbal, of my method, and of the results to which it conducted, resolving of course to investigate the subject much more carefully, and to calculate much more at my leisure before I submitted my investigations, as I always contemplated doing, to the Royal Irish Academy. The conclusion really deducible from my experiments was, to a certain extent, a confirmation of the opinion which has of late been so much advocated, that all gases have under equal volumes the same capacity for caloric*.

* This idea, first started by Dalton, and since supported by the experi-

SIVATHERIUM

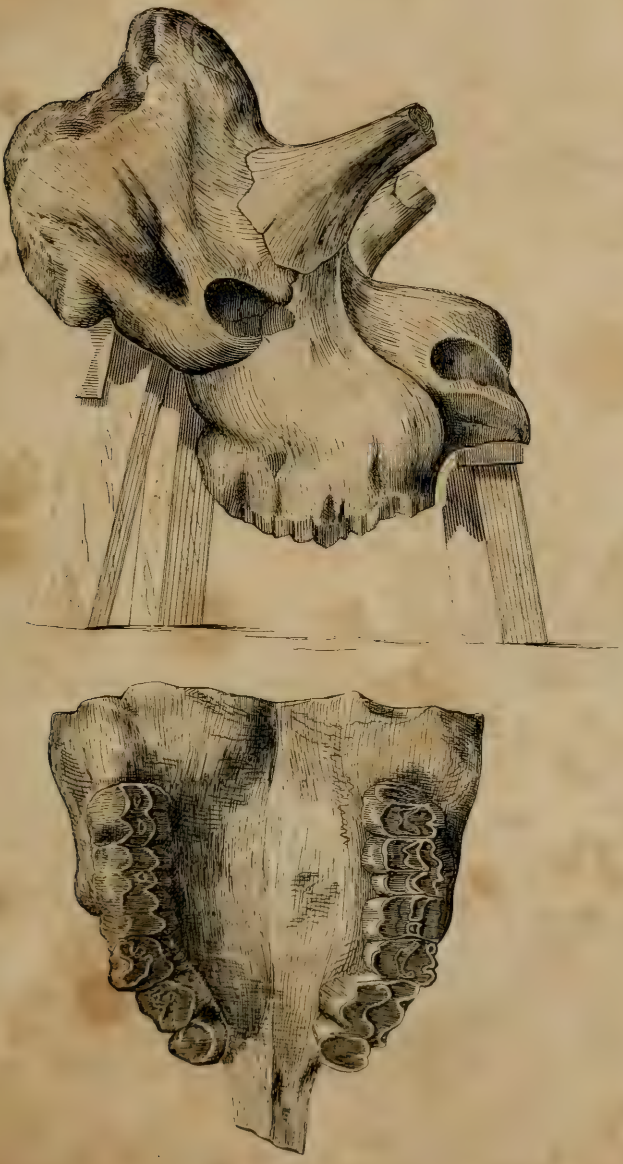
On a Scale of one-seventh



From an Etching by Mr. Prinsep after a Drawing by Capt. Cautley.

GIGANTEUM.

of the Original.



J. Basire lithog.

The conclusion, however, as stated by me was that they have all the same specific heats under equal weights; and this error I fell into from omitting, in my hurry, (most of my calculations were made on the day preceding the meeting of the Association,) to divide by the specific gravities, or, in other words, applying to all the formula for atmospheric air, in which the specific gravity being unity does not appear. Dr. Hudson, however, who would seem to have a passion for repeating my calculations, and a good deal of leisure, it is to be presumed, when he devotes himself to so uninteresting an occupation, observed this omission (a venial one I trust, and one which I hope I am not vain in thinking I should, on returning to the subject, have been able to remedy without Dr. Hudson's assistance,) and informed me of it. For this communication I felt thankful, and on a subsequent occasion, I believe, so expressed myself; but I was not I confess aware at the time that Dr. Hudson, after supplying the omission in question, had forwarded a paper to the Philosophical Magazine, entitled, "On an error in Dr. Apjohn's formula for inferring the specific heats of dry gases*." This course it was undoubtedly perfectly competent to Dr. Hudson to take, and I would not wish to be understood as complaining of it: however anxious to protect myself against misrepresentation, I feel no disposition to contest with him the credit which he may be conceived to have acquired from the proceeding under consideration.

28, Lower Baggott-Street, Dublin, July 5, 1836. JAMES APJOHN.

XLI. *On the* Sivatherium giganteum, *a new Fossil Ruminant Genus, from the Valley of the Markanda, in the Sivâlik branch of the Sub-Himâlayan Mountains. By* HUGH FALCONER, *M.D., Superintendent Botanical Garden, Sehâranpur, and Captain* P. T. CAUTLEY, *Superintendent Doâb Canal.*†

[With an Engraving: Plate II.]

THE fossil which we are about to describe forms a new accession to extinct Zoology. This circumstance alone would give much interest to it. But in addition, the large size, surpassing the rhinoceros, the family of Mammalia to which it belongs, and the forms of structure which it ex-

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ments of Dulong, Haycraft, Marcet and De la Rive, Dr. Hudson seems to speak of as originating with himself. "Accordingly the experiments, except with hydrogen, rather favour my view that the capacities of gases are equal in equal volumes." (See Phil. Mag. for January 1836, p. 22.)

* [If we remember right, Dr. Hudson's paper as we received it had no title, that which it now bears, as above, having been prefixed to it by us.—EDIT.]

† From the *Asiatic Researches*: Transactions of the Physical Class of the Asiatic Society of Bengal, Part iii.

hibits, render the *Sivatherium* one of the most remarkable of the past tenants of the globe that have hitherto been detected in the more recent strata.

Of the numerous fossil mammiferous genera discovered and established by Cuvier, all were confined to the *Pachydermata*. The species belonging to other families have all their living representatives on the earth. Among the *Ruminantia*, no remarkable deviation from existiug types has hitherto been discovered, the fossil being closely allied to living species. The isolated position, however, of the Giraffe and the *Camelidæ*, made it probable, that certain genera had become extinct, which formed the connecting links between those and the other genera of the family, and further between the *Ruminantia* and the *Pachydermata*. In the *Sivatherium** we have a ruminant of this description connecting the family with the *Pachydermata*, and at the same time so marked by individual peculiarities as to be without an analogue in its order.

The fossil remain of the *Sivatherium*, from which our description is taken, is a remarkably perfect head. When discovered, it was fortunately so completely enveloped by a mass of stone, that although it had long been exposed to be acted upon as a boulder in a water-course, all the more important parts of structure had been preserved. The block might have been passed over, but for an edging of the teeth in relief from it, which gave promise of something additional concealed. After much labour, the hard crystalline covering of stone was so successfully removed, that the huge head now stands out with a couple of horns between the orbits, broken only near their tips, and the nasal bones projected in a free arch, high above the chaffron. All the molars on both sides of the jaw are present and singularly perfect. The only mutilation is at the vertex of the cranium, where the plane of the occipital meets that of the brow; and at the muzzle, which is truncated a little way in front of the first molar. The only parts which are still concealed, are a portion of the occipital, the zygomatic fossæ on both sides, and the base of the cranium over the sphenoid bone.

The form of the head is so singular and grotesque, that the first glance at it strikes one with surprise. The prominent features are—1st, the great size, approaching that of the elephant: 2nd, the immense development and width of the cranium behind the orbits: 3rd, the two divergent osseous cores for horns starting out from the brow, between the orbits: 4th, the form and direction of the nasal bones, rising with great prominence out of the chaffron, and overhanging the external nostrils in a pointed arch: 5th, the great massiveness, width and shortness of the face forward from the orbits: 6th, the great angle at which the grinding plane of the molars deviates upwards from that of the base of the skull.

* We have named the fossil, *Sivatherium*, from *Siva*, the Hindú God, and *θηρίον bellua*. The *Sivâlik* or Sub-Himálayan range of hills, is considered in the Hindu mythology, as the *Lútiah* or edge of the roof of Siva's dwelling in the *Himálaya*, and hence they are called the *Siva-ala* or *Sib-ala*, which by an easy transition of sound became the *Sewálik* of the English. The fossil has been discovered in a tract which may be included in the *Sewálik* range, and we have given the name of *Sivatherium* to it, to commemorate this remarkable formation, so rich in new animals. Another derivation of the name of the hills, as explained by the *Mahant* or High Priest at *Dehra*, is as follows:

Sewálik, a corruption of *Siva-wála*, a name given to the tract of mountains between the Jumna and Ganges, from having been the residence of Iswara Siva and his son Gane's, who under the form of an elephant had charge of the Westerly portion from the village of *Dúdhli* to the Jumna, which portion is also called *Gangaja*, *gaja* being in Hindí an elephant. That portion eastward from *Dúdhli*, or between that village and *Haridwár*, is called *Deodhar*, from its being the especial residence of *Deota* or Iswara Siva: the whole tract however between the Jumna and Ganges is called *Siva-ala*, or the habitation of Siva; unde der. *Sewálik*.

Viewed in lateral profile, the form and direction of the horns, and the rise and sweep in the bones of the nose, give a character to the head widely differing from that of any other animal. The nose looks something like that of the rhinoceros; but the resemblance is deceptive, and only owing to the muzzle being truncated. Seen from in front, the head is somewhat wedge-shaped, the greatest width being at the vertex and thence gradually compressed towards the muzzle; with contraction only at two points behind the orbits and under the molars. The zygomatic arches are almost concealed and nowise prominent: the brow is broad, and flat, and swelling laterally into two convexities; the orbits are wide apart, and have the appearance of being thrown far forward, from the great production of the frontal upwards. There are no crest or ridges: the surface of the cranium is smooth, the lines are in curves, with no angularity. From the vertex to the root of the nose, the plane of the brow is in a straight line, with a slight rise between the horns. The accompanying drawings will at once give a better idea of the form than any description.

Now in detail of individual parts; and to commence with the most important and characteristic, the teeth*:

There are six molars on either side of the upper jaw. The third of the series, or last milk molar, has given place to the corresponding permanent tooth, the detrition of which and of the last molar is well advanced, and indicates the animal to have been more than adult.

The teeth are in every respect those of a ruminant, with some slight individual peculiarities.

The three posterior or double molars are composed of two portions or semicylinders, each of which incloses, when partially worn down, a double crescent of enamel, the convexity of which is turned inwards. The last molar, as is normal in ruminants, has no additional complication, like that in the corresponding tooth of the lower jaw. The plane of grinding, slopes from the outer margin inwards. The general form is exactly that of an ox or camel, on a large scale. The ridges of enamel are unequally in relief, and the hollows between them unequally scooped. Each semicylinder has its outer surface, in horizontal section, formed of three salient knuckles, with two intermediate sinuses; and its inner surface, of a simple arch or curve. But there are certain peculiarities by which the teeth differ from those of other ruminants.

In correspondence with the shortness of jaw, the width of the teeth is much greater in proportion to the length than is usual in the family: the width of the third and fourth molars being to the length as 2·24 and 2·2 to 1·55 and 1·68 inches, respectively: and the average width of the whole series being to the length as 2·13 to 1·76 inches. Their form is less prismatic: the base of the shaft swelling out into a bulge or collar, from which the inner surface slopes outward as it rises: so that the coronal becomes somewhat contracted: in the third molar, the width at the coronal is 1·93, at the bulge of the shaft 2·24. The ridges and hollows on the outer surface descend less upon the shaft, and disappear upon the bulge. There are no accessory pillars on the furrow of junction at the inner side. The crescentic plates of enamel have a character which distinguishes them from all known ruminants: the inner crescent, instead of sweeping in a nearly simple curve, runs zig-zag-wise in large sinuous flexures, somewhat resembling the form in the *Elasmotherium*.

The three double molars differ from each other only in their relative states of wearing. The antepenultimate, being most worn, has the crescentic plates less curved, more approximate and less distinct: the penultimate and last molars are less worn, and have the markings more distinct.

* The figure of the palate and teeth in the Engraving is on rather a larger scale than the rest.—Edit. Asiat. Res.

The three anterior or simple molars have the usual form which holds in *Ruminantia*, a single semicylinder, with but one pair of crescents. The first one is much worn and partly mutilated: the second is more entire, having been a shorter time in use, and finely exhibits the flexuous curves in the sweep of the enamel of the inner crescent; the last one has the simple form of the permanent tooth, which replaces the last milk molar: it also shows the wavy form of the enamel.

Regarding the position of the teeth in the jaw; the last four molars, viz. the three permanent and the last of replacement, run in a straight line, and on the opposite sides are parallel and equi-distant: the two anterior ones are suddenly directed inwards, so as to be a good deal approximated. If the two first molars were not thus inflected, the opposite lines of teeth would form exactly two sides of a square: the length of the line of teeth, and the intervals between the outer surfaces of the four last molars, being almost equal, viz. 9·8 and 9·9 inches respectively.

The plane of detrition of the whole series of molars from rear to front is not horizontal, but in a slight curve, and directed upwards at a considerable angle with the base of the skull: so that when the head is placed so as to rest upon the occipital condyles and the last molars, a plane through these points is cut by a chord along the curve of detrition of the whole series of molars at an angle of about 45°. This is one of the marked characters about the head:

Dimensions of the Teeth.

	Length. Inches.	Breadth. Inches.
Last molar right side	——	2·35
Penultimate do.	2·20	2·38
Antepenultimate do.	1·68	2·20
Last simple molar	1·55	2·24
Second do. do.	1·70	1·95
First do. do.	1·70	1·90
	Outer Surfaces.	Inner Surfaces.
Interval between the surfaces of last molar	9·9	5·5
Do. do. do. third molar	9·8	5·5
Do. do. do. second do.	8·4	4·5
Do. do. do. first do.	6·4	3·2

Space occupied by the line of molars 9·8 inches.

Bones of the Head and Face.—From the age of the animal to which the head had belonged, the bones had become anchylosed at their commissures, so that every trace of suture has disappeared, and their limits and connexions are not distinguishable.

The frontal is broad and flat, and slightly concave at its upper half. It expands laterally into two considerable swellings at the vertex, and sweeps down to join the temporals in an ample curve; and with no angularity. It becomes narrower forwards, to behind the orbits; and then expands again in sending off an apophysis to join with the malar bone, and complete the posterior circuit of the orbit. The width of the bone where narrowest, behind the orbit, is very great, being 16·2 inches. Partly between and partly to the rear of the orbits, there arise by a broad base, passing insensibly into the frontal, two short thick conical processes. They taper rapidly to a point, a little way below which they are mutilated in the fossil. They start so erect from the brow, that their axis is perpendicular to their basement: and they diverge at a considerable angle. From their base upwards they are free from any rugosities, their surface being smooth and even. They are evidently the osseous cores of two intra-orbital horns. From their position and size they form one of the most remarkable features in the head. The connexions of the frontal are nowhere distinguishable, no mark of a suture remaining. At the upper end of the bone the skull is fractured, and the structure of the bone is exposed. The internal and outer plates are seen to be widely separated, and the interval to be occupied by large shells, formed by an expansion of the diploe into plates, as in the elephant. The interval exceeds $2\frac{1}{2}$ inches in the occipital. On the left side of the frontal, the swelling at the vertex has its upper lamina of bone removed, and the cast of the cells exhibits a surface of almond-shaped or oblong eminences, with smooth hollows between.

The temporal is greatly concealed by a quantity of the stony matrix, which has not been removed from the temporal fossa. No trace of the squamous suture remains to mark its limits and connexion with the frontal. The inferior processes of the bone about the auditory foramen have been destroyed, or are concealed by stone. The zygomatic process is long, and runs forward to join the corresponding apophysis of the jugal bone, with little prominence or convexity. A line produced along it would pass in front, through the tuberosities of the maxillaries, and to the rear along the upper margin of the occipital condyles. The process is stout and thick. The temporal fossa is very long, and rather shallow. It does not rise up high on the side of the cranium : it is overarched by the cylinder-like sides of the frontal bone. The position and form of the articulating surface with the lower jaw are concealed by stone which has not been removed.

There is nothing in the fossil to enable us to determine the form and limits of the parietal bones; the cranium being chiefly mutilated in the region which they occupy. But they appear to have had the same form and character as in the ox; to have been intimately united with the occipitals, and to have joined with the frontal at the upper angle of the skull.

The form and characters of the occipital are very marked. It occupies a large space, having width proportioned to that of the frontal, and considerable height. It is expanded laterally into two alæ, which commence at the upper margin of the foramen magnum, and proceed upwards and outwards. These alæ are smooth, and are hollowed out downwards and outwards from near the condyles towards the mastoid region of the temporal. Their inner or axine margins proceed in a ridge arising from the border of the occipital foramen, diverging from each other nearly at right angles, and inclose a large triangular fossa into which they descend abruptly. This fossa is chiefly occupied by stone in the fossil, but it does not appear shallow, and seems a modification of the same structure as in the elephant. There is no appearance of an occipital crest or protuberance. The bone is mutilated at the sides towards the junction with the temporals. Both here and at its upper fractured margin its structure is seen to be formed of large cells with the diploe expanded into plates, and the outer and inner laminæ wide apart. This character is very marked at its upper margin, where its cells appear to join on with those of the frontal. The condyles are very large, and fortunately very perfect in the fossil; the longest diameter of each is 4·4 inches, and the distance measured across the foramen magnum, from their outer angles, is 7·4 inches: dimensions exceeding those of the elephant. Their form is exactly as in the *Ruminantia*, viz. their outer surface composed of two convexities meeting at a rounded angle: one in the line of the long axis, stretching obliquely backwards from the anterior border of the foramen magnum; on the other forwards and upwards from the posterior margin, their line of commissure being in the direction of the transverse diameter of the foramen. The latter is also of large size, its antero-posterior diameter being 2·3 inches, and the transverse diameter 2·6 inches. The large dimensions of the foramen and condyles must entail a corresponding development in the vertebræ, and modify the form of the neck and anterior extremities.

The sphenoidal bone, and all the parts along the base of the skull from the occipital foramen to the palate, are either removed, or so concealed by stone, as to give no characters for description.

The part of the brow from which the nasal bones commence is not distinguishable. The suture connecting them with the frontal is completely obliterated: and it is not seen whether they run up into a sinus in that bone, or how they join on with it. Between the horns there is a rise in the brow, which sinks again a little forward. A short way in advance of a line connecting the anterior angles of the orbits, there is another rise in the brow. From this point, which may be considered their base, the nasal bones commence ascending from the plane of the brow, at a considerable angle. They are broad and well arched at their base, and proceed forward with a convex outline, getting rapidly narrower, to terminate in a point curved downwards, which overhangs the external nostrils. For a considerable part of their length they are joined to the maxillaries: but forwards from the point where they commence narrowing, their lower edge is free and separated from the maxillaries by a wide sinus: so that viewed in lateral profile their form very much resembles the upper mandible of a hawk, detached from the lower. Unluckily, in the fossil, the anterior margins of the maxillaries are mutilated, so that the exact length of the nasal bone that was free from connexion with them cannot be determined. As the fossil stands, about four inches of the lower edge of the nasals, measured along the curve, are free. The same mutilation prevents its being seen how near the incisives approached the nasals, with which they do not appear to have been joined. This point is one of great importance, from the structure it implies in the soft parts about the nose. The height and form of the nasal bones are the most remarkable feature in the head: viewed from above they are seen to taper rapidly from a broad base to a sharp point; and the vertical height of their most convex part above the brow at their base, is 3½ inches.

The form of the maxillaries is strongly marked in two respects: 1st, their shortness compared with their great width and depth: 2nd, in the upward direction of the line of alveoli from the last molar forwards, giving the appearance (with the licence of language intended to convey an idea of resemblance without implying more) as if the face had been pushed upwards to correspond with the rise in the nasals; or fixed on at an angle with the base of the cranium. The tendency to shortness of the jaw was observed in the dimensions of the teeth, the molars being compressed, and their width exceeding their length to an extent not usual in the *Ruminantia*. The width apart, between the maxillaries, was noticed before; the interval, between the outer surfaces of the alveoli, equalling the space in length occupied by the line of molars. The cheek tuberosities are very large and prominent, their diameter at the base being 2 inches, and the width of the jaw over them being 12·2 inches, whereas at the alveoli it is but 9·8 inches. They are situated over the third and fourth molars; and proceeding up from them towards the malar, there is an indistinct ridge on the bone. The infra-orbitary foramen is of large size, its vertical diameter being 1·2 inch: it is placed over the first molar, as in the ox and deer tribe. The muzzle portion of the bone is broken off at about 2·8 inches from the 1st molar, from the alveolar margin of which, to the surface of the diastema, there is an abrupt sink of 1·7 inch. The muzzle is here contracted to 5·8 inches, and forwards at the truncated part to about 4·1. The palatine arch is convex from rear to front, and concave across. No trace of the palatine foramina remains, nor of the suture with the proper palatine bones. The spheno-palatine apophyses and all back to the foramen magnum * are either removed or concealed in stone. In front, the mutilation of the bone, at the muzzle, does not allow it to be seen how the incisive bones were connected with the maxillaries: but it appears that they did not reach so high on the maxillaries as the

* With the exception of a portion of the basilary region, which resembles that of the Ruminants.

union of the latter with the nasals. The same cause has rendered obscure the connexions of the maxillaries with the nasals, and the depth and size of the nasal echancrure or sinus.

The jugal bone is deep, massive and rather prominent. Its lower border falls off abruptly in a hollow descending on the maxillaries; the upper enters largely into the formation of the orbit. The posterior orbital process unites with a corresponding apophysis of the frontal, to complete the circuit of the orbit behind. The zygomatic apophysis is stout and thick and rather flat. No part of the arch, either in the temporal or jugal portions, is prominent: the interval between the most salient points being greatly less than the hind part of the cranium, and slightly less than the width between the bodies of the jugals.

The extent and form of the lachrymals, cannot be made out, as there is no trace of a suture remaining. Upon the fossil, the surface of the lachrymary region passes smoothly into that of the adjoining bones. There is no perforation of the lower and anterior margin of the orbit by lachrymary foramina, nor any hollow below it indicating an infraorbital or lachrymary sinus. It may be also added, what was omitted before, that there is no trace of a superciliary foramen upon the frontal.

The orbits are placed far forwards, in consequence of the great production of the cranium upwards, and the shortness of the bones of the face. Their position is also rather low, their centre being about 3·6 inches below the plane of the brow. From a little injury done in chiseling off the stone, the form or circle of the different orbits does not exactly correspond. In the one of the left side, which is the more perfect, the long axis makes a small angle with that of the plane of the brow: the antero-posterior diameter is 3·3 inches, and the vertical 2·7 inches. There is no prominence or inequality in the rim of the orbits, as in the *Ruminantia*. The plane of the rim is very oblique: the interval between the upper or frontal margins of the two orbits being 12·2 inches, and that of the lower or molar margin 16·2 inches.

Dimensions of the Skull of the Sivatherium giganteum*.

	Eng. Inches.	Mètres.
From the anterior margin of the foramen magnum to the alveolus of first molar	18·85	·478
From do. to the truncated extremity of the muzzle	20·6	·5268
From do. to the posterior margin of the last molar	10·3	·262
From the tip of the nasals to the upper fractured margin of the cranium	18·0	·4568
From do. do. to do. along the curve	19·0	·4822
From do. do. along the curve, to where the nasal arch begins to rise from the brow	7·8	·198
From the latter point to the fractured margin of the cranium	11·2	·284
From the tip of the nasals to a chord across the tips of the horns	8·5	·216
From the anterior angle, right orbit, to the first molar	9·9	·251
From the posterior do. do. to the fractured margin of the cranium	12·1	·3075
Width of cranium at the vertex (mutilation at left side restored), about	22·0	·559
Do. between the orbits, upper borders	12·2	·3095
Do. do. do. lower borders	16·2	·4108
Do. behind the orbits at the contraction of the frontal	14·6	·3705
Do. between the middle of the zygomatic arches	16·4	·4168

* To facilitate comparison with the large animals described in Cuvier's *Ossemens Fossiles*, the dimensions are also given in French measure.

	Eng. Inches.	Mètres.
Width between the bodies of the malar bones	16·62	·422
Do. base of the skull behind the mastoid processes (mutilated on both sides)	19·5	·496
Do. between the cheek tuberosities of the maxillaries ...	12·2	·3095
Do. of muzzle portion of the maxillaries in front of the first molar	5·8	·149
Do. of do. where truncated (partly restored)	4·1	·104
Do. between the outer surfaces of the horns at their base	12·5	·312
Do. do. do. fractured tips of ditto................	13·65	·347
Perpendicular from a chord across tips of do. to the brow	4·2	·165
Depth from the convexity of the occipital condyles to middle of frontal behind the horns	11·9	·302
Do. from the body of the sphenoidal to do. between the horns	9·94	·252
Do. from middle of the palate between the third and fourth molars do. at root of the nasals	7·52	·192
Do. from posterior surface last molar to extremity of the nasals	13·0	·331
Do. from grinding surface penultimate molar to root of the nasals	10·3	·262
Do. from the convexity near the tip of the nasals to the palatial surface in front of the first molar...............	5·53	·14
Do. from middle of the alæ of the occipital to the swell at vertex of frontal	8·98	·228
Do. from inferior margin of the orbit to grinding surface fifth molar	7·3	·186
Do. from the grinding surface first molar to edge of the palate in front of it	2·6	·066
Space from the anterior angle of orbit to tip of the nasals	10·2	·2595
Antero-posterior diameter left orbit	3·3	·084
Vertical do. do.	2·7	·0685
Antero-posterior diameter of the foramen magnum	2·3	·058
Transverse do. do.	2·6	·066
Long diameter of each condyle	4·4	·112
Short or transverse do. of do.	2·4	·0603
Interval between the external angles of do. measured across the foramen................................	7·4	·188

Among a quantity of bones collected in the neighbourhood of the spot in which the skull was found, there is fragment of the lower jaw of a very large ruminant, which we have no doubt belonged to the *Sivatherium*: and it is even not improbable that it came from the same individual with the head described. It consists of the hind portion of the right jaw, broken off at the anterior third of the last molar. The coronoid apophysis, the condyle, with the corresponding part of the ramus, and a portion of the angle are also removed. The two posterior thirds only, of the last molar remain; the grinding surface partly mutilated, but sufficiently distinct to show the crescentic plates of enamel, and prove that the tooth belonged to a ruminant. The outline of the jaw in vertical section, is a compressed ellipse, and the outer surface more convex than the inner. The bone thins off on the inner side towards the angle of the jaw, into a large and well-marked muscular hollow; and running up from the latter, upon the ramus towards the foramen of the artery, there is a well-defined furrow, as in the *Ruminantia*. The surface of the tooth is covered with very small rugosities and striæ, as in the upper molars of the head. It had been composed of three semicylinders, as is normal in the family, and the advanced state of its wearing proves the animal from which it proceeded to have been more than adult.

The form and relative proportions of the jaw agree very closely with

Fig. 4

Fig 7.

Fig. 5.

those of the corresponding parts of a buffalo. The dimensions compared with those of the buffalo and camel are thus:

	Sivatherium.	Buffalo.	Camel.
Depth of the jaw from the alveolus last molar	4·95 in.	2·65 in.	2·70 in.
Greatest thickness of do.	2·3	1·05	1·4
Width of middle of last molar	1·35	0·64	0·76
Length of posterior ⅔d of do.	2·15	0·95	1·15

No known ruminant, fossil or existing, has a jaw of such large size; the average dimensions above given being more than double those of a buffalo, which measured in length of head 19·2 inches (·489 mètres); and exceeding those of the corresponding parts of the rhinoceros. We have therefore no hesitation in referring the fragment to the *Sivatherium giganteum.*

The above comprises all that we know regarding the osteology of the head from an actual examination of the parts. We have not been so fortunate hitherto, as to meet with any other remain comprising the anterior part of the muzzle either of the upper or lower jaw*. We shall now proceed to deduce the form of the deficient parts, and the structure of the head generally, to the extent that may be legitimately inferred from the data of which we are in possession.

[To be continued.]

XLII. *Experimental Researches into the Physiology of the Human Voice.* By John Bishop, *Esq., &c. &c.*†

THE human voice is a subject of universal interest, and attracts the attention of numerous individuals in every class of society. The facility with which its organs are brought into play, and the perfect ease with which its various tones are produced, convey no idea of the complex and elaborate mechanism by which they are effected, nor of the extreme intricacy in which the phænomena are involved.

Few perhaps are aware that the subject of the voice has been the cause of more laborious research and hypothetical reasoning, attended with more perplexing results, than almost any other object of inquiry connected with animal physiology.

The term voice is exclusively appropriated to those sounds which are produced by the vocal organs of animals. These sounds are of two kinds, namely, first, the primary inarticulate tones, with all their modifications of character, quality and in-

* In a note received from Captain Cautley while this paper is in the press, that gentleman mentions the discovery of a portion of the skeleton of a Sivatherium in another part of the hills: See Journal As. Soc. vol. iv. "During my recent trip to the *Siáwliks* near the Pinjôr valley, the field of Messrs. Baker and Durand's labours, I regretted much my inability to obtain the dimensions of one of the most superb fossils I suppose that ever was found. It was unfortunately discovered and excavated by a party of workpeople employed by a gentleman with whom I was unacquainted; and although I saw the fossil when in the rock, I was prevented from getting the measurements afterwards. This specimen appeared to consist of the femur and tibia, with the tarsal, metatarsal, and phalanges of our Sivatherium." It is much to be regretted that such an opportunity should have been lost of adding to the information already acquired of this new and gigantic ruminant.—Sec. Asiat. Soc.

† Communicated by the Author.

tensity, including the key or pitch and the whole range of modulation;

And, secondly, the interrupted sounds, or voces limitatæ, which constitute articulate language.

To the former division of these functions the following observations are applied.

The difficulties which for more than twenty centuries have obstructed the elucidation of this interesting branch of natural philosophy, may be ascribed to two principal causes.

First, The organs of voice, when anatomically examined, are found to be extremely complex, including portions of a system which is adjusted to perform several of the most important functions of the animal œconomy.

Secondly, The state of acoustic science is not yet sufficiently advanced to estimate all the effects resulting from air in conjunction with an elaborate series of elastic bodies in producing sounds.

The former of these difficulties has been in a great measure removed by Albinus, Bichat, Majendie, and others, and the latter have been considerably reduced by the investigations of M. Felix Savart.

Many of the phænomena of the voice, however, yet remain unexplained, and abound with subtile intricacies. Several of these which are of fundamental importance, the details of which this memoir consists, will, I trust, tend to illustrate.

The organs which are associated in the performance of the functions of the voice are principally the lungs, the trachea, the larynx, pharynx, nostrils, and the mouth with its appendages.

In reference to the vocal organs, the lungs may be regarded as a receptacle of air for their supply.

The *Trachea* is nearly a cylindrical pipe forming the *Porte-vent* and the connecting link between the lungs and the larynx. Its anatomical structure is well known, but its office, with respect to the voice, has hitherto been very imperfectly understood. It varies in length and diameter with the sex and age of the individual. In the adult male it is about four inches and a half in length, and from six to eight tenths of an inch in diameter: in the female, the length is about four inches, and the diameter from nine to eleven twentieths of an inch. It is open at both ends, for the free transmission of air; its lower extremity having a double embouchure called bronchia, which diverge at an angle of about fifteen degrees from the axis of the trachea.

The areas of the bronchial tubes are collectively greater than that of the trachea, owing to which the condensation of the air in the latter is more rapidly effected, and the voice acquires, according to M. Savart, a roundness and fulness which it would not otherwise have possessed.

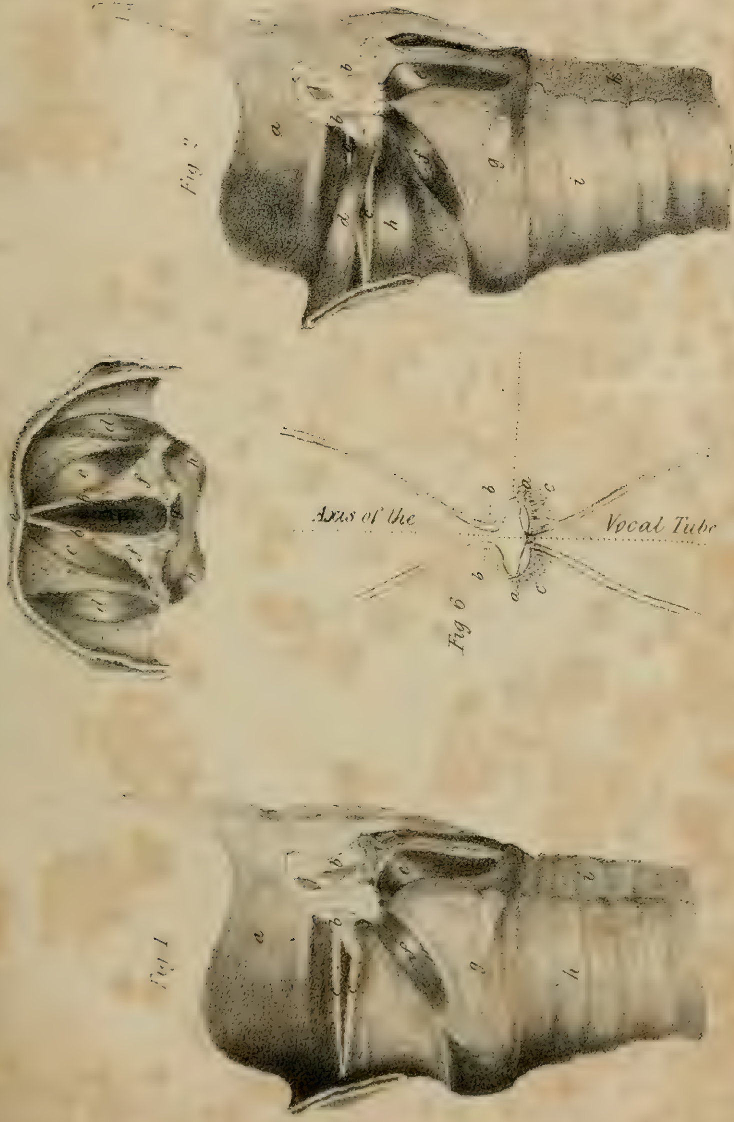


Fig 2
Fig 3
Axis of the Vocal Tube
Fig 6
Fig 1

The important properties of the trachea with respect to the voice are its elasticity, its power of suffering elongation and contraction, as well as of increasing or diminishing its diameter, and the adaptation of its surface to vibrate in unison with the glottis.

The *Larynx* is situated on the top of the trachea, and forms the superior termination of the vocal tube. Its mechanism and functions are exceedingly complicated, and furnish fit subjects both for anatomical and for philosophical discussion. It is the most important organ of the voice, and its structure requires to be well understood before its functions can be satisfactorily explained. Some brief anatomical details will therefore here be introduced. The larynx is a cartilaginous tube situated in the anterior part of the neck, and separated from the cervical vertebræ by the pharynx, within which it has a motion resembling that of the slides of a telescope. Its figure, although difficult to describe with precision, is symmetrical: broad and capacious in its superior chamber, it becomes narrower at its lower termination, where it is joined to the trachea, and presents externally an appearance very dissimilar to its internal conformation.

The frame of the larynx is composed of elastic cartilages, articulated with each other by fibrous and muscular bands in such a manner as to allow a free passage for the transmission of the air in respiration, as well as that mobility which is necessary to the production of the voice. They are five in number: the *Thyroid*, the *Cricoid*, the two *Arytenoid* cartilages, and the *Epiglottis*.

The *Thyroid**, the largest of these cartilages, lies in the front of the larynx, where it seems both to shield the internal mechanism from injury, and to contribute to its peculiar function. It is composed of two lateral portions united at the mesial line, where they form an angle more or less acute. These lateral surfaces are nearly smooth, and terminate in four borders; the superior connected by ligaments with the os hyoides, and the inferior with the cricoid cartilage; whilst the two posterior borders give attachment to some fibres of the *Stylo-* and *Palato-Pharyngei* muscles, and send off four angular processes, two of which are connected by ligaments with the extremities of the os hyoides above, and two with the cricoid cartilage below.

The *Cricoid Cartilage**, situated at the bottom of the larynx, serves by its annular shape and dense structure to form the solid portion of the vocal tube. It is narrow anteriorly, where it is connected with the *thyroid*, from which point it becomes gradually larger, and presents posteriorly a broad portion, on the most elevated part of which are seen two oblique

* Plate III., Fig. 1.

and convex articular surfaces on which the arytenoid cartilages rest: ridges appear on the outside of the cricoid for the insertion of muscles, and its inferior margin is joined by a fibro-cartilaginous membrane to the first ring of the trachea.

The *Arytenoid Cartilages* * are two exceedingly irregularly shaped bodies, situated at the posterior, inner, and upper surface of the cricoid; their figure approaches somewhat to the pyramidal and triangular; their posterior surfaces, to which are attached the oblique and transverse muscles, are concave; they have likewise a concave surface anteriorly, especially towards the lower part, where they are contiguous with a corresponding portion of the arytenoid gland. Their internal surfaces are closely connected with the mucous membrane of the larynx: the planes of these surfaces are perpendicular to the axes of their motion, and adapted to approximate closely with each other. They are terminated by three ridges, one internally, the second externally, and the third anteriorly, which last abounds with inequalities. The bases of the arytenoid cartilages have curved, grooved, oval, articular surfaces, which are furnished with synovial membranes; the grooves are directed downwards, and outwards, corresponding with the convex articulating surface of the cricoid. In front of these cartilages are two conical or pyramidal prominences, forming the posterior part of the chink of the glottis; these prominences project over the tube, about four twentieths of an inch in the male and about three twentieths in the female. At the point of these projections there are often small distinct cartilages, which give attachment to the thyro-arytenoid ligaments. The perpendicular projections of these bodies have also on each of their summits a small, distinct, isolated cartilage united by perichondrium. The arytenoids are endowed with extensive freedom of motion, including a rotatory motion, a sliding one transverse to their axes of rotation, and an oblique tilting motion. They are destined for the attachment of several muscles, whose forces are directed to regulate the movements of the glottis, and the modulations of the voice.

The *Epiglottis* † is a fibro-cartilage occupying a position between the summit of the larynx and the base of the tongue. It is articulated to the superior margin of the angle formed by the union of the lateral portions of the thyroid cartilage, and in its passive state stands almost perpendicular, but assumes a horizontal direction when the larynx is raised in the act of deglutition. In form it has been aptly compared to the leaf of an artichoke; and on both surfaces, but more especially on

* Fig. 1. † Fig. 4.

its laryngeal surface, are found numerous minute orifices in which glands lie imbedded. The epiglottis is repressed by two pair of small muscles called the *Aryteno-Epiglottidei*, and the *Thyro-Epiglottidei.* The effect arising from the depression of the epiglottis upon the fundamental key of the vocal tube is somewhat uncertain. Majendie and Mayo* have inferred from the experiments of M. Grenié † that the epiglottis prevents the tones of the voice from becoming more acute when they increase in intensity; this hypothesis is however decidedly erroneous ‡, in as much as neither the elevation nor depression of the epiglottis can affect or regulate the vibrations of the glottis.

The *Thyro-Arytenoid ligaments* §, or *chordæ vocales*, as they are commonly (though improperly) denominated, are composed of fasciculi of parallel fibres arising from the bases of the arytenoid cartilages; thence proceeding forwards and inwards, they meet, and are inserted together into the posterior surface of the thyroid cartilage at the junction of its alæ.

These *ligaments* are immediately after death almost transparent, and nearly inelastic. These characters, however, very soon disappear by exposure to the air, and they become opake, and yielding. Their length on an average, in the adult male, is six lines, and in the female four and a half lines.

The *chink* formed by the separation of these ligaments is the *Rimula Glottidis.* The form of this chink in a state of relaxation is elliptical, but when the cartilages are widely separated it assumes the form of an isosceles triangle. The breadth of the chink when relaxed is about three lines. The *movements* of the larynx are effected by two sets of muscles; the one attached principally to the *os hyoides*, which is the centre of motion of all these parts and serves to raise and depress the vocal tube; the other is destined to control the movements of the

* Vide Mayo, Physiol., p. 334.

† " M. Grenié a trouvé qu'on pouvait corriger ce défaut en mettant au-dessus des anches, dans le tuyau vocal, des petites lamelles de papier, fixes seulement par leur base, et qui, s'élevant quand le courant s'accélère, s'abaissant quand il se ralentit, peuvent, par ces positions diverses, modifier les ondulations de manière que le ton reste constant, avec une intensité de son différente."—*Précis Elémentaire de Physique Expérimentale*, par J. Biot, page 399.

‡ According to Liscovius (p. 34.), neither its depression, its elevation, nor even its entire removal have any effect on the voice. Haller (*loc. cit. El. Physio.* lib. ix. p. 572,) appears also to be of the same opinion: "Epiglottis equidem nihil faciat ad vocem; cum ea (vox) nata sit et perfecta quam primum aer ex glottidis rima prodiit et absque epiglottide aves suavissime canant."

§ Fig. 2.

cartilages and internal mechanism of the larynx. The muscles which elevate the larynx are the *Thyro-*, *Mylo-*, *Genio-*, and *Stylo-Hyoidei*, aided by the *Digastrici*. In this elevation the *Genio-glossi*, the *Lingualis*, the *Stylo-*, *Thyro-**, *Crico-* Pharyngei*, and the *Hyo-glossi* concur.

The muscles which have an opposite effect, and *lower* the larynx, are the *Sterno-Thyroidei*, the *Sterno-Hyoidei*, and the *Omo-Hyoidei*.

The second set of muscles exerts a very important influence on the voice, the functions of which being imperfectly understood will require a few details.

The *crico-thyroideus* † muscle approximates the cricoid to the thyroid cartilage anteriorly, and closes the chink between them; in this action, the posterior and upper edge of the cricoid is rotated backwards, by which the antero-posterior diameter of the larynx is enlarged, and the tension of the vocal ligaments increased.

The *crico-arytenoideus posticus* ‡ is situated on the posterior broad surface of the cricoid cartilage, from whence it originates. Its fibres, ascending obliquely outwards, are attached to the base of the cricoid cartilage, between the *crico-arytenoideus lateralis* and the *arytenoideus obliquus* and *transversus*. This muscle, by drawing the arytenoid cartilage backwards and rotating it outwards, opens the aperture of the glottis.

The *crico-arytenoideus lateralis* assists in closing the glottis. The peculiarity of the action of this muscle is, that by drawing the external angular base of the arytenoid cartilage forwards, its anterior pyramidal projection, to which the vocal cords are attached, is at the same time rotated inwards.

The *thyro-arytenoideus* is the most complicated, most important, and the least understood of any of the whole set. It forms the whole superior and inferior lateral boundary of the glottis, and is closely connected with the vocal ligaments; its direct force is to antagonize the *crico-thyroideus*, to rotate the cricoid on the thyroid, and to draw forwards and approximate the arytenoids anteriorly, as likewise to relax the vocal ligaments. The thickness of this muscle being increased and rotated upon itself inwards when contracting, forces the edges of the glottis together at its central part. By the various motions of the *thyro-arytenoidei* muscles on the vocal ligaments their edges are turned into the *vibrating position*, and by their action, in conjunction with that of their antagonists the *crico-thyroidei*, the *tension*, and the *vibrating length of the glottis* are

* Names given to some fibres of the Constrictor Pharyngeus inferior.

† Fig. 7.

‡ Fig. 2.

regulated. The *thyro-arytenoideus superior* serves to assist the *thyro-arytenoideus* in relaxing the vocal ligaments.

The *arytenoideus obliquus* and *aryt. transversus** are muscular bands situated between the two arytenoid cartilages, to which they are attached. Some of the fibres assume a horizontal, others an oblique course, and their united action is to bring these cartilages towards each other, by which the aperture of the glottis is closed posteriorly. It is commonly stated in anatomical works that these small muscles are capable of closing the glottis, but this is incorrect.

The *larynx* is lined throughout by a mucous membrane, which being continued from the mouth, over the epiglottis, forms in its descent those folds over the superior margin of the *thyro-arytenoidei* muscles to which anatomists have given the name of *pseudo-glottis*; thence swelling out into a pouch of considerable size, it forms on either side the ventricle, or *sacculus laryngis* †; and finally, after having been reflected over the *chordæ vocales*, it passes through the cricoid cartilage and becomes the membrane of the trachea.

A number of mucous glands are situated in the folds of the pseudo-glottis and in the triangular space at the base of the epiglottis, their excretory ducts opening on that fibro-cartilage. These glands doubtless assist in lubricating the vocal canal.

The *thyroid gland*, a singular substance so named (but in which no excretory duct has been discovered), is placed on the larynx and superior part of the trachea; it is composed of two lateral pyramidal portions, united in most subjects by a distinct glandular medium. The size of this gland varies in different individuals; it is said to be larger in the female than in the male. It has generally been supposd by anatomists that this gland has some influence on the voice, but its true functions are unknown ‡.

The exquisite sensibility of the larynx, its dependence on the will, as well as its muscular motions, are derived from the *superior* and *recurrent* laryngeal filaments of the *pneumo-gastric* nerves.

The distribution of these nerves to the muscles which act on the glottis is a subject of anatomical controversy. Experiments made on them by Martin ||, Professor Sue of Paris, Dr.

* Fig. 3.

† The *sacculus laryngis* insulates superiorly the vocal ligaments.

‡ It appears to me by no means impossible that the thyroid gland secretes a fluid transmitted by an invisible process which lubricates the vocal tube. The constant passage of air must render it requisite to be kept permanently moistened.

|| Edinburgh Essays.

Haighton *, Cruikshanks, Scarpa, Arneman, Magendie, and others have been attended with curious results. The description given of them by the latter however is opposed by Rudolphi, Andersch, Sœmmerring, Meckel, Bellengeri and others.

The truth is, that the superior and recurrent nerves anastomose in giving filaments to some of those muscles which dilate, as well as to those which close the glottis.

The action of these muscles may be briefly recapitulated as follows:

The *crico-arytenoidei postici* open the glottis.

All the other muscles close it.

The *arytenoideus obliquus* and *aryt. transversus* close the arytenoid cartilages posteriorly.

The *crico-arytenoidei lateralis* and the *thyro-arytenoidei* close them anteriorly.

The *thyro-arytenoidei* close the centre of the glottis, and with the *crico-thyroidei*, regulate its tension, position, and vibrating length.

The views here taken of the actions of these muscles differ from those entertained respecting them by anatomical authors in general.

Not having found any two anatomists strictly agreeing on the subject, I have been induced to make numerous dissections to ascertain their functions in producing voice.

The annexed figures were drawn by Mr. Henry Dayman, from these dissections.

The actions assigned to the *thyro-arytenoidei* admit of most discussion.

That these muscles relax the vocal ligaments, and at the same time close the glottis, may at first sight appear exceedingly doubtful; but all my attempts to close the glottis by the approximation of the arytenoid cartilages and the tension of the *thyroidei* muscles were unsuccessful, nor could any sound be produced until the *thyroidei* were brought into action, except by forcing such a volume of air through the glottis as it is almost impossible can take place during life †.

In confirmation of this view, it is observed by M. Magendie,

* Mem. of the Med. Soc. London.

† The quantity of air expelled to produce voice of ordinary intensity is about twenty-five cubic inches in the adult male, with a voice pitched in the tenor G. To produce its grave octave of the same intensity will require 50 cubic inches. In ten seconds therefore the lungs will be almost exhausted in producing the upper G, and in five seconds for the grave octave; allowing 200 cubic inches to be expelled, which is the average quantity of air the lungs are estimated to contain after a full inspiration.

The quantity of air expelled from the lungs will consequently vary with every note in the scale relatively with the key and the intensity of tone.

that if the *thyro-arytenoidei* are paralysed or their nerves divided, the vocal chords will no longer vibrate.

Although there is a great diversity of opinion respecting the actions of the other muscles, they may nevertheless be easily demonstrated on mechanical principles, which has been partly accomplished by Mr. Willis.

In reference to their functions, the vocal organs may be regarded as a wind instrument, of which the lungs are the bellows, the vocal tube the pipe, and the glottis, composed of elastic vibrating membranes, is the reed.

The type of these organs is found in all the higher classes of vertebrated animals, in mammalia, in birds, and in reptiles. In fishes it may be considered as rudimental.

The production of the most simple tone of voice requires the associated actions of a most extensive range of organs *, of which the following is a brief exposition.

When the tension of the thyro-arytenoid ligaments takes place they turn upon their axes; their planes (which in a state of relaxation are parallel † to the axis of the vocal tube) become perpendicular ‡ to it, and as the edges of the glottis approximate, its chink is closed up and acquires its true vibrating position §.

[To be continued.]

XLIII. *Reply to* Professor Young's *concluding Remarks on the Theory of Vanishing Fractions. By* W. S. B. Woolhouse. ||

IN my last paper, on the theory of vanishing fractions, (p. 18,) I have discussed the subject so fully and set forth so many plain and straightforward mathematical arguments, completely proving the truth of the general principles objected to by Professor Young, that I conceive it would be quite superfluous to make any further addition to what has been already so clearly and satisfactorily established.

Professor Young's concluding remarks on the theory of vanishing fractions are, however, of so unrestrained and extravagant a nature that I cannot possibly allow them to pass without some notice. His observation that the expression $\frac{x^2 - a^2}{x - a}$ implies an *operation* to be performed, and that

* In the ordinary modulations of the voice more than one hundred muscles are brought into action at the same time.

† Fig. 4.

‡ This state of the thyro-arytenoid ligaments is the vocalizing position of Mr. Willis: they will not however vibrate unless their edges (through the medium of the mucous membrane) be approximated, and when thus adjusted, they are in the state which I have denominated the true vibrating position of the glottis.

§ Fig. 6.

|| Communicated by the Author.

its *value* is $x + a^1$, is in my opinion totally without meaning. Any person possessing a slight acquaintance with the common definitions of plain algebra needs not to be told that the expression $x + a$ implies an "*operation* to be performed" just as much as the expression $\frac{x^2 - a^2}{x - a}$; and that the expressions are, every one of them, "symbolical forms." The fact is that the equation, $\frac{x^2 - a^2}{x - a} = x + a$, simply indicates a transformation of one symbolical form into another, by the operation of division; and I have distinctly shown that this operation is not justifiable when x is equal to a, as indeed Professor Young tacitly admits when he speaks of the form $\frac{a^2 - a^2}{a - a}$ as isolated or detached from its interpretation. I hold that the questions, What is the value of $\frac{x^2 - a^2}{x - a}$ when $x = a$? and What is the value of $\frac{a^2 - a^2}{a - a}$? are not "perfectly distinct" unless the condition of continuity be expressly introduced. In the case $x = a$ the operation of division by zero is actually performed in the above equation, and yet Professor Young pertinaciously asserts, with an undignified attempt at sarcasm, that he has "never seen" any work in which the zero processes occur: the objectionable process occurs in every example that he has adduced; and I am persuaded that he would not, in candour, have insisted on the problem of Clairaut, or indeed on any other example, as an obstacle to my principles, had he attentively considered my general reasoning at the bottom of page 24 of this volume. With respect to this problem I shall refer to Spiller's very neat translation of Lacroix's Algebra, as the more likely to be in the hands of the English student, and just observe that the operations are strict as far as the quadratic root at the top of page 167, but that the succeeding operation in which the numerator and denominator of the second of the two fractions contained in the surd are multiplied by $1 - m$, is unwarrantable in the case $m = 1$. If the surd be expanded as it is immediately and legitimately presented by the quadratic, and the value $m = 1$ substituted, the operation will lead to the proper results, viz. $\frac{a}{2}$, ∞. This will sufficiently account for my having passed over "in silence so decided an argument" as wholly inapplicable to the case.

Professor Young refers particularly to the examples in Bourdon's Algebra, see page 118, fourth edition, where Bourdon deduces the symbols of his results from the general formula on page 106. Now in all these examples the factors $b' c'' - c' b''$, $b c' - c b'$, are each *zero*, and therefore in finding the value of x from the equations (4), (5), page 105, where multiplications are directly performed by these factors, the objectionable zero process actually occurs. Let any one follow the steps of the general elimination, page 105, with one of the particular examples, and the fallacy will immediately present itself. It is the direct incorporation of these foreign zero factors that gives to the results the form $\frac{0}{0}$, which they would not otherwise assume; and they furnish Professor Young with another example of the zero processes which he has "never seen."

The expulsion of foreign zero factors, which are usually introduced in an investigation by direct multiplication, is the object of my fourth proposition. If they were allowed to enter, as legitimate factors, in mathematical reasonings, the form $\frac{0}{0}$ might very evidently represent any species of quantity whatever. The remarks of Bourdon on this symbol do not refer to what it strictly represents, but only to its general indication, considering all cases without any regard to the legitimacy of the process. What Professor Young states respecting the foreign factors is altogether foreign to the subject in dispute, as the general principles maintained in my essay expressly object to the introduction of all such factors. The real statement of the question is this: Is the value of a vanishing fraction indeterminate or not when the zero factors, as in the ellipse question, are not directly introduced, but arise spontaneously in the investigation? and I have thoroughly discussed it in my former papers.

Professor Young's remarks concerning multiple solutions, or what ought to have been properly termed indeterminate solutions, is a mere quibble about words. I must frankly confess that his important distinction between the two statements is one in which I cannot perceive the slightest difference in substance. It would be an idle waste of words to say anything more about it. His observations on singular solutions, however, call for some remark. No objection whatever has been offered to the statement which he defends so cavalierly in his present letter. It must be highly amusing to the readers to perceive him calling on the aid of Lagrange in support of a statement that no one, for aught I know, ever thought of disputing. The true state of the case is as follows. Speaking of the result of a general investigation furnishing every solution to a problem, in his last letter he alluded to the well-known fact of singular solutions not being comprised in the general integral, as a decided denial of my statement. In my reply I observed that a direct process of integration always leads to the singular solution at the same time with the general solution, thereby showing that the general result of the investigation does really furnish every possible solution in accordance with what I had said. Is it not, then, evident that the subject of singular solutions was utterly useless as an objection to my remark, that Professor Young had not sufficiently examined the matter, and that his expression, that I had done myself a "wanton injustice," was not justifiable on the score either of accuracy or of propriety?

I hereby close my remarks on this subject for the present. It is obvious that Professor Young, instead of applying his own mind to the discussion of the points at issue, has all along rested his evidence and opinions on a misinterpretation of authorities. I do not suppose I should have written these brief and concluding observations for the readers of the Journal, had I not felt myself imperatively called upon to notice the tone of assumption and dictation that prevails throughout his last letter. He ought not to be unconscious of the fact, that personal sarcasms and presumptuous language are not the generally received indications of a strong supply of argument or of a sincere desire after the development of truth.

London, August 13, 1836.

XLIV. *An Examination of the Question, whether the Discordancy between the Characteristics of Mechanical Electricity, and the Galvanic or Voltaic Fluid, can arise from Difference of Intensity and Quantity; with some Observations in Favour of the Existence of an Electro-motive Power independently of Chemical Reaction, but cooperating therewith: respectfully submitted to the British Association for the Advancement of Science. By* R. HARE, *M.D., Professor of Chemistry in the University of Pennsylvania.**

IN one of the papers, giving an account of Faraday's recent valuable researches in electricity, for copies of which I have been indebted to the flattering attention of the author, I find the following language:

"Hence arises still further confirmation, if any were required, of the identity of common and voltaic electricity; and that *the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive qualities* †." And elsewhere referring to Cavendish, as the author of this opinion, it is alleged that it "*only requires to be un-*

* Reprinted from a pamphlet privately circulated by the Author.

† [See Lond. and Edinb. Phil. Mag., vol. iii. p. 363.—EDIT.]

derstood in order to be admitted." Notwithstanding that in support of the opinion thus quoted, the much-respected authority of both Cavendish and Faraday is arrayed, it is one which I cannot so understand as to admit.

I am unable to form any other idea of intensity, than that of the ratio of quantity to space. Thus the intensity of the pressure of an elastic fluid, is as the quantity to the space in which it is confined. The space being the same, the intensity of the pressure will be directly as the quantity; the quantity being constant, the pressure will be inversely as the space.

Agreeably to an analogous mode of reasoning, the intensity of the light or the heat emanating from a radiant body, is always estimated to be inversely as the surfaces on which it may be diffused or concentrated; and hence the inference that the intensity is as the square of the distance inversely, or as the area of the receiving surface of a lens or mirror, to that of the focus into which the rays are collected.

It follows that if there be in any two cases, like quantities of electricity evolved by mechanical, and by galvanic apparatus, the space occupied by the fluid generated by the latter, must be as much greater, as its intensity is less.

In a memoir which I published upon this subject some years ago, I endeavoured to show that the spaces occupied by equivalent charges of galvanic and mechanical electricity were not such as to justify the idea that the former required for its existence a larger space than the latter. But on this subject, it is not now necessary to recur to the facts which I then adduced, since I find it conceded in one of the recent Memoirs of Faraday, that the spaces occupied by the electricity evolved by galvanic apparatus, as compared with those occupied by mechanical electricity, are almost infinitely small. "*A grain of water or of zinc, contains as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches.*" "*Four grains of zinc, with one of water, may yield as much electricity as is evolved during a thunder storm.*"

It follows inevitably that the electric matter evolved by galvanic action is, previous to its evolution, in a state almost infinitely intense, as compared with that of the same matter, when evolved by a machine or meteorological changes. Yet to the currents induced in this matter, in the first-mentioned form, an opposite state is ascribed, as respects intensity, to that in which it has previously existed.

It may be said with respect to currents, that the space being the same, the intensity will not only be directly as the quantity, but also inversely as the time in which it passes, or, in other words, directly as the velocity. But what is to create inequality of velocity when the channel, a wire for instance, is of the same size and nature in both cases? When the same fluid is in question, the velocity will be as the forces by which it may be impelled. The only forces to which electricity has ever been alleged to be liable, as far as I am informed, are either the self-attractive or self-repulsive power of its own particles, or their attraction for other matter. It will be admitted that the intensity of these forces must, in the case of electricity, as in that of caloric or light, be as the quantity to the containing space; and consequently, that it would be unreasonable to allege that the reciprocal repulsion of the electric atoms, or their attraction for other matter, and consequently any velocity thence arising, should not be in proportion to the state of condensation from which they may be liberated.

If the superior velocity displayed by electricity generated by friction be the cause, not the consequence of greater intensity, how are we to account for the superior velocity?

According to the doctrine of Du Fay, electricity is retained upon the surface of a charged pane by the reciprocal attraction of the heterogeneous fluids. According to the Franklinian doctrine it is retained by its attraction for the negative surface, on which side this attraction is not counterbalanced by repulsion from other electricity. When the circuit is completed by a conductor, according to the one doctrine, a surcharge on the one side is translated to the other; while, according to the other doctrine, two heterogeneous fluids rush from the surfaces in which they are previously accumulated in excess, to enter into combination, and, at the same time, to restore the equilibrium of the surfaces on which they have been respectively deficient. But according to either hypothesis, wherefore should the forces be greater for electric matter when generated by one means, than when produced by another? Why should a mass of electric matter evolved in a diffuse state by a machine, or from a cloud, rush like a bullet through conductors, which are almost impassable to the same fluid when evolved from a state of extreme density within a simple galvanic circuit?

During the process of exciting an electric battery, the electricity previously existing equally on both sides of the glass is so transferred from one side to the other, that the one becomes as much negative as the other becomes positive; and it must be evident that the intensity will be limited by the extent of the force by which this transfer is effected. It is difficult to conceive that merely by a change of capacity arising from friction, a force should be generated at all comparable to that which the electric matter must exert, in escaping, according to the premises, from a state of extreme density, as when extricated by galvanic action from water or zinc.

I ascertained, some years ago, that the galvanic fluid evolved by a large calorimotor of a single pair, will not ignite a wire which may be easily deflagrated by a much smaller apparatus of the same construction. Yet sheets of metal, about four inches in breadth, might be raised by a discharge from the larger instrument above the temperature of boiling water. In such cases, agreeably to the doctrine of quantity and intensity, the electric fluid exists, up to the period of its evolution, in a state of extreme condensation and consequent intensity, and yet at the moment when a perfect but restricted channel is afforded to it, becomes too diffuse to pass through it with a velocity sufficient to produce deflagration. How can the electricity which is in the one case so dense, become in the other so rare? Where, and in what manner does it exist intermediately between the period of its condensation within the pores of the generating materials, and its rarefaction in the wire which forms the circuit between them?

I am aware that to the want of adequate insulation, the inferior intensity of the charges communicated to coated surfaces by voltaic apparatus, will be attributed; as it cannot, without a palpable contradiction, be ascribed to any defect of intensity, in a source wherein the ratio of the quantity to the space is almost infinitely great.

Let us, then, examine the subject agreeably to this view of the case. Since the electricity liberated by electro-chemical reaction by means of a single galvanic pair must have preexisted in a state of extreme condensation and consequent intensity, it follows that it ought to be productive of a tension limited only by the insulating power of the menstruum within which it is extricated. It should then, when evolved as above described, attain the highest degree of intensity consistent with the insulating power alluded to. That this is not the fact, is fully established by general experience, and by the observations of Faraday, according to which the intensity of a voltaic series increases with the number of pairs employed.

It results also from the premises, that the tension should become as great in a large as in a small pair; and by employing one large pair, effects should be attainable, as potent in respect to intensity, and more potent as respects quantity, than those resulting from a series of pairs. Yet the experiments above mentioned prove, that as the surfaces, associated as a single galvanic pair, are enlarged, the intensity lessens; so that a calorimotor of a single pair containing fifty square feet of zinc, will not, in a wire of any size, produce an ignition of as high intensity as may be effected by the elementary battery of Wollaston, formed of a silver thimble, and piece of zinc proportionably minute.

It appears from the experiments of Professors W. B. and H. D. Rogers *, that the power of a galvanic pair in deflecting a magnetic needle, was increased by causing the surface of the copper plate to exceed that of the zinc; while by extending the zinc surface beyond that of the copper, little or no increase of power ensued. This result appears to be the opposite of that which the theory of Wollaston, supported by some recent observations of Faraday, would lead us to expect. As pursuant to that theory, the galvano-electric fluid is due exclusively to chemical reaction; if the charge were not promoted by an excess of extension in the oxidizable metal, it ought not to have been improved by similar extension of that which is insusceptible of oxidizement.

According to the observations of the Professors above mentioned, the deflection resulting from a galvanic discharge, on the first immersion of the plates, after a repose of two hours, was six times as great as that which could be permanently sustained. The greatest effect appeared always to ensue before there was any sensible extrication of hydrogen, and the commencement of the effervescence was invariably the signal for a decline of power.

It was by analogous observations respecting the igniting influence of galvanic apparatus that I was led to the construction of my deflagrators†, in which the deflagrating power appears, agreeably to my experience, to be exalted as much by the repose of the surfaces as the ability to influence the magnetic needle was ascertained to be, in the experiments of my sagacious friends above mentioned.

That the evolution of the galvanic fluid is not in proportion to the intensity of the chemical reaction, is corroborated by the fact, that the intensity of the ignition, excited in a wire by a galvanic discharge, diminishes, while the effervescence increases; and it is well known that the power of galvanic apparatus is not augmented by adding to the strength of the solvent, beyond a very moderate limit.

But if it be granted that chemical affinity, when reacting within a galvanic circuit, without any propulsive power from the elements of the circuit, can receive a peculiar impulse, so as to produce a current of the electric fluid, confining it at the same time to a very narrow channel, by what process can this species of chemical reaction be conceived to accelerate an elec-

* See Silliman's Journal for October, 1834.

† [See Phil. Mag., First Series, vol. lvii, p. 281, lix. p. 113. & lxiii. p. 241. —Edit.]

trical current already produced? The same propulsion must be given to the electricity liberated between the plates of the second pair as between those of the first; and it is inconceivable to me, that the accession of that derived from the first pair should add to the velocity of the portion evolved by the second. The current of the former cannot be supposed to move with greater velocity on account of its meeting with another, which moves at its own rate. Currents are not accelerated by their confluence, unless the head or pressure be increased, and the channel restricted. But in the case in point it has been shown, that by the reaction of the solvent with the first pair in the series, the tension must attain the highest degree consistent with the imperfect insulation; and no cause has been assigned for the restriction of the channel. It may be said that the current from the first pair cannot pass through the liquid in the second cell without causing the decomposition of that liquid; and that as its power is inadequate to effect this change, it has to pursue the same route as the electricity which is generated by the oxidizement of the second plate of zinc. It is still difficult to me to imagine that it can transfer its momentum to the current which thus precedes it; or that the chemical reaction by which the latter is evolved, should act only in accelerating the stream which it receives from the preceding pair. Granting that imponderable matter, at the moment of its extrication from confinement among ponderable atoms, were to receive an impulse which, by extraneous cooperating causes, should force it to move in a current, yet I cannot imagine that such atoms can, by any reaction originating between themselves, give an impulse to imponderable matter extricated from other atoms. Whether or not the electricity be derived from chemical reaction, it seems to me that the power which puts it in motion, and accelerates and condenses it into a channel, still smaller and smaller as its intensity increases, must be ascribed to some mysterious property arising from the arrangement of the elements of the series, which is, in the present state of our knowledge, inexplicable.

This electromotive power, if not antecedent, does not appear to me to be consequent to chemical reaction. I conceive that it operates upon all the imponderable elements within its scope, tending to accumulate them at the "electrodes" under a greater or less degree of tension. The potency of the resulting discharge, when the circuit is completed, is regulated both by the tension and the quantity of the imponderable matter accumulated. But the presence of reagents, which favour the extrication of imponderable materials, as in the more efficient voltaic apparatus, is compatible only with a feeble insulation, while arrangements more favourable to insulation, as in De Luc's Electric Column, are incompatible with a copious supply of the imponderable matter.

Probably upon an analogous ability to produce or annul, to promote or retard, chemical reaction, the efficacy of animal and vegetable organization is founded, being obviously dependent on an arrangement of masses. The voltaic series of a gymnotus is evidently an animal organ, and its analogy with the voltaic series produced by human ingenuity induces me to consider the latter in the same class of agents as the organs by which life is supported.

I should have expected, that in establishing the highly interesting fact that every elementary equivalent has the same quantity of electricity, the ingenious author of this discovery would have adverted to the analogous observations of Petit and Dulong respecting the specific heat of elementary atoms. It strikes me as important, that similar conclusions should have been arrived at by such high authority, both as respects caloric and the electric fluid. I am surprised that Faraday should appear to have overlooked this analogy in the explanation of the practical results which he has obtained.

No hypothesis appears to be more generally sanctioned at this time among chemists than that which ascribes the aeriform state to a union between caloric and ponderable matter. When hydrogen unites with oxygen, caloric is evolved. It follows that when these substances are made to resume the gaseous form, caloric must be supplied to them.

When it is considered that the inferences of Petit and Dulong, respecting the specific caloric, and those of Faraday respecting the electricity combined with ponderable equivalents, tend to demonstrate the coexistence in them of equivalent atmospheres of each of those imponderable fluids, does it not authorize a surmise that in the voltaic current they may be associated; and that with those equivalent measures of electricity which Faraday has shown to pass, corresponding portions of caloric are imparted? The idea of Berzelius, "*that the heat and light evolved during powerful combinations, are in consequence of an electric discharge at the same moment taking place*," being cited by Faraday in the language of this quotation, he observes, that it "is strictly in accordance with his view of the quantity of electricity associated with the particles of matter." To me it appears to be no less in accordance with the idea that heat and light are associated with those atoms to a commensurate extent; and since, by the premises, electricity reacts with them, they may be presumed to react with electricity.

That heat, light, and electricity are all concomitant products of electro-chemical reaction, is self-evident. Agreeably, then, to the strict rules of induction, wherefore is the principle last mentioned to be considered as the cause of the others?

Where is the proof that the heat and light evolved between the "electrodes" are effects merely of electricity? The fact of the apparently unlimited evolution of heat from a finite portion of wire duly subjected to a voltaic circuit, is inexplicable, consistently with the materiality of caloric, unless we suppose the fluid to be derived from the same electro-chemical reaction to which we owe the electricity associated therewith.

I conceive it to be almost self-evident, that mechanical and voltaic electricity are due to the same fluid, so far as they are strictly electrical. The only doubt with me is, whether the very different characteristics of the phænomena produced by the different means alluded to could be explained without supposing some other modifying causes. And at all events, from the reasons above given, I am dissatisfied with the explanation that the difference is dependent on quantity and intensity.

In terminating my observations, I subjoin the following statement of my opinions as heretofore expressed in one of my text-books.

"It does not appear to me that the production of electro-magnetic phænomena, both by galvanic and by electrical discharges, disproves my opinion, that caloric and electricity are connate and coordinate products of galvanic action.

As ignition is producible by either discharge, whether electric or galvanic, the fluid of heat, no less than the electric fluid, may in both cases be concerned; and it is yet to be shown, that magnetic phænomena are ever due to the unalloyed agency of electricity.

It is true that magnetism has been imparted, by discharges of mechanical electricity, without any ostensible agency of caloric; but it is equally true, that magnetic movements have been produced also, by the application of heat, unaccompanied by any ostensible agency of the electric fluid; and it seems as rational to suppose that caloric and electricity are associated in the first instance as in the last.

Those who consider electricity, varying in quantity and intensity, as the common cause of electrical and galvanic ignition, and of thermo-magnetic phænomena, must suppose that this principle and caloric are capable of a reciprocal action. In the first case, caloric is evolved by electric action; in the last, electric currents are produced by calorific repulsion. Hence, as action and reaction are equal and contrary, I deem it rational to suppose that in some cases the former, in other cases the latter, may be the prime mover; but that both participate in every galvanic, electro-magnetic, or thermo-magnetic current.

XLV. *On the Difference between the Attractive Powers for soft Iron of the Electro-magnet and the Steel Magnet; in reply to* Dr. Ritchie. By G. Rainey, *Member of the Royal College of Surgeons.*

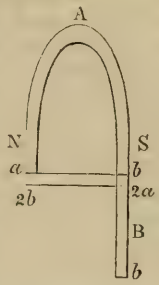
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE last Number of the Philosophical Magazine, at page 83, contains some remarks by Dr. Ritchie upon a paper which I communicated in May last (printed in the Number for July), concerning the cause of the great disproportion in the attractive power of the electro-magnet for soft iron when in contact and at a distance, compared with the attractive power of the common steel magnet under similar circumstances. The concluding paragraph of Dr. Ritchie's paper is as follows:

"In the explanation given by Mr. Rainey, the lifter is supposed to *react* powerfully on the electro-magnet, so as to increase its power, a supposition which cannot possibly be admitted. For the electro-magnet must, in the first place, give the lifter all its magnetic power, consequently the power of the lifter can *never* exceed that of the electro-magnet, and consequently *never* can induce a higher magnetic state in it than what has already been done by the voltaic helix." What Dr. Ritchie pronounces to be a supposition which cannot possibly be admitted, I will prove, I think to Dr. Ritchie's satisfaction, both by reasoning and experiment, to be an absolute and undeniable fact. My position is, that the lifter of a common horse-shoe magnet can exceed the power of the magnet to which it is applied; and in case that magnet has not received its maximum of magnetism, can induce in it a higher state of magnetism than that which existed before the application of the lifter; and that in proportion as the permanent magnet resembles in its texture soft iron, the effects of induction on it and on the electro-magnet more closely resemble each other, so as to render it obvious that the effect of the keeper on the two is the same, excepting in degree.

Let a and b denote the inducing power of the magnet A, and let B be a piece of soft iron, placed in the position represented in the annexed diagram, and sufficiently large to receive all



the magnetism S is capable of inducing. It is evident that a will represent the magnetism induced at the adjacent end of the soft iron, and b that at the remote one: now in this position of the soft iron B can have no more magnetism than it receives from S; consequently, cannot exceed the power of the magnet A, and therefore cannot induce in it a higher degree of magnetism than it possessed before the contact of B. So far Dr. Ritchie's assertion holds good; but the circumstances are very different under which the armature is placed in the electro-magnet, or the common horse-shoe magnet referred to in my paper.

Now let B be placed, with its extremities in contact with both poles of the magnet A; then, according to the first case, a will represent the magnetism at the S pole, and b that at the N pole; but as S has induced in B $a + b$ of magnetism, so N must induce in B $b + a$ of magnetism; therefore $2\,a$ will now represent the magnetism at the S pole, and $2\,b$ that at the N pole. According to this reasoning, the contact of the armature should tend to induce $a + b$ of magnetism in the magnet A, provided A has not received its maximum of magnetism, or is not too hard to admit of further induction by the reaction of the keeper.

This is exactly what occurs, which will be manifest from the following experiment. Take a common horse-shoe magnet, magnetized in the ordinary manner; then weaken this magnet by gliding repeatedly the armature from the poles to the neutral point: this mode of reducing the power of a magnet is much better than by applying its poles to the similar poles of another magnet, as it does not alter the disposition of the magnetism in the metal; consequently the position of the neutral point remains in the centre. The magnet being thus treated, note accurately its magnetic power; then apply a piece of soft iron to its poles, in the manner in which the lifter is employed in the electro-magnet; and after the removal of the soft iron, the magnet will be found to have acquired a considerable increase of magnetic power. If the lifter be slided from the neutral point to the poles, its reaction will have been exerted more generally upon the steel, and the magnetism induced by the keeper will consequently be increased. By performing these experiments upon magnets composed of steel of different degrees of hardness, it will be found that the softer the steel is the greater will be the increase of the magnetism it acquires from the reaction of the armature: and the density also of the armature has considerable influence over its inducing power, both on the steel magnet and the electro-magnet; if the armature applied to the electro-magnet be made of very hard steel, it will not sustain so much weight from the same intensity of galvanic current as if it be made of soft iron.

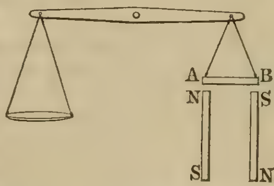
Thus it will be obvious that the more the steel is allied to iron in its temper, the more closely the effect of the keeper upon the steel magnet will resemble that upon the electro-magnet. Hence the explanation of the action of the armature upon the one will apply to the other.

In order that no misconception of my meaning may take place, I will give one application of the above reasoning to the case of the electro-magnet. Suppose a feeble galvanic current to have been passed around an electro-magnet, and that the magnetism induced by this current is represented by a and b; then, according to the foregoing explanation, $2\,a$ and $2\,b$ will represent the magnetism on each side of the centre of the armature; and as this exceeds the magnetic power induced by the voltaic helix in the electro-magnet by $a + b$, the armature will induce $a + b$ of magnetism in the electro-magnet. As soft iron offers so little impediment to the inducing power of the armature, this power may be considered to exert its maximum effect upon very soft iron, and its minimum on the hardest steel: now suppose the electro-magnet to have received an accession of magnetism from a stronger galvanic current, which may be represented by $c + d$, then the inducing power of the armature will be raised to $2\,(b + d)$ at one extremity, and $2\,(a + c)$ at the other; and the electro-magnet will have now received from the armature $c + d$ more of magnetism, and so on for all future additions of magnetism induced by the voltaic helix, until the electro-magnet has attained the highest state of magnetism it is capable of.

XLVI. *Remarks on certain proposed Improvements in the Magneto-Electric Machine. By the Rev.* William Ritchie, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain and in the University of London.*

In the last Number of the Philosophical Magazine, p. 120, there is a paper on Magneto-electric Machines, by Fred. Mullins, Esq., M.P., of such an extraordinary nature, that I feel myself called upon to point out its fallacy and inconsistency. As *facts* are by far the most stubborn things to get rid of, I shall simply state two experiments, which will be quite sufficient, without entering at all into the principles of the science.

Experiment 1. Take two bars of soft steel of any convenient size, cut one of them in the middle, and bend the other into the form of a horse-shoe magnet; temper both to the same colour, magnetize them *equally*, and try their powers in the following manner: bring the two bars, as in the figure, to different distances from the lifter A B, and ascertain their attractive power; remove the bars and substitute the horse-shoe, and its power of attraction, and consequently its *inducing* power, will be found to be much superior. Place the bars and horse-shoe in succession at the same distance from a revolving lifter, and the electricity induced on the coil will be found to be much greater, both in deflecting a needle and in decomposing water, with the horse-shoe magnet than with the equal bars.



Hence the absurdity of using *bar* magnets instead of horse-shoe ones to *induce* magnetism on soft iron, and thence voltaic electricity on a coil.

Experiment 2. Cut a bar of the same length as before into *three* portions, bend one of the pieces into an arc, magnetize the two parts, apply the arc to form a horse-shoe magnet as Mr. Mullins proposes, repeat the previous experiment, and the *old* horse-shoe magnet will be found to be much stronger than the *new* one.

It is easy to see how Mr. Mullins, in his experimental researches, has been led into these errors. Horse-shoe magnets, made by ignorant makers, are often left almost entirely *soft* at the bending and only hardened towards the poles or ends, from the old absurd notion that the magnetism was *accumulated* at the poles. The soft part of the circuit therefore, possessing scarcely any *coercitive* power, is very much in the same state with a piece of soft iron. Such a magnet *may* perhaps be improved by cutting off the bending and supplying its place by a properly magnetized arc. Another error into which Mr. Mullins has fallen is not so easily accounted for: he affirms that the size of the arc is of no consequence. A tempered steel wire would therefore be as good as an arc formed of steel, two inches broad and half an inch thick. Instead then of recommending gentlemen who have horse-shoe magnets applied to magneto-electric machines, to cut off the bending, the wisest course will be to let them alone if they are properly tempered, if not, to get them re-tempered and re-magnetized.

XLVII. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

Feb. 9, 1836.—THE reading was concluded of a paper "On the Anatomy of the *Lamellibranchiate Conchiferous Animals*, by Robert Garner, Esq., F.L.S.," a portion of which had been read at the meeting on November 24, 1835.

Founded principally on the author's individual observations, which have extended to the animals of several genera the anatomical structure of which is hitherto insufficiently known, this communication embodies also much information derived from the works of Poli, Cuvier, Bojanus, Home, M. de Blainville, and others. It is so arranged as to constitute a condensed memoir on the subject to which it is devoted, comprehending a summary of all that is yet known respecting it.

After some general remarks on the high importance of a knowledge of the structure of the animals that form those shells which have at all times attracted the attention of the curious, but to an acquaintance with which many naturalists, until of late years, have been content to limit themselves, Mr. Garner proceeds to speak of the position of the animal with respect to the shell; and thence to describe the variations in the form of the animal which occasion those appearances in the shell on which rest the primary subdivisions made by conchologists among the *Lamellibranchiate Conchifera*. He regards *Anomia* as being in some measure intermediate between this order and the *Brachiopoda*; and in illustration of this view describes with some detail the structure of the animal of that genus.

Mr. Garner then adverts to the mode of growth of the shells and to their structure, and considers them in the variations in form which some of them undergo in their progress from the embryo to the adult state. He dwells also on the diversity of form assumed by the several groups of *Bivalves*, and shows in what manner these are occasioned by the form of the animal that produces the shelly coverings; referring to the foot especially as exercising in this respect a very remarkable influence.

The general review of the external form of the animal is succeeded by an account of the several systems of which it is composed. These are treated of in the following order: 1. Muscular system; 2. Nervous system; 3. Digestive system; 4. Circulating system; 5. Respiratory system; 6. Excretory system; 7. *Cilia* (and into this part of his subject the author enters with more than usual detail); and, 8. Reproductive system. Under each of these heads a rapid review is taken of the principal variations that occur in the order, and the illustrative examples referred to are generally numerous.

Finally, the author devotes a section of his paper to the diseases and the parasites of the animals on which he treats.

In conclusion, Mr. Garner submits the subjoined tabular view of an

Anatomical Classification of the Lamellibranchiate Conchiferous Animals.

With but one adductor muscle. Monomyaria, *Lam.*	
Tentacles very long, not distinct from the *branchiæ*; an additional muscular system	*Anomia.*
Tentacles short, separate from the *branchiæ*.	
No foot	*Ostrea.*
A foot.	
Branchiæ disunited medianly.	
Foot long, cylindrical; *ocelli* at the edge of the mantle	*Pecten.*
Foot short, thick, with a disk at the extremity, from the centre of which depends a pedicellated oval body; *ocelli*	*Spondylus.*
Foot compressed; no *ocelli*	*Lima.*
Branchiæ conjoined medianly	*Vulsella.**
With two adductor muscles. Dimyaria, *Lam.*	
Mantle without separate orifices or tubes.	
Foot slender, byssiferous; tentacles fixed	*Avicula.**
Foot thick, rounded, with a callosity	*Arca.*
Foot compressed, securiform	*Pectunculus.*
Foot oval below, its margin tentacular, tentacles volute	*Nucula.*
Foot large, pointed anteriorly, bent at an angle	*Trigonia.**
Mantle with a distinct anal orifice.	
Foot small, byssiferous.	
Anterior muscle small; retractile muscles of the foot numerous; byssus large.	
Byssus divided to its base	*Mytilus.*
Byssus with a common corneous centre	*Modiola.*
Anus furnished with a long ligulate valve	*Pinna.**
Muscles equal; two pairs of retractile muscles only; byssus rudimentary	*Lithodomus.*
Foot large, not byssiferous	*Unio.*
Mantle with a superior and inferior orifice; not elongated into tubes.	
Mantle widely open	*Cardium.*
Mantle closed around the foot or byssus.	
Foot short and discal, byssiferous; anterior muscle small	*Tridacna.**
Foot small, cylindrical, bent at an angle; lips foliated	*Chama.**
Foot small, sharp; lips simple	*Isocardia.**

Mantle with two produced tubes, or siphons.
- *Branchiæ* not produced into the lower tube.
  - Mantle closed around the foot .... *Loripes*.*
  - Mantle open.
    - Tubes disunited; foot lanceolate.
      - Foot large, rather falciform; external *branchiæ* shortened; mantle tentacular; labial tentacles large...... *Donax*.
      - Foot small; external *branchiæ* shortened; edge of the mantle simple; tentacles small .... *Psammobia*.
      - Foot moderate; external *branchiæ* as long as the internal; tentacles large; margin of the mantle entire .... *Tellina*.
      - Foot small; *branchiæ* equal; mantle tentacular .... *Amphidesma*.
    - Tubes more or less united; foot various.
      - *Branchiæ* united medianly.
        - Tubes small, partially divided; foot very long, obtuse .... *Cyclas*.
        - Tubes small, united to the extremity; foot very long and pointed.................... *Mactra*.
        - Tubes large, foot short and prominent behind .... *Venerupis*.
      - *Branchiæ* disunited medianly.
        - Foot lanceolate, prominent behind; tubes small, united .... *Cytherea*.
        - Foot securiform; tubes larger and more or less distinct.......... *Venus*.
- *Branchiæ* produced into, or attached to, the lower tube; tubes always united.
  - Mantle only open inferiorly for the protrusion of the foot.
    - Tubes small; lips long.
      - Foot small; *branchiæ* of each side united into one................ *Pandora*.
      - Foot larger; *branchiæ* separate .... *Corbula*.
    - Tubes long; lips small.
      - Foot not byssiferous; tubes large and coriaceous.................... *Mya*.
      - Foot byssiferous; tubes moderate .. *Hiatella*.
  - Mantle open anteriorly.
    - Foot long, club-shaped; tubes short .. *Solen*.
    - Foot very short, rounded.
      - Two distinct adductor muscles, the anterior one situated below a reflected portion of the mantle uniting the beaks instead of a cartilage; tentacles large .... *Pholas*.

Body very elongated; adductor muscles united; end of the mantle with two calcareous pieces; tentacles small; no cartilage nor reflected portion of the mantle .... *Teredo.*

For the anatomy of the several genera marked in the above table with an (*), the author acknowledges himself indebted either to Cuvier, Poli, or M. de Blainville.

He refers occasionally to other genera, besides those enumerated, as included in the groups distinguished by the characters given above.

Mr. Garner's paper was accompanied by numerous drawings of the objects and structures described in it, which were exhibited in illustration of his communication.

Feb. 23.—Mr. Gould, at the request of the Chairman, exhibited specimens of numerous *Birds* forming part of the Society's collection; and directed the attention of the Meeting to those which he regarded as the most interesting among them.

He stated that one of them was especially curious as exhibiting a form of *Insessorial Bird*, not safely referrible to any known family; on which account he proposed to consider it as the type of a group to be designated

PARADOXORNIS.

Rostrum altitudine longitudinem superans, ad basin vibrissis instructum: *mandibulâ superiore* valdè compressâ; culmine acuto, valdè arcuato; tomio edentulo, apicem versus valdè incurvo, ad basin producto: *mandibulâ inferiore* ad basin latâ, robustâ; tomio emarginato.

Nares parvæ, rotundatæ, pone rostrum sitæ.

Alæ breves, rotundatæ: *remigibus* 4tâ, 5tâ, et 6tâ longioribus.

Cauda mediocris, gradata.

Tarsi robusti, læves.

Pedes magni, subtùs lati: *digitis* magnis; *halluce ungueque postico* maximis.

Ptilosis ampla, laxa.

The breadth of the under surfaces of the feet is so great as to indicate considerable powers of grasping.

PARADOXORNIS FLAVIROSTRIS. *Par. arenaceo-brunneus, subtùs pallidior; capite nuchâque rufo-brunneis; auribus partim aterrimis; facie guttureque albis nigro variis; pectore nigro.*

Long. tot. 8 unc.; *alæ*, $3\frac{1}{2}$; *caudæ*, $4\frac{1}{2}$; *tarsi*, $1\frac{1}{4}$; *hallucis* (arcuati), $\frac{7}{8}$.

Rostrum splendidè aurantiaco-flavum; *pedes* cœrulescentes.

Hab. (verosimiliter) in Nepaliâ.

Mr. Gould regarded another of the *Birds* exhibited as the representative of a new type among the *Thrushes*; and characterized it as the type of the genus

ACTINODURA.

Rostrum subcompressum, subarcuatum, ad apicem subemarginatum.

Nares basales, lineares, operculo magno tectæ.

Alæ molles, breviusculæ, concavæ: *remige* 1mâ brevissimâ, 4tâ 5tâque longioribus.

Cauda mollis, elongata, gradata.

Tarsi elongati.

Pedes majusculi: *halluce ungueque postico* longiusculis.

Ptilosis mollis, laxa.

The wings and tail in the birds of this group are transversely barred. The typical species are crested.

Actinodura Egertoni. *Act. cristata; suprà nitidè rufo-brunnea olivaceo tincta, subtùs pallidè rufo-brunnea; cristâ, occipite, genisque brunnescenti-cinereis; remigibus ad basin rufis, pogoniis nigro flavoque fasciatis; secundariis nigro brunneoque fasciatis; rectricibus sordidè rufo-brunneis, lineis saturatioribus transversim notatis, alboque apiculatis.*

Long. tot. $8\frac{1}{2}$ unc.; *alæ*, $3\frac{3}{4}$; *caudæ*, $4\frac{1}{4}$; *tarsi*, $1\frac{1}{4}$; *rostri*, 1.

*Rostrum pedes*que brunnei.

Hab. in Nepaliâ.

The specimen described was presented to the Society by Sir P. Grey Egerton, Bart., M.P.

The following species were also characterized by Mr. Gould . viz. *Corvus pectoralis, Corv. curvirostris, Prionites cæruliceps,* and *Plyctolophus productus,* of which the charactèrs are given in the Proceedings.

XLVIII. *Intelligence and Miscellaneous Articles.*

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SIXTH MEETING.

THE sixth meeting of this Association, held at Bristol, has been highly satisfactory in every respect, both as regards the number and the eminence of the men of science who attended, the quantity and value of the communications, and the zeal and cordiality which were manifested. A general outline of the proceedings has been given in the daily and weekly papers: we shall, we conceive, best promote the interests of science by reserving our pages as last year for authentic and official details. We may, however, just notice the subjects of two of the communications, on account of the lively interest which they excited.

On the change in the chemical character of Minerals induced by Galvanism. By R. W. Fox.

Mr. Fox mentioned the fact, long known to miners, of metalliferous veins intersecting different rocks containing ore in some of these rocks, and being nearly barren or entirely so in others. This circumstance suggested the idea of some definite cause; and his experiments on the electrical magnetic condition of metalliferous veins, and also on the electric conditions of various ores to each other, seem to have supplied an answer, in as much as it was thus proved that electro-magnetism was in a state of great activity under the earth's surface; and that it was independent of mere local action between the plates of copper and the ore with which they were in contact, was shown by the occasional substitution of plates of zinc for those of copper producing no change in the direction of the voltaic currents. He also referred to other experiments, in which two different varieties of copper ore, with water taken from the same mine, as the only exciting fluid, produced considerable voltaic action. The various kinds of saline matter which he had detected in water taken from different mines, and from different parts of the same mine, seemed to indicate another probable source of electricity; for, can it *now* be doubted, that rocks impregnated with or holding in their minute fissures different kinds of mineral waters, must be in different electrical conditions or relations to each other? A general conclusion is, that in these fissures metalliferous deposits will be determined according to their relative electrical conditions; and that the direction of those deposits must have been influenced by the direction of the magnetic meridian. Thus we find the metallic deposits in most parts of the world having a general tendency to an E. and W. or a N.E. and S.W. bearing. Mr. Fox added, that it was a curious fact, that on submitting the muriate of tin in solution to voltaic action, to the negative pole of the battery, and another to the positive, a portion of the tin was determined like the copper, the former in a metallic state, and the latter in that of an oxide, showing a remarkable analogy to the relative position of tin and copper ore with respect to each other as they are found in the mineral veins.

The Chairman (Dr. Buckland) said, it had been observed to them last evening, that the tests of some of the highest truths which philosophy had brought to light was their simplicity. He held in his hand a blacking-pot, which Mr. Fox had bought yesterday for a penny, a little water, clay, zinc, and copper; by which humble means he had imitated one of the most secret and wonderful processes of nature, her mode of making metallic veins. It was with peculiar satisfaction he contemplated the valuable results of this meeting of the Association. There was also a gentleman now at his right hand, whose name he had never heard till yesterday, a man unconnected with any Society, but possessing the true spirit of a philosopher; this gentleman had actually made no less than 24 minerals, and even crystalline quartz. He (Dr. B.) knew not *how* he had made them, but he pronounced them to be discoveries of the highest order: they were not made with a blacking-pot and clay, like Mr. Fox's, but the apparatus was equally humble; a bucket of water and a brickbat had sufficed to produce the wonderful effects which he would detail to them.

Artificial Crystals and Minerals.—A. Crosse, Esq., of Broomfield, Somerset, then came forward, and stated that he came to Bristol to be a listener only, and with no idea that he should be called upon to address a Section. He was no geologist, and but little of a mineralogist; he had however devoted much of hi stime to electricity, and he had latterly been occupied in improvements in the voltaic power, employing a battery which he had succeeded in keeping in full force for twelve months by water alone, rejecting acids entirely. Mr. C. then proceeded to state that he had obtained water from a finely crystallized cave at Holway, and by the action of the voltaic battery had succeeded in producing from that water in the course of ten days numerous rhomboidal crystals, resembling those of the cave: in order to ascertain whether light had any influence in the process, he tried it again in a dark cellar, and produced similar crystals in six days, with one fourth of the voltaic power. He had repeated the experiments a hundred times, and always with the same results. He was fully convinced that it was possible to make even diamonds, and that at no distant period every kind of mineral would be formed by the ingenuity of man. By a variation of his experiments he had obtained green and blue carbonate of copper, phosphate of soda, and 20 or 30 other specimens.

Mr. Crosse having also observed in a cavern in the Quantock Hills near his residence that the part of it which consisted of slate was studded with crystals of arragonite, while the limestone part was covered with crystals of ordinary carbonate of lime, or calcareous spar, subjected portions of each of these substances in water to long-continued galvanic action, and obtained from the

Slate	Limestone
Crystals of Arragonite.	Calcareous Spar.

It was mentioned to the Section on the following day, that although no doubt could be entertained of the independence and originality of Mr. Crosse's experiments, yet that he had been anticipated, in the artificial production of many of the crystallized bodies which he had formed, by M. Becquerel, and some other French chemists.

AURORA BOREALIS OBSERVED IN THE ISLE OF WIGHT, ON AUGUST 10TH.

An aurora borealis was observed at Ryde, Isle of Wight, on the night of Wednesday the 10th instant (August), which for its intensity, its occurrence so far south, and at such a time of the year, is worth recording. It was first observed about half-past eleven o'clock. There was the usual dark band upon the horizon under the magnetic north, and to some distance right and left of that point: at intervals of time the space above this became luminous, like a glow of twilight, and this broke out into columns, nearly upright, but inclining above to the east, and, as far as could be judged by the eye, parallel to each other, and not convergent to the zenith or any other point in the heavens. They were gradually transferred by a slow motion from west to east, and generally ended by assuming a fine red colour. The appearance continued for an hour or more, but about one o'clock all had resumed the ordinary state. The stars were out, twinkling very much, and falling stars were observed frequently on that and the three or four preceding evenings. M. F.

ON THE REDUCING POWERS OF ARSENIOUS ACID. BY G. BONNET, ORNSHAGEN, POMERANIA.

When arsenite of copper is put into a solution of caustic potash or soda the colour of the mixture turns to a yellowish brown. This effect takes place at ordinary temperatures, quicker or slower according to the strength of the alkali; upon application of heat the process is hastened and perfected. The same effect also occurs when arsenite of potash or soda is mixed with a solution of any salt of copper, and a caustic alkali is added to it. It also takes place on mixing arsenite of potash or soda with hydrated oxide of copper, and then adding a caustic alkali; the change is very slow with heated oxide of copper. Upon examination of the precipitate after washing the following action takes place.

Diluted sulphuric acid is coloured blue, and metallic copper is left; muriatic acid added in a small quantity changes the precipitate into a white powder, a larger quantity gives a brown solution; from which it follows that the precipitate is a suboxide of copper; this is confirmed by the experiments which follow. The reduction of the oxide of copper can only be attributed to the effect of the arsenious acid, which it is to be observed is converted into arsenic acid. 100 parts of arsenious acid, taking 16·1 parts of oxygen for its conversion into arsenic acid will therefore convert 159·6 of oxide copper into suboxide. In order to ascertain whether the reduction of the oxide of copper by the addition of the ingredients in this proportion would also result, as well as by the employment of arsenite of copper in which a larger quantity of arsenious acid was present, the following experiments were made:

1·6 gramme oxide of copper were dissolved in very dilute sulphuric, and 1 grm. of arsenious acid was dissolved in a solution of 10 grms. of caustic soda of specific gravity 1·20; the solutions were mixed together cold, and 20 grms. caustic alkali were added; the mixture was placed in a warm situation and often stirred; the colour soon changed and the precipitate completely formed. The solution was poured off, and the suboxide copper was washed with warm water and separated.

The solution was neutralized by nitric acid; sulphate of copper gave a blue precipitate, and nitrate of silver a reddish brown precipitate, indicating the presence of arsenic acid.

The washed precipitate mixed with boiled water acidulated by sulphuric acid changed immediately to brown flakes of metallic copper; the supernatant liquid was poured off, and the copper well washed with boiling water, collected and weighed; it weighed 0·62 gram.

One half of 1·6 gramme of oxide of copper will give 0·638 metallic copper. The result of the foregoing experiment agrees nearly with this; it may be considered therefore that all the oxide of copper is reduced by arsenious acid to the state of suboxide. If this experiment were repeated by taking double the quantity of arsenious acid the same result would take place, and the oxide of copper would be only reduced to the state of suboxide.

When caustic ammonia is used, the oxide of copper is only partially converted into suboxide. The solution remains perceptibly blue, and only becomes colourless upon the addition of a caustic alkali. If the blue ammoniacal solution is supersaturated by sulphuric acid, metallic copper is precipitated.

The carbonated alkalies occasion no perceptible reduction of oxide of copper, which is also the case with caustic lime.

Upon considering the reducing power of arsenious acid on oxide of copper, it is probable that oxygen may be separated from other metallic oxides; particularly in the case of the metallic acids which easily part with their oxygen. Several trials on metallic oxides gave negative results, but the idea was confirmed with regard to metallic acids.

Manganesiate of potash was dissolved in water, a caustic alkali, and then arsenite of soda were added; upon mixing, the green colour was restored, a dark brown precipitate fell, which was oxide of manganese.

Chromate of potash was dissolved, caustic alkali and arsenite of soda added, the solution on warming became green. In this case no oxide of chrome fell. The arsenious acid prevented the precipitation of the oxide of chrome by means of the alkali, which is evident by the following experiment.

Hydrated oxide of chrome was dissolved in muriatic acid, to which was added a solution of arsenite of potash: caustic alkali did not give the least trace of oxide of chrome. When to the solution of oxide of chrome in muriatic acid, arsenite of ammonia, and then ammonia, were added, no precipitate took place. It is to be remarked that arsenious acid has also a reducing power over other acids, for example, on molybdic and tungstic acid, but for want of these acids I was unable to make the necessary experiments.—*Poggendorff's Annals*, 1836, No. 2.

ON THE COMPOSITION OF PLAGIONITE. BY RUDERNATSCH.

This mineral has been analysed by Professor H. Rose, who found it to consist of

Lead..........	40·52
Antimony......	37·94
Sulphur........	21·53

and has given the formula of $4\ Pb\ S + 3\ Sb\ S^3$.

As this combination of sulphur in sulphuret of antimony is in a very uncommon proportion to the sulphuret of lead, namely as 9 to 4, and as Berzelius, in his *Jahresbericht*, has thrown some doubt on the existence of such a compound, which he considers likely to be a mixture of the two, Mr. Rudernatsch was induced to repeat the examination of some very distinct and well-defined crystals: he followed the same mode of analysis as Professor Rose, which gave the following results:

Lead	40·98	
Antimony......	37·53	
Sulphur........	21·49	—100·

In a second analysis, which was only to determine the quantity of lead, he found 40·81 per cent.

It therefore appears, by these analyses, that plagionite is a peculiar chemical combination.—*Poggendorff's Annals*, 1836, No. 4.

ON SOME TRIPLE COMBINATIONS OF CHLORIDE OF OSMIUM IRIDIUM AND PLATINUM WITH CHLORIDE OF POTASSIUM AND MURIATE OF AMMONIA. BY R. HERMANN OF MOSCOW.

1.—*Triple Salt of Chloride of Osmium and Chloride of Iridium with Chloride of Potassium.*—When the native mixture of iridium and osmium, as it is found in the platinum sand in the Uralian mountains, is mixed with chloride of potassium, and a current of chlorine is passed through it; the whole being heated, a combination takes place: when the solution is evaporated, dark brown, or nearly black octahedrons are deposited. They are composed of

Iridium	26·6	
Osmium.....................	13·4	
Chlorine and chloride potassium	60·	——100·

giving the formula Os $Cl^4 + 2 Ir Cl^4 + 3 K Cl^2$.

If this salt is mixed with its own weight of dry carbonate of soda and heated in a retort, one portion of the osmium is sublimed in the neck of the retort as a binoxide. By heating the residue in contact with the air, a further portion of osmium is sublimed. The remaining sesquioxide of iridium retains only a small quantity of oxide of osmium, which by digestion in nitro-muriatic acid and heating may be entirely driven off.

2.—*Triple Salts of Chloride of Iridium and Platinum with Muriate of Ammonia.*—This salt is formed in abundance in the platinum manufactory at St. Petersburg on the evaporation of the solutions from which platinum is precipitated by sal ammoniac. Hitherto it has been considered as a combination of chloride of iridium and muriate of ammonia. It is found however to contain, besides chloride of platinum and ammonia, a small quantity of palladium.

100 parts are composed of 31·76 iridium,
10·59 platinum,
1·25 palladium,
56·40 chlorine and muriate of ammonia.

It is therefore a combination of 1 atom chloride of platinum, 3 atoms chloride of iridium, and 4 atoms muriate of ammonia, with a small quantity of chloride of palladium and ammonia.

3.—*Triple Salt of Chloride of Iridium and Platinum with Chloride of Potassium.*—When the sesquioxide of iridium and platinum is digested with muriatic acid, the solution gives off the smell of chlorine; chloride of iridium and chloride of platinum are dissolved by the acid. If nitrate of potash is added to the solution, the yellow colour changes to a wine red, and by the addition of chloride of potassium dark red octahedral crystals are deposited on evaporation; this salt is composed of 8·00 iridium,
32·00 platinum,
60·00 chlorine and chloride of potash,

giving the formula Ir $Cl^4 + 4$ Pl $Cl^4 + 5 K Cl^2$.

This action of muriatic acid on a mixture of sesquioxide of iridium with platinum should not be overlooked in the analysis (according to the direction of Berzelius) of platinum sand. Berzelius directs that the mixture of the oxides of platinum, rhodium, and iridium, which remains after the precipitation by carbonate of soda of the double alkaline salts, should be digested with muriatic acid, in order to withdraw a portion of the alkali with which the oxides of rhodium and iridium have combined. By this means iridium and platinum will in every instance be held in solution by the muriatic acid, which is necessary to be kept in view in order to obtain a correct analysis.—*Poggendorff's Annals*, 1836, No. 2.

NOTICE OF THE LIFE AND CONTRIBUTIONS TO SCIENCE OF THE LATE M. NOBILI.

The following notice of the scientific labours of M. Nobili, from the pen of Professor A. de la Rive, is translated from an article in the *Bibliothèque Universelle.*

"It is with a feeling of the deepest grief that we announce the loss which science has recently experienced in the person of the Chevalier Leopold Nobili of Reggio. This distinguished natural philosopher died at Florence, on the 5th [17th?] of August 1835, at an age which gave the hope of a still longer continuance of his life: it appears that he sank under an affection of the chest. This loss, felt by all the learned world, is especially so by the editors of the *Bibliothèque Universelle,* to which M. Nobili was in the habit of consigning his important researches. Our journal has had the advantage of making known the first labours of the learned Italian, and of publishing thenceforward successively all the others. We therefore believe that we obey a sacred duty, and discharge the debt of a very natural gratitude, in endeavouring to recall in a few words the principal services which M. Nobili has rendered to science.

"After having occupied himself with investigations respecting magnetism and light, purely theoretical, M. Nobili began in 1825 to devote himself to experimental researches. He commenced by inventing the galvanometer with two needles, which has since rendered such great services to experimental philosophy, and which has been generally adopted; its description will be found in the *Bibliothèque Universelle,* vol. xxix. p. 119. More recently, he added to this first invention that of the comparative galvanometer. But the series of investigations which in an especial manner made M. Nobili known to the learned world, was that relative to the colours developed upon metallic plates acting as poles in the electro-chemical decomposition of different solutions. The discovery of this brilliant phænomenon, the study of all the circumstances which accompany and modify the production of these coloured rings, were the object of two important memoirs, which, inserted at first in vols. xxxiii. and xxxiv. of the *Bibl. Univ.,* were afterwards republished in most of the scientific journals.

"It was while pursuing the examination of this subject, that M. Nobili succeeded in demonstrating the cause of the electro-chemical motions of mercury, (*Bibl. Univ.* vol. xxxv. p. 161,) and in discovering in the de-formation which the coloured appearances undergo in certain cases, the existence of a reciprocal action exerted by electrical currents, and analogous to the *interference of luminous rays.* (*Bibl. Univ.* vol. xxxvi. p. 3.) Some years later, he resumed the questions which related to the form and the production of these electro-chemical appearances, and succeeded in employing them as a valuable criterion to follow the elementary electrical currents in their progress, their distribution, and their mode of grouping. He thus deduced from them important consequences respecting the interior mechanism of the pile, and the manner in which electricity distributes itself in it. (*Bibl. Univ.* vol. lvi. p. 150.)

"Whilst studying the electro-chemical appearances, in reference to the light which they might throw upon everything connected with the electric current, M. Nobili did not neglect to consider them independently, and with a view to the applications to the arts which they might present. He has described in a long memoir (*Bibl. Univ.* vol. xliv. p. 337, and vol. xlv. p. 35,) the series of processes by which he succeeded in constructing, by means of the electro-chemical appearances, a chromatic scale which presents every degree and all the different blendings of colours; this essay also contains many ingenious views respecting the theory of colours in general*. We cannot terminate what we have to say concerning this part of M. Nobili's researches, without again insisting on the beauty and variety of the colours which are obtained by the method which he has devised; we will add, that their permanency appears to us truly remarkable, and we do not doubt that sooner or later art will possess itself of the process, and derive great advantages from it. As to the cause of the phænomenon of the electro-chemical appearances, the learned Italian has scarcely occupied himself with the question; but it appears to us beyond doubt, that they are owing to a very strongly adherent deposition formed on the metallic plates, by excessively thin films of the substances decomposed by the electric current: this point, however, merits investigation.

"The analysis of the electro-physiological effects, obtained when a frog forms part of the circuit, and of the consequences in relation to animal electricity which may be drawn from them, was the object of laborious researches on the part of M. Nobili. (*Bibl. Univ.* vol. xliv. pp. 48 and 165.) He had already shown in a preceding essay, entitled '*Comparison between the two most sensible Galvanometers, the Frog, and the Multiplier with two needles,*' (*Bibl. Univ.* vol. xxxvii. p. 10,) that the frog may give rise, by itself and without any external agent, to an electrical current†; he had studied the direction of this current, and the circumstances which modify its direction and its intensity. But in the memoir which we have first quoted, he had deeply studied the phænomena which result from the combined action of the current of the frog and the external currents, and he had arrived at remarkable results respecting the influence of the direction of these last currents, in reference to the effects upon the animal economy which result from them. He lately returned again to this subject, (*Bibl. Univ.* vol. xlvii. p. 174,) less for the purpose of adding new facts to it, than to combat M. Marianini, who was also engaged in this question, but who had arrived at entirely different results. We confess that we do not conceive why such di-

* A translation of M. Nobili's Memoir here noticed, will be found in the *Scientific Memoirs*, Part I., published on the 1st of August.—Edit.

† Had not this been shown long ago, quite early in the history of Voltaic electricity?—Edit.

stinguished philosophers as Messrs. Nobili and Marianini have devoted so much time to researches of so little general interest, and in which it is so difficult to arrive at any precise and well-determined result, and we even regret that they have done so.

"In the memoir which we have cited above, (*Comparison, &c.*) and in a subsequent note, (*Bibl. Univ.* vol. xxxvii. p. 174,) M. Nobili had cited several remarkable experiments on the development of electric currents by chemical action, and on the laws of this development. He had placed beyond doubt the fact, disputed by Davy, of the production of electrical currents by chemical action, and had shown this production, in the case of simple solutions and double decompositions, as well as in the others. He had found no relation between the intensity of the currents obtained, and the intensity of the chemical action, or the electric nature of the combined elements. But having obtained sensible currents by applying heat to liquid or humid bodies (such as moistened clay), he had thence concluded that the electric currents developed in chemical action, are owing to the heat which always accompanies it. Pursuing this idea, he developed it more completely in a memoir, entitled '*On the Nature of Electric Currents.*' (*Bibl. Univ.* vol. xxxvii. p. 118.) In this paper, the author successively passes under review the currents which take place without chemical action, or the *thermo-electric* currents, and those which are accompanied by chemical action, or the *hydro-electric* currents. While occupying himself with the first, he carefully studies the case when the circuit contains only a single metal, that in which it contains two, that in which the circuit is humid, that in which it is mixed: in these two last cases, the current appears to him to proceed always from the hot to the cold part; in the two first this law appears to suffer exceptions. In the examination which he makes of the electric currents, M. Nobili finds also that the electric effects follow the course of the heat, more sensibly in the cases in which one of the elements is solid, because the heat is concentrated in it, than in the case when both are liquid, because the heat is there disseminated. Summing together all the proofs of the identity of caloric and electricity, he finally arrives at the conclusion, that the electric current is only caloric in motion. The inverse conclusion might with equal reason have been arrived at, that caloric is only electricity in motion; the truth is probably neither in one nor the other of these two identities, round which philosophers have, as it were, for a long time revolved. Singular weakness of the human mind, which, because effects resemble each other, absolutely requires that one be the cause, the other be the effect, as if they could not both depend on a more general cause common to both!

Whatever be the judgement awarded respecting the merit of the theory which we have just called to mind, the memoir of M. Nobili will remain not less remarkable, from the curious facts with which it has enriched science. Thenceforward, the subject of thermo-electric currents never ceased to occupy the learned Italian: he especially succeeded in making the thermo-electric pile the most sensible of thermoscopic instruments. His first attempts of this kind are recorded in the *Bibl. Univ.* vol. xliv. p. 225; more recently, in conjunction with M. Melloni, he has exemplified, by researches on the passage of radiant caloric through bodies, all the advantage that might be obtained from this new instrument placed in the hands of experimental philosophers. It is well known to what admirable discoveries M. Melloni has since made it subservient; but we ought not to forget that we owe the first idea to M. Nobili.* Recently again (*Bibl. Univ.* vol. lvii. p. 1,) he has published, accompanying it with some curious results, a description of two new forms of the thermo-electric pile, adapted to augment its degree of sensibility, already very great, and to suit it to certain calorific researches, for which it could not be used in its original form.

"M. Nobili had also made some investigations relating to magnetism (*Bibl. Univ.* vol. lvi. p. 82); he had studied in particular the magnetic state which is frequently imparted to the wires of a galvanometer by the vicinity of magnetized needles. But he paid particular attention to the phænomena of the currents developed by the induction of magnets. At the first news which he received of Mr. Faraday's discovery, he set to work with M. Antinori to explore this new and curious subject, (*Bibl. Univ.* vol. xl. p. 127.) These two experimentalists obtained, like Mr. Faraday, all the effects of electric currents, by employing solely the induction of a magnet; they especially succeeded in thus producing the electric spark, and in demonstrating that M. Arago's experiment of the rotating disc is satisfactorily explained by means of the electric induction of magnets†.

"M. Nobili was besides lately engaged in varous interesting researches: we have before us a memoir by him, dated the 25th of January of this year, the first part of which we insert in the present Number of the *Bibl. Univ.*; its subject is '*the distribution and effects of electric currents in conducting bodies.*' We are not aware that he has since finished any other labours; if, however, it should be other-

* Translations of the first two memoirs, by M. Melloni, on the transition of radiant heat have been given in Part I. of *Scientific Memoirs*; and the series will be continued in Part II., which will be published on the 1st of November next.—Edit.

† We must beg leave to express our dissent from the representations made by M. de la Rive, on these two subjects. In Phil. Mag. and Annals, N. S. vol. xi. p. 401, will be found a translation of a paper by MM. Nobili and Antinori, "On the Electro-motive Force of Magnetism," with notes by Mr. Faraday, in which it is shown, that M. Nobili and his coadjutor obtained the electric spark from a common magnet before Mr. Faraday had obtained it, simply in consequence, to use their own words at the conclusion of their paper, of their having "entered into a path before they knew all the steps taken in it by the illustrious philosopher who threw it open." After Mr. Faraday had discovered the means of eliciting the spark from the electro-magnet, to obtain it from the common magnet was a direct consequence of that discovery. Mr. Faraday also shows in these notes, that MM. Nobili and Antinori had altogether mistaken the character of the acting causes in Arago's experiments, the true theory of which he had himself, in fact, fully developed. A more detailed statement on this latter subject will be found in Mr. Faraday's letter, to M. Gay-Lussac, in the *Annales de Chimie et de Physique*, vol. li. pp. 404, 409, &c.—Edit.

wise, we shall hasten to publish whatever other researches have been made by this skilful philosopher.

"In recapitulation: during the ten years of his life which M. Nobili devoted to the sciences, he was principally occupied with electricity and magnetism, and the results of his numerous and interesting researches in this department of physics may be arranged under the following heads: —

"1st, The improvement of galvanometers and the invention of the thermoscope-multiplier.

"2ndly, The discovery of the electro-chemical appearances upon the metallic poles, and the study of the distribution of electric currents.

"3rdly, Investigations relating to electro-physiological phænomena.

"4thly, Researches relating to the production of electricity by heat and chemical action, and to the relations which subsist between these two modes of developing electricity.

"5thly, The study of magnetism, and more particularly of the production of electric currents by the induction of magnets.

"After having been momentarily interrupted in his labours by the events of 1831, M. Nobili resumed them, not long after, with renewed activity. Placed in that Museum of Florence in which the Academicians del Cimento performed their experiments; having at his disposal all the resources which this city, so worthy of the recollections which it excites, presents; honoured by the favour of a prince, at the same time the distinguished amateur and enlightened friend of the sciences; surrounded by cooperators of the highest merit, what would not M. Nobili have done? he whom we had seen, after a youth devoted to the military profession, notwithstanding the little assistance offered him by his native city, acquire in five years the scientific reputation which he enjoyed in 1830. Alas! why was it that a premature death came to destroy hopes so well founded, and to deprive physical science, which numbers so few at present, of one of its most able and devoted followers!

"A. DE LA RIVE.

"Geneva, 30th September, 1835."

We add the subjoined particulars from a notice of M. Nobili, by M. Matteucci, also given in the *Bibliothèque Universelle.*

"To trace the eulogium of so celebrated a philosopher as M. Nobili, is to write one of the brightest pages in the history of electricity; is to remind Europe, that in the country of Volta the germs of science cannot disappear; is to render homage to the prince who knew how to appreciate his labours, patronize his researches, and honour his mortal remains; it is also to discharge a debt arising from a high admiration for so many remarkable discoveries, and a sincere friendship, which encouraged me in my first steps in science, a friendship which very different scientific occupations have not since been able to change.

"Born at Transilico (Garfagnana, Ducato di Modena) in 1784, M. Nobili received his first scientific education in the military school of Modena; it was there also that his talents for the physical sciences began to develop themselves. Carried away in the military movement which swept over Italy, as well as the rest of Europe, he became Captain of Engineers, and he received on the field of battle, from the hands of the Emperor Napoleon, the Italian decoration.

"It would be difficult to describe the energy with which he related to me one day, whilst walking on the banks of the Arno, the whole of his military life, and particularly all the sufferings to which he was a prey during the too celebrated Russian campaign.

"Order and peace restored M. Nobili to his first studies, as they restored to them, by a remarkable coincidence, a man who has followed a similar route in science, M. Becquerel, who had also been Captain of Engineers.

"Appointed Professor of Natural Philosophy in the Museum of Florence, that establishment to which his celebrated friend and countryman M. Amici is attached as Astronomer, M. Nobili gave during two years, to a numerous auditory, lectures at once correct and full of new views. It is by his cares, and by those of the director of the establishment, that, thanks to the munificence of the sovereign, the *cabinet de physique* has become one of the first in Europe, particularly with respect to the history of science.

"M. Nobili was one of the forty of the Italian Society, a corresponding Member of the Royal Academy of Sciences at Paris, and of several other learned bodies; he died August 17th, 1835, of a slow *entréglite* under which he had suffered for a long time.

"A man of courage and rectitude, always lively and brilliant in his private conversation, a skilful experimentalist, a learned philosopher, he left in his friends, in the friends of Italian glory, a deep grief to have seen him so soon snatched from their friendship and from science.

"The Grand Duke of Tuscany has ordered a monument to be raised to M. Nobili, by the side of the most illustrious Italians, in the church of Santa Croce, the Italian Pantheon. Honour to the prince who thus knows how to appreciate real merit!"

METEOROLOGICAL OBSERVATIONS FOR JULY 1836.

Chiswick.—July 1—4. Very hot. 5. Excessively hot: thunder at night. 6. Thunder, which in London and its immediate neighbourhood was accompanied by hail of large dimensions: the latter, as also the lightning, did considerable damage. 7—11. Very fine. 12. Rain: very fine. 13, 14. Very fine. 15. Cloudy. 16—18. Fine. 19. Rain. 20. Showery and cold: highest temperature in the day 35° lower, in the shade, than that on the 5th. 21. Fine: thunder showers: hail in some parts of the country. 22—26. Cloudy and fine. 27. Overcast: lightning at night. 28. Very hot. 29. Heavy rain: stormy at night. 30. Showery. 31. Cloudy and fine: rain.

Boston.—July 1. Fine. 2. Cloudy. 3—5. Fine. 6. Cloudy: rain early A.M. 7—9. Fine. 10. Cloudy. 11. Fine. 12. Cloudy: rain early A.M. 13. Fine. 14. Cloudy. 15. Rain. 16. Cloudy: 17, 18. Stormy. 19. Cloudy. 20. Rain. 21. Cloudy: rain A.M. and P.M. 22. Cloudy: rain P.M. 23. Fine: rain P.M. 24. Rain. 25. Cloudy: rain A.M. and P.M. 26. Cloudy. 27. Cloudy: rain early A.M. 28. Fine. 29. Cloudy: rain early A.M.: rain P.M. 30. Cloudy: rain early A.M. 31. Fine.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. July.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Max.	Max.	Min.								
F. 1.	30·136	30·239	30·097	29·48	68·2	61·9	77·6	88	56	70	E.	W.	W.	...	...	...	62
S. 2.	30·220	30·247	30·185	29·40	75·0	65·3	76·9	88	46	71·5	SE.	W.	W.	...	...	...	67
⊙ 3.	30·275	30·219	30·203	29·55	69·6	56·4	79·9	84	47	66	SW.	SW.	calm	...	...	...	60
M. 4.	30·291	30·240	30·167	29·55	74·6	61·6	85·2	92	56	70	SW.	S.	calm	...	...	...	63
☾ T. 5.	30·170	30·165	30·120	29·41	76·2	63·8	77·8	94	63	75	E.	SE.	S.	...	·08	...	65
W. 6.	30·071	30·187	30·116	29·30	72·7	67·2	77·6	79	45	66	SSW.	NW.	NW.	·116	...	·02	70
Th. 7.	30·261	30·219	30·200	29·58	66·0	57·2	73·6	76	51	64	SSW.	SW.	NW.	...	...	...	58
F. 8.	30·305	30·271	30·257	29·65	64·5	58·3	73·5	77	48	60	WSW.	NW.	calm	...	...	...	58
S. 9.	30·257	30·220	30·162	29·60	69·6	58·9	76·7	79	55	64	SW.	W.	calm	...	...	...	59
⊙ 10.	30·158	30·135	30·111	29·38	72·0	61·4	80·7	87	56	70·5	SW.	W.	W.	...	...	...	65
M. 11.	30·142	30·089	29·890	29·38	73·3	63·8	81·2	88	60	71	SSW.	S.	NW.	...	...	...	65
T. 12.	29·734	29·946	29·706	29·	67·2	65·0	74·3	78	52	63·5	SSW.	NW.	W.	...	·01	·02	67
● W. 13.	30·075	30·041	29·969	29·36	64·5	53·8	72·2	76	54	62	SW.	SW.	W.	...	...	...	53
Th. 14.	30·002	30·002	29·962	29·30	65·4	58·6	71·7	74	51	63	SW.	SW.	W.	...	...	...	59
F. 15.	29·898	29·895	29·668	29·22	61·6	57·0	66·4	67	46	56	S.	S.	W.	...	·43	·02	59
S. 16.	29·774	29·821	29·734	29·11	61·2	50·4	69·0	70	56	57·5	SW.	W.	NW.	·316	...	·24	53
⊙ 17.	29·911	30·081	29·908	29·27	61·8	58·5	69·8	72	52	60	W.	SW.	W.	...	...	...	54
M. 18.	30·140	30·116	30·077	29·47	62·6	54·8	70·8	73	47	58	W.	W.	NW.	...	...	...	54
T. 19.	29·938	29·923	29·822	29·22	62·8	52·7	67·8	69	50	58	SSW.	SW.	S.	...	·38	...	54
W. 20.	29·560	29·699	29·441	29·00	54·3	52·8	59·2	59	43	53	E.	N.	calm	·311	·03	·04	54
☽ Th. 21.	29·677	29·738	29·658	29·07	59·2	46·2	63·2	68	47	57·5	SW.	SW.	W.	·269	·03	·06	51
F. 22.	29·794	29·897	29·778	29·17	58·5	50·3	65·0	71	47	54	WSW.	W.	N.	·233	·03	·14	51
S. 23.	30·070	30·071	30·043	29·48	60·4	51·7	65·8	70	45	58·5	SW.	W.	NW.	...	·14	·15	52
⊙ 24.	29·835	29·824	29·724	29·17	55·8	51·3	65·0	72	50	52	S.	S.	W.	·047	·15	·34	53
M. 25.	29·800	29·960	29·787	29·17	57·0	52·8	65·7	68	49	55	WNW.	W.	NW.	·186	...	·17	54
T. 26.	30·138	30·132	30·101	29·50	62·6	54·3	71·3	75	59	62	S.	SW.	W.	...	...	·05	55
W. 27.	30·168	30·140	30·107	29·44	64·7	60·2	73·6	74	47	66·5	SW.	S.	W.	...	...	·05	59
○ Th. 28.	30·091	30·059	29·903	29·38	67·5	55·8	76·8	83	59	65	SSW.	SW.	W.	...	·32	...	60
F. 29.	29·689	29·665	29·621	28·95	63·3	61·4	69·7	71	54	63	NE. var.	S.	S.	·250	·02	·08	62
S. 30.	29·990	30·289	29·928	29·30	61·6	54·9	65·5	66	46	57	SW.	W.	W.	..	·14	·22	54
⊙ 31.	30·453	30·418	30·325	29·68	59·2	50·5	66·8	68	52	58	SW.	W.	NW.	·158	·02	...	54
	30·033	30·418	29·441	29·33	68·2	57·1	71·9	94	43	62·1				Sum 1·886	1·78	1·60	582·

THE

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AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

OCTOBER 1836.

XLIX. *On the Limestones found in the Vicinity of Manchester.* *By* W. C. WILLIAMSON, *Curator of the Museum of the Manchester Natural History Society.**

SECT. I. *Introduction.*

THOSE districts in the geology of which the saliferous group forms a predominant feature, are seldom considered to possess any striking interest to the pursuer of that pleasing study. The uniformity of the deposits, the rarity of their organic remains, and their influence in producing a tameness of surrounding scenery, have caused the red sandstone districts to be regarded with anything but agreeable feelings; and this repugnance has often been the occasion of an almost total inattention to localities which, when carefully examined, have exhibited a series of results interesting to the pursuer and important to science. A stimulus is thus given to research, and even the unprolific red sandstone acquires a powerful degree of interest from its connexion with and affinity to strata replete with organic life of the most interesting kinds.

Such has in some measure been the state of geological science in the vicinity of Manchester. On the south and south-west of the town commences that extensive range of new red sandstone which diffuses itself over Cheshire, Staffordshire, and several of the southern counties, presenting nothing of in-

* Communicated by the Author.

terest in itself further than a few plain sections can afford, if we except the salt mines of Cheshire. Concerning these, as well as the variations of colour, much has yet to be learnt; but the young student finds these questions too difficult to grapple with, and the chief objects which would render the investigation more pleasing to him, organic remains, are entirely wanting.

The northern side of the town is bounded by the vast series of the Lancashire coal strata, which are always interesting, and have consequently received a proportionate degree of attention, especially from my friend Mr. Francis Looney; but the line of junction between the saliferous and carboniferous groups has been so little investigated, that the result has been an error respecting the nature and position of a series of limestones which have now become the most interesting portion of the geology of the district. This error is highly excusable, on account of the difficulty of obtaining sections of the strata. The surrounding country is so level that no natural ones can be obtained except on the banks of the Medlock at Ancoats. Human labour has exposed others on the line of the Liverpool railway and at the pits near Ardwick, where some of the limestones are worked, but these must be viewed in detail.

Mr. Elias Hall, in his geological map of Lancashire, has represented, by a green line, a series of limestones, under the general head of magnesian, passing eastward of Stockport, round the eastern and northern sides of Manchester, and extending in an undulating direction to the western coast of Lancashire, near the village of Crosby. But here he seems to have laid down the line with more distinctness than the appearances will justify, as there are few points on the range where any limestones are visible, and at three of the points out of five, from Manchester to the coast, a limestone connected with the coal-measures has been mistaken for and confounded with the true magnesian limestone. These two we must examine separately, the magnesian and the carboniferous series, and as we proceed, we shall detail the circumstances which induce us to differ from Mr. Hall's views.

Sect. II. *Magnesian Limestone.*

This stratum is but little exposed in this neighbourhood, appearing only at two localities, Collyhurst near Manchester, and Bedford near Leigh, between Manchester and the seacoast, at both of which localities the exposed portion is exceedingly circumscribed, and had we only been guided by external appearances, we should have hesitated long ere we identified the thin variegated deposit of Lancashire with the extensive lamellar series of Yorkshire, or the peculiar crystalline one of Durham.

The stratum at Collyhurst is exposed by a drain running down the side of the bank to the river Irk, where a red marly clay was visible, which on examination was found to contain fossil remains*. These were extensively collected by Messrs. Binney and Leigh, who made them the subject of a paper which was read before the Philosophical Society of Manchester and afterwards to the Geological Society of London †.

Although mistaken in his views, great praise is due to Mr. Binney, for his industry and perseverance in bringing to light a number of interesting fossils of great importance in drawing correct conclusions concerning the nature of the stratum exposed.

These gentlemen, in their paper, express a decided opinion that the fossils belong to the marles of the new red sandstone. Had this been correct, the discovery would have been a valuable addition to English geology; but subsequent investigation has proved that it is not. The portion of stratum exposed is so circumscribed and buried by the diluvial mass above, that little reliance can be placed upon the results of an examination of its relative position. What then must we turn to?—Its organic remains. Some of these were first shown to me by Dr. Charles Phillips, of Manchester, towards the latter part of October, amongst which I recognised a specimen of *Axinus obscurus*, which afterwards proved to be one of its most abundant fossils. From this circumstance I was of opinion that the stratum would prove to be one of the divisions of the magnesian limestone, and such a statement I laid before the Philosophical Society at the time Mr. Binney's interesting paper was read. The following section was given by that gentleman, which in its general features corresponds with what I have observed.

	Inches.
a. Top. Variegated marles. No organic remains	6
b. Strong red marle, traversed near its centre by a thin layer of fragile bivalve shells	5
c. Light-coloured calcareous marles, marked with lines and spots of a beautiful red	3
d. Light-coloured calcareous strong marle, containing immense numbers of univalves (Turbo) and bivalves	5
e. Clays, striped red and white, containing casts of bivalves	4
f. Light-coloured marle, similar to No. 4	3
g. Variegated marles, with an immense number of univalves and bivalves	2

The seams *d* and *f*, in the section called calcareous marles, are in fact concretionary nodules of limestone, containing spe-

* Specimens of which, as early as 1832, had been sent to London by my friend Mr. Francis Looney.

† [See our last volume, p. 571.—Edit.]

cimens of *Axinus obscurus*, and an Avicula, which will be noticed hereafter.

At Bedford the magnesian limestone reappears under a form very similar to that at Collyhurst, but has been far more extensively exposed. It has for some time been quarried for burning, but at present the pit is filled with water, so that the fine section there presented from necessity cannot now be examined. As at Collyhurst, it consists of a series of red clays, with thin beds of limestone. The limestones are in numerous thin seams, rarely more than twelve inches in thickness. Towards the upper part they become still thinner, varying generally from one to four inches, and not forming continuous seams, but layers of flattened concretions, on the surface of which are found abundance of fossils. These limestones are variable in colour; the lower and thicker seams are of a greenish yellow colour, and the thinner ones more tinged with the red substance which has given the colour to the separating marles.

Sect. III. *Fossil Remains.*

The fossils of the magnesian limestone are neither numerous nor of peculiar interest; they chiefly consist of *Axinus obscurus*, Avicula, Crassina?, Trochus, and several varieties of minute turbinated shells, probably species of Turbo. The *Axinus obscurus* of Collyhurst differs only from those of Bedford and Yorkshire in having a regular series of concentric striæ on its external surface. These I have observed in *impressions* of some from the above places; but neither the granular limestone of Bedford, nor the calcareous shelly marle of Yorkshire, is so well calculated to preserve the delicate characters of the shells as the fine clay of Collyhurst. The Axinus is found in every stage of growth, and at both localities, with the exception of one single specimen from Bedford, consists entirely of single valves. From this it would appear that the shells had been gradually covered up, the ligaments being destroyed before they were buried and protected from decomposition and disunion of the valves.

I am uncertain whether the Avicula be an undescribed species or not, but rather suppose it is. It approaches closely to *Avicula socialis* of Sowerby, which has induced some to consider the stratum as identical with the muschelkalk. I believe Professor Phillips is of opinion that the species is undescribed. The Trochus, which is a small and beautifully granulated one, as well as the minute turbinated shells, are certainly new, although some of them may be referred to by Professor Sedgwick, in his paper on the magnesian limestone of Yorkshire, without either figure or description. Two or three smaller bivalve shells occur, one of which, I believe, is Crassina, but as they are only casts, the species cannot easily be determined. The most important fossil is the Axinus, as from its extreme abundance in the magnesian limestone of Yorkshire it formed a ready means of identification.

From this it will be seen that the magnesian limestone of the neighbourhood of Manchester is an unimportant bed compared with that of many other districts. The German zechstein and kupferschiefer have their remains of the Monitor and Palæothrissum, together with the peculiar impressions of Fucoids. The limestones of Durham and Northumberland have their Zoophyta and Radiaria, with numerous Mollusca and remains of fish. In Yorkshire are thick ranges of a yellow lamellar limestone, so well exhibited on the line of the Leeds and Selby Railway, literally teeming with beautiful specimens of Axinus; and here we have only a few unimportant beds of limestone, almost lost amongst the clays in which they occur, and only exhibiting a few casts of fossils, of which but one or two (the specific characters of which are so striking as not to be mistaken,) can be distinctly recognised.

We will now examine the series of limestones, which as a means of local distinction we have called the

(Sect. IV.) *Ardwick Limestones.*

This provincial name has been given to the series of strata from the pits where they are chiefly worked being situated near the township of Ardwick. In this neighbourhood they are only exposed at the surface in the bed of the river Medlock, near Ancoats, but on the elevated ground above the river they are worked in several pits on a similar plan to the collieries. These are now the property of — Brocklehurst, Esq., and are under the management of Mr. F. Mellor, whose practical information and readiness to afford us every opportunity of examining the sections exposed in the different pits have been of the greatest importance to us.

The following is an average section of the general appearances of the strata at the above pits, in a descending order:

	Ft.	In.
Limestone	4	0
Coloured red and blue clay, by the miners called "clunch"	6	0
Coarse micaceous grit	6	0
"Clunch"	6	0
"Roofstone," a shaly sandstone	0	3
Limestone	3	0
Clunch	5	0
Limestone	2	0

	Ft.	In.	Ft.	In.
Clunch and shaly clay	17	0		
Limestone, according to Mr. Porter	1	0		
Shaly clay	15	0	79	6
Limestone, according to Mr. Porter	1	6		
Coloured clays	45	0		
Blue clay			1	0
Black bass			1	0
Coal			0	6
Blue clay, sometimes changing to red			5	0
Limestone, main seam			9	0
Coloured shaly clays (about)			60	0
Coal			1	3
Coloured clays, extent unknown.				

On the banks of the Medlock we find the following section exhibited:

	Ft.	In.
Red clays, extent unknown; one thin seam containing *Unionidæ*.		
Limestone	4	0
Red and blue clays	20	0
Limestone	3	0
Clunch	24	0
[Here the section is lost for some feet under the diluvium.]		
Red sandstone and clunch in thin laminæ	10	0
Hard seam of sandstone	1	3
Soft red lamellar sandstones	20	0
Hard grey sandstone	6	0

At this part of the section the weir is fixed, which raising the level of the water, conceals the black bass and main limestone. For a space of 360 feet, the low banks of the river consist entirely of alluvial and diluvial matter concealing the strata beneath. When they reappear, we observe the following series:

	Ft.	In.
Coarse gray sandstone	3	0
Blue marly clay	0	6
Black carbonaceous shale	0	6
Coloured clay with leaves of Stigmaria	2	0
Limestone	1	0
Coloured clays	16	0
Nodular limestone	2	0
Red sandy clay	1	8
Limestone, perhaps lenticular	0	4
Red sandy clay	3	0
Blue clay	1	0
Thin red sandstones	27	0
Shales and red sandstones	30	0
Red clays	—	

Both these sections are local ones, and of course liable to variation. The thin limestones I have no doubt will vary much both in thickness and position, and with respect to the clays, they are of little importance.

Three of the limestones are worked at Ardwick for the lime, viz. the "Four-Feet Mine," the "Yard Mine," and the main or "Three-Yards Mine." There seems to be little difference between the qualities of these, their chemical constitution as well as general appearance being very similar.

Although I have given the average thickness of the limestones in the section, their exact thickness cannot always be estimated, owing to their being divided into layers with irregular seams of clunch. These interposing clays are not much liked by the miners, being unfavourable to their process of blasting, by diminishing the extent of the fall.

This clunch, which forms an important feature in the vicinity of the limestones, consists of a dark brown clay, irregularly tinged with a bluish green substance. It is found in connexion with all the layers, and is often of considerable thickness.

Sect. V. *Mineralogical Character of the Limestones.*

The most striking character of the whole of these limestones is the singular conglomerate appearance they present. This is more or less visible in all the strata, but more especially in the first, or Four-Feet Mine. A seam of variable thickness, generally about ten inches, passes along the lower part of this limestone, possessing the conglomerate character in so striking a degree that the miners can always recognise it where it appears, although it is sometimes interrupted; they know it by the name of Maxfield, a corruption of Macclesfield: the name has no meaning in it, except from a local circumstance of no importance.

The limestone is generally of a light gray colour, often tinged with red: where the mottled appearance presents itself, the matrix is of a darker red or gray, and the imbedded fragments of a lighter hue, the ordinary colour of the stone.

All the limestones are liable to slight variations of colour, even in the different divisions of the same "mine," being sometimes of a dirty gray and sometimes of a reddish yellow. The lowest portion of the Yard Mine almost approaches to black.

The shales and clays between the limestones vary so much that a detailed description of them would be difficult. They are found of every variety, from a hard sandstone to a fine-grained strong clay or marl, which latter sometimes contains organic remains, as above the "Four-Feet" seam on the bank of the Medlock, where it contains an Unio abundantly, and about sixty yards above the main or Three-Yards seam it is filled with a variety of vegetable impressions, Ferns, Equisetæ, &c., which will hereafter be described more fully.

Some of the clays are compact, of a light blue colour, and soon become decomposed on exposure to the weather. Instances of these will be found above and below the black bass.

The most important seam to the geologist, excepting the roofs of the Four-Feet Mine, is the black bass, so called from its dark colour. The seam varies exceedingly, sometimes being less than a foot, and at others two feet in thickness, but appears to be regular in its position. It is of no use for burning, although it contains a small portion of lime. Its structure is lamellar, resembling a dark shale, but rendered exceedingly hard for the greater part of its thickness by the very large proportion of sulphuret of iron it contains, the formation of which seems to have been caused by the quantity of animal remains found in connexion with it. This pyrites is generally most abundant towards the upper part of the seam, where a thin vein of it especially affords an indication of the presence of an assemblage of remains of fish.

The number of freshwater shells and Entomostraca it contains afford good evidence of its freshwater origin, which will sufficiently separate the series from the magnesian limestone, the remains from which are all marine.

Immediately below the black bass is the seam of coal, generally about six inches thick; it is very impure, and wants the compactness of the regular coal-seams: when burned, it emits a strong sulphureous smell.

SECT. VI. *Series in the Bed of the Medlock.*

In the bed of the river Medlock we cannot examine either the main limestone or the black bass, as the former is concealed under a considerable depth of water collected by a weir, which in all probability rests upon the bass. Immediately below the weir, the other limestones are well exposed, with their separating clays and sandstones, which slightly vary from those of the Ardwick pits: I have not been able to detect the seam of plants found in the Tunnel at the latter point, but here we are able to trace the shales for a little distance above the first limestone. Nothing however of interest presents itself, except a thin seam of red marly clay, a few feet above the limestone, which contains a number of *Unionidæ*. Between the weir and the point where the water-works formerly stood, is an elevation now quarried into for the sandstones, which presents us with the section I have given of the series below the limestones worked at Ardwick. It presents but two limestones of a darker colour than those at the pits, but as one is not more than two feet four inches, including a seam of clunch which divides it, and the other is not above a foot in thickness, they are not worth following out. On the bed of the river, between this point and the weir, a coal was observed, and some of it picked out to burn in the lime-kiln. This was in all probability the same as the fifteen-inch seam met with at Ardwick.

With what is immediately below the clays forming the base of the Medlock section I am unacquainted, as the strata are not exposed, the whole length being buried under the mass of diluvium, which becomes of considerable thickness, completely covering up everything down to the level of the river.

[To be continued.]

L. *On the relative Signs of Coordinates.* *By* A. De Morgan, *Esq.**

BY the report given in the Athenæum of the proceedings of the British Association at Bristol, it appears that a discussion arose to this effect. The usual expression for the perpendicular let fall from a point (m, n) to the straight line $y = a\,x + b$, namely,

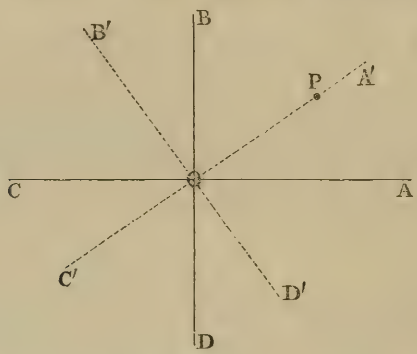
$$p = \pm \frac{a\,m + b - n}{\sqrt{1 + a^2}} \quad \ldots\ldots\ldots \quad (1.)$$

appears affected with a double sign. On the one hand, it was contended that this double sign has a meaning deducible from the existing conventions by which signs are interpreted; while on the other hand, the weight of authority seemed to be in favour of the $\pm$ sign in this case being purely indeterminate. The first opinion was advanced by Professor Stevelly; the last by gentlemen whom it is not fair to name, as their views were given on the spur of the moment, at the first reading of the communication, but whose opinions are not to be lightly treated.

On thinking of this question, it has appeared to me that according to the sense in which words are used, both are right or both are wrong, but that there is an interpretation which ought to be preferred from those strong reasons of convenience which dictate the method of choosing between + and − in other cases. Absolutely speaking, the preceding expression is certainly indeterminate; but so, for instance, is the method of fixing the sign of the angle made by a radius vector with

* Communicated by the Author.

the axis of x. If we say that this angle shall be called positive when it is formed by the rotation of a line from the positive part of the axis of x towards that of the axis of y, it is not that, *mutatis mutandis*, the negative sign would not equally apply to that angle, but because we always consider those conventions which make the resulting equations most simple as preferable, and even practically necessary. In the same way this practical necessity may be shown to exist *correlatively with another hypothesis* in the present case; and it may even be made obvious that without an implied *selection* between the two signs in (1.) many equations which are used as universally true are not so in reality. The principle I assume is, that continuity, and the universality of equations in their most simple and usual forms, are desirable.



Let O A and O B be the directions in which x and y are positive, and let P be a point whose coordinates are m and n, in that quarter of space in which both coordinates are positive. Now let moveable rectangular axes of ξ and η (new coordinates) first coincide with those of x and y, and then make a complete revolution. Let these axes be O α, O β, O γ, O δ (not drawn), and let θ be the angle (the direction of revolution being A B C D) by which O α has separated from O A to gain any position in question. Supposing that at the initial coincidence ξ and x are measured positive in the same direction (certainly the most convenient outset), the condition of continuity requires that ξ and η should remain positive until they become either nothing or infinite: the latter they cannot become for the given point P. Hence—that is to say, from this hypothesis, not of necessity, but of indisputable convenience,—it follows that when O α lies in the quadrant A′OD′,

ξ and η are both positive; when in A′ O B′, ξ is $+$ and η $-$; when in B′ O C′, ξ and η are both negative; when in C′ O D′, ξ is $-$ and η $+$. If we now take the equations of transformation in the form in which they are most usually given,

$$\left.\begin{aligned}\xi &= n \sin\theta + m\cos\theta\\ \eta &= n\cos\theta - m\sin\theta\end{aligned}\right\} \quad \dots\dots\dots \quad (2.)$$

we find these to be universally true on the conventions above mentioned, and not on any others. Let P O A $= \alpha$, and we have

$$\xi = \frac{m}{\cos a}\cos(\alpha - \theta) \qquad \eta = \frac{m}{\cos a}\sin(\alpha-\theta) \quad \dots\dots \quad (3.)$$

or $(\alpha < \frac{1}{2}\pi)$ ξ is positive from $\theta = \frac{3}{2}\pi + \alpha$ to $\frac{1}{2}\pi + \alpha$, and *vice versâ*; while η is positive from $\theta = \pi + \alpha$ to $\theta = \alpha$, and *vice versâ*. The other cases with respect to α easily follow.

To return to equation (1.), taking the line $y = a\,x$ parallel to $y = a\,x + b$ as γ O α, the axis of ξ, in which case

$$\eta = \pm\frac{n - a\,m}{\sqrt{1+a^2}},$$

this expression must by what precedes be interpreted as negative whenever O α falls between O A′ and O C′, and *vice versâ*. But as one part of the line $y = a\,x$ falls somewhere in the angles A′ O B′, B′ O C′, the preceding expression, absolutely considered, has either sign. But if we attribute one sign rather than the other to the corresponding value of ξ, we are then, simultaneously with this hypothesis, under the necessity of assigning the proper sign to η, as above determined. It must be observed that in the preceding hypothesis, if ξ be measured positively in the angle A O A′, η is positive; while if ξ be measured negatively in that angle, η is negative. Similar considerations apply to the whole perpendicular p, which is either positive or negative, according to the hypothesis made respecting the positive and negative direction of the line $y = a\,x$, and follows the same laws as any line which is made up of the sum or difference of coordinates with respect to given axes and assigned directions of positiveness or negativeness.

In the preceding considerations will be found the answer to the question relative to the sign of the radius of curvature in a curve. There is no such sign, except it be one of perfectly independent convention, until it is settled which is the positive and which the negative direction *on the tangent*. But the preceding will be sufficient to illustrate my view of the subject as contained in the following assertion: that relatively to fixed axes, with given directions of sign, a straight line oblique to the axes need not be considered as having either sign, unless in conjunction with an hypothesis relatively to the signs of lines perpendicular to it; in which case the law of signs which is absolutely necessary for continuity is the one just laid down. And by continuity in this case is simply meant the condition, that whenever a moving line comes momentarily to coincide with a given line, its relations of sign are the same as those in the given line. This being assumed, the assignment of the relations of sign in any two given perpendiculars determines a *relation* between the signs of any two other perpendiculars. The indeterminateness of equation (1.) simply implies that it was obtained without reference to any such relation. Those who are used to this subject will see how the expression of a length by means of the *tangent* of an angle instead of its *sine* and *cosine* introduces this ambiguity. When the two latter are both given, the line on one side of a point is distinguished from the other, which is not the case when the tangent is given.

I take this opportunity to append a few remarks upon the answer given by Mr. Graves in this Magazine of last April, to the objections to his view of the logarithms of unity contained in my *Calculus of Functions*. That I did not make a formal reply before this time arose from my considering both sides of the argument as brought to a sufficiently distinct point by Mr. Graves in his communication, and particularly in the following sentence.

"If I am told that logarithm ought to be so defined that x ought to be called an a-log., or logarithm to base a only of the *arithmetical* value of a^x, I must say that I should not approve such a restriction. It would, if the proposed theorems be correct, arbitrarily exclude from the *name* of logarithms orders of functions which enjoy the same fundamental and characteristic *properties* as those that are favoured with that name."

It is then proposed by Mr. Graves to extend the name of logarithm so as to include, not only what I should call the logarithms to the arithmetical form of the base, but also those to every other form yet considered in algebra. That is to say, writing ε in its most general form $\varepsilon^{1+2m\pi\sqrt{-1}}$ and y in its form $\varepsilon^{\log y+2n\pi\sqrt{-1}}$, where $\log y$ is the arithmetical logarithm, he defines x, the general logarithm of y, by the equation

$$\varepsilon^{(1+2m\pi\sqrt{-1})x} = \varepsilon^{\log y+2n\pi\sqrt{-1}}.$$

But in my remarks, the doubt arose from my not understanding that *an extension* was proposed, but imagining that it was asserted that the *ordinary* formulæ, under *ordinary* definitions, were incomplete. And this appeared likely from the title of the paper in the Philosophical Transactions, "An attempt to *rectify the inaccuracy* of some logarithmic formulæ;" from the comparison of the results with the corrections of trigonometrical formulæ by MM. Poisson and Poinsot, those corrections being of absolute errors caused by the processes not being as general as the definitions; and other things of the same kind. But supposing it explicitly admitted that an extension is proposed, I see no further objection to Mr. Graves's system, but quite the contrary; and should even suggest that the extension should include, not only the *positive* forms of the base ε, contained in $\varepsilon^{1+2m\pi\sqrt{-1}}$, but also the *negative* forms contained in $\varepsilon^{1+(2m+1)\pi\sqrt{-1}}$, which are equally the representations of the number ε, affected by the remainder of those symbolic relations, the consideration of which constitutes one principal difference between algebra and arithmetic. The subject is not without analogy to that treated in the former part of this paper. So long as ε is absolutely considered, the logarithms of y are contained solely in the form $x+2\pi m\sqrt{-1}$; but simultaneously with each of the algebraical forms under which ε can be exhibited, exists a new class, which, relatively to the particular form in question, are logarithms *to the base* ε, if so defined, but under the common meaning of the term logarithm, *to one of the algebraical forms of* ε. I should myself prefer the latter method of expression. But under either definition, properly understood, the extension may be highly useful, and I am happy to bear testimony to the ingenuity which suggested it, and the talent with which it has been carried out.

In page 287, I think Mr. Graves has not kept in memory a distinction which I have drawn in the *Calculus of Functions* already alluded to. The Alps upon Alps of solutions which I have shown to be possible in functional equations (§ 119. 212.) belong only to those in which there are no independent functional subjects. In the case where there are independent variables (§ 97. in which among others $\phi x \times \phi y = \phi(x+y)$ is treated,) I have made the following remark: "The use of the Calculus of Functions in regard to these is to show that the obvious algebraical form which satisfies them is also the *most general of continuous functions*." It would surprise me not a little to see any other solution of the last-mentioned equation more general than $\phi\, x = a^x$. But assign a relation between x and y, say $x = y$, and the solution of $(\phi\, x)^2 = \phi\,(2\,x)$ is

$$\phi\, x = \left\{\theta\left(\cos 2\pi \frac{\log x}{\log 2}\right)\right\}^x$$

where θ is any function which does not invert the cosine. And this is only a very limited solution.

LI. *Reply to* Disjota's *Remarks. By the Rev.* J. H. Pratt, *B.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I AM sorry that three of your valuable pages should have served no better purpose than to convince me that your correspondent Disjota, has entirely mistaken the object of my communication printed in your June Number.

It will be seen upon referring to that communication, that the words "only one" are emphatic: and by this I expected my readers to understand, that my object was not to revive the controversy respecting the proposition, that a function of θ and ψ can be developed in a series of Laplace's coefficients; but to show, upon the supposition of Poisson's demonstration of this proposition being rigorous, that there is *only one* such series. In short, I conceive that the whole question involves two propositions, *the first* to show that the development is possible, *the second* to show that *only one* such development exists; and it is clear that your correspondent fancies that I intended to prove the *first*, when, in fact, it was the *second* that I wished to demonstrate.

If Disjota will turn to page 387 of vol. ii. Camb. Phil. Trans., he will see that the object of the Astronomer Royal in that place is to show that $f(\theta, \psi)$ admits of *only one* development and it is to Laplace's demonstration of *this* that he objects; and he will observe that the *possibility* of the development is not called in question.

But I fear that without a few more words my communication will still be misunderstood by Disjota.

I agree entirely with what he says at page 85, in the paragraph, "It is obvious ..."; and wonder much why Poisson sets about to *prove* in the way that he does in art. 112. page 225. of the *Théorie Mathématique de la Chaleur*, that his method of development will lead to only one series of Laplace's coefficients, since it appears to me (as it does to your correspondent) self-evident, because P_i admits of only one form.

Now the objection I urge is this: that although the "analytical artifice" used by Poisson will lead to only one series of Laplace's coefficients, it does not at all follow that I cannot discover another analytical artifice which shall lead to another development. For instance, suppose α and β are two arbitrary constants, then I can easily, *à priori*, *conceive it possible* that $f(\theta, \psi, \alpha, \beta)$ can be developed in a series of Laplace's coefficients $Y_0 + \alpha Y_1 + \alpha^2 Y_2 + \ldots\ldots$ by one process, and also in another series $Z_0 + \beta Z_1 + \beta^2 Z_2 + \ldots\ldots$ by another process, and yet that when $\alpha = 1$ and $\beta = 1$ the corresponding terms of these series shall not be identical. To *prove* that they are identical is the object of my communication in your June Number; which I hope Disjota will again peruse, and I think he *must* then see that "his observations fall to the ground," since they have been directed to a wrong point.

In reply to the concluding paragraph of Disjota's communication I refer him to the same volume and page of the Cambridge Phil. Trans. as before; and he will learn that the Astronomer Royal was the first to point out that Laplace had assumed, and not proved, that $f(\theta, \psi)$ admits of only one development of the form required, and that Mr. Ivory had mentioned this proposition of Laplace's without making any objection to it. I am, Gentlemen, yours, &c.

Caius College, Aug. 12, 1836. JOHN HENRY PRATT.

LII. *Experiments on the supposed new Metal Donium. By Mr.* J. D. SMITH.*

HAVING been led to question the elementary nature of donium, from an experiment on artificial ultramarine hereafter noticed, I resolved on making some further researches to satisfy myself as to the existence or non-existence of this substance as a distinct metal. Circumstances obliged me to defer this examination until the present time; but now, from the particulars of the experiments I shall detail, I have no hesitation in expressing my perfect conviction that this supposed new body is a mere mixture of alumina with a minute portion of peroxide of iron.

Some artificial ultramarine having been digested in dilute hydrochloric acid, and the filtered solution tested with hydrosulphuric acid, for the purpose of ascertaining whether any metallic substance was contained in it, no precipitate was produced; but on the addition of a little ammonia a copious green precipitate fell, which exactly coincided with Mr. Ri-

* Communicated by the Author. A notice of Mr. Richardson's experiments on Donium was given in our Number for August, p. 157.

chardson's description of sulphuret of donium, in the Records of General Science of June last. This precipitate on a further examination proved to be alumina, with a trace of oxide of iron. This experiment, as I have before mentioned, suggested the suspicion that the oxide of donium of Mr. Richardson was in reality alumina and peroxide of iron.

H. J. Brooke, Esq., having kindly presented me with a small portion of the mineral (Davidsonite) from which the oxide of donium was stated to have been obtained, I reduced about 10 grs. of it to a very fine powder: this, which was of a light flesh-colour, was fused with thrice its weight of a mixture of carbonate of soda and potash in a platinum crucible. The fused mass was then treated with dilute hydrochloric acid, the solution evaporated to dryness, the residuum dissolved in distilled water acidulated with hydrochloric acid, and the silica separated by a filter, when about four fluid ounces of solution were obtained.

I then dissolved 0·4 gr. of peroxide of iron in hydrochloric acid, added it to a solution of 200 grs. of alum, and precipitated this mixture by ammonia; this precipitate when well washed was redissolved in dilute hydrochloric acid, and the solution evaporated to the consistence of a syrup: a few small prismatic crystals of a yellow colour, which deliquesced when exposed to the air, were deposited: and when evaporated to dryness a residue of a light yellow colour and of an astringent taste, at first sweetish and much resembling that of alum, was obtained: this was then redissolved in a pint of distilled water. $1\frac{1}{2}$ fluid ounce of this solution was diluted with about 3 ounces of water, and excess of ammonia added to it; the precipitate at first formed was immediately redissolved; but all further attempts again to procure this entire re-solution, with another portion of the solution of alumina and iron, either by pouring it into the ammonia or the ammonia into it, proved abortive. The solution after standing for 24 hours was decanted, leaving a very small quantity of a reddish brown flocculent precipitate, evidently the peroxide of iron. This solution having been carefully evaporated to dryness, and heated to redness in a platinum capsule, a light white friable mass remained, which, treated with hydrochloric acid, partially dissolved, and the solution when tested with hydrosulphate of ammonia gave a light green precipitate.

The solution obtained from the Davidsonite having been poured into a large excess of ammonia, gave a gelatinous precipitate; this was frequently shaken, and at the expiration of two or three days the supernatant liquor was carefully decanted. A fresh portion of ammonia was then added, and the digestion renewed; but as no diminution in the quantity of precipitate appeared to take place, the digestion was not continued, but the liquor was filtered. These solutions being mixed evaporated to dryness, and the residue heated to redness in a platinum crucible, there remained a fused mass of a light-brown colour: its fusion was doubtless owing to the chlorides of potassium and sodium, derived from the flux previously used: this mass when treated with hydrochloric acid gave a solution which afforded a slight greenish-coloured precipitate with hydrosulphate of ammonia, and there remained on the filter a small portion of an insoluble gelatinous substance, exactly resembling alumina.

My endeavours to obtain an ammoniacal solution of the substance contained in Davidsonite having been thus frustrated, I redissolved the precipitate left by ammonia in dilute hydrochloric acid, and submitted this solution, and the solution of alumina and iron, before described, to the action of the following tests; the results of which, for the sake of comparison, I have arranged in the following manner.

A. refers to the solution of donium as described by Mr. Richardson, B. to the solution of the substance I obtained from Davidsonite, and C. to the solution of alumina and oxide of iron.

Ammonia gave with A. " a white flocky precipitate soluble in excess"; with B. a gelatinous precipitate slightly soluble in excess of the precipitant; and with C. a gelatinous precipitate partially soluble in an excess of the precipitant.

Carbonate of ammonia with A. the same as with ammonia; with B. a white flocculent precipitate; when excess of carbonate of ammonia was added and the solution filtered, hydrosulphuric acid gave a very slight green precipitate, therefore it is slightly soluble in excess; with C. it acted in precisely the same manner as B.

Caustic soda with A. " white flocky precipitate soluble in excess"; B. and C. were also at first precipitated, and then by an excess of the precipitant redissolved.

Carbonate of soda gave with A. a " white flocky precipitate insoluble in excess;" with B. a white flocculent precipitate also insoluble in excess; with C. the same.

Hydrosulphate of ammonia with A. produced a " flocky green precipitate," with B. a beautiful dark-green flocculent precipitate: C. acted in a like manner to B.

Oxalate of ammonia with A. no precipitate, with B. and with C. none.

Phosphate of ammonia gave with A. a curdy white precipitate: with B. a dense white precipitate; with C. the same as B.

Arseniate of potash when added to A. gave a white flocky precipitate"; to B. slight white, which on the addition of a drop of ammonia became a dense white precipitate; with C. it acted in the same manner as with B.

Chromate of potash gave with A. a "copious yellow precipitate;" with B. and C. a gelatinous yellow precipitate.

Sulphate of soda: A. gave none, B. and C. none.

Tartrate of potash with A. produced a "slight white precipitate"; with B. the merest trace of a precipitate, and the same with C.

Tincture of galls gave no precipitate with A., but developed in B. a bluish black tint, and the same, only not so deep, in C.

Ferrocyanate of potash is not mentioned as having been tried with A.; with B. it produced a deep blue colour, and with C. the same colour, but of a lighter shade.

I may here remark, that when the dark green precipitates produced by hydrosulphate of ammonia from the solutions of Davidsonite, and of alumina and oxide of iron, were exposed to the air for about 12 hours, they were both converted into a brownish white gelatinous substance.

The remaining solution of Davidsonite, and an equal quantity of the alumina and iron, were then respectively treated with excess of potash, which in both cases redissolved the precipitate at first produced, except a small portion of a reddish brown gelatinous substance, which when dissolved in hydrochloric acid, and tested with ferrocyanate of potash, and tincture of galls, in each instance afforded abundant indication of oxide of iron.

The alkaline solutions having been respectively neutralized and the precipitates redissolved in hydrochloric acid, were boiled down in flasks to about $1\frac{1}{2}$ fluid ounce; and both of these solutions afforded, with the tests already noticed, precipitates precisely similar to those they gave previously to the addition of the caustic potash, with the exceptions of ferrocyanate of potash and tincture of galls, neither of which produced the slightest discoloration; the colour also of the precipitates produced by hydrosulphate of ammonia was an extremely light shade of green, very different from that produced before the addition of potash*.

From these experiments it would appear, that the substance combined with silica in Davidsonite, and alumina mixed with a minute quantity of oxide of iron, are identical,

* The solution of potash employed became tinted on the addition of hydrosulphuric acid.

agreeing in every particular; and differing from the characters assigned to oxide of donium only as regards the extent of solubility in ammonia and its carbonate. This substance, according to Mr. Richardson, when combined with potash and sulphuric acid, affords a salt which crystallizes in octohedra: this furnishes additional confirmation of its identity with alumina.

It is to be regretted that Mr. Richardson did not test his solution with ferrocyanate of potash, as the characteristic blue colour produced by this test would doubtless have directed his attention to the possibility of its being merely a mixture of alumina and iron, and not, as he supposed, a new substance.

I have not tried any experiment on its reduction to an apparently metallic state, owing, partly to my not having any donium remaining in my possession, and partly to my belief that the preceding experiments will serve to show that the supposed donium is nothing more than alumina containing a trace of peroxide of iron. But I may, perhaps, be allowed to hazard the conjecture, that the slate blue colour of the powder obtained by means of hydrogen gas, was owing to the reduction of the peroxide to the state either of protoxide, or of metallic iron.

St. Thomas's Hospital, Sept. 17, 1836.

LIII. *Further Observations on the Action of Nitric Acid upon Iron. By* Dr. Schoenbein.*

I HAVE already remarked that nitric acid, which generally attacks iron with violence, has no action upon an iron wire, one end of which, before its immersion in the acid, has been heated to dullness. From experiments since made, I find that the action is dependent on the quantity of water combined with the acid. This quantity I have not yet accurately ascertained, but I find that acid of the sp. gr. of 1·36 diluted with 15, 30, 60, 120, 240, 480, and 960 times its volume of water attacks an iron wire heated at one end in the same manner as when not heated, and that the oxidated iron falls off by degrees into the acid without being dissolved in it.

Diluted nitric acid acts upon iron wire protected by platina or gold in the same manner as when the end of the wire is heated. Many chemists state that iron is not acted upon by ordinary nitric acid when diluted with three times its vo-

* Communicated by Dr. Faraday. See pp. 53, 57, and 122.

sume of water; according to my experiments this metal is sensibly dissolved by nitric acid diluted with 1000 times its votume of water. As it is evident that the different action of she same nitric acid on iron is caused by a certain electrical tate of the metal, I endeavoured to ascertain its nature by making an iron wire the positive pole of a voltaic battery set in action by nitric acid. I experimented in the following manner:

Nitric acid, of sp. gr. 1·36, at the ordinary temperature, was used in a circle of 15 plates, with a voltaic cup apparatus; at the positive pole an iron wire, and at the negative a platina wire, dipped into the acid. When I closed the circuit with the negative wire, the iron wire was acted upon as usual; when I closed it with the iron wire, by first dipping one end in the nitric acid, and then making the other end the positive pole of the battery, the same effect also took place; but when I closed the circuit so that one end of the iron wire was first united with the positive pole, and the other end afterwards dipped into the acid, no action took place on the iron, and it possessed, after its separation from the positive pole, all the properties which it had by heating, or when protected by gold and platina,—precisely those which I have already so fully stated in my former paper. I heated the nitric acid used in the circuit to nearly its boiling point before it acted upon the positive iron wire. It follows, of course, that under these circumstances the water contained in the nitric acid was decomposed. No hydrogen gas is given out at the negative pole from strong nitric acid, for instance, of sp. gr. 1·36, but it combines with a part of the oxygen of the acid, and converts the latter into nitrous acid.

At a temperature of 70° centigrade, a gas is given out at the negative platina wire, which I have not yet particularly examined, but which is probably deutoxide of nitrogen. It has hitherto been considered that the other element of the water, the oxygen, combines with the positive iron wire, and forms a hydrate with the nitric acid.

If the circuit is so formed that the nitric acid has no action upon the iron wire, the free oxygen does not combine with the metal, but is given off in a gaseous state, precisely as when silver, gold, or platina wires are used. This is not only the case with acid of the above strength, but also with acid diluted with 1, 10, 100, and even 400 times its volume of water. That the iron is not partially oxidated is evident from its unchanged metallic lustre, as also from the proportions of gas given off at both wires, which I found according to several measurements to be as 1 to 2. If the two wires where the water is being decomposed are brought for a few seconds into contact (the nitric acid being diluted with about 10 parts of water), and then again separated, oxygen gas is no longer given out at the positive iron wire, but a yellow nitrate is formed, which sinks to the bottom; if, however, the end of the iron wire which has been dipped into the nitric acid is exposed to the air for a moment and the circuit then closed, oxygen gas is again given off from the iron. If the communication is broken by means of the negative wire and again made, after a few seconds oxygen continues to be given out at the positive iron wire. When the acid is very much diluted, it requires some time after closing the circuit before the oxygen gas appears. In whatever manner the giving out of the oxygen gas is interrupted, it always recommences if the iron wire is exposed for a short time to the air, and the circuit then formed by it in the usual way. Exactly the same effects take place when diluted sulphuric or phosphoric acid is substituted for nitric acid.

In order that oxygen gas should be given off by these acids at the positive iron wire, it is first necessary that the negative pole should be in communication, by means of a wire, with the decomposing fluid; that one end of the iron wire should be in contact with the positive pole, and the other end with the acid. By any other mode of closing the circuit, oxygen gas is not given out, even when the iron wire has been previously dipped in diluted sulphuric or phosphoric acid. The giving out of oxygen gas will be interrupted upon bringing the wires of the poles into contact when the last-mentioned acids are used, and will not return upon exposure of the iron wire to the air. No oxygen gas is given out at the positive iron wire when subphosphorous or phosphorous acid dissolved in water is substituted, probably because it combines with the acid: when diluted alkalis are used, oxygen gas is given out at the positive end, in whatever way the circuit may be closed. During my experiments I have observed many other singular phænomena, which I shall communicate hereafter, when the circumstances under which they occurred are better understood. One of these, however, I will mention in conclusion: in the same nitric acid in which a platina wire served for the negative pole, a quantity of hydrogen gas was given off, but when an iron wire was substituted for the platina, no gas appeared. It was only when a considerable time had elapsed after the substitution that hydrogen made its appearance.

From several of the above-mentioned facts, it appears that the law formerly laid down, that oxygen gas is only given out at the positive pole of a battery when it is terminated by a noble metal, does not hold good; that the same cause which renders an iron wire indifferent to nitric acid, prevents the oxidation of this metal during the decomposition of water by means of the galvanic battery. I shall not for the present enter further into the theory to be deduced from these facts, as I am satisfied many further researches still require to be made.

LIV. *Description of* E. M. Clarke's *Magnetic Electrical Machine.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

FROM the time Dr. Faraday first discovered magnetic electricity to the present, my attention, as a philosophical instrument maker, has been entirely devoted to that important branch of science, more especially to the construction of an efficacious magnetic electrical machine, which after much anxious thought, labour and expense, I now submit to your notice.

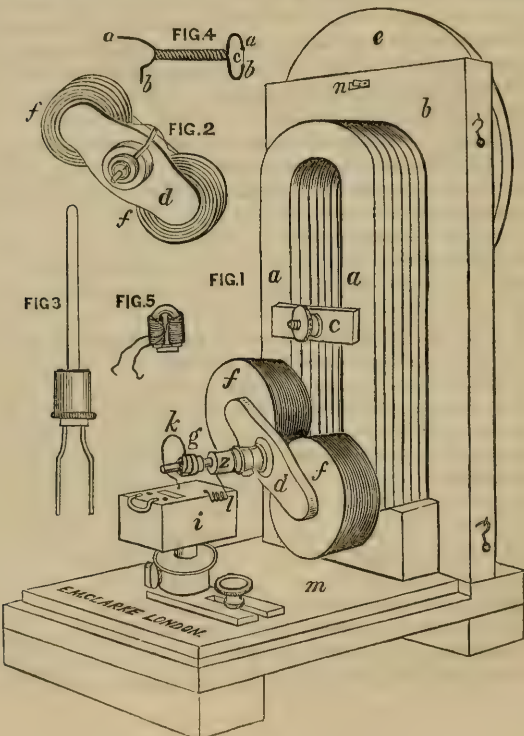


Fig. 1. $a\ a'$, the magnetic battery, resting against four adjusting screws, which pass through the mahogany back-board b: c, a strap of brass, having an opening in the middle, through which a bolt and nut pass that are fixed in the board b; d and d', fig. 2, two soft-iron armatures, screwing into a brass mandril, which is seated between the poles of $a\ a'$, rotatory motion being communicated to it by a band passing over the mandril, pulley, and the mahogany multiplying wheel e; $f f$, fig. 1, and $f' f'$, fig. 2, coils of insulated copper wire, the commencement of each coil being in metallic connexion with the break g, and the terminations of each in connexion with the hollow brass cylinder z; i, a closed box to contain mercury, the connexion being made with the commencement of the coils $f f$ and $f' f'$, by the copper hook k, one end of which rests on the break, the other end screwing through the top of the box i into the flood of mercury which it contains; l, a spiral spring of steel wire, one end dipping into the mercury in the box i, the other end rubbing on the hollow cylinder z.

Fig. 3. An apparatus to show the decomposition of water by magnetic electricity, for a description of which see my paper in the number of your Magazine for June 1835 (vol. vi. p. 427), but differing in as much as the gases are here collected mixed in one tube.

Fig. 4. An apparatus to show the ignition of platina wire by magnetic electricity; $a\ a'$, $b\ b'$, two pieces of insulated copper wire twisted together; c, a piece of very fine platina wire soldered to $a\ b$.

Fig. 5. A piece of soft iron bent in the shape of a horseshoe magnet, having two coils of insulated copper wire, and an iron keeper to show that magnetic electricity induces magnetism on soft iron.

The advantages of these arrangements are as follows:

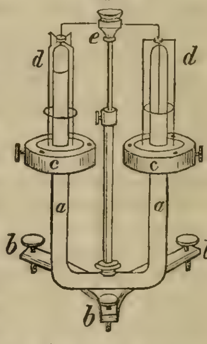
First. That I am enabled to give motion to my armatures without communicating any vibration to my magnetic battery, which I know by experience to be highly injurious to either bent or straight bar magnets.

Second. As my magnetic battery stands perfectly independent of the armatures, and also of the machinery which gives them motion, every facility is afforded of withdrawing the battery from the machine, so that it may be applied to any other purpose where powerful magnets are required.

Third. Having in November, 1834, tried the effects produced by coils of wire varying in diameter, I found that the thick copper bell-wire gave brilliant sparks, but no perceptible shock, whilst, on the contrary, very fine wire gave powerful shocks, but very indifferent sparks. I took advantage of my discovery, and furnished my machines with two armatures, thereby being enabled to give the separate effects of quantity and intensity to the fullest extent of power that my magnetic battery was capable of supplying.

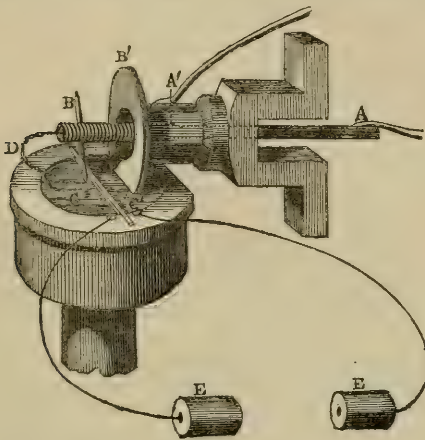
Fourth. By my intensity armature (which has 1500 yards of fine insulated copper wire on it) I am enabled to go through the various experiments that are usually performed by a number of separate galvanic plates. The effect this armature produces on the nervous and muscular system is such, that no person out of the hundreds who have tried it could possibly endure the intense agony it is capable of producing; it is capable also of electrifying the most nervous person without giving them the least uneasiness; it shows the decomposition of water (the gases can be obtained separately or mixed at pleasure) and also of the neutral salts; it deflects the gold leaves of the electroscope, charges the Leyden jar, and by an arrangement of wires from the mercury box to the magnets *a a′*, the electricity is made distinctly visible, passing from the magnetic battery to the armature, and by the same arrangement not only shocks, sparks, but also brilliant scintillations of steel can be obtained from *a a′*.

Fig. 6.



Fifth. By my quantity armature, fig. 2, (which has forty yards of thick copper bell-wire, and has twice the weight of iron of the other armature,) I am enabled to go through the various experiments that are usually performed by a single pair of voltaic plates or by a calorimotor, but it will not perform any of the same experiments that can be produced by the intensity armature; it gives large and brilliant sparks, sufficiently so that a person can read small print from the light it produces; it induces magnetism in the apparatus fig. 5; it ignites gunpowder and the platina wire of the apparatus fig. 4, *without the platina wire being inclosed in a hermetically sealed glass tube*; it deflagrates gold and silver leaf, and produces brilliant scintillations from a small steel file. The most interesting experiment it performs is that of producing rotary motion of the delicately suspended wire frames round the poles of a vertical horseshoe magnet. Fig. 6. *a a′*, a cylindrical horseshoe magnet, on a tripod stand, having levelling screws *b b′ b″*; *c c′*, improved flood cups; *d d′*, rotating wire frames, having two little cups at top to hold a drop of mercury; *e*, the connecting fork. When this apparatus is connected with the mercury box of the magnetic electrical machine, either rotatory or reciprocating motion can be produced at pleasure of the wires *d d*.

Sixth. In a paper of mine (see Phil. Mag., March 1835: vol. vi. p. 169.) I gave a diagram of that part of the magnetic electrical



machine called the mercury cup, in which a copper disk B′ revolves, (being in connexion with one end of the copper wire coils by the wire A′,) and a double-bladed or pointed piece of copper B also dips, it being connected with the other ends of the copper wire coils by the wire A. It is obvious that by this arrangement, when the point and disk are put into rapid motion, the mercury must be scattered about, which is not the only evil, for as you require a very accurate adjustment of the surface of mercury, so that the pointed piece B will leave the mercury at a particular period twice during each revolution, each succeeding rotation tends to destroy that adjustment, consequently the effect produced is constantly on the decrease, and ultimately ceases altogether unless you stop and renew the level you previously had. By my arrangement the mercury is inclosed in a wooden box, and the steel spring *l*, and the hollow brass cylinder *z*, do the duty of B′ A′ C′ of the old arrangement, and *k g* also do the duty of B A C, so that the mercury is not directly acted upon as formerly. *Whatever adjustment you make is permanent, and, come when you will, you are sure of a uniform and steady supply of electricity.* This is a most important point for medical practitioners, as the uncertainty of action of all electrical machines, both magnetical and rictional, has been the great drawback on their being more extensively used by the faculty.

Seventh. The great portability of my machine makes it very convenient for travelling, as there is a mahogany case which slides upon *m* against *b*, where it locks at *n*; there is a closet in the case, into which all the apparatus, figg. 2, 3, 4, 5, pack.

EDWARD M. CLARKE, Magnetician.

No. 9, Agar Street, West Strand, London.

LV. *On the Iodides of Gold. By* JAMES F. W. JOHNSTON, *Esq., A.M., F.R.S.E., &c. &c., Reader in Chemistry and Mineralogy in the University of Durham.**

I. *Proto-iodide of Gold.* Au + I.

WHEN iodide of potassium in solution is added in excess to a solution containing terchloride of gold, a precipitate is obtained of a greenish yellow colour, while the supernatant liquid becomes dark-coloured from the excess of iodine held in solution. If the terchloride be in excess, the precipitate has a blueish gray colour, being a mixture of the yellow powder with iodine, or with the teriodide of gold. If the precipitate be heated in the dark-coloured mother liquor, it diminishes in quantity, and on cooling gradually separates in minute golden yellow crystalline scales of great beauty, exhibiting apparently triangular and square faces, and not inferior in lustre to iodide of lead crystallized from a similar hot solution.

This beautiful yellow compound consists chiefly of protoiodide of gold. According to the analysis of Pelletier (*Ann. de Chim. et de Phys.*, vol. xv. p. 116) it contains 34 per cent. of iodine. Exposed to the air and light, it is gradually decomposed, and more readily when moist, giving off iodine. At 150° Fahr. it begins to give off iodine; at 230° Fahr., it undergoes almost total decomposition, losing only one per cent. additional when heated to 400° Fahr. Two portions prepared at different times, lost when decomposed by heat, 33·96 and 34·43 of iodine respectively. After exposure to the air for ten days it lost only 9·6 per cent.

A compound of one atom of gold with one of iodine should contain 38·83 per cent. of iodine. The low temperature at which the compound is decomposed rendering it probable that the crystallized portions analysed by Pelletier and myself might contain metallic gold, I collected first a heavier brighter portion which collects at the bottom when the precipitate is heated in the mother liquor: it lost when dried and heated

* Communicated by the Author.

only 0·1 per cent., and was therefore nearly pure gold. Another large portion, containing some of these brighter scales mixed with it, lost 24·86 per cent.

It appears then that the crystalline scales which fall from a hot solution of proto-iodide of gold are a mixture of this iodide and of metallic gold to the amount of 12 or 13 per cent.

A portion of the precipitate, prepared by adding terchloride of gold to iodide of potassium in excess and afterwards washing with water, of a bright yellow colour with little lustre, lost when heated 34·93 per cent. Obtained by this process, therefore, it contains 10 per cent. of metallic gold.

Prepared by pouring a solution of iodide of potassium on protochloride of gold, washing with water, drying in a cool place on bibulous paper, it has a pale green colour. Heated to redness, 10·346 grains, apparently of great purity, lost 38·85 per cent., almost the exact theoretical quantity.

The proto-iodide of gold, therefore, is a pale green powder, which decomposes slowly in the atmosphere, is soluble in hydriodic acid and in hot dilute solutions of the alkaline iodides, precipitating from the latter on cooling in brilliant scales of a gold yellow colour, intimately mixed with about 12 per cent. of pure gold. At 150° Fahr. it is partially, and between 300° and 400° wholly, decomposed; and it consists of

		Calc.	Exp.
Gold	24·86	61·167	61·15
Iodine......	15·783	38·833	38·85
	40·643	100	100

II. *Teriodide of Gold.* Au + 3 I.

When a solution of terchloride of gold is gradually added to one of iodide of potassium the solution becomes dark-coloured, and if agitated, the *dark green* precipitate which appears is nearly all redissolved. This continues till one equivalent of terchloride has been added for every four equivalents of iodide of potassium, and there are formed three equivalents of chloride of potassium and one of the double iodide (iodo-aurate) of gold and potassium, which, unless the solutions be very concentrated, all remain in solution. The reaction is as follows: $4\,KI\,.\,Au\,Cl_3 = 3\,K\,Cl\,.\,KI + Au\,I_3$.

This reaction, however, does not always take place exactly as here represented. The teriodide is so easily decomposed that, when precipitated, instead of being entirely redissolved by the excess of the alkaline iodide, it is partly decomposed, and a portion of the proto-iodide falls, the liberated iodine being held in solution.

If more terchloride be now added, the teriodide falls in the form of a dark green powder, and continues to fall on every successive addition till the solution becomes nearly colourless. The precipitate may be washed with water without much decomposition, but cannot be dried, either by exposure to the air or by artificial heat, without considerable loss of iodine. I have not therefore attempted any direct analysis of it. It is soluble in hydriodic acid and in solutions of the iodides, with partial decomposition and formation of the proto-iodide, and is decomposed by the alkalies and alkaline earths. Exposed to the air, it gradually becomes yellow, changing into proto-iodide, and ultimately into metallic gold.

Double Iodide (*Iodo-aurate*) *of Gold and Potassium.*—When concentrated solutions of terchloride of gold and iodide of potassium are mixed with agitation in the proportion of rather less than one equivalent of the former to four of the latter and set aside, crystals of this salt speedily begin to deposit themselves in long acicular prisms; or if a concentrated solution of iodide of potassium be digested on teriodide of gold, the latter is largely dissolved, and the solution becomes of a dark brownish red, almost black, and, except in thin layers, opake. Set aside, this solution deposits crystals sometimes an inch in length, which are slender, perfectly black, opake even by candle-light, possessed of a high degree of lustre, and having much resemblance to crystallized schorl. They have the form of four-sided doubly oblique truncated prisms, cleaving readily parallel to the terminal plane, and longitudinally striated on one pair of the faces, indicating, probably, a cleavage parallel to the other pair.

These crystals are soluble in a weak solution of an alkaline iodide, or of hydriodic acid, but are partially decomposed by pure water. At 150° Fahr. or under, they emit a faint odour of iodine, and gradually become purple; in the air, at common temperatures also, they lose iodine but very slowly, and are otherwise permanent. They contain no water of crystallization. Heated in a close tube they therefore give off no moisture, but emit violet fumes, and leave a skeleton of a gold yellow colour, exhibiting the original form of the crystal.

Dried at a temperature of about 100° Fahr., these crystals lost when heated, in two experiments, 50·61 and 51·43 per cent. of iodine, and left, after washing out the iodide of potassium, 26·46 and 26·95 per cent. of metallic gold. They consist therefore of

		Calc.	Exp. Mean.
1 equiv. gold	24·86	26·76	26·705
3 equiv. iodine	47·349	50·97	51·02
1 equiv. iodide of potassium	20·682	22·27	22·275
	92·891	100	100

Teriodide of Gold with Iodides of Sodium and Ammonium.—These salts are prepared by digesting solutions of the respective iodides on the teriodide. They both crystallize in black four-sided prisms, having much lustre, those containing ammonia being generally flattened. They are both deliquescent, that of sodium so much so that it is obtained in crystals with great difficulty; that of ammonia only in a moist atmosphere. I have not ascertained whether or not they contain any water of crystallization.

The teriodide dissolves also in solutions of the iodides of barium and strontium, giving similar dark-coloured solutions. A solution of the iodide of iron likewise takes it up in considerable quantity, and forms with it a crystallizable compound.

The solution in hydriodic acid has the same dark red colour. Set aside to spontaneous evaporation, it deposits minute black prisms, which, on exposure to the air, acquire a beautiful purple tint, probably from loss of iodine. Whether these are merely crystals of the teriodide, or a compound with hydriodic acid, I have not ascertained. The former is the more probable.

Like the solutions of the terchloride, those containing the teriodide are also decomposed by the addition of caustic ammonia. The precipitate is of a brown colour, more or less dark, sometimes almost black, and, like fulminating gold, it detonates when heated, giving off iodine and ammonia. The dark precipitates are obtained by pouring liquid ammonia into an excess of the teriodide; the lighter, by dropping a solution of the teriodide into caustic ammonia, and agitating at each successive addition. They are decomposed by hot nitric acid, evolving iodine, and leaving a mixture of proto-iodide and metallic gold.

Durham, August 30, 1836.

LVI. *Experimental Researches into the Physiology of the Human Voice.* *By* John Bishop, *Esq., &c. &c.*

[Continued from p. 209.]

THE lungs having been supplied with air by the muscles of inspiration, the ligaments of the glottis are drawn into the vibrating position.

The air in the chest and trachea being now condensed by the muscles of expiration, a portion of the glottis yields to its pressure, and the edges are curved upwards so as to be nearly parallel to the axis of the vocal tube, leaving between them an aperture through which the air passes. The tension and elasticity of the vocal ligaments tend to restore them to the plane of the vibrating position, whilst, at the same time, the air is acting in an opposite direction.

The vibration of the edges of the glottis thus produced, by communicating to the adjacent air an alternate state of condensation and rarefaction, the sounds of the voice are accomplished.

The relative length of the vibrating surface of the glottis is regulated conjointly by the pressure of the column of air in the trachea, and the tension and resistance of the vocal ligaments.

A certain degree of condensation of the air is therefore requisite, the maximum varying with the proportion of the area of the chest to that of the trachea. It is upon these hydrostatic principles that the small muscles which close the glottis are capable of resisting the whole force of the respiratory muscles*.

If a person possessing a deep bass voice be directed to expel his breath in a manner not quite sufficient to yield the lowest possible note, on applying the ear to his mouth a clicking motion is perceived. If the tension and velocity of the air are now increased, the clicking ceases, and a continued sound is produced, but of an exceedingly grave pitch. During the previous state, Dr. Young † observes, "a delicate ear may detect the vocal chords vibrating twenty-six times in a second, or about two octaves below the A of a common bass voice."

The intensity of the voice in the same medium‡, and under similar collateral circumstances, depends on the pressure of the column of air in the trachea, and the range of motion performed by the vibrating edges of the glottis.

The true vibrating surface of the glottis is the mucous membrane. The vocal chords confer on it the tension, resistance, position, and probably other conditions necessary for vibration. Aphonia often results from undue relaxation of this membrane.

When we consider the fundamental pitch of the human voice, and compare the length of the vocal ligaments with that of stringed instruments, or the length of the vocal tube with that of wind instruments, we must at once perceive that the

* The force of the expiratory muscles is about a pound on every square inch of the surface of the chest.

† Nat. Phil., vol. ii.

‡ Acording to Derham, the human voice has been heard at the distance of ten miles at Gibraltar. Boussingault, in his ascent of Chimborazo with Colonel Hall, at the height of 6004 metres, found the voice scarcely audible. inhaling hydrogen gas also greatly enfeebles the voice. The intensity of the tone varies reciprocally with the density of the air under similar collateral circumstances.

vocal organs vibrate very slowly, simply by their relaxation *, a subject which M. Savart has very successfully demonstrated.

The fundamental pitch of the voice will vary as the length of the vocal ligaments, and the power of adjustment in the vocal tube to vibrate in unison with the glottis. This is the cause of infants having acute voices, which gradually become of a graver pitch until they arrive at the age of puberty, when the voice of males assumes an altered character; the pitch suddenly becoming a fifth or an octave graver, attended with hoarseness, and a temporary inability to control and regulate the tones †. During that period, whilst speaking, in the same sentence the voice sometimes becomes suddenly elevated a fifth or an octave; but at the expiration of from eight to twelve months its character becomes finally settled ‡. Eunuchs do not undergo any change of this kind.

In the female also, at the age of puberty, the larynx undergoes a change, differing however in kind and less extensive in effect. In the male the whole larynx is enlarged, whilst in the female it chiefly increases in breadth; the junction of the wings of the thyroid anteriorly is at a more obtuse angle, and the prominence of the *pomum Adami* is less conspicuous. The voice at this period acquires a fuller and rounder character, with a greater intensity of tone.

The natural key or pitch of the vocal organs may be found by sounding the voice without elevating or depressing the larynx; the grave octave of that note will be the fundamental pitch of the voice §. I have frequently tried this experiment on singers, and have always found this hypothesis verified by the result. The pitch of the vocal organs being thus on the confines of the lowest tone of the acute, and the highest of the grave succeeding octaves, occupies a middle or central position, affording a great facility to their actions in varying the tone.

In illustration of the advantages of this position, let us suppose the pitch of the voice, in a state of rest, to have been

* Both temperature and moisture have also very sensible effects on the vibrations of elastic membranes.

† Bennati is of opinion that the voice should not be exercised by singing during this period. The cases of Donzelli and Donizetti (*Recherch. sur la Mec. de la Voix Hum.*) admit of a very different construction. It is well known that persons *acquire* as well as *lose* a good quality of voice after its breaking.

‡ The voice of a person possessing a grave, loud, and reedy character of tone will sometimes yield three or four harmonics of the fundamental note; this was first observed by M. Knecht, of Leipsic. It is singular that by closing the lips lightly, and making them vibrate with the voice, they will simultaneously yield vibrations in harmony with the glottis, but two or three octaves graver in pitch.

§ The tone which may possibly be produced of a graver pitch loses both its quality and intensity, and cannot be considered as belonging to the natural compass of the voice.

placed at either extreme of the grave or acute termination of these octaves. The vocal tube would then have to pass through very disproportionate spaces. Suppose, for example, the grave octave were C; to arrive at the G of the next succeeding octave the organs would require to ascend a twelfth; or had they been placed at the opposite extreme, to descend a fourth; but as they are now adjusted, it would only be necessary, for the accomplishment of the same tone, to ascend a fifth, or descend a fourth.

The pitch or key of the vocal organs, at the point of rest, is the basis which determines the different characters of voices recognised by musicians. Accordingly we find that what are denominated the bass, the barytone, the tenor, the countertenor, &c. amongst males, and the soprano, mezzo soprano, and soprano sfogato amongst females, are variations of pitch which give an enlarged compass of voice for the purposes of melody, and fill up the musical intervals between the gravest and most acute voices.

Ferrein, Fétis, and other French authors have observed that specific characters of voice are peculiar to certain localities. In Picardy, for instance, the finest bass voices occur. Languedoc, and Toulouse, with its environs, are celebrated for tenors; whilst in Burgundy and Franche-Comté, female voices of the first quality are found. No cause has hitherto been assigned for these peculiarities, which do not appear to exist in this country.

All these modifications of the voice are dependent upon the key of its fundamental tone. We may estimate the average compass of tones comprised between the lowest notes of good bass voices, being about the C string of the violoncello, and the most acute of female voices, reaching C on the second leger line above the G clef, to be four octaves; but there are individuals who can exceed these two extremes.

M. Biot calculates three octaves and a half to be the extreme range, but this I know from experience to be too low an estimate.

The power with which the vocal organs are thus endowed, of varying and modulating the grave and acute tones of the voice, has been from an early period a principal subject of inquiry. Aristotle and those who followed him, till the commencement of the last century, were of opinion that the acute tones of the voice depend upon the relative velocity, quantity, and temperature of the air passing through the glottis, combined with the size of its chink*. This theory was adopted

* ἡ δὲ μεγάλη φωνὴ, γάρ, ἐν τῳ πολὺν ἄερα κινεῖν, καὶ ὀξεῖα ἐν τῳ ταχέως, βαρεῖα δὲ ἐν τῳ βραδέως.—*Aristotelis Opera*, lib. 2. *Problematum*, sect. xi.

by Galen *, Boethius, Fabricius †, Marcienne ‡, Kircher, Perrault §, and many others ||.

Dodart ¶ also at first embraced the Aristotelian hypothesis respecting the velocity of the air in the aperture of the glottis, but finding it insufficient, he adopted a new theory, which, owing to its elaborate researches, has obtained the greatest attention.

His theory, however, of the parabolic curves of the glottis, the whistle, and the vox humana pipe of the organ, were by no means happy deductions from his researches.

Ferrein ** has proved by experiment that the vocal chords are capable of yielding all the tones of the voice, and has endeavoured to show that their tension regulates the modulations, and that they are governed by the same laws as stringed musical instruments.

Mr. Willis †† also appears to be of the same opinion as Ferrein with regard to the tension of the vocal ligaments. He observes, "To obtain the various notes from the glottis, therefore, it is only necessary to vary its longitudinal tension after the ligaments have been placed in the proper position."

M. Savart, whose valuable researches have contributed greatly to our acoustic knowledge, conceives that the superior and inferior ligaments of the glottis form an apparatus analogous to the duck-whistle ‡‡, and that all the variations of the glottis are regulated upon the same principle as that instrument, a view which has been adopted by several physiologists of the present day.

A consideration of these diversified and unsatisfactory theories induced me to reinvestigate this subject, and for this purpose to repeat the experiments of Ferrein. In the first place, I observed, that in order to produce any sounds whatever, it was requisite to close the chink of the glottis §§, by bringing the edges of the vocal ligaments into immediate contact, when, by straining them tolerably tight, the sounds be-

* Galen, *Opera, De Larynge*, lib. 7.

† *De Larynge, Vocis Organo.*

‡ *Harmonie Universelle.*

§ *Essais de Physique ; Traité du Bruit.*

|| Casserius, *De Organo Vocis, &c.*

¶ *Mém. de l'Acad. Royale*, 1700, 1707.

** *Ibid.*, 1747.

†† Camb. Philos. Trans., vol. iv.

‡‡ The transverse sections of the larynx in figg. 4. and 5. give an appearance not unlike the duck-whistle, which is a small circular box, perforated in the centre by two holes, situated opposite to each other: according to the hypothesis of Savart, the tones of the voice would have rather resembled the whistle than the reed.

§§ Liscovius (*loc. cit.*, p. 29–34.) confirms this remark. He says that no tones are formed where the glottis is very much dilated.

came loud and distinct, but possessing a more reedy quality of tone than belongs to the voice in the living body.

The difference was doubtless attributable to the absence of the mouth and nasal cavities, which powerfully influence the melody of the voice.

Finding that during the production of sound the chink of the glottis remained closed, excepting the parts in actual vibration, I next minutely observed the change produced in the vocal chords; I remarked that when the gravest tones were uttered, the ligaments vibrated throughout their whole length, and that as the tones became more acute, a proportionably smaller extent of the ligaments was thrown into vibration. During the production of the most acute tones, the tension of the vocal ligaments was but slightly increased, and the greatest possible tension was insufficient to produce acute tones, whilst these ligaments vibrated throughout their whole length.

A very slight movement of the thyro-arytenoid muscle seemed to be sufficient to vary the tones, and they would frequently become very acute without my being sensible of having altered the tension. Hence it appears demonstrated that the motions of the thyro-arytenoid and crico-thyroid muscles so affect the vocal chords that a portion of them only is rendered susceptible of vibration.

Since therefore a muscular apparatus has been shown expressly adapted to produce all the motions necessary for the modulation of the voice, it may safely be inferred that the phænomena observed in experiments thus conducted actually take place in the living body, and that this is the true mode by which all the tones of the voice, whether acute or grave, are effected *. The ligaments of the glottis being attached at their extremities, are subject, as regards their vibrations, to the laws observed in simple strings, modified perhaps in consequence of the reflections of the mucous membrane over them, owing to which a broad surface is presented to the current of air rather than isolated chords or strings.

Having now considered the manner in which the glottis is made to yield all the fundamental notes, whether acute or grave, of the human voice, I shall investigate the nature of those changes which take place in the vocal tube in order to yield vibrations isochronous with those of the glottis.

The intimate relation existing between the glottis and the

* The observations of Magendie tend to confirm those deduced from my experiments. He found that the glottis of a dog vibrated in a small proportion of its length during the utterance of acute tones, but in a larger proportion during the grave tones. This illustrates the identity of the motions of the glottis artificially excited, with those of the living animal.

vocal tube in which it is placed, the manner in which they are adjusted to each other, and the acoustic effects resulting from this reciprocal action, present an interesting field of inquiry.

Although it has been demonstrated that the glottis is capable of producing all the range of tone without the aid of the rest of the vocal organs, its function becomes more limited when placed in the vocal tube, for in that position it can vibrate in perfection only when in unison with the pitch of the tube.

When the vocal tube has by any means lost its power of vibrating in accordance with the glottis, the consequence is, that the glottis will either merely reach phonation, produce discordant tones, or become silent altogether.

Magendie mentions a person who having a small aperture in the trachea, was obliged to tie a cravat tightly round it, to restore the power of adjustment, and enable him to speak.

I lately witnessed an analogous case of loss of voice owing to a fistulous orifice below the glottis.

The glottis being situated near the superior extremity of the vocal tube, does not alter the fundamental pitch of the voice; hence Mr. Wheatstone * very justly remarks that the trachea exerts the same influence below the glottis as it would above it †.

When the voice is raised in the scale from grave to acute, a corresponding elevation takes place in the larynx, towards the base of the cranium.

By placing the finger on the *pomum Adami*, this motion can be easily felt, and at the same time the thyroid cartilage is drawn up within the os hyoides, and presses on the epiglottis; the small space between the thyroid and the cricoid closes ‡; the pharynx is contracted; the *velum palati* is depressed and curved forwards; the tonsils approach each other, and the uvula is folded upon itself §.

The reverse of these phænomena takes place during the descent of the voice.

These are the principal phænomena which can be recognised by external observation; the other changes being, on account of their situation, invisible.

* See Mayo's Physiology.

† Those physiologists who would assign to the thyroid gland the mechanical office of acting as a damper to stifle the reverberations of sounds in the chest, must have very erroneous notions of the functions of the vocal tube.

‡ See fig. 7.

§ Gerdy (*Bulletin Universel des Sciences,*) remarks, "La luette se raccourcisse graduellement jusqu'à s'effacer entièrement, lorsque la voix monte très-haut." See also Bennati, *Recher. sur la Voix.*

The effects of these variations on the tones of the voice have been hitherto little understood. It has always appeared incomprehensible why the vocal tube should apparently increase in length in the production of the acute tones, and shorten in the grave *, a circumstance which, theoretically, presents an acoustic paradox. Dodart and many others have conceived the elevation of the larynx to be merely for the purpose of shortening the vocal tube in the super-laryngeal cavity, and have considered the trachea as producing no effect on the key of the tone, an error which has already been pointed out by Mr. Wheatstone.

Magendie's remarks on the shortening of the vocal tube apply only to the approximation of the thyroid-cartilage to the os hyoides.

In order to ascertain the effect of these changes, I made the following experiments on the dead body: having laid bare the vocal organs of an adult male, I raised the larynx to the position it would occupy by the elevation of the voice an octave, being about half an inch, and at the same time minutely observed the position of the lowest ring of the trachea in reference to the sternum. By this operation I found the trachea was raised out of the chest, nearly to the same extent as the larynx had been elevated towards the base of the skull. My next step was to examine whether any change had taken place in the diameter of the tube. Having for this purpose measured the diameter of the trachea in its natural position, I again elevated the larynx to the same extent as before, and found the diameter diminished one third. These experiments prove that, contrary to the general preconception, the elevation of the larynx *shortens* the tube independently of the contraction between the thyroid cartilage and *os hyoides*, and at the same time lessens its diameter.

The same effects may easily be detected during life by placing the finger on the trachea immediately above the sternum during the elevation of the larynx, when the trachea is found to ascend out of the chest, and afterwards to return to its former position, a movement in which the lungs and bronchii participate. The alteration of the tube in diameter may also be perceived by grasping the trachea with the finger and thumb during the elevation and depression of the larynx.

Such are the principal means provided for adjusting the vibrations of the vocal tube to those of the glottis; but as the variation of length is not sufficient to render the tube capable of adjusting itself to the whole range of tones, the relative tension of its superficies supplies the deficiency. The influence

* See Richerand, p. 440.

of the tension of elastic membranes in modulating the tones produced by them has been very satisfactorily demonstrated by the interesting experiments of Savart*, and it is no doubt materially concerned in the analogous phænomena of the voice. The diameter of a tube does not influence the pitch of its sound, but there is an obvious appropriateness in the diminution of the diameter of the trachea as the sound becomes sharper; for experience has taught the makers of wind instruments that the best qualities of tone for the lower notes are obtained when the bore of the instrument is large, and for the higher notes when it is small.

The influence of the vocal tube, as far as relates to its effects on the key of the voice, is terminated at the *velum palati* by the several perforations of the nostrils, the Eustachian tubes, and the mouth. The opinion of Savart, that the mouth modifies the key of the tone is consequently erroneous †.

We find analogous acoustic effects in musical instruments; for instance, the lowest joint of the flute, which is six inches in length, having three perforations, when its keys are open lowers the tone of the instrument only half a note. The important distinction between the effects of air passing through the tubes of musical instruments, according as their sides are rigid or membranous, is, that in the former case, as exemplified in flutes, hautboys, &c., the air vibrates independently of the sides of the tube, whilst in the latter, the tube enters into compound vibrations with the column of air.

[To be continued.]

LVII. *On the* Sivatherium giganteum, *a new Fossil Ruminant Genus, from the Valley of the Markanda, in the Sivâlik branch of the Sub-Himâlayan Mountains. By* Hugh Falconer, *M.D., Superintendent Botanical Garden, Sehâranpur, and Captain* P. T. Cautley, *Superintendent Doâb Canal.*

[Continued from p. 201, and concluded.]

NOTWITHSTANDING the singularly perfect condition of the head, for an organic remain of such enormous size, we cannot but regret the mutilation at the muzzle and vertex, as it throws a doubt upon some very interesting points of structure in the *Sivatherium*: 1st, the presence or absence of incisive and canine teeth in the upper jaw, and their number and character if present; 2nd, the number and extent of the bones which enter into the basis of the external nostrils; and 3rd, the presence or absence of two horns on the vertex, besides the two intra-orbital ones.

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* *Annales de Chimie.*

† Tandis que la bouche en s'ouvrant plus ou moins, et en changeant par conséquence les dimensions de la colonne d'air, exerce aussi une influence notable sur le nombre des vibrations, conjointement avec les lèvres.—*Ann. de Chimie*, 1825.

Regarding the first point, we have nothing sufficient to guide us with certainty to a conclusion, as there are ruminants both with and without incisives and canines in the upper jaw; and the *Sivatherium* differs most materially in structure from both sections. But there are two conditions of analogy which render it probable that there were no incisives. 1. In all ruminants which have the molars in a contiguous and normal series, and which have horns on the brow, there are no incisive teeth. In the camel and its congeners, where the anterior molars are unsymmetrical and separated from the rest of the series by an interval, incisives are present in the upper jaw. The *Sivatherium* had horns, and its molars were in a contiguous series: it is therefore probable that it had no incisives. Regarding the canines there is no clue to a conjecture, as there are species in the same genus of ruminants both with and without them. 2. The extent and connexions of the incisive bones are points of great interest, from the kind of development which they imply in the soft parts appended to them.

In most of the horned *Ruminantia*, the incisives run up by a narrow apophysis along the anterior margins of the maxillary bones, and join on to a portion of the sides of the nasals; so that the bony basis of the external nostrils is formed of but two pairs of bones, the nasals and the incisives. In the camel, the apophyses of the incisives terminate upon the maxillaries without reaching the nasals, and there are three pairs of bones to the external nostrils, the nasals, maxillaries, and incisives. But neither in the horned ruminants, nor in the camel and its congeners, do the bones of the nose rise out of the plane of the brow with any remarkable degree of saliency, nor are their lower margins free to any great extent towards the apex. They are long slips of bone, with nearly parallel edges, running between the upper borders of the maxillaries, and joined to the ascending process of the incisive bone, near their extremity, or connected only with the maxillaries; but in neither case projecting so as to form any considerable re-entering angle, or sinus, with these bones.

In our fossil, the form and connexions of the nasal bones are very different. Instead of running forward in the same plane with the brow, they rise from it at a rounded angle of about 130°, an amount of saliency without example among ruminants, and exceeding what holds in the rhinoceros, tapir, and palæotherium, the only herbivorous animals with this sort of structure. Instead of being in nearly parallel slips, they are broad and well arched at their base, and converge rapidly to a sharp tip, which is hooked downwards, over-arching the external nostrils. Along a considerable portion of their length they are unconnected with the adjoining bones, their lower margins being free and so wide apart from the maxillaries as to leave a gap or sinus of considerable length and depth in the bony parietes of the nostrils. The exact extent to which they are free, is unluckily not shown in the fossil, as the anterior margin of the maxillaries is mutilated on both sides, and the connexion with the incisives destroyed. But as the nasal bones shoot forward beyond the mutilated edges of the maxillaries, this circumstance, together with their well-defined outline and symmetry on both sides of the fossil, and their rapid convergence to a point with some convexity, leaves not a doubt that they were free to a great extent and unconnected with the incisives.

Now to determine the conditions in the fleshy parts, which the structure in the bony parietes of the nostrils entails.

The analogies are to be sought for in the *Ruminantia* and *Pachydermata*.

The remarkable saliency of the bones of the nose, in the *Sivatherium*, has no parallel, in known ruminants, to guide us; and the connexion of the nasals with the incisives, or the reverse, does not imply any important difference in structure in the family. In the Bovine section, the Ox and the Buffalo have the nasals and incisives connected: whereas they are separate in the Yák* and Aurochs. In the Camel, they are also separate, and this animal has greater mobility in the upper lip than is found in other ruminants.

In the *Pachydermata*, both these conditions of structure are present and wanting in different genera; and their presence or absence is accompanied with very important differences in the form of the corresponding soft parts. It is therefore in this family that we are to look for an explanation of what is found in the *Sivatherium.*

In the Elephant and Mastodon, the Tapir, Rhinoceros, and Palæotherium, there are three pairs of bones to the external nostrils; the nasals, the maxillaries, and incisives †. In all these animals, the upper lip is highly developed, so as to be prehensile, as in the Rhinoceros, or extended into a trunk, as in the Elephant and Tapir; the amount of development being accompanied with corresponding difference in the position and form of the nasal bones. In the Rhinoceros, they are long and thick, extending to the point of the muzzle, and of great strength to support the horns of the animal; and the upper lip is broad, thick, and very mobile, but little elongated. In the Elephant, they are very short, and the incisives enormously developed for the insertion of the tusks, and the trunk is of great length. In the Tapir, they are short and free, except at the base, and projected high above the maxillaries; and the structure is accompanied by a well-developed trunk. In the other pachydermatous genera, there are but two pairs of bones to the external nostrils, the nasals and the incisives: the latter running up so as to join on with the former; and the nasals, instead of being short and salient, with a sinus laterally between them and the maxillaries, are long, and run forward, united to the maxillaries, more or less resembling the nearly parallel slips of the *Ruminantia.* Of this genera, the Horse has the upper lip endowed with considerable mobility; and the lower end of the nasals is at the same time free to a small extent. In all the other genera, there is nothing resembling a prehensile organ in the upper lip.

In the *Sivatherium*, the same kind of structure holds as is found in the *Pachydermata* with trunks. Of these it most nearly resembles the Tapir. It differs chiefly in the bones of the nose being larger and more salient from the chaffron; and in there being less width and depth to the naso-maxillary sinus, than the Tapir exhibits. But as the essential points of structure are alike in both, there is no doubt that the *Sivatherium* was invested with a trunk like the Tapir.

This conclusion is further borne out by other analogies, although more indirect than that afforded by the nasal bones.

1st.—The large size of the infra-orbitary foramen. In the fossil, the exact dimensions are indistinct, from the margin having been injured in the chiseling off of the matrix of stone: the vertical diameter we make out to be 1·2 inch, which perhaps may be somewhat greater than the truth; but anything approaching this size, would indicate a large nerve for transmission, and a highly developed condition of the upper lip.

2nd.—The external plate of the bones of the cranium is widely separated from the inner, by an expansion of the diploe in vertical plates, forming large cells, as in the cranium of the Elephant: and the occipital is expanded laterally into alæ, with a considerable hollow between, as in the Elephant. Both these conditions are modifications of structure, adapted for supplying an extensive surface for muscular attachment, and imply a

* Cuvier, *Ossemens Fossiles*, tome iv. p. 131. † *Ibid.*, tome iii. p. 29.

thick fleshy neck, with limited range of motion; and, in more remote sequence, go to prove the necessity of a trunk.

3rd.—The very large size of the occipital condyles, which are greater both in proportion, and in actual measurement, than those of the Elephant, the interval between their outer angles, taken across the occipital foramen, being 7·4 inches. The atlas, and the rest of the series of cervical vertebræ, must have been of proportionate diameter to receive and sustain the condyles, and surrounded by a large mass of flesh. Both these circumstances would tend greatly to limit the range of motion of the head and neck. But to suit the herbivorous habits of the animal, it must have had some other mode of reaching its food; or the vertebræ must have been elongated in a ratio to their diameter sufficient to admit of free motion to the neck. In the latter case, the neck must have been of great length, and to support it and the load of muscles about it, an immense development would be required in the spinal apophysis of the dorsal vertebræ, and in the whole anterior extremity, with an unwieldy form of the body generally. It is therefore more probable that the vertebræ were condensed, as in the Elephant, and the neck short and thick, admitting of limited motion to the head: circumstances indirectly corroborating the existence of a trunk.

4th.—The face is short, broad, and massive, to an extent not found in the *Ruminantia*, and somewhat resembling that of the Elephant, and suitable for the attachment of a trunk.

Next with regard to the horns:—

There can be no doubt, that the two thick, short, and conical processes between the orbits, were the cores of horns, resembling those of the Bovine and Antilopine sections of the *Ruminantia*. They are smooth, and run evenly into the brow without any burr. The horny sheaths which they bore, must have been straight, thick, and not much elongated. None of the bicorned *Ruminantia* have horns placed in the same way, exactly between and over the orbits: they have them more or less to the rear. The only ruminant which has horns similar in position is the four-horned Antelope* of Hindustán, which differs only in having its anterior pair of horns a little more in advance of the orbits than occurs in the *Sivatherium*. The correspondence of the two at once suggests the question, "had the *Sivatherium* also two additional horns on the vertex?" The cranium in the fossil is mutilated across at the vertex, so as to deprive us of direct evidence on the point, but the following reasons render the supposition at least probable:

1st.—As above stated, in the bi-cavicorned *Ruminantia*, the osseous cores are placed more or less to the rear of the orbits.

2nd.—In such known species as have four horns, the supplementary pair is between the orbits, and the normal pair well back upon the frontal.

3rd.—In the Bovine section of *Ruminantia*, the frontal is contracted behind the orbits, and upwards from the contraction, it is expanded again into two swellings, at the lateral angles of the vertex, which run into the bases of the osseous cores of the horns. This conformation does not exist in such of the *Ruminantia* as want horns, or as have them approximated on the brow. It is present in the *Sivatherium*.

On either supposition, the intra-orbitary horns are a remarkable feature in the fossil: and if they were a solitary pair on the head, the structure, from their position, would perhaps be more singular, than if there had been two additional horns behind.

Now to estimate the length of the deficient portion of the muzzle, and the entire length of the head:—

* The *Tetracerus* or *Antilope quadricornis* and *Chekara* of authors.

In most of the *Ruminantia*, where the molars are in a contiguous uninterrupted series, the interval from the first molar to the anterior border of the incisive bones is nearly equal to the space occupied by the molars; in some greater, in some a little less, and generally the latter. In other *Ruminantia*, such as the *Camelidæ*, where the anterior molars are insymmetrical with the others, and separated from them by being placed in the middle of the diasteme, this ratio does not hold; the space from the first molar to the margin of the incisives being less than the line of molars. In the *Sivatherium*, the molars are in a contiguous series, and if on this analogy we deduce the length of the muzzle, we get nearly 10 inches for the space from the first molar to the point of the incisives; and 28·85 inches for the whole length of the head, from the border of the occipital foramen to the margin of the incisives; these dimensions may be a little excessive, but we believe them not to be far out, as the muzzle would still be short for the width of the face, in a ruminant.

The orbits next come to be considered. The size and position of the eye form a distinguishing feature between the *Ruminantia* and the *Pachydermata*. In the former it is large and full, in the latter smaller and sunken; and the expression of the face is more heavy in consequence. In the *Sivatherium* the orbit is considerably smaller in proportion to the size of the head than in existing ruminants. It is also placed more forward in the face, and lower under the level of the brow. The rim is not raised and prominent, as in the *Ruminantia*, and the plane of it is oblique; the interval between the orbits at their upper margin being 12·2 inches, and at the lower, 16·2 inches. The longitudinal diameter exceeds the vertical in the ratio of 5 to 4 nearly, the long axis being nearly in a line from the naso-maxillary sinus across the hind limb of the zygomatic circle. From the above we infer that the eye was smaller and less prominent than in existing ruminants; and that the expression of the face was heavier and more ignoble, although less so than in the *Pachydermata*, excepting the horse; also that the direction of vision was considerably forwards, as well as lateral, and that it was cut off towards the rear.

This closes what we have been led to infer regarding the organs of the head. With respect to the rest of the skeleton we have nothing to offer, as we are not at present possessed of any other remains which we can with certainty refer to the *Sivatherium* *. Among a quantity of bones† collected from the same neighbourhood with the head fossil, there are three singularly perfect specimens of the lower portions of the extremities of a large ruminant, belonging to three legs of one individual. They greatly exceed the size of any known ruminant, and excepting the *Sivatherium giganteum*, there is no other ascertained animal of the order, in our collection, of proportionate size to them. We forbear from further noticing them at present, as they appear small in comparison for our fossil: and besides, there are indications in our collection, in teeth and other remains, of other large ruminants, different from the one we have described.

The form of the vertebræ, and more especially of the carpi and tarsi, are points of great interest to be ascertained; as we may expect modifications of the usual type adapted to the large size of the animal. From its

* See note to page 201.—Sec. Asiat. Soc.

† We note here a very perfect cervical vertebra of a ruminant in our possession, which must have belonged to an animal of proportions equal to that of the *Sivatherium*; but from certain characters, we are inclined to suspect that it is allied to some other gigantic species of ruminant, of the existence of which we have already tolerable certainty. Of the existence of the elk, and a species of *Camelidæ*, Lieut. Baker of the Engineers has shown us ample proof.

bulk and armed head, few animals could be strong enough to contend with it, and we may expect that its extremities were constructed more to give support, than for rapidity of motion. But, in the rich harvest which we still hope to reap in the valleys of the *Markanda*, it is probable that specimens to illustrate the greater part of the osteology of the *Sivatherium* will at no very distant period be found.

The structure of the teeth suggests an idea regarding the peculiarities of the herbivorous habits of the animal. In the description it was noticed that the inner central plate of enamel ran in a flexuous sweep, somewhat resembling what is seen in the *Elasmotherium*, an arrangement evidently intended to increase the grinding power of the teeth. It may hence be inferred, that the food of the *Sivatherium* was less herbaceous than that of the existing horned ruminants, and derived from leaves and twigs; or that as in the horse, the food was more completely masticated, the digestive organs less complicated, the body less bulky, and the necessity of regurgitation from the stomach less marked than in the present *Ruminantia*.

The following dimensions, contrasted with those of the elephant and rhinoceros, will afford a tolerably accurate idea of the size of the *Sivatherium*. They are characteristic, although not numerous :—

	Elephant.	Sivatherium.	Indian 1-horned Rhinoceros.
From margin of foramen magnum to the first molar	23·10 in.	18·85 in.	24·9 in.
Greatest width of the cranium	26·0	22·0	12·05
Do. do. of face between the malar bones..................................	18·5	16·62	9·20
Greatest depth of the skull............	17·80	11·9	11·05
Long diameter of the foramen magnum	2·55	2·6	2·6
Short do. do. do.	2·4	2·3	1·5
Average of the above	15·06	12·38	10·22

If the view which we have taken of the fossil be correct, the *Sivatherium* was a very remarkable animal, and it fills up an important blank in the interval between the *Ruminantia* and *Pachydermata*. That it was a ruminant the teeth and horns most clearly establish; and the structure which we have inferred of the upper lip, the osteology of the face, and the size and position of the orbit, approximate it to the *Pachydermata*. The circumstance of anything approaching a proboscis is so abnormal for a ruminant, that at the first view, it might raise a doubt regarding the correctness of the ordinal position assigned to the fossil; but when we inquire further, the difficulty ceases.

In the *Pachydermata*, there are genera with a trunk, and others without a trace of it. This organ is therefore not essential to the constitution of the order, but accidental to the size of the head, or habits of the animal in certain genera. Thus in the elephant, nature has given a short neck to support the huge head, the enormous tusks, and the large grinding apparatus of the animal; and by such an arrangement, the construction of the rest of the frame is saved from the disturbance which a long neck would have entailed. But as the lever of the head became shortened, some other method of reaching its food became necessary; and a trunk was appended to the mouth. We have only to apply analogous conditions to a ruminant, and a trunk is equally required. In fact, the camel exhibits a rudimentary form of this organ, under different circumstances. The upper lip is cleft; each of the divisions is separately moveable and extensible, so as to be an excellent organ of touch.

The fossil was discovered near the Markanda river, in one of the small valleys which stretch between the Kyárda-dún and the valley of Pinjór, in the Siválik or sub-Himálayan belt of hills, associated with bones of the fossil Elephant, Mastodon, Rhinoceros, Hippopotamus, &c. So far as our researches yet go, the *Sivatherium* was not numerous. Compared with the Mastodon and Hippopotamus (*H. Sivalensis*, Nobis, a new species characterized by having six incisors in either jaw,) it was very rare.

Northern Doáb, Sept. 15, 1835.

LVIII. *Observations on the Construction of Voltaic Batteries; with a Description of a Battery exhibited at the Royal Institution of Great Britain, June* 3, 1836, *in which an uniform and powerful current is sustained for any period required. By* Fred. Wm. Mullins, *Esq., M.P., F.S.S., &c.**

HAVING for some years devoted all the time I could spare from other avocations to researches in voltaism and electro-magnetism, I frequently experienced considerable inconvenience from the impossibility of keeping up an equally powerful current of electricity for a period sufficiently long to answer my purposes; and in one particular instance, which I shall more especially refer to in a future paper, as connected with a very important discovery, the obstruction to the inquiries I was then making was so great, that I resolved, if possible, to conquer the difficulty, and conceived that notwithstanding the disappointments that had previously attended similar attempts, some means might still be discovered by which those consequences of chemical action on the metals employed in the galvanic circuit, and which Sir Humphry Davy and other distinguished philosophers had decided to be the chief cause of the decline of electric power, might be prevented, or, at all events, considerably diminished. I therefore commenced a series of experiments on this subject, in the course of which it struck me that a conducting substance interposed between the two metals would effectually protect *both* metals from the injurious effect of the gases and oxides formed while the battery was in action, while the electric current would find a free passage, and a surface of copper, or whatever other metal performed *its* functions, in the fittest state to receive it from the electrolyte. I had been previously in the habit of using membranous substances as conductors of voltaic electricity, in a course of experiments in which I had been engaged with the view of obtaining a new mode of developing voltaic power; and having found that thin membranes, when moistened in alkaline or acid solutions, af-

* Communicated by the Author. An abstract of Professor Daniell's paper on the Constant Voltaic Battery, recently constructed by him, will be found in our last volume, p. 421.

forded a free passage to the electric fluid, I concluded that my object would be fully attained by their employment in the voltaic circuit. I accordingly prepared a very thin calf's bladder, and having placed in it a coil of thin sheet copper, with a small quantity of solution of sulphate of copper, I immersed both in an earthenware pot containing a cylinder of zinc fitting close to its inner surface, and distant from the surface of the copper an inch and a quarter, and a sufficient quantity of diluted sulphuric acid, in the proportion of 5 of the acid to 100 of water; and on testing the power of this battery with the voltameter, I found that the first deflection suffered very little diminution for several hours, though I had made no alterations in the fluids used, nor in any way disturbed the arrangement (in this experiment the pot used was only two and a half inches in diameter and three deep). In a second experiment, made with the same battery and with the same solutions, having connected it with an electro-magnet of the horse-shoe form, four inches in length and five eighths in diameter, the magnet sustained 50 lbs. attached to the keeper for three hours. Having thus proved the possibility of obtaining a continuous and equally powerful current for a long period, it next became a question whether the power could not be still further increased and prolonged by the use of other conducting liquids in contact with the same quantity of metallic surface; and after a long course of experiments upon the nature of chemical action on metals in voltaic connexion, and the comparative effects of different electrolytes of different degrees of strength,—the results of which I consider sufficiently important to form the subject of another paper,—I found that of the various acid, alkaline, and saline solutions tested, muriate of ammonia in the proportion of 5 parts of the saturated solution to 100 of water, gave me the best conducting fluid, combined with the least injurious action on the zinc surface; in fact, so slight that, after several days of constant action, the zinc plate, which was amalgamated, was scarcely corroded or reduced in thickness.

It next became important to examine what connexion existed between the power produced and the distance between the zinc and copper; and it appeared natural to conclude that the more remote the surface of the copper from that of the zinc, the less would be the effect produced on completing the circuit; but, as I was aware that great diversity of opinion existed upon this subject, with the view of fully satisfying myself with respect to the best mode of construction of voltaic batteries, I prepared an apparatus by means of which I was enabled most accurately to measure the increase or diminution of power according to the relative distances of the two metals. It consisted of a cylindrical earthenware vessel, four inches in diameter, in which was placed a cylinder of zinc, as close to the inner surface as possible: the centre contained a copper coil, of one inch diameter, inclosed in a membranous bag, and there were four other copper coils, of greater and different diameters, so constructed, however, that the largest did not contain a greater surface of copper than the smallest. When the coil of least diameter was used, the deflection of the voltameter only reached 60°; on introducing the next in size and removing the first, the deflection increased to 65°; the third brought the needle to 71°; the fourth to 74°; and the fifth, which was distant only a quarter of an inch from the zinc, to 87°. I repeated the experiment in an oblong trough, with square plates of zinc and copper, gradually advancing the zinc to the copper, and with the same results; thus demonstrating, that in the construction of voltaic batteries, whether square or cylindrical, the nearer the surfaces of the metal are brought, the greater will be the power developed.

It may be well here to state that in the course of these experiments I found that, in each pair of plates, an increase of zinc surface beyond a certain limit will not give an increase of power, and consequently I am of opinion that a larger quantity of zinc than is requisite is employed in all the batteries in general use.

Having thus experimentally ascertained the proper distances of the metals, as well as the best conducting fluids, I constructed a single voltaic battery, in which I brought the principles already stated to bear, as much as the interposed membrane and other circumstances would permit, and of which I will now give a brief description: in an earthenware pot, six inches deep and four wide, I place a cylinder of amalgamated zinc, standing on three legs, half an inch long, cut out of the cylinder, the depth of which, including the legs, is only two inches; within this cylinder, and at three eighths of an inch distant, stands a copper vessel, having a rim a quarter of an inch wide surrounding its outer edge, round which the bladder is tied; the bottom of the vessel rests on a circular piece of baked box wood, which projects one fourth of an inch beyond the cylinder; a thin oblong bladder, well cleaned and moistened, is drawn over all and fastened round the upper rim by a string, the wood at bottom preserving it from contact with the copper, which would otherwise injure the membrane. This cylinder, which is the depth of the pot, is pierced with six holes equidistant from the top and bottom, which communicate with an *inner* cylinder, separated from the outside one by a space of three fourths of an inch, the bottom being on a level with the lower edge of the holes and soldered to the larger cylinder; this chamber is intended to hold crystals of sulphate of copper when required, and to receive the solution, which should not rise higher than the *upper* edges of the holes. A small quantity of the ammoniacal solution is to be poured *outside* the membrane until it rises to the upper edge of the zinc; the latter solution does not require renewal; the former will require the addition of a few crystals of the sulphate every four hours. An electro-magnet of the horse-shoe form, the limbs of which were five inches long and one inch in diameter, having four coils of thin wire, each coil containing thirty feet, when connected with a battery of the dimensions given, sustained for several hours a weight of 112 lbs.; but this is not a safe test, for I have always observed that electro-magnets to which weights have been for some time attached, lose their temporary power to a certain extent, and acquire a slight permanent power. This arises, I apprehend, from the straining of the soft iron and the consequent unevenness of the poles, as well as from a certain change in the contiguity and internal arrangement of its particles, and presents, in my opinion, an insurmountable obstacle to the useful application of electro-magnetic power in the way alluded to by Mr. M'Gauley at the late meeting of the British Association.

Upon the same principle, conjoined with another which in a future paper I shall more particularly advert to, I have constructed a somewhat different form of battery for intensity effects; it consists of a number of zinc and copper cylinders, of the same proportions as those before described, placed in a pot of the same size as that already mentioned, one within the other, the copper cylinders being lined with *caoutchouc* for insulation. In this battery the power is immense in proportion to the quantity of the metals used, and arises, I conceive, from the application of a principle which I believe is quite novel in the construction of voltaic batteries, namely, that of *diminishing* the metallic surfaces as the fluid in its onward passage *accumulates*, thus acquiring *increased* force in proportion to the smallness of the substance to which it is restricted. By merely altering the connexions of the plates, which by the mode I have adopted can be done with the utmost facility, this battery can be turned into a powerful quantity one, and for *both*, a wine-glass full of the solutions is amply sufficient.

In a future paper I hope to be able to enter at greater length into these and other results of the investigations in which I have been for some time engaged.

Beaufort House, Killarney, Sept. 1, 1836.

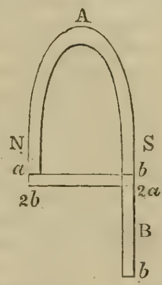
FRED. WM. MULLINS.

LIX. *Remarks on* Mr. Rainey's *Theory of Magnetic Reaction. By the Rev.* William Ritchie, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain and in the University of London.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

If the explanation given by Mr. Rainey, with regard to the reaction of the lifter, (p. 220,) be admitted, it will completely overthrow the Newtonian law of the perfect equality of *action and reaction*. To save you the trouble of preparing a new *cut*, I shall use the one employed by Mr Rainey in his last communication. Mr. Rainey takes for granted that a piece of soft iron B, placed in the direction of one of the sides of the horse-shoe magnet, will have a magnetic power, represented by b, induced on its remote extremity, whilst the power of the pole S still retains its inducing power b. Now the simplest experiment shows that S cannot *induce* a power on a piece of soft iron without having its own power *diminished* by an *equal* quantity. If, therefore, b represent the absolute power of the pole S, the magnetism induced at the other extremity of the soft iron must be less than b, the diminution depending on the length of B. Hence when brought into the position across the poles, the pole S has *not* induced the magnetism represented by $a + b$, otherwise its *own* magnetism would have been completely destroyed. The same remark applies to the pole N. Hence $2\,a$ will *not* represent the magnetism at the pole S, nor $2\,b$ that at the pole N. Therefore the contact of the lifter cannot induce the magnetism $a + b$ in the magnet A, whatever be its temper or state of magnetism with regard to saturation.



The example which Mr. Rainey gives of increasing the magnetism of a *weak* magnet, which has been *previously* magnetized to saturation and has had its magnetism *diminished*, is one of the most unfortunate examples he could have chosen. I have formerly shown, in the Philosophical Magazine, that if magnetism be *induced* in *one* direction and then *destroyed*, the original magnetic state is easily restored.

The reaction of the soft iron does not therefore *create* or *induce* magnetism, it only assists by its reaction in restoring in some measure the weak magnet to its original state. If

Mr. Rainey can increase the power of a horse-shoe magnet of tempered steel, which has been magnetized in the usual way, to the same strength as the *weakened* magnet, by means of perfectly soft iron, then it must be admitted that the lifter can by its *reaction* induce a greater degree of magnetism than the *prime motor* itself possessed. Till then we must admit the truth of the Newtonian law of action and reaction without exception or reservation.

LX. *On the Optical Phænomena of certain Crystals.* *By* H. F. Talbot, *Esq., F.R.S.**

SOME time ago I had the honour to communicate to the Royal Society an account of my invention of the polarizing microscope†. This instrument possesses so great a power of developing the internal structure of transparent bodies, even in their minutest visible particles, that I feel confident the employment of it will lead to many new and interesting results. At present I mean to confine myself to the description of a phænomenon which shows strikingly the beautiful order and regularity with which nature disposes the fabric of some of her minutest visible works.

The object I speak of is a kind of minute crystallization which may be obtained in peculiar circumstances, and I doubt not, in many different ways; but the manner in which it has presented itself to my observation is as follows.

A crystal of borax is placed in a drop of phosphoric acid somewhat diluted upon a plate of glass, and then moderately heated until the crystal dissolves in the acid. It is then set aside to crystallize. It is well to prepare a number of these plates at once, varying the relative proportion of the acid and salt, in order that the desired kind of crystallization may be found in one or other of them; for there is a considerable variety in the crystalline forms obtained by this method, some of which indeed are very singular. But when that kind of crystallization takes place which it is more particularly my intention to speak of, the field of view of the microscope is seen covered with minute circular spots, each of which is like a tuft of silk radiating from a centre, and is composed of a close assemblage of delicate acicular crystals forming a star. But besides these, are seen interspersed among them a number of circular transparent bodies, which are evidently modifications of the former,

* Read before the Royal Society May 5th, 1836: and now communicated by the Author.

† See Lond. and Edinb. Philosophical Magazine, vol. v. p. 321.

being, in fact, tufts or stars of acicular crystals in such close assemblage as to be in optical contact with each other and to produce the appearance of a single individual. Now let us suppose a group of these circles to be under examination with the polarizing microscope, and when the polarizers* are crossed, we observe the following phænomenon. The field of view being dark, the little circles become luminous, and we see upon each of them a well-defined and dark cross, dividing the crystal into four equal parts. All these crosses are placed similarly, and are parallel to each other, and their direction remains unaltered when the crystals are turned round in their own plane by revolving the plate of glass upon which they stand. This beautiful appearance can be seen with a moderate magnifying power. I measured the diameter of some of the larger crystals, which I found to be from $\frac{1}{300}$ to $\frac{1}{600}$ of an inch. But there are many much smaller, and indeed they may be seen decreasing in size, until nothing remains visible of their structure but the four luminous quadrants, appearing like four minute dots of coloured light placed close together.

I proceeded to examine the circles with a high magnifying power, and under favourable circumstances of illumination, and I observed in them a very admirable structure.

Each circle has upon it one or more coloured rings arranged concentrically, but the number as well as the colour of these rings is different in different individuals.

The innermost ring is deeply coloured or black, and incloses a central space of white light, which is traversed by the arms of the cross intersecting in the centre. This part of the cross, which stands within the innermost ring, is beautifully well defined, and perfectly black. The general appearance resembles the figure 98, in Brewster's Optics, which is a representation of the rings seen in uniaxal crystals. It especially resembles it in the circumstance above mentioned, viz. the more defined outline of the part of the cross which is within the innermost ring.

We have hitherto supposed the polarizers to be crossed, but if we place them in a parallel position we shall see a phænomenon complementary to the above. The circle now presents four patches of coloured light, one in each quadrant; and we generally see near the centre four black or obscure spots, which correspond to the arms of the cross in the other position.

Such is an outline of the microscopic appearances presented

* By this term, for the sake of brevity, I here designate the polarizing and analysing prisms of single-image calcareous spar, or the plates of tourmaline which may be employed in their stead.

by these little crystals, which are probably the minutest bodies in which so complicated an optical structure has hitherto been witnessed. I find that the smaller the circles are, the more perfect is their form and the brighter their colours.

These crystals, as I have already observed, probably consist of spicula diverging from a point, but which are in the closest possible contact, and in a state of complete mechanical cohesion. It seems to follow as a consequence from such a structure that their density must increase from their circumference towards their centre. Now it is worthy of remark, that Sir David Brewster has discovered very similar phænomena by polarized light in the crystalline lenses of certain fishes, which are known by direct experiment to increase in density towards the centre. Indeed the figure which he has given of the lens of the codfish in the Philosophical Transactions for 1816 (Plate XII. fig. 1.) is so like the appearance of one of the crystals which I have described, that it might be supposed to have been intended for a representation of it.

Having pointed out this resemblance, I may also mention another class of facts to which I think those I have described possess a considerable analogy. I mean the optical figures which Brewster has discovered in spheres of glass whose density was rendered variable by heating them.

He says* that, "if we take a cold sphere of glass and immerse it in a trough of hot oil, placed in a polarizing apparatus, we shall observe *a black cross with four sectors of polarized light*. If the sphere is turned round it will exhibit in every position the very same figure. If we now suppose the trough to be filled with such spheres they will exhibit the same phænomena in whatever direction the polarized light is transmitted through them, and even if they were in a state of motion. A fluid composed of such spherical particles would exhibit the same polarizing structure in every possible direction, and even if it were in a state of rapid gyration. If the particles possessed the structure that produces circular polarization the fluid would develop the phænomena exhibited by oil of turpentine, &c."

And again†, "The structure of the particles of a circularly polarizing fluid must be exactly the same along every one of its diameters; that is, the structure must be symmetrical round the centre of the particle, or analogous to that which takes place in common polarization when a sphere of glass has its density regularly increasing or regularly diminishing towards its centre."

* Library of Useful Knowledge, art. "Polarization of Light," p. 51.
† p. 45, *ibid.*

I have quoted these remarkable passages at length, because it appears to me that what is there advanced merely as a hypothesis, acquires a considerable degree of probability from the facts which I have stated, since I have succeeded in rendering actually visible circular particles of excessive minuteness, in each of which the microscope detects the very structure imagined by Brewster, viz. the black cross and four sectors of light. So that it appears not improbable that the circular-polarizing properties of fluids may be owing to the presence of multitudes of particles similar to these, which they hold in solution.

LXI. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

Nov. 13, 1835.—THE following communications were read:—

I. Two elementary solutions of Kepler's Problem, by the angular Calculus. By William Wallace, A.M., &c. &c., Professor of Mathematics in the University of Edinburgh.

II. Observations of *Mars* at the opposition 1834-35, made at the Observatory, Cape of Good Hope. By Mr. Maclear.

These observations, which, including the stars observed with the planet, are nearly 200 in number, were made between November 23, 1834, and February 10, 1835. They are entirely made with the mural circle, and appear to have been forwarded immediately to England.

III. Letter from Mr. Snow to the Secretary, dated October 9, 1835, on the latitude of his observatory at Ashurst.

The method here adopted by Mr. Snow was invented by the celebrated Römer, above 130 years ago, but has ever since remained unnoticed, till within these few years, when it has been used in the determination of the latitudes of places, in some geodesical measurements on the Continent. The method is described by Bessel, in Schumacher's *Astronomische Nachrichten*, a translation of which is given in the *Philosophical Magazine* for May, 1825 (First Series, vol. lxv. p. 354.); and it is also noticed by Dr. Pearson, in his *Astronomy*, vol. ii. p. 594. If the declination of the star can be relied on, the method is capable of great accuracy: the mode pursued was as follows.—A transit instrument was placed with its *axis* north and south, so that the eastern and western passages of a star over the prime vertical might be observed, and the latitude might then be deduced from the known declination of the star. The assumed declinations were taken from Pond's catalogue of 1112 stars; the instrument was kept carefully adjusted for level, and the error of collimation, known to be small, was eliminated by reversing the telescope on different evenings. The transit telescope was of twenty inches focal length, and the object was to try "with how little expense and trouble, compared with that which is encountered in the use of a circular instrument, a very fair latitude may be obtained."

Of twenty different observations, two were rejected as evidently affected by errors of level; the remaining eighteen gave results, of which the extremes differed by six seconds of space. The mean was 51° 15′ 56″, being 2″ less than that obtained from the Ordnance map by Lieut. Murphy. The latitude and longitude, from the latter, are

51° 15′ 58″ N. and $0^{h}\ 1^{m}\ 9^{s}\cdot 3$ W.

IV. Observations of occultations made at Ashurst, by Mr. Snow.

These are, April 6, 1835, immersion of κ Geminorum; April 12, immersion of Saturn's centre; April 16, emersion of θ Ophiuchi; August 14, immersion of v Arietis.

V. Observations made as Trinity College, Cambridge, by Mr. Rothman.

These are Oct. 7, 1834, emersion of θ Ophiuchi; and several immersions and emersions of *Jupiter's* satellites, in October and December, 1835.

VI. Observations made at Fort Charles, Port Royal, Jamaica, by Captain Sir Everard Home, Bart., and Mr. Drury, of His Majesty's ship Racehorse. Communicated by the Rev. G. Fisher.

These consist of observations of the end of the solar eclipse of Nov. 30, 1834, and of eclipses of *Jupiter's* satellites. Mr. Fisher has annexed an observation of his own of the occultation of *Saturn*, on April 12, 1835.

VII. Transits of the moon and stars observed at Argos, at the observatory of General Gordon. By Mr. James Robertson.

These observations were made in the last quarter of 1834 and the first of 1835.

The longitude of the Acropolis at Argos has been deduced in the French survey of the Morea. According to the estimation of the relative positions of the Acropolis and Gen. Gordon's Observatory, the longitude of the latter, as determined from the moon culminating stars, exceeds that obtained from the survey by 5′ 36″·4 of space. The observatory is a small low building, in the garden of Gen. Gordon at Argos; the transit instrument is by Jones, and is firmly fixed on a stone pillar; the clock is by Lepaute, has a mercurial pendulum, and goes tolerably well; the telescope with which the satellites of *Jupiter* were observed is by Harris, of $3\frac{1}{2}$ feet focal length, and the power used was about 130.

VIII. Various observations, made at the Royal Observatory, Edinburgh. By Professor Henderson.

These consist of occultations, and of eclipses of *Jupiter's* satellites, in 1834-35; and observations of *Pallas, Ceres,* and *Saturn,* at their last oppositions.

IX. Extract of a letter from Dr. Pearson to Francis Baily, Esq., containing an observation of Halley's Comet:

"On the 19th of October I had an opportunity of directing a telescope to the Comet, as it passed κ *Ophiuchi,* to the north side of which it approached within about 2′ of the nucleus, at a quarter past six solar time. As this star is in the catalogue of the Astr. Soc., the right ascension and declination of the Comet at that time may be nearly ascertained. On looking into the ephemeris of the *Nautical Almanac*, I observed, that from the 28th to the 30th of October would be a desirable time for determining the Comet's place; but as my large circle is now used only in the meridian, I was obliged to substitute a small altitude and azimuth circle, which reads 10″ in altitude and 15″ in azimuth, with a telescope magnifying about 20 times, and having an aperture of an inch and half diameter. Having placed this small instrument in the meridian previously, in an open situation, and made all the adjustments for my purpose, on the 29th I had fortunately a view of the Comet between the south and west, though it was frequently covered by passing clouds, and at last totally obscured; however, I succeeded in making two observations, while my assistant marked the time by a sidereal clock, giving the right ascension of the mid-heaven: thus obtaining data for the accompanying computations. A mean of two results, corresponding to the mean of the two times noted, it is presumed will not be far from the truth; though not so correct as if the large circle had been used.

"I send the computations to you, partly to show how a similar portable instrument, with circles not exceeding seven inches in diameter, may be used for this and similar purposes; and, partly, because the observations were taken at the latest period that offered, before the Comet's arriving at the perihelion of its orbit. Should the determination now sent be found tolerably correct, you will perceive that the right ascension on the 29th of October exceeded the *maximum* given in the Ephemeris, above referred to, by about *thirty minutes*."

X. A letter from M. Mossotti to Mr. Baily.

This letter contains computations and deductions relative to the transit of *Mercury* over the sun's disc, on May 5, 1832, as recorded in the *Memoirs* of this Society, vol. viii. p. 268. From fourteen observations of the distance of the planet from the sun's centre he has formed as many equations of condition, and resolving them by the method of least squares he has deduced certain results, which are still subject to a further correction, depending on the true time of conjunction of the sun and planet at Greenwich; which was not known to M. Mossotti when he wrote the letter. M. Mossotti also transmitted, at the same time, some further remarks on the solar eclipse of January 20, 1833, recorded in the same volume of the *Memoirs*, p. 224. In the computations relative to this subject, he has employed an equation which he considers to possess an advantage (over all those given by the common method) of presenting all the unknown quantities, that we are in search of, in a linear form; and is, in fact, the very formula by means of which he deduced the results in the equations employed in the transit of *Mercury* above mentioned. The formula is a transformation of the general equation for an eclipse, or occultation, inserted in Dr. Pearson's *Introduction to Practical Astronomy*, vol. ii. p. 675, &c., but which is too long to be here inserted.

By applying this formula, however, to the eclipse in question, he deduces certain results, which, in this case also, are subject to further correction, depending on the true time of conjunction at Greenwich. M. Mossotti likewise continued his observations on certain stars, with which the Comet of Encke was compared on June 6, 1832, as recorded in the same volume of the *Memoirs*, p. 245, for the purpose of identifying the principal determining star used on that day. The result of his observations confirms the accuracy of Mr. Henderson's deduction, as recorded in p. 250. There is a remarkable circumstance, however, attending this inquiry, which is worthy of notice. It appears, from p. 248, that M. Mossotti observed three stars, which he compared with his determining star above mentioned, for the purpose of identification when he should have an opportunity of seeing the stars again; they being at that time too near the sun. On his reexamining this portion of the heavens at a subsequent period, he was surprised to find that the second star was no longer visible. He says that he looked for it several times, without success; and has only been able to recognise the very small star, that was visible in the field of view with the comet, as represented in the diagram.

XI. The following communication was received from Mr. Baily, relative to the medal offered by His Majesty the King of Denmark for cometary discoveries:

"His Majesty the King of Denmark has been pleased to found a gold medal, of the value of twenty ducats, to be given to the first discoverer of a telescopic comet, subject to the following conditions, which are, in some respects, different from those published in the year 1832.

"1. The medal is to be given to the person who may first discover a telescopic comet (that is, a comet not visible to the naked eye at the time of its discovery), and not of known revolution.

"2. The discoverer, if in any part of Europe except Great Britain, must send *immediate* notice to Professor Schumacher, of Altona; and if in Great Britain, or any other quarter of the globe except Europe, must send *immediate* notice to Francis Baily, Esq., of Tavistock Place, London.

"3. Such notice must be sent by the *first* post after the discovery, and in case no post should be established in the place, then by the *first* conveyance that presents itself, without waiting for more observations. A strict attention to this condition is absolutely necessary, for, when it is not complied with, the medal will not be awarded at all, if there be only one who has seen the comet; and, where it has been seen by more than one, it will be given to the discoverer next in order of time who does comply with this condition.

"4. The first notice should contain, not only the time of the discovery, as nearly as the same can be ascertained, in order to avoid any disputed claims, but also the best possible determination of the position of the comet, and the direction of its course, if these points can (even approximately) be ascertained from the observations of one night.

"5. If the first night's observations are not sufficient to determine all these points with sufficient accuracy, the discoverer must, as soon as he gets a second observation, send *another* communication as above directed, together with a statement of the longitude of the place, if it should not be a known observatory: but the hope of getting a second observation will not be admitted as an excuse for delaying the communication of the first.

"6. The medal is to be adjudged twelve months after the discovery of the comet, and no claim can be admitted after that period has elapsed.

"7. Professor Schumacher and Mr. F. Baily are to determine whether a discovery is to be considered as established or not: but, should they differ in opinion, Dr. Olbers, of Bremen, is to decide between them.

"N.B. Professor Schumacher and Mr. F. Baily have undertaken to communicate to each other, respectively and *immediately*, such information as they may receive relative to the discovery of these comets."—Dated November 1, 1835.

XII. Observations of Moon-culminating Stars, made at the Observatories of Greenwich, Edinburgh, and Cambridge, during the months of June, July, August, September, and October, 1835.

December 11, 1835.—The following communications were read:—

I. Sixth Catalogue of Double Stars observed at Slough, with the 20-feet reflector, in 1831 and 1832. By Sir J. F. W. Herschel. In a letter to Mr. Baily.

As this is, in reality, the *eighth* communication which appears in the Memoirs relative to this subject, it has been considered desirable that the following index should be given to the whole. The first three are not numbered, but are headed differently from each other: and a distinction must be drawn between the series of *lists of measures* and the series of *catalogues*.

Volume and Page.	When read.	Number of first and last Stars.	Name in the Heading.
II, 459	June, 1826	1— 321	Account, &c.
III, 47	May, 1827	322— 616	Approximate Places, &c.
III, 177	Jan., 1828	617—1000	Observations, &c.*
IV, 331	April, 1830	1001—1937	Fourth Series, &c.†
V, 13	June, 1831	1— 735‡	*Micrometrical measures*, &c.
VI, 1	June, 1832	1938—3241	Fifth Catalogue, &c.†
VIII, 37	May, 1834	736—1112‡	*Second Series of measures*, &c.
IX, —	Dec., 1835	3242—3346	Sixth Catalogue, &c.†

These observations (the last of the preceding list) were made at Slough, and would have been forwarded to the Society before the departure of Sir J. Herschel for the Cape of Good Hope, had he not been prevented, by pressure of other business at that time, from re-

* Contains a comparison of Slough and Dorpat observations of 384 stars.

† Only new stars numbered: others referred to other catalogues.

‡ These are *measures*, not *Stars*. [Abstracts of these communications will be found in Phil. Mag. and Annals, and Lond. & Edinb. Phil. Mag., for the above years respectively.—Edit.]

ducing and arranging them. He therefore took the rough observations with him, and availed himself of an interval of leisure to arrange them in the same form as his other catalogues. The stars observed are less in number than those of his former catalogues, but they are in some respects more interesting, from the greater number of delicate and difficult objects comprised; the measures of which, with those in Struve's great work on Double Stars (said to be in course of publication), it will be important to compare. The catalogue contains 105 *new* double stars, as well as the observations of several others that were previously known; the positions and distances of which, as observed by Sir J. Herschel, are here recorded: some of the stars, however, inserted in Struve's catalogue, he has been unable to find in this review of the heavens.

Sir J. Herschel states, that he has nearly gone over the whole south circumpolar region, to 60° from the pole; the observations of which are in the course of arrangement. He is somewhat surprised at the extraordinary paucity of *close double* stars, which cannot arise from want of power in the telescope, or from the nature of the climate: for he considers his mirrors as perfect as it is possible to make them; and he represents the beauty and tranquillity of the climate to be such, that the stars are reduced to all but mathematical points, and thus allow of their being viewed like objects under a microscope. But although the number of double stars is so small, considering the richness of the southern heavens in stars, yet he represents the *nebulæ* as very copious; and has accordingly collected a numerous list, which will doubtless, in due time, be laid before the public.

II. A Letter from the Superintendent of the *Nautical Almanac* to the Secretary, of which the following is a copy:

"*Nautical Almanac Office, Dec.* 6, 1835.

"My dear Sir,—I have the pleasure of forwarding, for distribution, some copies of an Ephemeris of Halley's Comet, founded on the revised elements of the orbit, which appear, as far as I have yet tested them, to represent the observations very well; so well, indeed, that I shall not hesitate to adopt them as the basis of my future proceedings with the cometary calculations.

"An accurate Ephemeris for the period of the Comet's apparition is now absolutely necessary, to enable observers to identify the stars with which the Comet has been compared, to determine parallax, and, finally, to settle their observations. It may, therefore, be as well to apprise you, that such an Ephemeris, founded upon the revised elements, and embracing the period between Aug. 1, 1835, and March 31, 1836, is now in progress, and will be published next week.*

"It is my intention afterwards to determine, for the same period, the effects produced upon the right ascension and declination by a minute variation of each of the elements of the orbit; and, finally, to compute the effects of the disturbing forces of the old planets. These tables will be prepared and published with all possible dispatch. We

* This Ephemeris has since been completed and printed.—Sec.

shall thus have an accurate representation of the Comet's track, derived from good approximate elements, and corrected for perturbations; together with the most ample means of rectifying the orbit, as soon as astronomers shall be prepared with their reduced observations.

"There are many observers who are either unaccustomed to, or have a distaste for, the labour of reducing observations; and very few persons, I apprehend, who will undertake the task of resolving the final equations of condition. I therefore take this opportunity of inviting observers to transmit their observations to me, with a full statement of all particulars necessary to an accurate estimate of their value; and of making known my intention, as soon as the observations can be collected, to get out the best orbit that they are capable of yielding.

"Yours, very truly,

"W. S. Stratford."

"A. De Morgan, *Esq., Sec. Royal Ast. Soc.*"

III. Extract of a Letter from Captain Smyth to the President, containing the translation of a notice from M. Cacciatore:

"One important thing I must communicate to you. In the month of May I was observing the stars that have proper motion; a labour that has employed me several years. Near the 17th star, 12th hour, of Piazzi's Catalogue, I saw another, also of the 7·8th magnitude, and noted the approximate distance between them. The weather not having permitted me to observe on the two following nights, it was not till the third night that I saw it again, when it had advanced a good deal, having gone further to the eastward and towards the equator. But clouds obliged me to trust to the following night. Then, up to the end of May, the weather was horrible; it seemed in Palermo as if winter had returned: heavy rains and impetuous winds succeeded each other, so as to leave no opportunity of attempting anything. When at last the weather permitted observations at the end of a fortnight, the star was already in the evening twilight, and all my attempts to recover it were fruitless: stars of that magnitude being no longer visible. Meanwhile the estimated movement, in three days, was $10''$ in Æ, and about a minute, or rather less, towards the north. So slow a motion would make me suspect the situation to be beyond *Uranus.* I was exceedingly grieved at not being able to follow up so important an examination."

IV. Report on the new Standard Scale of this Society, drawn up at the request of the Council, by Mr. Baily.

The commencement only of this Report, which is very long, was read; the remainder of it being deferred till the next meeting of the Society. An abstract of the whole will be given in our next Number.

V. Observations of Moon-culminating Stars, made at the Observatories of Greenwich, Edinburgh, and Cambridge, during the month of November, 1835.

ZOOLOGICAL SOCIETY.

(*Feb.* 23, *continued.*)—A paper by Mr. Owen was read, entitled, "Descriptions of some new or rare *Cephalopoda*, collected by Mr. George Bennett, Corr. Memb. Z.S." The subjects referred to in it included specimens of *Cranchia scabra*, Leach; a small nondescript *Loligo*; the head and principal *viscera* of a *Decapodous Dibranchiate Cephalopod* from Port Jackson; a small nondescript species of *Octopus*; and a very small specimen of *Argonauta hians*, with its *Cephalopodous* inhabitant (*Ocythoe Cranchii*, Leach), and a large cluster of *ova*: all of which were exhibited, in illustration of the communication, by permission of the Curators of the Museum of the Royal College of Surgeons, of which collection they now form part.

The specimen of *Cranchia scabra* was taken by Mr. George Bennett in a towing net in lat. 12° 15′ S., long. 10° 15′ W.; and was at first regarded by him as a species of *Medusa*: and Mr. Owen observes, that from the uncommon form which this very remarkable *Cephalopod* presents, one cannot feel surprised that it should have been, at the first view, referred by its captor to a *Radiate* family, with which the *Cephalopods* bear, in more than one respect, an analogical relation.

As the type of its genus Mr. Owen considers the *Cranch. scabra* with reference to the generic characters that separate *Cranchia* from the neighbouring groups: from *Loligo* and *Onychoteuthis* it is distinguished by the continuity of its mantle with the dorsal *parietes* of the head; and from *Sepioteuthis, Sepiola*, and *Rossia* by the proportions and position of its fins. The form of the fins alone is evidently insufficient in *Cephalopods* for generic distinctions, as will appear from considering the variations in this respect that occur in the several species of the well-marked genus *Onychoteuthis*, Licht.; and also in the several species of *Loligo* as at present restricted, some of which, especially *Lol. brevis*, Blainv., make so close an approximation to *Cranch. scabra* in the rounded contour, as well as the terminal position, of their fins, that were it not that the exterior margin of the mantle is in all of them free on its dorsal aspect, the latter *Cephalopod*, notwithstanding its singular form, could not be separated generically from the *Loligines* on external characters alone. As in the figures published by Férussac of the *Cephalopods* named *Cranch. cardioptera* by Péron and *Cranch. minima* by himself, the anterior margin of the mantle appears to be free on its dorsal aspect, similarly to that of the true *Loligines*, it must be doubted whether these species are correctly referred to the genus *Cranchia*: and the same doubt may perhaps be extended to *Cranch. Bonelliana*, Fér., in the description of which no mention is made of the adhesion or otherwise of the mantle to the posterior part of the head. This adhesion Mr. Owen regards as an essential character of the genus.

The specimen of *Cranchia scabra* on which the genus was founded by Dr. Leach, having been imperfect in some of its parts, Mr. Owen carefully describes the species anew from the perfect individual obtained by Mr. George Bennett; which is smaller than the original specimen, measuring only 1 inch 8 lines in total length to the end of the outstretched tentacle. The body is remarkable for its great flaccidity, which is owing to the very small space occupied by the *viscera*: these are situated at its anterior part, and not, as in *Loligopsis*, at the bottom of the sac. Besides this disproportion between the bulk of the *viscera* and the capacity of the containing sac, *Cranchia* has other relations with *Loligopsis* in the absence of the infundibular valve, which exists in all the other *Decapodous Cephalopods*; and in the non-articulation of the base of the siphon by a double ball and socket joint to the internal surface of the ventro-lateral parts of the mantle. In the *Decapodous Cephalopods* generally the funnel is articulated to the mantle, at the anterior part of its base, by two ball and socket joints, the projection being on the mantle and the socket on the funnel; both consisting of cartilage, covered with a fine synovial membrane. The projecting cartilage is of an oval form in the *Cuttle-fish*: but in *Loligo* it forms an elongated ridge; which in *Onychoteuthis* commences at the anterior margin of the mantle and extends one third down the sac, forming two thin lateral cartilaginous *laminæ* placed rather towards the ventral aspect of the mantle: an elongated groove in the opposite sides of the funnel plays upon each of these ridges. In *Loligopsis* the sides of the funnel adhere to the corresponding cartilaginous *laminæ*, which differ from the lateral cartilages of other *Decapodous Cephalopods* only by their greater length and tuberculated form. In *Cranchia*, as in the *Octopoda*, these cartilages are entirely wanting; but the ventral *parietes* of the base of the siphon become expanded, thin, and transparent; and adhere to and become continuous with the corresponding parts of the mantle.

Mr. Owen regards as new the species of *Loligo* referred to, and describes it under the name of *Lol. laticeps*: four specimens of it, the largest of which measures only 1½ inch from the extremity of the mantle to the end of the outstretched tentacle, were obtained by Mr. George Bennett among the Sargasso weed, in lat. 29° N., long. 47° W. When alive they were of a fine purple colour with dark red spots. The specimens are now destitute of colour on the fins and on the under surface of the third and fourth pairs of arms, and the spots are but few on the under part of the head and mantle; on the inner surface of the first, second, and third pairs of arms the dark pigment is disposed in broad, irregularly shaped, transverse bands, passing across between each of the pairs of suckers.

The head, as is indicated by the trivial name, is comparatively broad; and the arms which it supports are relatively longer than in the *Loligines* generally, the second and third pairs being nearly equal in length to the trunk. The body is subcylindrical and conical, gradually diminishing in circumference till it terminates in a point at the posterior margin of the fins, which do not extend conjoined together beyond this part. The fins are terminal and dorsal, a space of about half a line intervening between their origins anteriorly, whence their bases converge and are united at the *apex* of the trunk: their superior contour is an obtuse angle; their inferior margin is rounded.

In the *Cephalopod* described as *Cranchia cardioptera*, Pér., to which the species under consideration has a superficial resemblance, the terminal fins have a semicircular contour, and their origins are widely separated anteriorly; they also extend beyond the termination of the trunk: the trunk, moreover, is broader in proportion to the head, and does not diminish gradually to a point, but is rounded off at the posterior extremity. The *Cranchia minima* of Férussac may be at once distinguished from *Lol. laticeps* by the extension of the trunk beyond the small rounded fins, which gives a trilobate contour to the termination of the body.

In internal organization *Lol. laticeps* agrees with the other *Loligines* whose anatomical structure has been ascertained.

The fragments of the *Decapodous Cephalopod* obtained at Port Jackson are too imperfect to allow of their being satisfactorily referred generically: they may, however, have belonged to a species of *Loligo* or of *Sepioteuthis*. As in some species of both these genera, the outer lip was characterized by eight short processes, on the inner surface of which, at the extremity of each, were three or four small suckers, attached by peduncles, and having precisely the same structure as those of the eight large exterior arms. In this repetition of the structure of the external series of cephalic processes there is an evident analogy to the different series of labial processes of *Nautilus*. In some species, as for instance *Lol. Pealii*, Le Sueur, the acetabuliferous labial processes are more developed than in Mr. George Bennett's specimen. In *Lol. corolliflora*, Til., they have been compared by Bojanus to the internal shorter series of tentacles of a *Medusa*; affording another evidence of the analogy, though remote, between the *Cephalopods* and the *Radiata*.

The two lateral processes at the termination of the *rectum* being, in this instance, evidently adapted to form a valve for the closure of the *anus*, Mr. Owen was induced to examine the corresponding structure in other species; and to conclude, from his examination, that similar appendages, although varying in form and position, perform the same office in other *Decapoda*. The slenderness of the anal processes in *Onychoteuthis* and *Loligopsis* being such as to preclude the possibility of their acting as mechanical guards, it is inferred that they may perform the function of instruments of sensation, and convey the stimulus to contract to the muscular parts that close the outlet of the alimentary canal. In the *Octopoda* the *anus* is not similarly provided; and, indeed, it may be generally remarked that valvular or other guards are developed among the *Cephalopoda* only in such as have the power of propelling themselves forwards in the water.

The generative apparatus forming part of the fragments referred to, Mr. Owen examined it with some care. His most important observation relative to these organs relates to a small round flat fleshy body, attached near the anterior aperture of each of the two nidamental glands, destitute of any outlet, and of an orange colour.

A single bilobed organ, of a bright orange or red colour, similarly connected with the anterior extremities of the nidamental glands, exists (as was long since pointed out by Swammerdam) in the *Cuttle-fish*. In *Sepiola* the corresponding body is single, and of a rose colour. And there exist two such bodies in a small *Cephalopod* taken by Capt. Ross on the shore of Boothia, which Mr. Owen has recently described under the name of *Rossia palpebrosa*. Considering the bright colours which these bodies commonly present, and their structure and relations to the generative apparatus, Mr. Owen feels authorized in regarding them as analogous to the suprarenal bodies, hitherto regarded as peculiar to the *Vertebrate* series.

The small *Octopus* described by Mr. Owen was obtained by Mr. George Bennett, like the *Loligo laticeps*, among the Sargasso weed; which forms, as it were, a bank in the midst of the ocean, affording shelter to many marine animals of littoral genera. The condition of the generative organs would appear to indicate that the specimens brought home were not adult, and the species consequently may be assumed to attain a greater size than that of the largest individual in the collection, which measures only $1\frac{1}{2}$ inch from the end of the sac to the extremity of the longest arm. Of the eight arms the first, or dorsal, pair is the longest, as is the case in many species of *Octopus*; the second pair is nearly of the same length as the first; the third pair (which in the *Decapods* is commonly the longest) is scarcely half the length of the first; the fourth pair is nearly two thirds of the length of the first. The musculo-membranous web, which is usually extended between the bases of all the arms in the *Octopi*, is in this species developed to the ordinary extent between the four dorsal arms only: the webs between the second and third arms, and the third and fourth arms, on each side, are very short; that between the fourth pair is wanting. From this peculiarity Mr. Owen proposes to name the species *Octopus semipalmatus*.

Its anatomy generally agrees with that of *Oct. vulgaris*.

The remaining specimens described by Mr. Owen are the shell and animal of *Argonauta hians*, Lam. They were obtained in lat. 4° S., long. 17° W. The animal was alive at the time of its capture by Mr. George Bennett, but fell out of its shell when it was moved on the following morning. A mass of eggs was then exposed in the involuted portion of the shell, which increased so greatly in size after being put into spirit that they now occupy so much of the cavity that not more than one third of the body of the parent could be forced into it.

Referring to the fact that the *Cephalopods* hitherto found in the shells of each species of *Argonauta* have invariably presented characters as specifically distinct as those of the shells in which they were found, each species of animal having appropriated to it its own peculiar species of shell—a fact which extends not only to *Arg. Argo*, *Arg. tuberculata*, and *Arg. hians*, but also to an undescribed species obtained in the Indian seas by Capt. P. P. King, R.N., for which Mr. Owen proposes the name of *Arg. rufa*, he is disposed to believe that the shell really belongs to the animal that occurs in it.

On this account he speaks of the animal in question as the *Arg. hians*, discarding the name of *Ocythoë Cranchii* applied to it by Dr. Leach.

In carefully describing the specimen before him, Mr. Owen corrects some errors in the account given of the animal by its original describer, and furnishes various particulars which, from the contracted state of his individuals, were unobserved by Dr. Leach. He also adverts to the statement made by that able zoologist, that in this species all the internal organs are essentially the same as in *Octopus*: and remarks that *Arg. hians*, like *Arg. Argo*, recedes from the naked *Octopods* and approaches the *Decapods* in the structure of the branchial hearts, which are provided with a fleshy appendage, in the form of the appendages of the *vena cava*, which are shorter and thicker; and in the relative position of the lozenge-shaped ink-bag, which is not buried in the substance of the liver, but lies in its anterior concavity: the inferior salivary glands are also relatively smaller. The following differences, as compared with *Octopus*, occur in other internal organs which adhere to the type of structure that characterizes the *Octopodous* tribe of the *Dibranchiata*: the laminated pancreatic bag is of a triangular form, and not spirally disposed; the two oviducts are devoid of the circular laminated glands which surround them in *Octopus* about the middle of their course; they are also disposed in four or five convolutions as they pass behind the roots of the *branchiæ*; and they terminate at a relatively greater distance from the base of the funnel.

Mr. Owen then describes various portions of the internal structure of *Argonauta*; and especially its brain, its principal nervous cords, and the lateral muscles, here at their minimum of development, which attain in *Nautilus*, as the muscles of attachment to the shell, so enormous a size.

The eggs are in nearly the same state of development as those which have been described by Mr. Bauer and by Dr. Roget; and consequently afforded no conclusive proof as to the nature of the connexion of the animal with the shell. In one of them, from the form of the opake body contained within it, Mr. Owen for a moment entertained the idea that the *nucleus* of the real shell might be found: on tearing open, however, the external tissue, the contained substance turned out to be nothing more than the yelk, separated by an intervening stratum of clear fluid from the transparent *membrana vitelli*; and the whole substance of the opake mass separated into the flakes, granules, and globules of oil, of which the *vitellus* is usually composed: there was not a trace of any consistent parts of an embryo, nor the slightest particle of calcareous matter.

Mr. Owen concludes his communication by a tabular view of the *Cephalopoda*, exhibiting the external and internal characters common to the entire class; those of the several orders and families comprised in it; and the names of the genera included in each family.

March 8.—Mr. Ogilby read a paper, entitled "Observations on the opposable power of the Thumb in certain *Mammals*, considered as a zoological character: and on the Natural Affinities which subsist between the *Bimana*, *Quadrumana*, and *Pedimana*."

In the summer of 1829 it occurred to Mr. Ogilby to observe that two living individuals of *Mycetes Seniculus* did not use the extremities of their anterior limbs for the purpose of holding objects between the fingers and thumb, as is common among the *Quadrumana*; and he ascertained also, on closer examination, that the thumb, as it has generally been considered, was not in these animals opposable to the other fingers, but originated in the same line with them. Struck with the apparent singularity of the fact, he was induced to pay particular attention to all the other animals, referred by zoologists to the *Quadrumanous* family, to which he had access; and the continued observation of more than six years has assured him that the non-opposable character of the inner finger of the anterior extremities, which he first observed in the specimens referred to, is not confined to the genus *Mycetes*, but extends throughout the whole of the genera of the South American *Monkeys*, individuals of all of which have now been seen by him in the living state. In none of them, consequently, does a true thumb exist on the anterior limbs: and as a further consequence it follows, that the whole of them have hitherto been incorrectly referred to the *Quadrumana* by zoologists generally. There is a solitary exception among descriptive writers from this mode of viewing the subject, D'Azara (as Mr. Ogilby has very recently become aware) having spoken of the anterior extremities of some of the species observed by him as having five fingers originating on the same line with each other: but the statements of that original observer appear, in this respect, either to have been unnoticed by other authors or to have been passed by as undeserving of attention, so entirely were they at variance with the preconceived notions of all.

Of the eight natural genera which include all the known *Monkeys* of the Western Hemisphere, one, *Ateles*, is entirely destitute of a thumb, or has that member existing only in a rudimentary form beneath the skin. In five others, *Mycetes*, *Lagothrix*, *Aotus*, *Pithecia*, and *Hapale*, the anterior thumbs (using the ordinary expression for them) are placed absolutely on the same line with the other fingers, are of the same form with them, act invariably in the same direction, and are totally incapable of being opposed to them. In the two remaining genera, *Cebus* and *Callithrix*, the extremities of the anterior limbs have a greater external resemblance to the hands of *Man* and of the *Monkeys* of the Old World: the internal finger is placed further back than the general line of the other fingers, and has, on that account, when superficially noticed, the semblance of being opposed to them; but, as has been correctly observed by D'Azara with reference to *Ceb. capucinus*, it is less separated than in *Man*: it is, besides, of precisely the same slender form with the rest, is weaker than them, absolutely without power of opposition to them, and habitually acts in the same direction with they. The impression derived from contemplating the hands of the Old World *Monkeys* might induce the belief that the extremities of the *Cebi* are similarly constituted: but if the knowledge that in *Mycetes*, *Pithecia*, &c., there are no opposable thumbs, lead to a close observation of the anterior extremities of the *Cebi*, it will be found that they do not act as hands, and cannot be considered as possessing the powers of those organs. From innumerable observations of many species of that genus Mr. Ogilby states that it was very evident, notwithstanding the fallacious appearance occasioned by the backward position of the organ, that they had not the power of opposing the thumb to the other fingers in the act of prehension: and, in fact, their principal power of prehension seems to be altogether independent of the thumb, for, generally speaking, that member was not brought into action at all, at least not simultaneously with the other fingers, but hung loosely on one side, as Mr. Ogilby has seen it do, in like circumstances, in the *Opossums*, *Phalangers*, and other arboreal *Mammals*: when actually brought into play, however, the thumb of the *Cebi* invariably acted in the same direction as the other fingers. *Cebus* consequently agrees in the character of non-opposableness of thumb with the nearly allied genera. And in this hitherto unsuspected peculiarity zoologists obtain a far more important character by which to distinguish the *Monkeys* of the Old and New World than that hitherto relied on, the comparative thickness of the *septum narium*, or than the accessory aids afforded by the absence of cheek-pouches and callosities. Hence, according to Mr. Ogilby, as the *Monkeys* of America have now been ascertained to be destitute of anterior hands, they can be no longer included among the *Quadrumana*; and he proposes in consequence to regard them as *Pedimana*. He considers that in the latter series, the *Monkeys* of America form a group parallel to that of the *Monkeys* of the Old World among the *Quadrumana*: and viewing the *Quadrumana* as consisting of two primary groups, that of which *Simia* forms the type, and the *Lemuridæ*, he proceeds to analyse the *Pedimana* in order to determine whether any group analogous to the *Lemurs* exists in it. He finds such a group in the association of the genera *Didelphis*, *Cheironectes*, *Phalangista*, *Petaurus*, and *Phascolarctos*, (together with a new genus, *Pseudochirus*, which he has found it necessary to separate from *Phalangista* as at present constituted); and for this association he uses the name of *Didelphidæ*. Aware that the modifications observable in the dentary systems of these several genera have been regarded by many zoologists as betokening a difference of regimen, which has led to their being viewed as constituting distinct families; he, in the first place, states, as the result of his observation of the habits of the numerous species of all these genera which have been, from time to time, exhibited in the Society's Gardens, that there is little or no difference, in this respect, between the *Opossums* and *Phalangers*, but that all are equally omnivorous; and then proceeds to discuss the modifications that exist among them in the number and form of the several kinds of teeth, which are not, in his estimation, so very different in reality between the *Opossums* and *Phalangers* as they appear to be at first sight. In further support of his opinion that this association of genera forms a natural family, Mr. Ogilby refers to the gradual and uninterrupted transition from the naked-prehensile-tailed *Opossums* of South America, through the equally naked-tailed *Couscous, Balantia,* of the Indian Isles, to the true *Phalangers*; and from these to the *Petaurists* directly on the one hand, and by means of the *Pseudocheirs* to the *Koalas* on the other.

On the prehensile power of the tail Mr. Ogilby particularly insists, as on a faculty possessed by the greater number of the *Pedimana,* and as one which is, in truth, almost confined to them: only three known genera belonging to other groups, *Synetherus, Myrmecophaga,* and *Cercoleptes,* being endowed with it. He remarks on this faculty as on one of considerable importance, affording as it does, in some degree, a compensation for the absence of opposable thumbs on the anterior limbs. Combined with the prehensile tail, in every known instance, whether among the *Pedimana* or in other groups, is a slowness and apparent cautiousness of motion, not observable in any of the *Quadrumana* except in the *Nycticebi.* In none of the true *Quadrumana* is the tail prehensile.

Another evidence of the distinctness, as two groups, of the *Quadrumana* and the *Pedimana,* is furnished by their geographical distribution. The *Quadrumana* are strictly confined to the limits of the Old World: the *Pedimana,* almost as exclusively to the New World; for Mr. Ogilby considers the continent of Australia to belong more properly to America than to Asia. The very few apparent exceptions that occur to this latter position are in the presence of some species of *Phalangers* in the long chain of islands that connect the south-eastern shores of Asia with the north-eastern coast of Australia; islands which may, in truth, be fairly regarded as belonging partly to the one and partly to the other, and the productions of which might consequently be expected to partake of the character of both.

Mr. Ogilby subsequently adverts to another *Pedimanous* animal, the *Aye-Aye* of Madagascar, constituting the genus *Cheiromys*; respecting the affinities of which he speaks with hesitation, because, having never had an opportunity of examining the animal itself, he is acquainted with its characters only at second-hand. He is, however, disposed to regard it as representing a third group among the *Pedimana,* to be placed in a station intermediate between the *Monkeys* of the New World and the *Didelphidæ.* With the latter he would, in fact, be disposed to associate it, were it not destitute of the marsupial character which belongs to all the other animals comprised in that group. In some of the *Didelphidæ,* the *Phalangers* and *Petaurists* especially, there is a marked approximation to that rodent form of incisor teeth which obtains in *Cheiromys,* and which has hitherto been regarded as especially attaching to it an abnormal character.

Man is the only other animal furnished with hands; and however distinct he may be as regards his moral and intellectual powers, he must, zoologically, be considered on physical grounds. By his structural characters he becomes associated with all those of which mention has previously been made in Mr. Ogilby's communication;

although he unquestionably constitutes among them a peculiar group, sensibly exalted above the rest, as well as above all other *Mammals*.

Mr. Ogilby concludes by proposing the name of *Cheiropeds, Cheiropoda*, to include all the *Mammals* that are possessed of hands; and by subjoining a table of the families and genera included in this order, as he regards it. Of this table the following may be regarded as an abstract.

Class. MAMMALIA.
Order. CHEIROPODA,
Mammals with opposable thumbs

On the anterior extremities only	BIMANA.
On both anterior and posterior extremities....	QUADRUMANA.
And with anthropoid teeth, *Monkeys of the Old World.*	
——— abnormal teeth, *Lemuridæ.*	
On the posterior extremities only............	PEDIMANA.
And with anthropoid teeth, *Monkeys of the New World.*	
——— rodent teeth, *Cheiromys.*	
——— abnormal teeth, *Didelphidæ.*	

March 22.—Notes by Mr. Martin on the visceral and osteological Anatomy of the *Çariama, Dicholophus cristatus*, Ill., were read, and are given in No. xxxix. of the "Proceedings."

In illustration of Mr. Martin's Notes, the mounted skeleton of the *Çariama* was exhibited; as were also preparations of several of the *viscera*.

Notes by Mr. Martin, of the anatomy of a specimen of *Buffon's Touraco, Corythaix Buffonii*, Vaill., were subsequently read, and are also given in No. xxxix. of the "Proceedings."

Mr. Bennett directed the attention of the Meeting to an interesting series of the *Indian Antelope, Antilope Cervicapra*, Pall., now at the Society's Gardens. It consists of four individuals: an adult and aged male, brought by Col. Sykes from Bombay, and presented by him to the Society nearly five years ago; a younger, yet adult, male, which was presented, in an immature condition, about two years since; an immature male, lately arrived in the Menagerie, and in about the same state of development as that in which the last-mentioned individual was when it was originally presented; and an emasculated individual of full growth. In the older of these *Antelopes* the rich deep colour of the body generally is so intense as almost to approach to black, and the horns are strong and fully developed: the possession of horns and the depth of colouring, which are peculiar to the male sex, are exhibited in it at their maximum. The second individual approximates nearly to it in the degree in which these secondary sexual characters are developed. In the third, the youngest of the series, there exist the horns characteristic of the male, but these organs are yet of small growth, are only beginning to be annulated at their base, and are commencing their first spiral turn; its colour, as is very generally the case among the young of animals that in adult age are differently coloured in the sexes, is that of the female, which in this instance is a dull fawn with a pale stripe along the side: it has, consequently, in these two striking particulars, full evidence of immaturity. The emasculated individual was probably, at the period when that accident or operation occurred which prevented the development of its sexual characters, at nearly the same age as the one last adverted to: it has since continued to increase in bulk, and it even exceeds in size, as often happens in castrated animals, the perfect adult male of the same species: but the secondary sexual characters of the male have not been developed in it: it retains the dull fawn colour of immaturity, and its horns have not acquired the strength, the annulation, or the spiral turns which belong to those of the adult and perfect male. One of the horns has been broken off; perhaps the more readily from some weakness in its structure, consequent on its unimportance to an animal so degenerated: the other retains, at a short distance from its normally formed tip, a few rings, but beyond these the surface has become smooth, the substance remains weak and comparatively small, and the direction, instead of being in a succession of spiral turns, is in a single sweep, passing backwards above the base of the ear and then descending along the curve of the neck: it has, though weaker, much of the character of the horns of the African race of *Sheep*. The general appearance of the animal is also sheep-like and tame.

Mr. Bennett proceeded to remark that these animals, although curious and interesting on account of the variations exhibited by them, in accordance with their several conditions, in those acknowledged secondary sexual characters, colour and horns, were yet more interesting when considered with reference to the state of another organ, the use of which has long remained a problem to zoologists, but which, it appeared to him, must be referred to sexual relations; he alluded now to the lacrymal sinus. Referring to its structure as to that of a sac, opening externally by a lengthened slit, but perfectly closed within, he remarked, that that organ could not possibly be in any degree connected with the functions of respiration; there being no aperture through it for the passage of air. Its inner surface is covered by a smooth skin, with a few scattered and very short bristles, and is defended by a dark-coloured and copious secretion of ceruminous matter, which has a slight urinous or sexual odour. He did not feel himself competent, he stated, to explain the precise manner in which this organ is available for sexual purposes; yet he felt convinced that such is its use, from the consideration of its relative development in the several *Indian Antelopes* of the Society's Menagerie.

In the more aged of these individuals, as indeed in the adult *Indian Antelope* generally, the large cutaneous follicle beneath the eye known as the lacrymal sinus, is so prominent as to form a most striking feature in the animal's physiognomy: it never appears as a simple slit, its thickened edges pouting so widely as to be at all times partially everted. When the animal is excited, and it is constantly highly excitable, the eversion of the bag becomes complete, and its thick lips being thrown widely back, the intervening space is actually forced forwards so as to form a projection instead of a hollow: the animal is, on such occasions, delighted to thrust repeatedly the naked lining of the sac against any substance that is offered to him, which soon becomes loaded with the odour that has been referred to as belonging to the secretion. In the second individual, although it is perfectly mature, the protrusion of the inner surface of the sac is not quite to so great an extent as in the more aged male; and the less thickened edges of the sinus allow of a nearer approximation to its closure in the unexcited state of the animal. The youngest male has the lips of the sinus small and closely applied to each other, so as to hide completely the whole of the internal lining of the sac, and to exhibit, externally, a mere fissure: in it the lips are but slightly moved when the animal is interested. The emasculated individual, notwithstanding its full growth, has its suborbital sinus nearly in the same condition as that of the immature male: it is merely a slight fissure, the edges of which are closely applied to each other; and in it those edges do not appear to be at all moved, the animal being generally careless and inanimate. It would consequently seem that the same cause which induced the retention, by this individual, of its immature colours, and which arrested the perfect growth of its horns, was adequate also for the checking of the development of the suborbital sinuses. Those organs, therefore, would appear to be dependent on sexual perfection; and consequently to be, in some manner yet to be ascertained, subservient to sexual purposes, with the capacity for which they are evidently, in the phases of their development, essentially connected.

Mr. Owen, who had conceived it possible that the secretion of these glands, when rubbed upon projecting bodies, might serve to direct individuals of the same species to each other, remarked that he had endeavoured to test the probability of this supposition by preparing a tabular view of the relations between the habits and habitats of the several species of *Antelopes,* and their suborbital, maxillary, post-auditory, and inguinal glands; in order to be able to compare the presence and degrees of development of these glands with the gregarious and other habits of the *Antelope* tribe. He stated, however, that it was evident from this table, that there is no relation between the gregarious habits of the *Antelopes* which frequent the plains, and the presence of the suborbital and maxiliary sinuses; since these, besides being altogether wanting in some of the gregarious species, are present in many of the solitary frequenters of rocky mountainous districts. The supposition, therefore, that the secretion may serve, when left on shrubs or stones, to direct a straggler to the general herd, falls to the ground.

Mr. Owen's Table is as follows:

Suborbital and maxillary sinuses.	Inguinal pores.	*Antilope Sumatrensis.* Hilly forests; habits of the Goat.
Suborbital sinuses large.		*Cervicapra.* Open plains of India; gregarious.
		quadriscopa. Senegal.
?		*melampus.* Open plains of Caffraria; flocks of six or eight.
?		*Forfex.* Africa.
?		*adenota.* Africa.
?		*quadricornis.*
		picta. Dense forests of India
		scoparia. Open plains of S. Africa; subgregarious.
		Tragulus. Stony plains and valleys of S. Africa; in pairs.
		melanotis. Plains, hides in underwood; in pairs.
		Dorcas. Borders of the desert; gregarious.
		Kevella. Stony plains, Senegal; gregarious.
		subgutturosa. Plains, Central Asia; gregarious.
		Bennettii. Rocky hills of Deccan; not gregarious.
		Arabica. Stony hills of Arabia.
		Sœmmerringii. Hills in Abyssinia; not gregarious.
		Euchore. Dry plains of S. Africa; gregarious.
		pygarga. Plains, S. Africa; gregarious.
		Mhorr. Deserts of Morocco.
		Dama.
		ruficollis. Deserts of Nubia; gregarious.
small.		*Antilope Colus.* Vicinity of lakes; gregarious, migratory.
		gutturosa. Arid deserts, Asia; periodically gregarious.
Suborbital sinuses.	No inguinal pores.	*Antilope Saltiana.* Mountainous districts, Abyssinia; in pairs.
		Oreotragus. Mountains of the Cape; like the *Chamois.*
		Thar. Hills of Nepaul; not gregarious.
		Gazella. Senegal. ?
Suborbital glands.		*Antilope Bubalis.* Mountains and deserts, Tripoli; gregarious.
		Caama. Plains of S. Africa; gregarious.
		? *lunata.* S. Africa.
		Gnu. Karroos of S. Africa; gregarious.
		taurina s. *Gorgon.* S. Africa; gregarious.
Maxillary sinuses.	Inguinal pores.	*Antilope silvicultrix.* Thickets and underwood, Africa.
		mergens. Forests and underwood, S. Africa; in pairs.
		Grimmia. Guinea.
		Burchellii.
		platous.
		perpusilla. Bushes, S. Africa; in pairs.
		Maxwellii.
		pygmæa.

No suborbital, or maxillary sinuses.	Inguinal pores.	*Antilope Strepsiceros.* Woods and banks of rivers, Caffraria; subgregarious.
		sylvatica. Woods, Caffraria; in pairs.
		scripta.
		Koba. Senegal.
		Kob. Senegal.
		Eleotragus. Reedy banks, Cape; subgregarious.
		redunca. Goree.
		Capreolus. Underwood, S. Africa; subgregarious.
		Landiana. Underwood, S. Africa; subgregarious.
(Post-auditory sinuses.)		*Antilope Rupicapra.* Mountains, Europe; subgregarious.
No suborbital, or maxillary sinuses.	No inguinal pores.	*Antilope Addax.* Deserts, N. Africa; in pairs.
		Leucoryx. Acacia groves, N. Africa; gregarious.
		Oryx. Woods and plains, S. Africa; subgregarious.
		leucophæa. Open plains, S. Africa; subgregarious.
		barbata. Open plains, S. Africa; in pairs.
		equina. Plains, S. Africa; in pairs.
		ellipsiprymnus. S. Africa.
		Oreas. Open plains, S. Africa; gregarious.
		Canna. Deserts, Cape; gregarious.
		Goral. Elevated plains, Himalaya; gregarious.

Mr. Ogilby remarked, with reference to this subject, that he had had opportunities of observing, at the Surrey Zoological Gardens, a female of the *Indian Antelope*, in which, when he first saw her, the lacrymal sinus was in a state of quiescence : but when he observed her again, a month afterwards, and probably in improved condition, that organ was in a state as excitable as it is in the old male of the Society's Gardens.

He added, as a general remark, which, however, he stated was not universal, that in intertropical animals the lacrymal sinus is larger than in more northern species, and in those whose range is limited to mountainous districts.

He also described the lacrymal sinus of a species of *Gazelle*, which he had observed after death : it consisted of a gland furnished with six excretory ducts placed nearly in a circle, and with one central duct : from the orifices of these ducts, when squeezed, there issued out strings of a dense ceruminous matter.

Mr. Bennett stated in conclusion, that since making his observations on the *Indian Antelope*, which had led him to form the opinion he had advanced with respect to the use of the lacrymal sinus, he had received from Mr. Hodgson of Nepal, a Corresponding Member of the Society, a letter in which, among other subjects, some remarks are made on this organ as it exists in the *Thar Antelope*, and in the *Cervus Aristotelis*: in the former of those animals, Mr. Hodgson's observations prove that during the breeding-season the lacrymal sinus is in a high state of activity. Mr. Hodgson's letter, which is dated Nepal, June 18, 1835, refers also to other glands in some other *Antelopes*, as will be seen by the following extract.

"The *Chiru Antelope* has exceedingly large inguinal sacs, which hang by a long narrow neck from the loins. The longitudinal quasi maxillary gland of the *Cambin Otan* I doubt the existence of, and believe its 'suborbital sinus' to be similar to that of *Thar*.

"The latter differs essentially from that organ in any *Deer* or *Antelope* I have seen; being furnished with a huge gland, filling the whole cavity or depression on the scull, and leaving the cuticular fold void of hollowness: it is filled up, like the bony depression, by the gland; whereas the gland of this sinus, in most *Deer* and *Antelopes*, is a tiny thing, and a dubious one. As to any *Cervine* or *Antilopine* animal breathing through the suborbital sinus, it cannot be, unless they can breathe through bone and skin! If you pass a fine probe down the lacrymal duct, you see the probe through the bottom of the osseous depression holding the cuticular fold called the suborbital sinus. But, however thin the plate of bone at the bottom of the former, it is there, without breach of continuity; and the cuticular portion of the apparatus has a continuous course throughout, leaving no access to the inside of the head. I am watching closely a live specimen of *Cervus Aristotelis*, to discover, if I can, the use of this organ. In a recently killed male of this species, I passed a pipe into the nose, up to the site of the suborbital sinus, and tried, in vain, for half an hour, with the aid of a dozen men's lungs, to inflate the sinus. Not a particle of air would pass; nor could I cause the sinus to unfold itself, as the live animal unfolds it, by means of a set of muscles disposed crosswise round the rim of it. In dissecting the sinus, I found only a feeble trace of a gland; so also, in the *Muntjac*.

"But in the *Thar*, the gland is conspicuous, being a huge lump of flesh, bigger than, and like in shape to, the yolk of an egg. The live *Thar*, too, in the spring especially, pours out a continuous stream of thin viscid matter from the sinus; not so in any *Deer*. The *Thar's* gland seems to me connected with the generative organs: and I take its profuse secretion to be a means of relieving the animal (when it has no mate particularly) from the extraordinary excitement to which it is liable in the courting-season. I have witnessed that excitement, and have been amazed at its fearful extent, topical and general, for six weeks and more.

"The *Chiru's* labial sacs, or intermaxillary pouches, are, most clearly, accessory nostrils, designed to assist breathing at speed. They spread with the dilatation of the true nostril, and contract with its contraction. This species has but five molar teeth on each side of either jaw."- B. H. H.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The following is a list of the Officers of the Association appointed at the late Meeting at Bristol :

President.—The Earl of Burlington.

Vice-Presidents.—Dr. Dalton, F.R.S. Sir P. Egerton, Bart., F.R.S. Rev. E. Stanley, F.G.S.

Treasurer.—John Taylor, F.R.S.

General Secretaries.—Rev. W. V. Harcourt, F.R.S. R. I. Murchison, F.R.S.

Assistant General Secretary.—Prof. Phillips, F.R.S.

Secretaries for the Liverpool Meeting.—W. C. Henry, M.D., F.R.S. C. S. Parker, Esq.

Treasurer for the Liverpool Meeting.—S. Turner, F.R.S.

Members of Council.

Members elected.—Prof. Christie, F.R.S. Prof. Daniell, F.R.S. G. B. Greenough, F.R.S. H. Hallam. Prof. Henslow. R. Hutton, F.G.S. Sir W. Rowan Hamilton, F.R.S. J. W. Lubbock, F.R.S. Prof. Lindley, F.R.S. Prof. Owen, F.R.S. Prof. Powell, F.R.S. John Robison, Sec. R.S. Edinb. N. A. Vigors. Rev. W. Whewell, F.R.S. Wm. Yarrell, F.R.S.

Members ex-officio.—The Trustees: C. Babbage, F.R.S. R. I. Murchison, F.R.S. John Taylor, F.R.S. The Officers named above.

Secretaries.—E. Turner, M.D., F.R.S. Rev. James Yates, F.L.S.

The Reports which have been undertaken for the ensuing Meeting are as follows :

Capt. Sabine. A continuation of his Report on the state of knowledge as to the Phænomena of the Earth's Magnetism.

Mr. Lubbock. The result of the deliberations of a Committee appointed to consider a proposition of that gentleman for the construction of new Empirical Lunar Tables. The Committee consists of the Astronomer Royal, Mr. Baily, Prof. Challis, Sir W. R. Hamilton, Mr. Lubbock, Prof. Rigaud.

Prof. Johnston. On the present state of knowledge of the Chemical and Physical Properties of Dimorphous Bodies in their respective forms.

Mr. J. Taylor. On the Mineral Riches of Great Britain, in relation more particularly to Metalliferous Districts.

Mr. Yarrell. On the state of knowledge of Ichthyology.

Rev. Wm. Taylor. On the various Methods of Printing which have been prepared for the use of the Blind.

The following is a statement of the new and renewed Grants of Money for the advancement of particular branches of science, which have been made by the Association at this Meeting.

Mathematical and Physical Science.

250*l.* For the discussion of Observations of the Tides; at the disposal of J. W. Lubbock, Esq.

150*l.* For the discussion of Observations of the Tides in the port of Bristol; at the disposal of the Rev. W. Whewell.

70*l.* For the Deduction of the Constant of Lunar Nutation; under direction of Sir T. Brisbane, Mr. Baily, and Rev. Dr. Robinson.

30*l*. For Hourly Observations of the Barometer and Wet-bulb Hygrometer; at the disposal of W. S. Harris, Esq.

100*l*. Renewed grant for the establishment of Meteorological Observations and Experiments on Subterranean Temperature; at the disposal of a Committee, consisting of Prof. Forbes, W. S. Harris, Esq., Prof. Powell, Col. Sykes, and Prof. Phillips, who will act as Secretary.

500*l*. Enlarged grant for the procurement of accurate Data to determine the question of the Permanence or Variability of the Relative Level of Land and Sea. The Committee consists of Mr. Baily, Mr. De la Beche, Col. Colby, Mr. Cubitt, Mr. Greenough, Mr. Griffith, Mr. Lubbock, Capt. Portlock, Mr. G. Rennie, Rev. Dr. Robinson, Prof. Sedgwick, Mr. Stevenson, and Rev. Wm. Whewell, who will act as Secretary.

100*l*. For Experimental Investigations on the Form of Waves; at the disposal of John Robison, Esq., and J. S. Russell, Esq.

500*l*. Renewed grant for the Reduction of Observations in the *Hist. Céleste* and in the volumes of the *Académie des Sciences* for 1789 and 1790; under the direction of the Astronomer Royal, F. Baily, Esq., J. W. Lubbock, Esq., and Rev. Dr. Robinson.

150*l*. For experiments on Vitrification; under the direction of Dr. Faraday, Rev. W. V. Harcourt, and Dr. Turner.

80*l*. Renewed grant for the construction of a rock-salt Lens; under the direction of Sir D. Brewster.

Chemical and Mineralogical Science.

50*l*. Renewed grant for Researches on the Specific Gravity of Gases; under the direction of Dr. Dalton and Dr. C. Henry.

30*l*. For Researches on the Quantities of Heat developed in Combustion and other chemical Combinations; at the disposal of Dr. Turner.

15*l*. For Researches on the Composition of Atmospheric Air; at the disposal of Dr. Dalton and Mr. W. West.

24*l*. 13*l*. For the publication of Tables of Chemical Constants drawn up by Prof. Johnston.

60*l*. For Researches on the Strength of Iron made with hot and cold blast; at the disposal of Messrs. Fairburn and Hodgkinson.

Geology and Geography.

20*l*. Renewed grant for Experiments on the Quantity of Mud suspended in the water of Rivers; under the direction of Mr. De la Beche, Mr. G. Rennie, and Rev. J. Yates.

30*l*. For special Researches on Subterranean Temperature and Electricity; at the disposal of Mr. R. W. Fox.

50*l*. For Researches into the Nature and Origin of Peat Mosses in Ireland; under the direction of Col. Colby.

Botany.

25*l*. For experimental Researches on the Growth of Plants under glass and excluded from air, according to the plan of Mr. Ward; under the direction of Dr. Dalton, Dr. Daubeny, Rev. James Yates, and Prof. Henslow, who will act as Secretary.

Medical Science.

50*l*. Renewed grant to the Committees appointed to investigate the subject of the Anatomical Relations of the Absorbents and Veins.

50*l*. Renewed grant to the Committees appointed to investigate the Motions and Sounds of the Heart.

25*l*. For Researches into the Chemical Composition of Secreting Organs; under the direction of Dr. Hodgkin, Dr. Roget, Dr. G. O. Rees, and Dr. Turner.

25*l*. For Investigations on the Physiological Influence of Cold on Man and Animals in the Arctic Regions; at the disposal of Mr. King.

25*l*. Renewed grant for the Investigation of the Effects of Poisons on the Animal Economy; under the direction of Dr. Roupell and Dr. Hodgkin.

25*l* Renewed grant to the Committee formerly appointed to investigate the Pathology of the Brain and Nervous System. Of this Committee Dr. O'Beirne has been requested to act as Secretary.

25*l*. For Investigations on the Physiology of the Spinal Nerves; under the direction of Mr. S. D. Broughton, Mr. E. Cock, and Dr. Sharpey.

Statistics.

150*l*. For Inquiries into the actual State of Schools in England, considered merely as to numerical analysis; under the direction of Mr. Hallam, Mr. Porter, and Col. Sykes.

Mechanical Science.

50*l*. Renewed grant for an Analysis of the Reports of the Duty of Steam-Engines in Cornwall; under the direction of Mr. Cubitt, Mr. J. Rennie, and Mr. John Taylor.

LXII. *Intelligence and Miscellaneous Articles.*

ON SEVERAL NEW COMBINATIONS OF PLATINUM. BY J. W. DOEBEREINER.

THE cyanuret of potassium and platinum, produced according to the method of L. Gmelin, is known to form, with a solution of the acid protonitrate of mercury, a beautiful smalt-blue precipitate, with the disengagement of a small quantity of nitrous gas.

On a closer examination this precipitate affords several interesting appearances and new products (cyanuret of mercury and platinum, cyanuret of platinum, and a compound of hydrocyanic acid and cyanuret of platinum), as well as the proof that platinum and cyanogen are mutually disposed to enter into very close combinations.

This precipitate may be washed with cold water acidulated by nitric acid and then dried, without suffering any alteration in its colour. But if boiled with this water, it yields protonitrate of mercury, and becomes quite white. If a solution of protonitrate of mercury be poured upon this white precipitate and left at rest on it at ordinary temperatures for several hours or longer, the precipitate gradually becomes as blue as it was at first; and after some days the intensity of this colour increases, if the nitrate of mercury remains in excess: if the precipitate be heated with the latter it remains white, but after the evaporation of the water of the nitrate of mercury, it becomes blue, and if the heat be increased, orange red.

When the blue and orange red precipitate is strongly heated on a piece of platinum foil, it detonates, giving out sparks and smoke, and flying on all sides like sky-rockets, with a hissing noise. It is dissolved in heated muriatic acid, with the formation of nitric acid and hydrocyanic acid, and forms a colourless solution which is not rendered turbid by alcohol nor precipitated by muriate of ammonia.

The white precipitate kindles when heated on platinum foil without detonating, burns away without flame, and leaves behind about 38 per cent. of spongy platinum, strongly disposed to ignition. Boiling muriatic acid dissolves it, but without development of nitric or hydrocyanic acid, and forms an almost colourless solution, which a solution of potash turns yellow, and which, on evaporation to dryness, gives a residue, appearing partly red, partly yellow, and partly blue, and this by a strong heat is decomposed into hydrocyanic acid, chloride of mercury, and cyanuret of platinum.

The solution of the alkalies and alkaline earths decompose the coloured and the bleached precipitates, and separate, from the first, protoxide and peroxide of mercury, from the latter, only peroxide; at the same time they leave their radical in combination with the cyanogen of the cyanuret of mercury, and in this combination form, with the cyanuret of platinum, easily crystallizable double cyanurets of platinum. When the white precipitate is gradually heated to redness in a glass retort, it is decomposed into cyanogen gas, fluid mercury, and cyanuret of platinum. The quantity of the latter is about 48 per cent. (4·8 grains from 10 grains on which I experimented); we may therefore consider the white precipitate as a combination of

$$\left.\begin{array}{l}\text{48 Cyanuret of Platinum}\\ \text{52 Cyanuret of Mercury}\end{array}\right\} = \text{Pt Cy} + \text{Hy Cy, or Pt Hy Cy}_2,$$

and we may consider the blue (and red) precipitate as a combination of this double cyanuret with protonitrate of mercury. I have not yet examined in what relation these salts stand to one another, and whether the blue colour belongs to the whole combination or merely to one of its products (perhaps the oxide of platinum).

The cyanuret of platinum which remains after the pyro-chemical decomposition of the colourless cyanuret of platinum and mercury, is a fine olive-yellow pulverulent substance, insoluble in water, acids, or alkalies, combustible, and when burnt in the air leaving 78 to 79 per cent. of pure platinum, and when heated with oxide of copper in the pyro-pneumatic apparatus giving carbonic acid and nitrogen gases nearly in the proportion of 2 to 1; it therefore consists of one atom of platinum and one of cyanogen = Pt Cy.

If the cyanuret of mercury and platinum is diffused in water and treated with hydrosulphuric acid, we obtain sulphuret of mercury and a colourless fluid with a strong acid reaction, which contains, in solution, a combination of cyanuretof platinum with hydrocyanic acid. If we evaporate this liquid, this new combination presents itself as a greenish yellow substance, with a metallic lustre on the surface, partly gold, partly copper colour; it deliquesces in damp air, and is very easy of solution in water and absolute alcohol, and combines with the alkalies so as to form double cyanurets.

If this combination, which on account of its acid properties I shall call (*Cyanplatinwasserstoffsäure*) a compound of hydrocyanic acid and cyanuret of platinum (H Cy + Pt Cy), be dissolved in absolute alcohol, and this solution left in the open air to evaporate on a watch-glass or plate of glass, the compound offers to the eye of the observer a peculiar crystallization, and at the same time an interesting but indescribable cameleon-like play of colours. If the dry acid be allowed to deliquesce in moist air and is then evaporated in dry air or in the sunshine, it crystallizes in exceedingly beautiful needles, grouped together like stars, which have a metallic lustre and are sometimes gold-, sometimes copper-coloured, like oxalate of platinum, but still more beautiful than the crystals of the latter. In the light and at a temperature of boiling water this compound undergoes no change, but when heated to above this point it is decomposed into hydrocyanic acid and cyanuret of platinum. If its solution in alcohol is mixed with some nitric acid we obtain a liquid which evaporates, and heated strongly on a glass plate, forms an exceedingly beautiful platinum mirror.

There may be formed also a similar compound with iridium (Hy Cy + Ir Cy), whose properties are perfectly analogous to the above compound with platinum.—*Poggendorff's Annals*, 1836, No. 3.

ON DECREPITATION.

M. Baudrimont observes that authors have generally attributed the phænomena of decrepitation to the presence and sudden vaporization of water, or to the sudden production of aëriform matter. It is remarkable, he observes, however, that the greater number of bodies which decrepitate are really anhydrous and fixed; such are sulphate of potash, sulphate of barytes, chloride of sodium, &c. In order to explain this anomaly it is supposed that these substances contain water interposed between their constituent parts. M. Baudrimont found, however, that after drying several fixed and anhydrous bodies in several ways, at a low temperature, as completely as possible, they still decrepitated when quickly heated; he also observed that the slaty clays mixed with coal decrepitated strongly when thrown into a hot furnace, and that those which presented most surface decrepitated the loudest. Slaty clay possesses a well-marked lamellar structure; and it was found that anhydrous bodies susceptible of decrepitating did not undergo it, except they were crystallized and possessed a smooth easy cleavage. This disposition to divide evenly on large surfaces offers a ready explanation of their decrepitation. In fact, substances which decrepitate, being such as are considered bad conductors of heat,—and their external parts are the first heated,—the dilatation which they suffer forces them to separate from the neighbouring parts which have not yet acquired the same temperature, and this is facilitated by their property of cleaving.

There are substances which may decompose and yield volatile products when heated, and in this case it is difficult to say whether it is to an unequal dilatation of their parts or to the repulsive action of these volatile products that the decrepitation is to be attributed. However, as they possess almost all a crystalline structure or an easy cleavage, this structure is probably often the only cause, for they decrepitate actually without having suffered the least apparent decomposition, as the cyanide of mercury and emetic tartar. Substances which do not possess a crystalline structure, may decrepitate when they have not been perfectly dried; such are plastic clays and argillaceous schist.

From what has been stated, decrepitating substances may be divided into two classes, viz. fixed bodies, and those which yield aëriform products. Among the first are sulphate of barytes, sulphate of strontian, sulphate of potash, chromate of potash, bichromate of potash, fluoride of calcium, chloride of sodium, chloride of potassium, bromide of sodium, bromide of potassium, iodide of potassium, and galena. Of the bodies which decompose and yield aëriform products at a high temperature, some are anhydrous, as nitrate of barytes, nitrate of lead, rhombic carbonate of lime, and cyanide of mercury. Others are hydrates, as tartar emetic, lamellar sulphate of lime, acetate of copper, bitartrate of potash, and ferrocyanide of potassium.

Those substances which contain a large quantity of water in a state of combination do not really decrepitate, unless they are susceptible of cleavage: such are the carbonate of soda, sulphate of soda, sulphate of magnesia, &c.

Cleavage therefore is as essential a condition of decrepitation as water or its elements.—*Journal de Pharmacie*, July 1836.

ATOMIC CONFUSION.—MONOHYDRATED SULPHOCARBETHERIC ACID.

The above title is suggested by the perusal of a paper in the *Journal de Chimie Médicale*, July 1836, entitled, "Sur un Mémoire de M. Couerbe sur la Chimie du Sulfure de Carbon." "M. Zeise, on examining the reaction of sulphuret of carbon and potash dissolved in alcohol, obtained a crystallized salt which appeared to be formed of potash and a hydracid equivalent to hydrogen, and a sulphuret of carbon which he regarded as very probably different, in the proportion of its elements, from the sulphuret of carbon of Lampadius. There was then, according to M. Zeise, a sulphuret of carbon, which acted the part of a complex supporter of combustion (*comburant complexe*), resembling cyanogen. In consequence of this analogy, M. Zeise called this sulphuret *xanthogen* (on account of the yellow colour of several of its compounds with metals). Its hydracid he named *hydroxanthic acid*, and its compounds with metals he called *xanthurets*. He considered the oily liquid derived from the reaction of sulphuric, hydrochloric, or acetic acid on his crystallized salt as *hydroxanthic acid,* and the precipitates derived from the mixture of metallic salts with the solution of hydroxanthate of potash as *xanthurets*. In this reaction, the hydrogen of the acid formed water with the oxygen of the metallic oxide, while the reduced metal combined with the xanthogen."

"M. Couerbe, examining the sulphuret of carbon at a time when the composition of æthers occupied much attention, considered it not as a *comburant* analogous to cyanogen, but as a sulphacid, represented by two atoms of sulphur and one atom of carbon, and thus corresponding to carbonic acid. He calls it *sulphocarbic acid*.

"The salt of M. Zeise is not represented according to M. Zeise, by sulphuret of carbon and hydrogen united to potash, but by two atoms of sulphuret of carbon and one atom of æther combined with an atom of potash.

"When this salt is decomposed by a dilute acid, it is not an hydracid which is obtained, but, according to M. Couerbe, an acid represented by two atoms of sulphuret of carbon and an atom of alcohol, which is thus analogous to sulphovinic acid, represented by two atoms of sulphuric acid and one atom of alcohol.

"M. Couerbe thinks then that, when sulphuret of carbon is added to potash and alcohol, an atom of potash unites to two atoms of sulphocarbic acid and an atom of æther, and that the elementary atoms of these two compounds constitute an acid, which he calls monohydrated sulphocarbetheric acid (*acide sulphocarbéthérique monohydrate*). He thinks that this acid, when it is separated from potash, absorbs an atom of water and becomes a bihydrated acid.

HYDROSULPHURIC AND HYDROSELENIC ÆTHERS.

M. Löwig states that oxalic æther answers well for the preparation of some hydracid æthers which have been hitherto unknown, such as hydrosulphuric and hydroselenic æthers; these are prepared by adding it to sulphuret and seleniuret of potassium: hydrocyanic and hydrosulphocyanic æthers are prepared in the same manner. The mutual decomposition always takes place with difficulty; and even with a great excess of the compound of potassium, there always distils a considerable quantity of oxalic æther undecomposed. Hydrosulphuric æther is obtained in the following manner: Sulphuret of potassium, prepared by decomposing the sulphate with charcoal, is reduced in a hot mortar into a fine powder, and put while yet warm into a retort, and then mixed with oxalic æther sufficient to make a thin paste. After the mixture has been exposed for some hours to a moderate heat,—the presence of water being carefully avoided, that sulphuretted hydrogen and alcohol may not be formed,—the distillation is to be commenced, and continued with an increasing heat until the oxalate of potash formed begins to decompose. The product of the distillation is the hydrosulphuric and oxalic æthers, and it is to be agitated for a considerable time with a concentrated solution of pure potash or sulphuret of barium, which decomposes the oxalic æther. The hydrosulphuric æther thus purified is decanted and rectified from chloride of calcium. Its purity is known by its not giving a precipitate with a solution of sulphuret of barium. Hydrosulphuric æther is lighter than water, and has an extremely disagreeable odour of asafœtida as well as an æthereal one; a single drop is sufficient to diffuse this smell over a great space. It has a sweet taste, which continues long; it has no action on test-papers. It is slightly soluble in water, but imparts its strong peculiar smell and its taste to it. With alcohol and æther it mixes in all proportions: it burns with a blue flame, and disengages sulphurous acid. It suffers no change by the contact of air. When boiled with a solution of potash it is not decomposed; but if distilled from finely powdered hydrate of potash, sulphuret of potassium and alcohol are obtained; but this decomposition takes place with difficulty, and a very large quantity of æther passes over without decomposition. Potassium decomposes it at a moderate heat; but the decomposition soon ceases, because the sulphuret of potassium produced forms a crust on the potassium and prevents a continuation of the action. There is no disengagement of hydrogen during this decomposition.

Hydrosulphuric æther does not in the slightest degree act upon peroxide of mercury, and is distinguished by this character from mercaptan, as will be foreseen. Nevertheless it precipitates some heavy metallic salts, especially an alcoholic solution of acetate of lead; the precipitate is of a yellow colour. Mixed with a concentrated alcoholic solution of acetate of lead, a white precipitate is formed which strongly resembles the mercapturet of potassium of Zeise. This æther may probably be prepared by distilling an intimate mixture of sulphuret of potassium and dry sulphovinate of barytes. The distilled liquor possesses all the properties of the æther described; water must be carefully avoided in this operation. M. Löwig intends to describe the analysis and properties of this æther.

Hydroselenic æther was obtained only in small quantity and but little is known of its properties; its odour is extremely disagreeable; it burns, depositing selenium and giving a smell of horse radish.

M. Löwig found that hydrocyanic æther prepared with oxalic æther, possessed the same properties as that obtained by M. Pelouze.

Journal de Pharmacie, July 1836.

Part II. of SCIENTIFIC MEMOIRS will be ready for publication on the 31st of October.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1836.

Chiswick.—August 1. Overcast: cloudy: clear and fine. 2—10. Fine. 11. Fine: cloudy and windy at night. 12. Overcast: hot and dry 13. Dry haze: fine: lightning and rain at night. 14. Heavy rain with thunder. 15, 16. Hazy: fine. 17, Very fine. 18. Overcast: fine. 19. Cold and dry: fine. 20. Hazy: rain. 21, 22. Very fine. 23. Hazy: rain. 24. Heavy rain: clear and cold at night. 25. Fine. 26. Drizzly: rain. 27. Fine. 28. Rain. 29—31. Very fine.

Boston.—August 1. Fine: rain P.M. 2, 3. Cloudy. 4. Cloudy: rain P.M. 5—7. Cloudy. 8, 9. Fine. 10—13. Cloudy. 14, 15. Fine. 16. Cloudy. 17. Fine. 18. Fine: rain P.M. 19. Cloudy. 20. Fine: rain P.M. 21. Fine. 22. Cloudy: rain P.M. 23. Cloudy. 24, 25. Fine. 26. Rain. 27, 28. Cloudy. 29. Fine: rain P.M. 30. Fine. 31. Cloudy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. Aug.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Self-registering. Max.	Max.	Min.								
M. 1.	30·181	30·178	30·026	29·40	60·4	54·3	69·7	71	48	62	SSW.	SW.	W.	...	...	·11	56
T. 2.	30·080	30·075	30·037	29·36	68·9	51·6	69·0	76	46	57	SW.	NW.	NW.	...	...	...	51
W. 3.	30·029	29·999	29·736	29·40	65·3	54·3	74·6	84	53	63	SE.	S.	NW.	...	...	...	57
☾ Th. 4.	29·859	29·897	29·823	29·20	66·3	57·0	72·2	79	56	63·5	SSW.	S.	calm	...	...	·09	60
F. 5.	30·000	30·022	29·972	29·40	61·6	58·7	70·5	71	55	57	N.	NE.	calm	...	...	...	60
S. 6.	30·119	30·161	30·093	29·57	63·6	57·2	67·5	70	47	61	N.	NE.	calm	...	...	...	59
⊙ 7.	30·245	30·212	30·150	29·65	59·2	51·9	68·9	70	48	61·5	N.	NE.	E.	...	...	...	57
M. 8.	30·202	30·169	30·160	29·61	62·0	54·5	70·5	72	44	59	N.	NE.	calm	...	...	...	57
T. 9.	30·241	30·224	30·184	29·64	60·8	54·9	69·2	71	42	61	N.	N.	calm	...	...	...	58
W. 10.	30·204	30·204	30·160	29·65	59·6	50·9	69·0	74	50	61	N.	E.	calm	...	...	...	57
Th. 11.	30·307	30·340	30·285	29·77	62·0	53·7	68·9	73	55	60	NE.	E.	calm	...	...	...	54
● F. 12.	30·348	30·334	30·272	29·80	59·5	56·4	69·0	74	49	61	ENE.	E.	calm	...	·44	...	58
S. 13.	30·225	30·204	30·027	29·70	65·4	54·5	72·0	77	49	57·5	NE.	E.	calm	·272	·15	...	62
⊙ 14.	29·930	29·921	29·847	29·30	62·2	59·5	71·9	76	53	66	N.	SW.	calm	...	...	...	62
M. 15.	29·960	30·075	29·932	29·32	62·5	59·0	72·5	77	52	65	WSW.	W.	E.	...	...	...	62
T. 16.	30·194	30·188	30·129	29·55	62·7	54·5	71·6	73	57	64	NNE.	S.	calm	...	...	...	59
W. 17.	30·111	30·098	30·077	29·40	66·4	57·2	73·4	75	50	66	WSW.	W.	W.	...	·02	...	63
Th. 18.	30·081	30·069	29·953	29·39	63·8	56·7	72·0	72	51	63	SW.	W.	W.	·044	...	·08	61
☽ F. 19.	30·196	30·178	30·152	29·55	59·2	54·5	67·0	70	43	56	SSW. var.	W.	NW.	...	·13	...	52
S. 20.	29·956	29·964	29·790	29·33	59·0	52·4	62·4	61	49	58·5	SSW.	SW.	NW.	·075	...	·28	58
⊙ 21.	29·954	29·955	29·909	29·34	58·5	51·9	66·8	70	47	56	NNE.	W.	NW.	·013	·06	...	52
M. 22.	29·794	29·787	29·633	29·14	59·0	53·3	67·4	68	55	57	SW. var.	SW.	W.	·027	·69	·19	58
T. 23.	29·641	29·748	29·602	29·05	60·2	57·2	63·9	64	49	54	N.	NE.	calm	·125	·02	...	57
W. 24.	29·976	30·159	30·002	29·45	55·4	49·5	61·5	67	40	58	NE.	NE.	E.	...	...	...	55
Th. 25.	30·168	30·171	30·049	29·58	58·2	48·4	65·3	70	52	56	E.	SE.	calm	...	·14	·33	53
○ F. 26.	29·984	30·012	29·973	29·26	59·0	55·6	65·5	70	46	56	SW.	S.	calm	·088	·12	...	58
S. 27.	29·936	29·954	29·926	29·29	62·0	52·7	70·2	73	55	59	WSW.	SW.	W.	·161	·20	...	57
⊙ 28.	29·964	30·011	29·962	29·37	59·2	56·4	65·7	71	43	56	E.	S.	calm	·188	...	...	59
M. 29.	30·148	30·124	30·110	29·50	58·5	51·0	67·7	69	45	57	WNW.	W.	W.	...	...	·14	54
T. 30.	30·212	30·186	30·135	29·52	61·0	52·5	68·0	73	53	56	SSW.	SW.	calm	...	...	...	55
W. 31.	30·113	30·076	29·956	29·44	63·5	56·0	70·9	76	45	61·5	SSW.	S.	W.	..	...	...	60
	30·076	30·340	29·602	29·44	61·5	54·5	68·9	84	40	59·6				Sum 0·993	1·97	1·22	57·5

THE

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[THIRD SERIES.]

NOVEMBER 1836.

LXIII. *On a volatile Liquid procured from Caoutchouc by destructive Distillation: with Remarks on some other empyreumatic Substances.* By William Gregory, *M.D., F.R.S.E., Lecturer on Chemistry.**

SOME years ago a patent was taken out by Mr. Enderby, of London, for the production of a volatile inflammable liquid by the distillation of caoutchouc. This liquid possesses very remarkable properties. As prepared by Mr. Enderby, it is colourless, very fluid, has no taste, but a peculiar æthereal smell. Its specific gravity is very low, being = 0·680, and it boils at a temperature below 100° Fahrenheit. As no examination of this remarkable substance has yet been published, I venture to offer the results of some experiments which I have made on it at intervals during the last two years.

As Mr. Enderby purifies his oil by rectification alone, my first object was to push that process as far as possible, so as to get rid of the less volatile matters which might be present. By successive rectifications, carried on without ebullition at about 80° to 90° Fahr., I at last obtained a liquid having the specific gravity of 0·666 at 60° Fahr., approaching very nearly to that of the eupion of Dr. Reichenbach, obtained by the distillation of oil, viz. 0·655. The new liquid, however, is not eupion, for it boils at about 90° Fahr., and is instantly

* Communicated by the Author.

decomposed by oil of vitriol; while eupion boils at 110° Fahr., and resists the action of that acid.

The boiling-point of the new oil is not constant. That of sp. gr. 0·670 begins to boil at 95° Fahr., but the temperature soon rises, and towards the end of the distillation reaches 170° Fahr. Consequently, we cannot consider it as an unmixed compound.

Having taken some of this liquid with me to Giessen last year, I submitted it to analysis under the eye of Professor Liebig. Although I did not expect any very satisfactory result from the analysis of a substance confessedly not free from mixture, I was rather surprised to obtain results indicating a composition, in 100 parts, identical with that of olefiant gas.

I next proceeded to examine the action of sulphuric acid on the new liquid. When the acid is added to it in large quantity, part of the oil is decomposed instantaneously, while the rest is dissipated by the heat evolved, leaving a black semifluid mass. But if the acid be added gradually to the oil at the bottom of a long tube, which is closed by the thumb and cooled down after each addition, a colourless liquid is obtained, swimming on the surface of the black mass above mentioned. When the addition of new acid causes no further development of heat, the liquid may be decanted, washed with solution of potash, and rectified over chloride of calcium.

This liquid has properties very distinct from those of the oil which has yielded it. It has acquired an aromatic smell, resembling that of turpentine, and it now boils at about 440° Fahr. Notwithstanding, however, this remarkable change, the analysis of the modified oil gave results closely coinciding with those of the original liquid, viz. a composition identical, in 100 parts, with that of olefiant gas.

These analyses were made in September 1835. In the *Annalen der Pharmacie*, October 1835, Professor Liebig has directed the attention of chemists to the remarkable alteration produced by sulphuric acid on the oil of caoutchouc; and has conjectured that the change consisted in the conversion of that oil into eupion, which resists sulphuric acid. If so, he proceeds, then it is possible that eupion may be only produced from the oil of tar by the action of the sulphuric acid employed in its preparation.

I cannot acquiesce in this view. The eupion of Reichenbach boils at 110° Fahr., and the oil alluded to at 440° Fahr.; moreover their odour is quite different: and if we are to admit them to be essentially the same, one of them must be in the highest degree impure, implying the presence of some other substance.

I must, however, mention here a singular fact. Having myself subjected caoutchouc to destructive distillation, I obtained none of Mr. Enderby's oil; but on adding sulphuric acid to the more volatile products, I got what I conceived to be impure eupion. My failure in procuring the new oil I ascribe to a difference in the temperature employed.

Professor Liebig is inclined to the opinion that the other substances described by Reichenbach are products of reaction, and not educts. But I venture to remind him that creosote, for example, may by detected in tar, by its smell and by its antiseptic virtue; that, according to Reichenbach, both eupion and paraffine may be sufficiently purified, by rectification alone, to exhibit their characteristic properties; and that paraffine may be extracted from the petroleum of Rangoon, in a state of absolute purity, without the employment of any more active solvent than sulphuric æther.

M. Hess, in a note read in the Imperial Academy of Sciences of St. Petersburg, on the 11th of March* 1836, mentions some circumstances which are highly interesting in reference to the present subject. After referring to the analogy of the oil of petroleum with the eupion of Reichenbach, (an analogy which I had demonstrated in a paper read to the Royal Society of Edinburgh in December 1834, and since published in their Transactions,) he states that in following Reichenbach's process for the preparation of eupion from oil he obtained a liquid of sp. gr. 0·71, which by the action of potash he obtained at last so light as 0·648, and boiling from 68° Fahr. to 110° Fahr.

This liquid he found to have the composition of olefiant gas; it contained, he says, but very little eupion, which might be separated by sulphuric acid.

Now my experiments above described render it extremely probable that M. Hess's liquid is identical with that of Mr. Enderby; and that the oil separated from it by sulphuric acid is not eupion, but the second oil analysed by me.

As M. Hess had previously shown that the pure oil of petroleum had the same composition as olefiant gas, and was in its properties, as I had also proved, very analogous to eupion, he thinks it highly probable that the composition of eupion is the same.

He then proceeds to divide the very numerous compounds, which agree in containing, like olefiant gas, about 85·7 per cent. of carbon, and 14·3 per cent. of hydrogen, into two series. The one, of which paraffine, eupion, and olefiant gas

* *Annales de Chimie*, vol. lxi. p. 331.

are examples, he calls passive, because they do not act on sulphuric acid; the other, of which Faraday's quadro-carburetted hydrogen and the oil obtained by himself, as well as Mr. Enderby's oil (if the two latter be not identical), are specimens, he calls active, as they act strongly on the same acid. I have mentioned his views here, in order to direct attention to the striking fact, established by my experiments, that Mr. Enderby's oil, belonging to one of these classes, is, partially at least, converted by the action of sulphuric acid into a liquid belonging to the other series, and retaining the same composition.

Another circumstance well worthy of notice is the fact that several of the liquids in question yield uniform results as to composition, even when the portions analysed differ in density and volatility. This has been shown to occur likewise in the oils of turpentine and lemons, and seems to indicate the existence of an almost unlimited number of polymeric combinations of carbon and hydrogen.

When Mr. Enderby's liquid was first made known it was stated to be an excellent solvent for caoutchouc, which, from its great volatility, would have rendered it extremely valuable. But it is necessary to state, that I have not yet seen one specimen of it which, under any circumstances, in my hands, possessed this property. Two gentlemen informed me that they had succeeded in dissolving caoutchouc by means of it; but when asked to repeat the experiment with the same liquid they both failed. If therefore this liquid be a solvent for caoutchouc, it must be under conditions with which I am not acquainted.

LXIV. *Experiments on Gaseous Interference.* *By* William Charles Henry, *M.D., F.R.S.**

THE singular power exerted by certain gases, of suspending the action of finely divided platina on mixtures of oxygen and hydrogen, was first observed and announced by Dr. Turner †, shortly after Döbereiner's discovery. It was also about the same period noticed by Dr. Henry ‡ in the course of his researches on the application of platina to the analysis of complex gaseous mixtures. More recently Dr. Faraday § has described similar interferences, affecting the

* Read before the Chemical Section of the British Association at Bristol: and communicated by the Author.

† Jameson's Journal, vol. xi. pp. 99 and 311.

‡ Phil. Trans., 1824, p. 266.; [or Phil. Mag., First Series, vol. lxv. p. 272.—Edit.]

§ *Ibid.*, 1834, p. 71.

activity of prepared plates of the same metal. Various modes have been proposed of interpreting these phænomena. Dr. Turner (Chemistry, 5th edit., p. 647) observes, "One would be tempted to suppose that these gases act by soiling the metallic surface, though in some respects this explanation is not satisfactory." In the Essay by Dr. Henry already referred to (p. 282), it is suggested that this property "is most remarkable in those gases which possess the strongest attraction for oxygen, and it is probably to the degree of this attraction, rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect." Finally, the latest writer on this subject, Dr. Faraday, concludes (par. 655.): "Whether the effect produced by such small quantities of certain gases depends upon any direct action which they may exert upon the particles of oxygen and hydrogen, by which the latter are rendered less inclined to combine, or whether it depends upon their modifying the action of the plate temporarily (for they produce no real change on it) by investing it through the agency of a stronger attraction than that of the hydrogen, or otherwise, remains to be decided by more extended experiments." It was in the hope of illustrating the nature of these interesting phænomena in gaseous chemistry that the following experiments were instituted.

The carbonic oxide and olefiant gases are stated by all the above-mentioned inquirers to possess, in the most remarkable degree, the property of interference. I have selected those gases as fitter subjects for experiment than others which manifest the same property, because, as Dr. Faraday testifies, they prevent combination, without at all injuring or affecting the power of the platina (644. and 6.), and hence exhibit the phænomenon in its simplest form. The platina which I employed was in the various conditions of (1.) plates prepared according to Dr. Faraday's method; (605.) (2.) sponge from calcination of the ammonia-muriate, either alone or moulded with China clay into small balls; and (3.) the black powder precipitated by the addition of alcohol to the solution of protochloride of platina in potassa, according to Liebig's process.

1. *Carbonic Oxide.*—To a mixture of oxygen and hydrogen, in the proportions required to constitute water, carbonic oxide was added, so as to form $\frac{1}{12}$th, $\frac{1}{19}$th, and $\frac{1}{30}$th of the entire volume. The prepared plate when admitted caused no appreciable diminution, nor was any change observed during three or four hours in any of the three mixtures. The following morning evident but slight diminution was visible in all.

Since the gaseous products admit of exact analysis only over mercury, a fluid through which the plate cannot be passed without destroying the purity of surface essential to its activity, I was induced to employ balls of platina and China clay in the greater number of instances. Such balls, introduced over mercury into mixtures containing from $\frac{1}{21}$th to $\frac{1}{30}$th of carbonic oxide, produced no instantaneous action. But in the course of five minutes there was generally a visible diminution of volume, and in the space of two hours the action was complete. When the carbonic oxide formed a larger element of the mixture, as from $\frac{1}{8}$th to $\frac{1}{3}$rd, the action was still longer delayed. With one third there was scarcely any visible decrease of volume during the first hour; and even on the following day there remained much gaseous matter unconsumed. Thus the residue of a mixture of ·35 cu. in. of carbonic oxide with ·70 of hydrogen and oxygen in combining proportions (together 1·05), amounted after twenty-four hours' contact with the ball to ·82. The proportions were extensively varied between $\frac{1}{2}$ and $\frac{1}{30}$th as limits; and between these it was observed, that combination to a greater or less amount was in all cases induced by a sufficient duration of contact. Carbonic oxide, it appears then, does not *prevent* but only *retards* gaseous union.

To ascertain the proportions in which the oxygen had been shared by the gases opposed to each other, the residue was washed with limewater or with caustic potassa. In all cases a notable diminution, due to the absorption of carbonic acid, was observed, and from the limewater carbonate of lime was precipitated. The quantity of carbonic acid so formed varied with the varying proportion which the carbonic oxide had borne to the hydrogen of the explosive mixture. When the hydrogen and carbonic oxide were present in equal volumes, and the oxygen was sufficient to saturate one of them only, I found that the carbonic oxide had appropriated to itself, in one experiment eight times, in another as much as ten times, the volume of oxygen that the hydrogen had taken. When the carbonic oxide was inferior in volume to the hydrogen, less oxygen was expended in the production of carbonic acid, but still considerably more in proportion than had combined with the hydrogen. It is then a general fact, that carbonic acid is generated in all cases in which carbonic oxide suspends the action of platina on mixtures of oxygen and hydrogen.

The next object of inquiry was to ascertain the action of platina in its various forms on mixtures of carbonic oxide with oxygen only. Dr. Faraday had noticed that at common temperatures "a mixture of two volumes carbonic oxide and one volume oxygen was unaffected by the prepared platina plate in several days." (574.) I found however that after four days' contact with the plate, an appreciable diminution had ensued. Thus 3 cu. in. of such a mixture were reduced to $2\frac{3}{4}$, indicating the formation of nearly $\frac{1}{2}$ cu. in. of carbonic acid. But the union of carbonic oxide with oxygen was much accelerated by causing the gases to stand in contact with the platina over a solution of caustic potassa, instead of distilled water, which had been used by Dr. Faraday. In such experiments there was a daily decrease of volume in the confined gases, till the fluid by ascending covered the greater part of the plate. Thus $3\frac{1}{4}$ cu. in. were reduced to 1 cu. in. in about seven days.

Platina in the form of sponge had been previously stated to cause the slow union of carbonic oxide and oxygen*. This action I found was also quickened by admitting potassa fusa with or without water above the confining surface of mercury. Thus 1·40 cu. in. of a mixture of carbonic oxide and oxygen in equivalent proportions became 1·25 in the first five minutes after introducing the platina sponge and potassa; in the next five minutes 1·15, and in half an hour much less than 1 cu. in. remained. In two hours less than half a cubic inch was left, and the potassa, by rising so as to moisten the sponge, terminated the experiment. Finally, the black powder of Liebig admitted into mixtures of carbonic oxide and oxygen became incandescent at the moment of contact, and continued to glow until all the carbonic oxide was converted into carbonic acid. Platina therefore, in all its forms, determines, at atmospheric temperatures, the union of carbonic oxide with oxygen. In the state of plate the change goes on with extreme slowness; in that of sponge, much more quickly; and in the most minutely divided condition of powder, with ignition and with great rapidity.

In this unequivocal action of platina upon mixtures of oxygen with carbonic oxide only, it appears to me, is to be found the interpretation of the property residing in the latter gas, of suspending the combining tendencies of hydrogen and oxygen in mixtures of the three gases. Possessing, as has been already shown, a stronger affinity for oxygen than is inherent in hydrogen, carbonic oxide seizes, when mingled with those two gases and exposed to the agency of platina, a much larger proportion of the oxygen than is due to its comparative volume. In such complex mixtures, just as in simple mixtures of carbonic oxide and oxygen, the slow formation of carbonic

* Phil. Trans., 1824, p. 267. [Phil. Mag., vol. lxv. p. 269.—Edit.]

acid continues to be the predominant action. Hence the phænomenon of "interference" is better explained by contemplating the gaseous mixture as one of carbonic oxide and oxygen to which hydrogen has been added, than as one of hydrogen and oxygen with which carbonic oxide has been mingled. Such a simple mixture of carbonic oxide and oxygen, in presence of the prepared plate or sponge, is in a state of slow acidification; and the admission of hydrogen, a gas endowed with a feebler affinity for oxygen, occasions no essential change of the chemical actions previously in operation.

This view of the mode in which carbonic oxide overrules the combining tendencies of hydrogen and oxygen is supported by experiments, long ago recorded, on the influence of increased temperature. For it has been shown* that the phænomena of interference disappear at a heat between 300° and 340° Fahrenheit, the union of the elements of a gaseous mixture which is slowly acted upon at common temperatures being then rapidly accomplished. Now this is precisely the temperature at which carbonic oxide, when heated simply with its equivalent of oxygen in presence of the platina sponge, is rapidly converted into carbonic acid. Instant action, with incandescence, I have found also follows the admission of the black powder into mixtures of hydrogen, oxygen and carbonic oxide; and it has been already stated that simple mixtures of carbonic oxide and oxygen (or carbonic oxide alone, the oxygen being supplied by the powder,) are inflamed by platina in the state of powder. The phænomena of interference are observed then only at those temperatures, and with that form of platina, which induce the *slow* union of carbonic oxide and oxygen; and wholly disappear at the higher temperature, or with that more active state of the metal which causes *rapid* combination.

An obvious objection, however, presents itself to the view that carbonic oxide possesses a stronger affinity for oxygen than hydrogen exerts, viz. that while hydrogen and oxygen are speedily detonated at common temperatures by the plate or sponge, the union of carbonic oxide and oxygen takes place with great slowness. The explanation of this apparent anomaly I believe to be, that the product of the combustion of hydrogen (aqueous vapour) at once quits the surface of the metal, and is liquefied by the cold sides of the tube; while the combustion of the carbonic oxide yields a gas which remains for a while adherent to the metallic surface next to which it is generated, and thereby prevents a sufficiently rapid

* Dr. Henry, Phil. Trans. 1824, pp. 278 and 280.

access of fresh unaltered gas to elevate materially the temperature of the platina. In confirmation of this view I found that caustic potassa, by absorbing the carbonic acid as fast as it is formed, accelerates the acidification of carbonic oxide. When the metallic superficies is so extensive (for instance, in the powder of Liebig,) that a high temperature is quickly attained by the metal in contact with the first portions of gas that are consumed, it has been already shown that carbonic oxide, like hydrogen, then unites with oxygen, with incandescence. Finally, it is well known that even mixtures of hydrogen and oxygen do not detonate on first admitting the prepared plate. During the first minute the union is generally very slow, and it only becomes explosive when the temperature of the plate has been raised by its action upon the gaseous mixture.

2. *Olefiant Gas.*—The olefiant gas which I employed had been carefully washed with caustic potassa, and in the experiments made over mercury, it had stood for many days in contact with dry potassa fusa. It resulted from all my experiments with platina in its various conditions, that the property of interference is much less energetic in olefiant gas than in carbonic oxide*. Of this I could not feel satisfied without repeated examination, because the reverse is the result of Dr. Faraday's experience, according to which olefiant gas interferes when it constitutes $\frac{1}{18}$th of the entire mixture, and carbonic oxide only when it amounts to $\frac{1}{8}$th. I found, however, that in a mixture of 3·00 cu. in. explosive mixture with ·08 of olefiant, in which the olefiant is about $\frac{1}{38}$th of the whole, the plate began to act visibly on first admission. In ten minutes there remained 2·50, and in a quarter of an hour 2·00, when the action became very rapid, the plate being so hot as to cause the ascending water, in contact with it, to boil, and only ·25 cu. in. were left unconsumed. Even when the olefiant amounted to $\frac{1}{19}$th of the entire mixture there was manifest action in the course of a quarter of an hour, and in two days the water had covered the plate.

Olefiant gas in much larger proportion, even when constituting $\frac{1}{4}$th or $\frac{1}{3}$rd of the mixture, did not in the slightest degree retard the action of the platina balls or sponge. Several trials were made, in which the olefiant and explosive mixture were mingled in equal proportions. In all of these the ball acted instantaneously, and ascended rapidly into the tube during one or two minutes, when its rise was suddenly checked. Thus 1·02 cu. in. were reduced in the first minute to ·90, and became ·82 after an hour's contact. The following day only ·56

* See also Mr. Graham's Experiments, Journal of Science, 1829, p. 356.

were left, which were not diminished by washing with potassa. Even when the volume of the olefiant was double that of the explosive mixture, there was instant action, though to a less extent than in the last experiment.

When the volume of the olefiant was three times that of the explosive mixture, no immediate action was visible, though a notable diminution always took place in the course of a few hours or on the following day. Finally, the activity of the black powder of Liebig was not suspended by the addition of twenty volumes of olefiant to one of explosive mixture.

On subjecting to examination the gaseous products of these and other experiments on olefiant gas, mingled with various proportions of the explosive mixture, very different results from those afforded with carbonic oxide were obtained. In the greater number of experiments with olefiant gas, no appreciable diminution was caused by prolonged contact, or by subsequent washing with potassa; and though in some cases, when the explosive mixture considerably exceeded the olefiant, there *was* a perceptible absorption, yet the carbonic acid thus evidenced was always of small amount. In three successive experiments, in which olefiant gas and explosive mixture were mingled in equal proportions, there was no measurable product of carbonic acid. Yet in all these cases, though the ball ascended rapidly on first contact, its activity was soon suspended. Olefiant gas then possesses, like carbonic oxide, an undoubted power of retarding the union of hydrogen and oxygen, but it differs from carbonic oxide in not necessarily affording carbonic acid by so acting.

Similar differences presented themselves on submitting mixtures of olefiant and oxygen only to the action of platina. To the observation of Dr. Faraday, "that the most prolonged contact with the prepared plates never induced the union of the elements of olefiant with oxygen," I may add, that the contact was not efficient, even when aided by the presence of liquid potassa. The spongy metal, similarly conjoined with potassa, was also for the most part entirely inert, and in the few instances in which carbonic acid appeared, it was formed very slowly, and in small quantity. At the temperature 480° Fahrenheit, however, the sponge has been shown to occasion the speedy though inexplosive combustion of olefiant in mixture with oxygen. Liebig's powder too caused the slow combination of the two gases at atmospheric temperatures, as was evidenced both by considerable diminution of volume, and by the test of lime-water; and when the tube containing the gaseous mixture and powder was surrounded with boiling water combination ensued with rapidity. Now the mode in which platina acts is of course identical in the various states of plate, sponge, or powder; the powder presenting only an infinitely larger extent of surface for gaseous contact than the plate or sponge. Hence the tendencies of the constituents of olefiant to unite with oxygen, which are evidenced by combination to considerable amount in presence of the powder, may be inferred to be operative, though less effectively, on the surface of the same metal under other forms; and viewed in connexion with the unequivocal proofs of the nature of the interference of carbonic oxide, these tendencies may be admitted to furnish an adequate explanation of the more feeble interference of olefiant gas*.

The influence of temperature in quickening the action of the sponge or powder on mixtures of olefiant and oxygen strongly confirms this view, when taken in connexion with a peculiarity observed in the mode of interference of olefiant gas. Thus it has been stated that olefiant mingled in equal bulk with explosive mixture did not prevent instant action; and that it began to interfere only when much of the oxygen and hydrogen had combined together, with the disengagement of great heat. The interfering power of olefiant gas, feeble at atmospheric temperatures, is then greatly augmented by heat, which has been already shown to determine the separate combination of the elements of olefiant with oxygen, and which is well known to exalt chemical affinity.

In recapitulation, it may be stated that carbonic oxide interferes with the action of platina upon mixtures of oxygen and hydrogen, by virtue of its stronger affinity for oxygen, which causes it slowly to take the larger portion of that gas. Olefiant gas, which at common temperatures has a weaker attraction for oxygen than hydrogen, suspends the combining tendencies of those two gases, only when its volume greatly exceeds that of the mixture, in which case the weaker affinity is aided by a greater number of atoms. Even with this advantage olefiant is unable to appropriate the oxygen to itself, but only retards its union with hydrogen by opposing a weaker attractive tendency.

Nor is the admission of attractive forces between the particles of mixed gases, even when not manifested by any visible action (as between oxygen and the elements of olefiant gas), inconsistent with what is known of chemical affinity as operating in the solid and liquid forms of matter. In the eighth of his admirable series of memoirs on electro-chemistry, Dr. Faraday has shown that zinc, having its surface thinly amal-

* The interfering power of carbonic oxide as respects the action of the platina balls is eighteen times as great as that of olefiant; carbonic oxide in the proportion of $\frac{1}{24}$th interfering as completely as olefiant in that of $\frac{3}{4}$ths of the mixture.

gamated, though incapable, when immersed in dilute sulphuric acid, of effecting the decomposition of water, has yet the power, by its attraction for the oxygen of the particles of water in contact with its surface, to induce a peculiar state of electrical tension or polarity in those particles of water, as well as a similar but opposite state in the contiguous particles of zinc. By plunging a platina plate into the solution and completing the galvanic circle, this tension is relieved and decomposition of water instantly ensues. In the preparatory stage of this important experiment we have then a case of chemical forces in undoubted operation, yet giving no appreciable sign of their existence.

Other arguments present themselves in favour of the opinion, that the property inherent in certain gases of preventing or retarding the union of hydrogen and oxygen is to be referred to their attraction for oxygen, and not to any peculiar action of the metallic surface, by which it becomes invested with the interfering gas. (1.) All the gases which have been hitherto observed to exhibit this power, are such as are capable of uniting with oxygen; and the non-interfering gases are such as cannot, at least within a considerable range of temperature, be brought to combine with that element. (2.) The property of interference follows, in its comparative energy, the same order as the respective combustibilities of the gases. Thus Sir Humphry Davy observed that carbonic oxide and olefiant were most inflammable, while carburetted hydrogen required for its combustion a much higher temperature, being neither fired by white-hot charcoal nor iron. Dr. Henry also has shown that in presence of the sponge, carbonic oxide combines rapidly with oxygen at from 300° to 340° Fahrenheit; olefiant at 520°; and carburetted hydrogen not at any temperature to which the mercurial bath could be raised. I have ascertained that the two first gases observe the same order of union in presence of Liebig's powder, carbonic oxide inflaming at atmospheric temperatures, and olefiant gas being rapidly acted upon at 212°. Now this progression is precisely that of their interfering powers, carbonic oxide acting when it constitutes only $\frac{1}{24}$th of the mixture, olefiant gas not suspending action till it amounts to $\frac{3}{4}$ths, and carburetted hydrogen possessing no power whatever of retarding the action of platina even when its volume exceeds by ten times that of the explosive mixture. Finally, that the restraining force is wholly dependent upon the relations of the mixed gases among one another, is deducible from the fact that the same gases which suspend the combining tendencies of hydrogen and oxygen in presence of platina, resist also other modes of effectuating the union of those gases;

for instance, the discharge of a Leyden jar. The curious facts observed by Professor Graham, respecting the power of even smaller quantities of the same and other gases to arrest the slow oxidation of phosphorus, are also favourable to the doctrine that the interfering gases act solely by virtue of their superior attractions for oxygen.

LXV. *Simple Method of proving the Law of Gravitation. By* J. R. Young, *Esq., Professor of Mathematics in Belfast College.**

THE following is a concise mode of establishing the law of gravitation, with the aid of only the most obvious dynamical principles. Its novelty, and remarkable simplicity, may perhaps entitle it to a place in the Philosophical Magazine.

Let R be the radius of curvature at any point of the planetary orbit, r the distance of the planet from the sun, p the semiparameter, and P the perpendicular from the centre of force upon the tangent through the extremity of r.

By a known property of the conic sections, if from the foot of the normal, N, a perpendicular be drawn to the radius vector, the part intercepted between this perpendicular and the curve will be equal to the semiparameter. Hence by similar triangles we shall have

$$\frac{r}{\mathrm{P}} = \frac{\mathrm{N}}{p}.$$

Now from the usual expression for the radius of curvature we have

$$\mathrm{R} = \frac{\mathrm{N}^3}{p^2} = \mathrm{N}\frac{r^2}{\mathrm{P}^2}.$$

Moreover, by the resolution of forces,

$$\frac{\mathrm{N}}{p}\left(=\frac{r}{\mathrm{P}}\right) = \frac{\text{force of gravitation}}{\text{normal force}} \quad \ldots\ldots\ldots \quad (1.)$$

But from the well-known theorem of Huygens, the normal force is expressed by $\frac{v^2}{\mathrm{R}}$; and it is an obvious deduction from the first law of Kepler that the velocity varies inversely as the perpendicular upon the tangent. (*Principia*, sect. 2. prop. 1. cor. 1.) Hence

$$\text{Normal force} = \frac{v^2}{\mathrm{R}} = \frac{\mathrm{C}^2}{\mathrm{P}^2\mathrm{R}} = \frac{\mathrm{C}^2}{\mathrm{N}r^2} \quad \ldots\ldots\ldots\ldots\ldots \quad (2.)$$

Consequently (1.)

* Communicated by the Author.

$$\text{Gravitation} = \frac{C^2}{p\,r^2},$$

that is, the attractive force varies inversely as the square of the distance.

By substituting for p its value $a\ (1-e^2)$ we get the expression usually given in books on physical astronomy.

It may be proper to remark that the foregoing proof differs but little in principle from one lately given by M. Transon, in the *Journal de Mathématiques*; but as that is made to depend upon a property involved in the theory of caustics—a subject quite foreign to the inquiry—it is presumed that the process here given will be preferred on the score of elementary simplicity.

Belfast, Sept. 26, 1836.

LXVI. *Explanation of a remarkable Paradox in the Calculus of Functions, noticed by* Mr. Babbage. *By* John T. Graves, *Esq., M.A., of the Inner Temple.**

THE following passage occurs in a very valuable paper by Mr. Babbage, "On the Analogy between the Calculus of Functions, and other branches of Analysis," published in the Philosophical Transactions for 1817. (vol. cvii. pp. 211, 212.)

"Here perhaps it may not be misplaced to state a difficulty of a peculiar nature with respect to functional equations which are impossible; for the sake of perspicuity I shall consider a very simple case,

$$\psi x = c\,\psi\frac{1}{x} \qquad (1.)$$

for x substitute $\frac{1}{x}$, $\psi\frac{1}{x} = c\,\psi x \qquad (2.)$

and by multiplication, $\psi\, x \times \psi\frac{1}{x} = c^2\,\psi x \times \psi\,\frac{1}{x}$, or $1 = c^2$, from which it follows that $c = \pm 1$, or, in other words that the equation $\psi x = c\,\psi\,\frac{1}{x}$ is contradictory, unless $c = \pm 1$. Now the functional equation $F\,\{x, \psi x, \psi\,\alpha\,x\} = 0$ has been reduced by Laplace by means of a very elegant artifice to an equation of finite differences; nor am I aware that this profound analyst has pointed out any restriction or any impossible case. If we treat the equation $\psi x = c\,\psi\,x^n$ by his method, we shall find for its solution

$$\psi x = c^{\frac{-\log\log x}{\log n}} \qquad (3.)$$

* Communicated by the Author.

and this solution satisfies the equation $\psi x = c \psi x^n$ independently of any particular value of n, and if we suppose $n = -1$,

we have $$\psi x = c^{\frac{-\log 2 x}{\log(-1)}} \qquad (4.)$$

for the solution of the equation $\psi x = c \psi \frac{1}{x}$, whatever may be the value of c, and we have before shown that it cannot have a solution unless $c = \pm 1$. The only explanation I am at present able to offer concerning this contradiction is one which I hinted at on a former occasion, viz. that if we suppose ψ to represent any inverse operation which admits of several values, then if throughout the whole equation we always take the same root or the same individual value of ψ, it is impossible to satisfy the equation, but if we take one value of ψ in one part, and another of the values of ψ in other parts of the equation, it is possible to fulfill it by such means. This solution may, perhaps, appear unsatisfactory; it is however only proposed as one that deserves examination, and I shall be happy if its insufficiency shall induce any other person to explain more clearly a very difficult subject."

This passage is referred to by Professor De Morgan in his article on the Calculus of Functions, §. 72, in the *Encyclopædia Metropolitana*, and I generally agree with him in the cause which he suggests for the explanation of the difficulty, viz. a *discontinuity* in the form of ψ, by which I mean that for different sets of continuous values of x, ψ has partial non-concurrent solutions of different forms. What I now propose is to point out specifically the existence of that cause in the instance put forward by Mr. Babbage, and the precise *rationale* of its application in explaining the difficulty in question. The accomplishment of these objects, which do not seem to have been hitherto satisfactorily effected, would not only be interesting for its own sake, but would, in many analogous cases be conducive to clearness of reasoning, and tend to restore that confidence in the result of mathematical principles which is impaired when processes apparently legitimate lead to contradictory conclusions. If, according to the explanation thrown out by Mr. Babbage, (as I understand it) ψx had *many values for a given* x, and we were allowed to shift its different values among the equations (1.) and (2.), substituting one of those values for the symbol ψx in one part of either of those equations, and another in another, it is easy to see that we might get rid of the difficulty of supposing $c^2 = 1$, and might even, if we had a sufficiently indeterminate ψx, allow to c an infinite number of values. By this plan, for instance, if ψx had two values, v and v', $\frac{\psi x}{\psi x}$ might mean $\frac{v}{v'}$ and $\frac{v'}{v}$ as well as 1. But I think that, in the first place, such a shifting is not a legitimate proceeding, for the different values of a generic function of x are themselves different individual functions of x, while the equations in question suppose a comparison of the same functions, and are couched in an algebra applying to individual values; and, in the second place, even such an explanation would not succeed in the case before us in wholly removing the real difficulty, for, even by the utmost shifting, we cannot suppose c an *entirely arbitrary* constant, unless for any given x, $\psi\, x$ itself be wholly arbitrary, which $c^{\frac{-\log\log x}{\log(-1)}}$ assuredly is not. The true solution resembles while it differs from that thrown out by Mr. Babbage, unless (as may possibly be the case) the vague terms he uses are intended to adumbrate the very solution which I contend to be correct. The paradox is occasioned not because $\psi\, x$ has different values for one value of x, but because ψ has different forms for different values of x. Is this so, and, if so, how does it obviate the difficulty? These are the inquiries which it is the design of this paper to answer. At the very threshold of our researches a question that must present itself is, What do we mean when we put $\psi\, x = c^{\frac{-\log^2 x}{\log(-1)}}$, and assert that $\psi\, x = c\,\psi\,\frac{1}{x}$? Do we mean to assert that the expressions $c^{\frac{-\log^2 x}{\log(-1)}}$ and $c.c^{\frac{-\log^2 \frac{1}{x}}{\log(-1)}}$ are equivalent generic forms, (like $a^\theta\, b^\theta$ and $(a\,b)^\theta$) as having each the same infinite number of values for any given x, when we let in all the *acknowledged* indefiniteness of the notation $\log\theta$ and a^θ? As I presume that such an assertion of general equivalence is not intended, and as it is not necessary for my future argument to deny it now, I shall not stop to prove its incorrectness, but this, *at least*, I think we must be supposed to mean, viz. that, for any given x within certain limits, the form $c^{\frac{-\log^2 x}{\log(-1)}}$ includes some values or some fixed single value which respectively are or is equal to other severally corresponding values or another corresponding individual value included in the form $c.c^{\frac{-\log^2 \frac{1}{x}}{\log(-1)}}$. I proceed, therefore, to point out a case of $\psi\, x$ *included in the form* $c^{-\frac{\log^2 x}{\log(-1)}}$, which satisfies the equation $\psi x = c\,\psi\,\frac{1}{x}$; but I hope to show that "it satisfies that equation only for such values of x as to exclude one of the assumptions we must make if we would prove $c^2 = 1$," or that, "if it satisfies equation (1.), x must be classed with one set of values, and $\frac{1}{x}$ with another different set; and further, that if any of the latter set be substituted for x in the particular case of ψx included in $c^{\frac{-\log^2 x}{\log(-1)}}$ which before satisfied (1.), that equation will no longer hold good, so that (2.), which is obtained by substituting $\frac{1}{x}$ for x in (1.), cannot subsist simultaneously with (1.) for the assigned form of ψx." The instance I shall select will be given as being the simplest illustration I can present of the application of this principle, and as being, morever, independent of any hitherto *disputed* peculiarities in my results concerning logarithms;—not that I at all recede from those results, but that I wish to approve my present results to those who are not yet convinced of the former. The same principle may be proved to apply if we select any other individual case of ψx included in the form $c^{-\frac{\log^2 x}{\log(-1)}}$, which when x is within certain limits, satisfies equation (1.), when c^2 is not $= 1$.

To prevent misconception, it may be as well here to remark, that I admit no difference of meaning in *result*, whatever difference of intermediate *operation* may be denoted, between $c^{\frac{-\log^2 x}{\log(-1)}}$, $c^{-\frac{\log^2 x}{\log(-1)}}$ and $c^{\frac{\log^2 x}{-\log(-1)}}$, and that *as a generic form*, I consider $c^{\frac{\log^2 x}{\log(-1)}}$ to be precisely equivalent to $c^{\frac{-\log^2 x}{\log(-1)}}$, for if k be a logarithm of -1, so also is $-k$. Hence both expressions contain exactly the same values, it being remembered that any value common to both corresponds to different individualizations of $\log(-1)$ in each.

The proposed functional solution for ψ being included in the form $c^{\frac{\log^2 x}{\log(-1)}}$ leads us to consider algebraical symbols as capable of possessing imaginary values, since it involves $\log(-1)$, an expression which has no real value. In the present case we shall see that the discussion of the proposed function of x is cleared from the obscurity that surrounds it when x is real, by our knowledge of its properties when x is infinitesimally imaginary. Though this course may seem to savour of the system "*ignotum per ignotius*," it is no less true than singular that in this instance a difficulty more peculiarly affecting reals receives light from the consideration of imaginaries. To exhibit x therefore in its most general form admitted in algebra, including imaginaries as well as reals, let

$$x = y + \sqrt{-1}\, z \qquad (5.)$$

y and z, which I call the "constituents" of x, being independent quantities, positive, negative, or $+ 0$ or $- 0$. Then we shall have

$$\frac{1}{x} = \frac{y}{y^2 + z^2} - \sqrt{-1}\, \frac{z}{y^2 + z^2} \qquad (6.)$$

Here it is important to remark that the second constituent of x is always of a different sign from the corresponding constituent of $\frac{1}{x}$, for if z be positive, $-\frac{z}{y^2+z^2}$ will be negative, and *vice versâ*.

I must now lay down some necessary definitions of the notation employed, preparatory to the statement of a proposition which will be found further on, and I must here add that, in resorting to a new specificatory or individualizing notation, I have unwillingly yielded to necessity, from finding that the indeterminateness of ordinary exponential and inverse trigonometrical expressions almost always occasioned perplexity and frequently led into material errors of reasoning; not that I was unaware of the repugnance which any new notation has to encounter, and the increased difficulties it opposes to the reception of any new theory. Let the symbol $\sqrt{}$, placed before a positive quantity, be appropriated to denote the positive square root; then $\frac{z}{\sqrt{z^2}}$ will denote 1 or -1, according as z is positive or negative. When x is real, and therefore $z = 0$, we have no more right to consider z positive than negative, unless it be known absolutely that the variable x is in such a varying state as to be about to become $x + dy + \sqrt{-1}\, dz$, the sign of the real infinitesimal dz being known; but still, when x is real, and nothing else is known with reference to its varying state, this at least we know alternatively, that if we consider $\frac{z}{\sqrt{z^2}} = 1$, there would be a metaphysical impropriety, a wanton violation of analogy and continuity, in not considering $-\frac{z}{\sqrt{z^2}} = -1$, and *vice versâ*.

When once we introduce into our calculations the consideration of imaginary values, we have to treat x or $y + \sqrt{-1}\,z$, as essentially a variable in both its constituents, and therefore z can never fairly be supposed in the condition of an absolute central stationary zero. Let $l\sqrt{y^2+z^2}$ denote the arithmetical Neperian logarithm of $\sqrt{y^2+z^2}$. Let $\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}$ denote the circular arc not less than $+0$ and not greater than π, which, when radius $=1$, is equal to $\frac{y}{\sqrt{y^2+z^2}}$. It is obvious that such an arc is always assignable, since $\frac{y}{\sqrt{y^2+z^2}}$ can never be greater than 1, nor less than -1.

I come now to the following proposition, before referred to, viz.

"$l\sqrt{y^2+z^2} + \sqrt{-1}\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}$ is a Neperian logarithm or e-log of x." This proposition is proved in my papers on exponential functions cited in the 8th volume of this Magazine, p. 281, (Number for April 1836); but as the heads of the proof are very simple and at the same time illustrate my subsequent reasoning in the present case, I shall briefly recapitulate them here.

Let the notation $e_0{}^\theta$ denote that particular value of the ambiguous expression e^θ, which is represented by the sum of the series

$$1+\theta+\frac{\theta^2}{1\cdot 2}+\frac{\theta^3}{1.2.3}\cdots+\frac{\theta^n}{1.2\ldots n}\cdots \qquad (7.)$$

where θ is not limited to real values. It has not been unusual to treat of e^θ when θ is imaginary, and when this has been done, the meaning of e^θ must have been tacitly defined (though probably without regard to the possibility of e^θ having many values for any given θ) by reference to the preceding series, which is convergent for all values of θ. A series, when resorted to as a definition is most convenient if always converging, but in development a series is not to be considered as correct and safe merely on account of its convergence, for expressions may be assigned which are developable by some incomplete methods in a converging series, and yet may be shown, from the functional properties which constitute their best definition, to be equal to n terms of such series together with a *remainder* (sometimes representable in the form of a definite integral,) which, in certain cases, instead of approaching towards 0 as a limit like the remainder of the series, recedes from 0 as n increases. It is demonstrable from the nature of the series (7.), and may, I think, be assumed as an admitted proposition, that

$$e_0^{\theta+\theta'} = e_0^{\theta}\, e_0^{\theta'} \qquad (8.)$$

Hence $e_0^{\,l\sqrt{y^2+z^2}+\sqrt{-1}\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}}$

$$= e_0^{\,l\sqrt{y^2+z^2}} \times e_0^{\sqrt{-1}\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}} \qquad (9.)$$

Now, of the right-hand member of (9.), the first factor, viz. $e_0^{\,l\sqrt{y^2+z^2}}$ is evidently equal to $\sqrt{y^2+z^2}$, and if it can be shown that the other factor, viz. $e_0^{\sqrt{-1}\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}}$ is equal to $\frac{y+\sqrt{-1}z}{\sqrt{y^2+z^2}}$, our proposition will be proved, for it will be proved that $e_0^{\,l\sqrt{y^2+z^2}+\sqrt{-1}\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}}$ $= \sqrt{y^2+z^2}\times\frac{y+\sqrt{-1}z}{\sqrt{y^2+z^2}} = y+\sqrt{-1}z = x$; that is to say, it will be proved that $l\sqrt{y^2+z^2}+\sqrt{-1}\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}$ is an e-log of $y+\sqrt{-1}\,z$ or x, according to the most limited definition I can conceive of the term Neperian logarithm that will extend to imaginaries.

Now $$e_0^{\sqrt{-1}\theta} = \cos\theta + \sqrt{-1}\sin\theta \qquad (10.)$$

if by $\cos\theta$ be understood the sum of the series

$$1 - \frac{\theta^2}{2} + \frac{\theta^4}{1.2.3.4} - \&c. \qquad (11.)$$

and by $\sin\theta$ the sum of the series

$$\theta - \frac{\theta^3}{1.2.3} + \frac{\theta^5}{1.2.3.4.5} - \&c. \qquad (12.)$$

This not only follows immediately from the definition above given of the notation e_0^x, but the definitions of $\cos\theta$ and $\sin\theta$ accord with admitted theorems respecting the sine and cosine when θ is real. The two series (11.) and (12.) are convergent for all finite values of θ, and I can see no objection to them as definitions of sine and cosine, even when θ is imaginary. I do accordingly treat them as such in my general exponential theory, but for our present purpose it is enough that equation (10.) be admitted when θ is real.

It follows from that equation that

$$e_0^{\sqrt{-1}\,\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}} = \cos\left(\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right)$$

$$+ \sqrt{-1}\sin\left(\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right) \qquad (13.)$$

but $\quad \cos\left(\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right) = \frac{y}{\sqrt{y^2+z^2}} \qquad (14.)$

for an arc, whether positive or negative, has the same cosine,

and $\quad \sin\left(\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right) = \frac{z}{\sqrt{y^2+z^2}}$

for if an arc be respectively in the first positive or in the first negative semicircle, the sign of the sine of that arc will be positive or negative accordingly.

At this step it is important to remark the necessity that exists for the introduction of the expression $\frac{z}{\sqrt{z^2}}$, in order to secure the equality of $e_0^{\sqrt{-1}\,\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}}$ to $\frac{y+\sqrt{-1}\,z}{\sqrt{y^2+z^2}}$ in all cases, for if that expression were omitted, and therefore $\sin\left(\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right)$ substituted for $\sin\left(\frac{z}{\sqrt{z^2}}\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right)$, we should not have

$$\sin\left(\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}\right) = \frac{z}{\sqrt{y^2+z^2}},$$

unless z happened to be positive; for since the sum of the squares of the sine and cosine of any arc is equal to 1, and since the sign of the sine of an arc in the first semicircle is always positive, the sine of $\cos_{0+}^{-1}\frac{y}{\sqrt{y^2+z^2}}$ will be

$$= \sqrt{1-\left(\frac{y}{\sqrt{y^2+z^2}}\right)^2} = \frac{\sqrt{z^2}}{\sqrt{y^2+z^2}}.$$

[To be continued.]

LXVII. *Experimental Researches into the Physiology of the Human Voice.* By John Bishop, *Esq., &c. &c.*

[Continued from p. 277, and concluded.]

THE falsetto, or *voce di testa*, has always been considered a most embarrassing subject of research, and its peculiar quality has excited the attention both of the physiologist and of the musician. The change produced in the voice when passing from the falsetto into the common tone, or the reverse, is in some persons very sensible to the ear, whilst in others it is almost imperceptible. It is remarkable that some individuals have the faculty of producing, in the same pitch, three or four tones, possessing either the falsetto or the common character, a circumstance which indicates that the difference between them depends rather upon an altered state of the vocal tube than upon any change in the glottis.

The falsetto has generally been ascribed to some particular adaptation of the upper ligaments of the larynx. Dodart * has attempted to prove that it is a supra-laryngeal function, and that the nose becomes the principal tube of sound instead of the cavity of the mouth. Bennati † also considers these tones to be modified by the supra-laryngeal cavity, an opinion not justified by the experiments which he has detailed.

According to this hypothesis, we must suppose the influence of the trachea to be entirely annulled; but on what acoustic principle this is to be effected he does not explain, nor indeed can any one else. The changes observed by him in the pharynx were undoubtedly associated with corresponding changes in the whole length of the tube, and all the phænomena he has described may thus be readily explained.

It was suggested to me by Mr. Wheatstone, that it was only necessary to suppose the vocal tube capable of subdividing its vibrating length to account for this peculiar character of tone. Analogous effects are observed in the clarionet, the flute, and other instruments; the change taking place at the twelfth of the fundamental note in the former instrument, and at the octave in the latter. Having had an opportunity of examining the phænomena in some individuals possessing remarkably fine voices, I placed my finger lightly on the larynx, and requested them to gradually elevate the voice from the primary to the falsetto tones, when, although the ear could scarcely distinguish

* *Mém. de l'Acad.*, 1707.

† *Recherches sur le Mechan. de la Voix Hum.*

the moment of transition, I found that the larynx suddenly fell, and then continued to re-ascend as the tones became more acute. On observing the motions of the larynx in a mezzo-soprano voice, I found a double falsetto, consisting of several tones of each register, with the power of yielding either the primary or the falsetto character. In this case the larynx fell twice, but in a much smaller degree. An instance of this kind of voice occurs in Miss Lanza. At the moment the larynx falls, during the continued ascent of the tones, the column of air and the tube become divided into portions separated by nodes, yielding harmonics of the fundamental notes, and the modulations of the voice are regulated, as before, by the divided length and relative tension of the tube. A much smaller quantity of air is sufficient to produce these tones; consequently, public singers who chiefly employ the falsetto, suffer much less fatigue than those who use the primary notes.

In conducting these observations, care must be taken that the voice do not ascend or descend the scale too rapidly, otherwise the effect may escape detection. In further confirmation of these views it may be remarked, that when the glottis is injured and silenced by disease, the voice is entirely annihilated, which could not be the case if there were any means of producing sound by the superior ligaments of the larynx. There is, however, no doubt that the human voice derives a portion of its peculiar quality from the reverberations of sound in the cavities of the chest and head, modified by every change in these cavities as well as in the vocal tube. (The great effect produced by the nasal cavities on the voice is well known.)

Much pains have been taken by physiologists to find an analogy between the organs of voice and artificial musical instruments. Amongst those which have been selected for this purpose are the drum, the duck-whistle, the reed, and various other wind and stringed instruments. These attempts serve to illustrate the complicated structure and functions of the vocal organs; but it appears to me more simple, and at the same time nearer the truth, to consider them in the following point of view:

They consist of elastic membranes inclosed in a tube. The glottis is a most complex and beautifully constructed membranous vibrating apparatus, exquisitely adapted for producing all the tones of the voice. The vocal tube, or pipe, is adjusted on the most refined acoustic principles, to yield with the glottis isochronous vibrations.

The perfect adaptation of these organs, in a manner inimitable by mechanical art, to produce the most melodious sounds, and to vary them so as to imitate the tones of birds, beasts, and musical instruments, with an almost infinite variety of other sounds, justly excites our admiration and astonishment. Notwithstanding the great labour bestowed by musicians on the temperament of keyed instruments, with a view to correct the dissonances occurring in the construction of the diatochromatic and enharmonic scales, so as to satisfy the ear, such instruments are far inferior to the vocal organs, which can produce all the tones necessary for the most exquisite and perfect harmony.

The association of the organ of hearing with that of the voice tends materially to its utility and perfection. Congenital deafness deprives a person of the power of acquiring articulate language, except by a laborious process of tuition and to a limited extent.

By very slight modifications of the tube, the simple uninterrupted tones of the voice will produce the vowel sounds, which have accordingly been imitated by Kratzenstein, De Kempelen, Willis, and others, through the medium of artificial mechanism. The interrupted sounds, or *voces limitatæ*, require, on the other hand, the co-operation of the pharynx, tongue, teeth, cheeks, lips, and nostrils; the various actions of which, by checking the sounds, produce the gutturals, dentals, and labials of grammarians. According to the mode in which the interruption takes place, and to the varied adjustments of the organs employed in effecting them, these are distinguished into mutes, explosives, nasals, liquids, and gutturals: but the manner in which these effects are produced, it is not my present purpose to investigate, and indeed they have been already minutely analysed by Haller, Sœmmerring, Blumenbach, Bell, Magendie, Bichat, and others. In the use of articulate language the variations of the voice are usually within a minor third, either above or below the pitch of the vocal tube, and the inflections of tone are generally in the minor key. When in the vibrating position, the glottis is capable of yielding sounds during inspiration, which are used by some persons for the purposes of ventriloquism. The expressions of pleasure and pain are produced by mere variations of tone, without the aid of articulation. In laughing, the voice is repeatedly interrupted, in consequence of the glottis being alternately opened and closed in quick succession. In crying, the tones follow each other in enharmonic and discordant, but longer intervals.

The views here taken of the functions of the vocal organs, and of which the following is a brief summary, are confirmed both by analogy and by experiment, which, I conceive, afford demonstrative proof of the truth of the theory now advanced, and completely refute those to which reference has previously been made*.

First. The vibrations of the glottis are the fundamental cause of all the tones of the human voice.

Secondly. The vibrating length of the glottis depends conjointly on the tension and resistance of the vocal ligaments, and on the pressure of the column of air in the trachea.

Thirdly. The grave tones vary directly and the acute tones inversely as the vibrating length and tension of the vocal ligaments.

Fourthly. The vocal tube is adjusted to vibrate with the glottis, by the combined influence of its variations of length and of tension.

Fifthly. The elevation of the larynx shortens the vocal tube, and its depression produces the contrary effect. The diameter and tension of the tube vary reciprocally with the length.

Sixthly. The falsetto tones are produced by a nodal division of the column of air, together with the vocal tube, into separate vibrating lengths.

Seventhly. The pitch of the vocal organs, when in a state of rest, is in general the octave of their fundamental note.

In conclusion, it may be remarked that the physiology of the human voice cannot fail to be a subject of interest to every inquiring mind, and many whose names shed a lustre on science have devoted a considerable portion of their time to its investigation. The advantages resulting from the study of the voice not only tend to enlarge the sphere of natural knowledge, but also, in a medical point of view, serve as a basis for diagnostic, therapeutic, and pathological inquiry, and consequently contribute to the general benefit of mankind.

Animals far inferior in their organization and intellect to man, are endowed with the power of uttering tones sufficient for the sphere of their existence. The roar of the lion, the lowing of the ox, the song of birds, and the hiss of serpents constitute a natural language which adequately expresses their wants and their passions, and is sufficient for the degree of intelligence belonging to the rank which they occupy in the scale of animal organization.

* The hypotheses of Aristotle and Dodart respecting the size of the chink of the glottis must necessarily place the thyro-arytenoidean ligaments out of the vibrating position: the same objection applies to that of Ferrein. The theory of tension requires the glottis to be always open, and vibrating in its whole length, to produce every tone of the vocal scale, a supposition which is opposed both by observation and by experiment.

"'Tis sweet to hear the honest watch-dog's bark
Bay deep-mouth'd welcome, as we draw near home;
'Tis sweet to know there is an eye will mark
Our coming, and look brighter when we come.
'Tis sweet to be awakened by the lark,
Or lull'd by falling waters—or the hum
Of bees—the voice of girls—the song of birds—
The lisp of children, and their earliest words."

The human voice may be denominated the music of the mind; language, a figurative mode of expressing our ideas and sentiments. The effects flowing from this beneficent endowment are overwhelming in contemplation and almost infinite in extent. It is principally instrumental to all the moral and physical improvements of man, and enables him to pour forth his otherwise invisible, inaudible, unfathomable thoughts, to his fellow-man and to his God.

Explanation of Plate III.

Fig 1, Is a representation of the larynx, having the left wing of the thyroid cartilage removed, to expose a portion of the internal structure.

a. The right internal surface of the thyroid cartilage.
b b. The arytenoid cartilages.
c c. The thyro-arytenoid ligaments; the mucous membrane being removed.
d. The chink of the glottis.
e e. The posterior crico-arytenoid muscles.
f. The left lateral crico-arytenoid.
g. The cricoid cartilage.
h. The trachea.
i. The membranous and muscular portion of the trachea, which regulates its diameter.

Fig. 2, Is a representation of the larynx, similar to fig. 1, showing the whole of the muscles of the left side at one view. The mucous membrane is dissected away, and the upper edge of the thyro-arytenoid muscle slightly depressed, to expose the ligaments of the glottis.

a, *b*, *c*, *e*, *f*. The same as in fig. 1.
d. The thyro-arytenoid, superior muscle.
g. The cricoid cartilage.
h. The thyro-arytenoid muscle.
i k. The trachea.

Fig. 3. This figure presents a section of the larynx, immediately above and parallel to the plane of the glottis. The view is vertical, with the mucous membrane removed to show the mechanism by which the voice is principally modulated.

a. The rimula glottidis in a state of relaxation.
b b. The thyro-arytenoid ligaments.

c c. The thyro-arytenoid muscles.
d d. The lateral crico-arytenoideal muscles.
e. The edge of the thyroid cartilage.
f f. The arytenoid cartilages, with their perpendicular projections cut through at *f.*
g. A portion of the transverse arytenoid muscle.
h h. The posterior crico-arytenoid muscles.

Fig. 4. An internal anterior view of the larynx, produced by a section transverse to its antero-posterior diameter.

a. The epiglottis.
b b. The os hyoides.
c c. The segments of the internal surface of the thyroid cartilage.
d d. The thyro-arytenoid muscles.
e e. A portion of the thyro-epiglottideal muscles.
f f. The pseudo-glottis.
g g. The sacculus laryngis.
h h. The cricoid cartilage.
i i. The chordæ vocales, lying nearly parallel to the axis of the vocal tube.
k. The internal aspect of the trachea.

Fig. 5. The posterior segment corresponding to fig. 4.

a. The pharynx.
b b. The arytenoid cartilage invested by its mucous membrane.
c c. The chordæ vocales.
d d. The thyro-arytenoid muscles.
e e. The wings of the thyroid cartilage.
f f. The cricoid cartilage.
g. The trachea.

Fig. 6. This figure represents a transverse section of the larynx: the thyro-arytenoid ligaments are turned perpendicular to the axis of the vocal tube; the glottis is seen in the true vibrating position.

a a. A section of the thyro-arytenoid ligaments.
b b. The pseudo-glottis.
c c. A section of the thyro-arytenoid muscle.

Fig. 7, Is an external side view of the larynx, showing the action of the crico-thyroid muscles, by which the cricoid is rotated with the thyroid cartilage, and the tension of the local ligaments affected.

a. The situation of the insertion of the vocal ligaments.
b. The upper posterior edge of the cricoid cartilage, to which the arytenoids are articulated.
c. The left crico-thyroid muscle.
d. The articulation of the thyroid to the crico-cartilage.
e. The dotted line, showing the position of the cricoid

cartilage when rotated with the thyroid, whereby the antero-posterior diameter of the larynx is enlarged and the vocal ligaments stretched from *a d* to *a e*.

f. The chink between the thyroid and cricoid cartilages. The dotted line represents the closing of this chink when the cricoid is rotated on the axis of motion of these cartilages at the point *d*.

Bernard Street, Brunswick Square, June 7, 1836.

LXVIII. *On the Limestones found in the Vicinity of Manchester. By* W. C. Williamson, *Curator of the Museum of the Manchester Natural History Society.*

[Continued from p. 249, and concluded.]

Sect. VII.—*Other Localities where Limestones are exposed.*

At two more localities limestones have been pointed out to me by Dr. Charles Phillips, who has contributed so much towards the elucidation of the geology of this district; both of these were exposed by the cutting of the Liverpool and Manchester Railway. One of these is at the base of the Sutton inclined plane on the Manchester side, and the other near Whiston, on the opposite side of the hill, forming two of the points which guided Mr. E. Hall in laying down the range of the magnesian limestone, with which he confounded them.

That at Whiston is exposed for about seventy yards, and forms a seam six feet thick, but which never rises more than nine feet above the level of the railway and is covered for some extent by a series of solid sandstones. The top of the limestone consists of a greenish conglomerate, below which is presented the reddish conglomeroid structure peculiar to the Ardwick limestones, and the lower portion of the seam is composed of the solid gray limestone common at the above locality.

From its so strongly exhibiting the peculiar mottled appearance, I am of opinion that it will correspond with the uppermost or four-feet seam at Ardwick. This is in some measure confirmed by the occurrence of the same minute turbinated shells (Planorbis?) found in the Ardwick series; but as no borings have been made on the spot, I cannot be certain as to what exists below.

The limestone at Sutton I cannot identify with any exposed at Ardwick, although in its appearance and fracture it resembles the thin one of two feet below the main limestone on the bank of the Medlock. It is only about a foot thick, and rests upon a long range of the coal shales, which are very well exposed, exhibiting many partial faults and thin coal seams.

As I before described, at Collyhurst we have the magnesian limestone laid bare for a small space; and about half a mile further, on St. George's Road, the coal measures appear at the pits of E. Buckley, Esq. According to the regular range of the strata, we ought to find the Ardwick limestones exposed between these two points, but they do not appear: on an examination of the locality, the reason is obvious; the red sandstone ranges unconformably with the coal strata, overlying their outcropping edges, and thus completely covering up that portion in which the limestones ought to have been met with. The locality is a very difficult one to examine and decide upon, from the want of sections at the most important points; but as there is no doubt of the sandstone overlying the edges of the coal strata, and the dip of the latter being such as would carry their upper portions beneath where we find the red sandstone on the surface, the apparent deficiency is accounted for.

Sect. VIII. *Organic Remains.*

The fossil remains of this series of limestones, with one or two exceptions, are neither numerous nor exhibiting much variety, although they are of a peculiar character: these we will examine in their separate classes, and then from their evidences endeavour to draw some conclusion as to the nature of the limestones and the circumstances under which they were deposited.

1. *Vegetable Remains.*

These are not so numerous as, from the connexion of the strata with the coal measures, might have been expected, although further investigation will doubtless bring new deposits to light.

Stigmaria ficoides is found abundantly in the seam of coal immediately below the black bass. The coal appears to have been entirely composed of this plant, as it is the only one I have hitherto found in connexion with it. The character this extraordinary plant must have given to the primæval world cannot fail to have been highly singular, as from the highest to the lowest coals of this group of strata its abundance generally forms a conspicuous feature, whilst its range appears to have been almost universal. In a seam of indurated blue clay, below the black bass, the leaves of *Stigmaria* are found in the greatest profusion. The same is observed in a reddish clay on the bed of the Medlock, much lower in the series.

In the blue clay, immediately above the black bass, I found a small and beautiful species of *Sphenophyllum*, much like *Sphen. erosum*, but with broader leaflets, and fewer in each whorl. In the same clay occurred fragments of a species of *Pecopteris* and also of an *Equisetum*, neither of which I was able to preserve.

In working through a narrow passage of one of the mines, used only as a drain to carry off the water from the pits, Mr. Mellor and myself were rewarded by the discovery of a thin seam of red shale, about sixty feet above the main or three yards limestone, filled with the most beautiful remains of plants, which fully confirmed the opinion Dr. Charles Phillips had been the first to form and advance. Amongst these were large specimens of *Calamites decoratus*, Brongn., and *Cal. nodosus*; stems of *Lepidodendron Sternbergii*, an elegant species of *Neuropteris** with large leaflets, a small *Cyclopteris*, leaves of *Stigmaria ficoides*, fragments of a *Pecopteris*, *Asterophyllites*, and several other plants common to the coal series below, forming a character which, if any truth exists in the theory of identification, cannot for a moment be mistaken.

2. *Mollusca.*

These, like the plants, are not of numerous species. In the three limestones worked at Ardwick, and also at Whiston, but most abundant in the Three Yards mine at the former locality, are countless myriads of a small depressed turbinated shell, the merit of the first discovery of which is due to Professor Phillips, who is at present investigating its nature. It bears the strongest resemblance to a Planorbis, and is evidently of a nature very similar to the one found at Burdiehouse, by Dr. Hibbert, and figured in his memoir (page 13.). In form it closely resembles the recent *Planorbis Nautilus* (Fleming,) but is rather smaller. It occurs, as I said before, in all the limestones worked, and Dr. Phillips found the same fossil in the limestone at Whiston, and also in a fragment of shale from the colliery of E. Fitzgerald, Esq., at Pendlebury, about four miles from Manchester. It was found in sinking down from the upper or "two-feet coal" to Buckley's "three-quarters mine," the highest coal of any importance in the Lancashire coal-field.

The black bass is literally filled with fragments and perfect shells of a species of *Unio* of small size. It bears a considerable resemblance to Hibbert's *Unio nuciformis* from the Burdiehouse limestone, but is of a less globular form. This shell varies considerably in size, being sometimes one and a half inches in length, and at others not more than three quarters of an inch. The depressed and crushed state in which these fossils are found would indicate a shell of a thin and fragile nature, and such it has doubtless been: they are most fractured towards the lower portion of the bass, no perfect ones being there observable, but towards the top they are generally uninjured, further than has been the result of pressure. Of

* This I find to be *Neuropteris cordata*, which Dr. Phillips has since met with in the coal measures at Oldham.

this shell, I found a single specimen in the blue clay which contained the *Sphenophyllum*; and in one of the thin seams of red clay, above the first limestone, they are also to be met with, though of a smaller size. Dr. C. Phillips found them at Pendlebury, in the fragment of shale containing the Planorbis. This was the circumstance that first induced the above gentleman to differ from former observers in his opinion as to the relative position of the Ardwick limestone, separating it from the saliferous and connecting it with the carboniferous group of rocks, an opinion of the correctness of which there remains not the slightest doubt.

My friend Mr. Joshua Alder, of Newcastle, informs me that he has met with an *Unio*, closely resembling our specimens, in the coal strata at the above place. This shell differs from the *U. nuciformis* of Burdiehouse in being broader and wider in proportion to its length, as well as in being a more fragile and delicate shell. I am inclined to think it is an undescribed species; if so, I would propose the name of *Unio Phillipsii* as a slight return, not only for the private kindness I have met with from Dr. C. Phillips, but for the service that gentleman has done to geology by his indefatigable exertions in investigating the nature of these limestones, as well as being under his guidance in a district new to me when I first discovered this most characteristic shell.

Towards the upper portion of the third limestone is found a thin seam of comminuted fragments of shells, amongst which may occasionally be found traces of more perfect specimens. These bear a close resemblance to, if they are not absolutely the same as, the fossil now under consideration. From the several localities where these shells are found, and from their extreme abundance, combined with the rarity of other Mollusca of equal size, they must have formed an important feature of the fresh waters of that early æra.

In the blue clay, immediately above the black bass, are a series of remains, in attempting to decide upon the nature of which I find myself completely puzzled. They are very thin bodies of a brown colour, nearly square in their form, two of the corners being angular, and the opposite one rounded: I have some nearly a quarter of an inch across. At first I imagined that these were scales of fish, but now think they must be same bivalvular shell. Their surface is marked with strong concentric ridges, and passing from the hinge (?) to the opposite corners, are two diverging elevated lines. I cannot detect any traces of teeth, but have found several specimens in which the two valves (?) were connected at the hinge, and the four ridges commencing from one common point in the centre and diverging two each way: these I pointed out to

Professor Phillips, who will perhaps be able to lay before the public some more decided opinion as to their nature.

3. *Entomostraca.*

Microscopic fossils are always with difficulty assigned to their proper situations in the scale of organized life. This difficulty was experienced by Dr. Hibbert amongst the Entomostraca of Burdiehouse, and as a necessary consequence, we experience the same amongst those of Ardwick. Throughout the whole extent of the black bass, but especially amongst the broken *Unionidæ* near the seam of coal, we find abundance of minute remains, generally about $\frac{1}{20}$th of an inch in length, which can only be assigned to the above-named group of Crustacea. I am uncertain whether there are one or two species: if two, the one will be a *Cypris*, approaching very closely to *Cyp. Faba* in its beanlike form, but rather more elongated: the chief objection is, our not being able to detect the hinge. The other species, if different, is in reality subunivalve, with a lateral opening on one side: this is closely allied to the recent *Daphnia*, and is probably of Hibbert's genus *Daphnoidea*. It is of the same size and outward form as the one above described, of which it may only be the opposite side, showing the natural groove: when largely magnified, I cannot compare it to anything better than the berry of the coffee-tree after it is burned. To judge from the drawings Dr. Hibbert has given, I think our *Daphnoidea* presents the lateral opening much more distinctly than any he has observed, from which ours differ in being perfectly smooth instead of granulated.

From these remains we have, without any other evidence whatever being wanted, a strong proof of the freshwater origin of this portion of the series.

4. *Remains of Fish.*

We now come to the group of remains which first attracted my attention in the limestones of Ardwick. In June 1835 I first detected remains of fish in the black bass, and have since then at various periods made new additions to my collection. As we have in no one instance discovered a perfect specimen, the difficulty of identifying them with any known species must of necessity be great, especially as they are in such a crushed state that not even two scales can be found preserving their relative positions.

In the fifth number of the Zoological Journal, published April 1825, is a drawing and description, by the late amiable and talented E. S. George, Esq., and J. D. C. Sowerby, Esq., of a remarkable bony plate, which the writers imagined to be a portion of the palatal bone of some fish, found near Leeds in the seam of coal commonly known in Yorkshire by the name of the Beeston seam: this has since been examined by M. Agassiz, who pronounced it to be a portion of the large tuberculated scale of some species of fish.

In the main limestone at Ardwick was found a specimen so exactly resembling the one from Leeds, that the drawing would almost serve for a representation of either. The only difference I am able to perceive is, that the marginal portion, which in the Leeds specimen is smooth, in ours is slightly tuberculated. It is also of an irony colour, whilst the other is described as being of a bright glossy black. The specimen was presented to the Society by Mr. Francis Mellor, the director of the Ardwick lime-works.

In one very thin seam passing through the black bass, the position of which is generally marked by a line of pyrites, are found remains of a small species of fish, much crushed, the fragments being all detached.

Of the scales I possess several forms: they are generally small, rhomboidal, and of a bright glossy black, often corrugated on the surface. Others are arrow-shaped, about one third of an inch long, having a depressed sulcus passing along the centre especially at the broad extremity, and irregular striæ towards the apex. A third form, very thin, irregular in size, and marked with dots and undulating lines, I at first mistook for palates, but on comparing them with a specimen of a Palæoniscus from the copper slate of Eisleben, I was enabled to determine to what portion of the fish each scale belonged: the thin undulated ones are portions of the reticulated covering of the head; the rhomboidal, some smooth and some corrugated, belong to the body; and the larger arrow-shaped ones have formed a single line along the dorsal ridge, from near the dorsal fin to the insertion of the tail. I cannot venture to say that they are of the same identical species, but they certainly approach very near in their characters.

In one instance I found part of a small jaw-bone of some species of fish, which may have some connexion with the scales above described: it is about $\frac{1}{3}$rd of an inch in length, and is furnished with a regular row of obtuse, glossy black teeth, eleven or twelve in number, and about $\frac{1}{16}$th of an inch in length. It is from the black bass.

The same fruitful seam contains strong bony rays, similar to what we often find supporting the large dorsal fins of many species of fish. They are generally depressed, although we occasionally meet with them in their original rounded form. I have one specimen $2\frac{1}{2}$ inches in length, but which must have been considerably longer.

The most singular fossil I have yet met with is a specimen from the black bass, and evidently connected with the teeth of fish. Attached to a peculiar round body are two teeth (?) about half an inch long, with the two lateral cutting edges finely denticulated: they are separated about $\frac{1}{10}$th of an inch at their base, but diverge until their points are nearly half an inch asunder. I only know one portion of a fish to which this singular fossil could belong, and that is, the apex of the upper jaw. In many species of foreign fish are two teeth in that situation which diverge as in our specimen, and this must have been of a similar nature. It may belong to the same species as the long rays*.

In the roof of the Four-feet mine, Professor Phillips has been so fortunate as to discover remains of *Megalichthys Hibbertii*, a fossil which is now apparently diffused through several of the limestones of the coal series: Mr. Mellor has in his possession a beautiful specimen of a lower jaw with a row of five teeth, with several other fragments. These are generally indistinct and ill defined in their outlines. With the exception of Mr. Mellor's beautiful jaw, the finest specimens of this interesting animal have fallen into the possession of Professor Phillips†, who will doubtless, in his expected paper at the meeting of the British Association, give a detailed account of them, and their affinity with specimens from other districts which I have not had an opportunity of examining.

The following catalogue comprehends such remains as we have now discovered in and above the third or main limestone:

Plantæ.—Neuropteris cordata, Pecopteris, Sphenophyllum, Sphenopteris linearis ?, Cyclopteris, Lepidodendron Sternbergii, Stigmaria ficoides, Calamites decoratus, Calamites nodosus, Asterophyllites.

Mollusca.—Planorbis, Unio, unknown Bivalve ?.

Entomostraca.—Cypris, Daphnoïdea ?.

Ichthyolites.—Palæoniscus (scales and teeth). Teeth, opercular bone, and rays of an unknown species. Megalichthys Hibbertii (scales, lower jaw, teeth, &c.).

Sect. IX. *General Results and Inferences.*

From these detailed descriptions and simple facts, we may draw a few inferences as to the nature of the limestones and the circumstances under which they have been deposited.

This group of limestones has hitherto, as I before observed,

* Since the above was written I have discovered a second specimen of this most interesting fossil, closely resembling the one described, with the exception of being rather smaller: it throws no new light upon its nature, except exhibiting a small rounded tooth or process, about $\frac{1}{8}$th of an inch long, fixed between the other two. At the same time I found an opercular bone, probably connected with the same species.

† Since then I have found a large scale of this animal in the black bass, of a rhomboidal form and closely resembling the scales of the thigh of the American alligator.

been confounded with the magnesian limestone, which latter stratum had never been distinctly identified in this neighbourhood, until an examination of the fossils of Collyhurst led me to the conclusion, that the clays and thin limestones there exposed were the representatives of that series, so important in Yorkshire and Durham. On comparing the fossils of the magnesian series with those from Ardwick, we shall find that no one species found at the latter place agrees with any yet discovered in the magnesian limestone in England, the strongest evidence that they do not belong to the above series of rocks. The small fish bears a considerable resemblance to the Palæoniscus from the copper slate or zechstein, a stratum concerning the relative position of which I have some doubt.

On comparing the fossils, however, with those from the carboniferous system, we immediately observe their identity: we have *Stigmaria ficoides*, the almost universal characteristic of the coal measures of Lancashire and Yorkshire; several Filices, especially a *Sphenophyllum*, a genus, I believe, confined to the coal series; Calamites, and the still more important leaves and stems of the *Lepidodendron Sternbergii*. However undecided we might be previously, the discovery of these remains cannot leave the slightest doubt as to their connexion with the carboniferous group.

The merit of the discovery of this important generalization belongs to Dr. Phillips, who, in May last, explained his views to the section of the Manchester Philosophical Society, and expressed his firm conviction that geologists had hitherto been in error in connecting these limestones with the magnesian series; and the deciding upon the relative position of so large a mass of strata, in such close connexion with the Lancashire coals, cannot be viewed otherwise than as an important result.

On a slight examination of the fossils, we observe another important fact: *no marine remains whatever have yet been discovered.* When we find freshwater remains mingled with those of marine origin, the probability is that the deposit was formed in some estuary or mouth of some large river; but here the remains are entirely of freshwater origin. All the most important fossils which guided Dr. Hibbert in arriving at his splendid results at Burdiehouse, we find here. The entire absence of marine remains: the extreme abundance of microscopic freshwater Entomostraca, of Unionidæ; fish of the genus *Palæoniscus*; minute univalves, in all probability of the freshwater genus *Planorbis*; and lastly, the discovery of remains of *Megalichthys Hibbertii*, all concur in assigning to the limestones a freshwater origin. It would be needless for me to enter here into any long discussion on the nature of freshwater limestones, or the manner and circumstances under which they have been produced, as that has been done in so masterly a style by Dr. Hibbert, in his memoir on the Burdiehouse limestones: and the fact of the occurrence of freshwater limestones in the carboniferous group has become so firmly established, that what remains to be done is a careful investigation of the districts where the coal measures are exposed, in order to trace how far they extend, and what varieties of remains of animal life they present. The ultimate result, I have no doubt, will be a vast mass of evidence respecting the circumstances under which the coal measures generally have been deposited, and a considerable additional light will thus be thrown upon the origin and formation of the coal itself. Thus, as the small mountain rivulet, receiving new force and power from the most insignificant sources, gradually rolling on towards the wide ocean, becomes the broad and noble river, so each new fact, however trifling in itself, will give a slight but additional impetus to the stream of knowledge which is fast bearing us forward to the ocean of some grand theory of geology: the collection of facts thus slowly accumulated will one day be grasped by some comprehensive and master mind, —a new Newton will arise and place in our hands one universal outline of the laws that have guided and still guide nature in her unvarying progress.

W. C. Williamson.

Hall of Manchester Natural History Society,
August 12th, 1836.

LXIX. *On the Beds immediately above the Chalk in the Counties near London.* *By* James Mitchell, *LL.D., F.G.S.**

THERE is a description of flints found in beds immediately over the chalk, and below the sand, in all the places where these strata are seen to meet, in the counties of Surrey, Kent, Essex and Hertford, and may very probably also be found in other counties, also in similar situations.

There are sixteen localities in which I have seen this flint on the south side of the Thames, and five on the north side.

The pits on the south side are:

Pit close to Croydon in Coomb Lane.

Road to Tunbridge beyond Farnborough.

Pit on the right of the road from Bromley to Chiselhurst.

Pit in a vale on the south side of Elmstead near Chiselhurst.

* Communicated by the Author.

Loam-pit hill near Lewisham.

Mouth of the Cavern on the side of Blackheath hill.

Pit in Old Charlton on the south side of the Woolwich road.

Great pit at New Charlton.

Cliff opposite Woolwich dock-yard.

Pit at Erith.

Three pits near the bank of the river Cray near Crayford.

Pit on the north side of the churchyard at Dartford.

Trenches dug for forming a common sewer near Gravesend, on property belonging to Mr. Rosier.

The entrance to the tunnel of the Thames and Medway canal at Higham.

On the north side of the Thames the localities observed are, Purfleet, a pit on the west side and another on the east side ofBelmont Castle near Grays, and a pit on the east side of Grays; also a pit at George's Farm near Hertford.

A similar stratum is seen at Newhaven in Sussex, and the same flints have been seen in other counties, but not in their original site.

At Purfleet the name given to these flints is iron flints. The bed in which they are found is generally about eight or nine inches deep, seldom above a foot, and consists of a reddish clay with an abundance of oxide of iron. Scarcely any sand can be got from this clay by washing. It is stuck quite full of flints. Some of these flints are very small, not exceeding an inch in length; but the greater portion are three or four inches long, and some much longer. They are round, and terminating in a point at each end, and on the whole in form not unlike a cucumber. Some however are of a triangular form. The exterior is covered with a rough black crust, which is found to be a combination of silex and oxide of iron. When broken by the hammer the oxide of iron is found to penetrate about a quarter of an inch all round, and there are frequently streaks of iron further in the interior. The body of the flint is black, but decidedly distinct from the dark blue flints found in chalk. The fracture is conchoidal, but these flints, though not unmanageable, do not yield flakes in any direction so readily as the chalk flints. When burnt in the fire the exterior crust becomes reddish, and the rest of the flint is of a dirty white colour, not nearly so bright and beautiful as the porcelain substance made by burning chalk flints. It is exceedingly difficult, with ever so great caution, to get them burnt without cracking and flying in pieces. Such of these flints as have been exposed to the atmosphere have become partially decomposed immediately under the black crust.

I have been informed by two gun-flint makers that such flints afforded the best sort of gun-flints for gentlemen's fowling-pieces; but being less easily made than the gun-flints from the chalk flints, and the material being less abundant, they were more expensive. On account of the presence of so much iron they are totally useless in the porcelain manufacture; but I have been informed that on one occasion as many as ten tons were obtained at the bottom of the great pit at Erith which were so pure as to be saleable for that purpose. Few specimens are large enough for building, and therefore their chief use is for road-material.

An irregular broken line of flints of this description is to be seen at the sides of the deep cuttings at the entrance of the tunnel of the London and Birmingham railway beyond Watford. Great diluvial action has taken place, and the upper surface of the chalk is torn and ridged; and if, as we have no reason to disbelieve, there was a bed of sand here over the chalk, we must in consequence suppose that it has all been carried away. But the flints peculiar to such beds are seen above the solid chalk, and below the diluvial matter, scattered along on both sides of the cutting.

I have seen such flints in considerable quantity in the fields on the east side of Margate, in the Isle of Thanet, and likewise in Norfolk and Suffolk.

In the pit on the south side of Elmstead near Chiselhurst, and in the pit on the south side of the Woolwich road, there are in the same beds with these flints innumerable small fragments of the same kind of flint, but not in the least rounded, and with sharp edges; which proves that in these localities there had been agitations, and that the flints had readily been broken into fragments.

The most extensive section of this stratum which I have ever seen is at Newhaven in Sussex. It extends upwards of a mile along the top of the cliff, and the peculiar flints may be collected all along the foot of the cliff. They are well known to the fishermen who collect chalk-flint boulders for the Staffordshire potteries, and are carefully avoided. One of these men said to me that he knew that one of them would be enough to spoil a hundred pounds' worth of good material. I found a cast of an Echinus there, but not a particle of the shell remained upon it.

There is another and very distinct variety of flint, which may be seen in some of the pits in the same bed in which the preceding variety has been found. I may mention the pit at old Charlton near the Woolwich road, in the trenches dug by Mr. Rosier near Gravesend, and on the south side of Elmstead near Chiselhurst; also at Purfleet.

These flints are very large and of very irregular shape. They bear some resemblance to very large blocks of flint frequently seen towards the top of some chalk-pits. When broken with the hammer, however, they present instead of the deep dark blue of the chalk flints a grayish surface, in many parts whitish, as if composed of silex differently granulated, and probably mixed with argillaceous or other matter. When burnt in the fire they appear still more decidedly different in appearance. There are streaks of oxide of iron diffused near the surface, and in many instances throughout the whole mass. The whole flint has not been formed at once, but one part has aggregated after another, and the divisions are very perceptible to the eye.

In the face of a fractured flint of this species is frequently seen one dark black piece surrounded with a mass of grayish flint, and sometimes more than one such dark piece. But sometimes also there is a grayish piece of flint surrounded by black flint: many divisions in curved and generally circular lines are perceptible.

When burnt in the fire these flints separate into pieces, leaving almost smooth surfaces on both sides at the places of separation. These flints are totally useless for gun-flints or for the porcelain manufacture, but are excellent for building.

Such flints are well known at Northfleet, being found very abundantly in the loam immediately over the chalk-pits near to Gravesend. But the watery action there has been so considerable that there is merely a thin stratum of diluvial matter and vegetable mould over the chalk; so that although I have known of them for several years past, it was not until a few months past I discovered them *in situ* in the places already stated. They are not unfrequently seen in the fields in the counties round London where the chalk comes up near to the surface. If a name be given to them it might be the gray iron flint.

In this flint I have found at Northfleet abundance of Echini, the *Echinocorys*, *Scutatus*, *Conulus*, and *Spatangus*, generally much crushed, and the shells themselves totally gone, probably corroded and destroyed by the oxide of iron.

Mr. Parkinson, in a paper published in the first volume of the Geological Transactions, has remarked the difference of the casts found in the gravel from the casts found in the chalk, and contends that they are of different origin. I have no doubt they are the same species as those in the chalk, and that this bed which I have attempted to describe is the original habitat of the fossils to which he refers.

The BEDS of SAND immediately above these flints are of different kinds.

At New Charlton it is a fine white sharp sand, and is used in the foundries about London. The sand at Old Charlton is almost as good, but it is chiefly used for sanding floors. At Erith, Purfleet, and Grays it is only fit for ballast. But in the trenches dug by Mr. Rosier, and at Elmstead, it is quite different. On taking samples of this sand to London and washing it, and examining it when dry, I found it to be sand exactly the same as that which is well known near London as brickmakers' sand, which is exceeding soft when felt between the fingers, and is used for sprinkling the brickmakers' moulds to prevent the adhesion of the clay. It is obtained for that purpose from above the mud opposite to Woolwich, and off Crayford Point, and is, no doubt, washed down by the rain and brought into the Thames by the rivers Cray and Darent from the districts through which they run.

In respect of the chalk immediately below these beds of flint, in most of the localities examined the surface is perfectly level, even, and unbroken; and it is not furrowed and indented as some observers have represented the upper surface of chalk always to be.

That opinion must have been formed from the circumstance of the chalk, in almost all places where it is near the surface of the ground, having suffered much diluvial action; and from chalk-pits generally being opened, from a motive of saving expense, on the sides of hills, where there is little top surface to be carried off. In the pit at Elmstead it is, however, different from the other localities. There the chalk is cut into ridges, the hollows of which are filled with these flints and clays coloured with iron: which is a proof that in one instance at least the chalk may have suffered diluvial action before the formation of these flints and of the beds of sand over them; but being only one locality out of twenty-one, it shows that the general rule is otherwise.

LXX. Mr. J. Saxton *on his Magneto-electrical Machine; with Remarks on* Mr. E. M. Clarke's *Paper in the preceding Number.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I REGRET that I am called upon to notice a very disingenuous article in the Number of the Philosophical Magazine for October. A reader unacquainted with the progress which magneto-electricity has made since this new path of science was opened by the beautiful and unexpected discoveries of Faraday, might be misled, from the paper I have alluded to, to believe that the electro-magnetic machine there represented was the invention of the writer, and that the experiments there mentioned were for the first time made by its means. No conclusion, however, would be more erroneous. The machine which Mr. Clarke calls *his* invention, differs from mine only in a slight variation in the situation of its parts, and is in no respect superior to it. The experiments which he states in such a manner as to insinuate that they are capable of being made only by his machine, have every one been long since performed with my instrument, and Mr. Clarke has had every opportunity of knowing the truth of this statement.

Though my machine is tolerably well known to the public from its constant exhibition at the Adelaide-street Gallery since August 1833, and my claims as its inventor have been acknowledged by Professors Faraday, Daniell, and Wheatstone, in papers of theirs published in the Philosophical Transactions, yet as no description of it has yet been published, I will thank you to insert the following in the ensuing Number of the Philosophical Magazine. I think the figures and their explanation will be sufficiently intelligible to enable any workman to construct a similar one.

Fig. 1. is a side view of the magneto-electrical machine; the magnet is placed in a horizontal position, and consists of twelve plates of the horse-shoe shape firmly fastened together. A vertical wheel communicates motion to a spindle, which carries round with it a cross of soft iron, on the extremities of which are fixed four soft iron cylinders. Fig. 2 represents the spindle and cross before the wire is coiled round the cylinders: when the wheel is turned, the bases of each of the cylinders pass in succession the two poles of the magnet as closely as possible without actual contact. Fig. 3 represents the side of the armature next to the poles of the magnet; A and B are the soft iron cylinders on which the long wire for giving the shock is coiled; and C D are the cylinders round which the short wires for giving the spark are coiled: the circular brass plates 1, 2, 3, 4 are for the purpose of keeping the wire on the cylinders. The wires are of copper covered with silk; that for producing the shock is a double wire 400 yards in length, and each $\frac{1}{60}$th of an inch in diameter; and that for obtaining the spark consists of 20 lengths, each 75 feet long and $\frac{1}{50}$th of an inch in thickness, united together at their two extremities. Fig. 4 is a *front* view of the armature, showing the soft iron cross to which the cylinders carrying the

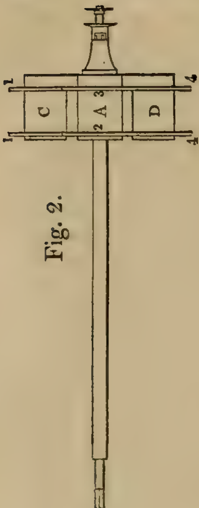

Fig. 2.

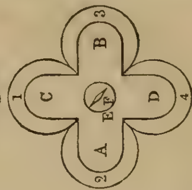

Fig. 4.

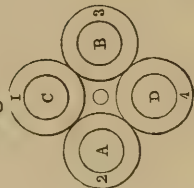

Fig. 3.

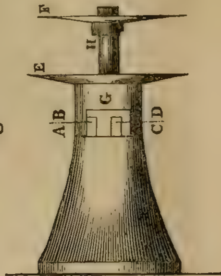

Fig. 5.

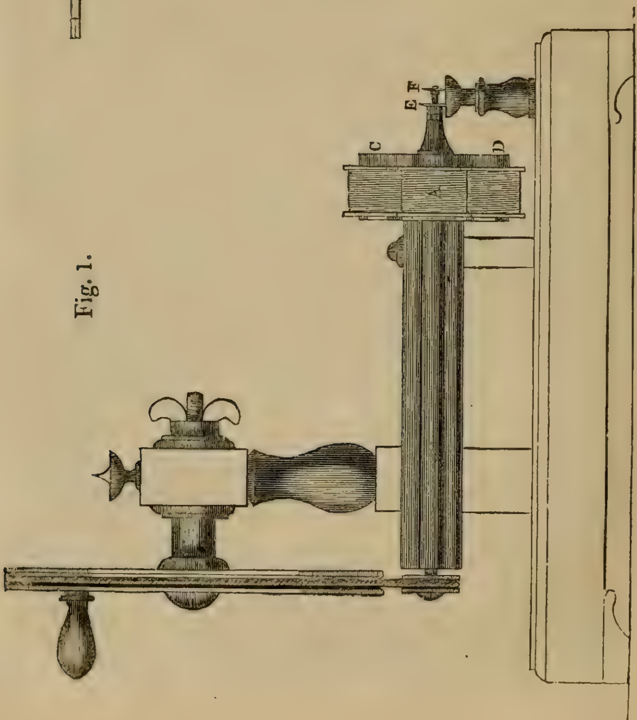

Fig. 1.

coils are fixed. The front end of the spindle is, for the purpose of insulation, made of ivory or hard wood, and the lancet-shaped blades F are mounted on a copper wire, which passes through the centre of the spindle, and to which one end of each coil of wire is soldered. E is the copper disc which always remains in contact with the mercury in the cup below, and is by a simple contrivance brought into contact with either of the other ends of the two coils. This contrivance is shown in fig. 5, which is an enlarged and side view of the front end of the spindle: at G, in the socket of the copper wheel, is a notch, in which terminate one end of each of the wires A B and C D; one side of the notch is represented in contact with A B, or the long wire for giving the shock, but by twisting the socket partly round the other side of the notch it will be brought in contact with C D, or the short wires for showing the brilliant spark, and producing the strongest heating effect. The points F in fig. 4, are in the proper position to take the spark from the coils C D, provided the socket of the disc is in contact with C D. To obtain the spark from the coil A B, which is however far less bright than the former, the notch must be brought into contact with A B, and the points twisted round one quarter of a revolution, or to that position that they will leave the surface of the mercury at the instant when the coils from which the spark is to be taken arrive at their greatest distance from the poles of the magnet.

For obtaining the shock, igniting wires, decomposing water, &c., the points should be removed, and the two ends of the wire forming the circuit should be connected, one with the mercury in the cup, and the other with the termination of the wire which passes through the insulating end of the spindle.

The action of the machine will be more readily understood by confining the attention to a single circuit; for this purpose we must suppose two of the cylinders (those opposite each other) with their coils of wire to be removed. Each of the soft iron cylinders becomes, from the known laws of induction, a temporary magnet when it is opposite one of the poles of the permanent magnet: as each cylinder passes successively both poles of the magnet, its poles are changed twice during each revolution, and the cylinders cease to be magnetic when they are at equal distances between the two poles. Electric currents are induced in the coils round each cylinder, and on account of the alternate change of the poles, these currents are alternately in opposite directions. The part of the coil round one cylinder being, as above described, connected with the copper disc, and that round the other cylinder connected with the dipping points, so that the current in both parts of the coil is continuous in the same direction, it is obvious that by the rotation of the spindle the circuit is alternately broken and renewed, and a spark occurs every time either of the copper points leaves the surface of the mercury, into which the copper disc also dips, thus completing the metallic communication twice during the revolution of the spindle. In the arrangement here described the successive transient currents are in opposite directions; to obtain a series of currents in the same direction, the double must be replaced by a single point, but in this case one half of the effect is lost.

The first electro-magnetic machine, that is, an instrument by which a continuous and rapid succession of sparks could be obtained from a magnet, was invented by M. Hypolite Pixii of Paris, and was first made public at the meeting of the *Académie des Sciences* on September 3, 1832. A description of this invention will be found in the *Annales de Chimie* for July 1832 (that journal is always published several months after its date), and a representation of it in Becquerel's *Traité de l'Electricité*, vol. iii. It differs from mine principally in two respects: first, in M. Pixii's instrument the magnet itself revolves and not the armature; and secondly, the interruptions, instead of being produced by the revolution of points, were made by bringing one of the ends of the wire over a cup of mercury, and depending on the jerks given to the instrument by its rotation for making and breaking the contact with the mercury. With this machine, furnished with a coil about 3000 feet in length, sparks and strong shocks were obtained, a gold-leaf electrometer was made to diverge, a Leyden jar was weakly charged, and water was decomposed.

My first complete magneto-electrical machine was exhibited at the meeting of the British Association at Cambridge in June 1833, and that now in the Gallery of Practical Science in Adelaide-street was placed there in August of the same year. The effects first shown by my machine were the following: 1st, the ignition and fusion of platina wire; 2ndly, the excitation of an electro-magnet of soft iron (these were first shown August 25th, 1833); and 3rdly, those of the double armature, producing at pleasure, either the most brilliant sparks and strongest heating power, or the most violent shocks and effective chemical decompositions; this was added to the instrument in December 1835.

I was led to furnish my magnet with the double armature from the following circumstances. In November 1833, Count di Predevalli brought from Paris one of Mr. Pixii's machines, and it was sent to the Adelaide-street Gallery in order that its effects might be compared with those of mine *. Mine was found to excel in the brilliancy of the spark, while M. Pixii's machine was more effective in giving the shock and affecting the electrometer. M. Pixii's machine had a larger keeper and a much greater extent of copper wire. Shortly after, Mr. Newman of Regent-street made a smaller instrument on my construction, which gave the shock more powerfully than my large one did: this also had a greater length of coil, but the effect was at that time partly attributed to the better insulation of the wire. I then convinced myself by some experiments that the increased shock solely depended on the length of the wire. The cause of the difference of effect in the two cases admitted no longer of dispute after the publication of the experiments of Dr. Henry of Philadelphia, Mr. Jennings, and Dr. Faraday; as their investigations fully proved that the spark is best obtained from a magneto-electric coil when short, and the shock when it is long. Mr. Clarke has no more claim to the application of the double armature to the magnet than he has to the discovery of the facts which suggested that application.

In conclusion: I think it will be evident from the preceding statement, that the magneto-electrical machine which Mr. Clarke has brought forward, "after much anxious thought, labour, and expense," is a piracy of mine; the piracy consisting not in manufacturing the instrument,—for every one is at full liberty to do so, but in calling it an invention of his own and suppressing all mention of my name as connected with it. I do not presume that Mr. Clarke is so ignorant as not to know the meaning of the word "invention," but he has strangely misapplied it by calling several other well-known pieces of apparatus his inventions. Thus he has appropriated to himself Ampère's *Bascule Electrique*, and calls it the Electrepeter. Among other uses of this simple contrivance of the French philosopher, it was employed in Pixii's magnet for the purpose of changing the direction of the current in the wire.

24, Sussex-street, London University. JOSEPH SAXTON.

* See the Literary Gazette, No. 878.

LXXI. *Reply to* Dr. Boase's "*Remarks on* Mr. Hopkins's '*Researches in Physical Geology*'," *in the Number for July. By* W. Hopkins, *Esq., M.A., F.G.S., of St. Peter's College, Cambridge.*

[Continued from p. 175, and concluded.]

THE two theories of the formation of veins of which I have spoken, equally depend on some process of infiltration or segregation into previously existing fissures, and differ only in the manner in which those fissures are supposed to have been formed, in the one case by dislocation, in the other by the mass becoming jointed. There is no reason why both should not be true as applied not merely to veins of different districts, but also to different veins of the same district, and both will enable us to account for nearly all the phænomena referrible to mechanical causes which veins present to us. Let us now proceed to analyse the rival theory of the contemporaneous formation of veins, of which Dr. Boase has been one of the ablest advocates.

In the first place, then, What are the physical causes which this theory assigns for the observed phænomena? We cannot of course do better than answer this question by a quotation from Dr. Boase's "Primary Geology *." "Is it not within the bounds of probability that the chemical union of the elements of the fused mass (of the earth's crust) whence resulted such a vast body of definite minerals, should be accompanied by the evolution of numerous currents of electricity, or of analogous fluids? for we know that the oscillations of the particles of matter, whether produced mechanically or during chemical combinations, will elicit streams both of common and galvanic electricity. If, then, it be acceded that the primary rocks may have been traversed by such currents during their formation, we have an explanation of the regular disposition of the granitic rocks, of veins, and other crystalline substances; and indeed not only of the subordinate parts but of the entire mass.

"This idea will remind the reader of Mr. R. W. Fox's experiments, from which he has concluded that the Cornish metalliferous veins were formed by electro-magnetism. By such imaginary currents, crossing each other in different directions, we also fancy that the phænomena of intersecting veins might be accounted for, the more powerful ones having uninterruptedly continued their course, whilst the weaker ones ex-

* p. 385.

perienced various degrees of diversion, being either partially or altogether involved in the impetus of their stronger opponents."

Now it appears to me that all we can conclude from the above reasoning (and I am not aware that it has been put by any one in a better form) is this,—that *it is not impossible* that veins may have been the effect of certain electric currents which, *it is possible*, may have existed. The theory rests, not on our knowledge, but entirely on our ignorance. We know not that these electric currents could be produced as above supposed, and we know not whether if they did exist they could produce the effects assigned to them. Let any one consider whether by any reasoning like the above he could give any rational account of such phænomena as the following:

1. The approximate rectilinearity and parallelism of veins.
2. The relations which their directions usually bear in stratified masses to the dip and strike of the beds.
3. Their division into two principal systems, approximately perpendicular to each other.
4. The irregularity of the *cross courses* in width, as compared with the bearing veins.
5. The *throw* of a vein, or the difference of level of the same stratified bed in the opposite walls of the vein.
6. The general relation between the *throw* and the *hade*, or inclination of the vein to the vertical.
7. The numerous appearances of *heaves* and *shifts* in veins.

These are some of the most obvious and general characters which mineral veins present to us; and yet I am not aware that the advocates of contemporaneous formation have made even an attempt (for as such we cannot regard the second paragraph of the above quotation) to account for one of these phænomena as a necessary or probable consequence of any definite physical cause connected with their theory, while all of them are, I conceive, perfectly accounted for on the hypothesis of an elevatory force, considered either as the original cause of fissures, or as modifying them when previously produced by joints. In the present imperfect state of our knowledge of geological causation, I would not positively reject any hypothesis carrying with it the most remote plausibility, provided it could be received without giving up others of stronger claims to our notice; and therefore I would not absolutely reject this hypothesis of contemporaneous formation as possibly applicable to certain veins, though I must still regard the process as an inconceivable one; but that we should adopt it with reference at least to the veins of our limestone districts, while it offers no rational explanation of a single phænomenon they present to us, can scarcely be expected, I conceive, even by its warmest advocates. I wish Dr. Boase had stated more distinctly the extent to which he conceived it applicable. Perhaps he will leave me quiet possession of the limestone districts and entrench himself within the Cornish veins, not allowing that the phænomena above mentioned are to be distinctly recognised in them. If, however, we consult for a moment the map published by Mr. Thomas of the Camborne and Chacewater district, and that by Mr. Carne of the district of St. Just, we recognise immediately a system of veins and of cross-courses such as has been mentioned above. The irregular width also of the cross-courses is universally recognised; and though I am not disposed to place implicit reliance on all that has been advanced respecting the heaves and shifts of Cornish veins, I should regard any one as a bold theorist who, for the sake of his theory, would set aside some of the facts of this kind which have been adduced. Whether the other phænomena above stated under the heads (2.), (5.), and (6.) exist in many Cornish veins it is impossible to say, because evidence of them can only be obtained where veins occur in masses so stratified as to enable us to identify some particular stratum on opposite sides of the vein. But leaving these out of the question, does the theory of contemporaneous formation offer the smallest explanation of the other phænomena?

It appears to me difficult to conceive a theory in a more perfectly unsatisfactory state than the one of which I am speaking; and, indeed, I scarcely understand what claim that which affords no intelligible explanation of anything can have to the title of *theory*. It seems mere hypothesis, without any direct support from physical facts or physical reasoning,—a negative of other theories rather than a theory itself; and the only foundation on which it appears to me to rest is the assumed insufficiency of more definite theories to account for some of the appearances presented by certain veins. Whether the two theories I have discussed will hereafter be deemed sufficient by geologists when careful observation shall have been made *with direct reference to them*, I pretend not to say, but I am quite certain that the theory of contemporaneous formation must rest on foundations very different from those on which it now stands before it can be admitted as affording any explanation of the formation of veins in general.

One of Dr. Boase's objections is founded on the want of sufficient coincidence between the Cornish veins and joints. At the same time it will be observed that he fully allows as a fact, established by his own observation, the general coincidence of their directions; and this general coincidence, which seems equally to have struck Dr. Boase and myself, is probably all that the theory of the formation of veins in joints would require. If we could descend into any master joint, and follow its course, we should no doubt find it frequently intersected by other joints, some of them belonging to other cross-systems, and others more partial and irregular. The continuity, however, of a cross-joint through the matter of the vein itself I should think a rare occurrence in our limestone districts, and I confess that I should be much surprised to find such to be anything like a general rule in the Cornish lodes. That such cases might occur, and not very unfrequently, appears very possible, because it is probable, I think, that the process of the formation of joints was one of long duration, and might be continued after the segregation of many of the Cornish lodes into the earlier joints; and more particularly, perhaps, might this be expected in those cases in which the causes producing joints have acted with the greatest intensity, which appears to have been in the older rocks. It must be understood too that I here speak of the absolute continuity of a particular *joint*, not of the *general directions of cleavage* of the mass. It would appear by no means improbable that the matter deposited in an open fissure should become subject to the same kind of action which might have previously produced a laminated structure in the surrounding mass. This presents, I conceive, no difficulty, because it does not necessarily lead to the inference that this laminated structure in the vein must have been superinduced contemporaneously with that in the containing mass, an inference which might be drawn from the continuity of distinct joints through the lode itself.

With respect to what Dr. Boase appears to consider a *reductio ad absurdum*, it is manifestly unnecessary to say anything in direct reply, except what I have before stated, that it is totally inadmissible to assume the earth's crust to have become jointed before the action of the dislocating force upon it.

In what I have now advanced respecting these theories of the formation of veins, and of their relative claims to our acceptance, I would beg to be understood as speaking with reference to existing evidence. With respect to the limestone districts I can feel little doubt of the result of more extended investigation. How far the two methods of formation will be found ultimately satisfactory as applied to the whole of the Cornish veins, I pretend not to say with equal certainty. It is not that I feel the geology of Cornwall, as your correspondent has stated, a stumbling-block,—because, even if I were ultimately to adopt the theory of contemporaneous formation with respect to many of the Cornish veins, it would not change my opinion respecting the origin of those of our limestone districts,—but it is that the evidence obtained from the Cornish veins is essentially defective in a most important point, viz. as to the relative elevations of the beds on opposite sides of the vein, which can only be determined in distinctly stratified masses. It is this serious defect which, in a great measure, renders it so much more difficult to arrive at any positive conclusion respecting them than in other districts, and which ought to satisfy us that it is not in Cornwall that we must expect to find the tests of the truth of a theory which would attribute the phænomena of veins to the dislocations of the mass in which they exist. I trust, however, that important evidence may shortly be expected to be placed before us. From the ability and extensive knowledge of Mr. De la Beche we may hope to see new light thrown on Cornish geology; the researches of Mr. Henwood in the mines of that district are likely to abound with important facts; and we may perhaps be allowed to hope that Dr. Boase, who has the merit of being among the first to declare the importance of attending to the regular structure of rocks, will repeat his observations with more immediate reference to theories in which, however our present views may seem to differ, I feel happy to have interested him. Whatever may be the difference of our opinions, however, in speculative geology, I would express the hope which he has himself well expressed at the conclusion of his "Primary Geology"—"that as fellow-labourers in one common cause we shall be actuated by a mutual esteem, and only strive, in friendly competition, who can render the best iuterpretation of the great and glorious mysteries of Nature."

I am, Gentlemen, yours, &c.

Cambridge, July 20, 1836. W. HOPKINS.

LXXII. *Addendum to Article LXV. in the present Number, by Professor* YOUNG.

THE *inverse* problem, or that which determines the orbit from knowing the law of attractive force, may be solved just as readily as the direct problem by regarding

$$R = N \frac{r^2}{P^2},$$

or, which is the same thing,

$$R = p\frac{r^3}{P^3}, \;\ldots\ldots\ldots\; (3.)$$

as the general equation of the conic sections.

For since the force in the radius vector varies as $\frac{1}{r^2}$, its component in the direction of the normal must vary as $\frac{1}{r^2}\cdot\frac{P}{r}$. But the normal force is

$$\frac{v^2}{R} = \frac{C}{P^2 R} \therefore \frac{P^3}{r^3} R = C';$$

and consequently

$$R = C'\frac{r^3}{P^3}, \;\ldots\ldots\; (4.)$$

which equation, as it agrees with (3.), represents a conic section, whose parameter, 2 C′, is determinable from the initial circumstances of the motion.

LXXIII. *Reviews, and Notices respecting New Books.*

The Botanist; containing accurately Coloured Figures of tender and hardy Ornamental Plants, with Descriptions. Conducted by B. Maund, F.L.S., assisted by Professor Henslow. 4to. To be continued Monthly. No. I.

In the progress of science there occur periods when the establishment of works devoted to any of its branches, upon new plans, becomes absolutely necessary, in order to promote its advancement, by being conformable to the improvements already achieved. Former works cannot so easily be moulded to the changing conditions of modern science as new ones can be accommodated to its state at the time of commencing their career. Hence, in the science to which the work refers, of which we have above transcribed the title, every succeeding work undertaken by competent persons has been an improvement upon its precursors. Most of those in existence up to this time have been suited or addressed to those only who were already conversant with its language and classification. But an attempt is here made to render an illustrated work suitable to those who have mastered its elements, and conducive to the acquisition of these by beginners. The terms used in the descriptions of plants are in general unintelligible to the uninitiated, and therefore little more is learnt by reading the description than by looking at the plate. But in the work just started an explanation of every botanical term is given, by means of a glossary, from the pen of Professor Henslow, a portion of which, till it be completed, will accompany each number, and to which reference can be made in every case of doubt. This is an advantage which belongs to no previous work, and must greatly enhance the usefulness of The Botanist. The plates, both in the matter of engraving and colouring, speak for themselves: they are "beautiful exceedingly!" The selection and treatment of the subjects figured reflect credit upon the conductor and his assistants, and if they avail themselves in the future numbers as judiciously of the varied resources at their command, the result will be the production of a volume, or, as we hope, many volumes, calculated to delight and instruct all who may open them, of whatever age or sex. * * *

M. Mirbel's *Report on a Memoir of* M. Gaudichaud, *relative to the Development and Growth of the Stems, Leaves, and other Organs of Plants, read in the Academy of Sciences at the sitting of the* 21*st December* 1835.*

When we have collected a great number of facts, when we have viewed them on every side, and have compared them with one another, observing with care their resemblances and differences, we feel ourselves stimulated by the desire to seek out the laws of their existence, to generalize those which are susceptible of it, and to form them into a theory. Without doubt prudence would often lead us to keep to the simple exposition of facts, but we cannot deny that it is very useful for science, that those who have discovered them should apply themselves to show us their connexion and dependence. Exact observations are never slow in obtaining the assent of all; theories, on the contrary, are subject to be for a long time contested. In this conflict of different opinions, the opposing parties bring forward all the known facts, put them to the test of a more rigorous examination, and discover others which had escaped preceding researches. Now, numerous and well-observed facts are what essentially constitute the unchangeable foundations of science. Thus, whatever be the issue of the struggle, there is a victory in favour of the human mind, and both the victors and the vanquished have often equal claims to public esteem.

These reflections are suggested to us by the perusal of the work which M. Gaudichaud has addressed to the Academy,—a work which, on the one part, is composed of a multitude of new facts, of acute observations, and inductions as just as they are evident; and, on the other, presents a general theory which rests upon that of Du Petit-Thouars, and considerably enlarges its basis. The material facts are certain, the theory which generalizes and professes to explain them, is still in doubt. De la Hire conceived it without supporting it by proofs: Du Petit-Thouars, by bringing together all the observations that seemed to him calculated to support it, gave it a scientific existence; Agardh applied himself to reconcile it with the re-

* From the *Annales des Sciences Naturelles*, tome v. p. 24.—The prize for experimental physiology founded by M. Montyon for the year 1835 was divided between this memoir and that of M. Poiseuille upon the causes of the motion of the blood in the capillary vessels.

ceived opinions; and, quite recently, Lindley, an excellent observer and a man of sound solid judgement, has strengthened it with all the weight of his approbation. But we must allow that it reckons as yet at least as many adversaries as partisans. M. Gaudichaud arms himself to defend it with the arguments which his own discoveries supply. It is by the help of time only, and after a very serious examination, that we shall be entitled to pronounce on the validity of consequences deduced from facts too recently known for us to be able as yet to measure their just bearing. We shall therefore confine ourselves to stating briefly the theory unfolded by the author, without venturing to approve or condemn it; but we shall not hesitate to give our opinion as to the accuracy of the numerous facts which he has brought together.

The task which M. Gaudichaud has undertaken is no light one. He reviews in the following order the whole history of vegetable life:

1. Organography, or development and growth of the stems, &c.;
2. Physiology, or phænomena of the life of plants;
3. Organogeny, or anatomical study of the development of vegetable tissues.

Organography, which forms the subject of the first part, subdivides itself into three chapters: 1. the dicotyledoneæ, 2. the monocotyledoneæ, 3. the acotyledoneæ.

The author delivers at the present time, for the judgement of the Academy, the two first chapters of this great undertaking, the precious materials for which are deposited in the botanical galleries of the Jardin du Roi, where they are become an object of study and admiration for connoisseurs.

He sets forth the general principles by which he means to explain not only the mode of development and the organization of stems, but also the mode of development and the organization of the *processiles* or *appendicular parts*, that is to say, of scales, leaves, stipulæ, bracteæ, calyxes, corollas, stamens, pistils, &c. which all take their birth in the bud. These parts are only, according to his idea, modifications of a single primitive organ of which the monocotyledonous embryo is the type.

In fact, in the same way that we see in the monocotyledonous embryo, when it has taken all its normal expansion, a radicular mamilla which constitutes its *descending system*, and a cauliculus, a cotyledon, and its support, which form together its *ascending system*, in the same manner also we see in the more advanced plant the root which represents the radicle, that is to say, the *descending system*, and the merithallus with the leaf and its petiole, which represent the cauliculus, the cotyledon, together with its support, that is to say, the *ascending system*.

This ascending system, modified in the other appendicular parts, is not, however, so modified as that there is found in it no trace of its distinctive features.

The simple type which represents the monocotyledonous embryo becomes double, triple, quadruple, quintuple, &c., in the dicotyledonous or polycotyledonous embryo, and the same also is the case with the vascular provision which it contains. We cannot be silent on the merit of this sketch ; it is of an accuracy which is rigorously demonstrated by the anatomy of the young plant.

The vascular provision is composed of two sets of vessels : the one is carried from the neck of the root to the bud ; the other from the bud to the extremity of the root. The first raises as far as the bud the raw sap which is there elaborated ; the second conducts as far as the root a part of the elaborated sap. This, in the dicotyledoneæ, being carried along between the bark and the wood, forms the new woody layers by its union with the utricles originating from the stem, and contributes in this manner to the growth in diameter ; whilst the other, by extending itself forwards at the centre, and terminating at the bud which transforms into organized matter a part of the sap come from the root, carries on the longitudinal growth. It thence follows that the bud receives from below nothing solid, nothing organized, that it creates altogether the vessels which enter into its composition, and that it is these same vessels, developed below, which are represented in the woody layers of the stem and of the root, of which they constitute the most important portion. And as to the utricles of the layers, whether they are carried on from beneath upwards, or from the centre to the circumference, they become organized on the spot between the bark and the wood, and have nothing in common with the bud.

This series of phænomena, which takes place in the natural state of individuals, exists equally in those which are grafted. All the wood of the stem and of the root placed below the graft is composed of vessels emanating from the buds of the graft and of utricles engendered by the subject. This proposition is the cornerstone of the theory ; for if it were invalidated by observation, the theory would fall to pieces.

The double vascular apparatus and the phænomena which result from its presence, belong not to the dicotyledoneæ alone ; they are also found in the monocotyledoneæ ; but they there undergo the modifications required by the particular arrangement of the threads of which the wood is composed.

Such is in substance the doctrine which M. Gaudichaud professes. If we consider attentively, it is, as we have already remarked, only that of Du Petit-Thouars and of Lindley ; but M. Gaudichaud has impressed upon it a character of generality which it had not. To come to this result, he has brought together a multitude of facts which, in whatever way they are interpreted, will conduce powerfully to the progress of science. His opponents it must be expected, will not fail to say that these facts, curious and unexpected as they may be, might be explained quite as well by their doctrine as by his. But notwithstanding this assertion, which should not be received on the strength of a simple dictum, as coming from persons who for a long time have formed another idea of the phænomenon of the growth of plants, all will agree, that by his new work, M. Gaudichaud has raised himself as high as our most skilful phytologists. It is worthy of remark that, during the fatigues of two long voyages, in spite of the wretched state of his health, this indefatigable naturalist never ceased to apply himself to researches of extreme delicacy, and that he has carried them as far as he would have done in the quiet of his closet. Here we can only name the least part of his most interesting observations.

He has examined, drawn and described a multitude of seeds and embryos belonging to families still little known, such as the *Nympheaceæ*, the *Piperaceæ*, the *Gnetaceæ*, the *Cycadeæ*. This last family supplied him, during his first voyage sixteen or seventeen years ago, with a succession of ovological facts of which some are still new, notwithstanding the recent investigations of MM. Corda and Robert Brown. He caused to germinate in their native climate, seeds of *Piper*, *Peperomia*, *Loranthus*, *Avicennia*, *Bruguiera*, *Rhizophora*, &c.; and he now gives us positive notions respecting the first developments of these plants, which will take the place of vague or erroneous opinions in science. At the same time that he was bringing together numerous herbarium specimens, he studied the interior of stems, and found in the structure and arrangement of the ligneous body, strange anomalies which would little have been suspected there. It was particularly these observations which suggested to him the project of bringing together all the facts of development and growth under general laws, a project the execution of which he has constantly followed up since his return to France.

In order that every one may be able to verify the facts, he has chosen many examples amongst our commonest plants, and these have often furnished him with new views: we shall point out among others the radish, the turnip, the carrot, the beet, the horse chestnut. From the better known organization of these different vegetable productions he has been able to derive arguments in favour of his opinions. Some have also been furnished him by the phænomena which the processes of barking, cuttings, grafts, lopping, and other operations of culture present. There is not, so to speak, a single important fact of vegetation which he has not tried to bring under the rule of his doctrine; and his efforts, even when in certain cases some persons may have thought his conclusions too hasty, have never been unproductive.

Explanations concerning each fact would carry us far. Let us dwell only upon three points, which amongst so many other remarkable ones, merit more particularly to engage the attention of the Academy.

At the base of a cauline bud of *Dracæna* stript of its herbaceous envelope by maceration, there appears, if we may so express it, a kind of paw, a continuation of the superior ligneous fibres, which is fastened on the ligneous body of the stem, and elongates itself into threadlike fingers, numerous and divergent. These fingers are evidently minute vascular fasciculi. Would they have descended to the roots if the vegetation had not been stopt? This is very probable.

The bud of a slip of *Cissus hydrophora*, stript of its bark, presents at its base a ligneous network which partially clothes the inferior portion of the old wood, and escapes on every side in root.

These two examples taken, the one in the monocotyledoneæ, the other in the dicotyledoneæ, appear at first sight undeniable proofs of the solidity of the doctrine of M. Gaudichaud; yet, notwithstanding, several phytologists, whilst they accept the facts, reject the theory. This is because the question is not so simple as it appears. It is certain that it will not cease to be a subject of controversy until there shall be an agreement as to the physiological results of the process of grafting.

The third point touches the scientific reputation of an excellent man, who sat here during more than forty years, and whose memory will be ever dear to us. Every one knows the work of M. Desfontaines on the stems of palms. A German phytologist, M. Hugo Mohl, treating of the same subject with more numerous and more varied materials, and all the resources of the science such as fifty years' progress has made it, advanced, a short time ago, that the numerous ligneous fibres were not formed at the centre, but at the circumference; and that it was in crossing the older fibres obliquely that they reached as far as the heart of the tree. From this fact he concluded that M. Desfontaines had deceived himself; yet it is not so, although the observations of M. Mohl are perfectly accurate. The researches of M. Gaudichaud show that M. Desfontaines has very well observed and described what he saw, and that M. Mohl, far from having overturned the work of this learned man, has rendered it more unassailable by completing it.

The considerations set forth in this Report sufficiently make known the motives which have determined the commission to divide the prize between M. Gaudichaud and one of his competitors, M. Poiseuille, whose admirable works on the motion of the blood, render him for the third time worthy of a distinguished testimony of the esteem of the Academy.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

(Continued from vol. viii. p. 553.)

1836. April 21.—A PAPER was read, entitled "Additional Observations on Voltaic Combinations." In a letter addressed to Michael Faraday, Esq., D.C.L., F.R.S. Fullerian Professor of Chemistry in the Royal Institution, &c. By J. Frederick Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London.

The author has found that the constant battery, of which he described the construction in a former communication to the Royal Society, might be rendered not only perfectly steady in its action, but also very powerful; as well as extremely efficacious and convenient for all the purposes to which the common voltaic battery is usually applied. With this view he places the cells which form the battery in two parallel rows, consisting of ten cells in each row, on a long table, with their siphon-tubes arranged opposite to each other, and hanging over a small gutter, placed between the rows, in order to carry off the refuse solution when it is necessary to change the acid. Having observed that the uniformity of action may be completely maintained by the occasional addition of a small quantity of acid, he is able to dispense with the cumbrous addition of the dripping funnel; an arrangement which admits with facility of any combination of the plates which may be desired.

April 28.—On certain parts of the Theory of Railways; with an investigation of the formulæ necessary for the determination of the resistances to the motion of carriages upon them, and of the power necessary to work them. By the Rev. Dionysius Lardner, LL.D., F.R.S.

The author observes, in his prefatory remarks, that an extensive and interesting field of mathematical investigation has been recently opened in the mechanical circumstances relative to the motion of heavy bodies on railways; and having collected a body of experiments and observations sufficient to form the basis of a theory, he purposes, in the present paper, to lay before the Society a series of mathematical formulæ, embodying the most general expressions for the phenomena of the motion of carriages on these roads.

The author begins by investigating the analytical formulæ for the traction of trains over a level line which is perfectly straight, and finds, first, the distance and time within which, with a given amount of tractive power, the requisite speed may be obtained at starting; and also the point where the tractive power must be suspended, previous to coming to rest. The excess of tractive power necessary to get up the requisite speed is shown to be equal to the saving of tractive power previous to a stoppage; and formulæ are given for the determination of the time lost under any given conditions at each stop.

The motion of trains in ascending inclined planes which are straight, is next considered; and formulæ are given combining the effects of friction and gravity, in opposition to the tractive force. The circumstances which affect every change of speed, and the excess of tractive force necessary, in such cases, to maintain the requisite speed, are determined; as well as the other circumstances already stated with respect to level planes.

The friction of trains upon descending planes is next investigated; and an important distinction is shown to exist between two classes of planes; viz., those whose acclivities are inferior to the angle of repose, and those of more steep acclivities. A remarkable relation is shown to exist between the tractive forces in ascending and descending the first class of planes. For descending planes of greater acclivity than the angle of repose, the use of breaks becomes essentially requisite. The effect of these contrivances is investigated, as well as the motion of trains on the accidental failure of breaks.

In any attempts which have been hitherto made to obtain the actual velocities acquired by trains of carriages or waggons under these circumstances, an error has been committed which invalidates the precision of the results; the carriages having been treated as sledges moving down an inclined plane. The author has here given the analytical formulæ by which the effect of the rotatory motion of the wheels may be brought into computation; this effect, depending obviously on the amount of inertia of the wheels, and on the proportion which their weight bears to the weight of the waggon.

The properties investigated in this first division of the paper, are strictly those which depend on the longitudinal section of the line, presumed to be straight in every part of its direction. There is, however, another class of important resistances which depend on the ground-plan of the road, and these the author next proceeds to determine.

The author then gives the analytical formulæ which express the resistance arising,—*first*, from the inequality of the spaces over which the wheels, fixed on the same axle, simultaneously move; *secondly*, from the effort of the flanges of the wheels to change the direction of the train; and *thirdly*, from the centrifugal force pressing the flange against the side of the rail. He also gives the formulæ necessary to determine, in each case, the actual amount of pressure produced by a given velocity and a given load, and investigates the extent to which these resistances may be modified by laying the outer rail of the curve higher than the inner. He assigns a formula for the determination of the height which must be given to the outer rail, in order to remove as far as possible all retardation from these causes; which formula is a function of the speed of the train, the radius of the curve, and the distance between the rails.

In the latter part of the paper, the author investigates the method of estimating the actual amount of mechanical power necessary to work a railway, the longitudinal section and ground-plan of which are given. In the course of this investigation he arrives at several conclusions, which, though unexpected, are such as necessarily arise out of the mechanical conditions of the inquiry. The first of these is, that all straight inclined planes of a less acclivity than the angle of repose, may be mechanically considered equivalent to a level, provided the tractive power is one which is capable of increasing and diminishing its energy, within given limits, without loss of effect. It appears, however, that this condition does not extend to planes of greater acclivities than the angle of repose; because the excess of power required in their ascent is greater than all the power that could be saved in their descent; unless the effect of accelerated motion in giving momentum to the train could properly be taken into account. In practice, however, this acceleration cannot be permitted; and the uniformity of the motion of the trains in descending such acclivities must be preserved by the operation of the break. Such planes are therefore, in practice, always attended with a direct loss of power.

In the investigation of the formulæ expressive of the actual amount of mechanical power absorbed in passing round a curve, it is found that this amount of power is altogether independent of the radius of the curve, and depends only on the value of the angle by which the direction of the line on the ground-plan is changed. This result, which was likewise unexpected, is nevertheless a sufficiently obvious consequence of the mechanical conditions of the question. If a given change of direction in the road be made by a curve of large radius, the length of the curve will be proportionably great; and although the intensity of the resistance to the tractive power, at any point of the curve, will be small in the same proportion as the radius is great, yet the space through which that resistance acts will be great in proportion to the radius: these two effects counteract each other; and the result is, that the total absorption of power is the same. On the other hand, if the turn be made by a curve of short radius, the curve itself will be proportionately short; but the intensity of the resistance will be proportionately great. In this case, a great resistance acts through a short space, and produces an absorption of power to the same extent as before.

In conclusion, the author arrives at one general and comprehensive formula for the actual amount of mechanical power necessary to work the line in both directions; involving terms expressive, *first*, of the ordinary friction of the road; *secondly*, of the effect of inclined planes, or *gradients*, as they have been latterly called; and, *thirdly*, of the effect of curves involving changes of direction of the road, the velocity of the transit, and the distance between the rails; but, for the reason already stated, not comprising the radii of the curves.

Although the radii of the curves do not form a constant element of the estimate of the mechanical power necessary to work the road, nevertheless they are of material consequence, as far as regards the safety of the transit. Although a short curve with a great resistance may be moved over with the same expenditure of mechanical power as a long curve with a long radius, yet, owing to the intensity of the pressure of the flange against the rail, the danger of the trains running off the road is increased: hence, although sharp curves cannot be objected to on the score of loss of power, they are yet highly objectionable on the score of danger.

In the present paper, the author has confined himself to the analytical formulæ expressing various mechanical effects of the most general kind; the coefficients and constants being expressed merely by algebraical symbols: but he states that he has made an extensive series of experiments within the last few years, and has also procured the results of experiments made by others, with a view to determine the mean values of the various constants in the formulæ investigated in this paper. He has also, with the same view, made numerous observations in the ordinary course of transit on railways; and he announces his intention of soon laying before the Society, in another paper, the details of these experiments, and the determination of the mean values of these various constants, without which the present investigation would be attended with little practical knowledge.

A paper was also read, entitled "Register of the State of the Barometer and Thermometer kept at Tunis, during the years 1829, 1830, 1831 and 1832." Presented by Sir Thomas Reade, His Majesty's Agent and Consul General at Tunis. Communicated by P. M. Roget, M.D., Sec. R.S.

The observations here registered are those of the thermometer at 9 A.M., at noon, and at 6 P.M., and the points of the wind, and height of the barometer for each day of the abovementioned years.

May 5.—A paper was in part read, entitled "On the Optical Phenomena of certain Crystals." By Henry Fox Talbot, Esq., F.R.S. See our last Number, p. 288.

May 12.—The reading of Mr. Talbot's paper was concluded.

May 19.—A paper was read, entitled, "On the valuation of the mechanical effect of Gradients on a line of Railroad." By Peter Barlow, Esq., F.R.S.

The exact amount of the influence of ascents and descents occurring in the line of a railway on the motion of a load drawn by a locomotive engine having been differently estimated by different persons, the author was induced to investigate the subject. A few observations are premised on the erroneous assumptions which, he conceives, have in general vitiated the results hitherto deduced. The first of these is that the expenditure of power requisite for motion is equal to the resistance to traction; whereas it must always greatly exceed it. No account, he remarks, has been taken of the pressure of the atmosphere on the piston, which the force of the steam has to overcome before it can be available as a moving power. Another source of error has been that the statical and dynamical effects of friction have been confounded together; whereas they are the same in amount only when the body is put in motion by gravity; but not when it is urged down an inclined plane by an extraneous force. In the latter case these effects are no longer comparable; friction being a force which, in an infinitely small time, is proportional to the velocity, while that of gravity is constant at all velocities; or, in other words, the retardation from friction is proportional to the space described, while that from gravity has reference only to the time of acting, whatever space the body may pass over in that time. It is an error to assume that the mechanical power of the plane is equivalent to a reduction of so much friction; for the friction down the inclined plane is the same as on a horizontal plane of the same length, rejecting the trifling difference of pressure; and the whole retardation in passing over the plane, or the whole force required to overcome it, is the same at all velocities, and by whatever force the motion is produced; but the assisting force from gravity is quite independent of the space or of the velocity.

In the investigations which the author has prosecuted in this paper, he assumes that equal quantities of steam are produced in the same time at all velocities; and he adopts for his other data, those given by Mr. Pambour in his Treatise of Locomotive Engines. He deduces a formula from which, the speed on a level being given, we may compute the relative and absolute times of a train ascending a plane; and consequently also the ratio of the forces expended in the two cases; or the length of an equivalent horizontal plane; that is, of one which will require the same time and power to be passed over by the locomotive engine as the ascending plane.

The next objects of inquiry relate to the descent of trains on an inclined plane, and comprise two cases: the first, that when the power of the engine is continued without abatement; and the second, that when the steam is wholly excluded, and the train is urged in its descent by gravity alone. The author arrives at the conclusions, that in the first of these cases, when the declivity is one in 139, the velocity, on becoming uniform, will be double that in a horizontal plane: and that for a declivity of one in 695, the uniform velocity of descent will be one fifth greater than on the horizontal plane; and this, he observes, is perhaps the greatest additional velocity which it would be prudent to admit. A plane of one in 695 is therefore the steepest declivity that ought to be descended with the steam-valve fully open; all planes with a declivity between this and that of one in 139 require to have the admission of steam regulated so as to modify the speed, and adjust it to considerations of safety; and lastly, all planes of a greater slope than this last require, in descending them, the application of the brake.

A paper was also read, entitled, "On the application of Glass as a substitute for metal balance-springs in Chronometers." By Messrs. Arnold and Dent. Communicated by Francis Beaufort, Esq., Captain R.N., F.R.S., Hydrographer to the Admiralty.

In their endeavours to determine and reduce the errors arising from the expansions of the balance-spring of chronometers consequent on variations of temperature, the authors came to the conclusion that there exist certain physical defects in the substances employed for its construction, beyond the most perfect mechanical form that can be given to it, which interfere with the regularity of its agency: so that however exquisite may be its workmanship, and however complete its power of maintaining a perfect figure when in different degrees of tension, yet the imperfect distribution of its component parts may give rise to great incorrectness in its performance. Hence the balance-spring not only should be made of a substance most highly elastic, but its elasticity should not be given to it by any mechanical or chemical process: as a body in motion, it should be the lightest possible; and, as far as the case admits of, it should be free from atmospheric influence. Glass suggested itself as the only material possessing, in the greatest degree, all these desirable properties. Its fragility, although apparently a great objection to its employment, was found, on trial, to constitute no obstacle whatever; for it was found to possess a greater elastic force than steel itself, and thus to admit of greater amplitude in the arc of vibration.

It was first proposed to ascertain how far a glass balance-spring would sustain low temperatures; and it was found by experiment that it resisted completely the effects of a cold as great as that of $+ 12°$ of Fahrenheit's thermometer; thus satisfactorily removing any objection which might be brought against its use from its supposed fragility in these low temperatures. The next object of solicitude was to determine whether it would withstand the shock arising from the discharge of cannon in the vicinity; and its power of resisting concussions of this nature was fully established by experiments made with this view on board H.M.S. Excellent at Portsmouth.

On comparing the performance of glass balance-springs with metallic ones, when the temperatures were raised from 32° to 100°, it was found that while the loss in twenty-four hours in the gold spring was $8^m\ 4^s$, that of steel $6^m\ 25^s$, and that of palladium $2^m\ 31^s$, that of a glass spring was only 40^s. These differences the authors ascribe principally to the different degrees in which the substances had their elasticity reduced by an increase of temperature. As glass was thus found to suffer a much smaller loss of elasticity by this cause than metals, they proceeded to construct a glass balance suited to the correction of the small error still occasioned by this cause, employing a glass disc for this purpose. The compensation being completed, they next tested the isochronism of the glass spring, and it proved to be as perfect as any metallic spring. Chronometers thus constructed are now in course of trial at the Royal Observatory. In common with all other instruments of the same kind they have shown a disposition to progressive acceleration, the cause of which is but little known, but which appears to be influenced by the action of the air.

GEOLOGICAL SOCIETY.

(Continued from vol. viii. p. 580.)

April 13.—A memoir on the Geology of Coalbrook Dale, by Joseph Prestwich, Jun., Esq., was commenced.

April 27.—The memoir on the Geology of Coalbrook Dale, by Joseph Prestwich, Jun., Esq., began on the 13th of April, was concluded.

In a paper read before the Society in February 1834, Mr. Prestwich gave an account of some of the principal faults of this coal-field, and in the present memoir describes fully the extent and physical features of the district, the formations of which it consists, the dislocations not previously noticed, the superficial detritus or drift, the organic remains, and the inferences which the author conceives may be drawn from the facts enumerated. In the first place, however, he acknowledges the assistance which he has received from Mr. Murchison, Mr. Anstice of Madeley, and the gentlemen connected with the coal-works; he also acknowledges the aid which he has derived from Mr. Arthur Aikin's labours in the same district.

The coal-field is bounded on the east by a slightly undulating line ranging from Lilleshall to Bridgenorth; on the north-west by a line nearly coincident with the main road from Lilleshall to Watling-street, near Wellington, and thence by the Wrekin; on the west the boundary is broken by the gorge of the Severn, but is formed, in part, by the elevated ridges of Benthall and Wenlock; and on the south-east it is defined by the road from Much Wenlock to Bridgenorth.

The area thus circumscribed consists of a platform raised about 400 feet above the Severn at Madeley, or 500 above the level of the sea; the surrounding country seldom rising to a height exceeding 350 feet. It is intersected by numerous picturesque glens, including the celebrated defile through which the Severn flows at the Iron Bridge, and is traversed by several low hills, the most elevated of which is about 746 feet above the level of the sea; but the Wrekin, which forms part of the north-western boundary, rises to the height of 1320 feet.

The formations of which the district consists are, commencing with the oldest, 1st, some members of the Lower Silurian rocks; 2ndly, the Wenlock and Ludlow rocks, belonging to the Upper Silurian system; 3rdly, the old red sandstone; 4thly, carboniferous limestone; 5thly, coal measures; 6thly, new red sandstone; and, 7thly, trap.

In describing the formations subjacent to the old red sandstone, Mr. Prestwich states that he owes his knowledge of their order of superposition entirely to the previous labours of Mr. Murchison, and that unassisted by them it would have been impossible for him to have determined correctly their relative antiquity.

1st. The lower Silurian rocks consist of quartzose grit succeeded by micaceous flags, which are overlaid by a coarse-grained sandstone alternating with beds of light grey clay. They occur on the flanks of the Wrekin and Arcol Hills.

2ndly. The Wenlock rocks are composed, in the lower part, of beds of shale, and in the upper of limestone abounding with organic remains. They form the escarpment of Wenlock and Benthall Edges, Lincoln Hill, &c. The Ludlow rocks consist of three divisions: the lowest being formed of grey-coloured, soft, calcareous sandstones and shales; the middle of very thin beds of light grey and brown limestone; and uppermost of sandstones. They are stated to occur at Much Wenlock and Wyke, also near Apley, in the Meadow-pits and in several other pits in Broseley parish; likewise between Dean and Willey, &c.

3rdly. The old red sandstone skirts the southern parts of the coal-field, and consists of beds of dark red marl alternating with dark, micaceous sandstones.

4thly. The carboniferous limestone appears on the south of Little Wenlock, at Steeraways and Lilleshall Hills, and presents thin beds of argillaceous limestone and shale.

5thly. The *coal measures* consist of the usual alternations of shale, sandstone, and coal, amounting at the Madeley Meadow pits to 135 beds, having an aggregate thickness of about 250 yards. The first 70 or 80 beds are light grey, yellow, or red; the succeeding 20 are nearly black, and the underlying are mostly light-coloured. These distinctions are general, but not universal. In the uppermost part of the series clays and soft calcareous sandstones predominate; in the middle argillaceous sandstones and indurated clay; while in the lowest part fine hard sandstones. The upper coal seams are thin, generally sulphurous, widely separated, and extremely irregular, but the lower are nearer together, and are persistent throughout the field. The average thickness of the seams is about 3 feet, and the number in different pits varies from 1 to 16. The following table contains the aggregate thickness of the seams and the number at each of the localities mentioned:

	Yards.	ft.	in.	Number of beds.
Hadley	15	0	0	16
Sned's Hill	14	2	2	12
Malmslee	11	0	10	13
Langley	11	2	6	11
Dawley	14	0	0	16
Lightmoor	13	2	0	17
Madeley	10	2	10	24
Broseley	7	0	9	13

In these pits the measures are fully developed, and consequently the variations may be explained by the thinner beds not having been equally included. In the Madeley list every thin seam is given.

The shales and sandstones vary greatly in their characters, though the former are said to be more uniform than the latter, and to contain layers of argillaceous carbonate of iron. A bed of freshwater limestone occurs in the upper part of the measures, at Inet, the Frog's Mill near Nordley, and at Tasley. It is very hard, has a fine conchoidal fracture, and varies in thickness from one to two yards. A minute account is then given of the changes presented at different pits, and it is shown that the thinning out of the strata of sandstone and shale is frequently of great advantage to the miner by bringing into contact, beds of coal, which would otherwise be separated many feet.

Carburetted hydrogen is disengaged in greater abundance from the lower than from the upper measures, and in greatest quantity on commencing a new work, especially on approaching a fault, when large masses of coal are constantly blown off the main beds with loud reports. Carbonic acid gas is rarely found in a pit at work, and Mr. Prestwich suggests that the quantity sometimes noticed may have been accumulated in adjacent old pits.

The mineral contents of the coal measures are confined to the argillaceous carbonate of iron, sulphuret of iron, sulphuret of zinc and petroleum. The celebrated spring, which once yielded more than a hogshead a day, produces now only a few gallons a week; but another abundant spring has been discovered, and considerable quantities of petroleum have been obtained in working the Dingle pit. Titanium is found in considerable quantities in the hearthstones of the old furnaces often beautifully crystallized, but in greatest abundance in a massive state. In analysing some crystals of sulphuret of zinc found in the coal measures the author detected titanic acid.

6thly. New red sandstone.—Only the lower divisions of this formation occur in the immediate neighbourhood of Coalbrook Dale, flanking the eastern and north-western sides of the field, and in some places abutting against the dislocated edge of the coal measures. They consist of clay, marl, and sandstones, overlaid by calcareous conglomerates, to which succeed coarse sandstone, marls, and other conglomerates. The lowest beds pass conformably into the coal measures, the line of distinction being chiefly distinguishable by the change in the colour; but some of the vegetable remains of the coal may be, though rarely, detected in the sandstone series. Mr. Prestwich is of opinion that there is a want of conformity between the lower and upper systems of the new red sandstone series.

7thly. Trap rocks.—The greater portion of the Wrekin, Arcol, Maddox, and Lilleshall Hills, &c. are composed of greenstone, felspar rocks, and amygdaloid. Smaller bosses also rise to the surface at various points within the coal-field, and others have been discovered in the deep workings; but it is worthy of remark that no trap has been noticed in any of the crevices or fissures connected with the faults. The trap does not appear to have charred the coal; but at New Hadley, at a point where a boss appears at the surface, the coal in its vicinity loses its cohesion and becomes sooty, and the same change was noticed elsewhere near dislocations, though no trap was visible.

Dislocations.—The author says that there is probably no coal-field of equal size in the kingdom so greatly shattered as that of Coalbrook Dale. The faults are most numerous and complicated where the measures are thinnest, the miner in those parts rarely proceeding 20 yards without interruption, and frequently not more than two or three; but when so close together the dislocations are small in effect and extent, and are connected with others of greater magnitude.

The larger faults tilt the strata in various directions, but have generally a parallelism of strike, and deviate but very slightly from a straight line. Sometimes the sides of the disjointed strata are in contact, when the edges of the beds of coal and shale have a shining striated surface, but at others the sides are separated several yards, the interval being filled with the debris of the strata. The inclination of the principal faults as well as of the minor, obeys no general law, and even in the same fault it occasionally varies from 45° to 90°. The difference of level on the opposite sides of the principal dislocations also varies considerably; thus the Lightmoor fault, at Malmslee and Old Park produces a difference of level of 600 or 700 feet, but at Sned's Hill of only 300, and a branch of it does not affect the strata more than 50 or 60 feet. In some instances the change of level is by steps or hitches, owing probably to unequal resistance, or a series of small dislocations. Another character of the large faults is their subdividing, more especially at the extremities—the subdivisions occasionally taking a direction at right angles to the main fault, but when they are numerous they diverge from it only a few degrees and extend but a short distance.

The author then describes minutely the chief faults; the two principal of which, bounding the field on the east and partly on the west, bring the disjointed edges of the coal measures in a level with those of the new red sandstone; and he afterwards gives a table of the minor faults, containing the name of each fault with its direction, extent, average angle of inclination, breadth, fall, the greatest difference of level produced by it, and the localities at which the difference of level varies; and from the phenomena presented by the faults, and the fact that the field is a platform raised above the level of the surrounding country, the author infers that the coal-field has been elevated above its original position; he also adds that the contortions of the beds are not of any great magnitude.

Superficial detritus or drift.—Thick beds of gravel and sand cover a large portion of the surface, and are considered by the author as consisting of two distinct deposits. The lower, which is of local occurrence, though from 20 to 50 feet thick, consists of fine-grained red sand, containing beds of small angular pebbles of the adjacent rocks, and thin, distinct seams of marl or clay. Imbedded in the sand are frequently found masses of coal, some of them six feet in diameter. The upper deposit is composed of rolled pebbles of rocks, composing the coal-field and its boundaries, imbedded in coarse reddish sand. Its distribution is more regular than that of the lower division; and it is distinguished by the abundance of fossils derived from the Dudley limestone and the coal measures, as well as the presence of marine shells of existing species.

Organic remains.—The fossils of the coal measures are described with great detail, as well as the phenomena of beds containing marine remains, alternating with others in which freshwater shells and land plants occur; and a comparison is made with the Ganister coal-field, in which similar alternations have been noticed. The following are the principal points detailed in the paper respecting these alternations at Coalbrook Dale. The lowest part of the coal measures presents numerous beds of sandstone and shale, with seams of good coal; some of the beds containing in abundance vegetable remains, occasionally associated with Unios. To these succeed the bed called the penny ironstone, in which has been found a few vegetable remains and casts of Unios and Cyclades, but great abundance of marine remains belonging to the genera Producta, Spirifer, Ammonites, Nautilus, Bellerophon, Conularia, Euomphalus, Pecten, Orbicula, Terebratula, Venus, Asaphus, and Pentacrinites; remains also of fishes, namely, the *Megalichthys Hibbertii* and *Gyracanthus formosus*. The next series of beds, consisting of the usual alternation of sandstone, shale, and coal, inclose vegetable remains and Unios. Upon these repose a stratum of micaceous shale, containing ironstone nodules in which have been found land plants, Unios in considerable quantities, remains of the Megalichthys and Gyracanthus, and Trilobites of a distinct genus. This singular stratum is surmounted by a series, of great thickness, of the usual coal measures, in which organic remains and land plants have been observed, and is succeeded, in two localities, by the Chance penny-stone, in which *Productus scabriculus* occurs in vast abundance. The uppermost beds of the series, consisting of many thick beds of sandstone with layers of shale and one seam of coal, are almost destitute of organic remains. The distribution of the fossils is extremely irregular in different parts of the coal-field, being most persistent in the lower beds; and though they are most commonly found in the ironstone nodules, yet they sometimes occur in the sandstones and shales adjacent to the coal seams.

In the concluding part the author reviews the facts detailed in the memoir, and draws the inferences which he conceives they warrant.

1st. Mr. Prestwich is of opinion that the alternations of freshwater shells with marine remains, do not prove as many relative changes of land and sea; but that the coal measures were deposited in an estuary, into which flowed a considerable river, subject to occasional freshes; and he conceives that this position is supported by the fact of frequent alternations of coarse sandstones and conglomerates with beds of clay or shale; and for the same reason he is of opinion that the vegetable remains did not grow where they are found.

2ndly. After recapitulating the evidence in support of the protrusion of Coalbrook Dale through once continuous overlying formations, he calls attention to the important inquiry whether there may not be buried beneath the new red sandstone districts other considerable coal-fields, which are unknown, because they have not been subject to disturbing agents similar to those which exposed the district under review.

Lastly. With respect to the agents which have modified the surface of Coalbrook Dale, the author is of opinion that it was denuded, in part, while beneath the level of the ocean; that the lower bed of detritus, containing angular gravel and large masses of coal, proves a sudden and short cataclysm; while the upper beds of rounded gravel, containing recent shells, indicate the long-continued action of a body of water, since the existence of the present Testacea of our coasts.

A letter from R. W. Fox, Esq., addressed to Sir Charles Lemon, Bart., M.P., F.G.S., "On the Formation of Mineral Veins," was then read.

Mr. Fox is of opinion that mineral veins were originally fissures probably caused by changes in the earth's temperature; that they were small at first and gradually increased in their dimensions; and that the mineral contents progressively accumulated during the whole period of the development of the fissures; and as changes in the earth's temperature might produce changes in the direction and intensity of the terrestrial magnetic curves, he conceives that electricity may have powerfully influenced the existing arrangement of the contents of these fissures.

Copper, tin, iron, and zinc, in combination with sulphuric and muriatic acids, being very soluble in water, Mr. Fox says, they would in this state be capable of conducting voltaic electricity; and as the rocks forming the walls of the veins contain different salts, they would be in opposite electrical conditions, and hence currents would be generated and readily transmitted through the fissures, and in time the metals would be separated from their solvents and deposited in the veins. But, on the known principles of electro-magnetism, Mr. Fox adds, it is evident that such currents would be more or less influenced by the magnetism of the earth; and therefore that they would not pass from north to south or from south to north as easily as from east to west, but more so than from west to east.

The author then offers some observations relative to the production of sulphurets from the decomposition of the metallic sulphates; and explains how fissures, gradually widening, would be successively filled, and would account for veins occurring within veins; he offers some remarks also on the greater productiveness exhibited at the points where veins pass from one formation to another, and is of opinion that the fact may be explained by supposing the rock in which the vein is productive to have been electro-negative.

In conclusion Mr. Fox states, that if in other parts of the world veins may be found to deviate from an east and west direction much more than they do in England, the apparent discrepancy may be explained by the rocks having yielded more easily in one direction than in another, and from a difference in the direction of the magnetic meridian in different countries, as well as from the probability that it has varied greatly at different epochs.

ZOOLOGICAL SOCIETY.

April 12.—Mr. Bennett directed the attention of the Meeting to a living specimen of the *brush-tailed Kangaroo, Macropus penicillatus*, Gray, which had recently been added to the Menagerie; having been presented to the Society by Captain Deloitte, Corr. Memb. Z. S. He remarked particularly on the peculiarity of its actions, as compared with those of the typical *Kangaroos*; and especially on the ease with which it vaults from the ground to any slight ledge, on which it remains perched, as it were, with its tail extended behind it: the tail, in fact, appearing to be in no respect aiding in the progression of the animal.

Referring to some observations which he had made on the exhibition of a skin of the same species, at the Meeting of the Society on January 13, 1835, (Lond. and Edinb. Phil. Mag. vol. vii. p. 67,) he stated it to be his intention to reduce into order his various remarks on the subject, and to accompany them by a figure of the animal taken from the living specimen.

Mr. Owen read the following notes of the morbid appearances observed in the dissection of the specimen of the *Chimpanzee, Simia Troglodytes*, Linn., which lately died at the Gardens; and respecting the habits and faculties of which some observations by Mr. Broderip were read at the Meeting of the Society on October 27, 1835. (Lond. and Edinb. Phil. Mag., vol. viii. p. 161.*)

"Adhesions of the abdominal *viscera* to the *parietes* of the cavity existed in many parts, but more especially of the ascending *colon* and *cæcum* on the right side. On separating these adhesions a purulent cavity was exposed, with which the *ileum*, near its termination, communicated by an ulcerated aperture about half an inch in diameter. An abscess also existed between the lower end of the *cæcum* and the *peritoneum*, and the whole of the *fundus* of the *cæcum* was destroyed by ulceration, together with part of the vermiform process; the remainder of which was much contracted and shrivelled, and was found adhering to the sound part of the *cæcum*. The efficiency of the adhesive process in repairing, or at least preventing, the immediate evil consequences of a solution of continuity in the intestinal *parietes*, was remarkably exemplified in this instance; for notwithstanding the extent to which this had taken place, not a particle of the alimentary matters had escaped into the general cavity of the *abdomen*, nor was the mischief suspected until the adhesions were separated.

"On laying open the *ileum* it appeared that the original seat of the ulcer had been a cluster of the aggregated intestinal glands: similar patches in the immediate neighbourhood were in a state of ulceration; and others were enlarged, or more than usually conspicuous, as they were situated farther from the seat of the disease.

* An abstract of Mr. Owen's paper on the comparative osteology of the *Orang* and *Chimpanzee* appeared in Lond. and Edinb. Phil. Mag., vol. vi. p. 457.

In the commencement of the *colon*, the solitary glands presented a state of enlargement and ulceration, and here and there an inordinate vascularity ; but in the general track of the intestinal canal traces of recent or active inflammation were very few. The condition of the mucous membrane of the intestines closely resembled that which is so generally observed in phthisical subjects; here, however, the strumous matter was not developed in the lungs, but was confined to the mesenteric glands and spleen. All the mesenteric glands were more or less enlarged by a deposition of caseous matter : two, which are usually found adhering to the termination of the *ileum*, were even in a state of suppuration and ulceration, so that the *parietes* of the gut may have been attacked by the ulcerative process on both sides,—from without by that commencing in the mesenteric glands,—from within by that of the *glandulæ aggregatæ* : it was most probably, however, progressive from the latter point.

"The spleen was greatly enlarged, measuring 5 inches long and 4 broad, with numerous small scattered tubercles, none exceeding half an inch in diameter. Its substance was firm, but so disorganized as to enable it to fulfil in a very slight degree the functions of a reservoir of venous or portal blood.

"The liver was enlarged about one third beyond its usual size, and was of a pale colour ; but upon a close inspection it presented no other morbid appearance than a congested state of the portal veins: a condition frequently associated with strumous *viscera*, and which was very well marked in this case, and perhaps dependent on the diseased state of the spleen. The gall-bladder contained thick but healthy-coloured bile.

"The stomach seemed free from disease; but had a large perforation, the margins of whch showed that it had resulted from the *post-mortem* action of the gastric secretion.

"The *pancreas* was healthy.

"In the chest there were no adhesions. The heart was healthy. The lungs were somewhat firmer than usual, and the air-passages contained an unusual quantity of fluid secretion, in some parts stained with blood ; but none of the air-cells had been obliterated by either inflammatory action or strumous deposition: there had been recent subacute inflammation of the mucous lining of the air-passages, but nothing more.

"No *Entozoa* were met with in the dissection; although the alimentary canal was carefully searched for them.

"The brain and its membranes were healthy.

"With respect to the organization of the *Chimpanzee*, so far as the dissection was carried, the parts corresponded with the descriptions given by Tyson in his 'Anatomy of a Pygmie' ; and by Dr. Traill in the 'Wernerian Transactions,' vol. iii.

"The *tunica vaginalis testis*, which communicates with the *abdomen* in the *Simia Satyrus*, was here a completely closed or shut sac, as in the human subject."

"Descriptions of some Species of Shells apparently not hitherto recorded: by W. J. Broderip, Esq., V.P.Z.S., F.R.S., &c." were read. The reading of the communication was accompanied by the exhibition of specimens of the several species referred to in it: viz.

SPONDYLUS *albidus*; VOLUTA *Beckii* and *Concinna*; CONUS *Adamsonii*; PURPURA *Gravesii*; and BULINUS *Crichtoni*, *inflatus* and *Pusio*.

The characters, &c., of these shells are given in the "Proceedings" of the Society, No. XL,, from which we retain the following:

BULINUS CRICHTONI. *Bul. testá fusiformi, longitudinaliter costatá et corrugatá, costis rugisque validis, subalbidá maculis spadiceis notatá; labio rosaceo-violaceo, labro pallidiore, expanso, subreflexo: long.* 3 (*circiter*), *lat.* $1\frac{1}{8}$ *poll.*

Hab. ad Ambo juxta Huanuco Peruviæ.

Mus. Brod.

This curious shell, which at first sight reminds the observer of *Bulinus Labeo*, Brod., (Zool. Journ., vol. iv. p. 222,) brought home by Lieut. Maw, R.N., and presented by him to the Zoological Society of London, from whose Museum it has been stolen*, differs strongly from it, as will be seen by a reference to the figure in the 'Zoological Journal' which is very accurate, excepting that the longitudinal lines in the engraving are rather too strongly expressed. The *apex* of the shell under description, the only specimem I ever saw, is broken, and its actual length is 2 inches and $\frac{7}{8}$. It will be observed that the specimen is notched at the base, but I suspect that this arises from accidental distortion.

April 26.—A Note was read, addressed to the Secretary by J. B. Harvey, Esq., Corr. Memb. Z.S., and dated Teignmouth, April 24, 1836. It referred to a series of specimens of *Rostellaria Pes Pelicani*, Lam., presented by the writer to the Society, and which he regards as interesting on account of the evidence afforded by them of the curious fact, that in the shells of this species the outer lip is most thickened at a time antecedent to the full development of the shell; absorption of the incrassated part of the lip taking place as the animal advances in age. "This series," Mr. Harvey remarks, "clearly shows that the shell, when not more than one half or three quarters grown, is much thicker than when all the processes are perfected: and that, when each process has a groove or channel in it, the shell is quite thin, and has arrived at its full period of growth."

The shells referred to in Mr. Harvey's letter were exhibited.

Characters were read of the *Vespertilionidæ* observed in the central region of Nepâl; being a communication transmitted to the Society by B. H. Hodgson, Esq., Corr. Memb. Z.S. They have already been published in the 'Journal of the Asiatic Society of Calcutta', for December, 1835, vol. iv. p. 699.

* This certainly was, and I believe (wherever it may be) is, the only specimen in Europe. It was in remarkably fine condition.

The following are the species characterized:

Rhinolophus armiger, Hodgs.
Rhin. tragatus, Ej.
Pteropus leucocephalus, Ej.
Pter. pyrivorus, Ej.
Vespertilio formosa, Ej.
Vesp. fuliginosa, Ej.
Vesp. labiata, Ej.

Mr. Hodgson's characters of these species are accompanied by remarks on the habits of the several genera of *Bats* which are represented by them in the district in which they occur.

A second communication by Mr. Hodgson was read, which has also been published in the 'Journal of the Asiatic Society of Calcutta' (vol. iv. p. 648.) It was entitled "Specific Name and Character of a New Species of *Cervus*, discovered by Mr. Hodgson in 1825, and indicated in his Catalogue by the local name of *Báhraiya*."

The animal to which this paper refers is regarded by Mr. Hodgson as constituting an important link in the chain of connexion between the *Deer* of the *Rusan* and of the *Elaphine* groups: possessing in the numerous snags into which the summit of its horns are divided one of the principal characteristics of the latter group; but agreeing with the former in the absence of any median process on the stem of the horn, and in the singleness of the basal antler. In stature and aspect the species is intermediate between *Cervus Hippelaphus*, Cuv., and *Cerv. Elaphus*, Linn. Its general resemblance to the latter is indicated in the trivial name assigned to it by Mr. Hodgson, that of *Cerv. Elaphoides*.

It is referred to in his 'Catalogue of the *Mammalia* of Nepâl' (Proceedings, part ii. p. 99.) under the name of *Cerv. Bahraiya*, Hodgs.

Specimens were exhibited of numerous species of British *Fishes*, forming part of the collection of Mr. Yarrell. They consisted of dried preparations of rather more than one half of the skin of each individual: a mode of preservation peculiarly adapted, as Mr. Yarrell remarked, for travellers over land; specimens so prepared occupying but little space, and being consequently as portable as dried plants. An incision is made in the first instance round one side of the fish, at a short distance from the dorsal and anal fins, and the whole of the *viscera* and flesh are removed, so as to leave only the skin of the other side with the vertical fins attached to it, and with rather more than one half of the head: the loose edge of skin left from the side in which the incision has been made, is then fastened by means of pins to a piece of board, so as to display the entire side of the fish which it is intended to preserve, and it is then hung up to dry in an airy but shady situation. The more rapidly the drying is completed, the more effectually will the colours be preserved. As soon as the skin is dried it is varnished; and the loose edge of the skin on that side from whence the operation of removing the flesh has been effected is trimmed off with a pair of scissors, as being no longer useful. The preparation is then completed, and consists of the entire skin of one side of the fish, of the vertical fins, and of rather more than one half of the head, the latter being important for the preservation of the *vomer*, so as to show the absence or presence of teeth on that bone, and their form. All the essential characters of the fish are consequently preserved, if care be taken that the skin be so attached to the board on which it is dried, as to retain its original dimensions of length and depth: the due thickness of the fish may be secured in the preparation, if it be considered desirable, by inserting beneath the skin, when extending it on the board, a sufficient quantity of prepared horse-hair.

After explaining the mode which he had adopted in the preparation of the specimens exhibited, Mr. Yarrell made various remarks on those which he regarded as the most interesting among them; and particularly on a series of *Trout* and *Charr* from different localities, and varying in colour according to situation, to season, and also, in some instances, to food.

He then directed the attention of the Meeting to the specimens of the British species of *Rays* which formed part of the collection, and pointed out particularly the difference, as regards surface, which obtains in the sexes of many of these fishes; the skin of the female being, in every instance, comparatively smooth. He added also, by reference to these specimens, and to specimens of the jaws exhibited for that purpose, an explanation of the differences which exist, in adult individuals, in the teeth of the sexes respectively; those of the male becoming exceedingly lengthened and pointed, while in the female they retain very nearly their original flattened surface: the form of the teeth, equally with the armature of the surface, constituting in these fishes a secondary sexual character, although both the one and the other have repeatedly, but erroneously, been considered as adapted for the establishing of specific distinctions.

LXV. *Intelligence and Miscellaneous Articles.*

EHRENBERG'S FOSSIL INFUSORIA.

M. DUJARDIN laid before the Philomathic Society of Paris, some of the tripoli or *polierschiefer* of Bilin in Bohemia, together with a microscope, by means of which it could be perceived that this tripoli is formed, as M. Brongniart has announced from the information of M. Ehrenberg, entirely of the siliceous remains of organized bodies.

These bodies, all proceeding from the same living species, appear under two different forms: according as they are situated, transversely or perpendicularly, they are minute rings, or rectangles *en échelle*, with transverse bars corresponding to each ring: some of those bodies, viewed obliquely, show well the identity of some with others; they originally formed articulated tubes, perfectly cylindrical, from 10 to 16 thousandths of a millimetre in size, formed of contiguous rings, whose height is less by half, and which each have an extremely thin partition, as some broken rings show perfectly well; but, at the present time, we find an analogous structure only in the Diatomæ which are placed by many naturalists in the vegetable kingdom, but which have the shield more or less depressed, and each joint of which, instead of having a single partition, is closed at the two extremities. On the other hand, the Bacillaria have a prismatic shield, often streaked or furrowed, but without real partitions.

M. Dujardin observes that the greater part of the tripolis in the mineralogical collections of Paris by no means present this character, and under the microscope only show grains of silex; and that the same is the case with regard to the silex of Saint Ouen (*silex nectique*), as well as to the schists which envelope the menilite, which some German authors had referred to the *polierschiefer*. It would seem that the tripoli or *polierschiefer* of Bilin, very different from the others, belongs to a very recent lacustrine deposit. That of Santa Fiora, which is mentioned in the letter of M. Brongniart as presenting also the character specified by M. Ehrenberg, seems also to be the product of a deposit of recent formation, which M. Dufrenoy considers as probably presenting some analogy with the siliceous deposits of the Geyser.—*Société Philomathique de Paris*, July 16. *L'Institut*, No. 168.

METEOROLOGICAL OBSERVATIONS MADE DURING THE SOLAR ECLIPSE OF MAY 15, 1836, AT GREENWICH, BY MR. W. R. BIRT.

h	m	Observatory Vane.	
11	15		Howard's fair-weather cumulus, forming on the vapour plane; motion N. by E. As the masses passed over the river they were seen to break into smaller patches, and the thinnest dissolved.
12	30	due E.	The cumuli are now prevalent.
3	0	N.E.	Cumuli still prevalent, their motion being N.W. Considerable diminution of light. White glare in irregular patches noticed round the sun, and filling a circular space of about 40° in diameter.
3	10	S.E.	White glare more evenly dispersed; at this time the wind suddenly shifted to S.E. and more white glare formed in irregular patches. Venus was now perceptible, and continued visible for about half an hour. The cumuli less numerous.
3	20		White glare more evenly dispersed; the cumuli confined to a space of 40° round the sun, except a large mass to the S.W. under the sun.
3	30	S.E. by S.	White glare still filling a space of 40°. A few masses of very thin cumuli before the sun. Motion N.W.
3	45		White glare not estimable.
4	0		Cumulus augmenting in the S.W.
4	10		Cumuli nearly gone.
4	15		Breeze freshening.
4	20	S.E.	
4	35	S.S.E.	
5		S.E.	

During the latter part of the eclipse, from 3 P.M., the upper current was steadily from the N.W. The most interesting portion of the observations is that relating to the white glare, which was evidently due to the diminution of heat. A beautiful stratum of *cirro-cumulus* passed over between 8 and 9 in the evening. I was not able to observe its motion. The white glare was very prevalent during the morning and forenoon, but the atmosphere was quite clear at the commencement of the eclipse.

The above observations may be divided into four portions, namely, those previous to the commencement of the eclipse; 2nd, from this time to 3 hours; 3rd, between 3 hours and 3 hours 45 minutes; and lastly, those taken after that period. The first period was characterized by considerable haziness in the atmosphere, which was the only modification of cloud observed until 11 hours 15 minutes, when *cumulus* began to form the haze; then gradually diminished as the *cumuli* increased, and when the eclipse commenced the sky was free from haze, but much diversified with *cumuli,* which were very prevalent, their motion being north by east, while the lower current was due east.

The second portion was characterized by the prevalence of *cumulus*, with an otherwise clear sky. The third portion was the most interesting, as the effect of the eclipse on the state of the atmosphere was now exhibited. The most striking feature consisted in the production of a white haziness that filled a space around the sun of about 40 degrees in diameter; this was first noticed at 3 P.M., and appeared evidently due to the diminution of temperature occasioned by the interposition of the moon. I have used the term *white glare,* by which Sir John Herschel designates the hazy appearance observed around the sun in his observations of the summer solstice, December 21 and 22, at the Cape, inserted in the Athenæum of May 14th. This appearance continued visible until 3 hours 45 minutes, so that it disappeared about the same time after the greatest obscuration as it appeared previous to it: at the commencement of this portion of the observations the lower current was north-east, the upper current having varied to north-west; during it the lower current changed to south-east, but the clouds kept moving steadily from north-west. Another interesting feature at this time was the clearing of the atmosphere of clouds, similar to that which takes place on a fine summer's evening after a fine day similar to the present, when the fair-weather *cumulus* alone is observed: as the temperature declines, less moisture is exhaled from the surface, consequently no more *cumuli* are formed, and a clear evening follows. A precisely similar phænomenon was observed on the present occasion, but at a much earlier period: this was also probably due to the diminution of temperature. Nothing occurred to mark the fourth period, except a beautiful stratum of *cirro-cumulus*, the largest variety which was noticed: between eight and nine in the evening it passed over rapidly; I did not particularly observe its motion, but it was from the western horizon.

ON A NEW SPECIES OF ACETATE OF COPPER.

M. F. Wöhler has found that the neutral acetate of copper will combine with another proportion of water than that which is contained in the common crystallized verdigris. This new salt is interesting in many respects: it forms large, beautiful, transparent crystals, of the same shade of blue as sulphate of copper, which at once serves to show that a difference exists between it and the common neutral acetate. When a crystal of this salt is heated to about 90° Fahr. it soon becomes opake and green like verdigris, without changing its exterior form, but by slight pressure is converted into a mass of small crystals of verdigris. This transformation is immediately perceived by throwing a crystal into warm water, and the slower a crystal is heated the larger and more distinct are the small crystals of verdigris into which it is changed. This phænomenon exactly resembles the known changes of form which take place without change of composition, which have been observed in sulphate of magnesia, sulphate of zinc, &c., and it is for this reason that this salt of copper deserves attention; for it shows that in phænomena of this kind we ought to be careful to distinguish between those cases in which change of form occurs without change of composition, and those in which the one is the cause of the other. The preceding phænomenon belongs to the latter class; for the change of colour and form is connected with the separation of four fifths of the water of crystallization of this salt. This latter modification does not occur when the crystal remains entire and is become pseudomorphous, for the disengaged water remains inclosed between the new formation of small crystals; and for the same reason, immediate analysis would show the same proportion of water to exist in it, as in the modified crystal. This circumstance might easily be overlooked, for when a crystal changed to green is exposed to the air, it gradually parts with the interposed water; this quantity of water, although in itself small, may be detected by pressing one of the transformed crystals between blotting-paper, which will become damp, from the interposed water retained between the new formation of the small crystals of common verdigris. The quantity of water which the blue salt loses by its conversion into the green is 26·48 per cent.; this is four times as much as that which is still retained by the resulting green salt, that is, the common crystallized verdigris. Thus the blue salt contains 33·11 per cent., or 5 equivalents of water; it is very easily prepared by dissolving verdigris in warm, but not boiling, water acidulated with acetic acid, and crystallizing the solution.—*Journ. de Pharmacie,* July 1836.

FACTS RELATIVE TO THE HISTORY OF ÆTHER.

Some time ago M. Liebig was led, from the results of an analysis of phosphovinate of barytes, to consider the acid of this salt as a combination of phosphoric acid and æther. A similar composition would naturally be assigned to the sulphovinates, but the experiments which were made to verify this supposition served only to show that by the aid of heat this class of salts lost a portion of their water without suffering decomposition. Here the matter rested, until M. Marchand discovered that the sulphovinates lose their water, with extreme facility, at ordinary temperatures, when placed over sulphuric acid *in vacuo*. It follows from his experiments that the sulphovinates of lime, barytes, and soda may be represented by the following formula:

$$2\ So^3 + Ba\ O\ (Ca\ O, Na\ O) + E\ O + 2\ Aq.$$

And that, if we abstract, by means of the air-pump, the two equivalents of water which they contain, we then obtain a salt which is composed of 2 eqs. of sulphuric acid, 1 eq. of base, and 1 eq. of æther. Sulphovinate of potash does not contain any water of crystallization.

M. Liebig has repeated the experiments of M. Marchand to verify their important results and to confirm them in a more complete manner; but he does not admit the doubt that this chemist has raised, as to the formation of alcohol, when sulphovinate of potash is distilled with quicklime. This formation, which M. Mitscherlich has noticed in his treatise, is not only, he says, an accurate fact, but there is produced at the same time the oil of wine and combined hydrogen (*l'hydrogène combiné*) of Serullas. Thus, if we mix sulphovinate of potash with hydrate of lime and expose it to a heat of not above 392° Fahr., we only obtain alcohol, and the mixture does not blacken; but if we use quicklime instead of its hydrate, distillation affords a liquid from which, when mixed with water, sulphate of oil of wine is precipitated; and if from the beginning a strong heat has been applied, the mixture blackens, and there is olefiant gas obtained along with the alcohol and sulphate of oil of wine. The formation of the alcohol is easily explained by the composition of sulphate of oil of wine: this substance consists of 2 eqs. of sulphuric acid, besides $8\ C + 18\ H + O$; and by adding to this formula an equivalent of alcohol, $4\ C + 12\ H + 2\ O$, we obtain $12\ C + 30\ H + 3\ O$, that is to say, 3 eqs. of æther. At the close of this investigation, M. Liebig relates the two following experiments, which are remarkable for their elegance. When a mixture of five parts of sulphovinate of lime and one part of acetic acid—such as is obtained from dry acetate of lead and sulphuric acid—is distilled with a gentle heat, a large quantity of pure acetic æther is obtained. By distilling five parts of sulphovinate of potash with five parts of sulphuric acid diluted with one part of water, we obtain perfectly pure æther. Pure acetic æther is also procured by heating concentrated phosphovinic acid with acetate of potash.—*Journ. de Pharmacie*, Fev. 1836.

HAS HEAT WEIGHT?

To the Editors of the Phil. Mag. and Journal of Science.

Gentlemen,

The question, "Has heat weight?" has been long matter of dispute, and it is not easy to answer those who contend, that if its weight be in the same ratio to that of hydrogen, as that is to the weight of platina, the most delicate balance would not turn with so light a load; besides, they would argue, there are many sources of minute error, which prevent the delicate experiment of ascertaining the supposed no change of weight, with change of heat, being decisive; such as change of length of the arm of the balance, change in the specific gravity of the body heated or cooled, and other errors the exact amount of which cannot be ascertained. The same objection may be urged against the apparently decisive conclusion, derived from the fact that when given weights of hydrogen and oxygen are combined by combustion, the weight of the water is equal to the sum of the weights of the elements though very intense heat is produced during the combination. It must, I think, be acknowledged that human experiment proves nothing more than that if there be any gravitating force it is extremely small. But nature tries the experiment for us on a scale of magnificence, which we may in vain attempt to imitate, for if heat have weight it must necessarily when in motion have momentum; and if the velocity of radiant heat from the sun be equal to that of light, some momentum should be discoverable. But granting that the momentum is too small in amount to show itself on the small scale, it would be sure, did it exist at all, to increase the period of revolution of the planets. It has been proved that the planets revolve at such distances from the sun, and with such velocities, as that the centrifugal and attractive forces shall be equal, if the later force be inversely as the square of the distance. But if heat had *any* momentum, its particles, acting upon such large masses as the planets, must produce an evident effect by increasing their distances and periodic times. Now these are not increased; there can then be no centrifugal force from this cause, heat can have no momentum, and therefore no weight.

If it be objected to this conclusion that the ratio of attraction has been over-estimated, that it is sufficiently powerful to balance both the tangential force of revolution and the centrifugal force of the momentum of radiant heat; I answer that this supposition, violent as it is, would not remove the difficulty unless all the planets were of the same size and mass, or unless the sectional area of all were proportionate to the mass.

The density, or rather rarity, of the resisting medium which has accelerated Encke's comet, has not been ascertained, but it can hardly be such as to counterbalance the supposed centrifugal momentum of heat; for if the density be uniform, then the velocity of the planets through it should be proportionate to the decrease of the rays of heat, that is, inversely as the square of the distance from the sun. If the density be inversely as the square of the distance (supposing the medium analogous to an atmosphere), the velocity of the planets through it should be uniform: neither being in accordance with facts. Yours, &c.

Manchester, Aug. 13, 1836. P. W. Holland.

DR. HUDSON'S REPLY TO DR. APJOHN'S PAPER INSERTED IN THE PHILOSOPHICAL MAGAZINE FOR SEPTEMBER.

In the course of September last we received from Dr. H. Hudson a reply, dated Stephen's Green, Dublin, 10th September 1836, to Dr. Apjohn's paper inserted in our Number for that month, accompanied by a private note addressed "To the Editors of the Philosophical Magazine," and dated September 12th, requesting the insertion of the reply. We acknowledged the receipt of Dr. Hudson's communication in the notice "To Correspondents" on the wrapper of our last Number, for October, stating it to be "under consideration". Having now given it full consideration, we regret that from the very personal form which the controversy between Dr. Hudson and Dr. Apjohn has now, perhaps unavoidably, assumed, we feel called upon to terminate the discussion in our pages. We have no intention whatever, in doing this at the present juncture, to express any opinion on the merits of the subject; but were Dr. Hudson's reply to be inserted, Dr. Apjohn would have an equal claim to the publication of his rejoinder, and a controversy, in which nothing would be added to the progress of science, (for the points in dispute do not involve any principles which have not already been fully explained in the original papers), might be continued indefinitely, or we might be compelled to close it at some future stage. Dr. Hudson, however, is entitled to the most explicit record of the promptitude of his reply, and of his contradiction of Dr. Apjohn's statements, on which account we have noted above the reception and date of his communication, and also inserted the present paragraph.—EDIT.

FUSELI'S PORTRAIT OF PRIESTLEY.

It is not generally known that a portrait exists of Dr. Priestley, painted, when he was about fifty, by the celebrated Fuseli, which derives a value, not only from the interest of the subject, but from the faithfulness of the resemblance and the spirit and excellence of the execution; as well as from the circumstance of its being almost the only portrait which the celebrated artist is known to have painted. That his powers were zealously employed on this picture may be inferred from the circumstance that it was undertaken at his own particular request, and presented to the common friend at whose house they occasionally met, Mr. Johnson, the Bookseller, St. Paul's Churchyard, after whose death it was removed to the Library in Redcross Street.

Mr. Turner has been for some time employed upon this portrait, and has produced an excellent engraving from it, for a number of gentlemen who have entered into a subscription for the purpose. He has succeeded in giving to this print, which is a very faithful copy of the picture, a powerful and pleasing effect; and the size is conveniently adapted either for the cabinet or the folio.

Subscribers' names are received by Mr. Richard Taylor, at the Office of the Philosophical Magazine, Red Lion Court, Fleet Street, where the copies (Proofs) will be delivered on application.

A very few Proofs have been taken *before the letters* for those who may be desirous of possessing them.

SCIENTIFIC MEMOIRS, selected from the Transactions of Foreign Academies of Science, and from Foreign Journals.

Part II., just published, contains

Researches relative to the Insects, known to the Ancients and Moderns, by which the Vine is infested, and on the Means of preventing their Ravages. By M. le Baron Walckenaer, Hon. Memb. of the Entomological Society of France.

The Kingdoms of Nature, their Life and Affinity. By Dr. C. G. Carus, Physician to His Majesty the King of Saxony.

Researches on the Elasticity of Bodies which Crystallize regularly. By Felix Savart.

Researches concerning the Nature of the Bleaching Compounds of Chlorine. By J. A. Balard.

On the Laws of Conducting Powers of Wires of different Lengths and Diameters for Electricity. By E. Lenz.

Memoir on the Polarization of Heat. By M. Melloni.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1836.

Chiswick.—Sept. 1. Very fine. 2. Overcast: stormy showers: clear and cold. 3. Fine. 4. Rain: fine. 5. Cloudy: very fine. 6. Stormy showers. 7. Fine, but cool. 8. Very fine. 9. Overcast: cloudy: rain at night. 10. Fine. 11. Cloudy: stormy at night. 12. Stormy showers: clear and windy at night. 13. Cloudy and cold: boisterous. 14. Cold haze: fine. 15. Fine. 16. Showery. 17. Fine: thunder showers. 18—20. Cloudy, and fine. 21. Cold and damp: fine. 22. Foggy: very fine. 23. Stormy and wet. 24, 25. Fine. 26, 27. Hazy: fine. 28. Clear: heavy showers: fine. 29. Rain. 30. Clear and cool: stormy showers. The summer, late in commencing as regards temperature, may be said to have terminated with the beginning of this month. The temperature falling so early and abruptly was confidently expected to remain only temporarily depressed; but such expectations have been disappointed.

Boston.—Sept. 1. Fine. 2. Cloudy: rain early A.M.: rain A.M. 3. Fine. 4. Rain: rain P.M. 5. Cloudy: rain early A.M. 6. Cloudy: rain early A.M.: rain P.M. 7. Cloudy: rain P.M. 8. Fine. 9. Rain. 10. Cloudy: rain early A.M.: rain P.M. 11. Cloudy: rain P.M. 12. Stormy. 13. Rain. 14. Cloudy: rain early A.M.: rain P.M. 15. Cloudy. 16. Cloudy. rain early A.M.: rain P.M. 17. Cloudy. 18. Fine. 19. Cloudy: 20. Cloudy: rain P.M. 21. Fine: rain P.M. 22. Cloudy: rain P.M. 23—25. Fine. 26. Cloudy. 27. Cloudy: rain P.M. 28. Cloudy. 29. Cloudy: rain P.M. 30. Cloudy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. Sept.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
Th. 1.	29·816	29·921	29·775	29·14	66·4	56·2	69·6	72	50	61	SW.	SW.	calm	...	·01	...	57
☾ F. 2.	29·768	29·929	29·720	29·10	60·3	54·4	63·5	67	38	56	SW. var.	SW.	calm	...	·22	·05	57
S. 3.	29·959	29·939	29·639	29·37	57·1	46·9	64·2	68	48	54	S.	S.	calm	·169	·45	·15	52
☉ 4.	29·386	29·472	29·372	28·85	61·8	54·0	68·0	70	51	57	SW.	S.	calm	·463	·14	·38	55
M. 5.	29·560	29·658	29·538	28·84	56·4	53·5	62·5	65	47	55	SW. var.	W.	NW.	·113	·16	·12	54
T. 6.	29·360	29·476	29·303	28·71	56·3	50·9	61·7	64	48	55	SSE. var.	SW.	W.	·091	·70	·02	54
W. 7.	29·641	29·785	29·611	29·02	53·7	51·7	60·5	63	46	53·5	NW.	N.	calm	·158	...	·07	52
Th. 8.	29·780	29·784	29·738	29·21	55·9	52·0	63·6	66	47	55	WNW.	SW.	calm	...	...	·02	53
F. 9.	29·746	29·895	29·730	29·18	57·0	51·9	62·5	63	39	53·5	SSW.	SW.	calm	...	·38	...	53
S. 10.	29·792	29·924	29·773	29·27	53·6	46·5	56·8	60	39	49	WNW.	W.	calm	·272	...	·09	50
● ☉ 11.	30·025	30·026	29·919	29·43	50·6	42·4	56·7	61	49	48·5	WSW.	W.	W.	·050	·08	·07	46
M. 12.	30·002	30·091	30·023	29·47	54·5	49·5	57·2	57	51	56	NE.	N.	NE.	·122	·02	·22	50
T. 13.	30·005	30·111	30·078	29·52	55·8	51·7	58·3	59	48	54	NNE.	N.	NE.	...	·01	·05	51
W. 14.	30·117	30·150	30·121	29·64	55·8	48·4	59·0	61	44	55	NNE.	N.	E.	·022	·01	·05	52
Th. 15.	30·170	30·163	30·153	29·62	55·2	47·4	60·2	64	48	56	NE.	NE.	calm	...	·02	·06	52
F. 16.	30·075	30·128	30·092	29·54	53·7	50·0	58·4	65	44	55	NE. var.	NE.	calm	...	·04	·21	51
S. 17.	30·077	30·082	30·073	29·50	56·4	49·5	60·6	64	49	58	NE.	NE.	E.	·088	·06	·30	50
☽ ☉ 18.	30·002	30·030	29·985	29·47	54·8	50·5	58·5	58	46	56	NNE.	NE.	NE.	·141	...	...	52
M. 19.	29·974	29·982	29·962	29·38	51·9	48·3	58·2	61	43	51	NW.	W.	N.	...	...	...	48
T. 20.	29·989	30·018	29·902	29·43	51·6	48·7	59·0	60	35	50	W.	W.	W.	...	·08	...	48
W. 21.	30·152	30·260	30·146	29·60	48·4	42·6	52·3	53	32	44	W.	W.	N.	·091	...	·14	45
Th. 22.	30·277	30·288	30·179	29·33	47·4	40·7	57·8	62	49	58	W.	S.	W.	...	·08	·28	43
F. 23.	29·958	30·038	29·937	29·48	57·8	46·7	63·2	63	53	58·5	SW. var.	SW.	W.	·044	·22	·02	52
○ S. 24.	30·152	30·162	29·999	29·48	60·0	53·9	66·4	70	52	58	WSW.	SW.	W.	·161	...	...	54
☉ 25.	30·178	30·205	30·154	29·50	59·9	53·9	66·7	68	57	58	SW.	W.	W.	...	...	...	55
M. 26.	30·144	30·126	29·930	29·47	60·4	57·9	67·9	73	56	61·5	SSW.	S.	calm	...	...	...	57
T. 27.	29·837	29·866	29·746	29·11	62·3	58·6	65·2	66	48	62	SSW.	SW.	calm	...	·04	...	59
W. 28.	29·683	29·684	29·546	29·00	58·6	52·2	62·6	62	49	55	SSW.	S.	W.	·047	·52	·03	57
T. 29.	29·272	29·480	29·270	28·72	54·4	51·9	58·9	59	43	52·5	NNE.	SW.	calm	·486	·33	...	54
F. 30.	29·426	29·611	29·406	28·77	52·5	46·6	54·6	55	34	51	WSW.	W.	calm	·255	·24	·05	49
	29·877	30·288	29·270	29·27	56·0	50·3	61·2	72	32	54·9				Sum 2·773	3·81	2·38	52·1

THE

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DECEMBER 1836.

LXXVI. *Facts relating to Optical Science. No. IV.* *By* H. F. Talbot, *Esq., F.R.S.**

§ 1. *Experiments on the Interference of Light.*

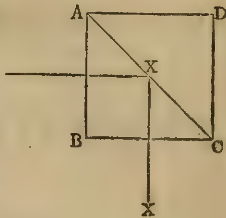
ALTHOUGH so much has been explained in optical science by the aid of the undulatory hypothesis, yet when any *well-marked phænomena* occur which present unexpected peculiarities, it may be of importance to describe them, for the sake of comparison with the theory.

Such appears to me to be the case with those which I am about to mention, in which, by means of a remarkable compensation of some kind or other, common solar light appears to play the part of homogeneous light, and *to achromatize itself*, if I may use such an expression, in a very high degree of perfection.

Sir William Herschel was, I believe, the first who took notice of the very beautiful coloured bands which are seen by looking through two prisms placed in contact. Thus, let A B C, A D C be two equal right-angled glass prisms in contact. We will suppose the sides A B, B C to be equal, and the thickness of the prisms to be equal to A B, in which case the combination of the two will form a cube. Let the two prisms be gently pressed together by their face A C, which must be previously well cleaned from any adhering dust, and

* Communicated by the Author.

let them be fixed firmly in this position. Then if the observer looks through the cube at a bright white object, or at the sky, he will see a number of coloured parallel bands, the direction of vision being supposed to be perpendicularly through two opposite sides, as A B, C D. If instead of this he looks through A B at a light coming from the direction X X and then reflected internally on the face A C, he will again see numerous coloured bands upon A C, but these will be complementary in their tints to the former ones. These coloured bands are analogous in their nature to Newton's rings, differing only in being formed between two *plane* surfaces either parallel or very nearly so, and viewed by the observer at an incidence of 45°.



But the beauty of the appearances may be surprisingly increased by transporting the apparatus into a dark chamber, and suffering a single pencil of the brightest solar light to pass through the prism, or to be reflected from the face A C. If then a sheet of white paper be held up, at any distance from the prism, the coloured bands are depicted upon it with the greatest vivacity and distinctness. The transmitted bands have altogether a different *character* from the reflected ones, so that it is impossible to mistake one for the other, even without reference to the path of the ray.

This experiment, easily tried, is one of the most beautiful in optical science; I shall not, however, dwell upon it, because I believe it is sufficiently well known, and that it has been exhibited in some public lectures.

Now, in making this experiment with care, I have observed some remarkable circumstances.

The coloured bands are not, as has been supposed, *isochromatic* lines. The deviation is sometimes very marked, so that a band in the course of its progress acquires very different tints from those which it possessed originally. This fact may be considered of some importance with respect to the theory. It takes place when the prisms are in close contact, and the bands few in number. But the following is still more deserving of attention. When the contact of the prisms is diminished by interposing a hair between them, (still pressing them together,) the coloured bands depicted upon the paper, become more numerous, narrow and crowded. Frequently they alternate a great number of times with two complementary colours. This appeared to me so remarkable that I repeated the experiment with additional care. The radiant point of solar light was made smaller, by transmitting the ray through a lens of short focus, and the position of the combined prisms was slowly altered by turning them round on their centre. The appearance of the bands on the paper was all the time carefully noted. I soon found a position of the prisms in which the remarkable phænomenon occurred of a complete compensation of colour: that is to say, that the bands were black and white. At the same time they were become exceedingly narrow and numerous. A friend, who had the kindness to count the lines, found *one hundred and ten* of them in the space of two inches. On another occasion they were evidently much closer, so that we estimated their number at *two hundred* in the same space of two inches. The aid of a lens was requisite to see them distinctly. They resembled more than anything else the closely-ruled parallel lines by which *shadows* are produced in some kinds of engraving, and which are often employed in maps to represent the sea.

Now, it requires in ordinary circumstances the employment of *very homogeneous* light, in order to produce bands anything like these in number and distinctness. In the present instance, on the contrary, common solar light was employed. The result therefore is quite unexpected, and it will be interesting to learn in what manner it is explained by theory.

These bands are best seen in the light *reflected* from the face A C. And since the reflected ray does not enter the prism A D C at all, it cannot matter, I think, of what kind of glass it is composed. With respect to the other prism, it appeared to me that the experiment succeeded equally well whether it were of crown or of flint glass.

§ 2. *Experiments on Diffraction.*

In the original experiments of Grimaldi and Newton the diffracted images of objects were merely received on screens of white paper, by which method a great part of their brightness was necessarily lost. Fraunhofer first introduced the use of the telescope in these observations; and Fresnel, I believe, that of the lens or microscope. Both these were very great improvements, though of an opposite character, and have caused the discovery of numerous most curious phænomena.

In order to see these appearances in their perfection, it is requisite to have a dark chamber and a radiant point of intense solar light, which, for the sake of convenience, should be reflected horizontally by a mirror. I will relate a few, out of several experiments which were made in this manner.

1. About ten or twenty feet from the radiant point, I placed in the path of the ray an equidistant grating* made by Fraunhofer, with its lines vertical. I then viewed the light which had passed through this grating with a lens of considerable magnifying power. The appearance was very curious, being a regular alternation of numerous lines or bands of red and green colour, having their direction parallel to the lines of the grating. On removing the lens a little further from the grating, the bands gradually changed their colours, and became alternately blue and yellow. When the lens was a little more removed, the bands again became red and green. And this change continued to take place for an indefinite number of times, as the distance between the lens and grating increased. In all cases the bands exhibited two complementary colours.

It was very curious to observe that though the grating was greatly out of the focus of the lens, yet the appearance of the bands was perfectly distinct and well defined.

This however only happens when the radiant point has a *very small* apparent diameter, in which case the distance of the lens may be increased even to one or two feet from the grating without much impairing the beauty and distinctness of the coloured bands. So that if the source of light were a mere mathematical point it appears possible that this distance might be increased without limit; or that the disturbance in the luminous undulations caused by the interposition of the grating, continues indefinitely, and has no tendency to subside of itself.

2. Another grating was then placed at right angles to the first, and the light transmitted through both was examined by the lens. The appearance now resembled a tissue woven with red and green threads. It seemed exactly as if each colour disappeared alternately behind the other. An alteration in the distance of the lens, altered the tints of the two complementary colours.

3. A plate of copper pierced with small circular holes of equal diameter and in regular rows, was substituted for the gratings. When this plate was held perpendicular to the ray, it produced a beautiful pattern consisting of rows of circles divided by coloured lines or bars. When the lens was approached to the plate, there was a particular distance between them at which there appeared in the centre of each circle a

* A plate of glass covered with gold-leaf, on which several hundred parallel lines are cut, in order to transmit the light at equal intervals.

black spot, as small and well defined in appearance as a *full point* in a printed book, being a curious instance of the well-known fact, of the interference of rays of light producing darkness. This black spot was seen in all the circles at once, in consequence of their having equal diameters.

4. When the copper-plate was placed obliquely and held in various positions, a great variety of very singular patterns were displayed, which can be compared to nothing so well as to tissues woven with threads of various colours. It would be impossible to describe these, any more than the ever-changing figures of the kaleidoscope. They seem to vary *ad infinitum*, and in whatever position the plate is placed, they appear always as distinct as if they were in the focus of the lens.

5. In most optical experiments it is essential that vision should be performed along the axis of the lenses which are employed, or very nearly so. But in these experiments this singularity occurs, that the lens may be placed in any position; so that when held *even very obliquely* the only effect is a considerable alteration in the pattern, which in other respects remains as distinct to the eye as before. The experiments hitherto related, are some which I had the pleasure of showing to some distinguished members of the British Association a short time previously to the late meeting at Bristol; and are communicated in the hope that they may prove interesting to the cultivators of optical science.

§ 3. *Remarkable Property of the Iodide of Lead.*

This substance possesses a property of a singular nature, which I believe differs from anything previously described; or if it is reducible to known laws of chemical and molecular action, offers at least a very striking and beautiful example of them.

If a solution of acetate of lead is mixed with a saturated solution of hydriodate of potash, and the mixture well stirred, the iodide of lead which is formed in abundance, though at first yellow, speedily grows pale, and afterwards becomes perfectly white. If a small quantity of this is taken when freshly made and moist, and squeezed between two plates of glass, it may be seen by the help of a microscope to be entirely composed of very delicate capillary crystals; and if in this state it be laid aside, I do not find that it undergoes any change after being kept several months.

But if, while fresh, it be warmed over a spirit-lamp, it suddenly turns yellow, the first impression of the heat being sufficient to produce that effect. As soon as this happens, it should be removed from the lamp and again examined with the microscope, and it will be seen, not only that the colour is changed, but that all trace of the white capillary crystals has vanished, and instead of them the field of view of the microscope is covered with an assemblage of transparent yellow crystals which are in shape *thin flat regular hexagons.*

But after a few minutes, as the plates of glass grow cool, the white colour returns as before, and the microscope now shows again a multitude of white capillary crystals, the hexagonal ones having in their turn entirely disappeared.

The singularity of this change, which may be repeated several times,—the remarkable fact of being able to view the same substance, alternately of two different colours, and with different *forms* belonging to those colours, induced me to endeavour to see in what manner such a singular metamorphosis took place. I therefore took the plates of glass when cold and adjusted the microscope upon one of the capillary crystals contained in them. It looked, when much magnified, like a cylindrical thread of glass, of a clear white colour and transparent. I then, without deranging the adjustment, placed a small spirit-lamp beneath the glass, at a moderate distance, and watched the effects of the heat. After a short time I observed the cylindrical thread shrink in diameter, and at the same moment the axis of the cylinder split open, and a yellow crystalline plate protruded itself through the opening, increasing in size every moment, while the remainder of the white crystal quickly dissolved and disappeared. This happened at several points of the axis of the cylinder, so that when the change was complete, the yellow hexagons were not unfrequently found arranged in a row or straight line indicating the position of the former crystal. When the heat is more suddenly applied, the dissolution of the white crystal is proportionably more rapid, and the yellow hexagons start into existence before the observer's eye with a suddenness which is very surprising, and increase so rapidly as to triple or quadruple their diameter in a second of time, preserving all the time the exact figure of the regular hexagon. Most of them are of a full yellow tint, but some are of a greenish yellow, and some of a peculiar light brown, which variety of tint appears a circumstance worthy of remark, but I do not know upon what cause it can depend.

There is something in this experiment which is very peculiar. We are accustomed to see salts dissolved or melted by heat; or if they are of an insoluble nature, at any rate they remain inert and passive when heated.

But here we have a salt which crystallizes when heated, and the more rapidly the greater the heat. I have described the manner of change of this substance from its white to its yellow crystalline form. And the following is nearly what happened during its return to its former state.

When it cools, the white crystals begin to shoot, and if the microscope is adjusted upon one of the yellow hexagons, it is seen to remain quiet and undisturbed until one of the white needles, which elongate rapidly, passes near it. But when the needle passes it, even at what appears in the microscope a considerable distance, the hexagon becomes corroded on its edges, and then breaks up irregularly, and quickly dissolves.

I observed that when a needle, during its growth, happened to strike a hexagon, this seemed to check it for an instant, and then it subdivided itself into a number of ramifications or smaller needles which diverged from that point; as if the force (probably of an electrical nature) which caused the growth or formation of the needle-crystal had been deranged or subverted by the disturbing influences which it had met with.

The change from the white to the yellow form may be repeated four or five times; but when too much water has been evaporated by the heat, it ceases to occur. The white crystals then merely dissolve when heated, without the formation of the yellow ones.

Remarks.—Are the white and yellow crystals identically the same substance, assuming different forms at different degrees of temperature? Is this a case of what has been termed dimorphism? If I may venture a conjecture, I should say that the yellow crystals are a definite compound of the white crystals with water. But however this may be, it appears to me that this and other properties of the iodide of lead are worthy of being more particularly examined.*

LXXVII. *On the Carboniferous Series of the United States of North America.* *By* Richard Cowling Taylor, *Esq., F.G.S., &c.*†

I HAD just completed two articles on the upper series of transition rocks, and the relative positions of the depositories of bituminous and anthracitous coals in Pennsylvania, with various detailed illustrative sections, which I had pro-

* These two forms of iodide of lead are noticed in Dr. Inglis's "*Extracts from his Prize Essay on Iodine*;" Lond. and Edinb. Phil. Mag., vol. viii. p. 19.—Edit.

† Communicated by the Author.

posed to myself the honour of laying before the Geological Society of London, when the interesting paper of Mr. Weaver in the Lond. and Edinb. Phil. Mag. for August 1836, reached me. I do not know how far what I have therein communicated may influence the opinions of this experienced geologist on the subject of the age of those coal deposits which I have imperfectly defined under the denomination of transition; but for similar reasons to those which have led to Mr. Weaver's communication in the Magazine, I am induced, through the same medium, to state how I have arrived at a different opinion to that which this gentleman entertains, on a very interesting portion of American geology. I should greatly hesitate in differing from an authority so deservedly eminent, and should be disposed to adhere with much less tenacity to the views which he has done me the honour to quote, but for the frank admission that he has, unfortunately, not had the advantage of seeing the district in question. However, I rejoice to perceive that the geology of this country is attracting the attention of scientific observers, who have laboured so much and so usefully in Europe, and who apply the experience acquired in one quarter of the globe to the elucidation of unsettled geological phænomena in another.

Mr. Weaver inclines to the opinion, in support of which he adduces more than one authority, that the immense series of Pennsylvania rocks, amongst which are some inclosing numerous thick seams of anthracitous, passing into bituminous coals in certain places, belong altogether to the secondary carboniferous series or order. It is scarcely necessary to enter into a detailed statement of all the evidence which has occasioned a contrary decision, and which led to the classification of the eastern coal-fields and the vast succession of conglomerates and red shales and sandstones, with the grauwacké and the upper series of transition rocks.

The arrangement I have adopted, for the present, may be very shortly recapitulated; commencing with the highest.

1. The (almost) horizontal carboniferous series, forming the great western bituminous coal-field of this country, whose eastern outcrop is the summit and the escarpment of the Alleghany mountain range, through the greater portion of Pennsylvania. All geological writers, I believe, concur in denominating this a *secondary coal formation*. This series includes the conglomerate, or pudding-stone, and grit, resembling the millstone grit, on which the series is unquestionably based.
2. *The Old Red Sandstone*, and red shales, many thousand feet thick. The dip of its numerous beds, passing

beneath the coal formation, increases gradually in descending, until they are but a few degrees from vertical, in central Pennsylvania. Mr. Weaver is satisfied with the existence and identity of this group in York State, but not that it is identical with that which I have traced from the same State and shown to pass immediately beneath the bituminous (secondary) coal-field of Pennsylvania in Tioga.

3. *The Upper Transition and Grauwacké Series*, commencing at the termination of the red shales and sandstone at the base of the Alleghany mountain, and dipping at a very high angle under that mountain and the bituminous coal on its summit, the whole series being much broken and heaved up on its edges, inclining in several anticlinal and synclinal groups. In Pennsylvania this series consists of at least eight zones of (transition) limestone, in general deficient in fossils; and as many zones of sandstones and conglomerates, stretching parallel with the Alleghany. The aggregate of this upper transition system, even on the lowest computation, is of enormous thickness. It comprises four or five troughs or basins containing coal, which on the east side of the State is anthracitous, and on approaching the south-west, contains upwards of sixteen per cent. of bitumen and volatile matter.

Mr. Weaver, and one or two other writers, conceive that the whole series, from No. 1 to No. 3, inclusive, is secondary. If so, then must the grauwacké and upper transition series be absent; nor can the red sandstone under the Alleghany mountain and coal-field, be the old red sandstone, as I presumed.

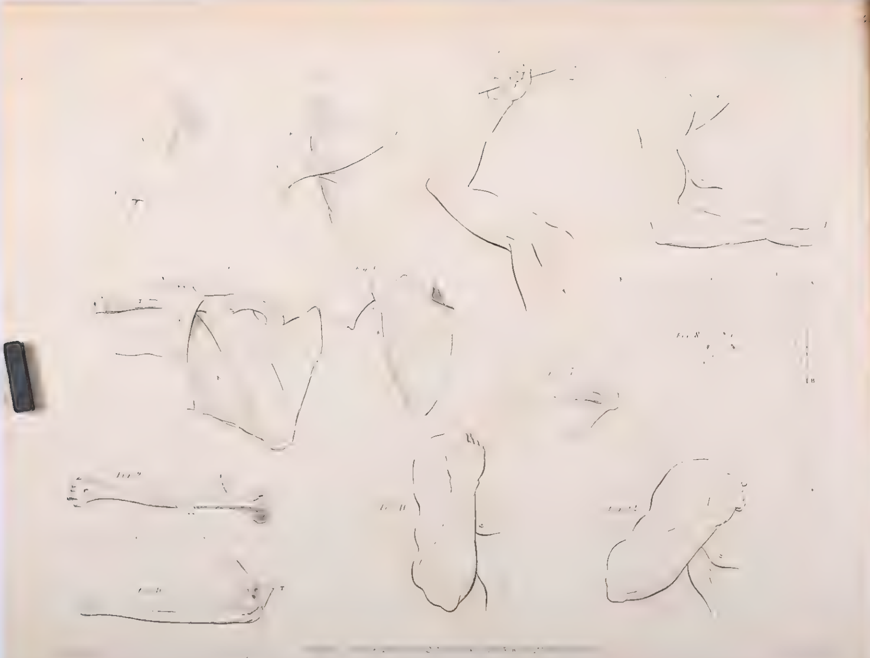
I must confess that I have not seen beneath the great Alleghany coal-field a formation fully answering to the characters of the carboniferous limestone. No such rock interposes between this secondary coal-field and the red sandstone, for the occasional beds of thin gritty gray limestone, resembling the "cornstone" in the old red sandstone, cannot of course be its representative. If we select for this purpose one out of the eight zones of limestone, we might expect to find it in the first and most western; but although this slaty limestone contains some fossils in particular localities, it has no claim, *par excellence*, to the title of the carboniferous limestone.

In York State the limestone which is thought to resemble in geological age and character the carboniferous limestone of Europe, appears decidedly *lower* in the series than the group I have designated as the old red sandstone, but at the same time it (the limestone) reposes upon another red sandstone which Mr. Weaver considers the true old red sandstone in question. I have carefully traced these upper red sandstones from the State of New York until they sink under the great bituminous coal-field of Pennsylvania at Blossbury.

The carboniferous limestone appears on the west side of the Alleghany mountain, accompanying the coal, in Tennessee, Illinois, Kentucky, and Indiana. Its resemblance to the mountain limestone of England is, I believe, admitted by all European as well as American geologists. I am far from being certain of its presence to the eastward of the outcrop of the secondary coal-field in Pennsylvania; and I doubt if in York State this is the same formation which in the west is in close approximation with, and even contains, coal seams. I do not know which of the calcareous rocks is meant by Professor Eaton, as the "limestone which supports the strata containing the Pennsylvania coal." My diagrams, which have been laboriously worked out, exhibit no limestone in Pennsylvania, between the secondary coal series and my old red sandstone group which averages a mile thick. The description, therefore, refers to the contorted and frequently highly-inclined limestones, which range alternately with the upheaved arenaceous rocks in front of the Alleghany mountain, along an area seventy miles broad. Mr. Weaver infers (p. 117.) that all the carboniferous limestone series of the north-east part of Pennsylvania supports equally the bituminous secondary coal of Clearfield, Lycoming, Tioga and Bradford, and the anthracitous deposits of Wyoming, Lehigh and Schuylkill; the red sandstone and shales being in this view continuous or identical.

Here, therefore, is the point of difference with the views I have been led to entertain. I see that both in the acknowledged secondary bituminous coal region, and in those of the anthracite districts, the carbonaceous deposits are alike based on red sandstones and red shales; but the sections distinctly show, that these are neither similar nor continuous beds, either of coal or sandstones; but are of different dates, the latter being referrible to the transition series, the former to the secondary.

On the north-east extremity of the secondary coal-field, the subjacent rocks, being much more horizontally disposed than further to the south, are more obscurely developed; but all along the eastern escarpments of the Alleghany the relative position of the entire series seems apparent enough, when traced out with ordinary caution. Mr. Weaver, however, maintains that the application of the term old red sandstone, to that rock



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immediately beneath the great coal-field, is incorrect, it being only " an alternating series lying above the great body of carboniferous limestone." This objection is obvious, if what I have termed the transition limestone at the base of this series be secondary, "carboniferous;" and if it can be proved, in the district I have imperfectly described, to " repose conformably on the extensive formation of old red sandstone."

On this head I wish to be understood, not as insisting on the infallibility of my own individual sentiments, but as desirous of apprising geologists of the grounds on which those views were founded. At the same time I am not quite prepared to admit, with Mr. Weaver, that the whole of the coal deposits in Pennsylvania, belong to one great carboniferous order, and that the fact " is fully established by Professor Eaton." (p. 131.)

A vast deal of investigation remains yet to be entered into before these debateable points can be adjusted. There is good prospect of some of them being shortly elucidated by the geological surveys simultaneously going on in the States of New-York, Pennsylvania, Maryland and Virginia.

Philadelphia, Sept. 23, 1836.

LXXVIII. *Physiological Remarks on certain Muscles of the Upper Extremity, especially on the Pectoralis Major. By* F. O. Ward, *Esq., King's College, London.**

[With a Plate.]

THERE is a remarkable fold in the tendon of the pectoralis major, which, though described by all anatomists, has never yet, I believe, been explained. The muscle consists of two portions, one smaller and upper, arising from the clavicle, and passing downward and outward; the other larger and lower, arising from the sternum and ribs, and having a general direction upward and outward. The fibres of the muscle thus converging towards each other, terminate in a flat tendon several inches wide, which is attached to the upper part of the humerus.

Instead, however, of having the usual simple insertion represented in Plate IV. fig. 1, the lower part of this tendon is folded up, behind the upper portion, so that the margin B appears above the margin A, as represented in fig. 2.

As it is an axiom in physiology that every arrangement is to be accounted for, this peculiar twist has given rise to several speculations. Some suppose it designed merely to diminish the extent of the insertion. Others believe it to have

* Read before the Royal Society June 16th, 1836: and now communicated by the Author.

the effect of equalizing the length of the muscular fibres. But independently of the consideration that the insertion would have been quite as compact if the tendon had been thick and single, instead of thin and double; and that the fibres are *not* by any means equal in length, according to the second hypothesis,—both explanations are defective, in as much as they show no reason for the muscular fibres crossing each other, so that the upper are attached *below*, and the lower *above*, the medium point of the whole insertion.

I think that the arrangement becomes perfectly intelligible when the separate actions of the upper and lower portions of the muscle are considered with reference to the species of motion those actions require.

The separate action of the lower fibres is to depress the arm when raised; that of the upper fibres, to raise the arm when depressed. Of this any person may convince himself by laying the hand on the muscle; first, while imitating the action of hammering; and then, while raising or supporting a weight: in the former case he will perceive a momentary convulsive contraction of the lower fibres; and, in the latter, a steady, continued tension of the upper.

In the third and fourth sketches which exhibit these positions, the several directions of the humerus, and of the upper and lower portions of the muscle, are represented by lines, the arrow-head denoting in each figure which set is exerted, and in what direction it acts.

Now since the humerus is a lever having the fulcrum at one end and the resistance at the other, the velocity it acquires must be directly, and the force inversely, proportionate to the proximity of the moving power to the fulcrum.

The most common, and therefore most important purpose, to which the depressing fibres are applied, is that of bringing down the arm in using the hammer, pickaxe, &c., as the carpenter, blacksmith, goldbeater, and a hundred other artizans testify. In these motions velocity alone is required from the muscle, the gravity of the tool giving force to the blow; and to produce this velocity the lower division is attached near to the fulcrum. Again, the commonest employment of the upper fibres consists in such actions as lifting, drawing, and the like, in which force, not velocity, is the desideratum; and, in order to obtain force at the expense of velocity, the insertion of these fibres is brought down as far as possible towards the resistance.

It is remarkable that, in each instance, that very fasciculus of the muscle, which possesses most of the action peculiar to its division, possesses likewise that very point of the insertion which affords it most of the leverage it requires. Thus it is not the uppermost portion, *a*, of the elevating division, (see the Italic letters in figs. 2 and 4,) which is attached to the lowest point, D, of the insertion, because there are succeeding fibres (as *b*, figs. 2 and 4,) which form a less acute angle with the humerus while *depressed*: whereas it *is* the lowermost fasciculus (C, fig. 2,) of the lower division, that seeks the highest point, B, of the insertion, because this portion forms the least acute angle with the humerus when *elevated* (see fig. 3). This trait adds another to the innumerable proofs of the *minute* accuracy of the animal organization.

Any action which requires from either portion, a species of motion contrary to that which it is adapted to produce,—as raising the body by the hands, which requires force from the fibres of velocity,—soon fatigues the muscle. Turning a winch, which is another instance of the same kind, is notoriously a very disadvantageous application of human strength; and any employment in which steady and forcible pushing has to be performed by the arms raised above the head, is extremely fatiguing. In throwing a heavy quoit, which requires both accuracy and force, the arm is swung by the side; but in throwing a light ball, for which velocity is requisite, the arm is always swung above the head. Cricketers are practically such good physiologists in this respect, that they have enacted a law which compels the bowler to swing his arm by his side in throwing the ball,—because, if the ball were flung "overhanded" at the wicket, from so near a point as the bowler's station, its velocity would be unmanageable; whereas the "long-throw" who has to send up the ball from a distance, always swings his arm above his head.

The muscles associated with each division of the pectoralis major bear out the proposed explanation by the analogy of their insertions. Thus the coraco-brachialis, and the anterior fibres of the deltoid, which cooperate with the upper division of the pectoralis major, are attached to the front of the humerus, half-way down; evidently for the purpose of gaining force, which they do want, at the sacrifice of velocity, which they do not.

On the contrary, the teres major and latissimus dorsi, which assist the lower division of the pectoralis major in depressing the humerus, act, like that muscle, near the fulcrum of the lever; being attached to the inner margin of the bicipital groove, just opposite to the pectoralis tendon. These two muscles, indeed, are in several respects analogous to the two divisions of the pectoralis major. The teres major, which is superior and smaller, and arises from the scapula, may be compared to that portion of the pectoral which is superior and smaller, and arises from the clavicle; and the latissimus dorsi which is inferior and larger and arises from the vertebræ and ribs, resembles that portion of the pectoralis major which is inferior and larger, and arises from the sternum and ribs. The tendons of the two dorsal, like those of the two pectoral muscles, are continuous at their lower margins, and, as if to render the analogy complete, (though, in fact, to render the leverage suitable,) the lowest fibres of the latissimus dorsi are folded around the teres major, and inserted above it into the humerus; because they are most nearly at right angles with the bone when lifted to strike, and therefore most effective in drawing it down. Fig. 5 is a front view of the insertion of these two muscles, B representing the teres major, C the latissimus dorsi folding round it to gain a higher point of attachment, A the tendon of the pectoralis major raised out of its natural position, and D the bicipital groove to the borders of which these muscles are attached. The proposed explanation is further borne out by the comparative anatomy of the pectoral muscle in birds, in which it is developed to a very large size on account of being the principal motor of the wing. In these animals there is no crossing of the fibres of the pectoralis; they all assist in performing one action, and are consequently inserted in regular order, those which are superior at their origin having also a superior insertion, and *vice versâ*, as may be seen in fig. 6, which is a sketch of the pectoral muscle of a pigeon. The turning under of the fibres represented at *a* seems at first sight to indicate a decussation of the upper and lower portions of this muscle, similar to that which occurs in the corresponding organ of man. But the resemblance disappears when the muscle is divided along the dotted line *b c*, and the humeral portion reflected as in fig. 7. It then becomes evident that the lower fasciculi though forming a little bundle partly distinguishable from the rest of the muscle, and inserted by a separate slip of tendon, nevertheless join the bone *below* the upper fasciculi, and *below* the central point of the whole insertion. Professor Rymer Jones, who very kindly examined with me the muscles of the breast in the pigeon, confirms the accuracy of this observation.

There is, however, an action, which, as it furnishes man with his most obvious means of self-protection, must have been carefully provided for by Nature, and which seems to throw doubt on the correctness of the foregoing explanation. I mean the action of throwing the extremity forward, as in boxing. In this action, which requires great velocity, although *all* the fibres of the pectoralis major are in some measure brought into play, the *upper* set, that namely of least velocity, are, it must be admitted, the principal agents, so far as this muscle is concerned: in other words, Nature, according to my explanation, causes a muscle to work at disadvantage, in an action of essential importance.

This, I think, is only an apparent difficulty, for in this motion as correct a balancing of leverage is displayed, as can anywhere be found throughout the body.

The fist is thrown forward by a double motion. The humerus, represented by A B, fig. 8, revolves round the point A till it takes the position A C, while the forearm, represented by B D, revolves round the point B till it takes the position B E, so that the resulting position of the whole extremity is A C F. The upper division of the pectoralis major, the anterior fibres of the deltoid, and the coraco-brachialis, are the main causes of the first motion; the triceps, anconæus, and supinator muscles, of the second.

The distance which the forearm passes through, represented by the curve 2, exceeds considerably the space traversed by the upper arm, represented by the curve 1; but as the motion of the forearm round the point B is from above downwards, its extensors have no weight to raise; on the contrary, are assisted by gravity. Whereas the humerus, though it moves through a shorter distance, moves upward, and carries with it the forearm, so that its elevators have to raise a considerable weight. In order that these two motions may be completed in the same time, the former requires the greater velocity, the latter the greater force. Accordingly the triceps and its associate extensors, act on the ulna by a lever between one and two inches long, while the three elevators of the humerus act by levers whose respective lengths are about four, five, and six inches. See figs. 8 and 9, in which P represents the tendon of the pectoralis major, D that of the deltoid, C that of the coraco-brachialis, and T that of the triceps.

I may just add, (for it is interesting to observe the unconscious acquaintance which every man has gradually acquired with the precise capabilities and most effective application of every fibre in that complicated machine, his own frame,) that in preparing to strike a blow the elbow never hangs close to the side, as in fig. 11, but is always thrown out, as in fig. 12; in order that the elevator muscles, all of which draw more or less inward, as well as upward and forward, may act during the strong effort at their full advantage.

Thus, then, not only is the leverage of the upper and lower portions of the pectoralis major accurately adapted to the actions of lifting and hammering which they respectively perform, but it is so proportioned to the leverage of the triceps, that the two muscles cooperate harmoniously in the action of striking a blow forward; unequal spaces being traversed and unequal resistances overcome, in the same period of time, so that the resulting position of the limb is precisely the one required: while the strength of the one set of muscles bears such proportion to that of the set with which it acts in concert, that both remain unfatigued for the same number of actions.

It is this *diversified* adaptation of parts, which forms the chief characteristic of the mechanism of Nature. Working with unlimited means, she yet works with scrupulous œconomy; in her structures no power is redundant, nor a single advantage lost; so that, however completely an arrangement may subserve one primary purpose, we find, upon renewed examination, an equally accurate adjustment to several secondary ends.

When the means of estimating with precision the contractile force of the muscular fibre, are obtained, I have no doubt that these compound relations of power, lever, and motion produced, will form an interesting study*.

Magendie † observes, that the intensity of muscular contraction depends partly upon certain peculiarities in the organization of the fibres, such as size, firmness, colour, &c., and partly upon the energy of the cerebral influence, or the "puissance de volonté," by which they are excited to action. Muscles acquire far more than their ordinary power, during those affections of the mind which stimulate the brain to strong action, such as rage, madness, &c., and also during certain convulsive

* Borelli in his posthumous work *De Motu Animalium*, published in 1680, has entered into an elaborate analysis of the mechanical relations of the body, with a view to determining the absolute force of the muscles. But unfortunately his experimental data (see, for instance, *Pars prima*, cap. 8,) are as loose and unsatisfactory as the subsequent calculations are minutely accurate; and his reasonings are interwoven with a purely speculative hypothesis of the nature of muscular fibre, which he supposes to consist of minute rhomboidal vesicles, contractile by inflation. By these means he brings out very startling results. Thus to the flexor longus pollicis manus alone, he attributes a tractile force of 3720 pounds; to the deltoid of 61,609 pounds; to the intercostals of 32,040 pounds; to the glutæi of 375,420 pounds, &c. (see cap. 17, prop. cxxiv. *et seq.*) Dr. Bostock considers his estimate of the force of the muscles of the thumb to be a hundred times too great. He has not noticed the twisted tendon of the pectoral in man, nor calculated its force and leverage. The only remarks upon its strength I can discover, are in cap. 22, prop. cciv., where, from its small relative size, he proves it to be impossible "ut homines propriis viribus artificiosè volare possint."

† *Physiologie*, vol. i. p. 275.

diseases which have similar cerebral effects. From these and some other facts adduced, he infers that cerebral influence on the one hand, and certain qualities of the muscular tissue itself on the other, are the two elements of muscular contractility.

Mayo has indicated a method of determining the maximum strength of individual muscles, by ascertaining the weight that is required to rupture their tendons. This mode is founded upon the argument that the tension which the tendon can sustain, probably exceeds but little that which the fibres can exert; a supposition which is analogically probable, and in some measure supported by facts, since in præternatural contraction sometimes the tendon, sometimes the trunk, of a muscle gives way*; proving that there is no *great* difference between the active and passive strength of these organs.

The constant and equable stream of galvanism, afforded by Daniell's new battery, will furnish, I think, a good means of comparing the strength of muscles, of regular shape and equal size, by ascertaining the contractile force it induces in its passage through each.

In order to subject any muscle to this experiment, it should be separated from its fellows, and, at the distal end, from its insertion. By the tendon, thus detached, it should be connected with a spring moving an index; and the bone, into which its opposite extremity is inserted, should be firmly fixed at a known distance from the spring. The trunk of the muscle should then be made part of the circuit; and the distance to which it moved the index during the transmission of the current for a given period (say one minute) might be taken to express the force of the muscle as compared with others submitted to the same treatment†.

The comparative dimensions and weight of such muscles as resemble each other in colour, firmness, and texture, would also probably bear some proportion to their comparative force.

Although neither of these methods of estimating muscular contractility could be depended on alone, yet by a judicious application of each in turn, to corroborate or correct the results furnished by the others, a close approximation might at last be obtained. And since we have proof that there is an accurate balancing of muscular force in the fact that muscles, or sets of muscles working together, are fatigued, equally and simultaneously, we may fairly expect that whatever the ab-

* See Tetanus Cooper's First Lines of Surgery.

† The contraction of the muscle only occurs at the instants of completing and interrupting the circuit. Contact must therefore be broken and renewed at regular intervals during the experiment; which is readily effected by means of a pendulum connected with the wire. See Becquerel's *Traité de l'Electricité*, vol. iv. p. 306.

solute strength of muscles in different individuals may be, their relative strength will be found nearly alike in all, exception being of course made for the influence of habitual employments upon particular muscles. If, for example, in one arm the power of the biceps were one, and that of the triceps two, in another arm the power of whose triceps was two, that of the triceps would be four, or thereabouts; or if not so, the difference would be compensated by a counter-variation in the leverage.

It is also probable that in the same individual, under various conditions of lassitude or excitement, whether produced by bodily or by mental affections, each muscle retains its normal relation in point of strength to the others, whatever may be its actual gain or loss of contractility. So that if this ratio were once established by the mean results of cautious experiments, it would be possible, from the absolute strength of one muscle, or set of muscles, to deduce by calculation the absolute strength of each of the remaining muscles in the same individual. We should of course meet with irregularities; some caused by disproportionate growth, and bearing an ascertainable relation to its degree; and others depending on circumstances beyond the range either of observation, or of calculation; but if a standard proportion does really exist, the deviations from it are certainly in opposite directions, and the true ratio will be discovered by taking the average of an extensive series of measurements and estimates. And when we reflect that within the last few years constant numerical proportions have been developed by Wenzel, Berzelius, Dalton, and others, in the chemical affinities of ponderable matter, and by Faraday in the action of the imponderable forces; and—still more to the purpose—that mathematical laws so fixed and definite as to serve for the distinction of species, have been discovered by Schimper and Braun* to regulate vegetable growth; it seems not unreasonable to surmise, that numerical proportions, as certain and invariable, may govern the secret workings of animal life, and be hereafter revealed by the discovery of accurate, though involved, mathematical relations, between the several organs of the animal machine†.

A rigorous analysis of the mechanical relations of the mus-

* *Archives de Botanique*, vol. i.; Martin's Abstract of Braun's Paper; Henslow's Introduction to Botany, p. 124; Lindley's Introduction to Botany, second edition, p. 91. Lindley thus states the result of the inquiry: "The whole of the appendages of the axis of plants,—leaves, calyx, corolla, stamens, and carpels,—form an uninterrupted spire governed by laws which are nearly constant." For the causes of the occasional deviations from these primary laws, see Henslow's Introduction, § 121.

† I trust that an hypothesis thus indicated by the analogy of several ascertained laws, capable of inductive examination, and whether erroneous

cular and osseous systems in various animals, would form a good foundation from whence, in future, to push forward such inquiries; and, besides this remote and dubious utility, contingent on the soundness of the foregoing speculation, such researches would be of considerable immediate advantage to science. They would give the geologist a new point of view in which to examine fossil bones, and might enable him to deduce, from the relative size, shape, and situation of the marks indicating muscular insertion, new particulars concerning the strength and speed of extinct creatures; they would probably point out to the comparative anatomist analogies and differences in the structure of animals, where none have hitherto been suspected; and above all, they would tend to introduce into physiology an exactness and certainty which the science has not yet attained. As a first step to such an analysis, I intend shortly to attempt a set of experiments on the contractility of the muscular fibre, by the several methods that have just been described. Those who undertake such researches should bear in mind that the friction of the tendons is an important element of the calculation. Muscles which are extended in a straight line between their attachments, and undergo no friction but that of the investing cellular tissue (as the gastrocnemius), have greatly the advantage of those whose tendons play over trochlear surfaces (as the obturator

or not, likely to suggest to its investigators *some* useful experiments, will not be classed with the extravagant iatro-mathematical speculations which retarded the progress of physiology in the seventeenth and beginning of the eighteenth century. " Prudens quæstio dimidium scientiæ," says Lord Bacon, a sentiment admirably elucidated by Herschel in the Preliminary Discourse. " A well-imagined hypothesis," he says, " if it have been suggested by a fair inductive consideration of general laws, can hardly fail at least of enabling us to generalize a step further, and group together several such laws under a more universal expression,......and we may thus be led to the trial of many curious experiments, and to the imagining of many useful and important contrivances which we should never otherwise have thought of." To which may be added the following judicious remarks of Mr. R. Young: " As in practice nothing is perfect, and few things wholly without merit, so, in theories, perhaps, none are without error, nor any devoid of truth. The difference between opinions seems to lie chiefly in the different proportions of truth and error which they contain. If this be true, every advance in principles is only substituting a less imperfect theory for one more so, and the last ever leaves something for futurity to correct."—(Essay on the Powers and Mechanism of Nature, p. ix.)

[We may refer the reader, on the subject of numerical proportions in animal organization, to our abstract of Dr. W. Adam's paper, " *On the Osteological Symmetry of the Camel*," in Phil. Mag. and Annals, vol. ix. p. 364: the paper itself will be found in the Transactions of the Linnæan Society, vol. xvi. p. 525 *et seq.* See also Lond. and Edinb. Phil. Mag., vol. iii. p. 457, vol. vi. p. 57, for notices of papers by Dr. Adam on the osteological symmetry of the human skeleton.—Edit.]

internus), or run in grooves (as the long head of the biceps), or perforate other tendons (as the deep flexor of the fingers), or turn through fibrous pulleys (as the digastric, the extensor of the toes, &c.). By comparing the effect of a known force acting on particular tendons, at first in their natural situations, and afterward detached and free, the influence of friction in each case would be readily determined. This source of error seems to have been very generally overlooked by writers on animal mechanics.

I conclude, for the present, with suggesting that to distinguish the pectoralis major into "portio elevans" or "attollens," and "portio deprimens," might serve to impress the rationale of its peculiar insertion and twofold action, upon the memory of the student.

LXXIX. *Researches in the Undulatory Theory of Light, in continuation of former Papers. By* John Tovey, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

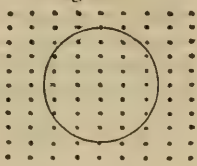
Gentlemen,

HAVING deduced, (p. 500 of your last volume,) by a new method, the laws of the propagation of plane and spherical waves in elastic media, I will now, with your permission, show how the formulæ may be extended to the most simple cases which are known to occur in the undulatory theory of light, of waves not spherical emanating from a center of agitation.

(1.) It will be remembered that in my paper at p. 270 of the last volume, the sums Σ were considered as comprised in three classes, when it appeared that those of the first class, composed of odd products of the differences, vanish, in consequence of the first supposition there made respecting the arrangement of the molecules. The sums also of the second class, composed of even products involving odd powers of the differences, were neglected; because the terms of these sums must be about half of them positive and half negative, and consequently the sums themselves very small in comparison with those of the third class, which last, being composed of even powers of the differences, have their terms all positive.

(2.) If the radius of the sphere of influence be not very much greater than the intervals between the molecules, the sums may or may not be sensibly the same for different directions of the coordinates, according as the intervals are the same or different for the different directions. Suppose, for example, every eight adjacent molecules to be at the corners of a rectangular parallelopiped; suppose fig. 1 to be a section of the medium, the dots denoting the molecules in their places of equilibrium; and suppose the circle to be a section of the sphere of the influence of the molecule which occupies its centre; then, the intervals between the molecules being greater in the horizontal than in the vertical direction, it is manifest that the sums in general will vary according to the directions of the coordinates, and that, when the planes of the coordinates are parallel to the planes which form the parallelopipeds, the sums of the second class will have for every positive term an equal negative term, and consequently that these sums will vanish.

Fig. 1.



If the molecules of the æther and those of a transparent body form a compound vibrating medium, and if the observations just made be regarded as having reference only to the molecules of the body, the consequences will still be the same. (See paper at p. 270 of vol. viii.)

(3.) Since no light can be discovered to arise from the displacements ξ *, we will neglect them, and then the equations (3.) of the paper at p. 7 of vol. viii. become

$$\frac{d^2\eta}{dt^2} = m\,\Sigma\left\{\phi(r)\,\Delta\eta + \psi(r)\,(\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\,\Delta y\right\},$$

$$\frac{d^2\zeta}{dt^2} = m\,\Sigma\left\{\phi(r)\,\Delta\zeta + \psi(r)\,(\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\,\Delta z\right\}. \qquad (1.)$$

Putting

$$\Delta\eta = \frac{d\eta}{dx}\,\Delta x + \frac{d^2\eta}{dx^2}\cdot\frac{\Delta x^2}{2} + \&c.$$

$$\Delta\zeta = \frac{d\zeta}{dx}\,\Delta x + \frac{d^2\zeta}{dx^2}\cdot\frac{\Delta x^2}{2} + \&c.$$

and substituting these values in the previous equations, it is obvious that the principal sum of the second class (art. 1) is $\Sigma\,.\,\psi(r)\,\Delta x^2\,\Delta y\,\Delta z$. Now y and z may be taken in any directions which are perpendicular to x and to one another; and if the directions be so chosen as to make this sum vanish, we may neglect the other sums of this class, and then the substitution will transform the equations into the second and third of the equations (2.) at p. 272, and the velocities of the waves will be determined by the formulæ previously deduced in the paper at p. 500.

(4.) Suppose then $x_{\prime}, y_{\prime}, z_{\prime}$, to be rectangular coordinates, and the axis of $x_{\prime}$ to be fixed in the medium; suppose the plane of x and y to coincide with this axis; and suppose the molecules to be so arranged that the turning of the coordinates round it would not sensibly affect the values of the sums. In

* Airy's Math. Tracts, p. 340, art. 101.

this case the sum $\Sigma . \psi(r) \Delta x^2 \Delta y \Delta z$ (art. 3) will vanish: for let the plane of x and y pass through the molecule m and divide its sphere of influence into two hemispheres; then, since the arrangement of the molecules will, by the supposition, be sensibly the same in both, it follows that the terms of this sum will be half of them positive and half negative, and will destroy each other.

(5.) If we denote the length of the waves $\frac{2\pi}{k}, \frac{2\pi}{k_{,}}, \frac{2\pi}{k_{,,}}$, by $\lambda, \lambda_{,}, \lambda_{,,}$; and their velocities $\frac{n}{k}, \frac{n_{,}}{k_{,,}}, \frac{n_{,,}}{k_{,,}}$, by $v, v_{,}, v_{,,}$; the equations (3.) of the paper at p. 500, give

$$v = s\sqrt{\left(1 - \frac{s'^2}{s^2} \cdot \frac{4\pi^2}{\lambda^2} + \&c.\right)},$$

$$v_{,} = s_{,}\sqrt{\left(1 - \frac{s_{,}'^2}{s_{,}^2} \cdot \frac{4\pi^2}{\lambda_{,}^2} + \&c.\right)}, \qquad (2.)$$

$$v_{,,} = s_{,,}\sqrt{\left(1 - \frac{s_{,,}'^2}{s_{,,}^2} \cdot \frac{4\pi^2}{\lambda_{,,}^2} + \&c.\right)}$$

(6.) Since our object is only to ascertain the forms of the wave-surfaces, we will, for the present, neglect the terms in these equations which depend upon the lengths of the waves, and suppose $v_{,} = s_{,}$, $v_{,,} = s_{,,}$; then, by the formulæ at p. 271, we have

$$\left.\begin{aligned} v_{,}^2 &= \frac{m}{2} \Sigma . (\phi(r) + \psi(r) \Delta y^2) \Delta x^2, \\ v_{,,}^2 &= \frac{m}{2} \Sigma . (\phi(r) + \psi(r) \Delta z^2) \Delta x^2. \end{aligned}\right\} \qquad (3.)$$

Now, let the axis of z coincide with that of $z_{,}$, and let θ be the angle formed by the axes of x and $x_{,}$; then, when x, y, z, and $x_{,}$, $y_{,}$, $z_{,}$, have the same origin, and are coordinates of the same molecule, we have, by the principles of analytical geometry,

$$x = x_{,} \cos\theta - y_{,} \sin\theta,$$
$$y = x_{,} \sin\theta + y_{,} \cos\theta,$$
$$z = z_{,};$$

and consequently,

$$\Delta x = \Delta x_{,} \cos\theta - \Delta y_{,} \sin\theta,$$
$$\Delta y = \Delta x_{,} \sin\theta + \Delta y_{,} \cos\theta,$$
$$\Delta z = \Delta z_{,}.$$

The last equations give

$$\Delta x^2 = \Delta x_{,}^2 \cos^2\theta + \Delta y_{,}^2 \sin^2\theta - 2 \Delta x_{,} \Delta y_{,} \sin\theta \cos\theta,$$
$$\Delta z^2 = \Delta z_{,}^2;$$

and if we substitute these values in the second of the equations (3.), we have

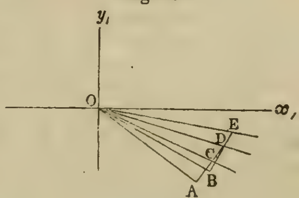
$$v_{\prime\prime}^2 = \frac{m}{2}\left\{\cos^2\theta\,\Sigma\,.\,(\phi(r)+\psi(r)\,\Delta z_\prime^2)\,\Delta x_\prime^2+\sin^2\theta\,\Sigma\right.$$
$$\left.\cdot(\phi(r)+\psi(r)\,\Delta z_\prime^2)\,\Delta y_\prime^2\right\}$$
$$-m\sin\theta\cos\theta\,\Sigma\,.\,(\phi(r)+\psi(r)\,\Delta z_\prime^2)\,\Delta x_\prime\,\Delta y_\prime\,.$$

The third sum in this equation must be zero in consequence of the supposed arrangement of the molecules round the axis of $x_\prime$, and therefore, if we denote by $c_\prime^2$ and $c_{\prime\prime}^2$ the products of the first and second sums multiplied by $\frac{m}{2}$, we have

$$v_{\prime\prime} = \sqrt{(c_\prime^2\cos^2\theta + c_{\prime\prime}^2\sin^2\theta)}\,. \qquad (4.)$$

(7.) Let C D, D E, (fig. 2,) be elementary portions of a wave-surface diverging from the centre of agitation O; let A D, B E, be planes coinciding with C D, D E; and let O A, O B, be perpendiculars to these planes. Then the velocity with which the wave is, at C D, transmitted in the direction perpendicular to C D, must be equal to the velocity of a plane wave moving in the direction of O A; and the velocity with which the wave is, at D E, transmitted in the direction perpendicular to D E, must be the same as that of a plane wave moving in the direction of O B. Consequently, if we conceive an indefinite number of plane waves, which, at the commencement of the time t, all pass through the centre of agitation O, the wave surface will be that touched by all these plane waves at any instant.

Fig. 2.



(8.) Now let a number of planes like A D and B E, all perpendicular to the plane of the angle $x_\prime\,O\,y_\prime$, be so drawn that their perpendicular distances from O, the origin of the coordinates, may be proportional to the values of $v_{\prime\prime}$ given by the equation (4.); where θ is the angle which the perpendicular x drawn from O to any plane wave makes with the axis O $x_\prime$. Then the curve in the plane of $x_\prime\,O\,y_\prime$ touched by all these planes will, by the property of the equation (4.), be an ellipse, the axes of which are proportional to $c_\prime$ and $c_{\prime\prime}$.

(9.) The phænomena of chemistry show that molecular attractions and repulsions vary rapidly at particular distances of the molecules from each other. Suppose then the forces $m\,f(r)$, of the paper at p. 7, to vary rapidly at particular values of r. The differential coefficients $\frac{d f(r)}{d r}$ may, in consequence, become, for these values, so large as to make the parts of the sums Σ which contain them so much greater than the other parts, that the latter may be neglected. Accordingly we will assume this to be the case; and then the first of the equations (3.) becomes

$$v_{\prime}^{2} = \frac{m}{2} \Sigma \,.\, \psi(r) \Delta y^2 \Delta x^2 .$$

This equation, being symmetrical with respect to x and y, gives for $v_{\prime}$ the same value whether x coincides with $O x_{\prime}$ or with $O y_{\prime}$ (fig. 2). We shall therefore assume that $v_{\prime}$ is sensibly the same for all values of θ. And then if we put

$$\frac{m}{2} \Sigma \,.\, (\phi(r) + \psi(r) \Delta y_{\prime}^2) \Delta x_{\prime}^2 = c^2$$

we have $$v_{\prime} = c. \qquad (5.)$$

(10.) Now conceive a number of plane waves, perpendicular to the plane of $x_{\prime} O y_{\prime}$, (fig. 2,) all of which, at the commencement of the time t, pass through the centre O; and, since $v_{\prime}$ is the same for all values of θ, conceive the velocities of these waves to be all equal; then their distances from the centre O will constantly be equal, and the curve, in the plane of $x_{\prime} O y_{\prime}$, touched by all of them at any instant will be a circle.

(11.) If the system of coordinate planes be turned on the axis of $x_{\prime}$ the circle and ellipse (art. 10 and 8) will describe a sphere and spheroid. And since this turning of the coordinates will not, by the supposition (art. 4), sensibly affect the values of the sums, and consequently not alter those of $v_{\prime}$ and $v_{\prime\prime}$, it follows that the agitation at the centre O will in general produce two sets of waves; of which one set will be spheroidal, and the other spherical: the vibrations in the spheroidal waves being perpendicular to the axis of $x_{\prime}$, and the vibrations in the spherical waves perpendicular to those in the spheroidal.

(12.) From the supposed arrangement of the molecules round the axis of $x_{\prime}$ it follows (art. 6 and 9,) that $c = c_{\prime}$, and consequently that when θ is zero we have $v_{\prime} = v_{\prime\prime}$. Hence by limiting our view to a spherical and spheroidal wave, both of which emanate from the centre of agitation at the same instant, we perceive that they will constantly coincide along the axis of $x_{\prime}$. And when θ is a right angle we have $v_{\prime\prime} = c_{\prime\prime}$, which shows that the spherical wave will include, or be included by, the spheroidal wave, accordingly as c is greater or less than $c_{\prime\prime}$.

By referring to Professor Airy's Mathematical Tracts, p. 346—350, it will be seen that the results obtained in this and the preceding article are sufficient to explain the principal optical phænomena presented by what are called uniaxal crystals. The Professor, following, as I suppose, the method of M. Fresnel, has deduced results similar to these, except that, by his reasoning, the direction of vibration in the spherical waves is the same as we have found it to be in the spheroidal, and the converse. But I apprehend that the 104th and 111th articles of his valuable tract on this subject are inconsistent with each other; and that the latter, on which the question depends, is erroneous.

I perceive, from the British Association's Report on Physical Optics, that the investigations of M. Cauchy give, for the direction of vibration, the same result as mine; but it appears that his investigations are not founded so immediately as mine upon the physical constitution of the medium.

(13.) The deductions of the last two articles rest upon the supposition (art. 6.) that $v_{\prime} = s_{\prime}$, $v_{\prime\prime} = s_{\prime\prime}$; but by the equations (2.),

$$v_{\prime} = s_{\prime} \cdot \sqrt{\left(1 - \frac{s'^2_{\prime}}{s^2_{\prime}} \cdot \frac{4\pi^2}{\lambda^2_{\prime}} + \&c.\right)},$$

$$v_{\prime\prime} = s_{\prime\prime} \cdot \sqrt{\left(1 - \frac{s'^2_{\prime\prime}}{s^2_{\prime\prime}} \cdot \frac{4\pi^2}{\lambda^2_{\prime\prime}} + \&c.\right)};$$

hence these deductions require a corresponding modification. But this is easily effected; for we may suppose the variations of the sums s^2, $s^2_{\prime}$, $s^2_{\prime\prime}$, s'^2, $s'^2_{\prime}$, $s'^2_{\prime\prime}$, &c. depending upon the directions of the coordinates, to be in general small fractions of the sums themselves. Hence, in the last equations, we may regard $\frac{s'^2_{\prime}}{s^2_{\prime}}$, $\frac{s'^2_{\prime\prime}}{s^2_{\prime\prime}}$ as constant, and (paper at p. 270 of vol. viii.,) equal one to the other; and thus, if we put $\frac{s'^2_{\prime}}{s^2_{\prime}}$ $4\pi^2 = A$, we have, instead of the values of $v_{\prime}$ and $v_{\prime\prime}$ found in articles 9 and 6,

$$\left.\begin{aligned} v_{\prime} &= c \cdot \sqrt{\left(1 - \frac{A}{\lambda^2_{\prime}} + \&c.\right)} \\ v_{\prime\prime} &= \sqrt{\left(c^2_{\prime} \cos^2\theta + c^2_{\prime\prime} \sin^2\theta\right)} \cdot \sqrt{\left(1 - \frac{A}{\lambda^2_{\prime\prime}} + \&c.\right)}. \end{aligned}\right\} \quad (6.)$$

It appears by these equations that the values of $v_{\prime}$ and $v_{\prime\prime}$, and the ratio of one of them to the other, depend, in some measure, upon $\lambda_{\prime}$, $\lambda_{\prime\prime}$, the lengths of the waves. This is confirmed by experience. See Airy's Tracts, p. 354.

(14.) It is well known that light moves through glass, in its ordinary state, with the same velocity in all directions;

and that the velocity is not affected by any change in the direction of the vibrations: consequently the sums s^2, s_i^2, &c. must, for this medium, be the same whatever be the directions of the coordinates. But it is found, by experiment, that if glass be expanded or contracted in one direction only, it exhibits the same optical phænomena as an uniaxal crystal; the optical axis lying in the direction of the expansion or contraction. (Airy's Tracts, p. 403, art. 178.) Now, it is manifest that since the sums s^2, s_i^2, &c. are originally the same for all directions of the coordinates, these sums must, in the altered state of the glass, be still such that their values will not be affected by turning the coordinates upon an axis taken in the direction of the expansion or contraction; and consequently this experiment affords a verification of our formulæ.

(15.) Whatever be the arrangement of the molecules, we have, by the equations (3.) and the assumption of article (9.),

$$v_i^2 = \frac{m}{2}\,\Sigma\,.\,\psi(r)\,\Delta x^2\,\Delta y^2,$$

$$v_{ii}^2 = \frac{m}{2}\,\Sigma\,.\,\psi(r)\,\Delta x^2\,\Delta z^2;$$

provided (art. 3.) the directions of y and z are so taken that

$$\Sigma\,.\,\psi(r)\,\Delta x^2\,\Delta y\,\Delta z = 0.$$

Let x_i', y_i', z_i' be rectangular coordinates having fixed directions, and the same origin as x, y, z; let the axis of x coincide with that of x_i'; and let θ_i' be the angle between y_i' and y: then

$$\begin{aligned}
\Delta x &= \Delta x_i',\\
\Delta y &= \Delta y_i' \cos\theta_i' - \Delta z_i' \sin\theta_i',\\
\Delta z &= \Delta y \sin\theta_i' + \Delta z_i' \cos\theta_i'.
\end{aligned}$$

By substituting these values and, for the sake of abridgement, putting

$$\begin{aligned}
\Sigma\,.\,\psi(r)\,\Delta x_i'^2\,\Delta y_i'^2 &= \sigma,\\
\Sigma\,.\,\psi(r)\,\Delta x_i'^2\,\Delta z_i'^2 &= \sigma',\\
\Sigma\,.\,\psi(r)\,\Delta x_i'^2\,\Delta y_i'\,\Delta z_i' &= \sigma'',
\end{aligned}$$

we have

$$v_i^2 = \frac{m}{2}\,\sigma\cos^2\theta_i' + \frac{m}{2}\,\sigma'\sin^2\theta_i' - m\,\sigma''\sin\theta_i'\cos\theta_i',$$

$$v_{ii}^2 = \frac{m}{2}\,\sigma\sin^2\theta_i' + \frac{m}{2}\,\sigma'\cos^2\theta_i' + m\,\sigma''\sin\theta_i'\cos\theta_i',$$

$$\Sigma\,.\,\psi(r)\,\Delta x^2\,\Delta y\,\Delta z = (\sigma-\sigma')\sin\theta_i'\cos\theta_i' + \sigma''(\cos^2\theta_i' - \sin^2\theta_i'.$$

From these equations we find

$$\frac{d\,(v_{,}^{2})}{d\,\theta_{,}'} = -\frac{d\,(v_{,,}^{2})}{d\,\theta_{,}'} = -\,m\,\Sigma\,.\,\psi\,(r)\,\Delta\,x^{2}\,\Delta\,y\,\Delta\,z:$$

so that when

$$(\sigma-\sigma')\sin\theta_{,}'\cos\theta_{,}' + \sigma''\,(\cos^{2}\theta_{,}' - \sin^{2}\theta_{,}') = 0,$$

the sum $\Sigma\,.\,\psi\,(r)\,\Delta\,x^{2}\,\Delta\,y\,\Delta\,z$ is zero as required, while, of the expressions for $v_{,}^{2}$ and $v_{,,}^{2}$, one is a maximum and the other a minimum. The last equation is always possible; for since $\sin\theta_{,}'\cos\theta_{,}' = \frac{\sin 2\,\theta_{,}'}{2}$, and $\cos^{2}\theta_{,}' - \sin^{2}\theta_{,}'$, $= \cos 2\,\theta_{,}'$, it gives

$$\tan 2\,\theta_{,}' = \frac{2\,\sigma''}{\sigma'-\sigma}.$$

(16.) It has been observed (art. 1,) that the sums composed of even products involving odd powers of the differences must, in general, be very small compared with the sums composed of products of the same degree in which the powers of the differences are all even. Let it then be supposed that x', y', z' are rectangular coordinates of which the axes are fixed in the medium; and that the arrangement of the molecules, with respect to these axes, is such that the sums of which the terms involve odd powers of the differences $\Delta\,x'$, $\Delta\,y'$, $\Delta\,z'$, are either zero (art. 21,) or insensibly small. Let the axis of $x_{,}$ coincide with that of x'; and let θ' be the angle between y' and $y_{,}$; then

$$\begin{aligned}\Delta\,x_{,} &= \Delta\,x',\\ \Delta\,y_{,} &= \Delta\,y'\cos\theta' - \Delta\,z'\sin\theta',\\ \Delta\,z_{,} &= \Delta\,y'\sin\theta' + \Delta\,z'\cos\theta';\end{aligned}$$

and consequently, when we omit the terms involving the odd powers of the differences $\Delta\,x'$, $\Delta\,y'$, $\Delta\,z'$, we have

$$\Delta\,x_{,}^{2}\,\Delta\,z_{,}^{2} = \Delta\,x'^{2}\,\Delta\,y'^{2}\sin^{2}\theta' + \Delta\,x'^{2}\,\Delta\,z'^{2}\cos^{2}\theta'.$$

For the reasons mentioned in article (9.) we leave out of the expression for $v_{,,}^{2}$ (art. 6.) the function $\phi\,(r)$, and then

$$v_{,,}^{2} = \cos^{2}\theta\,.\,\frac{m}{2}\Sigma\,.\,\psi\,(r)\,\Delta\,z_{,}^{2}\,\Delta\,x_{,}^{2} + \sin^{2}\theta\,.\,\frac{m}{2}\Sigma\,.\,\psi\,(r)\,\Delta\,z_{,}^{2}\,\Delta\,y_{,}^{2}$$

$$-\sin\theta\cos\theta\,.\,m\,\Sigma\,.\,\psi\,(r)\,\Delta\,z_{,}^{2}\,\Delta\,y_{,}\,\Delta\,x_{,}\,.$$

Now, when the coordinates $x_{,}$, $y_{,}$, $z_{,}$ are turned on the common axis of $x_{,}$ and x', the sum $\Sigma\,.\,\psi\,(r)\,\Delta\,z_{,}^{2}\,\Delta\,y_{,}^{2}$ must be of the same value, whether $y_{,}$ coincide with y' or z'; we will therefore suppose it to be sensibly the same for all values of θ': when, again, we change the coordinates $x_{,}, y_{,}, z_{,}$, for x', y', z', the last sum in the equation will be composed of terms involving odd powers of $\Delta\,x'$, and will therefore, by the supposition, be insensible; hence we shall have, by substituting for $\Delta x_{\prime}^2\, \Delta z_{\prime}^2$ its value previously found,

$$v_{\prime\prime}^2 = \cos^2\theta \,.\, \frac{m}{2}\, \Sigma \,.\, \psi(r)\, (\Delta x'^2\, \Delta y'^2 \sin^2\theta' + \Delta x'^2 \Delta z'^2 \cos^2\theta')$$

$$+ \sin^2\theta \,.\, \frac{m}{2}\, \Sigma \,.\, \psi(r)\, \Delta z'^2\, \Delta y'^2 ;$$

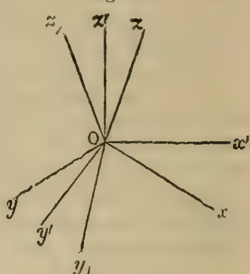
or, $\quad v_{\prime\prime}^2 = c^2 \cos^2\ \ \sin^2\theta' + c'^2 \cos^2\theta \cos^2\theta' + c''^2 \sin^2\theta$;
if, for the sake of abridgement, we put

$$\frac{m}{2}\, \Sigma \,.\, \psi(r)\, \Delta x'^2\, \Delta y'^2 = c^2, \quad \frac{m}{2}\, \Sigma \,.\, \psi(r)\, \Delta x'^2\, \Delta z'^2 = c'^2 ,$$

$$\frac{m}{2}\, \Sigma \,.\, \psi(r)\, \Delta y'^2\, \Delta z'^2 = c'^2 .$$

(17.) Let $O x'$ (fig. 3,) be the common axis of x' and $x_{\prime}$; $O y'$, $O z'$, $O y'_{\prime}$, $O z'_{\prime}$, $O x$, $O y$, $O z$, the axes of y', z', $y_{\prime}$, $z_{\prime}$, x, y, z; $y' O y_{\prime} = \theta'$, and $x' O x = \theta$. Now $O x$ being, by the supposition of art. 6, in the plane of $x' O y_{\prime}$, we will suppose $O y$ to be also in the same plane. Then $y_{\prime} O y = x' O x = \theta$, $\cos x' O y = \sin\theta$, $\cos y' O y = \cos\theta$, $\cos\theta'$, $\cos z' O y = \cos\theta \sin\theta'$; and thus, by the last expression for $v_{\prime\prime}^2$, we arrive at

Fig. 3.



$$v_{\prime}^2 = c^2 \cos^2 z' O y + c'^2 \cos^2 y' O y + c''^2 \cos^2 x' O y .$$

In deducing this equation we have supposed $O x'$, $O x$, $O y$, to be in the same plane; but we take for granted that the value of v''^2 would not be sensibly affected by turning the coordinates upon the axis of y; because

$v_{\prime\prime}^2 = \frac{m}{2}\, \Sigma \,.\, \psi(r)\, \Delta x^2\, \Delta z^2$, and this sum, being symmetrical with respect to x and z, retains the same value when x and z are interchanged. Hence it follows that the equation is true in general, and, consequently, that if we change the angles $z' O y$, $y' O y$, $x' O y$, for $z' O z$, $y' O z$, $x' O z$, it gives also the value of $v_{\prime}^2$.

(18.) It will be remembered that $v_{\prime\prime}$ is the velocity of waves moving in the direction of $O x$, and consisting of vibrations in the direction of $O z$; and that v' is the velocity of waves moving in the same direction, and consisting of vibrations in the direction of $O y$. The directions of $O y$ and $O z$, which determine those of the vibrations must (art. 15.) be so taken at right angles to each other and to $O x$, that of the expressions for $v_{/}^2$ and $v_{//}^2$, one shall be a maximum, the other a minimum.

If the last expression for $v_{//}^2$, and the observations subsequently made, be compared with the expression in art. 119, and the observations in articles 120 and 121 of Professor Airy's Tract before quoted, it will be perceived that we have now deduced the fundamental laws of M. Fresnel's theory of refraction for biaxal crystals. But the direction of vibration is, as we have previously found in the case of uniaxal crystals, perpendicular to that which this ingenious philosopher supposed. The consequences of these laws have, as it appears from the British Association's Report on Physical Optics, been so ably traced and verified by Sir William Rowan Hamilton and others, that I deem it unnecessary to pursue this part of the theory any further.

I am, Gentlemen, yours, &c.,

Evesham, June 28, 1836. JOHN TOVEY.

P.S. In my last paper, vol. viii., p. 501, line 2 from the bottom, *for* 1 *read* ξ ; p. 502, l. 8 from the bottom, *dele* comma after β; p. 505, l. 11, *for* $\frac{a}{x}$ *read* $\frac{\alpha}{x}$.

In the valuable paper from M. Cauchy in your last [June] Number, I have noticed the following errors. Vol. viii. p. 461, formula (2.), for $a u + b v + c w$ read $a u_1 + b v_1 + c w_1$; and line 5 from bottom, for w read u. P. 462, formula (7.), for ε_n in the denominator read u_n. P. 463, l. 13, *prefix* of; and line 3 from the bottom, *for* 0 *read* a. P. 464, line 14, for u_i read v_i; and l. 15, *for* we *read* We. P. 465, l. 12, for 19 read 16; and l. 28, for $a w$ read $a u$. P. 466, l. 13, *for* C *read* β. P. 467, lines 1 and 3, for $= v$, in every place, read $= 0$; and line 10 *dele* the first w.

LXXX. *On Meteoric Stones. By Professor* BERZELIUS.*

THE author commences this interesting memoir by considering which of the conjectures respecting the formation of meteoric stones is the most probable. That which refers these bodies to eruptions of the volcanos of our earth cannot be supported, on account of the distance of the places where they have fallen from any volcano, and also from the different constitution of volcanic products and meteorites; neither can

* From the *Journal de Pharmacie* for February 1836: communicated by J. D. Smith, Esq., being a translation of an extract, by M. Vallet, from a memoir in Poggendorf's *Annalen der Physik und Chemie*, vol. xxxiii. p. 1.

the opinion of their formation from either the common, or even the accidental constituents of the atmosphere be admitted. Anaxagoras imagined that a stone which fell in his time in Ægos Potamos came from another world. This, which is probably a correct opinion, is supported by the researches of our own age. Olbers in a paper on the fall of a meteorite which occurred at Sienna in Italy, on the 16th of July 1794*, suggested, in 1795, the possibility of these bodies being projected from the moon, but it appeared to him much more likely that they came from Vesuvius. Laplace likewise adopted this opinion in 1802. That part of the moon which is turned towards us is covered with elevations, and it is found that there are many mountains which precisely resemble in their external appearance those volcanos of our earth which have craters; these mountains are of such magnitude that the interior of their craters may be seen with good telescopes; and it can be readily perceived that one half of the interior is illuminated by the sun, and the other is in the shade, whilst the circular opening of the crater is extremely distinct. It may then be supposed that these mountains owe their form to the same cause as terrestrial volcanos, viz. to eruptions; but if the force which produces lunar eruptions is as considerable as the projectile force of our volcanos, the bodies thrown out ought to be projected much further from the moon than the earth; for, 1st, the mass of the moon is to that of the earth only as 1·45 to 100, and its weight is in the same ratio; 2ndly, the moon has no atmosphere, or at most one so highly rarefied that when the fixed stars are eclipsed by the moon, no refraction of the rays of light can be perceived: consequently the projection occurs *in vacuo*, and without that mechanical resistance to projected bodies which is caused by the atmosphere of the earth, in which they soon become quiescent; 3rdly, if a body is projected towards the earth from the moon the attraction of the earth for it continually increases, whilst that of the moon diminishes more and more; 4thly, the limit of equilibrium between the earth and the moon is much nearer the latter than the former.

Many circumstances connected with the composition of meteoric stones agree with what we know respecting the moon. Some of these bodies contain metallic iron, which when exposed to air and moisture is by degrees converted into hydrated peroxide of iron, and this is the case with the minerals of the crust of our globe under such circumstances; therefore in their primitive situation they are without atmo-

* [The fall at Sienna in 1794 was of a number of meteoric stones.—E. W. B.]

spheric air, or even possibly without either air or moisture.

Astronomical researches have not as yet discovered in the moon any traces of water large enough to be distinguished by good glasses, and M. Berzelius considers that water has not been met with chemically combined in meteoric stones. We shall see hereafter that the greater number of meteoric stones resemble each other so much in their composition that they may be considered to come from the same mountain, that is, from the central culminating point of that side of the moon which is always turned towards the earth. A small number only present a different appearance, and it is therefore probable that these proceed from mountains situated on other parts of the moon.

Nevertheless meteoric stones may have their origin in another planet. Olbers considers that the asteroids between Mars and Jupiter may be fragments arising from the destruction of a larger planet, an idea which has induced the search for more of these fragments, and the discovery of one of them by Olbers himself. If such a catastrophe has occurred, which seems established by the great angle that the course of Pallas makes with that of the other planets, an immense number of small fragments would be projected in such directions that their course around the sun being diminished, they would then during their revolution come within the sphere of attraction of other planets, and fall on them. From these preliminary considerations on the origin of meteoric stones, M. Berzelius proceeds to the chemical examination of many of them. Unable in this place to notice the processes which he employed in his analyses, we confine ourselves to the results at which he has arrived, and the conclusions which he has drawn from them.

I. *The Meteorite of Blansko.*—This stone, which induced M. Berzelius to undertake this work, fell at a quarter past six on the evening of the 25th of November 1833, in the neighbourhood of Blansko in Moravia. As usual it produced a very brilliant light, and its flight was preceded by noise resembling thunder. M. Reichenbach, who witnessed the phænomenon, could only collect a few fragments; the principal mass has not yet been discovered, the surrounding country being thickly wooded*.

It resembles those meteoric stones which are most commonly met with, and may therefore be ranked with those of Benares, L'Aigle, Berlongville, &c. One portion is magnetic, the other is not so; this latter part is but partially soluble in acids: that which was dissolved gives in one hundred parts:

* [See Lond. and Edinb. Phil. Mag., vol. vi. p. 159.]

Silica	33·084
Magnesia	36·143
Protoxide of iron	26·935
Protoxide of manganese	0·465
Oxide of nickel mixed with tin and copper	0·465
Alumina	0·329
Soda	0·857
Potash	0·429
Loss	1·273
	100·000

The insoluble part of the non-magnetic portion having been analysed, part by carbonate of barytes and part by carbonate of soda, affords results which slightly differ:

	Carb. Barytes.	Carb. Soda.
Silica	57·145	57·012
Magnesia	21·843	24·956
Lime	3·106	1·437
Protoxide of iron	8·592	8·362
Protoxide of manganese	0·724	0·557
Oxide of nickel mixed with tin and copper	0·021	
Alumina	5·590	4·792
Soda	0·931	
Potash	0·010	
Chromium and iron mixed with tin	1·533	1·306
Loss	0·505	1·579
	100·000	100·000

The small globules which are commonly met with in meteoric stones, which Howard had already observed, and endeavoured to analyse, were not attracted by the magnet. On examining these globules M. Berzelius found, as had also Howard, that they were not a different description of mineral from the meteorite itself (or that they do not differ from the meteorite in which they occur).

The magnetic portion, or the meteoric iron, consisted of,

Iron	93·816
Nickel	5·053
Cobalt	·347
Tin and copper	·460
Sulphur	·324
Phosphorus, a trace	
	100·000

The meteorite of Blansko may be considered in a mineralogical point of view as composed of,

An alloy of iron and nickel, containing cobalt, tin, copper, sulphur, and phosphorus	17·15
Of a silicate of magnesia and protoxide of iron, in which the silica contains as much oxygen as the bases, with a little sulphuret of iron	42·67
Of silicate of magnesia and protoxide of iron, mixed with silicates of potash, soda, lime, and alumina, in which the silica contains twice as much oxygen as the bases	39·43
Of chrome and iron mixed with tin-stone	·75

It can hardly be doubted that the relative quantities of the constituent parts of this mixture vary in different fragments of the stone*.

Meteoric Stone of Chantonnay.—This stone fell at two o'clock in the morning of the 5th of August 1812, not far from Chantonnay, in the department of La Vendée: a fragment was sent to M. Berzelius by the late M. Lucas, a French mineralogist. It is not affected by the magnet, and, like the non-magnetic portion of the Blansko meteorite, contains in 100 parts, 51·12 parts soluble in acids, and 48·88 parts insoluble in these agents. The portion dissolved contained:

Silica	32·607
Magnesia	34·357
Protoxide of iron	28·801
Protoxide of manganese	·821
Oxide of nickel combined with oxide of copper and tin	·456
Potash and soda	·977
Loss	1·971
	100·

The portion insoluble in acids is composed of

Silica	56·252
Magnesia	20·396
Lime	3·106
Protoxide of iron	9·723
Protoxide of manganese	·690
Oxide of nickel with oxide of tin and copper	·138
Alumina	6·025
Soda	1·000
Potash	·512
Chrome and iron	1·100
Loss	1·070
	100·

* In this abstract no notice is taken of the protoxide of manganese mentioned in the analyses of the non-magnetic portion.—J. D. S.

M. Berzelius is convinced by later researches, that in this last analysis the quantity of the alloy of chromium and iron ought to be increased to 1·7 per cent., and it also contains about one tenth per cent. of oxide of tin.

Meteorite of Loutolox.—This stone fell on the 13th of December 1813, near the village of Lontalax in Finland. It has been described by Nordenskiöld, who presented a fragment of it to M. Berzelius. The magnetic portion is composed of deutoxide of iron (*oxide ferroso-ferrique*); the remainder affords by analysis:

	In the whole quantity.	In 100 parts of the soluble portion.
Silica	42·5	37·411
Magnesia	34·4	32·922
Protoxide of iron	32·5	28·610
Protoxide of manganese	·9	·793
Alumina	·3	·264
Oxides of copper and tin, a trace.		
Potash and soda	a trace.	
Insoluble	7·9	
	121·5	100·

From this analysis it may be concluded, that the portion soluble in acids is a silicate of magnesia and protoxide of iron, probably in reciprocally variable proportions, but in which the silica contains as much oxygen as the bases. The mineral here analysed gives plainly enough the formula f S + 2 M S; nevertheless there is reason to suppose that the atomic proportion is accidental, and that meteoric olivine contains these isomorphous silicates in variable proportions. The insoluble part, which is equal to 6·37 per cent. of the weight of the stone, afforded about one per cent. of the alloy of chromium and iron, mixed with oxide of tin, magnesia, lime, protoxide of iron, alumina, and protoxide of manganese, in proportions which appear to indicate that the insoluble portion of this stone has the same composition as the preceding meteorites.

Meteoric Stone of Alais.—This stone, which fell near Alais in France on the 15th of March 1806, at half-past five in the afternoon, differs from all the others: it resembles indurated clay and falls to pieces in water, emitting an argillaceous odour. M. Thenard who first examined it, found, besides the general constituents of meteoric stones, some carbon; this fact was afterwards confirmed by Vauquelin. A small specimen sent to M. Berzelius by M. Lucas, has afforded to this skilful chemist the opportunity of examining it. One portion (12 per cent.) is attracted by the magnet, which he found was composed of a minute quantity of metallic iron, a little sulphuret of iron, but chiefly of the deutoxide of iron (*oxide ferroso-ferrique*). This stone when treated with water afforded some organic matter, and 10 per cent. of a salt which contained no iron, being a mixture of the sulphates of nickel, magnesia, soda, potash, and lime, with a trace of sulphate of ammonia. The meteorite deprived of its soluble constituents and dried at 212° Fahr., was heated to redness in a small distillatory apparatus, and the disengaged gas passed into an inverted flask filled with lime-water. This operation afforded,

Black residue	88·146
Gray-brown sublimate	·944
Carbonic acid	4·328
Water	6·582

138·2 parts of the black residue gave by analysis,

Silica	43·15
Magnesia	30·70
Lime	·32
Protoxide of iron	40·11
Oxide of nickel	1·90
Protoxide of manganese	·36
Alumina	3·25
Chromium and iron	·87
Oxide of tin mixed with copper	1·10
Insoluble carbonaceous residue	12·00
Loss	4·44
	138·20

The insoluble carbonaceous residue was composed of

Carbon	2·586
Chromium mixed with oxide of tin	·525
Magnesia	·500
Protoxide of iron	2·660
Oxide of nickel	·550
Alumina	·250
Oxide of tin	·200
Silica	4·620

It contained no lime: the magnesia was mixed with a trace of protoxide of manganese, and the oxide of nickel with a trace of cobalt. It is therefore evident that the Alais meteorite is not of the same nature as the foregoing ones. Neither can it be considered as merely a lump of earth. The presence of metallic iron and its sulphuret, and of the oxides of nickel, cobalt, tin, copper, and chromium, which occur in it, proves that this earth has been formed from the usual meteoric mass, which was in this place chiefly composed of meteoric olivine.

So that there can be no doubt that this stone, in spite of the difference of its external appearance, is but a meteorite which in all probability had its origin in the same situation as other meteoric stones. The carbon here occurs in the state of a combination, which, when decomposed by heat, affords carbonic acid, either by itself or accompanied by water, and leaves a carbonaceous residue. In the first case the carbon is combined only with oxygen, so as to form a body resembling mellitic acid, but in the latter it is in combination with oxygen and hydrogen. However, there is no other substance known which is not converted into carbon, carbonic acid, and water.

It will be perceived by the preceding analyses that the results of M. Berzelius differ a little from those of M. Thenard.

Iron and Olivine of Pallas.—This celebrated meteoric mass which Pallas made known in Europe, was discovered lying on the peak of a schistus mountain in Siberia, between Krasnojarsk and Abekansk. Pallas estimated its weight at 1600 pounds: it was chiefly composed of a skeleton of iron resembling a well-risen loaf, the cavities of which are round and close to one another; these are filled with the glassy and greenish olivine, which has been already noticed.

According to the analysis of M. Berzelius the iron of Pallas freed from olivine by the hammer, is composed of

Iron	88·042
Nickel	10·732
Cobalt	·455
Magnesium	·050
Manganese	·132
Tin and copper	·066
Carbon	·043
Sulphur	a trace
Insoluble residue	·480
	100·000

This insoluble residue is an extremely interesting part of the meteoric iron, it being precisely the same combination of phosphorus and iron that M. Berzelius has already analysed and described in his Researches on the Meteoric Iron of Bohumiliz; it is composed of

Iron	48·67
Nickel	18·33
Magnesium	9·66
Phosphorus	18·47
Loss	4·87
	100·00

M. Berzelius has also found that the iron of Pallas dissolved with heat in an acid slightly diluted, it left, after the solution had become strongly saturated with a neutral salt of iron, a skeleton, of the form of the iron, black, very light and porous, 100 parts of which are composed of

Iron	57·18
Nickel	34·00
Magnesium	4·52
Tin and copper	3·75
Carbon	·55
	100·00

with a slight trace of phosphorus. The presence of magnesium proves that this metal in combination with iron and nickel is less soluble than iron itself.

The olivine of Pallas has been examined by Walmsted and by Stromeyer: the first has found that the composition of this mineral may be exactly indicated by the formula $\left.\begin{matrix}M\,g\\ f\end{matrix}\right\} S$.

The latter, who had met with nickel in other olivines, found, contrary to all conjecture, that the olivine of Pallas contains none, although Howard had already stated it to contain about one per cent. of oxide of nickel.

The results of M. Walmsted have been verified by M. Berzelius, which is seen on comparing the analyses of these chemists:

	Walmsted.	Berzelius.
Silica	40·83	40·86
Magnesia..................	47·73	47·35
Protoxide of iron	11·53	11·72
Protoxide of manganese	0·29	0·43
Oxide of tin		·17
	100·39	100·53

Two terrestrial olivines, one, that of Boscovich in Bohemia, the other, occurring in masses of lava in the department of Puy-de-Dome, have been subsequently examined by M. Berzelius, comparatively with the meteoric olivine of Pallas, and have afforded him, like all the meteoric stones previously examined, oxide of tin mixed with oxide of copper in a quantity equivalent to about 0·2 per cent. of the whole mass.

Meteoric Iron of Elbogen.—This meteorite, of the flight of which no account exists, but which appears to have fallen towards the end of the fourteenth or at the commencement of the fifteenth century, is now preserved at Vienna. M. Berzelius states that the iron of Elbogen is composed of,

Iron	88·231
Nickel	8·517
Cobalt	·762
Magnesium	·279
Metallic phosphurets	2·211
Sulphur and manganese, traces	——
	100·000

The nickel contains tin and copper. The insoluble metallic phosphurets altogether resemble those of the iron of Pallas and Bohumiliz: but it is to those of the latter that they approach the closest in the proportion of the principal constituents; these are

Iron	68·11
Nickel and magnesium	17·72
Phosphorus	14·17

The metallic phosphurets of the Bohumiliz iron are composed of iron 65·977, nickel 15·008, phosphorus 14·023, silica 2·037, and carbon 1·422.

These researches show that meteoric stones are the mixed matrices of many minerals in variable proportions. These minerals are as follows:

1st. *Native Iron.*—This sometimes forms the principal mass of the meteoric stone; but not one of these has fallen, that we know of, since 1802*. Those meteoric stones in which iron is the chief constituent do not fly to pieces in their fall, and consequently the largest meteorites are composed of it: the iron is sometimes in a compact mass, sometimes in rounded portions, large or small; generally it is full of cavities containing a gangue. The iron is mixed with other metals, especially with nickel, of which the quantity does not appear to be constant; it also contains small quantities of cobalt, magnesium, manganese, tin, copper, sulphur, carbon, and sometimes a trace of phosphorus.

2nd. *Sulphuret of Iron.*—This is not the magnetic pyrites, but probably a sulphuret of iron containing an equivalent of each constituent. This accounts for its feeble magnetic polarity, as well as the great facility with which acids decompose it, disengaging hydrosulphuric acid gas. It is intimately mixed with the mass of the meteoric stones: it probably contributes to their dusky colour, and is not readily attracted by the magnet.

* [There is an error here, either in the original or in the French translation: the last fall of meteoric iron known took place at Agram in the year 1751.—E. W. B.]

3rd. *Magnetic Ore of Iron.*—Although the iron in meteoric stones is principally either in the metallic state, or at the minimum of oxidation, nevertheless it is certainly found in the state of deutoxide (*oxide ferroso-ferrique*) in the meteorites of Loutolox and Alais: the magnetic portion of the first is wholly composed of it, and in great part, that of the second.

4th. *Meteoric Olivine.*—This constitutes about half of that which remains after the separation of the magnetic portion; it is decomposed by acids, leaving a siliceous residue. Its formula is exactly the same as common olivine, *i. e.* $\left.\begin{matrix} M \\ f \end{matrix}\right\} S$, in which M and f vary in their relative proportions: it contains as isomorphous substitutes small quantities of the silicates of oxide of nickel and protoxide of manganese, as well as a portion of oxide of tin, like terrestrial olivine: it is worthy of remark that it scarcely ever contains lime.

5th. *Silicates of magnesia, lime, protoxide of iron, protoxide of manganese, alumina, potash, and soda, insoluble in acids*, in which the oxygen of the silica is double that of the bases: these probably constitute another mineral, analogous to pyroxène,

$\left.\begin{matrix} M \\ C \\ f \end{matrix}\right\} S^2$; and another, analogous to leucite, $\left.\begin{matrix} M \\ C \\ N \\ K \end{matrix}\right\} S^2 + 3\,A\,S^2$.

The black crust that covers the surface of meteoric stones is owing to the fusibility of their silicates: these also contribute to the fusion of the olivine, which by itself is infusible.

That which deserves particular attention is, that if meteoric stones were formed of terrestrial olivine and pyroxene, their colour would be green, or even black, in consequence of the higher degree of oxidation of the iron: this is a sufficient proof that the fused black crust has not been formed in the terrestrial atmosphere.

6th. *Alloy of Chromium and Iron.*—It is very remarkable that this mineral so constantly accompanies meteoric stones, considering that it always occurs in such very small quantity. The preceding experiments show that it separates from the meteorites without alteration, yet it is always partially decomposed, when it must be sought for in the separated oxide of iron.

7th. *Ore of Tin.*—The tin of meteoric stones partly proceeds from the native iron they contain, and partly from a small portion of oxide of tin which is disseminated through the chromiron, and which on analysis dissolves or remains undissolved with the latter body. The oxide of tin is mixed with copper.

M. Berzelius considers that a more profound study of meteorites in the point of view from whence he has set out would undoubtedly make known a much larger proportion of their principal constituents.

If we consider meteoric stones as mineralogical specimens, and compare them with those of our earth, we shall find essential differences, even putting out of the question the existence of the native iron. The abundance of magnesia which is in all the chief constituent, the poverty in silica, and the small proportion of the silicates of alumina and of the alkalies, distinguish the meteoric minerals. On this earth it is just the contrary: here silica is the predominant substance, and the silicates of alumina and the alkalies form everywhere its principal constituents. Magnesia is rare.

The fineness of grain and feeble cohesion of meteoric stones would lead one to suppose that they are projected in a fused state, and consequently resemble the products of terrestrial volcanos, yet this does not appear to be the case. If we carefully examine the texture of a large fragment of a meteoric stone, it will be found to be split, and the fissures filled with another kind of mineral, for the most part of a deeper colour, which indicates a slower and calmer formation. If olivine is found amongst the products of terrestrial volcanos, and rarely in other minerals, it is no proof that it must always be a volcanic product. It is infusible, and is found inclosed in volcanic minerals, because it could not be fused with them. On the contrary, in meteoric stones it is so uniformly mixed with the other constituents, that its presence in these is evidently owing to another cause which does not exist in lava and basalt.

The Alais meteorite proves that in their original situation rocks are altered by the influence of some geognostic accident, and are converted into a kind of earth, and that even this mass, resembling olivine and mixed with native iron, constitutes the rock from which it is broken. The presence, in this earth, of salts soluble in water would seem to prove that this phænomenon had occurred either without the presence of water, or in water which contained such large quantities of these salts that they remained after desiccation. The carbonized substance that this earth contains, in a state of mixture, would not authorize the conclusion that in its original habitat, this earthy substance was of an organic nature. This property of the earth appears, more than any other circumstance, to show that these meteoric stones have not been projected in a state of fusion and afterwards cooled, for under such circumstances such a formation could not have occurred.

The preceding remarks apply to the majority of meteoric stones, which may be regarded as having their origin in the same locality; but three of them afford a composition so essentially different from that of the others, that it may be said with certainty that they do not come from the same spot, but originate in another globe, or perhaps from another part of that globe to which we refer the remainder. These three, however, resemble each other so much that we may assign them a common origin. These are the stones which fell at Stannern in Moravia, and at Jonzac and Juvénas in France. The first was examined by Moser and then by Klaproth; the other two by Laugier. They differ from others in not containing native iron, but constitute an agglomeration of evidently separable minerals, as well as in that the particles of the mixture are of a very small size, and that silicate of magnesia enters into their composition in but very small proportion. They contain, on the contrary, besides a little sulphuret of iron, silicates of lime, alumina, and protoxide of iron, and also some chromium. The proportion between the oxygen of the silica and that of the bases is such that the former is more considerable than the latter, but, however, without being double. About a third of their mass (not including the silica) is, according to the analysis made by Laugier of the meteorite of Juvénas, soluble in acids; from which it may be supposed that in the soluble portion the silica and bases contain an equal portion of oxygen, but that in the insoluble part the oxygen of the former is double that of the latter, as in the meteorites already described. G. Rose has carefully examined this species of meteoric stones, and has rendered it probable that they are mixtures of labrador and pyroxene, with a little magnetic pyrites free from nickel, which however, according to his researches, is not attracted by the magnet.

The following analyses by Klaproth and Laugier show the differences which distinguish these three from other meteoric stones:

	Stannern.	Jonzac.	Juvénas.
Silica	48·25	46·00	40·0
Magnesia . . .	2·00	1·60	·8
Lime	9·50	7·50	9·2
Protoxide of iron	23·00	32·40	23·5
Alumina	14·50	6·00	10·4
Oxide of manganese	...	2·80	6·5
Potash	...	...	·2
Oxide of copper. .	...	...	·1
Oxide of chromium	...	1·00	1·0
Sulphur	2·75	15·0	0·5

LXXXI. *On a new Method of preparing Iodous Acid. By* Lewis Thompson, *Esq., Member of the Royal College of Surgeons.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I SEND you a new method of preparing iodic acid; it is cheaper and safer than the process of Sir Humphry Davy, and affords a purer acid than the plan pursued by Gay-Lussac. I say purer, because from some experiments which I have lately made, and intend to repeat more carefully, I am led to conclude with Sir Humphry Davy, that the acid of Gay-Lussac is sulpho-iodic acid.

Process for preparing Iodic Acid.

Put one atom or 126 grains of iodine into a proper bottle with 24 ounces of water, and pass chlorine, previously washed in cold water, through the mixture until it shall have become colourless; set the solution aside for an hour; then heat it to 212° Fahr., to disengage the uncombined chlorine, and add $2\frac{1}{2}$ atoms or 295 grains of recently precipitated oxide of silver; boil the whole for ten minutes, filter, and evaporate carefully to dryness: the product is pure anhydrous iodic acid.

It will be at once perceived by the above process that there is no such acid as the chloriodic, the acid so called being in fact merely a chloride of iodine, which when dissolved in water is converted into muriatic and iodic acids, with a variable quantity of iodine. How this mistake can have passed so long unnoticed is to me a matter of surprise; at the same time I must observe that I have not been able to unite chlorine and iodine in the proportions necessary to form these acids without the intervention of water; there is always an excess of iodine: but I have no doubt that this may be effected in a sufficiently reduced temperature. In the last experiment which I made on this subject 50 grains of iodine combined with 41·5 cubic inches or about 30 grains of chlorine: the substance thus formed when put into a large quantity of water, and exposed for some days to the sunshine, deposited 8 grains of iodine and became of a pale yellow colour.

That the muriatic and iodic acids exist ready formed in the solution I am confident, not only from the taste and smell, but because I have obtained free muriatic acid from it by distillation, although when this is continued until the solution becomes a good deal concentrated, these acids react upon each other and produce chlorine and iodine.

As the iodate of ammonia is not noticed in any work with which I am acquainted, I think it right to observe here that it is a highly crystalline granular powder, possessed of but little solubility: it may be prepared by saturating the solution of the muriatic and iodic acids with pure ammonia, when it will fall down, the muriate remaining dissolved. I find that iodic acid is decomposed by sulphocyanic acid and the sulpho-cyanates of potash and soda; and also that saliva, in consequence probably of the sulpho-cyanate of potash it contains, decomposes iodic acid, and produces with it and starch a blue precipitate not to be distinguished from that produced under similar circumstances by morphia. The importance of this discovery in a medico-legal point of view is considerable, since iodic acid is now very much relied upon as a test for morphia.

I am, Gentlemen, yours, &c.

Roebuck Place, Great Dover Road, Southwark.

LEWIS THOMPSON, M.R.C.S.

LXXXII. *Explanation of a remarkable Paradox in the Calculus of Functions, noticed by* Mr. Babbage. *By* JOHN T. GRAVES, *Esq., M.A., of the Inner Temple.*

[Continued from p. 341, and concluded.]

HAVING thus proved that

$$e_0^{\sqrt{-1}\,\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}} = \frac{y+\sqrt{-1}\,z}{\sqrt{y^2+z^2}}, \qquad (15.)$$

we have seen in what manner it follows that

$$l\sqrt{y^2+x^2}+\sqrt{-1}\,\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}$$

is an e-log of x. Q. E. D.

Let $l\sqrt{y^2+z^2}+\sqrt{-1}\,\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}}$ (which I call the 0th e-log of x of the 0th order) be denoted by $l(y+\sqrt{-1}\,z)$ or $l\,x$. It is plain that when x is real and positive, $l\,x$ resolves itself in point of quantity (as it ought to do, if our notation be consistent) into the arithmetical e-log of x. Following the same notation,

$$l\frac{1}{\sqrt{y^2+z^2}}-\sqrt{-1}\,\frac{z}{\sqrt{z^3}}\cos^{-1}_{0+}\frac{y}{\sqrt{y^2+z^2}} \qquad (16.)$$

may be denoted by $l\frac{1}{x}$, since it may be similarly proved to be an *e*-log of $\frac{1}{x}$, and since it is the same individual function of $\frac{1}{x}$ that $l\,x$ is of x. That it is the *same individual function* (so far as such a phrase can be considered applicable), or in other words, that it has a better right than any other logarithm of $\frac{1}{x}$ to be considered the logarithm of $\frac{1}{x}$ *corresponding* to $l\,x$, will appear on substituting in $l\,x$ the respective constituents of $\frac{1}{x}$ for those of x, that is to say $\frac{y}{y^2+z^2}$ for y and $\frac{-z}{y^2+z^2}$ for z, for by such substitution the expression which I denote by $l\frac{1}{x}$ will be obtained.

Observing that in $l\frac{1}{x}$ the constituent (see (16.))

$l\frac{1}{\sqrt{y^2+z^2}} = -l\sqrt{y^2+z^2}$, we find

$$l\frac{1}{x} = -\,l\,x. \tag{17.}$$

Let $l\sqrt{y^2+z^2} = y'$ and $\frac{z}{\sqrt{z^2}}\cos^{-1}_{0+}\frac{y}{\sqrt{y+z^2}} = z'$, and let $l\,l\,\theta$ be denoted by the notation $l^2\,\theta$, then, by what precedes, we have

$$l\,(y'+\sqrt{-1}\,z') \text{ or } l^2\,x = l\sqrt{y'^2+z'^2} + \sqrt{-1}\,\frac{z'}{\sqrt{z'^2}}\cos^{-1}_{0+}\frac{y'}{\sqrt{y'^2+z'^2}} \tag{18.}$$

we have also $l\,(-y'-\sqrt{-1}\,z')$ or $l^2\frac{1}{x}$

$$= l\sqrt{y'^2+z'^2} - \sqrt{-1}\,\frac{z'}{\sqrt{z'^2}}\cos^{-1}_{0+}\frac{-y'}{\sqrt{y'^2+z'^2}} \tag{19.}$$

but it is easy to see that

$$\cos^{-1}_{0+}\frac{-y'}{\sqrt{y'^2+z'^2}} = \pi - \cos^{-1}_{0+}\frac{y'}{\sqrt{y'^2+z'^2}} \tag{20.}$$

Hence $l^2\,x = l^2\frac{1}{x} + \sqrt{-1}\,\frac{z}{\sqrt{z^2}}\,\pi$ (21.)

since $\frac{z}{\sqrt{z^2}}$ evidently $= \frac{z}{\sqrt{z^2}}$. But $\sqrt{-1}\,\pi$ or $-\sqrt{-1}\,\pi$ (one or other of which values $\sqrt{-1}\,\frac{z}{\sqrt{z^2}}\,\pi$ must possess at any time) is an e-log of -1, for

$$e_0^{\pm\sqrt{-1}\,\pi} = \cos(\pm\pi) + \sqrt{-1}\,\sin(\pm\pi) = -1. \qquad (22.)$$

Hence, if z be *positive*, or, x being real, if we choose or have reason to consider the infinitesimal or zero z *positive*, and if $\psi x = c^{\frac{l^2 x}{\sqrt{-1}\,\pi}}$, I say that we shall have $c\,\psi\frac{1}{x} = \psi x$, whatever be c; it being understood that corresponding powers are to be compared in the expressions $c^{\frac{l^2 x}{\sqrt{-1}\,\pi}}$ and $c\,.\,c^{\frac{l^2 \frac{1}{x}}{\sqrt{-1}\,\pi}}$.

For $$c\,\psi\frac{1}{x} = c\,.\,c^{\frac{l^2\frac{1}{x}}{\sqrt{-1}\,\pi}} = c^{1+\frac{l^2\frac{1}{x}}{\sqrt{-1}\,\pi}} = c^{\frac{l^2\frac{1}{x}+\sqrt{-1}\,\pi}{\sqrt{-1}\,\pi}}. \qquad (23.)$$

but by equation (21.) since z is now supposed positive,

$$c^{\frac{l^2\frac{1}{x}+\sqrt{-1}\,\pi}{\sqrt{-1}\,\pi}} = c^{\frac{l^2 x}{\sqrt{-1}\,\pi}} = \psi x. \qquad (24.)$$

On the other hand, if z be *negative*, or so considered, and $\psi x = c^{\frac{l^2 x}{\sqrt{-1}\,\pi}}$, we shall no longer have $\psi x = c\,\psi\frac{1}{x}$, but if, z being negative or so considered, $\psi x = c^{\frac{-l^2 x}{\sqrt{-1}\,\pi}}$, we may prove in similar manner that equation (1.) will subsist whatever be c.

We have thus therefore obtained two correlated and mutually complemental examples, both possessing, to a certain extent, the property of satisfying (1.), and both of them included in the generic form $c^{\frac{-\log\log x}{\log(-1)}}$, mentioned by Mr. Babbage as derived from the process of Laplace, but in neither case does equation (1.) hold good for all values of z, positive or negative, nor even for all *real* values of x, *without an annexed supposition relating to those real values.* Thus we see matters so arranged, with most curious delicacy, that we are never at liberty to suppose $\psi\frac{1}{x} = c\,\psi x$ (a supposition which it is necessary to make if we would prove' $c^2 = 1$), without making ψ itself change the form it had when $\psi\, x$ was equal to $c\, \psi\, \frac{1}{x}$; in other words, (1.) and (2.) are essentially non-simultaneous equations in the illustrative instances before us, for the z of x, of whatever sign it be or be considered to be, though such z be infinitesimal or zero, as in the case of x real, is always or must always be considered to be *of a different sign* from the z of $\frac{1}{x}$. When $\psi\, x = c^{\frac{l^2 x}{\sqrt{-1}}}$ equation (1.) will not be satisfied for all real quantitative values of x, unless zero be considered positive, nor again when $\psi\, x = c^{-\frac{l^2 x}{\sqrt{-1}\,\pi}}$, unless zero be considered negative. *Vice versâ* with respect to equation (2.). One supposition excludes the other: zero may be considered either positive or negative, but not both together. Hence, even in the case of x real, where the solutions would appear on first view to be concurrent, they are, in truth, alternative. We are bound to consider x the same in *state* as well as *quantity* on both sides of the equations (1.) and (2.), and here obscurity arises from the symbols of algebra not expressing to the eye a difference of state between reals having the same quantity. Such difference of *state* in things denoted by algebraic symbols is in most cases immaterial, unless no *quantity* remain in either of their constituents; but we know that it is of importance in the case of vanishing fractions, and we perceive that it may become so in certain other fine circumstances, such as those which we have just discussed.

We have shown therefore by a particular example (or rather by two correlated examples) that the paradox noticed by Mr. Babbage is only a remarkably subtle instance of the following general proposition which is not *à priori* improbable. Though we may prove it to be impossible to find *one fixed form* ψ, such that the equation $\psi\, x = \mathrm{F}\, \psi\, \alpha\, x$ (F and α being given functions) shall hold good simultaneously in different cases where particular values of x are assumed (the term "value" including state as well as quantity), we are not therefore to despair of finding *distinct forms* of ψ, absolute or alternative, which for certain values of x, within appropriate limits, shall severally satisfy the equation $\psi\, x = \mathrm{F}\, \psi\, \alpha\, x$. Such a partial form of $\psi\, x$ and the corresponding partial form of $\mathrm{F}\, \psi\, \alpha\, x$ taken with it may be likened to two curves which coincide for a certain continuous space and divaricate in the rest of their course.

Those parts of this paper in which *infinitesimals* have been spoken of in the more popular language of mathematics, may advantageously be translated into the more rigid phraseology of *limits*. Various distinct continuities may terminate in the same quantity as a limit, as, for example, a line may be looked upon as having moved through any of the infinite number of planes of which it may be the boundary, and it is easy to conceive that there are properties of such a line, which (all things else remaining the same) vary with the plane in which its motion is deemed to have taken place; but it is, I believe, a novelty in algebra, to present an instance of a given individual function of a positive or negative quantity, which varies accordingly as the functional subject is regarded as the limit of this or that kind of imaginary quantity.

Professor De Morgan, in the place before cited, (p. 335.) mentions $\left(\frac{1-x}{1+x}\right)^{\frac{\log c}{\log(-1)}}$ as a form of $\psi\, x$ not obviously discontinuous that appears to satisfy equation (1.) independently of c. We may assume that always on the opposite sides of that equation $\frac{\log c}{\log(-1)}$ is intended to denote the same quantity, and that in the expressions $\left(\frac{1-x}{1+x}\right)^{\frac{\log c}{\log(-1)}}$ and $c\left(\frac{1-\frac{1}{x}}{1+\frac{1}{x}}\right)^{\frac{\log c}{\log(-1)}}$ corresponding powers are to be compared. With respect to this instance I shall only add that it would not be difficult to show by reasoning similar to that which I have already employed in this paper, that no definite case included in the indeterminate expression $\left(\frac{1-x}{1+x}\right)^{\frac{\log c}{\log(-1)}}$ can be other than a partial or alternative solution for $\psi\, x$, unless $c^2 = 1$; for let $\frac{1-x}{1+x} = y + \sqrt{-1}\, z$, then it may be proved by my exponential theorems that the equation

$$\left(\frac{1-x}{1+x}\right)^{\frac{\log c}{\log(-1)}} = c\left(\frac{1-\frac{1}{x}}{1+\frac{1}{x}}\right)^{\frac{\log c}{\log(-1)}},$$

if, being individualized, it hold good for z of one sign positive or negative, cannot hold good for z of an opposite sign, unless $c = \frac{1}{c}$.

Inner Temple, July 1836.

P.S. It is not out of place to mention, that I am gratified with the view which Professor De Morgan has taken in the last Number of this Magazine (October 1836, vol. ix. p. 252,) of my researches on logarithms, and that I agree with him in considering my results rather upon the whole as extensions or (as I should say) completions, than as corrections of what had before been accomplished. He has also properly noticed an oversight I committed in not observing the distinction he drew in his Calculus of Functions, with reference to the possibility of obtaining the most general solution, between functional equations where there are and where there are not independent variables. I may be permitted, however, to assent to his remarks on some other points with some qualifications, which may seem over-nice and pedantic, but are required by the delicacy of the subject, and I wish to prefix some explanation relative to the actual progress or improvement which I consider this branch of science to have received from my researches. The deficiencies in the ordinary theory which I have endeavoured to supply are the following: first, I found no formula which assigned even one value of a^x, much less all of them, when a and x were imaginary; secondly, I considered that as -2 was a value of $4^{\frac{1}{2}}$, $\frac{1}{2}$ would be admitted to be, or at least deserved to be reckoned a logarithm of -2 to the base 4, and yet in no formula which I had met with for the 4-logs of -2 was any such quantity as $\frac{1}{2}$ included; thirdly, I observed generally great laxity, not to say inaccuracy, in the use of ambiguous exponential expressions, and saw equations employed without apparent restriction where, perhaps, the two sides had but one value in common. For instance, the equation $e^{1+\sqrt{-1}\,2m\pi} = e$ is not correct without restricting the meaning of the left-hand side, for though every quantity included in the formula $1 + \sqrt{-1}\,2m\pi$ is an e-log of e, $e^{1+\sqrt{-1}\,2m\pi}$ has an infinite number of real values besides e for any given m, except $m = 0$. Hence, I scruple to call $e^{1+\sqrt{-1}\,2m\pi}$ merely *a general algebraic form* of e, and think it necessary to devise a notation to characterize that particular value of $e^{1+\sqrt{-1}\,2m\pi}$ which is equal to e. The still more general form $e^{\frac{1+\sqrt{-1}\,2m\pi}{1+\sqrt{-1}\,2n\pi}}$ has, equally with $e^{1+\sqrt{-1}\,2m\pi}$, one of its values equal to e, and is *the most general exponential form* that possesses this property; a property which seems to me, even according to ordinary acceptation, to confer on every quantity included in the formula $\frac{1+\sqrt{-1}\,2m\pi}{1+\sqrt{-1}\,2n\pi}$ a right to the appellation e-log of e. Here let me remark that I cannot conceive how any difference in results can be obtained from operating correctly on two *strictly equivalent algebraic forms* such as $\cos\theta$ and $\cos(2i\pi \pm \theta)$. It is true that one may *suggest* what we have to *recollect* with respect to the other, and it is true that in the treatment of such forms there are many specious fallacies to be guarded against. Thus, it would not be correct to reason as though $\cos\{c(2i\pi \pm \theta)\}$ were the same function of $\cos(2i\pi \pm \theta)$ that $\cos(c\theta)$ is of $\cos\theta$, if θ denote a particular individual value of $\cos^{-1}\cos\theta$. On account of the preceding considerations, among others, instead of obtaining my formula for x, the general logarithm of y, by the method stated by Professor De Morgan as substantially the same as mine, viz. by setting out at once with the unelementary definition $e^{(1+\sqrt{-1}\,2m\pi)x} = e^{ly+\sqrt{-1}\,2n\pi}$, I should prefer building, as I have done, on received principles of analogy, which, I think, would naturally entitle ψy to the name of an e-log of y, if any value of $a^{\psi y}$ were equal to y, especially if we found that ψy possessed the property $\psi y + \psi y' = \psi yy'$. I do not meet in books any *explicit exclusion* of $\frac{1}{2}$ from the name of logarithm of -10 to the base 100 on the ground that -10 is not what is called the arithmetical value of $100^{\frac{1}{2}}$, and, in legal phrase, I submit that the *onus* of making out their case lies on those who advocate such an exclusion. It would refuse the name of logarithm to any function whatever of y, where y and the base a were not real and positive, or would require some definition of what is meant by the arithmetical value of a^x for all values of a and x, as well imaginary as real. Now, for some values of a, it may be matter of arbitrary decision to determine which one of a certain pair of values of a^x is to be considered as *corresponding* to that which is the arithmetical value of a^x, when a and x are real and positive. Is $\sqrt{-1}$ or $\frac{1}{\sqrt{-1}}$ the arithmetical value of $(-1)^{\frac{1}{2}}$? For some values of a and x, a^x has more than one real positive value, and for some, again, a^x has a single real positive value corresponding to what would be in general an imaginary value of a^x, if a and x were real and positive. But I have already occupied too much space, and need not labour these arguments, for Professor De Morgan does not materially differ from me here. He seems to regard the ordinary theory as an edifice complete in itself, but is content to receive my results as an extension which may prove useful, whereas I regard them rather as the erection of a wing, required for symmetry, if not for use.

LXXXIII. *On the Conducting Power of Iodine for Electricity.* *By* James Inglis, *M.D.**

[Addressed to the Chemical Section of the British Association.]

IT may not, perhaps, have escaped the notice of some of the members of this Section, that in extracts from a Prize Essay of mine, published some months ago, in the Philosophical Magazine, I stated that I had found iodine to be a conductor of electricity. Nor may the experiments of Mr. Solly tending to prove the contrary have passed by unobserved. Nevertheless, being satisfied in my own mind what I had published was correct, I determined at the earliest opportunity to resume the investigation, and instead of answering that gentleman directly through the medium of the Philosophical Magazine, I thought it might be better to lay before you the result, in as much as I shall by experiment prove my former statement, and then furnish you with that portion of iodine which you have seen conduct, that you may for yourselves judge of its purity.

In Mr. Solly's first paper, no mention is made of experiments performed with fused iodine; but his attention being drawn to the subject by a note of mine, he published a second, in which he throws a doubt on the purity of the iodine I had used, saying that it contained "most probably the iodide of iron, which is not unfrequently present in the iodine of the shops." (Lond. and Edinb. Phil. Mag., No. 48. p. 401.)

The iodine I used was obtained from the manufactory of Mr. Whitelaw of Glasgow, where no iron vessel is ever employed, and in which, in its veriest impurity, no iron can be detected. Here, for instance, is one tube containing an aqueous solution of ioduret of iron; a second, an aqueous solution of the iodine to be tested; and a third having in it a solution of the ferrocyanate of potassa. Now, on adding a small portion of this last solution to the one containing iron, immediately the blue ferrocyanate of the peroxide of iron results. But no such effect is produced when the test is added

* Read before the Chemical Section of the British Association at Bristol, Aug. 26, 1836: and now communicated by the Author.

to the solution of iodine; it remains the same as before admixture. Add, however, but a single drop of the solution of the ioduret of iron and the blue colour instantly appears. But supposing that a small portion of the ioduret by some chance happened to be present, we know that from its great affinity for water it could be removed by washing; I therefore washed several times, and thoroughly dried, the iodine with blotting-paper; and lastly, thrice sublimed it; so that now I presume it is as pure as possible.

Of this iodine thus prepared, I put a portion into a tube with a platinum wire hermetically sealed into one extremity; and introducing a second wire at the other till one end approached the former to within about the fourth of an inch, I hermetically sealed this extremity also: so that we have here a closed tube containing dry pure iodine, with two separate platinum wires communicating together *only* through the medium of the iodine.

Three galvanic troughs, containing each 30 pairs of plates, were charged, (but 20 pairs, or fewer, as in the trough now to be used, are sufficient,) and one of the platinum wires fixed to the positive pole, whilst the other was placed in a glass of acidulated water. On forming the galvanic circle, no effect was produced, either by the decomposition of water, or by sensation on the tongue: nor was there any difference on reversing the poles.

The iodine being now liquefied by the flame of a spirit-lamp, and the tube attached to the negative pole, the platinum wire was placed as before in water; and on completing the circle by a copper wire from the positive, instantly bubbles of gas appeared and were evolved at the platinum wire, whilst none appeared at the copper, being positive. The order being reversed, globules of gas appeared at both wires, showing clearly that decomposition had been effected.

Again, if the platinum wire be placed on the tongue, and the copper wire be taken hold of with the hand, instantly the galvanic sensation is felt.

The heat being removed, the power of conduction gradually dies away; so that in seven minutes it is incapable of transmitting even sufficient electricity to be perceived by the tongue. When therefore I stated in a note attached to Mr. Solly's paper, that iodine when cold and concrete still conducted, I was in error, being led to say so from recollection only. But my general statement *that iodine is a conductor*, is, I hope, satisfactorily shown to be borne out this day by experiment.

Dr. Cumming considered that the conduction might be explained by the fact made known by Mr. Faraday, that air when heated becomes a conductor. But that could not apply here, for in the first place, it is not air at all that is the medium of conduction, it is *liquid iodine*; and in the second, on melting the iodine and inverting the tube the conduction is suspended.

Dr. Apjohn now suggested that the iodine at the temperature required for its liquefaction might act on the platinum, and that an ioduret of platinum thus formed would conduct.

But iodine does not act on platinum at 225° Fahr., and 225° is the point at which iodine fuses.

This I stated at the time the objection was made, and since my return I have accurately weighed a piece of platinum wire, and allowed iodine to act on it for half an hour, at and above the point of fusion; when on weighing again, the platinum wire was found to have lost nothing; so that Dr. Apjohn's objection thus loses its weight, no ioduret having been formed.

The conducting power of iodine, atmospheric air, and some other substances when heated, and their non-conducting when cold, adds, I think, an argument in favour of that theory which considers electricity to be but an action of matter; and heat and electricity to be but modifications of each other.

Castle Douglas, Oct. 3, 1836. JAMES INGLIS.

LXXXIV. *An Account and Explanation of some remarkable Results obtained during a Course of Electro-Magnetic Experiments. By the Rev.* J. W. MACGAULEY*.

IT is impossible not to remark that the electro-magnetic helix seems to increase the power of a given battery, for the brilliancy of the spark increases with the magnitude of the apparatus. I expected that such an intensity might be given by a very powerful electro-magnet, as that a small galvanic arrangement and a single circle might be made to communicate a considerable *shock*. I coiled nearly 2000 feet of copper wire, in ten helices, upon a bar of soft iron, during the experiments I was making preparatory to the construction of a large and greatly improved machine on the principle which I exhibited last year (1835) to the British Association, and which is now nearly completed: from this magnet I obtained a powerful shock.

It is not necessary to detail a great variety of arrangements adopted and results obtained; among others I came to the following conclusions:

* Read before the Royal Dublin Society on June 14th; and now communicated by the Author.

1st. That the spark and shock obtained from an electro-magnet, on breaking battery communication, are not the spark and shock of the *battery* nor of the *electro-magnet*, but, most probably, the electricity induced on the wire of the helix by the electricity of the battery, or, if it be true that a current passes along the wire, the electricity intercepted in its passage from the copper to the zinc.

2nd. That the spark and shock do not depend, except within certain limits, on the size of the battery.

3rd. That they confirm what I ventured to assert at the last meeting of the British Association (1835) on the nature of magnetism.

4th. That the real power of the battery is not *increased* but *diminished* by the electro-magnetic, or rather, electro-galvanic helix.

1st. The spark and shock (the latter of which I do not recollect to have seen remarked before,) obtained with an electro-magnet on breaking battery communication, are not the spark or shock of the *battery*, for neither one nor the other can exist until after battery communication is actually broken. Again, if they arise from the battery, to receive the shock it would be necessary to form a part of the communication between the copper and zinc. This, however, is not required; it is necessary only to form a part of the communication between the extremities of the helix, or between one extremity of the helix and either the copper or zinc of the battery. Neither does the shock or spark arise from the influence of the bar of soft iron inclosed in the helix: on the contrary, the retention of magnetism in the bars, either from the nature of its iron or the action of a keeper, will proportionably diminish the effect; and I have no doubt that if a large portion of magnetism were retained in a powerful electro-magnet by the keeper, and the keeper were torn off with violence from the magnet, a shock and spark would be perceived at the moment of disruption, which, together with those obtained when battery communication was broken, would form a spark and shock exactly equal to what were obtained had there been no retention of magnetism by the keepers when battery communication was interrupted.

Of the apparatus submitted to the Society for the purpose of demonstrating the facts contained in this paper by experiment, the part to which it was desired more particularly to direct attention consisted of 588 feet of copper wire, No. 13, coiled in seven helices of 84 feet each, on a thin brass tube, $5\frac{1}{4}$ inches in length, $\frac{3}{4}$ internal diameter, having discs of brass, 4 inches in diameter, attached to its extremities. The tube and discs were intended merely as a convenient means of coiling the wire and submitting bars of iron or of steel to the action of the helix. The corresponding extremities of the coils were soldered to two thick pieces of copper wire, which were made to dip respectively into two cups of mercury, forming a connexion between the poles and a calorimotor 1 foot square, double cell, charged with 400 drachms of water and 12 of muriatic acid. Wires lead from the same cups of mercury to vessels containing a solution of common salt, into which the hands are dipped for the purpose of obtaining a shock when battery connexion is broken. Though the wire, for convenience, is coiled upon brass, it is immaterial how it is arranged. I have thrown it into a *heap*, and believe the effect was equally powerful.

The spark and shock must be produced *after* battery communication is *broken*, because while it exists, every electrical effect must be prevented by the helix, as it affords a good conducting communication between the copper and zinc of the battery. Inclosing a bar of soft iron in the helix diminishes the effect.

2nd. The size of the battery, only within certain limits, affects the spark and shock. In constructing a galvanic helix, or a system of such helices, it is evident that the length of the wire must be limited, on account of its imperfect conducting power. We must therefore, to produce a considerable effect, multiply the helices: on the other hand, if the battery be very small, a minute subdivision of its electricity among so many wires may render the portions in each insufficient for any considerable disturbance of electrical equilibrium. The number of the coils and their lengths must therefore be regulated by the size of the battery and the conducting power of the wire.

3rd. Those properties of the electro-galvanic helix are strongly confirmatory of the theory I ventured to advance before the British Association on the nature of magnetism: "That its existence does not depend on the continuance of electrical currents; that continued electrical currents are not the consequence of magnetization; and that magnetism is mere electrical excitement." For if electrical currents were essentially connected with magnetism, and if we can obtain a shock and spark—the acknowledged indications of a current by a simple helix—how much more should we obtain these indications when both causes are simultaneously in action, either of which, of itself, were sufficient for their production! Yet the existence of magnetism within the helix proportionably injures its effect. Magnetism is merely induced electricity, for it is produced by the action of an excited helix. The action of an excited body is the production of an opposite excitement on any body in its proximity, which induction increases its own capacity for electricity. Supposing magnetized substances to be merely modified instances of this seemingly universal law, all their properties may easily be explained.

4th. The power of the battery is not increased, but diminished, by the helix; for, after passing through one helix, its power of exciting another is lessened, nor will it affect a galvanometer so much. If it were increased, judging by the spark and shock, and supposing these to arise from such increase, it ought to have acquired an intensity which would easily carry it through any length of wire.

When 400 drachms of water and 12 of muriatic acid were used, the effect was transitory; but the spark was very brilliant with the helix alone, less brilliant with a *magnet* having 12 feet more wire than the helix, less still with a smaller magnet. The shock with the *helix* was stronger than with the larger magnet; but when 400 drachms of water, 8 of sulphuric acid, and 4 of nitric acid were used, the spark from the helix remained the most brilliant, but the shock from the magnet became stronger than from the helix. To get a shock at a maximum from the helix, contact must be rapidly broken; from the magnet, slowly. The magnet had 600 feet of wire coiled in three helices; the helix 588 feet in seven coils. The shock was considerably *increased* when two persons dipped their hands into the vessels containing the solution of salt; at the same time each received a *greater shock* than when only one person formed the communication.

If the mercury be not clean, or if some of the battery charge be found upon its surface, neither shock nor spark will be obtained, because as soon as the wire leaves the mercury, contact for the battery, as its electricity is of very low tension, ceases; but the other fluid will conduct away the electricity, from whence arise the shock and spark. If one extremity of the helix, or one wire of the battery, be lifted out of the mercury in contact with the wire leading from the same cup to the solution of salt, no shock will be felt.

A very cheap, permanent, and convenient apparatus, on the same principle as that submitted to the Society, may be constructed; one which would bear to the galvanic battery of a single circle the same relation as the Leyden jar to the electrical machine. Like the Leyden jar, it may be made a magazine of power, ready to be exerted on any object the experimenter may desire; but it is not frail and perishable, does not demand much care nor attention, does not depend on the state of the atmosphere for its efficiency, nor require almost any expense to produce or continue its actions.

To construct it, however, on a large scale, a considerable quantity of wire, covered with an insulating substance, is required. The covering of the wire is at present, I believe, expensive, but a machine I constructed for the purpose, which leaves *nothing* to the care or skill of the operator, almost nothing to his labour, and which may be applied to many branches of manufacture, has changed what would otherwise be troublesome and laborious into the work of a few moments. Some judgement may be formed of the *rapidity* of its *execution* when I mention that very lately I covered with it 20,000 feet of wire of various diameters; and of the *exactness* with which it covers it, by the nearly 1300 feet employed in the apparatus submitted to the Society.

79, Marlborough Street, Dublin, June 16, 1836.

LXXXV. *On the Art of Glass-Painting.* *By* A Correspondent.

AS the accounts to be found in various works respecting this curious art are by no means satisfactory or complete, I have thought that a few observations on the subject, comprising a concise account of the processes employed, both in ancient and modern times, might be deemed of sufficient interest to obtain a place in the Lond. and Edinb. Philosophical Magazine.

It is a singular fact, that the art of glass-painting, practised with such success during former ages from one end of Europe to the other, should gradually have fallen into such disuse, that in the beginning of the last century it came to be generally considered as a lost art*. In the course of the eighteenth century, however, the art again began to attract

* [Our Correspondent will doubtless be glad to learn that a very able and interesting work on Glass-Painting has lately been published at Rouen, entitled, "*Essai Historique et Descriptif sur la Peinture sur Verre ancienne et moderne, et sur les Vitraux les plus remarquables de quelques monumens Français et étrangers*; *suivi de la Biographie des plus célèbres Peintres-Verriers:* Par E. H. Langlois, du Pont-de-l'Arche, orné de Planches dessinées et gravées par Mademoiselle Esperance Langlois, 1832." The beautiful and curious windows of the churches of St. Godard, the Cathedral, St. Ouen, St. Patrice, and St. Vincent in Rouen have been copied by Mademoiselle Langlois with great spirit, skill and faithfulness.

M. Langlois disproves the notion ("l'aveugle préjugé"), that the art had been lost, p. 193; and states that this error was more unaccountable in England, where, according to one of the memoirs of M. Al. Brongniart, a number of the finest windows were painted from 1616 to 1700, and by Jervis Forrest up to 1785.—R. T.]

attention, and many attempts were made to revive it. It was soon found by modern artists, that by employing the processes always in use among enamel-painters, the works of the old painters on glass might in most respects be successfully imitated; but they were totally unable to produce any imitation whatever of that glowing red which sheds such incomparable brilliancy over the ancient windows that still adorn so many of our churches*. For this splendid colour they possessed no substitute, until a property, peculiar to silver alone among all the metals, was discovered, which will presently be described. The art of enamelling on glass differs little from the well-known art of enamelling on other substances. The colouring materials (which are exclusively metallic) are prepared by being ground up with a *flux*, that is, a very fusible glass, composed of silex, flint-glass, lead, and borax: the colour with its flux is then mixed with volatile oil, and laid on with the brush. The pane of glass thus enamelled is then exposed to a dull red heat, just sufficient to soften and unite together the particles of the flux, by which means the colour is perfectly fixed on the glass. Treated in this way, gold yields a purple, gold and silver mixed a rose-colour, iron a brick-red, cobalt a blue†; mixtures of iron, copper and manganese, brown and black. Copper, which yields the green in common enamel-painting, is not found to produce a fine colour when applied in the same way to glass, and viewed by transmitted light; for a green therefore recourse is often had to glass coloured blue on one side and yellow on the other. To obtain a yellow, silver is employed, which, either in the metallic or in any other form, possesses the singular property of imparting a transparent stain, when exposed to a low red heat in contact with glass. This stain is either yellow, orange, or red, according to circumstances. For this purpose on flux is used: the prepared silver is merely ground up with ochre or clay, and applied in a thick layer upon the glass. When removed from the furnace the silver is found not at all adhering to the glass; it is easily scraped off, leaving a transparent stain, which penetrates to a certain depth. If a large proportion of ochre has been employed, the stain is yellow; if a small proportion, it is orange-coloured; and by repeated exposure to the fire, without any additional

* In 1774 the French Academy published Le Vieil's treatise on Glass-painting. He possessed no colour approaching to red, except the brick-red or rather rust-coloured enamel subsequently mentioned in the text, derived from iron.

† It appears by a boast of Suger, abbot of St. Denis, which has been preserved, that the ancient glass-painters pretended to employ sapphires among their materials; hence, perhaps, the origin of the term *Zaffres*, under which the oxide of cobalt is still known in commerce.

colouring matter, the orange may be converted into red. This conversion of orange into red is, I believe, a matter of much nicety, in which experience only can ensure success. Till within a few years this was the only bright red in use among modern glass-painters; and though the best specimens certainly produce a fine effect, yet it will seldom bear comparison with the red employed in such profusion by the old artists*.

Besides the enamels and stains above described, artists, whenever the subject will allow of it, make use of panes coloured throughout their substance in the glass-house melting-pot, because the perfect transparency of such glass gives a brilliancy of effect, which enamel-colouring, always more or less opake, cannot equal. It was to a glass of this kind that the old glass-painters owed their splendid red. This in fact is the only point in which the modern and ancient processes differ, and this is the only part of the art which was ever really lost. Instead of blowing plates of solid red, the old

* [The barbarous devastations to which the productions of this beautiful art have been subjected are deeply to be regretted. It appears from the interesting "Account of Durham Cathedral" lately published by the Rev. James Raine, that there was much fine stained glass in the fifteen windows of the Nine Altars which

"shed their many-coloured light
Through the rich robes of eremites and saints;"

until the year 1795, when "their richly painted glass and mullions were swept away, and the present plain windows inserted in their place. The glass lay for a long time afterwards in baskets on the floor; and when the greater part of it had been purloined the remainder was locked up in the Galilee." And in 1802 a beautiful ancient structure, the Great Vestry, "was, for no apparent reason, demolished, and the richly painted glass which decorated its windows was either destroyed by the workmen or afterwards purloined." The exquisite Galilee itself had been condemned, but was saved by a happy chance.

The destruction of these

"storied windows, richly dight,
Casting a dim religious light,

has not then been the work of the calumniated cotemporaries of our divine poet, but of the successive Deans and dignitaries of the Church. And if Painting and Architecture have to complain of such devastation in our cathedrals, the treatment of the sister art has been still more deplorable. The ample funds with which the Choirs were endowed, as distinct corporations established for the cultivation of the highest species of sacred music, and its employment in divine worship, having been misappropriated by private cupidity, no longer does

"the pealing organ blow
To the *full-voiced* quire below,"

but to perhaps a third of the complement prescribed by the statutes, and those often too ill paid and inefficient to realize the poet's beautiful description. As for "service high," in many cathedrals it is quite out of the question, as very few of the minor canons, are musicians and the choirs, instead of being "full-voiced," are reduced to the lowest number by which the skeleton or outline of the cathedral service can be exhibited. But bad as these things are, the proposed changes, in the hands of ignorance and barbarism, may yet be for the worse, and the choirs, having been now brought to the glass-makers used to *flash* a thin layer of red over a substratum of plain glass. Their process must have been to melt side by side in the glass-house a pot of plain and a pot of red glass: then the workman, by dipping his rod first into the plain and then into the red glass pot, obtained a lump of plain glass covered with a coating of red, which, by dexterous management in blowing and whirling, he extended into a plate, exhibiting on its surface a very thin stratum of the desired colour*. In this state the glass came into the hands of the glass-painter, and answered most of his purposes, except when the subject required the representation of white or other colours on a red ground: in this case it became necessary to employ a machine like the lapidary's wheel, partially to grind away the coloured surface till the white substratum appeared.

The material employed by the old glass-makers to tinge their glass red was the protoxide of copper, but on the discontinuance of the art of glass-painting the dependent manufacture of red glass of course ceased, and all knowledge of the art became so entirely extinct, that the notion generally prevailed that the colour in question was derived from gold †. It is not a little remarkable that the knowledge of the copper-red should have been so entirely lost, though printed receipts have always existed detailing the whole process. Battista Porta (born about 1540) gives a receipt in his *Magia Naturalis*, noticing at the same time the difficulty of success. Several receipts are found in the compilations of Neri, Merret and Kunckel, from whence they have been copied into our Encyclopædias‡. None of these receipts however state to what purposes the red glass was applied, nor do they make any mention of the *flashing*. The difficulty of the art consists in the proneness of the copper to pass from the state of prot-

lowest ebb, finally extinguished. With regard to our national and ecclesiastical monuments, we would hope that these may no longer be left at the mercy of chapters and churchwardens, but put under the protection of men of taste and of professional skill empowered to watch over their preservation and to administer the funds devoted to the purpose.—R. T.]

* That such was the method in use, an attentive examination of old specimens affords sufficient evidence. One piece that I possess exhibits large bubbles in the midst of the red stratum; another consists of a stratum of red inclosed between two colourless strata: both circumstances plainly point out the only means by which such an arrangement could be produced.

† In 1793, the French government actually collected a quantity of old red glass, with the view of extracting the gold by which it was supposed to be coloured! Le Vieil was himself a glass-painter employed in the repair of ancient windows, and the descendant of glass-painters, yet so little was he aware of the true nature of the glass, that he even fancied he could detect the marks of the brush with which he imagined the red stratum had been laid on!

‡ [M. Langlois names the following writers: "Neri en 1612, Handicquer de Blancourt en 1667, Kunckel en 1679, La Vieil en 1774, et plusieurs

oxide into that of peroxide, in which latter state it tinges glass green. In order to preserve it in the state of protoxide, these receipts prescribe various deoxygenating substances to be stirred into the melted glass, such as smiths' clinkers, tartar, soot, rotten wood, and cinnabar.

One curious circumstance deserves to be noticed, which is, that glass containing copper when removed from the melting-pot sometimes only exhibits a faint greenish tinge, yet in this state nothing more than simple exposure to a gentle heat is requisite to throw out a brilliant red. This change of colour is very remarkable, as it is obvious that no change of oxygenation can possibly take place during the *recuisson*.

The art of tinging glass by protoxide of copper and flashing it on crown-glass, has of late years been revived by the Tyne Company in England, at Choisy in France*, and in Suabia in Germany, and in 1827 the Academy of Arts at Berlin gave a premium for an imperfect receipt. To what extent modern glass-painters make use of these new glasses I am ignorant; the specimens that I have seen were so strongly coloured as to be in parts almost opake, but this is a defect which might no doubt be easily remedied †.

I shall now conclude these observations by a few notices respecting glass tinged by fusion with gold, which, though never brought into general use among glass-painters, has I know been employed in one or two instances, flashed both on crown- and on flint-glass. Not long after the time when the art of making the copper-red glass was lost, Kunckel appears to have discovered that gold melted with flint-glass was capable of imparting to it a beautiful ruby colour. As he derived much profit from the invention, he kept his method secret, and his successors have done the same to the present day. The art, however, has been practised ever since for the purpose of imitating precious stones, &c., and the glass used to be sold at Birmingham for a high price under the name of *Jew's glass*. The rose-coloured scent-bottles, &c., now commonly made are composed of plain glass flashed or coated

autres écrivains à diverses époques, decrivaient ces procédés." (p. 192.) He fixes the restoration of the art in France at about the year 1800, when Brongniart, who had the direction of the Sèvres porcelain manufacture, worked with Méraud at the preparation of vitrifiable colours. p. 194. Among modern artists he particularly mentions Dihl, Schilt, Mortelègue, Robert, Leclair, Collins, and Willement.—R. T.]

* *Bulletin de la Société d'Encouragement pour l'Industrie Nationale*, 1826.

† Though it is difficult to produce the copper-glass uniformly coloured, it is easy to obtain streaks and patches of a fine transparent red. For this purpose it is sufficient to fuse together 100 parts of crown-glass with one of oxide of copper, putting a lump of tin into the bottom of the crucible. Metallic iron employed in the same way as the tin throws out a bright scarlet, but perfectly opake.

with a very thin layer of the glass in question. I have myself made numerous experiments on this subject, and have been completely, and at last uniformly, successful, in producing glass of a fine crimson colour. One cause why so many persons have failed in the same attempt*, I suspect is that they have used too large a proportion of gold; for it is a fact, that an additional dose of gold, beyond a certain point, far from deepening the colour, actually destroys it altogether. Another cause probably is, that they have not employed a sufficient degree of heat in the fusion. I have found that a degree of heat, which I judged sufficient to melt cast-iron, is not strong enough to injure the colour. It would appear, that in order to receive the colour, it is necessary that the glass should contain a proportion either of lead, or of some other metallic glass. I have found bismuth, zinc, and antimony to answer the purpose, but have in vain attempted to impart any tinge of this colour to crown-glass alone.

Glass containing gold exhibits the same singular change of colour on being exposed to a gentle heat, as has been already noticed with respect to glass containing copper. The former when taken from the crucible is generally of a pale rose-colour, but sometimes colourless as water, and does not assume its ruby colour till it has been exposed to a low red-heat, either under a muffle or in the lamp. Great care must be taken in this operation, for a slight excess of fire destroys the colour, leaving the glass of a dingy brown, but with a blue transparency like that of gold-leaf. These changes of colour have been vaguely attributed to change of oxygenation in the gold; but it is obviously impossible that mere exposure to a gentle heat can effect any chemical change in the interior of a solid mass of glass, which has already undergone a heat far more intense. In fact I have found that metallic gold gives the red colour as well as the oxide, and it appears scarcely to admit of a doubt, that in a metal so easily reduced, the whole of the oxygen must be expelled long before the glass has reached its melting point. It has long been known that silver yields its colour to glass while in the metallic state, and everything leads one to suppose that the case is the same as to gold.

There is still one other substance by means of which I find it is possible to give a red colour to glass, and that is a compound of tin, chromic acid, and lime; but my trials do not lead me to suppose that glass thus coloured will ever be brought into use.

* Dr. Lewis states that he once produced a potfull of glass of beautiful colour, yet was never able to succeed a second time, though he took infinite pains, and tried a multitude of experiments with that view.—*Commerce of Arts*, p. 177.

LXXXVI. *Observations on some of the Fossils of the London Clay, and in particular those Organic Remains which have been recently discovered in the Tunnel for the London and Birmingham Railroad.* By NATH. THOMAS WETHERELL, *Esq., F.G.S., M.R.C.S., &c.**

FROM the number of railroads now in progress in different parts of this country, and the necessary excavations required in the making of them, there probably never was a time more favourable to the researches of the geologist than the present; and it is sincerely to be hoped that no spot will remain untouched, the examination of which holds out a prospect of adding to our knowledge of the strata of the earth. It is my intention in the following memoir to notice more particularly those remains of the London clay which I have recently collected from that portion of the London and Birmingham railroad which passes in the immediate neighbourhood of Chalk Farm. I shall, however, occasionally advert to discoveries made in other parts of this important and highly interesting formation, which, having closely examined it in different places, I find contains many minute and exceedingly curious fossils, well worthy the attention of the naturalist. Before proceeding to a detail of the organic remains, it may be as well to give a short sketch of the relative position and appearance of the clay here, although there may be no perceptible difference from other places where this stratum has been exposed. Immediately beneath the vegetable mould is a thin bed of diluvium, containing a few bones of animals; and below this is the London clay, which may be easily traced along the line of road, from Chalk Farm to a field in front of Mornington-place, Hampstead road. This portion presents to the eye a reddish or yellowish brown colour, with occasional patches of blue. It is of a loose texture, and contains septaria, casts of shells, selenite, and decomposing masses of sulphuret of iron. In the tunnel, about 60 feet below the most elevated part of the surface, the clay assumes a dark bluish brown colour, and is much more compact, although here and there mixed with sand. The greater part of the organic remains are procured from the depth of from 30 to 60 feet, and very few have been seen in the septaria. A few days since I observed at the top of Park-street, Camden Town, where the men were working at the depth of 10 or 12 feet, a layer of masses of septaria horizontally disposed; this is of common occurrence, and the septaria are sometimes of a very large

* Read before the Camden Literary and Scientific Institution, April 26, 1836, and communicated by the Council of that Institution.

size. The fossil copal*, or Highgate resin, so abundant at the Highgate Archway, occurs here but rarely. Fossil fruits analogous to those of Sheppey, crabs, lobsters, sharks' teeth, scales and vertebræ of fish, and the remains of a Trionyx, or marine turtle, have been found. If three divisions of the formation were made, I should consider the following fossils as characteristic of each division, viz. Upper, *Murex coronatus*, *Modiola elegans*, *Cardium nitens* and *Pectunculus decussatus*, as at the Highgate Archway. Middle, *Pholadomya margaritacea*, *Cardium semigranulatum*, *Nautilus regalis*, *Nautilus centralis*, and *Terebratula striatula*, in the Regent's Park. Lower, *Axinus angulatus*, *Pentacrinites subbasaltiformis*†, as found at Islington and in the cliffs between Herne Bay and Whitstable, which are capped with diluvium resting on the London clay. In enumerating the characteristic shells of each division, I have principally selected those which are most abundant in it, and which are either very rare, or not found at all in the other divisions. For example, the *Nautilus centralis*, *Nautilus regalis*, and *Cardium semigranulatum* are only found in the middle division. The *Axinus angulatus* is very common in the lower, exceedingly rare in the middle, and does not occur in the upper division. And lastly, the *Pectunculus decussatus* and *Cardium nitens* abound in the upper, and are very scarce in the middle. In making a collection of fossil shells I have endeavoured to procure them at several periods of growth: this is the more desirable, since it is well known that many shells vary so much at different stages of growth, as to appear to be of several species, when they in reality belong only to one. That there are considerable difficulties in the way of accomplishing this object I admit, but the result whenever it can be done, is most satisfactory. The following is a classed list, including all those (railroad) shells figures of which will be found in the first six volumes of Sowerby's Mineral Conchology. Several of the localities are also taken from the same work. Among the unfigured shells are the following genera: *Phasianella*, *Tornatella*, *Eulima*, *Cerithium*, *Pleurotoma*, *Pyrula*, *Voluta*‡, &c.

Abbreviations of the Names of Places.

HA. Highgate Archway. F. Finchley. HW. Well at

* Phillips's Elementary Introduction to Mineralogy, last edition, 1823, p. 375.

† Miller's Natural History of Crinoidea, p. 140.

‡ Note by Mr. J. De Carle Sowerby.—One species of Voluta naerly resembles *Voluta Lamberti*, differing from it however in having a longer spire and in being more oval. I propose to name it *Voluta Wetherellii*, as a just tribute to the author of this paper.

Lower Heath, Hampstead. H. Hornsey. SH. Sheppey. SOU. Southend. BR. Brentford. R. Richmond. BOG. Bognor rocks. AB. Alum Bay, Isle of Wight. BB. Bogwell Bay, Isle of Thanet. FOL. Folkestone. BC. Barton Cliff. CHL. Well at Colney Hatch-lane, near Muswell Hill. WCH. White Conduit House Tunnel, Islington. VR. Vauxhall-road. HB. Cliff between Herne Bay and Whitstable. BH. Brackenhurst, Hampshire. CH. Child's Hill. RP. Regent's Park. SU. Sussex. HP. Hyde Park. JP. St. James's Park. STU. Stubbington, Hampshire. HOR. Hordwell. FR. France. HAM. Hamsey. HORN. Horningsham. D. Dax. GR. Grignon. LS. Site of the Old Post Office, Lombard-street, City. HAMP. Hampshire. BBS. Bracklesham Bay, Sussex. LYN. Lyndhurst. NOR. Normandy.

The following abbreviations are introduced in order to point out those shells which are rare and those which are common at the railroad.

R. Rare. VR. Very rare. C. Common. VC. Very common.

CONCHIFERA.

Order I. Dimyaria.

	Vol.	Tab.	Fig.	Other Localities.	Rare or Common.
Teredo antinautæ	1	102	1, 2, 4-8	HA. F. HW. H. SH. SOU.	VC.
Pholadomya margaritacea[a]	3	297	1, 2, 3	HW. BR. R. BOG. AB. BB. FOL.	C.
Corbula globosa	3	209	3	HA.	C.
Lucina mitis	6	557	1	BC. HA. F.	VR.
Astarte rugata	4	316		HA. HW. CHL. SH.	R.
Axinus angulatus	4	315		HW. H. WCH. VR. HB.	VR.
Venus incrassata	2	155	1, 2	HW. BH.	VR.
Cardium nitens[b]	1	14		HA. CH. F. HW. SH.	VR.
Cardium semigranulatum[c]	2	144		BC. WCH. RP. SU.	C.
Isocardia sulcata	3	295	4	WCH. SH.	VR.
Pectunculus decussatus	1	27	1	HA. CH. F. HW. SH. BOG.	VR.
Nucula amygdaloides	6	554	4	HW. SH. SOU. HP. JP. FOL.	VC.
Nucula inflata	6	554	2	HA. CH. F. HW.	C.
Modiola depressa	1	8		HA. CH. F. HW.	VR.
Pinna affinis	4	313	2	HA. BOG. HW. H.	R.

Order II. Monomyaria.

Avicula media	1	2		HA. F. HW. SH.	C.
Pecten corneus	3	204		STU.	VR.
Ostrea dorsata	5	489	1, 2	HOR. FR.	R.
Anomia lineata	5	425		BC. BOG. HA. HW.	R.

Order III. Brachiopoda.

Terebratula striatula[d]	6	536	3, 5	SOU. SH. HAM. HORN. D. HW.	C.
Lingula tenuis	1	19	3	HA. CH. F. HW. BOG.	R.

MOLLUSCA.

Order I. Gasteropoda.

	Vol.	Tab.	Fig.	Other Localities.	Rare or Common
Bulla constricta	5	464	2	BC.	C.
Bulla attenuata	5	464	3	HA. SH.	C.
Natica glaucinoides[e]	1 5	5 479	 4	H.A. CH. F. HW. CHL. H. SH.	VC.
Vermetus bognoriensis[f]	6	596	1-3	HA. HW. CHL. SH. BOG.	VC.
Dentalium nitens	1	70	1, 2	HA. HW.	VR.
Dentalium incrassatum	1	79	3, 4	HA. HW. R.	C.
Solarium patulum	1	11		HA. F. HW. CHL. H. F.	C.
Trochus extensus	3	278	2, 3	HA. SH.	C.
Fusus errans	4	400		BC. HOR. STU.	R.
—— bifasciatus	3	228		HA. SH.	C.
—— tuberosus	3	229	1	HA.	VR.
—— interruptus	3	304		BC.	VC.
—— trilineatus	1	35		HA. BR. SH.	R.
—— regularis	2 5	187 423	2 1	HA. SH. BC.	R.
—— coniferus	2	187	1	HA.	R.
—— porrectis	3	274	8, 9	HA. HOR.	VR.
Pyrula nexilis	4	331		HA. BC. GR. LS. F. HW.	C.
—— Greenwoodii	5	498		HA. HAMP.	C.
Triton argutus	4	344		BC. BBS.[g]	R.
Murex cristatus	3	230	1, 2	HA. SH.	R.
Rostellaria lucida	1	91	1-3	HA. WCH. CH. HW. CHL. SH.	VC.
—— Sowerbyi	4 6	349 558	1-5 lower 3	HA. SH. BOG.	R.
Cassis striata	1	6		HA. SH.	R.
—— carinata[h]	1	6		HA. F. HW. H. SH.	C.
Cancellaria læviuscula	4	361	1	HA. BC. LYN. NOR. CH. F. HW.	R.
Auricula turgida	2	163	4	HA. F.	VR.
Acteon (Tornatella) simulatus	2	163	5-8	HA. SH. BC. F. HW. H.	R.
—— elongatus	5	460	3	HA. F.	R.
Cypræa oviformis[i]	1	4		HA. SH.	R.
Conus concinnus	3	302	2	HA. BC. SH.	R.

Order IV. Cephalopoda.

Septa concave.

	Vol.	Tab.	Fig.	Other Localities.	Rare or Common
Nautilus imperialis	1	1		HA. BR. SH. BOG.	C.
—— centralis	1	1		R.	VC.
—— regalis	4	355		HP. WCH.	R.
—— ziczac	1	1		HA. SH.	VR.

NOTES.

[a] A specimen of *Pholadomya margaritacea* and also one of *Nucula amygdaloides* have been found in the gault at Folkestone by that indefatigable friend to science J. S. Bowerbank, Esq. I have much pleasure in stating that our knowledge of the organic remains of the London clay (particularly those of Sheppey) will ere long be most materially increased by the praise

worthy exertions of that gentleman. The great pains he has taken in collecting and arranging specimens can only be duly appreciated by those who have undertaken a similar task; and the splendid series of fossils in his collection, principally appertaining to the tertiary and secondary rocks, are well worthy the attention of the geologist.

[b] Also found in the lias. See Sowerby's Index to the first six volumes of the Mineral Conchology of Great Britain, page 242.

[c] This shell is also figured by Brocchi, in his *Conchologia Fossile subapennina*, tom. ii. tavola xiii. fig. 14. He has given the name of *Venus cypria*, although he considers that it very nearly approaches the form of a Cardium. It is evident that the specimen in his possession did not exhibit the hinge.

[d] Gault to London clay. See Sowerby's Index to the first six volumes of the Min. Con., p. 245.

[e] Also in the crag. See Sowerby's Index, p. 246.

[f] Mantell's Geol. of S.E. of England, page 367. Hist. Suss., vol. iii. pl. 1. f. 3. Geol. Suss., 272.

[g] Mantell's Geol. of S.E. of England, page 366.

[h] Also found at Stubbington. Webster, Geol. Trans., first series, vol. ii. page 204, On the strata over the chalk in the S.E. part of England.

[i] Webster, Geol. Trans., first series, vol. ii. page 204. In a list of the organic remains in the lower marine formation above the chalk in England, *Cypræa Pediculus* is marked as having been met with at Highgate and Stubbington. Only one species has been found at Highgate; it is therefore very probable that the one referred to is the same, although the specific name is different. Stubbington may therefore be considered an additional locality for this beautiful shell.

Fifty-five species are enumerated in the above list, besides which I have (from the railroad) forty species which have as yet not been figured as British shells, independently of a few, which are extremely minute, of the order Cephalopoda (Foraminifera), consisting of Spirolina, Orbulites, Nummulites, &c., making altogether above one hundred species. A part of the unfigured shells having also been found in digging a well at Lower Heath, Hampstead, plates of them, with descriptions, will be given in the Geological Transactions, to illustrate a paper read before the Geological Society on the 4th of June, 1834*; the remainder will appear in some of the early numbers of the continuation of Sowerby's Mineral Conchology. I have several examples of the *Nautilus regalis*, which present a very singular marking. A number of serrated lines may be seen in the outer lamina of the shell, running across it, and generally two or three parallel to each other. Although they pass some depth into the substance of the outer lamina, they do not, as far as I have hitherto observed, extend to the inner one. The great zeal which Mr. J. DeC. Sowerby has always evinced in the advancement of science has induced me to give his name to a new species of Nautilus which I have discovered at the railroad. This distinguished naturalist, on more than

* Proceedings of the Geological Society of London, vol. ii. page 93.

/ named by [illegible]

one occasion, has most kindly and readily afforded me information on subjects connected with my scientific pursuits. In my cabinet are two Nautili, brought from the Isle of Sheppey by J. S. Bowerbank, Esq., which appear to belong to the new species, but Mr. Sowerby, having examined them, considers that they are not sufficiently perfect to state that they do so with certainty. It was in the year 1833 that I first discovered the minute cephalopodous Mollusca in some masses of clay which I had brought from a well on Hampstead Heath. This led me to a further examination of the clay in other spots, when I again met with them in the clay and on the surface of some pieces of iron pyrites. Of one species I found several attached to the whorls of *Vermetus bognoriensis.* Another species has several spines projecting from different parts of the outer surface, which I have not seen in any of the Grignon shells. The President of the Geological Society, in his Anniversary Address *, mentions that Mr. Lonsdale, in arranging the collection of the Society, had found "that our common white chalk, especially the upper portion of it, taken from different parts of England (Portsmouth and Brighton among others), is full of minute Corals, Foraminifera, and valves of a small entomostracous animal, resembling the Cytherina of Lamarck. From a pound of chalk he has procured, in some cases, at least a thousand of these fossil bodies. They appear to the eye like white grains of chalk, but when examined by the lens, are seen to be fossils in a beautiful state of preservation."

Edward Charlesworth, Esq., F.G.S., in an able paper on the Crag Formation, published in the London and Edinburgh Philosophical Magazine for August 1835, gives an extract of a letter received from Searles Wood, Esq., of Hasketon, near Woodbridge, in which it is stated that his cabinet contains fifty species of minute cephalopodous Mollusca of the order *Foraminifera*, D'Orbigny, which belong to the lower division of the crag.

Among the donations to the museum of the Geological Society in 1835, is the following: Spirolinites† in chalk flints from Stoke near Chichester, presented by the Marquis of Northampton, F.G.S.

The shells of the most rare occurrence (at the railroad) are *Phasianella*, *Tornatella*, *Cypræa oviformis*, *Cardium nitens*, *Pectunculus decussatus*, and *Conus concinnus.* The most abundant, are *Rostellaria lucida*, *Fusus interruptus*‡, and *Natica glaucinoides.*

* Address to the Geological Society, delivered at the Anniversary, on the 19th of February 1836. by Charles Lyell, Jun., Esq , President. Proceedings of the Geological Society of London, p. 365.

† Proceedings of the Geological Society of London, vol. ii. p. 341.

‡ A well-known Barton shell.

In the year 1829 I met with (on breaking some masses of septarium, which I had brought from Child's Hill, north-west of Hampstead,) the remains of a species of Ophiura in a very good state of preservation; and as the remains of Ophiura had never before been noticed as occurring in the London clay, I considered the discovery of sufficient importance to make it the subject of a communication read before the Geological Society on January the 9th, 1833*. I have since seen a fine specimen from Harwich, which is in the cabinet of Edward Charlesworth, Esq.; there is likewise one in the collection of J. S. Bowerbank, Esq., recently obtained from the Isle of Sheppey.

The following list may be useful, as it contains a few more of the organic remains (from the railroad): I have also added some other localities.

Crustacea.

Cancer Leachii. Highgate Archway and Isle of Sheppey.
Astacus†. Do., Isle of Sheppey, and Hampstead well.

Radiaria.

Pentagonasta. Hampstead well and Isle of Sheppey.
Pentacrinites‡ *subbasaltiformis.* Hampstead well, Mr. Warner's well at Hornsey, and the cliffs between Herne Bay, and Whitstable.

Zoophyta.

Turbinolia. Sheppey.

Reptilia.

Trionyx§. Sheppey and Harwich.

In the above pages, I have as much as possible avoided technicalities, wishing to give a plain statement of those facts which have come more immediately under my own observation; and as other collections have been made from this rich

* Proceedings of the Geological Society of London, vol. i. p. 415.

† Two species, one of which appears to be the same as *Astacus cataclysmi* in the British Museum, the locality of which is not marked.

‡ The London clay portion of the cliffs between Herne Bay and Whitstable belongs to the lower division of the stratum. About three years since I brought away several dozen of stems of Pentacrinites, which are very common there. At the railroad the Pentacrinite is extremely rare. The remains I possess from there, consist of some stems, parts of the auxiliary side arms, tentacula, and a few bones of the pelvis.

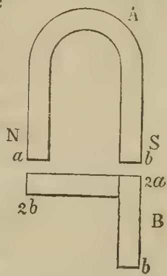
§ A splendid fossil Turtle found in the Harwich cement-stone is in the British Museum. It is from the collection of Edward Charlesworth, Esq., and a very excellent and correct engraving of it has been made by Standidge and Lemon of Cornhill.

deposit, I trust more information in a short time will be laid before the members of the Camden Literary and Scientific Institution.

Highgate, April 23, 1836.

LXXXVII. *Reply to* Dr. Ritchie's *Remarks on* Mr. Rainey's *Theory of Magnetic Reaction.* *By* G. Rainey, *Member of the Royal College of Surgeons.**

Dr. Ritchie, in his reply to my last letter, appears to have attached a meaning to my explanation totally different to that which it was intended to convey. Dr. Ritchie asserts that I take for granted "that a piece of soft iron B, placed in the direction of one side of the horse-shoe magnet A, will have a magnetic power represented by *b*, induced in its remote extremity, while the power of the pole S still retains its inducing power." As I cannot suppose that Dr. Ritchie has so far intentionally mistaken my meaning as to attribute to me the adoption of a postulate at variance with common sense, I must suppose this mistake to have originated in some verbal obscurity, or want of perspicuity in my phraseology.



As I have failed so far in making my meaning intelligible, and being allowed the indulgence of a second trial, I will endeavour to be more explicit, as I am particularly anxious that nothing like technical disputation should prolong this controversy. Dr. Ritchie says "that S cannot induce a power on a piece of soft iron, without having its own power diminished by an equal quantity." This is a fact which I am most ready to admit, and which I never doubted; and according to this fact, when the contiguous end of B has received all the magnetism S is capable of inducing, the magnetism at this end will be represented by *b*, which is equal to *a* in its inducing power, though of an opposite kind.

When magnetism is induced in ferruginous matter by the contact of either pole of a permanent magnet, the remote extremity instantly assumes a magnetic state, opposite in kind to the contiguous one, and consequently similar to the magnetism of the inducing pole. Now, as the one magnetism always induces that of a dissimilar kind, the magnetism of the remote end of B may be considered to result immediately from the induction which has been produced upon the conti-

* Communicated by the Author.

guous one by the permanent magnet, and not from the permanent magnet itself.

As the magnetism, then, at one extremity of B may be considered as immediately resulting from the induction on the other, it is highly probable that the inducing power of both is equal. This appears evident from the following experiment. Apply a piece of soft iron to either pole of magnet and ascertain the weight it will sustain; then apply another piece to the remote end of the former one, and it will be found to sustain nearly the same weight. This experiment will be influenced somewhat by the form and size of the pieces of iron, by the state of the surfaces in contact, and by any condition of the metal which could interfere with the free and equable distribution of magnetism throughout its substance. It is only when every obstacle to free induction vanishes that the magnetism of the keeper B will be correctly represented by $a + b$. Now, admitting that under these circumstances $(a + b)$ is a correct expression for the magnetism of the keeper, when it is in contact with the S pole, the same expression would be correct if it were applied to the N pole; and as there is no known limit at which soft iron ceases to allow of further induction by an increase of the inducing power, we must admit that the magnetism of the armature when under the inducing influence of both poles, will be double that which it had when exposed to the action of one pole only, and consequently represented by $2(a + b)$. This will account for the fact of the attraction of the keeper when in contact with both poles of a horse-shoe magnet, at the same time so very far exceeding that which it has for the two poles when applied separately.

Dr. Ritchie asserts that it is impossible for the lifter by its reaction to increase the power of an electro-magnet, because the lifter, having obtained all its magnetism from the electro-magnet, cannot give back more than it has received. Now, the following experiment will prove beyond all doubt that the lifter can increase the power of the electro-magnet; consequently by the converse of Dr. Ritchie's reasoning it may be inferred that the keeper can be made to possess a higher magnetic state than the electro-magnet to which it applied, which is perfectly consistent with the above reasoning. Heat a common horse-shoe magnet to whiteness, for the purpose of totally destroying its magnetism and rendering it more easy of induction. Pass a piece of insulated copper wire around it, in the manner of an ordinary electro-magnet, and let this magnet be exposed to the action of a galvanic battery, when it will be found to have acquired a small quantity of magnetism. Keep up the galvanic action until the magnet ceases to allow of any farther induction from this source. Now apply a very hard steel lifter to both poles, keeping up the same galvanic current, and the magnet will be found to have gained a considerable increase of permanent magnetism. After the magnet thus operated upon ceases to indicate any further increase of its magnetism, apply a lifter composed of very soft iron of precisely the same dimensions as the former one, the galvanic action being continued, and the magnet will be found to have acquired a still higher charge of permanent magnetism. Now as the induction from the galvanic current was the same in each stage of this experiment, and the effect produced varied according as the condition of the keeper was more or less favourable for induction, the increase of permanent magnetism would appear to depend upon the reaction of the keeper, which, by the converse of Dr. Ritchie's reasoning, must then possess a higher state of magnetism than the magnet to which it is applied.

The experiment proposed by Dr. Ritchie would certainly set this matter at rest provided it answered; but in case it does not, it will not in the least invalidate these facts concerning magnetic reaction: less conclusive evidence gained from experiment and reasoning is not to be totally abandoned because perfectly decisive experiments are unattainable. I believe the experiment proposed by Dr. Ritchie will not succeed, in consequence of the keeper, under the circumstances he has named not possessing an inducing power sufficient to overcome the resistance which steel furnishes to magnetic induction. It is well known that magnetism having once been induced in a piece of steel and partially destroyed by heat or the action of similar poles of another magnet, may be in some measure restored by the reaction of the keeper. This is probably owing to the particles of iron having acquired a tendency to resume their former state when the exciting cause is applied. This would be an instance of induction just as much as if the steel had never been magnetized, for the addition of magnetism is just as necessary to restore that which has been lost as to impart it in the first instance. Dr. Ritchie says that if the explanation which I have given be admitted, it will completely overthrow the Newtonian law of the perfect equality of action and reaction. I had always considered this law as applicable only to mechanical forces, and not extending in the least to these physical phænomena, the acting cause of which is altogether unknown. Suppose a number of pieces of steel properly tempered, and for convenience made into the form of the common horse-shoe magnet, and one of these magnetized to saturation; now by this one let all the others be magnetized, and afterwards let them be put together, and the process of magnetizing be performed repeatedly upon each by the rest; and it will be found that each magnet possesses nearly, if not quite as much magnetism as the one employed in the first instance, that is, as the prime motor itself. This fact can scarcely be doubted, although it is at variance with the Newtonian law of the perfect equality of action and reaction as applied to mechanical forces, as no force can be supposed capable of generating under the same circumstances a force greater than itself.

Again, suppose we consider caloric as a moving force; what relation is there between that which is required to ignite a quantity of gunpowder and the caloric developed by its combustion? Several similar instances might be adduced of the inapplicability of the Newtonian law to the explanation of obscure physical facts.

LXXXVIII. *On a new Galvanic Battery. By the Rev.* N. J. Callan, *Professor of Natural Philosophy in the College of Maynooth.**

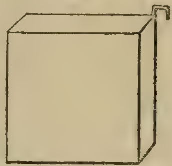
The following paper describes a new galvanic battery, consisting of 20 zinc and as many double copper plates, the whole of which may, by substituting one mercury trough for another, be made to act as a single pair of plates, or as 2, 3, 4, 5, 6, 10, or 20 voltaic circles; and which is capable of producing, by the aid of an electro-magnet, a voltaic current equal in intensity to that of a battery containing 1000 pairs of zinc and copper plates.

UNDER my directions, and on a plan suggested by me, a very large galvanic battery has been lately constructed for the College of Maynooth. This battery consists of 20 zinc plates, each two feet long and two feet broad, and of as many copper cells, each sufficiently large to contain one of the zinc plates. To each zinc plate is soldered a copper wire about half an inch thick and six inches long. The wire projects from the plate in a direction nearly parallel to the sides, and nearly perpendicular to the edge of the plate, which is vertical when the plate is in its copper cell. At two inches from its extremity the wire is bent so as to form nearly a right angle at the bend, and so that these two inches are parallel to the vertical edge of the plate. A wire of the same thickness, and about two inches shorter, is soldered to each of the copper cells: it is bent in the same way as the wires belonging to the zinc plates.

* Communicated by the Author.

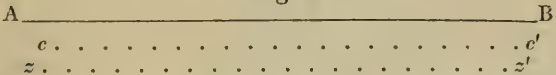
Fig. 1.

Fig. 2.



Figures 1 and 2 represent a zinc plate and a copper cell along with the wires soldered to them. The 20 copper cells are put into a wooden box about $3\frac{1}{2}$ feet long, 2 feet 2 inches deep, and nearly 3 feet wide, and are separated from each other by partitions of wood. The 20 zinc plates are let down into the copper cells, and are lifted up, at pleasure, by means of a windlass. To prevent the contact of the zinc plates with the copper cells, each zinc plate is covered with a woven net of hemp. When the 20 copper cells are in the wooden box, and the 20 zinc plates in the copper cells, the wires soldered to the copper cells project about $\frac{3}{4}$ of an inch from one of the sides of the box, and their extremities descend nearly 2 inches below the upper edge of the same side: but the wires soldered to the zinc plates project nearly 2 inches from the same side of the box, and their extremities descend as low as the extremities of the wires belonging to the copper cells. Thus, if A B (fig. 3.) in the exterior surface of one of

Fig. 3.



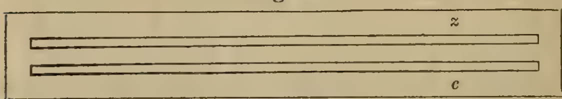
the sides of the box be parallel to the upper edge of the same side, and nearly 2 inches below it, then the row of points $c\,c'$ will represent the extremities of the wires belonging to the copper cells, and the row of points $z\,z'$ will represent the extremities of the wires soldered to the zinc plates.

The acid solution is poured into the copper cells, which are water-tight, and is let out, without lifting the cells, by means of a cock at the bottom of each cell. The cells are barely wide enough to allow the free ascent and descent of the zinc plates. About 30 gallons of fluid are required to charge the whole battery. Both sides of each zinc plate are exposed to the action of the acid mixture, and are within about a quarter of an inch of an equal and parallel surface of copper. Hence the acting surface in each plate is 8 square feet, and the acting surface of zinc as well as of copper in the whole battery is 160 square feet.

By eight mercury troughs, metallic communications may be formed between the 20 zinc plates and the copper cells, so as to make the whole act as a single pair of plates, each containing 160 square feet of surface; or as 2 voltaic circles, in which the zinc of each circle would contain 80 square feet; or as 3 circles, two of which would contain 56 square feet of zinc, and the third of which would contain 48 square feet; or as 4 circles, in each of which there would be 40 square feet of zinc; or as 5, in each of which there would be 32 square feet of zinc; or as 6 circles, four of which would each contain 24 square feet, and two of which would each contain 32 square feet of zinc; or as 10 circles, in each of which there would be 16 square feet of zinc; or as 20 circles, each of which would contain 8 square feet of zinc.

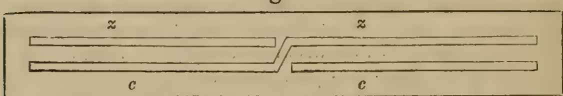
The mercury troughs are made by cutting holes for containing mercury in a piece of wood $3\frac{1}{2}$ feet long, $3\frac{1}{2}$ inches broad, and 2 inches thick. The mercury trough by which all the zinc and copper plates are made to act as a single pair, contains two parallel grooves, each nearly $3\frac{1}{2}$ feet long, $\frac{3}{4}$ of an inch wide, and an inch deep. One of the grooves receives all the wires of the zinc plates, and the other receives all the wires of the copper cells. Hence when mercury is poured into the two grooves, all the zinc plates are in metallic communication with each other, and act as one plate; and all the coppers likewise act as one copper plate. This trough is represented in fig. 4.; the letter *z* shows the groove for the wires of

Fig. 4.



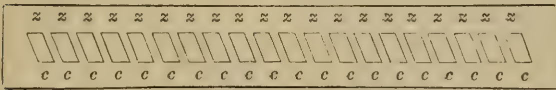
the zinc plates, and *c* the groove for the wires of the copper cells. In the second mercury trough there are four grooves, each of which receives 10 wires. There is a communication between one of the grooves containing the wires of ten copper cells, and that which receives the wires of the zinc plates immersed in the remaining 10 cells. Fig. 5. represents this

Fig. 5.



trough. The third trough contains 6 grooves; the fourth, 8; the fifth, 10; the sixth, 12; and the seventh and eighth each contain 20. Except the trough by which the battery is made to act as 20 voltaic circles, the rest are made like the trough represented in fig. 5. When the battery acts as 20 circles, the wire of each zinc plate, and the wire of the next copper cell, are in the same mercury hole. Fig. 6. represents

Fig. 6.



the upper surface of the trough by which all the zinc and copper plates are made to act as 20 circles.

From the preceding description of our battery, it is evident that the whole 20 zinc plates and copper cells may, by substituting one mercury trough for another, be made to act as a single pair, or as 2, 3, 4, 5, 6, 10, or 20 voltaic circles, and thus be made to supply the place of the calorimotor and of the battery hitherto used for electro-magnetic experiments.

So enormous is the quantity of electricity circulated by this battery when all the zinc and copper plates act as a single circle, that, on one occasion, after having acted without interruption for more than an hour, it rendered powerfully magnetic an electro-magnet on which were coiled 39 thick copper wires, each about 35 feet long, while the mercury in which the wires of the zinc plates were immersed, was connected by 6 copper wires, each $\frac{1}{5}$ of an inch thick and about 6 inches long, with the mercury in communication with the wires of the copper cells. On the fifth day it was tried: after having been in action without interruption for more than two hours, this battery melted very rapidly platina wire $\frac{1}{30}$th of an inch thick, and deflagrated in a most brilliant manner copper and iron wire about $\frac{1}{12}$th of an inch thick.

By this battery, with the aid of an electro-magnet, a current of electricity may be produced which will equal in intensity that of a battery containing 1000 voltaic circles. It is well known that when the connexion between the helix of an electro-magnet and the voltaic battery is broken, a current of electricity is, at the moment of breaking the connexion, made to flow through the helix; and that when the helix is long, that current is capable of giving a shock to any person who holds in each hand a copper cylinder in conducting communication with the ends of the helix. By experiments on the best means of obtaining the shock from the electro-magnet, I have found that the shock increases, within certain limits, with the length and thinness of the bar of soft iron, and with the length of the heliacal coil, as far perhaps as 200 feet, and in proportion, or nearly in proportion, to the number of plates in the voltaic battery from which the current of electricity is passed through the helix. The shock does not increase in proportion to the number of plates unless they are large. The electro-magnet which I first used was a straight bar of soft iron, about 2 feet long, and an inch thick. On this bar were coiled two copper wires, each about 200 feet long. The voltaic battery consisted of 14 pairs of zinc and of as many double copper plates: each plate was about 7 inches square. The end of the first coil and the beginning of the second were immersed into the same cup of mercury, the voltaic current was passed through the first coil only, and the shock was taken by making a communication with the beginning of the first coil and with the end of the second. When the current of electricity was passed through the helix from one pair of plates, the shock received on breaking contact with the battery was equal to that of a battery containing 20 pairs of plates. When two pairs of plates were used, the shock appeared to be doubled; with three voltaic circles, it appeared to be trebled; and with every increase in the number of voltaic circles, there appeared to be a proportional increase of the shock. With the 14 pairs of plates the shock was so strong that a person who took it, from an electro-magnet on which there were four coils of wire, felt the effects of it for several days. With a battery of 4-inch plates the shock increased with the number of plates, but not so rapidly as when large plates were used. I am inclined to think that with a battery of 4-inch plates, the shock increases but little when the number of plates exceeds a hundred. I could not induce any one to take the shock from the electro-magnet when a greater number than 16 of our large plates were used. With 16 of them the shock was exceedingly strong, although the acid mixture employed in charging the battery was very weak; and, from experience, I know that the electro-magnetic effects of a battery depend very much on the strength of the charge.

From all the experiments which I have made on the magneto-electric shock, I think I may fairly conclude, that, if 2000 feet of wire were coiled on a bar of soft iron 6 feet long and an inch thick, a shock might be obtained with the aid of a single pair of plates, which would equal that of a battery of 100 voltaic circles. Hence, since the shock increases in proportion to, or, at least, very rapidly with the number of plates, when they are large, the shock given by such an electro-magnet magnetized by our battery of 20 pairs of plates, should nearly equal, or perhaps exceed that of a battery of 1000 voltaic circles. Hence, by our battery of 20 pairs of plates, an electric current of the highest intensity may be produced. This battery then supplies the place of all the various kinds of galvanic batteries.

The shock given by the electro-magnet may be obtained as often as the connexion of the helix with the battery is broken. Now I have devised a small instrument by which communication with the battery may be broken and renewed 3000 or 4000 times in a minute. Thus 3000 or 4000 shocks may be received, and 3000 or 4000 electric currents of the highest intensity may, in the space of one minute, be passed through water, charcoal, metallic wires, or any other body. It should be remembered that the voltaic current from the battery should not be passed through more than 200 feet of the heliacal coil, and that the shock should be taken from the whole length of the helix.

When a voltaic current passes through a very long wire from a single pair of plates, the wire will give a shock at the moment of breaking contact with the battery. I have found that this as well as the shock from the electro-magnet increases with the number of plates.

I have made a great variety of experiments on electro-magnets. My object in these experiments was to ascertain four things: first, on what the quantity of attraction depends; secondly, on what the distance at which that attraction is exerted depends; thirdly, on what the shock depends; and fourthly, whether by a voltaic current from a large battery, a permanent magnet could be made, which would induce on soft iron magnetism equal to that which is given to an electro-magnet by a battery containing 20 large plates, or 300 four-inch plates. In these experiments I employed three different voltaic batteries, and electro-magnets of various forms. I used the large battery already described; a small battery of 14 pairs of plates, in which each zinc plate was seven inches square; and a Wollaston battery, containing 280 pairs of four-inch plates. Some of my electro-magnets were straight, and others of the horse-shoe form, and one was a square: the iron bars varied in length from 20 inches to six feet, and in thickness from two inches to half an inch. On one of these were coiled 39 copper wires, on another four, on a third three, and on others there was only one wire.

From the results of these experiments, I have deduced the following conclusions: First, that the quantity of attraction increases with the length of the bar of soft iron, at least as far as six feet, and with the thinness till it becomes about an inch; and that it increases nearly in proportion to the number of plates (when they are large) in the battery by which the electro-magnet is magnetized. When the plates are only four inches square the attraction increases, but slowly when the number exceeds 100. Secondly, that the distance at which attraction is exerted, increases also with the length and thickness of the iron bar, and with the number of plates when they are large; but with small plates the increase is very gradual when their number exceeds a hundred. With twenty of our large plates, an iron bar, nearly $3\frac{1}{2}$ lbs. weight, was attracted to a horse-shoe electro-magnet through the distance of an inch, and with ten plates the same bar was attracted to the same magnet, only through the distance of about half an inch. Again, with the twenty plates, the attraction of the same magnet for a sewing-needle was sensible at the distance of 15 inches, and with ten plates the attraction was sensible at the distance of 10 inches. Thirdly, that the shock from the electro-magnet increases within certain limits with the length and thinness of the iron bar, and nearly in proportion to the number of plates when they are large.

When the voltaic current was sent from a battery of 280 four-inch plates, through the heliacal wire coiled round a steel bar about 20 inches long and an inch thick, the steel became almost as strongly magnetic as if it were iron; and when the connexion with the battery was broken, the steel did not retain more than about $\frac{1}{100}$ of its magnetism.

In a paper published in the last (August) number of the Philosophical Magazine, Dr. Ritchie says that the use of the electro-magnet in the apparatus for continued rotation was long since abandoned, because it was incapable of inducing magnetism in an iron bar at a distance. Now he will find that, if instead of a single copper and zinc plate, a battery of 20 pairs of large plates, or of 200 small ones be used, the electro-magnet will have a greater power of inducing magnetism at a distance than any permanent magnet.

The advantages of the battery I have described are, first, that it supplies the place of all the various kinds of voltaic batteries, of the battery for producing a large quantity of electricity of low intensity, of the battery for exciting a large quantity of electricity of the intensity necessary for the rapid fusion and deflagration of metallic wires, and of the battery for producing an electric current of high intensity; and secondly, that it enables a person to compare the power of the very same zinc and copper plates acting as a single pair, with their power when they act as 2, 3, 4, 5, 6, 10, or 20 voltaic circles.

Nicholas Callan.

Maynooth College, August 23, 1836.

LXXXIX. *Observations on certain Liquids obtained from Caoutchouc by Distillation.* By John Dalton, *D.C.L., F.R.S., &c.**

Manchester, November 10, 1836.

DR. GREGORY having published in the last number of the Philosophical Magazine (p. 321.) some interesting experiments and observations on the liquid obtained by the distillation of caoutchouc, I have thought it would be acceptable to that gentleman, as well as to the public, to be made acquainted with the results I obtained from the same subject about two years and a half since, more especially as my experiments chiefly tend to establish additional properties to those deduced in Dr. Gregory's essay. For this purpose I send the Editors a copy of my paper read before the Literary and Philosophical Society of Manchester, on the 17th of October, 1834, which has not been published. I think it is obvious from what follows that most or all the varieties of vegetable combustible products of the oily character must be constituted of central atoms of carbon, oxygen, or carbonic oxide, along with a number of atoms of binolefiant gas, placed alternately around the central atom; and that the repeated distillation of them, at first with a greater and then with a less heat, gradually attenuates the compound atom, till at last it becomes one or two atoms of binolefiant gas slightly adhering to the less volatile parts of the oil, so that the gas, when not under sufficient restraint, expands into the atmosphere at the ordinary temperature.

Observations, &c. Read October 17, 1834.

The article caoutchouc is too generally known to require a particular description; it may suffice to observe that it is obtained from the milky juice of certain trees in South America, which juice is procured by incisions made in the bark of the trees. When the watery part of the juice, which constitutes more than half its weight, is evaporated, there remains a solid elastic substance, which is the caoutchouc. The properties and peculiarities of this singular substance have been mostly described in books of chemistry and other works, and therefore need not here be enumerated. Some new characteristics, however, seem lately to have been discovered by subjecting the article to repeated distillation, and it is upon those that we are about to make a few observations.

Most if not all vegetable products are liable to be decomposed by heat. They are mostly resolved into solid, liquid, and elastic substances, according, in some degree, to the temperature. The instance of the destructive distillation of

* Communicated by the Author.

wood may be taken for an example: by this process we find a solid body resolved into another solid body, charcoal; into various liquids, as water, acetic acid, and pyroxylic spirit; into gases, as carbonic acid, carburetted hydrogen, carbonic oxide, and hydrogen. Caoutchouc is highly combustible: when we burn a slip of it, the flame is white and brilliant; and if we extinguish the flame suddenly, the heated extremity is soft and nearly fluid; hence it would appear that this substance is reducible to a fluid before decomposition, and in that state might probably be distilled like the fat oils; but, like these, the products of the first, second, and future distillations would be successively more volatile, and require a lower temperature for their distillation. This it seems has been found to be the fact.

Having been favoured by an unknown friend with four phials of liquids obtained from caoutchouc by successive distillations (as I apprehend), I found

the 1st,	a deep-coloured liquid, marked sp. gr	·86;
2nd,	a slightly coloured liquid, marked ...	·837;
3rd,	a colourless liquid, marked	·752;
4th,	a colourless liquid, marked	·680;

all of which specific gravities I found very nearly correct. The last, I believe, is lighter than any other known liquid, except perhaps the one mentioned by Dr. Faraday,—see Phil. Trans. 1825, page 452.

The 1st liquid I did not find the boiling-point of, but it is higher than any of the following:

the 2nd	boils at about	290° or 300°;
3rd	———	140°;
4th	———	107° or 108°.

By letting a small portion of the 4th liquor through the mercury in a barometer tube, I find the force of its vapour *in vacuo* is very nearly the same as that of sulphuric æther. The other three liquors I did not expose in the same way, because it is obvious, from their boiling-points, that their elasticities *in vacuo* must be much inferior to that of the 4th.

In order to form some estimate of the relative evaporation of the four liquids, I poured out small portions of the several liquids into glass cups, and dipped the bulb of a thermometer into the liquids, withdrawing it immediately to notice the reduction of temperature by the evaporation. The thermometer being at the temperature of the room, 69°, it was cooled

15°	by four or five successive dips into the liquor	No. 4,
8°	by dips into	No. 3,
$1\frac{1}{2}$°	by dips into	No. 2,
2°	by dips into	No. 1,

By this we learn that the evaporation from No. 4 is exceedingly more than that from any of the others.

The most remarkable circumstance relating to this evaporation from liquor No. 4 remains to be noticed: it is one that distinguishes it from the vapour of every other liquid that I have yet examined. The vapours from æther, alcohol, sulphuret of carbon, &c., are rapidly absorbed by water, in the same manner as are muriatic acid and ammoniacal gases; but the vapour from caoutchouc liquor highly rectified as No. 4, may be passed through water repeatedly without any sensible diminution or alteration of its quantity; at least the action is not more than on olefiant gas. The way to charge a quantity of air with vapour of any kind is to fill a phial with mercury, let in the proposed air till the phial is half full, then inverting it carefully, drop in a little of the liquid to be evaporated, and immediately after again invert the phial over mercury in the trough. The vapour then expands the air, and in a short time the maximum of expansion is produced. The mixture of air and vapour may then be used for any purpose over mercury. In the case of æther, however, a mercurial apparatus is not absolutely necessary; in a narrow tube æther may be turned up through water, and, if sufficient to form a thin stratum over the surface of the water in the tube, the æther vapour will rise into the air, and be defended from the water by the stratum of the liquid: in this way the mixture of air and vapour may be confined in repose for a month.

In regard to the vapour from caoutchouc liquor No. 4, no such precaution is necessary. I take a graduated jar of 4 or 5 inches diameter; and having filled it with water, I let in 60 cubic inches of air, and turn the cock so as to confine it. I then put 20 or 30 water grain measures of the liquor into a tube and turn it into the water in the jar, through which it ascends to the surface and instantly spreads over it a thin film of an oily appearance. This film by degrees almost disappears; at the same time the air gradually expands, and in about twenty minutes acquires its full expansion, becoming about 90 inches in a temperature between 60° and 70°. It will remain for days in this state, and there will be no change of volume unless there be a change of pressure or temperature of the atmosphere.

This permanency of the vapour over the water affords an admirable facility for finding its specific gravity. Let 60 cubic inches of common air be expanded by vapour, suppose to 90; then, exhausting a flask and letting in a given number of inches of the mixture and weighing it, we shall have data to find the weight of the vapour, knowing previously that of common air. By two careful trials, I found by the above process the specific gravity of the vapour from the highest rectified caoutchouc liquor to be 2·07, common air being 1, and no allowance being made for aqueous vapour in a temperature between 60° and 70°. By another experiment the specific gravity of the vapour came out nearly 2.

Another advantage is possessed by this vapour which others have not, namely, that when a given weight or measure of the liquor is passed through water, we are enabled to ascertain how much of it is actually converted into vapour. Thus, to 60 cubic inches of common air I added 25 measures of the liquor, ·680 specific gravity:

in 4	minutes the air and vapour became	70 inches
7	———————	76 ——
27	———————	80 ——

and then remained stationary.

Now, by calculating the weight of the 20 inches of vapour, and comparing it with the weight of the 25 measures of liquor, we find the ratio nearly as 3 to 4; so that only three fourths of this highly rectified liquor is vaporized in such circumstances, and the rest forms a delicate and partial smearing of oil over the surface of the water. This shows that a higher degree of rectification of the liquor is attainable.

These four liquors, as might be expected, are all very combustible; a lighted taper presented to them ignites them instantly. They all burn with a white flame and great smoke. No. 4 leaves no residue; the others leave traces of carbon and moisture. The smallest electric spark lights Nos. 4 and 3.

The vapour also is highly inflammable, and when mixed with oxygen gas may be exploded in Volta's eudiometer. A mixture containing 1 measure of vapour requires 6 of oxygen, and produces 4 measures of carbonic acid; it would appear therefore to be constituted of 2 atoms of olefiant gas combined, and possessing the space of 1 atom of said gas nearly.

Chlorine gas acts upon the vapour much the same as on olefiant. In one instance they seemed to combine in equal volumes, but in another more chlorine was taken up. I did not pursue this inquiry.

Chloride of lime solution seems to have no effect upon the vapour.

Though the vapour is not absorbed by water in an eminent degree, yet I find that water takes up one eighth of its volume of the vapour, which is the same proportion as olefiant gas and phosphuretted hydrogen are absorbed. It may be expelled again by another gas, but not in the full proportion, as would seem from one or two trials.

It may be remembered that I read an essay on oil gas before the Society in 1820, which, with some additions, was published in the fourth volume (new series) of the Society's Memoirs, 1824. In that essay it was made to appear that the portion of gas usually found both in oil and coal gas, denominated *olefiant* gas on account of its combination with chlorine, was not the same as the gas from alcohol by sulphuric acid. The former is much more dense and requires more oxygen to burn it than the latter. For want of a more definite term I called it *superolefiant.* It was shown to be more absorbable by water than the other ingredients of oil gas (page 80); and it was conjectured (page 81) that the new gas might consist of a gas having two atoms of olefiant in one, or united, and possessing a greater specific gravity than the common olefiant. I have now no doubt that my superolefiant is the same as the vapour we have been considering. They are both obtained from oleaginous substances and from coal by heat; they agree in their action on chlorine, and in their absorbability by water, and, for aught that appears, in their specific gravity and in their products by combustion.

In 1825 Dr. Faraday published an essay in the Phil. Trans. of the Royal Society, in which he noticed some new products obtained during the decomposition of oil by heat, one of which he calls "a new carburet of hydrogen." This appears to have every characteristic of the vapour we have been describing. See p. 452.

The fat oils and the resinous body caoutchouc are composed chiefly of carbon and hydrogen; indeed, we may say of olefiant gas, or rather perhaps *binolefiant gas*, for those gases have their elements nearly in the same proportion as the oils and resins. It is a remarkable characteristic of these last bodies that they can sustain a high heat without volatilization in their ordinary state; but if subject to the temperature necessary for distillation, and this distillation be repeated, they become more and more volatile, till at length a liquid is obtained, the component atoms of which are a combination of 2 atoms of olefiant gas.

May it not be presumed then that the original constitutions of such combustible bodies are charcoal or water, holding in combination numerous atoms of binolefiant gas, and that these combinations become less numerous as they are more loosened by heat and repeated distillations?

XC. *On Voltaic Electricity, and on the effects of a Battery charged with Sulphate of Copper. By Mr.* W. De la Rue*.

THE greatest effect being always produced in those voltaic arrangements where the chemical agent exerted an action on only one of the metals constituting the battery, it occurred to me to use a saturated and perfectly neutral solution of the electro-negative metal, provided the other was capable of effecting its decomposition. I therefore tried the effect of a saturated solution of sulphate of copper† in an elementary voltaic battery of the ordinary construction. The zinc plate was four inches by two, the copper completely surrounding it: with this I was enabled to produce ignition of half an inch of platina wire one thirtieth of an inch in diameter, and continue it as long as the zinc plate lasted, which, being very thin, was dissolved in a couple of hours. The effects of this battery were considerably greater than those of one made of platina and zinc of the same dimensions, this being immersed in diluted nitric acid.

I afterwards constructed a battery with three four-inch zinc plates connected together; these were immersed in a copper trough with two partitions, so that the zinc should be opposed on both its surfaces to a plate of copper: with this battery one inch of fine iron wire was kept ignited for four hours. The zinc plate is always partially covered with a coating of copper, *which, however, is* NOT DETRIMENTAL *to the power of the battery*: the copper plate is also covered with a coating of metallic copper, which is continually being deposited; and so perfect is the sheet of copper thus formed, that, on being stripped off, it has the polish and even a counterpart of every scratch of the plate on which it is deposited. Besides this, the voltaic influence decomposes the water; the oxygen, uniting with a portion of the copper and hydrogen, being set at liberty. This may be readily shown by soldering at one end a piece of copper

* Communicated by the Author.

† Daniell uses sulphate of copper, but not as the exciting agent.

[Professor Daniell's object was to obtain a voltaic combination constant in its effects while the connexion is completed, and totally inactive when the circuit is interrupted. Sulphate of copper, used as an exciting agent, he found unsuited for this purpose, and therefore relinquished this employment of it in his battery. That it did not escape Prof. D.'s attention, the following passage from his paper on Voltaic Combinations, in the first part of the Phil. Trans. for 1836, page 117, will show: "Upon adding sulphate of copper, in any considerable quantity, to the liquid in the cells, notwithstanding the amalgamation of the zinc, there was local action enough upon that metal to disengage hydrogen, which, in however small a quantity, was sufficient to commence the precipitation of the copper upon it. Single circles were thus immediately formed by the two metals, and local action increased to such a degree as speedily to cover the zinc with reduced copper." See also page 109.—Edit.]

and a piece of zinc, coiling the two to form a small calorimotor, which is to be put into a glass jar filled with a solution of sulphate of copper, and inverted in a vessel of the same; metallic copper and its oxide will precipitate, and hydrogen gas fill the jar.

Seeing the effects so continuous in a simple battery, I tried a Cruickshank's, of one hundred pairs, each plate exposing to the action of the fluid a surface of twenty-five square inches. This was charged with a saturated *cold* solution of sulphate of copper, to each three gallons of which I added two ounce measures of nitric acid, for the purpose of cleaning the plates and freeing them from oxide; for half an hour the action was so feeble that I was on the point of emptying the trough, but I soon after noticed that the effect was rapidly increasing; I was then induced to proceed. The batteries attained their maximum of power in three quarters of an hour after charging.

Charcoal points were vividly and continuously ignited, the arc passing through a space of three eighths of an inch; this experiment was beautifully varied by dipping the charcoal in nitrate of strontian, the arc then being of a crimson colour.

Steel points of wire, a quarter of an inch thick, were then tried; the arc passed through an equal space; the steel rapidly fused, was deflagrated, and by the scintillations produced a beautiful effect.

Copper points treated in a like manner produced a green arc, and were rapidly destroyed.

Brass produced a blueish white arc; and the more fusible metals, such as bismuth and tin, produced likewise an arc, but the metal was soon carried from one point to the other and established a perfect contact.

A piece of platina wire, one eighth of an inch thick, was rapidly fused, by keeping it at a short distance from a disc of copper, so as to allow the arc to pass from it to the disc.

A *heap* of metallic leaves was burned with rapidity.

Thick tin-foil was deflagrated.

Very thick zinc-foil was rapidly consumed. A bunch of needles burned rapidly in mercury; the end of a file was deflagrated in the same manner.

Extraordinary as was the power of deflagrating metals, the effect of igniting was comparatively small; not more than an inch of iron wire could be ignited, though, if only twelve pairs of Wollaston's four-inch plates were used, charged with the same solution, two and a half inches could be kept ignited for some time.

The battery was then tried in decomposing common caustic potash, which it did with facility; the combustion of the potassium evolved, vividly igniting the thick platinum wire used for the negative pole. These experiments occupied about two hours. The charcoal points were then again tried; and if there were any alteration the power of the battery had increased. Batteries charged in this manner will continue in unabated action for upwards of three hours; in fact until there no longer remains any copper in the solution. It is worthy of notice, that after the batteries have been in action some time, a large portion of the sulphate of copper is expended, and replaced by sulphate of zinc, *yet the action continues the same.* This naturally suggests using a saturated solution of any neutral salt, common salt for example, and adding merely as much of the solution of copper as will serve for the time required. It is not unlikely that the effect would be more continuous than with a solution of copper only. I intend trying this, as I am still pursuing my inquiries on this subject, the object of which is to simplify as much as possible the voltaic battery.

At the Marylebone Institution, on Monday, September 12, when a lecture was delivered on this subject by Mr. Hemming, the President, the power used was the hundred pairs of Cruickshank's arrangement before alluded to, and one hundred and thirty-two pairs of Wollaston's four-inch plates, making in all two hundred and thirty-two pairs.

The batteries I charged *before* the commencement of the lecture, and they were not used till an hour afterwards; the effects were very striking. The arc from the charcoal points passed through a space of three quarters of an inch, and the effect continued unabated for as long a time as could be spared for this experiment; soda was rapidly decomposed, and the sodium brilliantly deflagrated: all the other experiments before cited were repeated on a much grander scale. The lecture being concluded two hours and a quarter after charging the batteries, the charcoal points were again ignited to light up the spacious theatre, the gas having been extinguished. The shock was very powerful, even when taken with the hands dry*.

Fifty pairs of four-inch plates on Cruickshank's plan suffice for all the above experiments, except the decomposition of the fixed alkalies.

* [As similar experiments to those here detailed have been performed with batteries of no extraordinary dimensions, charged in the usual way, it would have been more satisfactory had the author informed us of the size and number of the plates requisite to produce the same effects when sulphate of copper was not employed. We refer our readers who are interested in the philosophical investigation of this subject to an admirable Essay by Dr. Marianini of Venice, of which an abridgement will be found in the *Annales de Chimie et de Physique*, vol. xxxiii. p. 113. In his investigation of the various causes which influence the energy of the pile, he has been led to examine the effect of different liquid solutions, and gives a table of the relative advantages of forty-nine acids and salts, one part of each being dissolved in one hundred of distilled water.—Edit.]

Water was decomposed with extraordinary rapidity by a battery of this description, and also muriatic acid, the chlorine of which bleached a solution of sulphate of indigo in a few seconds.

Its effects on the animal system, as exhibited by Mr. Hemming to the audience, were almost terrific. A rabbit recently killed, an eel, and frogs were thrown into more violent muscular action than I had ever previously witnessed *.

The *tension* of electricity seems to be greatly increased by this mode of charging the voltaic battery.

Bunhill Row, Sept. 15, 1836.

XCI. *On the Constitution of Bitumens. By* M. Boussingault.

M. BOUSSINGAULT remarks, that bitumens, so abundantly met with on the surface of the earth, and the uses of which seem continually to increase, have hitherto been but slightly examined, so that, if we except the researches of M. de Saussure on the naphtha of Amiano, we are still nearly ignorant of the particular nature of these substances.

It has always been admitted that the great combustibility of bitumens is owing to their being chiefly composed of carbon and hydrogen, and the water which some varieties afford by dry distillation favours the idea that they are not always free from oxygen. In this memoir the author shows that they do not owe their fluidity to naphtha. The bitumen of Bechelbronn, which M. Boussingault has principally studied, is viscid and of a dark brown colour. From its uses it has been called mineral fat, it being advantageously used instead of organic fatty substances to diminish the friction of machines, &c. Alcohol at 40° acts on bitumen, particularly when heated, and acquires a yellow tint. Sulphuric æther readily dissolves it. Heated in a retort to 212° Fahr. nothing distils : this proves that it contains no naphtha.

By distilling the bituminous sand with water, M. Boussingault has obtained a volatile oily principle, which he calls *petrolene*, considering it to be the volatile principle of petroleum : it possesses the following properties :

Petrolene is of a pale yellow colour, of a slight taste, and possesses an odour resembling bitumen ; at the temperature of 70° Fahr. its specific gravity is 0·891 ; at 18° Fahr. it does not lose its fluidity ; it stains paper like the essential oils, burns with much smoke, boils at 536° Fahr. ; alcohol dissolves a small quantity of it, but it is much more soluble in æther. It is composed of

Carbon, ..	88·5
Hydrogen, ..	11·5

so that it is a carburet of hydrogen isomeric with the essential oils of

* [That a battery of two hundred and thirty-two pairs of four- and five-inch plates, or even of a hundred pairs, should violently convulse rabbits, eels and *frogs*, is by no means an extraordinary result. The really terrific experiments made by Dr. Ure on the murderer Clydesdale, at Glasgow, were performed with a voltaic battery consisting of 270 pairs of four-inch plates, charged with dilute nitro-sulphuric acid.—Edit.]

† From *L'Institut, Sept.* 21, 1836.

turpentine, citron, and copaiva. Its vapour, calculated by Dumas' process, is equal to 9·415, which is double that of the essential oil of turpentine. Supposing that 4 vols. of vapour constitute 1 eq. of petrolene, its composition will be

Carbon, ...	80 eqs. =	480
Hydrogen	64 eqs. =	64—544

Besides petrolene, there exists in this bitumen a black substance which remains after the separation of the petrolene: this is the solid principle of bitumen. It is very brilliant, of a conchoidal fracture, and is heavier than water; at about 570° Fahr. it becomes soft and elastic; it decomposes before it fuses, and burns like the resins, leaving a large quantity of coke. The author has called this substance *asphaltene,* from its forming the base of the minerals which mineralogists describe under the name of asphalt. Asphaltene may be procured by submitting bitumen purified by æther to a prolonged heat of about 470° Fahr. It is insoluble in alcohol, but æther, the fixed oils, and oil of turpentine dissolve it. It is composed of

Carbon	75·3
Hydrogen	9·9
Oxygen	14·8—100·

and may be represented by the formula $C^{40}H^{32}O^{3}$, or by $C^{80}H^{64}O^{6}$, which indicates that asphaltene results from the oxidation of petrolene.

The bitumen of Bechelbronn purified by æther may then be considered as a mixture of petrolene and asphaltene, at least this is the result of analysis. It contains

Carbon	87·0
Hydrogen	11·2
Oxygen	1·8—100·

It would then appear that the viscid bitumens are mixtures, probably in various proportions, of two substances, which may be separated, and each of which has a definite composition. One of these principles (asphaltene), solid and fixed, resembles asphalt; the other (petrolene), liquid, oily, and volatile, approaches, in some of its properties, to some varieties of petroleum. From this it will be seen that the consistence of bitumens depends on the predominance of one or the other of these principles in the mixture.

The analogy existing between asphaltene and asphalt has induced the author to examine whether their respective composition is the same. In consequence of this he analysed the asphalt of Coxitambo, which may be taken as a type of the species. This asphalt has a conchoidal fracture, and is of a brilliant lustre; its density is 1·68; it is dissolved by petrolene and the fixed oils with much greater difficulty than artificial asphaltene. Except this difference, which may arise from the cohesion of the particles of the native asphalt, the characters of these two substances are identical. It decomposes before it fuses, and burns, leaving 0·016 of a slightly ferruginous ash. It consists of

Carbon	75·0
Hydrogen	9·5
Oxygen	15·5—100.

Note.—This analysis would indicate the elementary composition of native asphalt, and the artificial asphalt obtained by M. Boussingault, to be the same.

Proceedings of Learned Societies.

GEOLOGICAL SOCIETY.

May 11. 1836.—A PAPER was read "On the Silurian and other Rocks of the Dudley and Wolverhampton Coal-field, followed by a Sketch proving the Lickey Quartz Rock to be of the same age as the Caradoc Sandstone," by Roderick Impey Murchison, Esq., F.G.S., V.P.R.S.

In previous memoirs the author has shown that the coal-field extending from Dudley into the adjacent parts of Staffordshire is surrounded and overlaid by the lower member of the new red sandstone; and on this occasion, laying before the Society an Ordnance map, geologically coloured, he gave, 1st, A general sketch of the structure of the coal-field in descending order: 2ndly, Detailed accounts of the Silurian rocks which protrude through the coal measures or lie beneath them: 3rdly, A sketch of the quartz rocks of the Lickey: 4thly, A description of the trap rocks: 5thly, General remarks upon the dislocations of the stratified deposits, and the dependence of these phænomena upon the intrusion of trap rocks.

1. *Coal measures.*—In most parts of the productive coal-field the coal measures are covered by a considerable quantity of detritus, the greater part of which has been derived from the breaking up of the new red sandstone which once overspread this tract, with which are mixed, especially in the northern part of the field, a few boulders of northern origin and some from the surrounding region.

General and detailed sections are then given of the regular succession of the carboniferous strata; for the greater part of which in the neighbourhood of Dudley, and for much valuable information, Mr. Murchison expresses great obligation to Mr. Downing; the best sections of the Wolverhampton field having been afforded by Mr. J. Barker. The principal points of novelty consist in drawing a clear distinction between the upper or thicker measures, which contain the 10-yard coal, generally known as the Dudley coal, and the *underlying* carbonaceous strata, or ironstone measures. The latter, rising from beneath the 10-yard coal, range to the N.N.E. from Wednesbury and Bilston, in a long tract between the parallels of Walsall and Wolverhampton, extending to Cannock Chase. At the southern end of the field, emerging from beneath the 10-yard coal, they occupy the district between Stourbridge and Hales Owen, containing the well-known "fire clay;" though some of the most valuable of the Wolverhampton iron-stones, beneath those called the "New Mine," are here wanting, viz. the "Gubbins," and "Blue Flats." This poverty in the lower coal measures extends over all the district south of Dudley. In the northern and southern ends of the district these lower measures represent the whole carboniferous system; and in various natural sections near the Hagley and Clent Hills, the author has detected them, in very feeble bands, passing upwards and conformably into the lower new red sandstone. Besides the open works formerly alluded to by him in previous memoirs, Mr. M. now states, that his former conjectures respecting the passage of the 10-yard coal beneath the new red sandstone which flanks it on the east and west have been verified by the efforts of the Earl of Dartmouth, who, after sinking to a depth of 151 yards through strata of the lower new red sandstone, has very recently succeeded by further borings, carried down to the depth of 290 yards, in discovering the 1-foot, 2-foot, and "Brooch" coal seams, which *overlie* the 10-yard coal throughout the Dudley field. These operations have taken place at Christchurch, one mile beyond the superficial boundary of the coal-field.

Besides the plants so common in all carboniferous tracts, the author has observed the presence of animal organic remains. Unios of several species are abundant; and in the northern or lower part of the field he has extracted fragments of fishes, which have been named by Professor Agassiz,

Megalichthys Hibbertii,
M. Sauroides,
Diptodus gibbus;

together with scales, coprolites, &c., proving an identity between the animals deposited in these coal measures and those of Edinburgh, described by Dr. Hibbert. The same species, it will be recollected, have been pointed out by Sir Philip Egerton as occurring in the N. Staffordshire coal-field, and one of them has been observed by Mr. Prestwich in the coal-field of Coalbrook Dale. Mr. Murchison, however, remarks that he has not yet observed any marine remains in these coal measures similar to those of Coalbrook Dale; and nothing *yet* found can invalidate the inference that the coal of Dudley and Wolverhampton may have been accumulated exclusively in fresh water.

b. Silurian rocks.—The mountain or carboniferous limestone and the old red sandstone, which in so many other parts of England form the support of coal tracts, being wanting, this field reposes directly on rocks which Mr. Murchison proves to consist of the two *upper members of the Silurian system*, viz. "the Ludlow rocks" and "Wenlock limestone."* As, however, these rocks rise up irregularly, like separate islands, through the surrounding coal measures, and not in their regular order of superposition, so it was obviously impracticable to have determined their *relative* age by any local evidences; and hence no attempts could have been made to distinguish the younger from the older deposits, until the structure and organic remains of the different members of the Silurian system, had been fairly worked out in other districts, where these types were fully and clearly displayed in their regular order.

2. *Ludlow rocks.*—These rocks appear at the surface in three detached points in this coal-field, viz. Sedgeley, Turner's Hill, and the Hayes. At Sedgeley they are thrown up in an elongated ellipse, very

* There is one spot, however, within the author's knowledge where the underground works reached a thick mass of red shale or marl *beneath* the coal-field; but the works having been long abandoned, no correct knowledge of these red rocks can be now obtained.

much resembling a large inverted ship, of which Sedgeley Beacon, 630 feet above the sea, may be considered as the keel. The upper Ludlow rock, though not thick, is plainly marked by containing the *Leptæna lata*, the *Serpula gigantea*, &c., and by overlying a limestone which is in every respect identical with that of *Aymestrey* or the middle member of the *Ludlow* rocks, presenting the same lithological structure, *i. e* a dull argillaceous grey limestone, which among other well-known shells, such as the *Terebratula Wilsoni* and the Lingula, contains also the beautiful *Pentamerus Knightii* so entirely *peculiar* to this stratum. As at Ludlow and Aymestrey, this limestone of Sedgeley, known here as the "black limestone," forms an excellent cement under water.

Turner's Hill, a small elevation between Gornals and Himley, is composed of Ludlow rocks; and the Hayes is a narrow short tongue of the same, with a central band of limestone, which rises at a high angle from beneath the coal measures, on the main road from Stourbridge to Hales Owen, a portion of the lower Ludlow rock being also well exposed.

2*a*. "*Wenlock limestone.*"—This limestone formation is much more largely developed than that of the Ludlow rocks, constituting several ellipsoidal masses near the town of Dudley, which have been long worked, and extensively known among collectors, from the number and beauty of their organic remains. Hence the rock has been usually termed the "Dudley limestone." As, however, it was impossible to have ascertained in this district the relative age of these rocks, their different members being independently in contact with the coal measures, the nomenclature of the Silurian system already selected is adhered to, because in Shropshire the Wenlock limestone, in its fullest standard, rises out regularly from beneath the Ludlow rocks, and the latter passing beneath the old red sandstone and carboniferous limestone (both of which are wanting at Dudley) complete the proofs required. The author therefore entreats geologists not to employ the term Dudley limestone except as the synonym of Wenlock, with which he proceeds to show its lithological and geological identity. This limestone is described in detail at the Castle Hill, Wren's Nest, and Hurst Hill, in all of which it forms ellipsoidal elevated masses, 500 to 650 feet high, protruding through the coal measures in lines parallel to similarly shaped masses of Ludlow rock at Sedgeley; &c., *i. e.* trending from 10° E. of N. to 10° W. of S. Two strong bands of limestone occur in these hills, overlaid and separated from each other by shale, charged with numerous small concretions of impure limestone, the "bavin" of the workmen. The limestone having been quarried out from these bands which have been raised up from a common centre, and disposed with a quâquâversal dip at high angles, it is evident that the hills themselves would ere now have been demolished, had they been composed throughout of calcareous masses of equal purity; but the "bavin" or refuse composes the framework of these perforated hills, and preserves their outline. The Wenlock shale, or underlying part of the formation, constitutes the nucleus of the Wren's Nest, the largest and most perfect of these ellipsoids, and of this the author gives a detailed plan. These ellipsoids usually feather off at one extremity with a broken-down margin, and thus complete their resemblance in physical features to ancient craters of eruption *. The greatest superficial extent of the Wenlock formation is in the neighbourhood of Walsall, where it rises both in dome-shaped masses and in rectilinear ridges, running from S.S.W. to N.N.E. parallel to the axis of the Wolverhampton coal-field, of which one of these ridges forms the eastern boundary, the limestone plunging beneath the coal-field at a rapid angle. The other ridge is continuous with the new red sandstone of the Bar-beacon, and is known as the Hay Head lime. In the Dudley or 10-yard coal tract few works have yet proceeded downwards beneath the lower coals, and hence the subjacent Silurian rocks are little known to the miners. A remarkable and accidental discovery of a mass of limestone took place recently near Dudley Port, on the rise side of a great fault, which bounds the downcasts of the coal, called "Dudley Trough." Having worked out the coal on the upcast side, a shaft was sunk in and upon the southern side of this fault, when at a depth of 208 yards, and about 100 yards below the exhausted coal strata, a mass of limestone was met with, which proved to be near 7 yards thick, and of very good crystalline quality. Being found to extend in a form more or less horizontal, extensive works were promptly opened in it for the extraction of a rock so precious in the heart of the coal-field. When the author visited it, a considerable cavity had been formed, in which no trace of moisture was discernible, whilst it was known that copious streams of water were flowing in the coal measures overhead. He accounts for this mass of limestone being hermetically excluded from the percolation of water, by the impervious nature of the Silurian shale which separates the coal measures from the limestone, and by the shafts being sunk in the fault itself, which, like other lines of fissure, is filled up with clay and other materials, so closely compacted as to form complete dams to water. At the north-western edge of the subterranean excavation the fault was stripped, and the materials of which it is composed having thinned out, the limestone was found in contact with a bed of coal, the edges of which appeared *bent*, both the *coal and the limestone* having a slickensides polish. By boring through the limestone a second calcareous stratum was found, thus completing the proofs of identity between this underground mass and that which rises to the surface in the hills of Dudley Castle and the Wren's Nest.

In the northern or Wolverhampton field, where the whole of the coal measures, even to beneath the lowest beds of ironstone, (the *blue flats,*) are traversed by shafts not exceeding 120 yards in depth, the field has been proved at several points to rest on shale and impure limestone, the equivalents of the Ludlow and Wenlock formations. For lists of the fossils in this group of Upper Silurian rocks the author refers to previous memoirs, announcing that more perfect lists will shortly be laid before the public in his large work upon the Silurian system.

* See account of Valley of Woolhope for similar phænomena on a larger scale, and with a greater number of concentric and enveloping formations. —Lond. and Edinb. Phil. Mag., vol. iv. p. 372.

3. *Lickey Quartz rock, Caradoc sandstone,* (*Lower Silurian rocks.*) Dr. Buckland first called the attention of geologists to the Lickey quartz rock* ; and, showing that it had been one of the principal magazines of the quartz pebbles in the new red sandstone and diluvium of the southern counties, he further compared it with certain rocks *in situ* in the neighbourhood of the Wrekin. The Rev. J. Yates has also clearly described the lithological structure of this rock, and has briefly touched upon some of its fossils †. Mr. Murchison undertakes to prove the true geological position of these rocks. He shows that they lie in the direct prolongation of the Silurian rocks of Dudley, and that, being partially flanked and covered by thin patches of coal, they emerge through a surrounding area of the lower new red sandstone and calcareous red conglomerate (described in previous memoirs). Unlike, however, the succession in the Dudley field, there are here no traces of the Ludlow rock and Aymestrey limestone. Nor are there masses of any size of the Wenlock limestone, but shreds only of the shale or lower part of this formation with some of its well-recognised fossils, (*Colmers.*)

The lower Silurian rocks rise from beneath the Wenlock shale in thin courses of bastard limestone, alternating with red and green courses of sandstone and shale, the equivalents of those bands, which at various places in Shropshire, and at Woolhope in Herefordshire, constitute the top of the formation of Caradoc sandstones. Like these, they are here underlaid by flaglike sandstones, sometimes rather more argillaceous and approaching to clay slate, the whole passing down into siliceous sandstones, both thick and thin bedded. In the latter are casts of several fossils of the Caradoc formation, such as Pentameri of two species, and corals peculiar to it. These fossilliferous strata are well exposed on the eastern side of the hills by recent cuttings, where the new road from Bromsgrove to Birmingham traverses the ridge. The ridge itself, however, consists essentially of quartz rock, which the author shows is nothing more than altered Caradoc sandstone, precisely analogous to that which he has on former occasions pointed out on the flanks of Caer Caradoc, the Wrekin, Stiper stones, &c. In those districts the passage from a fossiliferous sandstone to a pure quartz rock has been accounted for by the latter being in absolute contact with eruptive masses of igneous origin; and here it is suggested that the same cause may have operated, though the contact is not visible, because the line of quartz rock is precisely upon the prolongation of the trappean axis of the Rowley Hills, whilst the southern end of the parallel outburst of the Clent Hills, is but little distant. Notwithstanding their highly altered condition, it is shown that all the quartz rocks throughout this ridge of low hills are uniformly *stratified*, the dip being either to the E.N.E. or W.S.W., *i. e.* at right angles to the direction; and the parallelopipedal fragments into which the rock breaks are shown to be produced by fissures more or less at right angles to the planes of stratification; these fissures being so

* Transactions Geol. Soc., 1st Series, vol. v. p. 507.

† Transactions Geol. Soc., 2nd Series, vol. ii. p. 137.—[Also Phil. Mag., First Series, vol. lxv. p. 297.]

numerous where the mass is much altered, as almost to obscure the true laminæ of deposit.

4. *Trap.*—The composition and characters of the trap rocks and basaltic masses of the Rowley Hills are first described, together with the manner in which they are supposed to rise through and cut off the coal upon their flanks. Rocks of similar origin occur at various detached points to the west of Dudley, of which Barrow Hill is the principal, affording the most convincing proofs of the volcanic mass having burst through the carboniferous strata, since the latter are not only highly disturbed and broken, but fragments of coal and coal measures, in highly-altered conditions, are found twisted up upon the sides, and even mixed with the trap itself. In the Wolverhampton or northern coal-field, the chief vent of eruption is at Pouk Hill, two miles west of Walsall, where the greenstone is arranged in fan-shaped columns. After pointing out distinct evidences of the intrusion of similar rocks at Bentley Forge and the Birch Hills, in some of the old open works near which the trap is seen to overlie the coal, the author gives various sections of subterranean works, which prove the existence of greenstone, in bands more or less horizontal. As these bands of trap have jagged edges, are of limited extent, of exceeding irregularity in thickness, and often produce great alteration upon the inclosing carbonaceous masses, the author has no hesitation in expressing his belief that they are not true beds, but simply wedges of injected matter which have issued from central foci, and have been intruded laterally amid the coal strata; an opinion formerly expressed by Mr. A. Aikin in an able memoir*.

Although these lateral masses of greenstone in the Wolverhampton field are of origin posterior to the accumulation of coal strata, the author does not deny that the tufaceous conglomerates of Hales Owen, which have a strong analogy in composition to a certain class of volcanic grits described in former memoirs, may have been formed contemporaneously with the carboniferous deposits.

The trap of the Clent Hills is then briefly described, and is shown to be identical with that of the Abberley Hills, also mentioned in previous memoirs.

5. *Principal lines of dislocation.*—The whole of this carboniferous tract has been upcast through a cover of new red sandstone, the lower members of which are frequently found to have been dislocated conformably with the inferior carbonaceous masses, proving (as formerly expressed by Mr. Murchison) that some of the greatest of these movements took place *subsequently* to the deposit of the red sandstone. In describing the faults along the boundary of the new red sandstone, he directs particular attention to that of Wolverhampton, where the coal measures dip slightly inwards from the line of fissure, along which they are conterminous with the *overlying* strata, a fact perhaps without parallel in this or the adjacent coal-fields (including Coalbrook Dale), the usual phænomena being that, however disrupted, the carbonaceous or upcast strata always incline outwards, as if they would pass eventually beneath the lower new red sandstone on their flanks. This

* Transactions Geol. Soc., 1st Series, vol. iii. p. 251.

exception is supposed to have been caused by the upheaving of a subjacent mass of Silurian or trap rocks close to the edge of the line of fault.

Having next described the effect of the great longitudinal faults produced by the upcast of the Wenlock limestone of Walsall, he shows that the subterranean mass at Dudley Port (p. 492), is upon the same parallel, *i. e.* from N.E. to S.W., if not directly on the same line of fissure. This line of eruption is strongly marked on both edges of the northern half of the coal-field extending to Cannock Chase.

Another great axis of elevation which affects the Dudley field, diverges at a considerable angle from the former. It is prominently marked by the line of the Rowley Hills, and after concealment for a certain distance beneath the red sandstone to the S. of Hales Owen, reappears in the ridge of the Lickey quartz rock. The lofty trappean ridge of the Clent Hills is parallel to this last-mentioned axis. It is further pointed out as remarkable that at the angle formed by the confluence of these divergent lines of elevation, the Silurian or fundamental rocks of the tract are raised in inflated ellipsoidal forms from common centres, the strata having a quâquâversal dip, in one case completing the outlines of a very perfect valley of elevation. The author infers that such curvatures are exactly what might be expected at the point of greatest flexure in the axis of the coal-field, where the volcanic matter, unable to find issue, has produced these inflated masses. There are numberless faults in this coal-field to which no reference is made, it being stated that much additional labour is required to give a complete history of them; but attention is called to the Birch Hill, Lanesfield, and Barrow Hill faults, which are the principal *transverse* faults, and which the author conceives may be explained upon the principles of the theory of Mr. Hopkins, or as cross fractures which have resulted from elevation of the coal-field *en masse.*

The memoir concludes with referring to the importance of one of the problems to which the author has been directing public attention during the last few years, viz. the probable extension of carboniferous tracts of the central counties beneath the *surrounding* new red sandstone; and he rejoices that the deductions which necessarily follow from his observations in this and the adjacent coal-fields, have recently been so ably supported by the masterly observations of Mr. Prestwich upon Coalbrook Dale, with whose opinions he entirely coincides.

The quantity therefore of unwrought coal beneath the new red sandstone of Shropshire, Worcestershire, Staffordshire, &c. though previously omitted in statistical data, must form an element in all calculations concerning the probable duration of the carboniferous wealth of the empire.

May 25.—A paper was first read "On the part of Devonshire between the Ex and Berry Head and the Coast and Dartmoor;" by Robert Alfred Cloyne Austen, Esq., F.G.S.

The formations of which the district consists are transition rocks, new red sandstone, greensand, and trap.

The transition rocks are sometimes arenaceous, more often slaty, and contain beds of limestone rich in organic remains. The only portion of the system considered by the author as undescribed is a conglomerate, 100 feet thick, which occurs at the Park at Ugbrook. It is composed of rounded quartz pebbles and fragments of clay slate, united by a siliceous cement. It alternates in the upper part with beds of clay slate, and is older than any of the limestones of the country. These transition formations are traversed by numerous faults, the strata being thrown into the wildest confusion. In some places beds of trap are regularly interposed without producing any effect upon the adjacent strata; but in other localities dykes intersect the sedimentary deposits, and have produced great alterations both in their structure and dip.

The new red sandstone consists in the lower part of fine-grained fissile sandstone, and a coarse conglomerate, formed out of the surrounding older formations, including partially rounded fragments of slate, limestone, porphyry, greenstone, &c. This formation is also much disturbed by faults, some of which, the author thinks, are contemporaneous with the deposition of the sandstone, as they appear to affect the lower and not the upper beds in the same section.

The elevation of the greensand of the Haldons, Mr. Austen thinks, was due to the action of a subjacent mass of trap, portions of which are visible at the extremities of the hills: and he is of opinion that the preservation of these insulated patches of greenstone has been owing to their having been raised above the level of the waters which denudated the surrounding districts.

In conclusion the author briefly reviews the geological phenomena which this part of Devonshire presents, and infers from them, that during the transition epoch there were submarine volcanic irruptions, as shown by the interstratified trap; that the number of organic remains in the limestone prove that the ocean teemed, in parts, with life: that the new red conglomerate was due to the breaking up of the transition formations: that there were irruptions of trap at later periods, as proved by the dykes in the new red sandstone; and that Dartmoor was elevated after the deposition of the greensand, as the first traces of granitic debris are found in the Bovey deposit.

A notice was next read on the supposed existence of the Lias formation in Africa, by Roderick Impey Murchison, Esq., F.G.S.

Mr. Leach, of Milford Haven, a short time since presented to the Society some organic remains, stated to have been obtained by Commodore Sir Charles Bullen on the west coast of Africa. As these organic remains agree exactly with fossils of common occurrence at Lyme Regis, it was conjectured that some mistake might have occurred respecting them; but Mr. Leach has been subsequently informed by Sir Charles Bullen that they were collected by himself and officers at West Bay, Fernando Po, Accra, and Sierra Leone, and that they occur in abundance.

Mr. Murchison also announced in this notice, that Sir John Herschel has discovered Trilobites in a rock which occurs to the north of the Cape of Good Hope.

A paper was then read, entitled " A Notice on Maria Island, on the east coast of Van Diemen's Land, (S. lat. 42° 44′ E., long. 148° 8′,) by George Frankland, Esq., Surveyor-General of the Colony; and communicated to the Society by Robert W. Hay Esq., Under Secretary of State for the Colonies.

Maria Island is composed, for the greater part, of trap; but strata of freestone well calculated for building purposes frequently occur, and at the northern point of the island is a perpendicular cliff, from 200 to 500 feet high, composed of dark grey limestone, formed of oysters, muscles, and other shells, in a state of great preservation. On the eastern coast, near Cape Mistaken, are numerous caverns, some at the height of 600 feet above the level of the sea, the roofs of which are studded with stalactites. Mr. Frankland states that Van Diemen's Land in every part furnishes strong evidence of the ocean having once occupied a much higher level than at present. The paper also contains much valuable information respecting the natural productions of the island.

A letter was next read on the geology of the country included in the S.W. quarter of the Daventry, or 55th sheet of the Ordnance Survey, by J. Robison Wright, Esq., F.G.S., and addressed to Capt. Mudge, R.E., F.G.S.

The surface contained in this quarter sheet is about 168 square miles, including the towns of Southam and Kineton, and the field of the battle of Edge Hill. The formations of which the district consists are the new red sandstone, the lias, and the inferior oolite.

A notice on the occurrence of marine shells in a bed of gravel at Narley Bank, Cheshire, by Sir Philip Grey Egerton, Bart., M.P., F.G.S., was then read.

In proceeding from the valley of the Weaver, at the point where it is crossed by the Liverpool and Birmingham Railway, towards Delamere Forest, are two acclivities, each about 60 feet high, and distant about a mile and a half from each other. Narley Bank is situated on the summit of the second ridge, and the gravel-pit is in the face of the northern declivity, about 157 feet above low-water mark at Weston Point, and six miles from it. The gravel differs from the common gravel of the country by the prevalence of calcareous matter and the small proportion of fine sand; but agrees with the gravel at the Willington, described in a former paper*. This resemblance induced the author to search for shells, and he found, at a second visit to the pit, several imperfect fragments of apparently recent marine shells.

In conclusion, Sir Philip Egerton states that he has always found a marked distinction, in Cheshire, between the gravel containing recent shells and that which does not; and he infers, from the former being occasionally covered, as at the Willington, by a thick deposit of sand and gravel, that it was accumulated before the occurrence of the last drift, to which he ascribes the origin of the common detritus of the county.

A paper was afterwards read, entitled "Accompanying remarks to a section of the Upper Lias and Marlstone of Yorkshire, showing the limited vertical range of the species of Ammonites and other Testacea, with their value as geological tests," by Louis Hunter, Esq., and communicated by John Forbes Royle, Esq., F.G.S.

* Lond. and Edinb. Phil. Mag., vol. vii. p. 326.

The portion of the coast to which this paper immediately refers is called the Easington Height, situated between Whitby and Redcar, and presents the following details:

INFERIOR OOLITE.		
Upper Lias Shale.		
Shale		35 feet
Hard or cement stone bed		25
Shale, containing nodules of ironstone		90
Jet rock	20 to	30
Hard compact sandy shale		30
	about	200
Marlstone.		
Thin seams of shale, alternating with hard ironstone bands, a foot thick		25
Sandy shale, with beds of dogger		63 ?
Alternating beds of calcareous sandstone and sandy shale		40
Shaly sandstone, passing gradually into the lower lias shale		30 ?
		160
Lower Lias Shale		150

The beds of shale superior to the jet rock are characterized by the presence of *Nucula ovum*, *Orbicula reflexa*, *Plagiostoma pectinoide*, *Ammonites communis*, *A. heterophyllus*, *A. fimbriatus*, *A. Walcottii*, *A. subcarinatus*, *A. angulatus*, *A. crassus*, *A. fibulatus*, *A. subarmatus*, *A. Lythensis*, *A. Boulbiensis*, *A. annulatus*, *Nautilus astacoides*, and *Belemnites elongatus*. The species gradually decrease in abundance on approaching the jet rock, and the specimens which do occur in that stratum are stated to be smaller than in the higher beds. The jet rock contains a peculiar suite of Ammonites, viz. *A. elegans*, *A. signifer*, *A. elegantulus*, *A. exaratus*, *A. Mulgravius*, *A. concavus*, and *A. ovatus*. It is also distinguished by containing the remains of the gavial-snouted crocodile. With respect to the relative abundance of the fossils, Mr. Hunter observes that where they occur in the greatest number they are smallest in size.

The beds situated between the jet rock and the marlstone are very poor in fossils.

The marlstone series is distinguished not only by a change in the species, but in the preponderance of bivalves and the comparative rarity of Ammonites; the characteristic fossils being *Avicula cygnipes*, *A. inæquivalvis*, *Pecten sublævis*, *P. æquivalvis*, *Pullastra antiqua*, several species of Terebratula, *Cardium truncatum*, *Modiola scalprum*. The species of Ammonites are few, *A. vittatus* occurring about the centre of the series, and *A. maculatus* at the junction with the lower lias shale.

In conclusion, Mr. Hunter states that the difference between the distribution assigned to the fossils by himself and other authors may be owing to the prevalent practice of collecting fossils from subsided masses, and not from undisturbed portions of the cliffs.

A letter was, lastly, read from Robert Fitch, Esq., of Norwich, to Edward Charlesworth, Esq., F.G.S., on the discovery of the Tooth of a Mastodon in the crag at Thorpe, near Norwich.

The pit in which the tooth was found is stated to present the following section:

Top.	Alluvium	5 feet
	Gravel	6
	Brick-earth, sand, and gravel	14
	Crag	5
	Large chalk flints, mixed with crag shells, principally Pectens	
	Chalk	

It was in the bed of large chalk flints that Mr. Fitch found the tooth; and he adds that Thorpe adjoins the parish of Whitlingham, in which Mr. William Smith discovered the tooth figured in his "Strata Identified."

June 8.—A paper was first read, entitled, "Notice respecting a piece of Wood partly petrified by Carbonate of Lime; with some remarks on Fossil Woods, which it has suggested." By Charles Stokes, Esq., F.G.S.

Mr. Stokes lately received from Germany, with a collection of fossil woods, a piece of recent wood, stated to have been found in an ancient Roman aqueduct, in the principality of Lippe, in the Bückeberg, in which some parts are petrified by carbonate of lime, while the remainder of the wood, though in some degree decayed, is not at all mineralized. This fact has afforded an explanation of the peculiarities of some other instances of fossil wood, in which different parts of the specimen present different appearances. Two other instances are particularly described: one of silicified wood from Antigua, and one of a calcareous petrifaction from Allen Bank in Berwickshire. In both these cases it is inferred by the author, that the process of petrifaction commenced simultaneously at a number of separate points, and that it was suspended when only parts of the wood had been petrified. The unchanged parts would then be liable to decay; and in the specimen from Antigua the process has been renewed after this remaining part had decayed in a considerable degree, when that also became silicified. In the calcareous petrifaction from Allen Bank (which is described and figured by Mr. Witham, in his work on the structure of fossil vegetables), the parts which had not been petrified at the time the process was interrupted, have been entirely destroyed by the decay which then ensued, and the intermediate spaces have been filled up by the crystallization of carbonate of lime, without the removal of the petrified portions from the positions in which they grew and in which they had become mineralized.

In the specimen from the Roman aqueduct the petrified portions run in separate columns through the wood, as if conducted downwards by the vessels or woody fibres. In that from Allen Bank the separate portions are spherical in form and independent of each other; and in that from Antigua they are independent, and though nearly spherical not regularly so. Hence the author infers that a different explanation must be sought for the manner in which the solution of mineral matter was supplied in the first instance from that of the two last.

The paper notices also the fossil wood from Lough Neagh and Bonn, in which some small parts preserve their texture, although remaining still unchanged in the midst of the petrified mass.

The author concludes with a short notice of the different conditions in which the structure of wood is preserved in different specimens, and considers that the condition of the wood has not any influence on the process of petrifaction.

A paper was next read, entitled, "Further notice on certain peculiarities of Structure in the Cervical Region of the Ichthyosaurus," by Sir Philip Grey Egerton, Bart., M.P., V.P.G.S.

In a former communication* Sir Philip Egerton gave an account of the cervical vertebræ of the Ichthyosaurus, and announced the discovery that the atlas and axis are firmly united and strengthened below by an accessory articulating bone. In this paper he shows, that the union of the two vertebræ is perfect at all periods of the animal's growth, and apparently in all the species of the genus hitherto discovered, having observed it in vertebræ varying in size from half an inch to seven inches and a half in diameter. Externally there is a strong line of demarcation between the two bones, but internally the cancelli appear to pass from one to the other. The atlas, independently of the union of the two vertebræ, is distinguished by the form of the anterior cavity for the reception of the basilar process of the occipital bone; by the outer margin being rounded instead of sharp, and by the triangular facet on the inferior part of the circumference for the reception of the accessory bone: the axis, independently also of its union with the atlas, differs from the other vertebræ, by the facet on the under surface for the reception of the accessory bone: and the third vertebra is also distinguished from the remaining bones of the neck by a facet for the articulation of a very small accessory bone. The intervertebral cavities of the 4th and 5th cervical vertebræ, the author states, are less than in the vertebræ of the dorsal and caudal regions, and the anterior cavity is considerably smoother than the posterior one of the same vertebræ.

Sir Philip Egerton states that the spinal column does not, as described by other authors, decrease in diameter from the middle dorsal vertebra to the atlas, but that the minimum diameter is attained about the fifth cervical vertebra, from which point to the occipital bone the increase in size is very rapid, the atlas being fully one fifth more in diameter than the above-mentioned bone.

In the former memoir Sir Philip Egerton described only one accessory bone in the cervical region of the Ichthyosaurus; but in this paper he proves that there are three, and proposes to designate them by the name of subvertebral wedge-bones. One of them is supplementary to the atlantal socket, another is common to the atlas and axis, and the third, which agrees in form with the second, but is much smaller, articulates on the under surface of the third vertebra.

The author, then, in conclusion, enlarges upon the admirable

* Lond. and Edinb. Phil. Mag., vol. vii. p. 414.

adaptation of the structure of the Ichthyosaurus to the habits of the animal.

A communication was afterwards made "On the coal-fields on the north-western coast of Cumberland, &c., &c.:" by the Rev. Professor Sedgwick, M.A., F.R.S., F.G.S., and Williamson Peile, Esq., of Whitehaven, F.G.S.

In a former paper* the authors described the range of the carboniferous limestone from the neighbourhood of Kirkby Stephen to Egremont; and showed that the formation admitted of two divisions: the lower representing the scar limestone of the Yorkshire sections, the upper (also like the Yorkshire sections) exhibiting alternations of limestone, sandstone, and shale, with thin seams of coal. They commenced with a short notice of rocks and sections made through this upper division, which in its range towards the western coast of Cumberland, appears gradually to thin off, and lose its importance. They then proceeded to describe in more detail, and with many illustrative sections, a still higher coal-field; which is on the same parallel with the great Northumberland and Durham coal-fields, and in the quantity of carboniferous beds subordinate to it, is in no respect inferior to them.

This field is bounded by the red sandstone of St. Bees Head; by the carboniferous limestone, in a part of its range above described; by the sea coast between St. Bees Head and Maryport; and by the new red sandstone in its range from Maryport to Chalk Beck near Rosley. The whole system appears to thin off near Rosley, and is succeeded by some sterile, alternating, masses of red shale and sandstone, to which the miners, though improperly, have given the name of the "*great red metal dyke.*" To the east of this series of red beds the rich upper coal-field never appears to have extended. From many borings and workings near Whitehaven, it is inferred that the upper division of this carboniferous limestone, as well as the millstone grit, have almost disappeared; and that the coal measures are brought nearly into immediate contact with the lower division of the limestone. In some places the whole limestone has thinned off, and the coal measures seem to rest almost immediately on the Skiddaw slate.

The authors commence their details, in the present paper, with an account of the Whitehaven coal-field, which they separate into three divisions: the southern, or How-Gill colliery; the middle, or Town field bounded by a great downcast dyke towards the north; and the northern, or Whin-Gill colliery, bounded by an anticlinal line which enters the sea near Parton. The strata found in these several parts of the field are described by the help of the sinkings and borings of Croft Pit, and by other sinkings in various parts of the field down to the limestone. A comparison is then made between this series of strata and those exhibited in corresponding sections of the Harrington and Workington fields; and it is shown that the whole series may be conveniently separated into two divisions: the upper, containing two principal bands of coal, called the "*Bannock Band,*" and

* Proceedings Geol. Soc., vol. ii. p. 198.

Whitehaven "*Main Band*"; the lower, containing many thin seams of coal, but only one band which has been much worked near Whitehaven. They then proceed to describe the most remarkable workings in the several divisions of the Whitehaven field; the new field to be approached by the Parton tunnel; and the extension of the "Main and Bannock band" to the hills S.E. of Dissington; but these details, as well as an account of the works attempted in a small triangular field bordering the sea to the north of Parton, are necessarily passed over in this abstract.

They then describe the Harrington coal-field, bounded to the north and south by two enormous faults; between which the country is occupied by the lower division of the coal measures. It is impossible in an abstract to describe the complicated *faults* that everywhere intersect this field, and by which the limestone is in two instances brought up to the surface. The coal beds, worked within it, are five in number, and are described, in descending order, under the following names: (1.) Metal Band; (2.) Two-feet Band; (3.) Yard Band; (4.) Four-feet Band; (5.) Yendale Band. By help of a transverse section to Castlerigg, this field is connected with the upper division of the coal measures; in as much as pits have been sunk near that place, through the great beds of the upper division, down to the *two-feet band*; thus giving a consistency and unity to all the sections.

The authors next describe the Workington field, bounded to the south by the great *fault* which brings in the lower division of the Harrington field; and to the east and west by the sea and the turnpike road. The river Derwent was formerly regarded as its northern boundary; but the *main band* unfortunately thins out a little to the south of Workington, and thereby contracts the extension of the valuable part of this field. Nearly all the beds worked in this field belong to the upper division; and their general agreement with the Whitehaven bands of coal is proved by detailed sections; especially from the sinkings at Henry Pit near the mouth of the Derwent. The principal *faults* traversing this district, the outcrops of the principal bands of coal, and the extension of the works under the sea are described in some detail. Several other small divisions of the great field are then noticed: viz. the Starmire, Keekhill-Side, Brownrigg, Branthwaite-Edge, Gillgaron, and Graysouthern fields; after which the authors proceed to describe the phænomena on the north side of the Derwent.

To the north of the Derwent, there is near the sea-coast a sterile region, partly occupied by the *lower red sandstone*, and partly by the upper division of the coal measures, in which the main coal is wanting; a fact connected with the thinning off of the *main band* to the south of Workington. In a very extensive field, commencing a little to the N. of the village of Seaton, and extending over Broughton Moor, and from thence to Dearham, two beds of coal (known by the names of the "*ten-quarter band*" and the "*kernel and metal band*") have been very extensively worked, and are identified with the "*bannock-band*" and "*main-band*" of Whitehaven and Workington. The relations of the several parts of this extensive tract of country are exhibited in detailed sections, of which it is very difficult to convey a notion in a mere abstract; and the works carried on within them are, with very limited exceptions, referred to the upper division of the Whitehaven field.

The coal bands exhibited in the works near Gillerux, Aspatria, Plumland, and Weary Hall are then described; and detailed sections are given of the works in the Bolton field,—generally regarded as the north-eastern limit. There is, however, an unexplored tract to the east of a great fault which forms the northern limit of the Bolton field; and, in the neighbourhood of Rosley, a seven-feet coal; undoubtedly a member of the upper division) was formerly worked) though to a very small extent, in consequence of the complicated dislocations which intersect the district.

Having described, in the above order, the several portions of the great coal-field, and noticed some of its peculiarities of mineral structure, the authors endeavour to ascertain the limits of certain outlying masses of the *lower red sandstone*, of the *magnesian conglomerate*, and of the *new red sandstone*. From the facts stated, it appears that the coal measures pass, in some instances, in regular ascending order, into the lower red sandstone. In other instances, however, the coal measures appear to have undergone considerable movements of elevation before the existence of the lower red sandstone; in as much as the position of the two formations is discordant. Again, though the lower red sandstone forms the natural and immediate basis of the magnesian limestone and conglomerates, yet there are several places, within the south-western limits of the country described, where the conglomerates appear to have been deposited in hollows and inequalities presented by the waterworn beds on which they rest unconformably. From which facts it seems to follow, that the formations described in this paper have undergone, during their development, two considerable movements, affecting the position of the component strata: 1st, a partial movement of the coal measures, anterior to the deposition of the lower red sandstone; 2ndly, a partial movement, both of the coal measures and the lower red sandstone, anterior to the formation of the magnesian conglomerates.

This being the last evening of the Session, the Society adjourned, at its close, to Wednesday, November the 2nd.

ZOOLOGICAL SOCIETY.

May 10.—The following Note by the Rev. H. Dugmore was read.

"Lieut. Col. Mason, of Neeton Hall (four miles from Swaffham), has had a *Sea Eagle, Haliæetus albicilla*, Sav., in confinement for the last sixteen years. About a month since, it dropped an egg, which is now in my collection. The egg is perfectly white, and not quite so large as that of a *Goose*: the shell is rather harder."

A letter was read from Capt. Green of Buckden, Huntingdonshire, descriptive of a very fine specimen of the barn-door *Hen* in his possession, which has assumed the *Cock* plumage: the change took place about three years ago. The bird has since been presented to the Society by the writer.

Mr. Owen read the following Notes on the Anatomy of the *Wombat, Phascolomys Wombat*, Pér.

"The anatomy of the *Wombat* having already engaged the attention of Cuvier ('Leçons d'Anat. Comparée, *passim*) and Home (Phil. Trans. vol. xcviii. 1808, p. 304,) but little remains to be added on that subject.

"The individual lately dissected at the Museum of the Zoological Society had lived at the Gardens upwards of five years. The one which was dissected by Sir Everard Home in 1808 was brought from one of the islands in Bass's Straits, and lived as a domestic pet in the house of Mr. Clift for two years. This animal measured two feet two inches in length, and weighed about 20lbs.: it was a male. The Society's specimen was a female, and weighed, when in full health in October 1833, 59½lbs.

"On removing the integuments of the *abdomen*, much subcutaneous fat, of the lard kind, was observed.

"The muscles of the *abdomen* presented the same arrangement as in other *Marsupiata*; the internal pillars of the external abdominal rings being formed by the marsupial bones, round which a broad cremaster, emerging from each ring, wound inwards and upwards to terminate by spreading over the mammary gland.

"The digestive organs in the abdominal cavity presented a development corresponding generally to that which characterizes the same parts in the *phytiphagous Rodents*.

"The stomach precisely corresponded with the description and figure given by Home; but the occurrence of cardiac glands in the *Dormouse* and *Beaver* renders a similar structure in this *Marsupial*, in which the *Rodent* type of dentition exists, less extraordinary than it might otherwise appear. The *duodenum* commenced by a large pyriform dilatation, similar to that in the *Capybara* and *Spotted Paca*; beyond this part it presented a diameter of an inch; the small intestines then gradually widened to a diameter of 1½ inch, and as gradually diminished again to the diameter of an inch: their entire length was 11 feet 3 inches.

"The *ileum* entered obliquely the wide sacculated *colon*, the bulging commencement of which represented a short and wide *cæcum*; and from the angle between this part and the *ileum*, a cylindrical vermiform process 2 inches long, and 3 lines wide, was continued.

"The *colon* continued to be puckered up by two wide longitudinal bands into large *sacculi*, which could be traced becoming less and less distinct along an extent of the gut measuring five feet 2 inches. Cuvier observes that the large intestines were hardly more voluminous than the small*; in our specimen the *colon* measured 2½ inches in diameter, being more than double that of the *ileum*. But a more important difference was observed in the presence of a second *cæcum* at the distance from the first above mentioned. This consisted of a pyramidal pouch projecting 3 inches from the side of the gut, and communicating freely with the same at its base: its *parietes* were

* "Dans le Phascolome, les gros intestins ne sont guère plus volumineux que les petits." *Leçons d'Anat. Comp.*, nouv. ed.

thinner than those of the rest of the large intestine; it was situated below the pyloric end of the stomach, had only a partial investment of *peritoneum*, and adhered by a cellular medium to the *duodenum* and *pancreas*. Below this second *cæcum*, or lateral dilatation, the *colon* formed a large *sacculus*, and was then disposed in a series of smaller *sacculi*, which at length disappeared at a distance of 6 feet from the second *cæcum*; the rest of the large intestine, 3 feet in length, was of simple structure, and of smaller diameter, viz. 1⅛ inches.

"The internal surface of the small intestines presented some slight transverse corrugations; that of the *colon* was smooth, except below the second *cæcum*, where the lining membrane was corrugated irregularly; and a small patch of glands was here observable.

"The *rectum* terminated, as in other *Marsupials*, immediately behind the urethro-sexual aperture, and within a common outlet, both the excretory orifices being embraced by a common cutaneous sphincter.

"The liver was more completely separated into lobes than in the specimen dissected by Cuvier. Home is silent as to the structure of the liver; his observations respecting the digestive organs are limited to the peculiarities of the stomach. In our specimen the liver was divided by an extensive longitudinal fissure into two lobes, the right of which was again deeply subdivided into two, the gall-bladder being lodged in this second fissure: the gall-bladder was of an oval form, 2½ inches in length.

"The *pancreas* and spleen were both well developed, and had each the descending process which characterizes these parts in the *Marsupial* animals.

"The parotid glands were very thin, situated upon, and partly on the inner side of, the posterior portion of the lower jaw; they measured each 1¼ inch in length, and ½ inch in breadth; the duct passed directly upwards and outwards till it reached the orifice of the *sterno-cleido-mastoideus*; here it was buried in the cellular substance anterior to that muscle, then turned over the *ramus* of the jaw, and continued its course over the *masseter*, where it was slightly tortuous; it entered the mouth just anterior to the edge of the *buccinator*. The submaxillary glands were each about the size of a walnut; their ducts terminated, as usual, on each side of the *frænum linguæ*.

"The heart of the *Wombat* presented the usual peculiarities occurring in this part of the Marsupial organization; viz. 1st, the two appendages of the right auricle, one passing in front and the other behind the ascending *aorta*; 2ndly, the absence of the *annulus* and *fossa ovalis*; and 3rdly, the absence of the terminal orifice of the coronary vein which empties itself into the *cava superior sinistra* just before the wide termination of the latter vein in the auricle by the side of the *cava inferior*. The right auriculo-ventricular opening is widely open, and is guarded by an irregular narrow membranous valve, the outer portion of which is attached to the tendons of three *carneæ columnæ*; two of which are of a large size as compared with the third, and arise, as in the *Kangaroo*, from the *septum* near the angle where this is joined to the *parietes* of the ventricle. The muscular walls are continued obliquely upwards in a conical form to the origin of the pulmonary artery, somewhat resembling a *bulbus arteriosus*. This peculiarity is still more marked in the *Kangaroo*. The right ventricle descends nearer to the *apex* of the heart in the *Wombat* than in the *Kangaroo*, and the form of the heart is longer and narrower. The left auricle is smaller and more muscular than the right; the valve between it and the ventricle is, as usual, broader and stronger, and its free margin is attached to the tendons of two thick *columnæ carneæ*, having the usual origins distinct from the *septum*, leaving that part of the inner surface of the ventricle smooth for the passage of the blood to the *aorta*. The pulmonary veins terminate by two trunks in the left auricle.

"The lungs consisted of one lobe on the left side, and one on the right, with the *lobulus medius*; which was a small strip extended between the heart and diaphragm.

"The thyroid glands were elongated bodies of a dark colour, reaching from the thyroid cartilage to the seventh tracheal ring on each side.

"The kidneys were each $2\frac{3}{4}$ inches long, and 2 inches broad, and of a somewhat compressed oval figure; the *tubuli* terminated on a single obtuse *mammilla*.

"The specimen dissected by Cuvier being, like that examined by Home, a male, the female organs of the *Wombat* are only known by the description appended to the paper of the latter author, which relates to an impregnated individual. I found no part of the structure which supports the view taken by Sir Everard Home relative to the passage of the fecundating fluid to the *uterus*; the only natural communication between those cavities and the urethro-sexual canal being by the two lateral vaginal canals. The female organs consist, as in the *Opossum*, of two ovaries, two Fallopian tubes, two *uteri*, each opening by a separate *os tincæ* into a distinct *vagina*; the *vaginæ* having no intercommunication, but terminating in the common passage of Tyson, or urethro-sexual canal.

"The urethro-sexual canal is $1\frac{1}{2}$ inch in length; its inner surface is disposed in thick folds. The two anterior ones commencing united together form a semilunar fold above the urethral aperture; these folds are deeply intersected with oblique *rugæ*, the margins of which are villous, the *villi* becoming longer and finer as they approach the orifices of the true *vaginæ*. These commence $\frac{1}{2}$ an inch above the urethral orifice: their *parietes* are very thick for the extent of one inch, and the lining membrane of this part is disposed in minute longitudinal *rugæ*; it is then disposed in larger, coarser, and villous *rugæ*, similar to those of the first *vagina*, beneath which membrane several small vesicles were developed. Each of the true *vaginæ* having ascended with an outward curve for 2 inches, receives the *os tincæ* of its respective side, which is very projecting, and divided by deep fissures into numerous processes, resembling a short tassel. The *vaginæ* then descend to the upper part of the urethro-sexual canal, forming each a deep and large *cul de sac*, the inner surface of which is characterized by irregular villous *rugæ*, and the whole is highly vascular. The *culs de sac* are separate as in the *Opossum*, and do not communicate as in the *Kangaroo*.

"The *uteri* are each 2 inches long, and $\frac{3}{4}$ of an inch in diameter, somewhat flattened, pyriform, and giving off the oviducts from the inner or mesial part of their *fundus*. For the extent of an inch, the lining membrane presents a series of small but well-defined longitudinal *rugæ*, beyond which it assumes a fine texture, like velvet. The peritoneal covering of the *uterus* is reflected from it upon the ovarian ligament, the oviduct and the numerous vessels passing to the *uterus* on the outer side of this ligament, the duplicature or broad ligament containing which parts is $1\frac{1}{2}$ inch in breadth, and attached by its outer margin to the lumbar region of the *abdomen* as high as the kidney: just below this gland it is reflected upon the ovary, forming a large capsule for that part, and for the expanded extremity of the Fallopian tube, which presents an extraordinary development of fringe-like processes.

"The ovary presents the most distinct racemose structure which I have ever observed in the class *Mammalia*, consisting of about thirty ovisacs, of which the largest is half an inch, the smallest half a line in diameter; the whole ovary being of an oblong irregular figure $1\frac{1}{4}$ inch by 1 inch in dimensions. The mouth of the ovarian capsule is about 1 inch in width, the length of the Fallopian tube 3 inches."

Some Notes by Mr. George Bennett, Corr. Memb. Z.S., were read. They were transmitted from Sidney, New South Wales, in a Letter addressed to the Secretary, and bearing date October 25, 1835. They related to the habits of the *Spermaceti Whale*, and of the large species of *Grampus* known by the name of the *Killer*.

May 24.—A letter addressed to the Secretary by J. B. Harvey, Esq., Corr. Memb. Z.S., and dated Teignmouth, May 18, 1836, was read. It referred to a collection of various marine productions of the south coast of Devonshire, which accompanied it, and which were presented to the Society by the writer. These were exhibited.

Among them was a specimen of *Capros Aper*, La Cép., captured in Mr. Harvey's neighbourhood: and with the view of illustrating the colours of this species, he forwarded with it a painting made from the fish while yet recent. This also was exhibited.

With the collection were several specimens of a *Tubularia*, nearly related to *Tub. indivisa*, of which Mr. Harvey furnished a detailed description, accompanied by numerous figures. The description was read, and the figures were exhibited.

Mr. Harvey first observed the *Tubularia* in question at the steam bridge on the river Dart, where it grows in clusters between the links of the chain over which this floating bridge is propelled. The specimens obtained by him in this locality were necessarily injured in the hurried manner of taking them off during the rapid motion of the bridge; but as they were immediately placed in sea-water most of them have survived the force used in separating them, and he has thus been enabled to observe them for a week or ten days, during which he has carefully studied their form and structure. His drawings are intended to illustrate many of the different positions of the polype in various conditions as to growth, expansion, &c.

"This animal," Mr. Harvey remarks, "is evidently a *Tubularia.* It is something like *Tub. indivisa* figured by Ellis, Plate XVI. no. 2. fig. *c.*, but differs in several particulars. The tube of Ellis's *Tubularia* is jointed; the head has a lateral groove or opening; and the central projection (which is an elongation of the membrane covering the body) is much larger and higher, and is not surmounted by a row of slight long feelers. This *Tubularia* (for which, as a distinction, I submit the term *Tub. gracilis,*) has the tube hollow throughout and single; the body has no lateral groove; the central process has a row of fine long feelers near its termiination, and placed round the orifice: their office is to direct the food to the mouth. On the circumference of the cup is a row of very long flexible feelers, having much freedom of motion, and between each two of them is a smaller red feeler; from the circumference to the origin of the central process are two or three confused rows of alternate white and red short papillæ, giving the animal much the appearance of a flower.

"The powers of contraction and dilatation very much resemble those of the *Caryophyllia,* which I have still alive, and which I have kept for two years. Upon the slightest touch all the feelers are instantly contracted; but the shaking of the water does not at all incommode them. I kept several clusters in the same bowl with my *Caryophyllia*; but I found that, every time they came near it, (either by being touched or by shaking the vessel) they were devoured: I therefore, now keep them by themselves, but I fear that I shall not be successful in preserving them, as the river tide cannot be imitated in confinement.

"The locality of this polype is very confined. The Dart floating bridge is propelled upon two chains, about 6 feet distant from one another, and stretching across the river. On the western chain not a cluster could be seen, but on the eastern one there were upwards of a hundred groups of them, in spite of the immense friction to which they were exposed. They are only found within 100 feet of the northern shore at low water. I have since observed the same animals growing on the links over which the floating bridge at Devonport runs, and there they do not occupy a space exceeding 150 feet.

"The most singular circumstance attending the growth of this animal, and which I discovered entirely by accident, remains to be mentioned. After I had kept the clusters in a large bowl for two days, I observed the animals to droop and look unhealthy. On the third day the heads were all thrown off, and lying on the bottom of the vessel; all the pink colouring matter was deposited in the form of a cloud, and when it had stood quietly for two days, it became a very fine powder. Thinking that the tubes were dead I was going to throw them away, but I happened to be under the necessity of quitting home for two days, and on my return I found a thin transparent film being protruded from the top of every tube: I then changed the water every day, and in three days time every tube had a small body reproduced upon it. The only difference that I can discover in the structure of the young from the old heads, consists in the new ones wanting the small red *papillæ*, and in the absence of all colour in the animal."

The skin was exhibited of a species of *Cynictis*, Og., which had recently been presented to the Society by Captain P. L. Strachan, by whom it was obtained at Sierra Leone. The exhibition was accompanied by a description of the animal by Mr. Martin, which was read.

Mr. Martin regards the animal as especially interesting on account of its presenting the second instance of the new form among the *Viverridæ* which was described by Mr. Ogilby at the Meeting of the Society on April 9, 1833, under the generic appellation of *Cynictis*, and of which a detailed description and figure has since been published in the Transactions, vol. i. p. 29. It agrees with that genus, which is intermediate between *Herpestes* and *Ryzæna*, in its general form; in the number of the toes with which its feet are furnished; and in the number and form of its teeth, as far as they are preserved in the specimen exhibited, which, however, is that of a young individual. The points of the teeth are consequently in it unworn and acute: while in the specimen of *Cyn. Steedmanni* described by Mr. Ogilby, which was evidently an aged individual, the teeth were much worn down. The only other differences which exist between the teeth of the new species and those of *Cyn. Steedmanni* consist in the presence, in the outermost incisor in the upper jaw of the former, of a minute but decided internal tubercle, which is not found in the corresponding tooth of *Cyn. Steedmanni*; and in the inner lobe of the carnassier of the upper jaw being acute and conical, instead of blunt: the teeth behind this, in both jaws, are wanting in the specimen of the new species. The feet of the new species differ from those of *Cyn. Steedmanni* by their comparatively shorter claws; and by having a naked line extending along the under surface of the *tarsus* from the pad to the heel, the whole of the under surface of the *tarsus* being covered in *Cyn. Steedmanni* with hair.

The new species may be thus characterized:

Cynictis melanurus. *Cyn. saturatè rufus nigro punctulatus, ad latera pallidior; gulâ sordidè flavescenti-brunneâ; artubus internè abdomineque sordidè flavescenti-rufis; caudâ apicem versus latè nigrâ, ad apicem floccosâ.*

Long. *corporis* cum capite, 12 unc.; *caudæ*, pilis inclusis, 11; *capitis*, 2 unc. 1 lin.

In addition to the distinctive characters which have been noticed above, it may be remarked that *Cyn. melanurus* differs from *Cyn. Steedmanni* in the greater smoothness, shortness, and glossiness of the fur; in the less bushy character of the tail; in the dark tint of the head, back, and limbs; in the dusky colour of the throat; and in the black tip of the tail, the corresponding portion of this organ in *Cyn. Steedmanni* being white.

Mr. Ogilby remarked, that the animal described by Mr. Martin might probably be identical with the one noticed by Bosman under the name of *Kokeboe*; but added, that the notice given of it by that traveller was not sufficiently precise to admit of its being determined with certainty.

A specimen was exhibited of the *Chironectes Yapock*, Desm., on which Mr. Ogilby remarked as follows.

"I am indebted to Mr. Natterer for the opportunity of examining this rare and curious animal, of which he brought various specimens from Brazil. That now exhibited is a male, and possesses the same anomaly in the generative organs which characterizes the rest of the *Marsupials*. I have not seen the female, but Mr. Natterer informs me that the abdominal pouch is complete. The species is found in all the smaller streams of Brazil, and appears to extend from the southern confines of that empire, to the shores of the Gulf of Honduras; Buffon's specimen came from Cayenne, and a skin was recently obtained by Mr. W. Brown Scott, labelled '*Demerara Otter*.' Both this and Mr. Natterer's specimen agree with the figure and description of Buffon, except that they are of a larger size, and instead of a grey mark over each eye, have a complete band of that colour extending entirely across the forehead. In Mr. Natterer's specimen the terminal half-inch of the tail only is white; in Mr. Scott's, on the contrary, the last 4 inches are of this colour: the tail is exactly of the same length as the body; it measured 10 inches in the former specimen and 12 in the latter, but Mr. Natterer informs me that he has other specimens which measure 14 or 15 inches in length.

"The teeth of this animal are altogether different from those of the *Opossums* (*Didelphis*); and I am at a loss to reconcile my own observations with those of M. F. Cuvier upon this subject, as given in 'Les Dents des Mammifères' p. 73, unless by supposing that there must have been some mistake about the skull referred by M. Cuvier to the *Yapock*. For my own part, I could not be deceived in this matter, as the skull which I examined had never been extracted from the specimen. The incisors and canines are of the same form and number as in the true *Opossums*, the two middle incisors above being rather longer than the lateral, those below broader and a little separate. The molars are five on each side, two false and three real, both in the upper and under jaws. The first false molar is rather small and in contact with the canine, both above and below: the second is half as large again, and both are of a triangular form, with apparently two roots. The three real molars are of the normal form of these teeth among the *Opossums*. The first of the upper jaw is longer than it is broad, and has four sharp elevated tubercles with a low heel projecting backwards; the second resembles it in general form, but is larger and broader; the third is small and resembles the tuberculous molars of the true *Carnivora*. In the lower jaw the three real molars do not materially differ in point of size. They are narrower than those of the upper, have their tubercles arranged in a single longitudinal series, a single large one in the centre, and a smaller on each side.

"The *Yapock* has very large cheek-pouches which extend far back into the mouth, and of which the opening is very apparent. This circumstance, hitherto unobserved by zoologists, throws considerable light upon the habits of this rare animal, which thus appears, like the *Ornithorhynchus*, to feed upon freshwater *Crustacea*, and the *larvæ* of insects, spawn of fishes, &c. which it probably stows away in its capacious cheek-pouches. For 2 inches at the root the tail is covered with the same description of fine close fur as the body; from this part it tapers gradually to the point and is covered with small scales, arranged in regular spiral rows, and interspersed with bristly hairs, particularly on the under surface, a fact perfectly conclusive against the generally received opinion of this organ being prehensile in the *Chironectes*. Indeed, the tail so perfectly resembles that of the *Hydromys chrysogaster*, even to the white tip, that it would be impossible to distinguish these organs if separated from the respective animals. The useless appendage of a prehensile tail to an aquatic animal, must consequently be henceforth discarded from the history of the *Chironectes*, and the animal allowed to take its place among conterminous genera, not as a compound of anomalous and contradictory characters, but as a regular component link in the scale of existence. That its habits are purely aquatic, and that it has not the power of ascending trees, is further proved by the structure of the extremities. The hind feet are broad like those of the *Beaver*; the toes, including the thumb, united by a membrane, and, with the exception of the thumb, provided with small falcular claws; the thumb, as in all the other *Didelphidous Pedimana*, is without a claw. The fore-fingers are separate, very long and slender, (the middle and ring-fingers the longest of all,) and the last joint expanded and flattened as in the *Geckos*. The thumb is placed rather behind the general line of the other fingers, and seems at first sight to be opposable: it perfectly resembles those of the *American Monkeys**. The claws are very small and weak; they do not extend beyond the points of the fingers, nor even so far, and are absolutely useless either for climbing or burrowing. Considerably behind the others, on the outside of the wrist, there is a lengthened tubercle resembling a sixth finger, but much shorter than the others and without any bone. What purpose this unique organ may serve in the economy of the animal's life, it is impossible to conjecture, but the long slender fingers are probably used to pick out the food which it carries in the cheek-pouches."—W. O.

June 14.—Specimens were exhibited of various *Birds* from Northern Africa, which had recently been presented to the Society by Sir Thomas Reade, Corr. Memb. Z.S. They included the *Anas marmorata*, Temm., on which Mr. Gould remarked that in the form of the bill it approached nearly to the *Pin-tailed Duck*, *Anas acuta*, Linn., although it is altogether destitute of the elongation of the middle tail-feathers which occurs in that bird; the *crested Duck*; the *Gadwall*; the *Garganey*; the *Ruff*, and the *black-tailed Godwit*, in

* See Mr. Ogilby's remarks on the supposed antagonism of the thumb in the American Monkeys, present volume, p. 322.

their winter dress; the *Golden Oriole*; and other species: all of which were severally brought under the notice of the Meeting by Mr. Gould, at the request of the Chairman.

Mr. Gould subsequently exhibited specimens of various *Birds* which he had recently received from M. Temminck: including a new species of *Ptarmigan* from Siberia; and a *Trogon* from the Indian Islands, nearly allied in almost every particular to the *Trog. erythrocephala* of the Himalaya, but having the wing fully an inch shorter, with a tail bearing a relative proportion.

The Secretary announced the arrival in the Menagerie, since the last Meeting of the Society, of the four *Giraffes*, the capture of which was described by M. Thibaut in a letter read at the Meeting on February 9, 1836, and translated in the present volume, at p. 144.

He also directed the attention of the Members to a specimen of *Temminck's Horned Pheasant, Tragopon Temminckii*, Gray, which had recently been added to the Menagerie by the liberality of J. R. Reeves, Esq., of Canton: to a pair of the *Serin Finch, Fringilla Serinus*, Linn., brought from Italy for the Society, and presented to it by Mr. Willimott; and to a monstrous variety of the *Indian Tortoise, Testudo Indica*, Linn., which had also been lately added to the Menagerie, and which is remarkable for the great irregularity of the surface of its shell, each of the plates being raised into high conical eminences.

A paper was read by Mr. Martin "On the Osteology of the *Sea Otter, Enhydra marina*, Flem." It is founded on a perfect skeleton of the animal contained in the collection made by that energetic traveller the late David Douglas, and acquired, subsequently to his decease, by the Society. This skeleton was exhibited.

Mr. Martin refers in the first instance to the dentary characters of this remarkable animal, which were correctly described and figured by Home in the 'Philosophical Transactions' for 1796; and then adverts to some erroneous statements which have since been made respecting its molar teeth by various authors, including Cuvier, who appear to have possessed no opportunities of examining specimens. In the course of his communication he describes in detail the number and form of the teeth, which consist of six incisors in the upper jaw and of four in the lower, the outer one on each side in either series being larger than the others and assuming, in the upper jaw, somewhat of the form of the canines; of a strong canine on each side of the incisors in either jaw; and of four molars on either side in the upper, and five in the lower jaw, of which two in the upper and three in the lower are false and successively increase in size towards the true molars, the latter being large, broad teeth, with flattened crowns somewhat depressed in the middle: in the upper jaw the hindermost of the true molars is much larger than the other, while in the lower it is comparatively small.

The total length of the skeleton is 3 feet 2 inches; of which the skull measures 5 inches, and the tail, 10.

The general form of the skull nearly resembles that of the *Common Otter, Lutra vulgaris*, Storr; but it is proportionally broader, and is more convex on its lateral *parietes*, in this respect approaching to many of the *Seals*: the nasal bones form a broad plane, and do not gradually decline, like those of the *Common Otter*, towards the nasal opening; they are also shorter in proportion than in that species: the breadth of the nasal opening is greater than its depth, proportions which are reversed in the *Common Otter*: the post-orbital space is less contracted: on the base of the skull the space between the pterygoid processes is more considerable: and the whole contour of the *cranium* is not only broader but deeper also. The lower jaw maintains the same general tendency to greater compactness, and is stouter and shorter than in the *Common Otter*.

Detailed admeasurements are given by Mr. Martin of the skull of an individual more advanced in age than the one whose skeleton is preserved, and in which the entire length of the *cranium* is 5 inches; the greatest breadth, being across the occipital ridge behind the auditory *foramen*, nearly 4 inches, the breadth between the *zygomata* being the same; the depth from the point of union of the inter-parietal with the occipital ridge to the *foramen magnum*, $1\frac{6}{8}$; the distance from the *foramen magnum* to the bony palate, $2\frac{3}{8}$; and the length of the bony palate, $2\frac{1}{4}$.

The chest is rather wide in form, but much compressed; being 6 inches across at the sixth rib, while its greatest depth from the vertebral column to the *sternum* is $2\frac{1}{2}$ inches. The direction of the ribs is obliquely backwards, and they are rather slender: their number is thirteen, (not fourteen, as is stated by Home,) the last five being false and attached by very long cartilages to the cartilages of the true ribs.

The lumbar *vertebræ* are six in number.

The anterior extremities are short and small. The *scapula* is 3 inches in length and 2 in its greatest breadth: its spine is feeble and but slightly elevated. The *humerus* is 3 inches in length; and is stouter and less laterally compressed than that of a *common Otter* of the same longitudinal dimensions. The *ulna* and *radius* are stout, and are separated from each other by a greater interval than in the *common Otter*. The paws are remarkable for their diminutive size. In the *common Otter*, from the extremity of the *radius* to the nail of the last *phalanx* of the third finger the measurement is 3 inches; in the *Enhydra* it is $2\frac{1}{8}$.

The *pelvis* is long and narrow, measuring from the crest of the *ilium* to the *tuber ischii* 6 inches: in the *common Otter*, the measurement is but 4. The iliac bones are remarkably thick and solid, and turn out from the spinal column. The distance from the centre of the *acetabulum* to the crest of the *ilium* is 3 inches; the breadth of the *ilium* $1\frac{1}{4}$.

It is in the posterior limbs that the great power of the *Enhydra* appears to be developed. The *os femoris* is short but very thick, and its *trochanter* is bold and prominent: the *trochanter minor* is small. The head of the *femur* is globular, and is destitute of the *ligamentum teres*, as in the *Seals*: in the *Otter* this ligament exists as usual. The length of the thigh bone from the great *trochanter* to the condyles is $3\frac{1}{2}$ inches. Both the *tibia* and *fibula* are large and of great comparative length: in the *common Otter*, they do not exceed the *femur*; but here they exceed it by more than an inch, the measurement being $4\frac{1}{4}$ inches.

It is in the hind paws or paddles, Mr. Martin remarks, that the greatest difference exists between the *Otter* and the *Enhydra*. They are here admirably constructed as organs of aquatic progression. Their length from the *os calcis* to the last *phalanx* of the outer toe is $7\frac{1}{4}$ inches; and as the toes are long and connected by intervening webs they form broad efficient oars. The toes graduate regularly from the inner toe, which is the shortest, to the outer or fifth toe, which is the longest. The metatarsal bone of the inner toe measures $1\frac{1}{2}$ inch, the toe analogous to the thumb and composed of only two *phalanges* measures the same—the other toes have three phalanges as usual; the metatarsal bone of the fifth toe measures $2\frac{1}{2}$ inches; the toe itself 3 inches. The breadth of the foot, measured obliquely across from the end of the metatarsal bone of the first toe to that of the fifth is 2 inches.

The nails of the fore paws are small and sharp; those of the paddles are blunt, but curved.

The *os penis* is a stout bone $3\frac{3}{4}$ inches in length.

Mr. Martin concluded by remarking that as the hinder extremities are placed far backwards, and when stretched out in the act of swimming exceed the tail, this organ will appear placed between them, almost as much as it is in the *Seals*; between which animals and the *Otters* the *Enhydra* forms, in his estimation, a palpable link of union, approximating, in some portion of its osseous structure, even more to the former than to the latter.

Mr. Martin added that it was his intention, with the view of rendering his communication more complete, to review the osteology of the *Enhydra* in detailed comparison with that of the *common Otter* and of the *Seal*.

A drawing was exhibited of a *Saurian Reptile* of the family *Scincidæ* and of the genus *Tiliqua*, Gray, which forms part of the Museum of the Army Medical Department at Chatham, and which is regarded by Mr. Burton, Staff-Surgeon, in charge of the Museum, as hitherto undescribed.

It was accompanied by the subjoined character and description by Mr. Burton.

Tiliqua Fernandi. *Til. auribus profundis, latis, margine antico simplici; squamis dorsalibus valdè tri-carinatis: suprà pallidè brunnea strigis saturatioribus ornata, infrà albescens; lateribus brunneo variis alboque maculatis; gulâ brunneo lineatâ.*

Long. *corporis capitis*que 6 unc.; *capitis colli*que, $2\frac{1}{4}$; *caudæ*, ?
Hab. apud Fernando Po.

"There are eight rows of hexagonal imbricated scales on the back and tail, and two additional rows between the fore and hind legs; the lateral scales are irregular in form and size. Submental scales large, in three transverse rows; the first containing a single scale, the second a pair, the third a pair with an intermediate rudimentary one. Subcervical and ventral scales in eight rows; subcaudal in five rows, of which the middle row is the larger. There is a single row of anal scales, curved upwards. Scales of the upper surface of the body 3-keeled, of the lower smooth. A semicircular series of five plates over each orbit separated by a long narrow frontal: five occipital plates, the posterior ones largest: nasal, post-nasal, and labial plates varied in form and size.

"Head, back, tail and upper surface of the extremities reddish brown, a blackish line intersecting each row of scales; sides lighter, marked by a series of irregular blackish streaks; belly and under surface of tail a brownish white; throat alternated longitudinally with light and dark-brown lines; submental scales whitish, bordered with a broad dark-brown edge.

"A single row of blunt teeth on the margin of the jaws.

"Body of nearly uniform shape from the commissure of the lips to the tail."

June 28.—A note addressed to Colonel Sykes by Lieut. Henning, R.N., was read. It noticed the capture of an *Albatross* by a hook; and stated that the bird, while so attached, was fastened on by another of the same species, but whether with the intention of endeavouring to release it, or with the view of taking advantage of its helpless condition, the writer did not attempt to determine.

Some observations were read by Mr. Gray "On the genus *Moschus* of Linnæus, with descriptions of two new species."

The only character, Mr. Gray remarks, by which this genus, as established by Linnæus and others, differs from the genus *Cervus*, consists in the absence of horns; for the elongated canines are common to it and most of the Indian species of *Cervus*, especially the *Cerv. Muntjac*. The character of the fur, the degree of hairiness or nakedness of the *metatarsus*, and the presence or absence of the musk-bag in the male, offer, however, good characters for the subdivision of the group into three very distinct sections or subgenera.

The first of these divisions, for which Mr. Gray would retain the name of *Moschus*, comprehends only the *Thibet Musk*, *Moschus moschiferus*, Linn. In common with the *Deer* and *Antelopes* it has the hinder and outer side of the *metatarsus* covered with close erect hair; like many of the *Deer* also, its fur is quill-like and brittle; it has, moreover, a throat entirely clothed with hair; and the males are provided on the middle of the abdomen with a large pouch secreting musk. Its young, like those of most of the *Deer*, are spotted, while the adult animal is plain-coloured.

The division to which Mr. Gray in the year 1821, in a paper in the Medical Repository, gave the name of *Meminna*, also consists of but a single species, the *Moschus Meminna*, Linn. In this group the hinder edge of the *metatarsus* is covered with hair, but there is on its outer side, a little below the hock, a rather large smooth naked prominence, which is flesh-coloured during life; the fur is rather soft, spotted and varied with white, which becomes less conspicuous in the older specimens, but does not appear ever to be entirely lost; the throat is entirely covered with hair; and there is no musk-bag in either sex. The false hoofs are distinct, although denied to the animal both by Linnæus and Buffon.

The third and last subdivision is characterized by Mr. Gray, under the name of *Tragulus*, as having the hinder edge of the metatarsus nearly bald and slightly callous, a character which distinguishes them at once from all other *Ruminants*; the fur is soft, and adpressed like that of *Meminna*, but not spotted even when young; the throat is provided with a somewhat naked, concave, subglandular, callous disk, placed between the rami of the lower jaw, from which a band extends to the fore part of the chin; and they have no musk-bag. Like all the other species of the Linnean genus *Moschus*, they have false hoofs; and most of them have the edges of the lower jaw, three diverging bands on the chest, and the under surface of the body more or less purely white. The species of this division scarcely differ in colour in the various stages of their growth; the young fawn resembling the adult in every particular except in size.

In this division, the synonymy of which is extremely confused, Mr. Gray reckons four species, two of which he describes as new, arranging and characterizing them as follows:

Moschus Javanicus. *Mosch. ferrugineus nigro variegatus; collo saturatè brunneo griseo nebulato; menti margine, strigis pectoralibus tribus posticè latioribus, pectore, abdomine, femoribus internè, caudâque subtùs, albis; pedibus, capitis lateribus, prymnâque nitidè fulvis; occipite nigrescenti.* Long. *corp. capitis*que simul poll. 24; *metatarsi* $4\frac{1}{2}$ poll.

Moschus Javanicus, *Gmel., Syst. Nat.* 1. *p.* 174. *ex Pallasio. Raffles in Linn. Trans. xiii. p.* 261? *Benn., Zool. Gard., p.* 41.

Tragulus Javanicus, *Pall., Spic. Zool. xii. p.* 18. *in notâ.*

Moschus Indicus, *Gmel., Syst. Nat.* 1. *p.* 172.

Cervus Javanicus, *Osbeck, Iter, p.* 273.

Moschus Napu, *F. Cuv. Mamm. t.*

Chota Beta, *Rou de Ramon, Cab. Madr. t.* 9.

Hab. in Insulis Javâ et Sumatrâ.

This species, Mr. Gray states, is at once known by its larger size, pale colour, and the white of the entire under surface of the body, with the exception of the two longitudinal dusky stripes which separate the three white stripes of the chest from each other, and of a simple narrow pale band across the chest.

2. Moschus Kanchil. *Mosch. fulvus, nigrescenti variegatus; nuchâ strigâ latâ nigrâ longitudinali; gulâ, colli corporisque lateribus, pallidè flavescentibus, pilis nigro-apiculatis; antipedibus nitidè fulvis; menti marginibus, strigis tribus pectoralibus, pectore, abdomine, femoribus posticè, caudâque subtùs, albis; pectore abdomineque strigâ longitudinali, in illo saturatiore, in hoc pallidiore.* Long. *capitis corporis*que simul poll. 20; *metatarsi* $3\frac{1}{2}$ poll.

Moschus Kanchil, *Raffles in Linn. Trans. xiii. p.* 262.

Le Chevrotain adulte, *Buffon, Hist. Nat. tom. xii. p.* 344.

Le Chevrotain de Java, *Buffon, Hist. Nat. Suppl. tom. vi. p.* 219. *t.* 30.

Javan Musk, *Shaw, Zool. t.* 173, *ex tab. Buffon.*

Hab. in Javâ.

This species Mr. Gray states to be easily distinguishable from the former by its smaller size; darker colour; the strength and distinctness of its nuchal streak; the width of the band across its chest, which is besides continued backwards into a narrow streak; and the yellow band along the middle of the belly. These characters are common to two specimens of different ages in the collection of the British Museum. The lateral white streaks on the fore part of the chest are linear, the median one subtriangular, being narrow in front and widening backwards. The two dark streaks by which they are separated are linear, of the same colour with the sides of the neck, and do not unite together in front.

3. Moschus fulviventer. *Mosch. fulvus, nigrescenti variegatus; nuchâ strigâ longitudinali latâ nigrâ; gulâ, colli lateribus, antipedibusque rufescenti-fulvis; lateribus subtùsque flavescenti-fulvis; menti marginibus, strigis tribus pectoralibus, strigâ latâ utrinque in pectore abdomineque, femoribus internè anticèque, caudâque subtùs, albis.*

Le jeune Chevrotain, *Buffon, Hist. Nat. xii. p.* 342. *t.* 42, 43.

Hab. in Insulis Malaicis, et in Peninsulâ Indiæ Orientalis?

Very like the last, but differing from it in the under surface being pale fulvous with four white streaks, and in the lateral streaks on the chest being isolated anteriorly by means of a narrow transverse band which separates them from the white of the chin, while the median one is bounded in front by the union of the two dark streaks. There is also a small brown spot on each side of the chin just below the angle of the mouth, which is not found in the other species. The fawns only a few weeks old do not differ in colour from their parents. None of the three specimens in the collection of the British Museum have their habitats accurately marked. Two of them were from the collection of General Hardwicke, and the third was presented by Mr. Edward Burton of Chatham. Mr. Gray thinks it probable that this may be the animal indicated by Sir Stamford Raffles under the name of *Pelandoc.*

4. Moschus Stanleyanus. *Mosch. rufescenti-fulvus, pilis nigro-apiculatis, subtùs minùs nitidus; collo pectoreque nitidè fulvis; menti marginibus, strigis tribus pectoralibus, pectore, femoribus internè anticèque, caudâque subtùs, albis; syncipite, pedibusque a genubus inde saturatioribus; rhinario, strigâ utrinque oculos ambiente, auriculisque extùs et ad margines, nigris.*

Var. *menti marginibus minùs albis; strigis pectoralibus interruptis minùs conspicuis; gulâque paulò saturatiore.*

Hab.

This is immediately distinguishable from all the other species by the brightness of its colouring, and by the absence of the nuchal streak, and of the white on the under surface of the body. There are at present four living specimens in the magnificent collection of the Earl of Derby at Knowsley; and two others, consisting of a specimen of each of the varieties, in that of the Society, to which they were recently presented by Her Royal Highness the Princess Victoria. It is not known from what exact locality any of them were obtained.

Mr. Gray discusses the synonymy of the species above characterized as belonging to the subgenus *Tragulus*, especially with reference to the descriptions of Buffon, Pallas, Raffles, and M. Frederic Cuvier. From the imperfect manner in which they are described and figured, he is unable to identify with any of the foregoing species, or to separate from them as distinct, the *Pelandoc* figured in Marsden's Sumatra, or the *Pygmy Musk* of Sumatra figured in Mr. Griffith's edition of Cuvier's 'Animal Kingdom,' on which Fischer has established his *Moschus Griffithii*. The *Mosch. pygmæus* of Linnæus Mr. Gray states to belong to the genus *Antilope*; the hinder part of the tarsus being covered with hair, and the false hoofs very small and rudimentary, and entirely hidden under the hair of the feet; the *Mosch. Americanus* appears by its spotted livery to be the fawn of a species of *Deer*: and the *Mosch. delicatulus*, or *Leverian Musk* of Shaw, is also undoubtedly the fawn of a *Deer*. It is curious that Dr. Shaw quotes as a synonym of the last-named species the figure of Seba, on which alone the *Mosch. Americanus* is founded, while at the same time he enumerates the *Mosch. Americanus* as a distinct species.

Mr. Gray also made some observations "On the tufts of hair observable on the posterior legs of the animals of the genus *Cervus*, as a character of that group, and a means of subdividing it into natural sections." These tufts are found on the inside, or on the outside, or sometimes even on both sides, of the hinder legs of all the *Deer* which Mr. Gray has had an opportunity of examining, with the exception of the *Muntjac*, on which he has not been able to detect them either in the living state or in preserved skins. This circumstance may, however, have arisen from the fact of the living animal examined being confined in a cage; for he has uniformly found them much more conspicuous in animals which have a wide range than in such as are confined to small inclosures. Thus the various species of *Deer* in the magnificent parks of the Earl of Derby at Knowsley, in which the Ruminant animals are allowed an extensive range, and preserved in a state nearly approaching to wildness, exhibit the tufts in question in a much more ample state of development than such as are seen in menageries; and one of the *Axis Deer* at the Gardens of the Society, which has the run of a small paddock, displays them much more evidently than another specimen in the Gardens, which is confined to a stall. This difference of development, Mr. Gray suggests, may account for the little notice that has hitherto been taken of them by zoologists, who have only spoken of them incidentally, and with reference to one or two species of the group. They are found at all ages and in both sexes; and afford, therefore, a valuable adjunct in the determination of the species of the hornless females, as well as in distinguishing them from the females of the genus *Antilope*, in which no indication of them is to be observed; the tufts or *scopæ* that occur in some of the species of that genus being on the fore knees and evidently serving a very different purpose.

They were noticed in the *American Deer* by Buffon, who speaks of them as surrounding "*un lichen noirâtre long de neuf lignes, fort étroit, entouré par des poils blancs et longs, qui paroissoient former aussi une sorte de brosse*;" and according to M. F. Cuvier, who observed them in the *Wapiti*, they surround a narrow long horny substance, which is the appearance of the part in the dry state; but Col. Hamilton Smith, in his description of the same species, takes a different view of the structure with which they are connected, which he states to be "a gland imbedded in hair secreting an unctuous fluid." That the tufts really cover a glandular apparatus is rendered probable by the circumstance that in the living animal they generally assume a conical form as though imbued with some oily secretion; and the specimens preserved in spirit which Mr. Gray has examined, seem to justify this opinion; but he has had no opportunity, since his observations upon the subject were made, of confirming the fact by anatomical examination. They are generally of a paler colour than the rest of the hair upon the legs; and in some species, the *Cervus Virginianus* for instance, they are of a pure white which renders them very conspicuous.

To the existence of these tufts as a generic character common to all the *Deer*, Mr. Gray states that, among the species which he has had an opportunity of examining, he has met with only one exception, that of the *Muntjac* before mentioned; and he thinks that if this animal should prove to be really destitute of the appendages in question, it would afford an additional motive, combined with the permanence of its horns and some other characters, for excluding it from the genus *Cervus*. But these tufts have also another value, that of affording by the differences in their number and position three obvious sectional divisions, which have an evident advantage over those derived from the form of the horns and other characters of a sexual and temporary nature, in being permanent at all ages and common to both sexes. These sections Mr. Gray arranges as follows:

The first has a pencil of hairs seated on the outer side of the hinder part of the *metatarsus*, about one third of the distance from the *calcaneum* towards the hoofs. This section includes *Cerv. Elaphus, Canadensis, Axis, porcinus, Hippelaphus, Dama* and its varieties, and *niger*, as well as the *Stag* in the Museum of the Society, called the *greater Muntjac, Cerv. Tunjuc*, Vig. and Horsf., in the Catalogue for 1829, p. 17, No. 303, which Mr. Gray believes to be a species of the Rusan group of Col. H. Smith with deformed horns. In *Cerv. Canadensis*, and perhaps also in some other species, Mr. Gray states that there is a large pad of close erect hairs on the hinder edge of the *metatarsus*, commencing with this tuft.

In the second section there exist two tufts of hair, one seated on the outer side of the hinder part of the *metatarsus*, about two thirds of the distance from the *calcaneum* to the hoof; and the other on the inner side of the hock or heel. This structure occurs in the *Virginian Deer, Cerv. Virginianus*, and in its variety *Cerv. Mexicanus*, as well as in an allied species of which the female exists in the Society's Museum. The internal pencil is very distinct in the *Virginian Deer*; and the external is also very conspicuous in consequence of the whiteness of the hairs composing it. Lord Derby's gamekeeper, however, stated to Mr. Gray that there are two varieties of this species in Knowsley park, in one of which this tuft is much more conspicuous than in the other.

The third section comprehends those species which have a very distinct tuft on the inside of the hock, but none on the outer side of the *metatarsus*. Mr. Gray has observed this structure in two living specimens of a species from Demerara in the menagerie of Lord Derby, which agrees best with *Cerv. rufus*, Desm.; in another South American species, allied to the former but apparently different, which was presented to the Society in 1828 by Sir Philip Egerton, and is now in its Museum; and in a very young spotted *Fawn* (almost a fœtus) preserved in spirits in the collection of the British Museum. He suspects that the *Brockets* of South America may have the same character; and thinks he could observe the internal tufts on the specimen of the *Rein Deer* in the Society's Museum, but no trace of the external, the entire hinder edge of the *metatarsus* being covered with a uniform very thick coat of hair.

From an examination of the skin of the *Elk* in the British Museum, Mr. Gray is of opinion that it will probably enter into a fourth section; in as much as it appears to have very distinct tufts on the inner side of the hock, and others also on the outer side of the *metatarsus* about one third of its length from the heel, as in the first section; but of the existence of the latter tufts he is by no means certain, on account of the age and state of the specimen.

July 12.—Mr. Waterhouse, (Curator of the Society's Museum,) at the request of the Chairman, read a Paper, entitled "Description of a new genus of *Mammiferous Animals* from New Holland, which will probably be found to belong to the *Marsupial* type."

The skin on which this description was founded had been lent to Mr. Waterhouse, for the purpose of describing, by Lieut. Dale, of Liverpool, who procured it whilst on an exploring party in the interior of the Swan River Settlement, about 90 miles to the S.E. of the mouth of that river. Two specimens were seen; both of which took to hollow trees on being pursued, and one of them was unfortunately burned to death in the attempt to dislodge it from its retreat. The country abounded with decayed trees and ant-hills; and Mr. Waterhouse is of opinion, from this circumstance and from some peculiarities in the structure of the animal, that it lives chiefly, if not wholly, upon ants, for which reason he proposes for it the generic name of

MYRMECOBIUS.

Dentes incisores $\frac{8}{6}$, canini $\frac{0-0}{1-1}$, pseudo-molares $\frac{5-5}{4-4}$, molares $\frac{3-3}{4-4}=48$.

Pedes antici 5-dactyli, digitis tribus intermediis longioribus; postici 4-dactyli, digitis duobus intermediis internum superantibus; externo brevissimo; unguibus longis acutis subfalcularibus. Scelides antipedibus longiores. Caput elongatum; rhinario producto; auriculis mediocribus acutis. Corpus gracile. Cauda mediocris.

Mr. Waterhouse details at length the peculiarities of the dentition and other structural characters of the animal under consideration, and particularly notices the statement of Lieut. Dale that, when it was killed, the tongue was protruded from the mouth to the extent of two inches beyond the tip of the nose, its breadth being three sixteenths of an inch; which circumstance, combined with the dentition of the animal, confirms him in the belief that it feeds upon ants. With respect to its immediate affinities he confesses himself at a loss. In skinning the specimen, the part where the pouch would be placed in a marsupial animal, has been so mutilated as to render it difficult to determine whether or not it possessed one: it appears, however, to have been a female, and to have two *mammæ* and the remains of a pouch. Mr. Waterhouse is of opinion that it will prove to be allied to the genus *Phascogale*; and there are also, he states, points of resemblance between it and *Tupaia*, as well as with the ground Squirrels, the genus *Tamias* of modern authors.

The species Mr. Waterhouse proposes to name *Myrmecobius fasciatus*: he describes it as follows: "Length from the nose to the root of the tail (measuring along the curve of the back) ten inches; of the head, from the tip of the nose to the base of the ear, one inch and seven eighths; of the tail six inches and a quarter. The colour above is reddish ochre, interspersed with white hairs, the posterior half of the body being adorned with alternate black and white transverse fasciæ, disposed in a manner somewhat similar to those of *Thylacinus cynocephalus*. The under parts of the body are yellowish white; the anterior legs of the same colour on their inner sides, and of a pale buff colour externally; and the posterior legs of a pale buff colour, with the fore part of the tibiæ whitish, and the sole entirely bare. The hairs of the tail are mixed black, white and reddish ochre, each of these colours predominating in different parts. The reddish hue of the fore part of the body is gradually blended into the black, which is the prevailing colour of the posterior half, and which is adorned with nine white fasciæ; the first of these fasciæ (which is indistinct) commencing rather before the middle of the body, and being, in common with the second, interrupted on the back by the ground colour of the body; the third, fourth, and last extending uninterruptedly from side to side; and the fifth, sixth, seventh and eighth, extending over the back, passing without coming into contact, and thus as it were dovetailing, with those of the opposite side. The hair on the head is very short and of a brownish hue above, (being composed of a mixture of black and reddish-brown with a few white hairs); and whitish beneath. The nose and lips are blackish; and there are a few long black hairs springing from under the eyes and from the sides of the muzzle. The body is covered with hair of two kinds; the outer of which is moderately long, rather coarse, and compact on the back and fore parts of the body; but over the haunches, and on the under surface, where the pouch is situated in the *Marsupials*, the hair is long. The under fur is short, fine and rather scanty. The tail is furnished throughout with long hairs."

In illustration of his paper Mr. Waterhouse exhibited the skin, together with drawings of the animal, of its skull, and of its dentary characters.

Some notes of the dissection of a specimen of the *Chilian Bush Rat*, *Octodon Cumingii*, Benn., by Mr. Martin, were read, and are given in No. xliii. of the Society's "Proceedings."

July 26.—At the request of the Chairman, Mr. Gould exhibited specimens of two new species of *Birds* from the Friendly Islands and New Holland, of which he proposed to form a genus. He stated them to approximate, in his opinion, in nearly an equal degree to the genera *Lanius*, *Turdus*, and *Lamprotornis*; but believed that they might with propriety be arranged among the *Thrushes*. Their characters were given as follows:

APLONIS.

Rostrum capite paulò brevius, robustum, subcompressum; mandibulâ arcuatâ, ad apicem emarginatâ.

Nares basales, ovales, patulæ.

Alæ breves; remigibus 2do et 3tio longissimis, 1mo et 4to æqualibus.

Cauda brevis, lata, quadrata vel sub-bifurca.

Tarsi robusti; digitis magnis; unguibus magnis curvatis, hallucis præcipuè valido.

In both species the feathers of the head are lanceolate; and the general plumage above has a slight glossy hue, especially on the head and back of the neck. The species were characterized as APLONIS *marginata* and APLON. *fusca*.

ROYAL SOCIETY.

June 2, 1836.—A paper was read, entitled "Note relative to the supposed origin of the deficient rays in the Solar Spectrum; being an account of an experiment made at Edinburgh during the Annular Eclipse of May 15, 1836." By James D. Forbes, Esq., Professor of Natural Philosophy in the University of Edinburgh.

The observation that some of the rays of light, artificially produced, are absorbed by transmission through nitrous acid gas, had suggested to Sir David Brewster the idea that the dark spaces in the solar prismatic spectrum may, in like manner, be occasioned by the absorption of the deficient rays during their passage through the sun's atmosphere*. It occurred to the author that the annular eclipse of the sun of the present year would afford him an opportunity of ascertaining whether any difference in the appearance of the spectrum could be detected when the light came from different parts of the solar disc, and had

* See Lond. and Edinb. Phil. Mag., vol. viii. p. 392. The same explanation had been previously suggested by Sir John F. W. Herschel; see his Essay on Light, *Encyc. Metrop.*, Art. 505; also his Treatise on Astronomy, in the Cabinet Cyclopædia, p. 212, note; and L. and E. Phil. Mag., vol. iii. p. 406.—EDIT.

consequently traversed portions of the sun's atmosphere of very different thickness; and that accurate observations of this kind would put the hypothesis in question to a satisfactory test. The result of the experiment was that no such differences could be perceived; thus proving, as the author conceives, that the sun's atmosphere is in no way concerned with the production of the singular phænomenon of the existence of dark lines in the solar spectrum.

A paper was also read, entitled "On the connexion of the anterior columns of the Spinal Cord with the Cerebellum; illustrated by preparations of these parts in the Human subject, the Horse, and the Sheep." By Samuel Solly, Esq., Lecturer on Anatomy and Physiology at St. Thomas's Hospital, M.R.I., Fellow of the Royal Medical and Chirurgical Society, and Member of the Hunterian Society. Communicated by P. M. Roget, M.D., Sec. R.S.

The exact line of demarcation between the tracts of nervous matter, subservient to motion and to sensation, which compose the spinal cord, has not yet been clearly determined. The proofs which exist of a power residing in the cerebellum which regulates and controls the actions of muscles, would lead us to suppose that the fibres of the motor nerves are continuous with those of the cerebellum; but hitherto no observations have been made which prove the existence of this connexion; and it is the object of the author, in this paper, to establish, by a more careful examination of the anatomical structure of this part of the nervous system, such continuity of fibres between the anterior columns of the spinal cord and the cerebellum. The corpora pyramidalia have been hitherto considered as formed by the entire mass of the anterior, or motor columns of the spinal cord; but the author shows that not more than one half of the anterior columns enters into the composition of these bodies: and that another portion, which he terms the *antero-lateral* column, when traced on each side in its progress upwards, is found to cross the cord below the corpora olivaria, forming, after mutual decussation, the surface of the corpora restiformia; and ultimately being continuous with the cerebellum. These fibres are particularly distinct in the medulla oblongata of the sheep and of the horse. The author conceives that the office of the antero-lateral columns is to minister to the involuntary, as well as to the voluntary movements: that the facial nerve arises from both the voluntary and involuntary tracts; and that the pneumogastric nerve arises both from the involuntary and the sensory tracts.

June 9.—"Discussion of the Magnetical Observations made by Captain Back, R.N., during his late Arctic Expedition. By Samuel Hunter Christie, Esq., M.A., F.R.S.

The author, having been consulted by Captain Back, previous to the departure of the latter, in 1833, with the expedition for the relief of Captain Ross, respecting the nature of the magnetical observations which it might be desirable to make in the regions he was about to visit, and considering that, with a view to the attainment of the principal object of the expedition, the greatest economy of time in making these observations was of the first importance, limited his suggestions, in the first instance, to the methods proper so be employed for determining the direction and the dip of the needle, but more especially the latter. Captain Back, immediately on his return, placed all his magnetical observations at the disposal of Mr. Christie, who having since completed their reduction, gives, in the present paper, the results of his labours.

The first part of the paper relates to the observations of the Dip of the magnetic needle. With a view to economize as much as possible the time consumed in making each observation, the process of inverting the poles of the needle, which is usually resorted to in each instance, was here dispensed with. But in order that the dip may be determined independently of this operation, it is necessary not only that the position of the centre of gravity of the needle employed should be ascertained, but that it should be permanent. In giving an account of the observations made to verify this condition, the author commences with those at Fort Reliance, which was the first winter station of the expedition; and where the dip was determined by observations of the needle, both with direct and also with inverted poles. The author then enters upon an investigation of formulæ for the determination of the dip by means of a needle, in which the value of a certain angle, denoted by the symbol γ, determining the position of the centre of gravity, has been ascertained; and, conversely, for the determination of the value of the same angle, or, which is equivalent to it, the position of the centre of gravity of the needle, when the dip at the place of observation is given. He next inquires whether any tests can be applied to the observations under discussion, which may indicate the extent of the errors by which the results deduced from them may be affected; and he employs for this purpose the values of the terrestrial magnetic intensity furnished by certain equations obtained in the preceding investigation; making the proper allowances, first, for the needles used being ill adapted to this method of determining the relative intensities; secondly, for errors of observation in determining the times of vibration of the needle; and thirdly, for disturbing causes which might affect the observations. Considerable differences were found to exist in the results obtained by the two methods, at New York, Montreal, Fort Alexander, Montreal Island, and Fort Ogle; differences which can be accounted for only by errors in the assumed magnitude of the angle γ, and which, consequently, indicate the want of permanence in that angle. It was necessary, therefore, to inquire what changes in the angle γ will account for these discrepancies, and how far the value of the dip, thus obtained, may be affected by them. Formulæ are then deduced by which these changes may be determined.

From a comparison of the observed and computed values of the angles involved in these investigations, the author infers that the differences between those of one of these angles are, with a few exceptions, contained within the limits of the errors incident to dip observations: but with respect to the other angle, they in general exceed those limits. Upon the whole, he concludes that the discrepancies which appear between the values of the terrestrial intensity, as deduced from the times of vibration of the needle, and from the observed angles of inclination to the horizon, are principally attributable to a want of absolute permanence in its axis of motion. In the present case, the centre of gravity of the needle being nearly coincident with the axis, a very minute derangement in that axis would cause a considerable change in the value of the angle γ; so that the existence of differences in the values of this angle do not warrant the inference that the needle itself received any serious injury during the expedition; to which, indeed, from the care taken of it by Captain Back, it could not well have been liable.

The second part of the paper relates to the observations of the variation of the magnetic needle, which are already published in Capt. Back's narrative, and which are here introduced for the purpose of applying them, in conjunction with the observations of the dip, detailed in the preceding part, to a formula deduced from theory, with the view of ascertaining how far they may tend to support that theory.

The third section is devoted to the comparison of the observations of the dip and variation of the needle with theoretical results of a more general kind. The observations made by Captain Back are peculiarly adapted for verifying the hypotheses on which the theories of terrestrial magnetism rest, and that theory, in particular, which assumes the existence of two magnetic poles, symmetrically situated in a diameter of the earth, and near to its centre: for, on this hypothesis, the poles of verticity and of convergence will coincide; and the tangent of the dip will be equal to twice the tangent of the magnetic latitude. In no case has a progress towards the magnetic pole been made so directly, and to such an extent, as in the present expedition; whether that point be considered as the point of convergence of magnetic meridians, or that at which the direction of the force is vertical. It is deducible from the theory that the product of the tangent of the dip by the tangent of the polar distance is equal to two: and therefore, if the distance of the pole of convergence from two stations be determined by means of the observed variations at those stations, we may estimate, by the approximation of this product to the number two, in each case, the degree of coincidence which exists between theory and observation. A table is then given, exhibiting the several data on which this comparison is made, and the results deduced from them. From an inspection of the numbers in the column which indicate the deviations from theory it appears that there is not, in general, that accordance between the observations and the theory which might reasonably have been expected; and that although that theory may serve as a first approximation, yet it requires to be considerably modified to reconcile it with the observations. Hence the author arrives at the general conclusion that, unless considerable errors have crept into the observations of either the dip or the variation, the theoretical pole of verticity does not coincide with the pole of convergence, even when the positions of these points are deduced from observations made at very limited distances from those poles.

"On the Safety-valve of the right Ventricle of the Heart in Man; and on the gradations of the same apparatus in Mammalia and Birds." By J. W. King, Esq. Communicated by Thomas Bell, Esq., F.R.S.

In this paper additional evidence is given by the author in corroboration of the principles which he had announced in a former communication, which was read to the Royal Society in May 1835, on the influence of the tricuspid valve of the heart on the circulation of the blood*. His object is to demonstrate that the tricuspid valve in man occasionally serves the purpose of a safety-valve, being constructed so as to allow of the reflux of the blood from the ventricle into the auricle, during the varying states of distension to which the right cavities of the heart are at times subjected; that a similar function is maintained in the greater number of animals possessing a double circulation, and also that in the different orders of these animals the structure of this valve is expressly adapted to the production of an effect of this kind, in various degrees, corresponding with the respective characters and habits of each tribe. He is thus led to conclude that the function which the tricuspid valve exercises exhibits, in the extent of its development, a regular gradation, when followed throughout the different orders of Mammalia and Birds; and that it extends even to some Reptiles.

The force with which the circulating blood is impelled by the general venous trunks into the heart, and which is dependent on the action of the arterial system, and the degree of compression arising from muscular action, combined with the resistance of the valves of the veins, and which is also influenced by occasional accumulations of blood from rapid absorption, from impeded respiration, and from cold applied to the surface of the body, is shown to be subject to great and sudden variations. Any increase taking place in this force tends to produce distension of the right ventricle of the heart, followed by disturbance in the valvular action of the tricuspid membrane, owing to the displacement of its parts, which thus allows of a considerable reflux of blood into the auricle. Among the Mammalia, the lowest degree of this action, corresponding to that of a safety-valve, is found in the rodent, the marsupial, and the canine tribes. The next in degree is that which occurs in the order of Edentata and the feline genus. The Quadrumana occupy the next place in the scale of gradation. The human conformation exhibits this function in a very conspicuous manner, especially in the adult period; for at birth, when the right ventricle is unyielding, it scarcely exists; and in various states of disease the tricuspid valve acts with too much or with too little efficacy. The Pachydermata and Ruminantia come next in succession. The Seal exhibits this peculiarity in a still higher degree; but in no order of Mammalia does it exist to so great an extent as in the Cetacea, which appear, indeed, to possess a peculiar additional provision for effectually securing the permanent performance of this office, which the author compares to that of a safety-valve. A similar function, subject to similar gradations, is likewise traced in different orders of Birds. It is but slight in the Gallinaceæ; and rather greater in the predaceous tribes. In some of the Waders it exists to a considerable extent; but is greatest of all in the orders of Passerinæ and Scansores. Crocodiles

* An abstract of this paper will be found in Lond. and Edinb. Phil. Mag., vol. vii. p. 207.

and the Ornithorhynchus present some traces of this peculiar provision relatively to the circulation.

"Some Account of the appearances of the Solar Spots, as seen from Hereford, on the 15th and 16th of May, 1836, during and after the Solar Eclipse." By Henry Lawson, Esq., in a letter to Sir Henry Ellis, K.G.H., F.R.S., by whom it was communicated to the Society.

The spots on the sun's disc, at the period referred to, were very numerous; and one of great size, being many thousand miles in diameter, in particular attracted attention, from its penumbra presenting an appearance similar to a sky filled with small flocculent white clouds, perfectly distinct from one another; while on two sides were seen large masses of darker clouds, which seemed as if pouring their substance into the central chasm. The figure of the solar spots did not undergo any perceptible change of form during the progressive passage of the edge of the moon over them.

"On the Brain of the Negro, compared with that of the European and the Ourang-Outang." By Frederick Tiedemann, M.D., Professor of Anatomy and Physiology in the University of Heidelberg, and Foreign Member of the Royal Society.

It has long been the prevailing opinion among naturalists that the Negro race is inferior, both in organization and in intellectual powers, to the European; and that, in all the points of difference, it exhibits an approach to the Monkey tribes. The object of the present paper is to institute a rigid inquiry into the validity of this opinion. The author has, for this purpose, examined an immense number of brains of persons of different sexes, of various ages, and belonging to different varieties of the human race, both by ascertaining their exact weight, and also by accurate measurement of the capacity of the cavity of the cranium; and has arrived at the following conclusions. The weight of the brain of an adult male European varies from 3lbs. 3oz. to 4lbs. 11 oz. troy weight: that of the female weighs, on an average, from 4 to 8 oz. less than that of the male. The brain usually attains its full dimensions at the age of seven or eight; and decreases in size in old age. At the time of birth, the brain bears a larger proportion to the size of the body than at any subsequent period of life, being then as one sixth of the total weight; at two years of age it is one fourteenth; at three, one eighteenth; at fifteen, one twenty-fourth; and in the adult period, that is, from the age of twenty to that of seventy, it is generally within the limits of one thirty-fifth and one forty-fifth. In the case of adults, however, this proportion is much regulated by the condition of the body as to corpulence; being in thin persons from one twenty-second to one twenty-seventh, and in fat persons often only one fiftieth, or even one hundredth of the total weight of the body. The brain has been found to be particularly large in some individuals possessed of extraordinary mental capacity. No perceptible difference exists either in the average weight or the average size of the brain of the Negro and of the European: and the nerves are not larger, relatively to the size of the brain, in the former than in the latter. In the external form of the brain of the Negro a very slight difference only can be traced from that of the European; but there is absolutely no difference whatsoever in its internal structure, nor does the Negro brain exhibit any greater resemblance to that of the ourang-outang than the brain of the European, excepting, perhaps, in the more symmetrical disposition of its convolutions.

Many of the results which the author has thus deduced from his researches are at variance with the received opinions relative to the presumed inferiority of the Negro structure, both in the conformation and in the relative dimensions of the brain; and he ascribes the erroneous notions which have been hitherto entertained on these subjects chiefly to prejudice created by the circumstance that the facial angle in the negro is smaller than in the European, and consequently makes, in this respect, an approach to that of the ape, in which it is still farther diminished. The author denies that there is any innate difference in the intellectual faculties of these two varieties of the human race; and maintains that the apparent inferiority of the Negro is altogether the result of the demoralizing influence of slavery, and of the long-continued oppression and cruelty which have been exercised towards this unhappy portion of mankind by their more early civilized, and consequently more successful competitors for the dominion of the world.

June 16.—The following papers were read, viz.

1. "Researches on the Tides; Sixth Series. On the Results of an extensive system of Tide Observations, made on the Coasts of Europe and America, in June 1835." By the Rev. William Whewell, F.R.S., Fellow of Trinity College, Cambridge.

The author having, in several previous communications to the Royal Society, urged the importance of simultaneous tide observations made at distant places, here gives an account of the steps taken to carry this plan into effect, in consequence of his representations, both by the Government in England, and by the other maritime powers of Europe. He explains, in the present paper, the general character of the observations thus obtained, the mode employed in reducing them, and enters at considerable length into a discussion of the immense mass of information which they supply with respect to the phænomena of the tides. One of his principal objects was to fix with precision the form of the *Cotidal lines* by which the motion of the tide wave is exhibited. He devotes one section of the paper to an investigation of the general form of these lines; and another to a nearer approximation to an accurate map of these lines, more especially as they exist in the German Ocean. The 4th section treats of the height of the tide in its total range from high to low water; the 5th relates to the diurnal inequality; the 6th to the semimenstrual inequality; and the 7th and last comprises general remarks on the tables which accompany the paper.

2. "On the Tides at the Port of London." By J. W. Lubbock, Esq., F.R.S.

The discussions of tide observations which the author has hitherto at various times laid before the Society, were instituted with reference to the transit of the Moon immediately preceding the time of high-water; from which the laws of the variation in the interval between the moon's transit and the time of high-water have been deduced' But the discussion of nineteen years' observations of the tides at the London Docks, which is given in the present paper, has been made with reference to the moon's transit two days previously, and proves very satisfactorily that the laws to which the phænomena are subject accord generally with the views propounded long since by Bernouili. The relations which the author points out between the height of high-water and the atmospheric pressure as indicated by the barometer are particularly interesting and important. The influence of the wind is also considered; and such corrections indicated as are requisite in consequence of the employment by several observers of solar instead of mean time.

3. "Discussion of the Magnetical Observations made by Captain Back, R.N., during his late Arctic Expedition." By Samuel Hunter Christie, Esq., M.A., F.R.S. Part II.

The author proceeds, in this paper, which is a sequel to his former communication (p. 523.), to discuss the observations made by Captain Back relating to the magnetic intensity, and which were of two kinds; the first, obtained by noting the times of vibration of a needle in the plane of the magnetic meridian; the second, by noting the times of vibration of three needles suspended horizontally according to the method of Hansteen. The results are given in the form of tables.

Before deducing results from these observations, the author describes a series of experiments instituted with each needle, for the purpose of determining the corrections necessary to be applied in order to reduce the intensities which would result from observations made at different temperatures, to intensities at a standard temperature; and he gives formulæ for these corrections. He then determines the relative terrestrial magnetic intensities, at the several stations where observations were made, from the times of vibration of the dipping needle in the plane of the meridian, applying the corrections which he had obtained for difference of temperature; and gives the results in tables. A comparison is instituted between these results and a formula derived from the hypothesis of two magnetic poles not far removed from the centre of the earth. The author considers that this comparison is quite conclusive against the correctness of the formulæ, and consequently of the hypothesis itself, if applied to the results deduced from the observations in London, in conjunction with those in America; but that, in the tract of country comprised by Capt. Back's observations from New York to the Arctic Sea, the phænomena of terrestrial magnetic intensity are very correctly represented by the formula in question.

The author then proceeds to determine the intensity from the observations with horizontal needles, applying here, likewise, to the results, corrections for the difference in the temperatures at which the observations were made. In these results there are great discrepancies, which the author attributes to the inapplicability of Hansteen's method of determining the intensity by the times of vibration of horizontal needles to cases where the dip of the needle is very great, rather than to errors in the observations themselves, or to a variation in the magnetism of the needles employed. He concludes by a just tribute to the zeal which Captain Back has manifested in the cause of science, by availing himself of every opportunity of making these tedious observations, during an unknown and perilous navigation.

4. "On the Powers on which the Functions of Life depend in the more perfect Animals, and on the Manner in which these Powers are associated in their more complicated results." By A. P. W. Philip, M.D., F.R.S.

This paper is divisible into three portions. In the first, the author considers the functions and seat of each of the powers of the living animal; in the second, the nature of each power; and in the third, the manner in which they are associated in the more complicated results which constitute life.

Of these powers the simplest is the muscular, which consists merely in a contractile power residing in the muscular fibre itself: and various experiments are referred to in proof that it depends exclusively on the state of this fibre, and in no degree on that of the nervous system, which some physiologists have regarded as the real seat of this power: for, instead of being recruited, it is exhausted by the action of the nervous system upon it, as it is by other stimulants.

The next power considered is that of the nervous system, properly so called, in contradistinction to the sensorial system. The result of an extensive series of experiments made with a view to establish the exact line of distinction between these two systems, is that the functions of the nervous power are as remarkable for their complexity as that of the muscular power is for its simplicity. With regard to the nervous power it is shown that its functions (all of which are capable of existing after the sensorial power is withdrawn, and all of which fail when the nervous power is withdrawn,) are the following: 1. The excitement of the muscles of voluntary motion in all their actions; 2. The occasional excitement of the muscles of involuntary motion; 3. The maintenance of the process by which animal temperature is maintained; 4. The maintenance of the various processes of secretion; 5. The maintenance of the processes of assimilation. It further appears, from several experiments, that the seat of the nervous power is exclusively in the brain and spinal cord; not, however, in any particular part, but in the whole extent of these organs, from the uppermost surface of the former to the lowest portion of the latter; with the exception only that the lower portions of the spinal cord partake less of this power than the rest. It appears also that the nerves are only the medium of conveying the influence of the above-mentioned organs; and their ganglions and plexuses are only the means of combining the power of all the parts of these organs; such combination being shown to be necessary to the due excitement of the muscles of involuntary motion, and for the maintenance of the functions of secretion and assimilation.

The remaining powers of the living animal are the sensorial powers, and the powers of the living blood. The first of these classes of powers has its seat, not in the whole brain and spinal cord, as is the case with the nervous power, properly so called, but in certain parts of them;

these parts being, in man, almost wholly confined to the brain; while in some animals they extend also to a considerable portion of the spinal cord. The functions of the sensorial powers are those strictly termed mental, of which sensation and volition are the simplest, and the only powers of this class which are concerned in the maintenance of life.

The functions of the living blood are evidently those of supplying the proper materials, in their requisite condition, (to the preservation of which the vital powers are essential,) for the action of the nervous power, properly so called, in the processes of secretion and assimilation. The seat of the powers of the blood is in itself; as appears from its retaining them for a short time after it is separated from the body.

These four vital powers, viz. the muscular, the nervous, the sensorial, and that of living blood, have no direct dependence on one another; for each can, for however short a time, exist independently of the others: but each has an indirect dependence, more or less remote, on all the other three for the maintenance of their organs.

The author then proceeds to inquire into the nature of these several powers. The sensorial and muscular powers, and the powers of the living blood, are manifestly peculiar to the living animal, no analogous powers being perceptible in inanimate nature. But this exclusiveness does not belong to the nervous power, for experiment shows us that when the oxygen and carbon of the blood are combined by its influence, a substance results which is identical with that produced in the laboratory of the chemist. An analogy, too strong to be wholly disregarded, exists therefore between its effects and those of the powers which operate in inorganic nature. This consideration, as well as others stated by the author, induced him to make many experiments to determine how far the other functions of the nervous influence bear a similar analogy to the operations of inanimate nature; and, in particular, to inquire whether voltaic electricity, applied under the same circumstances as those under which the nervous influence operates, and applied after the removal of that influence, and the consequent cessation of its functions, would produce the same effects. His endeavours were crowned with complete success; all the functions of the nervous power being capable, as far as he and others could judge, of being perfectly performed by voltaic electricity. He states that the results of his experiments on this subject were confirmed by a public repetition of them both in London and in Paris; as were likewise those of another set of experiments suggested by the following reasoning. If the nervous influence could be made to pass through any other conductor than the nervous textures to which it belongs in the living animal, we should have a proof, independent of all other evidence, that this influence is not a vital power, properly so called; because it must be universally admitted that such a power can exist only in the texture to which it belongs. In this attempt he was for some time baffled; but at length, overcoming the obstacles which had impeded his efforts, he succeeded: and, having undergone the same public ordeal as the former, the results are no longer questioned.

From the whole of these experiments the author thinks himself warranted in concluding that the nervous influence is not a vital power, properly so called; and that when it is admitted that voltaic electricity is capable of performing all its functions, the proposition that they are powers of a different nature would be a contradiction in terms, for it is only by its properties that any principle of action can be distinguished.

He refers, in confirmation of these inferences, to the recent investigations of Mr. Faraday, from which it appears that electricity is the agent in all chemical processes; to the facts which prove that all the functions of the nervous influence, properly so called, are of a chemical nature; and also to the late experiments of Dr. Davy on the Torpedo, tending to show that the electric power, peculiar to electric animals, is a function of the brain, and thus affording direct proof that the brain has the power of collecting and applying, even according to the dictates of the will, the electric power.

It farther appears, from the facts referred to in this paper, that, whenever we can trace any analogy between the functions of the living animal and the operations of inanimate nature, an agent belonging to the external world is employed; that these functions are the results either of such agents acting on vital parts, or of vital parts acting on them; and that the sensorial functions, on the other hand, in which no such analogy can be traced, are the effects of vital parts acting on each other, and influencing each other by their vital properties alone.

In the concluding part of the paper the author considers the various functions of the living animal as forming two systems, in a great measure distinct from one another, in each of which all its powers are employed, but in very different ways: the object of the one of these systems being the maintenance of the body itself; of the other, the maintenance of its intercourse with the external world. The manner in which the different powers of the living animal are employed in the construction of each of these systems is pointed out; and the bonds of union which exist between them, and thus form the living body into a whole, no part of which can be affected without tending more or less to affect every other, are considered. These bonds of union consist chiefly in the employment of the same powers in the construction of both systems, and in the function of respiration, which so extensively influences all other functions both in health and disease, as pointed out by the author in his papers on the nature of sleep and death, and which differs from all the other vital functions in partaking of the sensorial as well as of all the other powers of the living animal.

5. "On the Respiration of Insects." By George Newport, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

Although a multitude of facts has been collected relating to the physiology of respiration in insects, attention has seldom been directed to the variations exhibited in this function in the different periods of the r existence. The author gives an account, in this paper, of the anatomical and physiological peculiarities which he has noticed in various insects, in their three states of larva, pupa, and imago. He traces all the several changes which the tracheæ and spiracles undergo during their transformations; describing particularly the successive development of the air vesicles in connexion with the power of flight. The system of muscles, both of inspiration and of expiration, is minutely detailed, and their various modes of action examined. He next investigates the series of nerves appropriated to the exercise of the respiratory function, and establishes a distinction in the offices of these nerves, corresponding to the sources from which they derive their origin, and presenting remarkable analogies with similar distinctions in the nerves of vertebrate animals. The manner in which respiration is performed, and the phænomena presented with regard to this function under various circumstances, such as submersion, and confinement in unrespirable or deleterious gases, are next considered. An account is then given of a series of experiments made with a view to determine the quantity of oxygen consumed, and of carbonic acid produced, by the respiration of various kinds of insects in different states, from which the conclusion is drawn that the quantity of air deteriorated is governed by several circumstances not necessarily connected with the natural habits of the species. When the insect is in its pupa state, and in complete hybernation, its respiration is at its minimum of energy: and, on the contrary, it is at its maximum when the insect is in the imago state, and in the condition of greatest activity.

In the concluding section of the paper the author institutes an inquiry into the capabilities which insects possess of supporting life, during longer or shorter periods, when immersed in different media; and gives a tabular view of the results of numerous experiments which he made on this subject. It appears from these observations that the order in which these media possess the power of extinguishing vitality is the following: viz. hydrogen, water, carbonic acid, nitrous acid gas, chlorine, and cyanogen. Some of these agents, however, affect respiration much more rapidly than others, which, though their action is slower, are eventually more fatal to the insect.

6. "Démonstration de l'égalité à deux droits de la somme des angles d'un triangle quelconque, indépendamment de la théorie des parallèles, et de la considération de l'infini." Par M. Paulet, de Genève. Communicated by P. M. Roget, M.D., Sec. R.S.

The author demonstrates the equality of the sum of the angles of a triangle to two right angles, by the aid of a preliminary theorem, of which the following is the enunciation. A straight line forming an acute angle with another straight line, will, when sufficiently produced, meet any line, perpendicular to the latter, and situated on the side of the acute angle.

7. "Experimental Researches into the Physiology of the Human Voice." By John Bishop, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.*

8. "Du Son et de l'Electricité." Anonymous, with the signature of *Hermes*. Being a Prize Essay for the Royal Medal.

* Mr. Bishop's paper will be found in the present volume, at p. 201 *et seq.*

This paper contains the account of a great number of facts and observations, collected from various sources, on the subject of the relations subsisting between electricity, the production of sound, the crystallization of bodies, the transmission of heat, the emission of light, and various atmospheric changes; from the consideration of which the general conclusion is drawn that all these phænomena are perhaps the results of the undulations of some ponderable material.

9. "Physiological Remarks on several Muscles of the Upper Extremity." By F. O. Ward, Esq., Medical Student at King's College, London. Communicated by P. M. Roget, M.D., Sec. R.S.*

There is a remarkable fold in the tendon of the pectoralis major muscle, described by all anatomists, but the purpose of which has never yet, as the author believes, been explained. The muscle itself consists of two portions, one smaller and upper, arising from the clavicle, and passing downwards and outwards to an insertion in the humerus at a greater distance from the shoulder-joint than the place where the tendon of the larger and lower portion of the muscle, which arises from the sternum and ribs, and has a general direction upwards and outwards, terminates. Thus the respective portions of tendon belonging to the two divisions of the muscle are found to cross each other; the margin of that proceeding from the lower division passing behind, and appearing above that which proceeds from the upper fibres of the muscle. The forces exerted by each portion of the muscle being thus applied to parts of the bone at different distances from the fulcrum, act with different mechanical powers; which the author finds in every case to correspond exactly with the variations in the effects required to be produced, under different circumstances, by these muscular actions. Those muscular fibres, the tendon of which is inserted nearest to the centre of motion, and which consequently act by a shorter lever, are adapted to motions requiring a less force, but a greater velocity: and such is precisely the mechanical condition of the lower portion of the pectoralis major, which is employed more especially in bringing down the arm, when previously raised, as in striking with the hammer, pickaxe, &c., where velocity is chiefly required, the weight of the instrument held in the hand sufficiently supplying the diminution of force. On the contrary, the lever by which the upper portion of the same muscle is enabled to act being, from the more distant insertion of its tendon, of greater length, is calculated to procure force at the expense of velocity, and is therefore peculiarly fitted for the performance of those actions by which the arm is elevated and weights raised; these being precisely the actions in which such muscles are employed. Adverting, also, to the respective obliquities in the direction of their action, the author traces the same express correspondence between the mechanism employed and the purpose contemplated. He pursues the same line of argument and obtains the same results in extending the inquiry to the structure and uses of those muscles, such as the coraco-brachialis, and the anterior fibres of the deltoid, which cooperate with the upper division of the pectoralis major; and the teres major and latissimus dorsi, which

* This paper appears at length in our present number, p. 411.

combine their actions with that of the lower division of the pectoral muscle.

This diversified adaptation of parts, he observes, forms the chief characteristic of the mechanism of Nature. Operating with unlimited means, she yet works with scrupulous economy; in all her structures no power is redundant, nor a single advantage lost: so that, however completely an arrangement may be subservient to one primary purpose, we find, on renewed examination, an equally accurate adjustment to various secondary and no less important ends.

The author then proceeds to inquire into the methods employed for determining the absolute and relative strength of muscles; and proposes, for that purpose, the application of the constant and equable stream of galvanism afforded by the new battery invented by Mr. Daniell.

10. "An Experimental Inquiry into what takes place during the Vinous, the Acetous, and different Putrefactive Fermentations of dissolved Vegetable Matter; and an Examination of some of the Products." By Robert Rigg, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author describes with great minuteness a long train of experiments on the subjects announced in the title of the paper. His first object of inquiry is into the nature of the changes which take place during the vinous fermentation; and the conclusion to which he arrives is, that in the formation of the products resulting from this process sugar is not the only vegetable principle which is decomposed, but that the changes consist in the combination of two equivalents of carbon, derived from the sugar of the malt, or other vegetable matter, (= 12·24) with two equivalents of hydrogen from water (= 2·) forming 14·24 parts of olefiant gas; and in the combination of one equivalent of the carbon from the sugar, &c. (= 6·12) with two equivalents of oxygen from water, (= 16·) forming 22·12 parts of carbonic acid. He thinks that, on this change taking place, the olefiant gas is held in solution by the water by an affinity which can be overcome, and that the foreign matter which, with the carbon, formed the sugar, or other vegetable substance, is then at liberty to form new combinations. He finds that the products resulting from the decomposition exceed the weight of the sugar, or other vegetable matter, by about 10 per cent. of the former, and from 11 to 12 per cent. of the latter, as calculated according to the prevailing theory that sugar, or vegetable matter, is the only substance decomposed during the process of vinous fermentation.

From his analysis of sugar he obtains certain proportions of water and of carbonic acid which are different from those given by preceding chemists, the carbonic acid being 45 to 45·5 per cent. His analysis of alcohol gives him 59·7 to 60 per cent. of olefiant gas, the remainder being water.

His experiments on the acetous and putrefactive fermentations are numerous and elaborate, and the results, which are nearly the same as those of former analyses, are given in a tabular form. He finds that in the acetous fermentation 57 parts by weight of olefiant gas, 5 of sugar, or other vegetable matter, and 64 of oxygen from the atmosphere, combine to form 100 parts of acetic acid, and about 24 of water; leaving an insoluble substance at liberty to form other combinations: and thus includes in his account of this process the decomposition of vegetable matter, which is overlooked in the generally received theory.

During the putrefactive fermentation of vinous fermented liquors, when exposed to the atmosphere, the author considers that one equivalent of carbon from the olefiant gas (= 6·12) unites with two of oxygen from the atmosphere (= 16·) to form 22·12 parts of carbonic acid: while one equivalent of hydrogen from the olefiant gas (= 1·) combines with one of atmospheric oxygen (= 8·) to form 9 parts of water; a portion of sugar, or other vegetable matter, being also decomposed; and an insoluble substance remaining, which, on exposure to the air, undergoes further decomposition, and forms products highly deleterious. The author is not aware that this latter decomposition has been hitherto noticed.

During the putrefactive fermentation of acetic acid exposed to the atmosphere, he regards one equivalent of carbon from acetic acid (= 6·12) as combining with two of atmospheric oxygen (= 16·) to form 22·12 parts of carbonic acid: the oxygen and hydrogen, with which the carbon had formed the acetic acid, remain in the state of water, as they are found by analysis in this substance: a portion of vegetable matter is also decomposed; and an insoluble substance left behind. Other substances are also formed during some of the changes resulting from exposure to the air.

During the direct putrefactive fermentation of solutions of sugar, or other vegetable matters, he finds, that one equivalent of its carbon (= 6·12) unites with two of atmospheric oxygen (= 16·) to form 22·12 parts of carbonic acid; leaving the water and an insoluble substance to undergo changes as before mentioned. The olefiant gas, formed during the vinous fermentation, whether the liquor be in the state of vinous fluid, weak spirit, strong spirit, or even of alcohol, or ether, is subject to precisely the same decomposition, under favourable circumstances for such changes, without any action upon, or relation to the water which may happen to be combined with it in each kind of liquor. This olefiant gas cannot, either by distillation or other means, be separated along with any of the water with which it is at first combined, and again united with the same materials, without forming a compound different from the original one: and in proportion as water is, by any means, removed, we obtain it in a somewhat different state; and this happens without reference to a separate and distinct substance which we may call alcohol, or ether. Thus neither of these two ill-defined substances ought to be regarded as a separate and distinct principle; but the whole series of bodies, from the weakest fermented liquor, separated from its vegetable matter, to the most highly rectified ether, consist only of different combinations of olefiant gas, the first product of vinous fermentation, and water.

11. "On the Chemical Changes occurring in Seeds during Germination." By the same.

The author infers, from his researches on the subject of his second paper, that during the process of germination there is a production of alcohol, and that oxygen unites with olefiant gas, under the influence of the radicle and plumula. He accounts for the increase of temperature during germination by an alleged difference in the specific heats of the principles before and after that process has commenced; but the methods he employed for establishing the reality of this difference are not detailed.

The following are the principal conclusions to which the author arrives:

1. Seeds may, by careful desiccation, be deprived of much water without injuring their vegetating organs.
2. Their capacity for absorbing water varies with the temperature at which they are kept.
3. The increase taking place in their volume by the absorption of water is influenced by temperature.
4. On steeping seeds in water at one temperature the vinous fermentation takes place, but at another this process does not occur.
5. A decomposition takes place in seeds previously to their germination, and the products are carbonic acid and olefiant gas.
6. The abstraction of carbon from seeds by the oxygen of the atmosphere is not, as is generally supposed, the specific action which gives rise to germination; but it rather conduces to putrefaction.
7. The germination of seeds appears to be an action taking place between the olefiant gas, which has been previously formed by a vinous fermentation, and the oxygen of the atmosphere; and is effected by the peculiar operation of the plumula and the rootlets.
8. This decomposition and combination of the different elements go on, in well-regulated processes, as long as there is any farinaceous matter to be decomposed: the food of the plant being at this time always the oxygen of the atmosphere and the newly-formed olefiant gas, differing in equivalent combinations, according to the peculiar constitution of the plant; and thus the foundation is laid for all that prodigious diversity which characterizes the numberless species of the vegetable creation.

Intelligence and Miscellaneous Articles.

ARTIFICIAL PRODUCTION OF CRYSTALLIZED MINERALS.

MUCH interest and inquiry have been excited by the experiments of Mr. Crosse on the artificial production of crystallized minerals in the humid way, by the agency of electricity, as announced by that gentleman at the late meeting of the British Association, and noticed in our number for September, at p. 229 of the present volume. We think it may be useful to recall the attention of our readers to the results which had previously been obtained by others, both in the humid and the dry way, in the same line of research, by transferring to our pages the following extract from the Rev. Mr. Whewell's "*Report on the Recent Progress and Present State of Minera-*

logy," read to the meeting of the British Association at Oxford in 1832, and published in the first volume of the Reports of the Association.

"The discovery of artificial crystals in the slags of furnaces was not unimportant to the chemistry of mineralogy. One of the first and most extraordinary instances was the detection of perfect crystals of titanium in the Welsh iron slag, by Dr. Wollaston and Professor Buckland*. It has appeared by examination that these accidental products are more free from any admixture of iron than it is easy to obtain titanium by the ordinary chemical processes. In 1825 Mitscherlich found in the Swedish furnaces bisilicate of iron (pyroxene,) mica, and other mineral species. About the same time, Berthier in France obtained in the furnace, by direct synthesis, regulated by the atomic theory, crystals similar to those found in nature. Professor Miller of Cambridge has examined several slags from the furnaces in Wales, and it appears that the crystals in those assume the form of olivine. It is satisfactory thus to find that the same substances affect the same crystalline form in our furnaces and laboratories, and in the great laboratory of nature. Indeed nothing can be more likely to help us in obtaining a knowledge of the chemical laws of crystalline forms, than to have the power of verifying our conclusions synthetically by forming crystals, as well as analytically by destroying them."

"In the same point of view, the examination of crystals formed from solutions is of great value to mineralogy; as, for instance, the many excellent measures of artificial salts by Mr. Brooke, Mr. Haidinger, and others. Such crystals may often be obtained in much greater abundance and perfection than natural crystals, and especially than natural crystals of similar chemical composition; and thus they widen very much the field of facts to which our inquiries lead. In former times the mineralogist was professedly restricted to substances which occur in nature; but we may venture to say that a line so arbitrary and accidental cannot be the true boundary of the science. Wherever crystalline forces act, the crystallographer is called upon to pursue his speculations; these speculations whether we call them mineralogical or not, are such as give interest and promise to our study. In this point of view mineralogy possesses not only the importance which belongs to its ancient subjects, but also an importance of another kind, which belongs to it as a necessary supplement to chemistry; for it takes into consideration those physical characters of chemical compounds (crystallization, specific gravity, hardness, fracture, lustre), which belong to them as solid bodies, and which indicate the law and intensity of the corpuscular forces by which each combination is bound together. The study of artificial crystals, therefore, whether obtained in the wet or in the dry way, may be recommended as very useful to the mineralogist."

"Haldat (*Ann. de Chim.* Jan. 1831,) has shown a mode of obtaining artificial crystals of iron oxide by the decomposition of

[* Dr. Wollaston's papers on this subject were reprinted from the Phil. Trans. in Phil. Mag., First Series, vol. lxii. p. 18, vol. lxiii. p. 15.]

water; and these resemble the natural crystals of "fer oligiste" from Elba. So Becquerel has obtained the oxides of copper, lead, zinc. But by far the most valuable and important of such experiments appear to be those of M. Becquerel on the sulphurets, iodurets, and bromurets of metals, which he has obtained by artificial chemical action in a perfectly crystalline form. The agency which he employs is very weak galvanic tension; and he has succeeded thus in producing sulphuret of silver in small octohedral crystals resembling the native mineral, and sulphuret of copper, also closely resembling the native sulphuret. The sulphurets of zinc and iron require additional precautions, but are also obtained like to the native species; and iodurets, bromurets, and seleniurets of various metals are procured as crystals by similar processes. (*Ann. de Chim.* Oct. 1829.) These important steps in synthesis will probably throw a new light upon known analytical results."

M. Beudant has made a number of interesting experiments on the subject of another class of causes which modify the forms of crystals, and of which the general laws are, if possible, more unknown and obscure than those which determine different fundamental forms to different compounds. He has examined the circumstances which determine the various modifications which a given fundamental form undergoes; and has proceeded so far as to be able to produce at will one or other of certain possible modifications. Thus (*Minéralogie*, i. 190.) common salt crystallizing in pure water affected almost always the cubical form; if it crystallized in a solution of boracic acid, it assumed the form of the cube with truncated angles. Alum in nitric acid had the same form; in muriatic acid it was a figure of twenty sides, the octohedron and dodecahedron combined; the faces of the former being much the larger. An addition of alumine to the liquor, produced, in addition to the former faces, those of the cube; in pure water this salt is the simple octohedron. Sulphate of iron has commonly a simple form; by adding a few drops of sulphuric acid, more complex forms are obtained; and this rule respecting the effect of the addition of acid appears to be extensively true. The sulphate of iron mixed with sulphate of copper has its simple form, an oblique rhombic prism; the mixture of sulphate of nickel produced the same effect, but that of zinc an opposite one, the crystals becoming less simple. It has long been known that common salt mixed with urea, affects the octohedron instead of its usual form, the cube; and that in similar circumstances, sal ammoniac becomes the cube instead of the octohedron. Alum in a concentrated solution of alumine assumes the cubical form; an octohedral crystal of alum placed in such a solution soon assumes a cubical form; by being placed again in a solution adapted to give octohedral crystals it may be made to assume the octohedron. It is impossible not to be tempted to refer phænomena similar to these, occurring, as they so often do, in natural crystals, to similar circumstances which have prevailed when the crystals have been forming."

"Several statements of a curious kind have been made concerning the recent crystallization of substances which we cannot cause to crystallize in our laboratories. Thus (Brewster's Journal, vol. x.) Repetti observed quartz in a pasty state and in the act of crystalling. The same kind of occurrence is said to have been observed of various other substances, as beryl, opal, heavy spar." *Report of the First and Second Meetings of the British Association*, p. 374–379.

DIRECT DEMONSTRATION OF THE RULE FOR THE MULTIPLICATION OF NEGATIVE SIGNS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

No direct proof, that I am aware of, has ever been given of the rule for the multiplication of negative signs, although proofs by Euler and a hundred authors have been given by *reductio ad absurdum*. You will oblige me by an insertion of the following *direct* proof, especially as I have heard it disputed whether a direct demonstration is practicable, and since it will take up but a very small portion of your admirable periodical. I remain, &c. J. O. H.

A direct Demonstration of the Rule for the Multiplication of Negative Signs.

This rule may be proved by the assistance of the following proposition:

The product of a negative quantity into any other quantity is equal to *minus* the product of the first quantity without the negative sign, and the other quantity.

The product of a negative quantity into any other quantity signifies that that negative quantity is to be added or subtracted as many times as there are units in that other quantity, accordingly as this other quantity is positive or negative, and consequently the result will be the same as if the negative quantity were positive, and the negative sign added to the result.

Let the quantities to be multiplied be $-a$ and $+b$. Then

$$(-a)\times(+b) = -(+a)\times(+b) = -ab$$

and

$$(-a)\times(-b) = -(+a)\times(-b) = -(-ab) = +ab.$$

Q. E. D.

ON THE SOLUBILITY OF CARBONATE OF LIME, ETC. IN HYDROCHLORATE OF AMMONIA.

M. Vogel, in the following notice, which is of the greatest importance in the analysis of many inorganic substances, remarks, that in analytical researches, particularly in those which are for the purpose of discovering the elements which enter into the composition of a mineral, confidence has hitherto been placed in the statement, that amongst the bases which are precipitated by the alkalies or their carbonates, magnesia was entirely redissolved by the addition of a solution of hydrochlorate of ammonia, and that this method has been generally used to separate this substance from other bases, such as lime, alumina, &c. &c. It is a well-known fact that hydrochlorate of ammonia has the property of dissolving with facility many insoluble or difficultly soluble bodies; this is the case with tartrate of lime, chloride of silver, and the recently precipitated carbonates of nickel and zinc, &c: but one that is much less known is, that recently precipitated carbonate of lime does not wholly resist the solvent power of this salt, which is likely to cause error, and lead us to imagine that lime is magnesia in analyses, particularly when the first occurs only in minute quantities.

Thus when a solution of sulphate of lime is decomposed by an alkaline carbonate, the precipitated carbonate of lime easily redissolves in a solution of hydrochlorate of ammonia. Again, when very dilute solutions of chloride of calcium and nitrate of lime are decomposed by an excess of carbonate of potash, so as to leave no traces of lime in the supernatant liquid, by the addition of a concentrated solution of this ammoniacal salt the precipitate entirely disappears. These clear solutions of carbonate of lime in hydrochlorate of ammonia become turbid by exposure to the air, and a portion of carbonate of lime is again deposited; but there always remains a certain quantity dissolved which cannot be precipitated even by boiling the clear solution.

If precipitated carbonate of lime is washed with a sufficient quantity of water, it does not dissolve, after a lapse of twenty-four hours, so easily in the ammoniacal salt as when just precipitated; and even if the precipitate is not washed, but suffered to remain in the liquid from which it has been precipitated, its solubility in hydrochlorate of ammonia, although not prevented, is considerably diminished.

Even the natural compact varieties of carbonate of lime, and the minerals into which it enters, do not completely resist the solvent power of this salt of ammonia. Calcareous spar or Carrara marble finely powdered, and merely shaken for a few minutes with a solution of this salt, affords a solution, which contains, after filtration, a notable portion of lime. This solubility of carbonate of lime in hydrochlorate of ammonia is, however, very much less than that of carbonate of magnesia.

Carbonate of barytes recently precipitated from a dilute solution of the chloride of barium by carbonate of potash, disappears on the addition of a solution of hydrochlorate of ammonia. This is also partially the case when native carbonate of barytes, in powder, is mixed with a solution of the ammoniacal salt and then filtered: by evaporation of the liquor, a residue is obtained which contains some chloride of barium. The solvent power of sal ammoniac is equally exerted on recently precipitated carbonate of strontia.

When carbonate of magnesia has been dried at the temperature of boiling water, it becomes much more difficult, and takes a much longer time, to dissolve in the ammoniacal solution than when recently precipitated. The concentrated solution of carbonate of magnesia in hydrochlorate of ammonia becomes turbid by exposure to air, but does not deposit any magnesia by ebullition.

At first sight it may seem singular that the earthy carbonates before mentioned should lose their solubility in hydrochlorate of ammonia; but this may be explained by admitting that by repose they acquire a greater degree of cohesion, and approach the crystalline state, which would naturally diminish their solubility.

To conclude: the preceding experiments show, 1st, that the solubility of an earthy carbonate in hydrochlorate of ammonia will not authorize us to conclude that the precipitate is magnesia; 2nd, that freshly precipitated carbonate of lime, and also both marble and calcareous spar, are dissolved by this salt; 3rd, that carbonate of barytes recently precipitated, and also witherite and carbonate of strontia, are likewise dissolved by this agent; 4th, and lastly, that the solution of the above-mentioned earthy carbonates affords, by its decomposition, carbonate of ammonia and a chloride of the base.—*L'Institut*, Sept. 28, 1836, and *Jour. für Prackt. Chimie*, No. 7, 1836.

Note. The importance of the above notice of M. Vogel, in the analysis of those earthy minerals, in the course of which the earth is precipitated from a hydrochloric solution by carbonate of ammonia, has reminded me of some unfinished and hitherto neglected experiments which were made in the early part of this year, on the mutual action of hydrochlorate of ammonia and the carbonates of lime, barytes, and strontia; and as these lead to exactly the same conclusion as that at which M. Vogel has arrived, viz. the mutual decomposition of hydrochlorate of ammonia and the earthy carbonates, I am now induced to notice them, as they tend to confirm a fact which must so greatly affect the analysis of many minerals, especially those containing magnesia.

One equivalent, or 54 grains, of hydrochlorate of ammonia was dissolved in a few ounces of distilled water, and to this solution was added an equivalent, or 98 grains, of perfectly dry carbonate of barytes, obtained by precipitating a solution of the chloride of barium by one of sesquicarbonate of ammonia; this mixture was then boiled for about four hours, occasionally renewing the water which had been evaporated. During the ebullition, particularly at the commencement, carbonate of ammonia was disengaged, and at the expiration of four hours a perfectly clear solution was procured, consisting of chloride of barium and a trace of hydrochlorate of ammonia, but without the slightest trace of carbonic acid.

Equivalents of precipitated carbonate of strontia, 74 grains, and hydrochlorate of ammonia, 54 grains, treated in the same manner, were boiled for eight hours: long before the expiration of this time the vapour had ceased to exhibit the slightest traces of carbonate of ammonia; the solution filtered left 3·34 grains of carbonate of strontia undecomposed; the solution, consisting of chloride of strontium, when boiled with caustic soda, did not afford the slightest indication of ammonia; it is therefore most probable that the whole of the hydrochlorate of ammonia in this experiment was decomposed.

Equivalents of carbonate of lime (precipitated) and hydrochlorate of ammonia, boiled together for eight hours, left ·97 grains of carbonate of lime, and the solution, when treated with soda, afforded traces of ammonia.

These experiments not only confirm M. Vogel's conclusion, but they also show that, by an elevation of temperature, the mutual decomposing power of these bodies is materially increased; and it must now be a question whether at comparatively low temperatures, 32° of Fahr., for instance, this power is, or is not, completely suspended. This winter, if opportunity offers, a few experiments shall be made to determine this point, and their results, if of any interest, noticed.

J. Denham Smith.

METHOD OF DETECTING SULPHUROUS ACID IN THE HYDROCHLORIC ACID OF COMMERCE.

This process is founded on the action that protochloride of tin exerts on sulphurous acid. Pelletier, sen., noticed (*Ann. de Chimie*, tome xii. page 231, 1792,) that when solutions of these substances are mixed, the salt of tin deoxidizes the sulphurous acid, and affords a beautiful yellow precipitate, consisting of sulphur and peroxide of tin.

The mode of proceeding is as follows: Put about half an ounce of the hydrochloric acid which is to be tested into a glass, and then add about a quarter of an ounce of the salt of tin, such as is very white and not altered by exposure to the air: stir these together, and then add about two or three times its bulk of distilled water, and mix well.

When the hydrochloric acid does not contain sulphurous acid, no particular action ensues on the addition of the chloride of tin and the water; the salt of tin dissolves, and the solution is merely rendered slightly turbid by the action of the air.

But should the hydrochloric acid contain sulphurous acid, it will be seen, that on the addition of the salt of tin, the acid becomes turbid and yellow, and then, on adding the distilled water, the odour of hydrosulphuric acid will be easily distinguished, the liquid will be-become of a brown tint, and deposit a powder of the same colour. These phænomena are so distinct that we cannot hesitate for an instant as to the presence or absence of sulphurous acid.

Sometimes the brown colour is not developed until after the lapse of a few minutes, and the larger the proportion of sulphurous acid the darker is the shade produced. The disengagement of hydrosulphuric acid occurs at the time the mixture is diluted with water; by standing, the liquor deposits a yellowish brown powder, consisting of sulphuret and peroxide of tin. The reason of this curious reaction is easily explained. One portion of the salt of tin is converted into perchloride at the expense of another portion of this compound, and the tin set free from this decomposition acts on the sulphurous acid so as to produce at the same time both peroxide and protosulphuret of tin. The hydrosulphuric acid which is disengaged immediately after the addition of the water is owing to the hydrochloric acid acting on a part of the sulphuret of tin; chloride of tin is again formed, and hydrosulphuric acid gas liberated.

To obtain the phænomena described it is necessary to add the salt of tin to the hydrochloric acid before the addition of the water, for if we commence by diluting the acid, the addition of the salt does not produce any discolouration. This test is one of such delicacy and accuracy that M. Girardin, the author, assures us that it will detect a one hundredth part of sulphurous acid in a sample of hydrochloric acid.—*Ann. de Chim. et de Phys.*, March 1836.

ON PLATINA. BY J. W. DÖBEREINER.

When native platina is fused with twice its weight of pure zinc, this alloy, after cooling, pulverizes; and put into moderately dilute sulphuric acid until the action has ceased, and then heated with very dilute nitric acid, assisted by heat, a residue is obtained which, after washing with water, consists of undissolved iridium, osmium, and, in silver coloured grains, a heavy blackish-grey powder, composed of platinum, palladium, iridium, rhodium, and osmium.

This compound metallic powder possesses the same properties as the platina separated from its alloy of potassium or iron, which J. W. Döbereiner has examined and frequently described. It absorbs and condenses oxygen gas, and is such an oxidizing agent, that it not only converts the oxalic and formic acids into carbonic acid, and alcohol first into acetal, next into aldehyd, and then into acetic acid, but also converts the contained osmium into osmic acid, which sublimes at a gentle heat, or can be dissolved out by an alkaline solution. In the latter case the oxidizing properties of this metallic powder are still increased, and a preparation obtained which not only suddenly ignites hydrogen gas, but also the vapours of pyroxylic spirit and alcohol, and detonates when heated on platina foil, a property Descotils pointed out fifty-five years ago, but which has not been since noticed.

This powder is dissolved in aqua regia nearly as easily as gold.

Muriatic acid destroys its property of absorbing oxygen gas; so that it ceases to detonate on heating, or to act *metallically* (metalylisch, or as Berzelius calls it, catalylisch,) on the above substances: however, by heating it with a solution of a fixed alkali, its former power is restored.—*Poggendorff's Annals.*

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1836.

Chiswick.—Oct. 1, 2. Stormy with rain. 3. Heavy rain: boisterous: clear and frosty at night. 4. Frosty: fine. 5. Hazy: very fine. 6. Hazy, with rain. 7. Overcast: rain. 8. Dense clouds: rain at night. 9. Clear and fine. 10. Heavy rain. 11—13. Boisterous. 14. Fine. 15. Showery. 16—18. Foggy. 19. Very fine. 20—26. Foggy in the mornings: fine. 27. Showery: clear and cold. 28. Cloudy and very cold: north-west wind. 29. Snow. 30. Clear: snow yet remaining on the ground. 31. Sharp frost: fine. The depth of snow on the morning of the 29th was nearly three inches; and during the day there were stormy showers of snow in very broad flakes. Notwithstanding the bright sunshine on the following day, the snow generally still continued to cover the ground.

Boston.—Oct. 1. Rain. 2. Cloudy. 3. Rain. 4, 5. Fine. 6. Fine: rain P.M. 7. Fine. 8. Cloudy. 9. Fine: rain early A.M. 10. Rain. 11. Stormy: very stormy night: rain early A.M. 12. Fine. 13, 14. Cloudy. 15. Cloudy: rain early A.M.: rain A.M. 16. Fine. 17. Cloudy. 18. Cloudy: rain P.M. 19, 20. Fine. 21, 22. Cloudy. 23. Fine. 24. Foggy. 25. Cloudy. 26. Fine. 27. Stormy: rain early A.M. 28. Fine: ice this morning. 29. Snow: snow six inches deep. 30. Fine: hail-storm early A.M. 31. Fine: great deal of snow on the ground.

Our Correspondent Mr. Veall adds the following note:

"I am in possession of journals of the weather kept at Boston during the last twenty years, but do not find such a fall of snow recorded in the month of October."

Dr. Hudson's Statement in reply to a Statement made by Dr. Apjohn in the London and Edinburgh Philosophical Magazine for September 1836.

To the Editors of the London and Edinburgh Philosophical Magazine.

Stephen's Green, Dublin.
5th November, 1836.

Gentlemen,

It was with reluctance I felt called on by Dr. Apjohn's statement in your Magazine for September, to occupy the pages of your useful Journal by a statement of the real facts of the case, feeling conscious that it could not tend in any way to the *progress* of science; I therefore freely acquiesce in your reasons for excluding the discussion from the pages of your Journal. As however, Dr. Apjohn's statement has been placed in the hands of your readers, I feel I have a right that my reply should also be conveyed to them in order to counteract any unfavourable impressions which might arise from his misrepresentations. I have therefore to request that you will permit my statement to be stitched up with the Number for next Month.

I am, &c., H. Hudson.

DR. HUDSON'S STATEMENT, &c.

Gentlemen,

It was not without surprise that I read the paper of Dr. Apjohn, dated the 5th of July, and published in your Magazine for this month (September), which purports to be a reply to certain statements contained in papers of mine inserted in your 7th and 8th volumes. It was not without surprise, because Dr. Apjohn had on the 12th of the same month of July, in a printed circular addressed to the Members of the Royal Irish Academy, (to whom the discussion of the question pending between us had previously been confined,) stated formally "that, as far as he was concerned, the controversy was closed;" and without any intimation of, not merely his intention to transfer, but of his having but one week before transferred the entire subject from the members of the Academy to the public at large, by means of a paper forwarded to your widely circulated Journal. I replied to that circular, also taking leave of the controversy, leaving the question between us to be determined, calmly, on the documents with which the members of the Academy had been furnished.

In Dr. Apjohn's new paper in your last Number he excuses his transferring this discussion to the public, by saying that "though it be contrary to his original intention, he finds himself eventually in some degree *compelled*" to it. He does not inform us how he became thus suddenly "compelled," nor has he subjoined any explanation of his suffering to appear in September his paper on a controversy which he so solemnly renounced in July.

But, however Dr. Apjohn may reconcile this proceeding to himself, he certainly has imposed upon me the necessity of submitting, with your permission, my statement of the facts to the same degree of publicity which he has obtained for his.

The question respecting the solution of the dew-point problem by means of the wet bulb hygrometer having been proposed by the British Association, a good deal of attention was excited with respect to the subject; and, among others, I had formed certain views with regard to it. On the 27th of October 1834, Dr. Apjohn read to the Royal Irish Academy his paper on the *theory* of the wet bulb hygrometer: I heard this paper read with pleasure, and on

Wednesday the 29th of October I wrote to Dr. Apjohn, (with whom I was not previously acquainted,) communicating the *formulæ* my views had led me to adopt. His reply of the following evening was in these terms:

(No. 1.)

"28, *Lower Baggot Street*,
"*Thursday Evening*. (30*th October*, 1834.)

"My dear Sir,—I feel much flattered by the observations which you have been pleased to make upon my paper read at the Academy, because they obviously proceed from a person who has devoted attention to the subject discussed in it. I have also read with much pleasure the views which have occurred to you in reference to the same question, and as I am sure you have communicated them to me in a scientific spirit, I shall not hesitate to state to you the opinion which I entertain respecting them. Your formula for expressing the amount of caloric necessary for the formation of a given weight * of vapour at any temperature t is perfectly correct; my impressions however are different in reference to the next topic which you discuss, namely, the temperature of *dry* air corresponding to any temperature of the wet bulb hygrometer, thus at temperatures indicated by the hygrometer of 92°, 65°, and 32°.† You state the corresponding temperatures of dry air to be 155°, 94°, and 42°·4; whereas by my formula they would be 215°·28, 118°·59, and 49°·4; but supposing the numbers in your table to be correct, I am disposed to think that your method, the conception of which is certainly highly ingenious, would give at least a good approximation to the dew-point. I have now only to observe upon your concluding formula. In it, I think you will find that there is some error, for upon making the calculation I find that the density of vapour at 65° is not (as you state it,) ·0163, but ·026.

"In conclusion, I beg to state, that I shall be most happy to communicate with you further, should you be inclined, on this subject, and that

"I am, very sincerely yours,
"James Apjohn.

"N.B.—I take advantage of the circumstance of this not being sent you as early as I intended it should, to retract to a certain extent an observation already made. I have said, that the principle of your method for collecting the amount of moisture in air, from the temperature of the wet bulb hygrometer, appeared to me sufficiently correct to afford a good approximation. Upon consideration, however, I fear that this is not the case. You assume that the depression indicated by the hygrometer, in dry air, is to the depression in air containing moisture, as the saturating amount of vapour at the temperature of the hygrometer to the difference between this quantity and what is actually present. Such proportion would be‡ true of two portions of air at the *same temperature*, the one dry and the other moist, but I believe in no other case."

(No 2.)

In reply I sent him the proofs of my formulæ (see Lond. and Edinb. Philosophical Magazine, vol. vii. p. 257,) and his answer was as follows:

"My dear Sir,—*Both your formulæ*,—that for the quantity of caloric in a given bulk of vapour at any temperature, t, and that for the density of vapour,—*are quite correct in reference to the standard which you adopt*. In the latter formula you assume

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* This should be *volume*; he has corrected it in letter 2.—H. H.

† These were only taken hypothetically, thus: "If dry air at 94° makes hygrometer fall to 65°, then the corresponding temperatures of dry air when the hygrometer is at 92° and 32° will be 155° and 42°·4 respectively." It was merely an exemplification of the use of the proportional numbers in my table.—H. H.

‡ This is quite erroneous. The proportion would be *impossible* if dry and moist air of the *same temperature* were to act on the hygrometer, for the stationary temperature of the latter would be very different in the two cases, and of course there could be no *comparison*. But it is true, when dry and moist air are of *such different* temperatures, that they cause the wet bulb to fall to the *same temperature* in each case.—H. H.

air at 212° to be represented by unity, upon which hypothesis ·625 is the specific gravity of steam at 212°, and ·0163 its specific gravity at 65°. You must be aware, however, that these numbers are not comparable with those in our tables, in as much as they are related to a different radix. My calculation of the density of vapour at 65° (by the way it is ·0127 and not ·026, as by mistake I wrote it,) is conducted upon the usual supposition, of air at 60° being the unit; your result however beomes the same with mine, by applying the proper correction, or by diminishing it in the ratio of 660 : 508. The general expression, in fact, for the specific gravity of the maximum amount of vapour corresponding to any temperature, is $\cdot 625 \times \frac{f}{30} \times \frac{508}{448+t}$, f being the elastic force of vapour, and t its temperature.

"I have now, Sir, much pleasure in informing you that your method* of inferring the exact amount of moisture at any time existing in air, is perfectly correct. I have deduced from my own method, that (D being the reduction of temperature experienced by the hygrometer in dry air, and d the reduction in moist) D : D—d : : f' : f''; f' being the elastic force of vapour at the temperature of the hygrometer, and f'' at dew-point, a conclusion equivalent to yours[a]. Your method, however, cannot admit of practical application until D be determined for every possible temperature of the hygrometer. This is a thing which I can do *theoretically* ; and as soon as I have leisure, I shall be happy to furnish you with such a table. I should, however, be most happy if you would undertake it experimentally. Even a few experimental results would be highly interesting.

" Believe me, dear Sir, very sincerely yours,

"James Apjohn."

[a] "It is scarcely necessary to say that this is but an approximate conclusion and that it will require the application of the corrections which I have described in my paper.—J. A."

I replied to this : but some expression in my letter appears to have annoyed Dr. Apjohn, judging from his answer, which follows :

(No. 3.)

"My Dear Sir,—In reply to your note just received, I beg to make the following statements :—

"1st. In my answer to your first communication, I stated that your formula for the amount of heat in a given volume of vapour, was correct, but that your value of the density of vapour at 65° was otherwise. These statements I repeated in my second note to you, so that, as far as respects them, I cannot perceive the insistency which you seem to glance at.

"2nd. I differ from you as to the number of corrections to be applied, and as to the possibility of—consistently with rigid accuracy—omitting any of them.

"3rd. I do not agree with you as to the effect of radiation on the wet bulb hygrometer, though I am not aware of any accurate method of correcting for it.

"4th. I am quite satisfied that your table† of the temperatures of the hygrometer and dry air is quite incorrect.

"5th. That such being the case, your value of D is erroneous, and so also must any conclusion be deduced from it.

"6th. I beg again to observe, that *as soon as you can tell the temperature of dry air corresponding to* ANY *temperature* of the hygrometer, and be able to apply the necessary corrections, *you will be in possession of a method for inferring the dew-point, by means of the wet bulb hygrometer.*

"7th. I am quite at a loss to understand what you mean by saying that "the temperature of the wet bulb hygrometer, *is*‡ the dew-point of the atmosphere in

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* Comparing the mistake pointed out in the postscript to his first letter, with the *deduction* here subsequently arrived at, I think it will be obvious to any one, that the very idea of the action of dry air having anything to do with the formula had not previously occurred to Dr. Apjohn at all, and of course that the experimental method is mine.—H. H.

† This is the same mistake pointed out in the 2nd note on his first letter.—H. H.

‡ For "*is*" read "*gives.*"—H. H.

which the experiment is conducted." If such were the case, you and I have been greatly mispending our time in searching by calculation for that which is given by observation.

"8th. Not having by me the work of Daniell, to which you refer, I cannot examine the table to which you direct my attention.

"I showed however several years ago, that many of his numerical results were erroneous. (Dublin Philosoph. Journal, No. 1. Review of Daniell's Meteorolog. Essays.)

"I am, dear Sir, very truly yours,
"JAMES APJOHN."

In his 6th statement he admits (above) that if I knew the temperature of dry air, corresponding to *any* temperature of the hygrometer, I should be in possession of a method for inferring the dew-point; in his second letter he had said that D must be determined for *every possible temperature*; in his fourth letter, again, he *apparently* relapses into his original error, saying that "a most *elaborate series* of experiments would be necessary, *in as much as the depression in dry air varies with the temperature of the wet ball.*" His fourth letter was as follows:

(No. 4.)

"MY DEAR SIR,—I have been prevented by the illness of * * *, from attending to your letter of yesterday; but in looking it over I do not find I have anything to add to what I have already stated to you. I will, however, in conclusion, repeat, that your numbers for the depression* of the hygrometer in dry air, are greatly erroneous, and *that a most elaborate series of experiments would be necessary to render your hygrometrical process available, in as much as the depression in dry air varies with the temperature of the wet bulb hygrometer.* I have not been able to look at the table to which you have referred me, but from your note just received, this can be a matter of no importance.

"J. APJOHN."

The Letters marked 2, 3, and 4, had no dates, but they were all received before the 10th of November, 1834. I then wrote the paper † which has been published in the London and Edinburgh Philosophical Magazine for October 1835, and inclosed it to Dr. Apjohn with a letter, (24th November 1834,) asking him to bring it before the Academy for me. The Academy met on the 29th November, and on the 30th Dr. Apjohn returned the paper with the following letter:

(No. 5.) "28, *Lower Baggot street.*
"*November* 30.

"MY DEAR SIR,—I have been so much occupied that I have had scarcely time to look over your paper with proper attention; most of the topics however touched upon in it, you have already been good enough to acquaint me with, and I believe I have ventured to observe upon them. The principle of your method I have, I believe, on a former occasion stated to be, in my opinion, correct, for V : V $-y$: : moisture of saturation, to actual moisture, is easily deducible from my formula; with the value of V, however, you do not appear to be acquainted, though I trust you will not continue so long, *but that you will investigate it by some of the* METHODS *which you have suggested*: until you have done this I think you had better not submit your paper to the public, but you will, of course, in this particular, be altogether governed by your own judgement; I trust you will also apply yourself to the corrections, particularly that rendered necessary by the

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* The same mistake again. (See my 2nd note †, on Letter 1.)—H. H.

† The notes in pages 259 and 261, together with the *numbers* in the fifth column of the Table, and from the words "the principal recommendations" in p. 263 to the end of p. 264 were added to the paper in March 1835, subsequently to the publication of Dr. Apjohn's first paper.

heat communicated to the hygrometer by *radiation*, for without such your method would afford but an approximate result. With respect to introducing your paper to the Academy, this I shall certainly do at the next or any subsequent meeting, should it be your wish; I think however that, being a member yourself, I could not with propriety take the subject out of your hands.

"Believe me, dear Sir, very truly yours,

"JAMES APJOHN."

"N.B. In reference to your postscript, I beg to say that I of course represent by f' and f'' the maximum elasticities of vapour at the temperatures of the hygrometer and dew-point; these elasticities, by the way, are not strictly as the saturating quantities of moisture at those temperatures; but the error is altogether inappreciable, and is in my method allowed for.—J. A."

The *methods* which Dr. Apjohn here so distinctly admits to have been *suggested* by me in that paper, were these:—One was to expose a wet and dry thermometer (equally) to a current of air dried artificially and heated at pleasure. This Method Dr. Apjohn *adopted in March* 1835, and published as his own. The other *method* I pointed out was to use *conjoint observations* of the "dew-point," the "temperature of the wet ball," and the "temperature of the air;" and the best mode of investigating by this method was stated by me to be *similar* to the former method, only using common (not dried) atmospheric air, heated artificially. This method Dr. Apjohn adopted in February 1835, and subsequently published without acknowledgment. I also stated in my paper that these *latter experiments* would be *best tried* when the atmosphere was *perfectly damp*, as the knowledge of the dew-point would then be perfect, and Dr. Apjohn's experiments (in which the air was rendered *perfectly damp* artificially before heating it,) were commenced about the middle of April 1835. (See his 2nd paper.)

Dr. Apjohn having advised me in the above letter not to bring my paper before the public, until I had made the experiments suggested in it, I laid it by, intending, when I should have leisure and could procure the necessary apparatus, to make those experiments. Feeling grateful for what I considered *judicious and friendly advice*, I instituted (in compliance with Dr. Apjohn's wishes in his letters, Nos. 3 and 5,) some additional experiments on the radiation of heat as *immediately* connected with the wet ball hygrometer, (see 5th Report of British Association, p. 165. Experiments 3 and 4,) and communicated them to Dr. Apjohn in February 1835. They apparently satisfied him that no error could arise from this cause. Early in March I learned from him, incidentally, that he had adopted my methods of experimenting, and being convinced that my paper would lose any little interest which it might have, if not published *before the experiments*, I sent it, about the *middle of March* 1835, to the London and Edinburgh Philosophical Magazine, mentioning in a note that Dr. Apjohn had adopted those methods, and having no idea that he would not have acknowledged my communications on the subject.

Dr. Apjohn read his 2nd paper to the Academy on the 27th of April 1835, containing his experimental results, and transmitted it to your Magazine without any acknowledgment of my having suggested such experiments to him: under these circumstances I felt myself justified in laying claim to "having suggested those *methods of experimenting* to Dr. Apjohn."

This claim having been alleged by Dr. Apjohn in the Academy to be wholly without foundation, I felt it to be most respectful to the Academy to propose that the correspondence between us should be submitted, either to the Council of the Academy, or to a committee, to be appointed by them to inquire into the subject and report thereon to the Academy. I stated that I would submit to them Dr. Apjohn's letters to me, and asked Dr. Apjohn to lay *my letters* *to him* also before the Council; he stated that "he had not kept them;" as I had not retained copies of my own letters, I inclosed a statement of facts, with Dr. Apjohn's letters, to the Secretary (the Rev. Dr. Singer), in a sealed parcel which was subsequently returned unopened, the Council having resolved that it was not expedient that scientific bodies should attempt to decide on claims of priority on scientific subjects; under these circumstances I had no course left for my vindication among the members, but to print the letters for the purpose of enabling them to judge for themselves; and as Dr. Apjohn has now removed the discussion from the limits of the Academy to the far wider field of your valuable Journal, I feel equally called on to put your readers in possession of the real facts of the case as proved by Dr. Apjohn's own letters. With respect to the complaint of my having *published* Dr. Apjohn's letters, I may repeat that Dr. Apjohn and I, up to the period of that correspondence, never had any intercourse, that the correspondence related solely to the subject of the moist bulb hygrometer, that it contained nothing private or confidential, and that Dr. Apjohn himself created the *necessity* for my producing his letters, by wholly denying my claim.

With regard to Dr. Apjohn's charge (p. 189 of your last Number [September 1836]) that I had "created an absurdity in his formula for the purpose of commenting on it," or else that I "continued to misrepresent him after he had complained of my having perverted his formula;" in reply, I give the following extract from Dr. Apjohn's letter to me on the subject:

"19*th February*, 1835."

"* * * * * I am sorry to be obliged to confess, that I may have led you somewhat astray by a communication I made to you at the commencement of our correspondence. I stated, I believe, that your proportion for arriving at the dew-point was a consequence of my formula; upon however looking at the matter again, I find that *such is not the case*. If D^{-} = depression in dry and d in moist air, f^{-} the elastic force of vapour at the stationary temperature of the hygrometer in dry, and f' in the moist air, and f'' the elastic force of vapour at the dew-point of the latter, then by my formula I can show that $D : d :: f^{-} : f'' + f^{-} - f'$. I was led to the erroneous result which I imparted to you, by confounding f^{-} with f', quantities which are quite distinct."

The above sufficiently shows that the absurdity was not of my creation. Dr. Apjohn undoubtedly complained that I had given an erroneous formula as his, but he did *not* enter into particulars, as he states he did; I have now, indeed, no doubt that it was his intention to particularize, and that he believes that he did so. I then conceived, however, that his complaint referred to the formula I gave as his (corrected) in the note, p. 259, vol. vii. of your Journal, and replied thereto. The extract from his letter shows also that I had an ample answer to his first charge, and consequently (if, even, I were capable of such a thing,) could have no object in *affecting* to misunderstand the formula to which he really intended to allude. It may be supposed that the error in the letter merely arose from the accidental omission of D in the second term of the formula; but is the context reconcileable with the explanation given by Dr. Apjohn on this assumption? He now says that having found that my proportion of $D : D - d :: f' : f''$ was only true in the *particular case to which I applied it*; he sent me the other more general formula $D : D - d :: f^{-} : f^{-} - f' + f''$ as embracing the former. But what says his letter? that he has found that "*the former proportion is not a consequence of his formula.*" This needs no comment.

Dr. Apjohn has stated positively (see p. 191,) that he "had resolved to employ *all* these methods of experimenting before I commenced that correspondence with him." In support of this he has adduced no evidence except a note from Professor Lloyd, written more than a year after the various papers had been communicated. In this note Professor Lloyd mentions a conversation with Dr. Apjohn relating to the determination of the specific heats of gases by means of *one* of those methods of experimenting, stating that it occurred on the evening on which Dr. Apjohn read his *first* paper to the Academy, a statement which Professor Lloyd has since distinctly reasserted. With the highest respect, however, for Professor Lloyd, and in the most ample manner admitting his truth and honour to be unquestionable, I cannot but think he is mistaken *respecting the evening* on which Dr. Apjohn made this communication to him, and that it probably took place in April 1835 when Dr. Apjohn read his *second* paper. After the lapse of more than a year undoubtedly a mistake as to the evening on which a particular conversation occurred is by no means impossible, and it appears to me in the highest degree probable, from my inability to reconcile the statement (or any other supposition) with the written evidence furnished by Dr. Apjohn himself. Now this is Dr. Apjohn's only evidence, and how do the facts stand on the other side?

First. Dr. Apjohn made no allusions whatever in his first paper to instituting any *experiments*. He says, "I shall not at present refer to my own *observations*, though I have of late amassed a considerable number on the hygrometer and dew-point;" so that he had been (just before) amassing *observations* to test his formula, which, if he had any intention, at the time, of instituting *experiments*, would have been a very useless waste of time; indeed he appears subsequently to have thought so, for in his second paper, (speaking of *observations* of this sort,) he says, "The observed depressions in the table are, generally speaking, so small that a formula in itself incorrect might, it must be admitted, yield results which would deviate from the observed dew-points by quantities not exceeding the possible errors of *observation*," and accordingly these *observations* which he had been amassing up to the reading of his first paper, he never has brought forward, although at the time (it would appear from the words "at present") he intended to have done so.

Secondly. He admits in his letters that I communicated experimental methods to him, and there is not a *single word* in those letters *implying* even that he had contemplated anything of the sort, *much less* that he had spoken to any one previously on the subject; and surely if he had done so he would naturally have mentioned it when I *suggested the methods* in question, as he has admitted in his fifth letter. On the contrary, when I gave him the proportion $D : D - d : :$ moisture of saturation : actual moisture, in the N.B. to his first letter he *denies its correctness*, and states it to be only true in a case in which (as I have shown) it would be wholly erroneous. In his second letter he *admits its correctness* because he *had deduced* from his formula that $D : D - d : : f' : f''$, a *conclusion equivalent to mine*; consequently, I presume, he had *not deduced* this when he wrote his first letter in which he denied its correctness. In his fifth letter he said again that my proportion was correct, *for* it was *easily deducible from his formula*; but in February 1835 (see the extract in the preceding page) he says that such is *not the case*, and attempts to account for his *erroneous result*.

Thirdly. In his second letter he says he can give the fall of the hygrometer in dry air *theoretically*, and offers to *furnish me with such a table*. Would not this appear to *imply* that he was *satisfied* with the correctness of his *theoretical values*, and had no idea of instituting experiments himself, *although*, as he says, "he would be happy if I would undertake it experimentally, as even a few experimental results would be *highly interesting*."?

I have only to add that Dr. Apjohn has admitted that I suggested the experimental methods to him, and never gave me the *slightest intimation* that they had previously occurred to him; consequently whatever opinion may be formed by the reader on the subject, Dr. Apjohn has only to blame himself for what has occurred. As to anything personal in Dr. Apjohn's numerous and ungracious insinuations against me, I shall not notice them: throughout the entire discussion it has been my wish to avoid all unbecoming language, not only because such a practice is repugnant to my habits of thinking and acting, but because I am convinced that by such means a good cause can only be injured.

With respect to the last subject alluded to by Dr. Apjohn, viz. his formula for the specific heats of gases, I saw it for the first time on the 4th of November 1835 (in the Philosophical Magazine for that month), and, on meeting him at the Academy on the 9th of November, told him that his formula was erroneous. He smiled and shook his head. I may add that *the moment* I saw his *experimental depressions* of the wet bulb's temperature in common air and hydrogen gas, I perceived that his deductions were *monstrously erroneous*, on a *principle* that I DID think must have been obvious to any one who understood the spirit of the method. It is simply this:—If a given volume of one gas (in producing a certain evaporation) fall 20°, while the same volume of another gas in producing the same (or a less degree of) evaporation, falls 25°, it is evident that the specific heat of the former gas (*estimated by volume*) is greater than that of the latter gas. I mentioned this principle to Dr. Apjohn on the occasion just alluded to, and his answer was "Oh no! depend upon it you are quite wrong; it has nothing to do with the capacities estimated by volume." Under these circumstances therefore I sent the correct formula to the Philosophical Magazine on the 11th of November, and (I believe on the same day) communicated it in a letter to Dr. Apjohn himself. To this letter, however, I received no answer.

The above is a plain statement of the facts as between Dr. Apjohn and me; but the reader of his account of the same matter would necessarily be led to believe that I was present at the meeting of the Chemical Section of the British Association in August 1835, when he brought forward his erroneous formula (which was not the case); that I then pointed out the error, which he immediately admitted, being thankful to me for the suggestion; and that I, notwithstanding his thus readily adopting my correction, forwarded a paper on the subject to the Philosophical Magazine. If such had been my course, I certainly might have deserved a share of that censure which Dr. Apjohn has endeavoured to direct against me; the reader, by the facts and dates which I have supplied, but which are suppressed in Dr. Apjohn's account, will now perceive that this, like his other charges against me, is wholly destitute of foundation.

I am, Gentlemen,

Your obliged and obedient Servant,

H. HUDSON.

24, *Stephen's Green*,
10*th September* 1836.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. Thompson at the Gardens of the Horticultural Society at Chiswick, near London; and by Mr. Veall at Boston.

Days of Month. 1836. Oct.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Self-registering. Max.	Max.	Min.								
S. 1.	29·356	29·393	28·959	28·90	52·3	41·5	55·3	54	41	46	SE. var.	S.	SE.	·125	·39	·05	48
☾ ⊙ 2.	29·247	29·354	29·194	28·65	47·9	42·9	52·5	54	41	47	SW.	SW.	NW.	·294	·48	·47	44
M. 3.	29·008	29·430	28·940	28·55	43·8	43·4	52·2	55	29	43	SSW. var.	W.	N.	·380	·04	·25	44
T. 4.	29·497	29·622	29·457	28·93	45·2	39·3	53·3	61	34	45	SSW.	SW.	calm	...	...	·03	41
W. 5.	29·829	29·956	29·869	29·35	44·6	40·4	56·2	63	36	46	SSE.	SW.	calm	...	·04	...	40
Th. 6.	29·853	29·929	29·525	29·42	49·3	43·3	57·2	59	52	45	ESE.	SE.	calm	·036	·78	...	45
F. 7.	29·511	29·528	29·374	28·95	56·6	48·6	59·0	61	50	56·5	SE.	S.	calm	·752	·14	·57	51
S. 8.	29·386	29·385	29·374	28·83	54·8	52·7	57·7	61	40	56	SSE.	S.	NE.	·091	·25	...	52
⊙ 9.	29·360	29·433	29·375	28·85	51·7	47·3	56·3	60	46	51	S.	S.	NW.	·283	·09	·10	50
● M. 10.	29·219	29·376	29·284	28·73	54·5	48·4	60·6	65	53	51·5	SE. var.	SW.	NW.	·133	·36	...	50
T. 11.	29·247	29·394	29·182	28·46	55·7	53·8	58·8	60	41	55·5	SSW.	SW.	NW.	·080	·01	·41	52
W. 12.	29·606	29·589	29·194	29·0	52·4	46·9	58·7	61	50	51	S.	SW.	NW.	...	·20	...	48
Th. 13.	29·140	29·453	29·093	28·50	58·7	51·5	59·2	61	51	53	SW. var.	SW.	W.	·097	·03	...	52
F. 14.	29·782	29·952	29·740	29·10	54·5	51·3	58·5	60	49	55	SW.	SW.	W.	·172	·02	...	50
S. 15.	29·853	30·051	29·857	29·34	55·6	51·0	59·3	64	39	53	E.	S.	E.	...	·06	·02	52
⊙ 16.	30·229	30·231	30·193	29·68	48·8	46·4	57·0	60	51	47	SSW.	E.	calm	·061	...	·06	48
☽ M. 17.	30·210	30·202	30·170	29·65	53·4	48·2	56·5	57	52	54	E.	E.	calm	...	...	...	51
T. 18.	30·174	30·169	30·139	29·52	56·2	52·8	61·2	63	53	56	SSE.	S.	calm	...	·13	...	53
W. 19.	30·241	30·426	30·224	29·56	57·6	54·4	59·4	64	36	56	SW.	W.	calm	·158	·01	·23	54
Th. 20.	30·469	30·495	30·396	29·90	46·2	41·8	50·3	55	36	44	SW.	S.	calm	...	...	...	43
F. 21.	30·332	30·334	30·310	29·81	49·2	44·8	52·6	56	42	47	ESE.	SE.	calm	...	...	...	46
S. 22.	30·398	30·413	30·397	29·85	49·3	45·8	53·8	58	35	48·5	E.	E.	calm	...	...	...	45
⊙ 23.	30·408	30·436	30·400	29·88	44·7	40·4	55·2	59	39	47	E.	SW.	calm	...	·01	...	42
○ M. 24.	30·394	30·417	30·340	29·82	49·5	43·9	53·2	53	46	48	SSW.	SW.	calm	...	·01	...	45
T. 25.	30·371	30·280	30·209	29·67	49·6	51·7	51·3	54	47	53	SW.	W.	calm	...	...	...	48
W. 26.	30·196	30·201	30·010	29·62	50·9	48·7	52·7	54	47	52	SSW.	SW.	calm	...	...	...	50
T. 27.	29·663	30·011	29·700	29·10	51·8	48·8	47·7	53	34	48	SW.	NW.	NW.	...	·15	·04	47
F. 28.	29·913	29·945	29·710	29·45	40·2	37·2	40·6	42	23	35	WSW.	NW.	calm	·080	...	...	39
S. 29.	29·473	29·865	29·495	29·23	35·6	33·6	34·2	37	27	34	S.	N.	calm	...	·42	...	35
⊙ 30.	30·019	30·184	30·014	29·68	35·2	32·9	37·7	40	23	36	W.	NW.	calm	·288	...	·50	33
M. 31.	30·089	30·141	30·133	29·74	33·2	30·0	39·3	41	26	30·5	W.	NW.	calm	·033	...	...	30
	29·822	30·495	28·940	29·28	49·3	45·3	53·5	65	23	48·				Sum 3·063	3·62	2·73	46·1

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END OF THE NINTH VOLUME.

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