THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

REGIUS PROFESSOR OF NATURAL HISTORY, LECTURER ON MINERALOGY, AND KEEPER OF THE MUSEUM IN THE UNIVERSITY OF EDINBURGH;

Fellow of the Royal Societies of London and Edinburgh; of the Antiquarian, Wernerian and Horticultural Societies of Edinburgh; Honorary Member of the Royal Irish Academy, and of the Royal Dublin Society; Fellow of the Royal, Linnean and Royal Geological Societies of London; Honorary Member of the Asiatic Society of Calcutta; of the Royal Geological Society of Cornwall, and of the Cambridge Philosophical Society; of the York, Bristol, Cambrian, Whitby, Northern, and Cork Institutions; of the Natural History Society of Northumberland, Durham, and Newcastle; of the Royal Society of Sciences of Denmark; of the Royal Academy of Sciences of Berlin; of the Royal Academy of Naples; of the Imperial Natural History Society of Moscow; of the Imperial Pharmaceutical Society of Petersburgh; of the Natural History Society of Wetterau; of the Mineralogical Society of Jena; of the Royal Mineralogical Society of Dresden; of the Natural History Society of Paris; of the Philomathic Society of Paris; of the Natural History Society of Calvados; of the Senkenberg Society of Natural History; of the Society of Natural Sciences and Medicine of Heidelberg; Honorary Member of the Literary and Philosophical Society of New York; of the New York Historical Society; of the American Antiquarian Society; of the Academy of Natural Sciences of Philadelphia; of the Lyceum of Natural History of New York, of the Natural History Society of Montreal; of the Geological Society of France; of the South African Institution of the Cape of Good Hope; of the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts; of the Geological Society of Pennsylvania, *&c. &c.*

APRIL...OCTOBER 1836.

VOL. XXI.

TO BE CONTINUED QUARTERLY.

EDINBURGH:

ADAM & CHARLES BLACK, EDINBURGH;

LONGMAN, REES, ORME, BROWN, GREEN & LONGMAN, LONDON.

1836.

PRINTED BY NEILL & COMPANY, OLD FISHMARKET.

CONTENTS.

CONTENTS.

CORRIGENDA.

Page 132, line 25, *for* 3 h. 2 m. 345 s. *read* 3 h. 1 m. 54.5 s.
— 161, — 1, *for* observed by him *read* observed by Dr Parnell (*the pronoun having reference to the preceding paragraph*).

THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL.

On the Geology of Auvergne, particularly in connexion with the Origin of Trap Rocks and the Elevation Theory. By Professor FORBES. Communicated by the Author.*

IT can hardly at the present day be required that I should present any formal apology for offering my speculations on a subject removed, as some may suppose, from those to which, professionally, my attention is habitually directed. Yet I willingly take the occasion of presenting to the Society, together with the few and somewhat desultory observations which these remarks are intended to introduce, some reflections on the position which geology ought to hold in relation to the other sciences; reflections which the recent application of mathematics to the elevation theory of Von Buch and Elie de Beaumont render the more fitted to the present occasion.

If geologists have had reason to congratulate themselves upon the escape of their science from the hands of the cosmogonists of a century ago, they may perhaps one day discover that, as regards the progress of knowledge towards the end which all consider as the ultimate aim of science, the discovery of *causes*, the reform which was happily wrought in geology, has been carried to an extreme.

The great mass of exertion which has, within the last forty years, been brought to bear upon the subject, has been, I fear we must confess it, rather the exertion of the hand than of the head. With a self-denial, in moderation pre-eminently praiseworthy, have geologists, especially those of England and Germany, been accumulating and classifying *facts*, which in inductive philosophy essentially form the basis of reasoning. Of this we

* Read before the Royal Society of Edinburgh on 7th and 21st Dec. 1835. A collection of specimens illustrative of the paper, was at the same time presented to the Society.

should be the last to complain, were these efforts always well directed; but even the rigid canons of the Baconian school admit of discrimination in the search of truth, a faculty nearly (though not quite) as essential in the observer as in the experimentalist, and without which he cannot have the slightest clew to separate what is important from what is trivial,—what is adapted for immediate application to the ends of science, from what it would be soon enough to detail a century hence,—without which the results of his labour, however accurate, may be redundant, and his painfully collected cabinet, as useless to science as if its elements had taken their natural course to the chisel or the limekiln.

The geologist has confined himself too much to the accumulation of undigested facts. He has considered his science too much as a mere department of natural history, without reference to the far more important questions of *cause* producing *change*, which brings it under the immediate dominion of natural philosophy. For although we cannot for a moment overlook the bold and admirable speculations of Hutton, and the researches consequent upon them conducted in the best spirit of inductive science by Playfair and Hall,—it must be admitted that this chapter of scientific history is in a great measure insulated from that of the age, and that the methods and results of these observers have scarcely been quoted, even by those who have adopted their conclusions, without materially enlarging their premises.

This neglect of the great ends of geology, may in some measure be ascribed to a discovery which ought to have been one of the principal sources of their advancement. The relations of the organized fossils found in the strata to the order of superposition of these strata, was calculated to afford an indication of the highest value for the classification of the groups of rocks where mineralogical characters were awanting, and for the identification of insulated deposits.

We ought not, perhaps, to be astonished that so pregnant a discovery was carried in its consequences rather to excess. But we could not have been prepared for the almost total devotion with which geologists have surrendered themselves to Palæontology,—a devotion which has produced a smile even on the part of zoologists themselves, when they found the good-natured facility with which the analogies of their science were received as the canons of geology.* The consequence has been, that geology, of late years, has become little more than a commentary on organized fossils. The great ends of the science seem to have been forgotten, and strata are no longer examined in the hope of detecting the proof of any mechanical or chemical change, but simply for the sake of classifying a new plant, shell, or reptile. The Dynamics of Geology have been overlooked, or, if adverted to, have been engaged in (too often) with partial views and inadequate preparation.

Yet it is to this point that the exertions of the geologist might be successfully applied. His object is the inquiry into the nature of changes which have supervened in the condition of the earth's surface. Such changes are of two kinds, mechanical and chemical; to investigate these, we might naturally expect that natural philosophy and chemistry would be thought essential,—but geologists in general have brought to their task but a very slight acquaintance with those sciences. I am persuaded that it will be by drawing the attention of three classes of persons, viz. natural historians, chemists, and natural philosophers, to this one very complicated but very important subject, that any real advances are to be made, and perhaps even the alphabet of the science of geology is scarcely yet formed. The natural historian is to collect the facts or data of the problems upon which the others are to reason; and such a division of labour has, we all know, been productive of the highest benefit to every one of the mixed sciences. Such geology undoubtedly is, and one, we are persuaded, even more complex than is commonly imagined. The ordinary theories of geological change often refer to purely hypothetical cases, much simpler than those which occur in nature. The mode of action of the forces unanimously admitted into geology is in reality so complex, that to expect to trace an immediate analogy between the conclusions of mere reasoning and

* "Il n'est bruit que des hautes révélations faites par la zoologie au profit de la géologie, et que cette dernière avec toute confiance et docilité se trouve avoir acceptées est adaptées aux principales bases de sa theorie. Pour moi, je ne partage pas l'idee qui a seduit tant de personnes, et je pense tout au contraire que l'importation n'a pas été aussi heureuse et aussi utile qu'on ne la croit généralement."—*Geoffroy St Hilaire, quoted by Boué.*

the phenomena of nature, would perhaps be too rash. Our ignorance is such, that direct comparison of observed effects of known causes by experiment, and the observed effects of *unknown* causes in nature, can alone lead us to well-founded conclusions. Yet geologists scarcely ever think of such an appeal to experiment. Excepting those of Sir James Hall and Mr Gregory Watt, we can hardly quote an example.*

I have perhaps said enough to shew why I conceive that geology ought *sometimes* to be treated as a branch of natural philosophy; and in the hope of contributing something, however imperfect, towards what I consider the neglected part of the science, I have from time to time made collections in connection with some of the leading problems, for the solution of which geology must be ultimately indebted to natural philosophy. One of these is the action of heat upon rocks, and another is the mechanical power of elevation which igneous rocks have occasionally exercised, and its consequences. Without pretending to have devoted much special time to the inquiry, I have directed my journeys so as to obtain the means of inspecting at least some of the most important sites of ascertained or suspected geological convulsion: nor can any merely local convulsions be considered of much value; it is by the comparison of many points that we are enabled to draw, with some degree of probability, the general (though as yet almost empirical) conclusions which geology, in its present state, admits of. With this view I have examined the trap-rocks of our own island, the ophites of the Pyrenees, and the serpentines of Anglesea and the Lizard,—the porphyries of Northern Italy, the granite veins of Mount's Bay and Glen Tilt,—the ancient volcanos of Auvergne, the Eifel, the Siebengebirge, and of Rome,—and the modern volcano of Vesuvius. Without proposing anything like an abstract of conclusions derived from these various sources, I may be permitted to offer some considerations of a general nature on two points, to which,

* I must not be supposed to have lost sight of the experiments of Dr Turner and Mr Harcourt in this country, and of Mitscherlich and Becquerel (whose very important results have by no means attained the celebrity which they seem to deserve) on the Continent. These we do not owe to geologists, but to chemists and *physiciens.*

in a recent excursion to Auvergne, my attention was more particularly directed: 1. The igneous character of the trap rocks; 2. The theory of elevation of Von Buch and Elie de Beaumont.

I. There is no better proof of the necessity of a pretty extensive induction in great geological questions than the alternate light and obscurity which the comparison of a new country with one little known affords. Even the most skilful analysis of one country or mountain group cannot make up for the extension of ideas which the examination of several affords. For example, were we to draw our conclusions as to the nature of granite from the single study of the British isles, of the Alps, or of the Pyrenees singly, we should certainly arrive at very considerably different results. Still more so if we have to compare the sedimentary deposits of those three very different localities, as the lias of the Bernese Oberland with that of Bath, or the mineralogical character of chalk on the coast of Kent and at the summit of the Mont Perdu. It was not the want of talent, but the want of extended observation, which led Werner into his greatest errors, and it is to the same cause that many of his followers have been led to support some of his least tenable opinions, as those respecting the origin of the trap rocks, simply by confining their attention to a single district. No one will deny that the origin of our Scotch trap rocks might be yet involved in great doubt, had we not indubitable specimens of igneous action wherewith to compare them: could we not compare the analyses of modern lava and basalt,—their mineralogical structure, their dykes and veins, and their enclosed minerals. On all these points a direct comparison was obtained, and to those who had seen and examined both classes of phenomena, the conclusion was irresistible. Still it required a certain effort of abstraction to realise, amidst our trap formations, the recollections of the torrefied flanks of Vesuvius or Etna, to trace the analogy, and to pronounce upon the *general* identity of origin. In Auvergne this effort of abstraction was spared. Both phenomena are side by side, nor perhaps has the most determined Neptunist ever retired from an examination of these Phlegræan fields, without feeling his former faith shaken; whilst the majority have read at once the recantation of their heresy, amidst the scarcely slumbering fires of Clermont and Le Puy. (To this

Charpentier and D'Aubuisson bear a candid testimony in their own persons.)

The effects of heat are of two kinds, *producing* rocks (by which we mean being the direct agent of their protrusion,) and *altering* them. Each of these, but especially the latter, affords a field of inquiry of the most interesting character, and often fills up a chain of evidence as solid and as convincing as any even in mathematical physics. Of both of these Auvergne contains examples, though perhaps the production is more frequent than the change of character. To go over what has been already so well enforced, the general deductions from the phenomena of Auvergne, is by no means my purpose; I would rather allude to one or two individual points which may pretend to something of originality; and first, I would remark, that the striking conviction which, as we have said, a sight of this country has seldom failed to impart as to the origin of trap (undoubtedly one of the most fundamental positions in geology), we attribute quite as much to the topographical condensation of the evidence as to its superior force. We discover in Auvergne vast *plateaux* of basalt, of the origin of which we know nearly or quite as little as of that of Salisbury Crags, near Edinburgh, or the Whin Sill of the north of England. In close apposition to these, we have flows of lava, as distinct, as rugged, and as obviously igneous as those of Vesuvius;—following the course of the actual valleys issuing from scorified craters, rolling their tide of desolation into the midst of fertility,—in short, presenting as complete a picture of volcanic energy, even in its most frightful form, as can any where be seen, with the single exception that all is cold and hard. Though, as later writers have pointed out,* there may be an insensible passage from the basalts of these platforms to the more modern streams, we seldom fail to discover a general reference to an older or a newer class, one anterior, the other posterior, to the *existing* condition of the valleys. The basaltic *plateaux* we cannot trace to their source, the *coulées* of lava we can; the evidence of a common origin is perhaps not greater than that which a trap rock country, and a modern volcanic country compared would afford, only that you have no separation of the two in point of space;—you may stand upon basalt and break lava; you may compare at one instant the

* Scrope on the Volcanos of Central France. Lyell's Geology, vol. iii.

configuration of both, the constitution of both, the included minerals of both.

The next remark I would make is an important one, and refers to the *alteration* of rocks. The group of volcanic rocks round Clermont forming what may be called the Monts Dôme, rise through an elevated table-land of granite, the flanks of which are abundantly covered by sedimentary deposits of the tertiary epoch. Associated with the latter are many of the basalts. Thus in the hill of Gergovia, we have a section of this kind, reckoning from below; 1. Tertiary limestone; 2. Basalt with nodules, and containing crystals of chalcedony; 3. Tertiary limestone, sometimes oolitic; 4. Basalt; 5. "Calcareous peperino" (Scrope), a sort of volcanic tufa containing veins of compact felspar; 6. Basalt at the summit. From the middle bed of basalt shoot forth veins into the superincumbent calcareous tufaceous matter, which, in the places I examined, presents no decided appearance of alteration by heat. M. Dufrenoy* thinks otherwise, and infers the posteriority of the contained basalt to the containing strata, which I am disposed to think the phenomena of veins just alluded to would force us to admit, though Mr Scrope † draws an opposite conclusion. This difference of opinion is not a little instructive; and the exact parallelism in that, as in other respects, between these phenomena and those of Scotland, as well as the almost total absence of apparent torrefaction at Gergovia, serve admirably to connect the most unequivocal volcanic appearances with those of a more dubious character.

Mineral Character.—The mineralogical character of the rocks of Auvergne is one of their most interesting features. Dr Daubeny ‡ has particularly alluded to the perfectly basaltic character of the recent *coulées* of Gravenoire, near Clermont. One of these, in particular, on the side next Montaudoux, presents most brilliant crystals of olivine and augite, such as our trap rocks associated with the older formations exhibit. The amygdaloids and compact felspars might every one be matched in Scotland, as well as many of the trachytes of Mont Dor and

* Memoires pour servir à la Géologie de la France, tom. i.

† Central France, p. 92.—Mr Lyell agrees with M. Dufrenoy, Geology, iii. p. 259.

‡ Letters on the Geology of Auvergne, Ed. Phil. Journal, 1820–21.

the Cantal, and the trachytic conglomerates of the latter, which much resemble those of Arthur's Seat. Where the trachytes are more perfectly crystallized, containing glassy felspar in six-sided tables, as at the Puy de Sancy, and in four-sided prisms, as at the Capucin, both in the Mont Dor, we have a perfect antitype in the rock of the Drachenfels; whilst the porous but durable lavas of the *coulées* of Pariou and the Puy de Nugère (the latter forming the well known Pierre de Volvic) very closely resemble the millstone lava of the environs of Andernach. Of the rock called Domite I shall say nothing, because the only proof of the prevailing hypothesis, that it is altered granite ejected like hasty-pudding (to use the phrase of Mr Scrope), is yet untried: we may hope for something like a result from Mr Harcourt's experiments at the Low Moor iron-works. The phonolite of the Puy de Griou, in the Cantal, is identical with that of Blackford Hill, near Edinburgh. Again, the lavas and scoriæ of the Puy de Gravenoire and the lava of the Puy de Côme, forming the vast and sterile Cheire de l'Aumône, offer the most perfect analogies to the productions of Vesuvius and other modern volcanos. The felspathose rock of the Mont Dor containing sulphur and alum, may be classed with that of the Solfatara, and the tufas of every part of Auvergne may find parallels at Ischia, at Pausilipo, and in the Campagna di Roma.

Structure.—Nor does the columnar form occur under less remarkable circumstances. We find basaltic pillars of exquisite symmetry, straight and bent, at Murat, in the Cantal, and in a thousand other sites, which may rank with those of Staffa and the Giant's Causeway. Whilst, what is far more remarkable, we discover the most obvious traces of columnar structure in the relatively modern lavas, as in the very remarkable locality of Pechadoire, near Pont Gibaud, and in the still more singular displays of the Vivarais, a country which I have not visited.

There is another circumstance respecting the structure of the undoubtedly igneous rocks of Mont Dor well worthy of remark. The *slaty* structure, so far from being opposed to the crystalline character of the rock, is found frequently combined with it, and that in the three leading characters of rock, trachyte, basalt, and phonolite. The columns of trachyte behind the baths of Mont Dor have so excessively slaty a structure, that it is with great difficulty that we can procure fresh surfaces even in hand specimens. As to basalt, its tabular form is too frequent in Auvergne not to be remarked. But I particularly noticed vast slabs of it in the valley above St Bonnet, one of the immense external indentations of the group of Mont Dor.

Two extremely picturesque eminences, not very far from the last mentioned valley, called La Thuilliere and La Roche Sanadoire, are composed of what has been called by almost all writers on Auvergne, *phonolite*, though it does not coincide precisely with my idea of that rock. It is composed, however, apparently altogether of felspar. In the case of La Thuilliere, the very polygonal arrangement, striking when viewed at a distance, merges into a most completely slaty structure when closely examined, the rock forming vast tabular masses which resemble strata. To these cases we might add that of the modern lava of the Puy Nugère, forming the *coulée* of Volvic, the stone of which may be cleft into tabular masses parallel to the bed in which the lava flowed, and these again through vertical planes coinciding in direction with the lava stream. These remarks find an important application in considering the tabular structure of rocks of dubious origin, such as granite, which may be even extensively slaty (as it rarely, and though sometimes is, as on the south side of the Canigou in Roussillon, near Prats-de-Mollo), without leading us to the conclusion of an aqueous deposition.

Passage of rocks into one another.—I have already observed, that, far from finding geological theories too simple, I imagine that they are rarely complicated in any proportion to the actual complication of nature. Especially the changes which occur *after* their deposition, and which may be oftentimes repeated, may serve to render inextricable the apparent disorder of superposition.

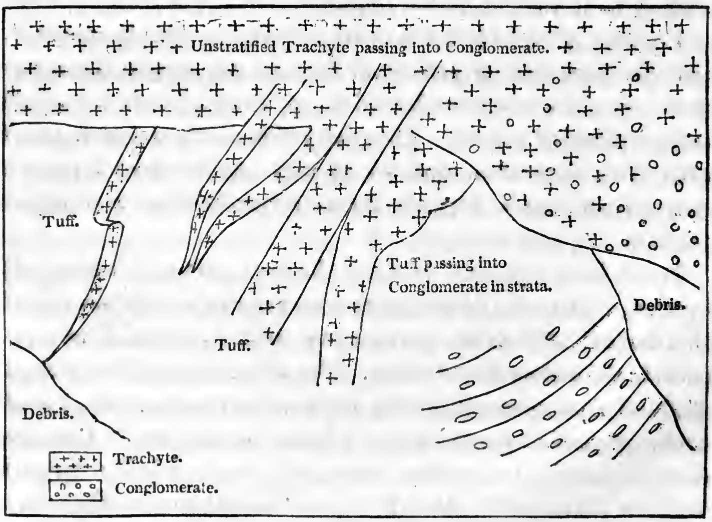
It has been a general (and for the most part a well founded) opinion, that trachytic eruptions were prior to basaltic ones; and that the existence of these rocks may refer to very different periods in the history of our globe. The universality of this view, a study of Auvergne will hardly allow us to acquiesce in. I shall confine myself at present to one curious section, which I was induced especially to examine from a reference to it in Mr Scrope's work on Auvergne * where he has called attention to the exist-

* Note p. 109.

ence of basalt underlying trachyte, a fact which he says has been denied by M. Beudant and other French geologists. That there is a basalt there can be no doubt, and I was hence led to trace the very curious relations of these and other strata at the same point. This section is a natural one formed by a rivulet descending in a cascade near the baths of Mont Dor. The general section is a very simple one, in a descending order:—

1. Trachyte.
2. Volcanic Tufa.
3. Trachyte.
4. Volcanic Tufa.
5. Basalt passing into Trachyte.
6. Volcanic Tufa.

Of bed No. 5. it is to be remarked, that though one portion of it be truly basaltic, black, compact, and heavy, it gradually passes into a distinct trachyte by imperceptible gradations, a circumstance which may possibly have escaped Mr Scrope. First the augite gradually disappears; the rock becomes less dark and much less heavy; then crystals of glassy felspar commence, and the transformation is complete. It may be observed, that the basalt in question does not appear to contain olivine, but it is a perfectly well characterized basalt in other respects.



It is in the 6th or lowest bed that we have the accompanying section. Beneath the general overlying trachyte, we have a portion of the same unstratified matter passing into a trachytic conglomerate (a very common rock of this country). To the right of the figure we have this same conglomerate becoming stratified, and passing undistinguishably into common paste-like volcanic tufa; and in this very tufa we have veins passing from the trachyte above, and even appearing to cut both one and the other. We shall find ourselves reasoning in a circle if we attempt to ascribe superior antiquity to any one of these substances. I am disposed to account for rather than explain such phenomena, by changes induced in structure, and particularly a *blending* of structure subsequent to the deposition of rocks by the action of heat.

It is worthy of observation, that this bed of tufa contains portions of wood in a high state of preservation.

Relative age of rocks.—The conclusion of the anteriority of basalt to trachyte, from the section just given, and from others in the neighbourhood, as near Quereilh, would not be quite legitimate, even supposing that the basalt were not found to blend with trachyte. For we have seen that, in the case of Gergovia, we must necessarily admit the insinuation of basalt posterior to the date of the strata it traverses, and we have also a very good specimen of this in the Mont Dor, on the great road from the baths to Murat. But there is one remarkable fact which seems necessarily to lead to this conclusion. In the Cantal between Aurillac and Vic-en-Carladez, I have observed masses of basalt inclosed in a trachytic conglomerate, a vast puddingstone formation with a base of trachyte, passing into and interstratified with that rock.

The common opinion is, that the *trachytic* eruptions belong to the oldest epoch, that the *basaltic* were subsequent to them, and the *phonolitic* still later. Certainly there are many facts seen in the Mont Dor and Cantal which confirm such an idea, yet probably we must not consider this as universally true, owing to a variety of facts, one of which I have just stated. That the trachytic eruptions were very frequently renewed seems almost undeniable, from the dykes of more compact trachyte which traverse the older rocks, and which add so much to the sublimity of the scenery (although it is not on a very great scale) in the Gorge d'Enfer in the Mont Dor. I think we are bound to conclude that the rocks of this country have undergone great changes in the constitution, structure, and mutual relation, by causes which have acted subsequently to the convulsions which brought them into juxtaposition. Such changes are what theory can take little account of, though they may be most numerous and most important; and this is one of the cases in which the processes of nature seem more involved than we can ever venture to render our theories, which, from our limited sources of induction, are perhaps universally *too simple*, though we dare not render them more complicated without deserting altogether the track of logical analogies, and running the risk of shipwreck amidst a chaos of possible contingencies.

The relations of the igneous to the stratified rocks are also very obscure; we have mentioned the instance of Gergovia, and we might extend our remarks to the fresh-water formations of the Cantal. A sketch of these has been given by Messrs Lyell and Murchison,* and also by M. Dufrenoy † and others. I examined the curious sections occurring between Aurillac and Tiesac, particularly that at the latter place, where I spent two days. But I own that the dynamical conclusions from these formations seem extremely difficult. We are forced to consider the limestone as anterior in point of date to the tufas and other volcanic rocks, yet we frequently find masses of the latter contained in the former.

Perhaps the origin of these tufas is the most difficult, and one of the most important problems presented by the Cantal and Mont Dor. I shall hazard a few remarks respecting them, in concluding with a notice of the theory of elevation.

II. *Theory of Elevation-Craters.*—The hypothesis proposed long ago by Von Buch, to account for certain appearances in volcanic countries, (as in the Isle of Palma, one of the Canary Islands), amounts to this; that, in some cases, the conical form of volcanic mountains, instead of being assumed in virtue

* Annales des Sciences Naturelles, tom. xviii.

† Annales des Mines, 2de Serie, tom. vii.

of successively imposed coats of lava trickling down in an inclined position, was assumed subsequently to the horizontal deposition of those beds, by a thrust upwards from below; hence such an external configuration may be independent of the volcanic nature of the materials. Cavities so formed are what Von Buch terms Craters of Elevation. The recently published ingenious views of M. Elie de Beaumont, on the epochs and modes of elevation of mountain chains, includes the theory of Von Buch as a particular case, namely, an elevation at a *point* producing a *group*, instead of elevation along a *line* producing a *chain* of mountains. Hence the groups of Mont Dor and the Cantal have excited the particular attention of that very distinguished geologist, who, (with M. Dufrenoy), has published a paper * expressly on the subject, which was my constant companion whilst examining that country. The great excitement which has recently prevailed amongst geologists, especially on the Continent, about this matter, led me to examine the evidences with the more attention, though my examination was curtailed extremely by the constant annoyance of bad weather, which I experienced in September last.

What is brought forward as the chief novelty in the general views of MM. Elie de Beaumont and Dufrenoy, is the actual calculation of the spaces or fissures which should be left in the slope of the cone (or rather pyramid of many sides). That such spaces must occur is obvious, from the circumstance that the surface of a cone is larger than its base, and that the materials, which when united occupied only the area of the base, are to be elevated towards a common vertex. Looking upon the question mathematically, nothing can be clearer or more satisfactory than such a statement; and perhaps there is room for regret, that, in the paper alluded to, a parade of analytical research appears, which adds neither to the conclusiveness nor to the elegance of the reasonings. The effect, however, of this novelty, cannot fail to be advantageous to geological science. It will give to geologists, in *some* cases, the means of reducing to definite trial vague trains of reasoning, and it will generally aid in fortifying the prosecutors of this speculative science with

* Annales des Mines, 2de Serie, tom. iii.

the sound principles which mathematical philosophy inculcates. M. Elie de Beaumont is one of the few living geologists who have considered a study of general physics and mechanical philosophy, as a fit commencement for the prosecution of a subject which should draw largely on both, and that in their more difficult departments. It is almost exclusively with the more intractable problems of the higher mechanics that geology has to deal; such as the equilibrium of free heterogeneous fluids,—the conditions of matter under circumstances which never can exist at the surface of our globe,—the analytical theory of heat,—the yet more imperfect rudiments of electro-magnetism,—the action of forces upon plastic and semi-rigid materials,—the impulsive force of fluids, and their motions at great depths;—these are but a few of the thorny paths of inquiry in which the speculative geologist, who wishes to take sound philosophy along with him, finds himself engaged. We should, therefore, congratulate the science on finding men of enlarged views and great acquirements, like M. Elie de Beaumont, ready to promote a general taste for such pursuits, even although the temptation to overstep a little the legitimate paths of mathematical investigation should at first be too great to be resisted.* It may at least induce a study of the allied sciences, when geologists themselves begin to find the works of their fellow-labourers too abstruse for their perusal.†

That the argument about the conical surface has been carried too far can hardly be doubted. The impossibility of forming numerical estimates in point of fact on the cases in nature, renders the whole rather a display of ingenuity than a substantive benefit to science. The chief conclusions of any importance, namely, that the higher the angle of elevation of the cone, the

* The reasonings of M. Elie de Beaumont have not of course failed to be caricatured. I recollect to have seen in a French provincial journal the obvious truth, that, as we proceed from the apex of the imaginary fissured cone towards its base, the *sum* of the incomplete spaces will diminish, proved by the aid of the differential calculus!

† At the very time that this paper was being read, I had the satisfaction of receiving a very remarkable physico-mathematical paper, by Mr Hopkins of Cambridge, upon what may justly be termed the Dynamics of Geology. It is now published in the *Cambridge Transactions*, and an abstract in the *Philosophical Magazine.*

larger the fissures; and the greater the diameter of the crater the smaller the *sum* of the interrupted spaces of its circumference, are obvious upon mere statement; and since the comparisons with nature do not admit of more precision, any formal calculation goes for very little.

Yet there are very strong grounds for adopting the conclusions of MM. Elie de Beaumont and Dufrenoy, nor do I find it easy to conceive on what grounds the earnest hostility manifested to their views has been maintained. Passing over most of the arguments which have been urged by themselves and others, I will point out one or two which have struck myself with peculiar force, as regards the Cantal and Mont Dor, and especially the former.

1. The Cantal presents a crater-like basin, in which rise phonolitic cones, and from which radiate numerous valleys, like deep scars, so similar to the divergent cracks of a piece of ice or of a *starred* piece of glass (*etoilé*), as to lead us to imagine that the excellent map given by MM. Elie de Beaumont and Dufrenoy had been drawn under the inspiration of theoretical views, did we not find it fully confirmed by the unbiassed authority of the accurate Cassini. An examination of the valleys, too, would lead us to the conclusion that, in general, they are valleys of disruption, not of érosion; but admitting, as I do, after paying long continued and earnest attention to this important and difficult question in geology, (the origin of valleys), that there seems no universal and unimpeachable criterion between the two, I would not now press that assertion. There is, however, this most striking in these valleys, (I speak with most confidence of the valley of the Cer in the Cantal, which I examined with most care, and the same may be added of some valleys of the Mont Dor), that they often rather contract than increase in width, as we retire from the centre of the group, at the same time that they are less profoundly excavated, till at last they merge in the slightly swelling surface, exactly as the theory of elevation would prescribe. They are frequently bounded on either side by mural precipices, and of lateral valleys *there are almost none.*

2. M. Elie de Beaumont has justly pointed out the regularity of the succession of layers or coats which occur over the conical

surface of the mountain groups: he has likewise observed that, as proper lava streams are always narrow, such an arrangement could not occur in all directions. But there is almost a physical impossibility to such an arrangement, namely, that, unless the crater had an edge or lip mathematically level all round, a universal effusion of the kind which I have alluded to could not take place; some lateral deficiency must have drawn off the rising fluid, unless we conceive a supply so boundless as to resemble that of a very copious spring rising up under hydrostatic pressure,—a phenomenon far less reconcilable with those now observed than the theory of Von Buch.

3. The enormous disproportion of the valleys to the drainage of a single conical mountain next strikes us; and this is equally applicable to the Mont Dor. It is hardly credible that such and so numerous valleys could be the mere work of erosion.*

4. This and similar difficulties seem to have been so strongly felt, that many or most writers have taken refuge in the theory of earthquakes having produced these rents;—this, however, M. Elie de Beaumont justly observes, is a begging of the question, since separation by fracture in this case implies elevation above the horizontal plane. [The contraction from cooling might have been mentioned, but that is wholly inadequate.] But there seems even a stronger objection, which brings those who entertain such views directly under the ranks of the elevation theory. Such earthquakes could not have accompanied or occurred between the deposition of these successive layers; for, had this been so, we should have wanted the perfect coincidence between five, six, or seven successive beds exhibited on each side of these valleys, and which seem perfectly to correspond. Indeed no overflow could have taken place after the formation of a single such valley, as the lava must have followed the direction of the valley, as we see constantly amidst the Monts Dôme. Consequently the earthquakes could not have taken place until the effusions had completely ceased, and hence correspond in

* In a memoir published since this paper was read, M. Elie de Beaumont puts this difficulty in a forcible, and, it seems to me, not an exaggerated light:—"On ne pourrait concevoir la réunion de l'une et l'autre circonstances, la divergence des vallées jointe à l'existence des barrages, qu'en imaginant *poëtiquement*, des courans diluviens descendant du ciel en ligne perpendiculaire!"—Mémoires pour servir, &c. tom. iii. p. 293. 1836.

point of time, as well as in effect, to those which the elevation theory assumes.

5. I have already said that the theory of the tufas and conglomerates of this country appears so difficult, that I should be unwilling to found theoretical conclusions upon them. Their stratification, such as, in some cases, I firmly believe to exist, affords, however, an evidence too important to be overlooked. The section formed by the river Cer near Tiesac (in the Cantal) is one of the best instances. The following memorandum was made at the time: "I crossed the valley to examine a section of the constituents of the mountain, exposed by a very extensive and picturesque landslip. Above the conglomerate or tuff which forms the bottom of the valley, is a bed of compact trachyte, very like the claystone porphyry of the Pentland Hills. It has a tendency to columnar structure transverse to the direction of the bed, and breaks with the hammer also in that direction, though the texture of the rock indicated by the crystals is parallel to the bed. Above this bed is one of tufaceous conglomerate, like those of Naples, and strongly appearing as if stratified by water; then another bed of compact trachyte; then coarse conglomerate; and the whole is capped by basalt (which, however, I did not ascend high enough to meet with). The lowest bed is of great thickness. It is extensively exposed in the chasm below Tiesac, through which the river runs. It is a matter of question whether this chasm is of erosion, or a crack." Now the whole of these beds dip at an angle of 3° 42′, at a mean, according to M. Elie de Beaumont, though in many places considerably more, from the vertex of the cone. If the third bed from the bottom which is conformably stratified really indicate aqueous deposition, the conclusion is nearly irresistible that such deposition took place whilst the bed of trachyte below was horizontal; and hence, by the reasoning of the last article, the beds above must also have been subsequently horizontally deposited.

The importance of the question I lay before future observers, nor shall I take upon myself to make any general statement relative to these tuffs, considering the short and imperfect examination which I gave them, and the difficulties which at the time I felt upon the subject. My observations in the valley of the Cer were, however, carefully made, and were entirely confirmed afterwards in the Mont Dor. The lowest tufaceous bed in the section at the cascade of Mont Dor already given, which contains recent wood, indicates, I think, a fluviatile or lacustrine origin. The latter is rendered more probable (and it is the most conformable to the views just stated) from the very remarkable tufaceous deposit in a neighbouring valley, above the spot called La Verniere, where a pumiceous conglomerate occurs interstratified with the most delicately deposited beds of fine white clay, which must evidently have had their origin in still water. This does not accord with the idea of Mr Scrope in his beautifully illustrated work on this district, that such beds were formed from the debris accumulated by torrents.

6. I must, however, observe, that the origin of these tufaceous beds seems to be distinct from that of the great mass of conglomerate which occupies the base of almost all these valleys to an immense depth. It is a proper rock, and is, as MM. Elie de Beaumont and Dufrénoy observe, to be distinguished carefully from the mud eruptions by which Herculaneum was covered. It has a trachytic basis, and the inclosed fragments are generally trachyte; so that it is probably the result of some internal interrupted process, not a recomposed rock in the proper sense of the words. This is entirely confirmed by the undoubted mechanical energy it has exerted, as, for example, in disturbing, elevating, and imbedding the tertiary stratified deposits with which it came in contact, as described by Messrs Lyell and Murchison*, and others. I own that the strong impression made upon my mind (notwithstanding its *prima facie* improbability) by the Cantal was, that this conglomerate had been the agent of elevation of the whole group. The evidence seems to strengthen upon reflection, nor can I easily account otherwise for the *entire* occupation of the bottom of the valleys by this conglomerate, which must have been itself fissured had it been merely passive. At all events, the disturbance of the stratified rocks by this material must be considered as entirely conformable to the elevation theory, and as indicating beyond any doubt a certain amount of convulsion.

7. Upon the whole, it seems to me that the evidence of earthquakes subsequent to the deposition (in whatever way) of the

* Annales des Science Nat. xviii. 172.

Cantal and Mont Dor, is a fact so indisputable as to render the argument about craters of elevation to a great extent merely verbal. It is impossible to look upon either of these districts, and especially the latter, without perceiving incontrovertible evidence of extensive convulsions. These convulsions could not have taken place without elevation, and elevation is a kind of action with which all geologists are familiar, and which has been repeatedly appealed to by some of the warmest opponents of the elevation craters. If this be an assumption then, it is a reasonable one; and the only other postulate of that theory, namely, the extensive horizontal beds of volcanic matter, is so entirely conformable to the facts observed in the comparatively level ground adjoining these very mountain groups, as in fact to be viewed as no assumption at all, or at least as possessed of equal probability with any other opinion as to its primitive position. I allude to the immense *plateaux* of basalt in the level country of Auvergne, so faithfully and admirably delineated in Mr Scrope's work. There seems, therefore, so much of probability, and so little of extravagance, in the theory, that we wonder how it could possibly have given rise to such animated opposition.

At the same time, looking on the subject with the impartiality of a spectator,—a mere straggler into the domains of geology, it seems perhaps not less unreasonable to expect that all cases of such actions should yield themselves with equal facility to the support of so simple a hypothesis. Whilst we see in the Cantal an example of elevation of great unity and simplicity, the Mont Dor, though bearing no less obvious traces of upheaving agency, seems to bid defiance to any thing like regular analysis. To attempt to calculate, on the principles of M. Elie de Beaumont, the magnitude of the fissured spaces produced by three simultaneous (or consecutive) elevations at as many points within a radius of three or four miles, appears unwarrantably bold. Nor are the rugged features of this picturesque country translatable into language sufficiently definite to authorize or to disprove such a conclusion. I own that an unbiassed and most attentive survey of the bearings of the group from the Pic de Sancy (the highest ground in Central France, 1887 metres above the sea) did not lead me to this conclusion,—I mean that three such elevations were *necessary* and *sufficient* to explain the observed phenomena. Many circumstances lead me to conclude that the changes undergone by this district have been vastly more complicated, more numerous, and more prolonged; and that it would be unreasonable to expect that such phenomena should always be of so simple and so elementary a description.

Some facts inferring many successive changes in the materials constituting the Mont Dor have been already adverted to. Others it would be easy to cite. The numerous dykes of trachyte and basalt are especially remarkable, and if accompanying or in any part posterior to the general elevation, must necessarily have altered most materially the configuration of the ground, and the relations of its constituents. That the three points or centres of elevation indicated by MM. Elie de Beaumont and Dufrénoy, viz. the Puy de Sancy, the Puy de la Tache, and the Roche Sanadoire, were really points of disturbance and elevation, seems scarcely to admit of doubt. The last, indeed, speaks for itself; the spectator, placed on one of the magnificent phonolitic masses of La Thuilliere or La Roche Sanadoire, finds himself nearly surrounded by mural escarpments. But I contend that there must probably have been many more centres of effervescence, whose successive outbreaks may have materially changed the original configuration of the district. The terminations of many of the valleys differ from what the elevation theory would point out, neither expanding into craters, nor vanishing in mere rents, but having frequently rounded mural terminations, which may be compared (as I rather think M. Elie de Beaumont himself has done) to the *oules* or theatre-like terminations of many of the Pyrenean valleys. One of these partial disturbances may, I conceive, be seen in a very remarkable crater-like cavity close to the Roc Crusau; nor does it appear that the disturbing energy was exhausted until it had spent itself by the comparatively modern orifices of the Puy de Tartaret, the lakes Pavin, Servieres, and others.

EDINBURGH, *5th December* 1835.

Questions for Solution relating to Meteorology, Hydrography, and the Art of Navigation. By M. Arago. (Continued from Vol. XX. p. 405.)

Mean height of the Barometer.—A few years ago a positive denial would have been given to the assertion, that there is any permanent difference between the barometrical heights corresponding in the different regions of the globe to the level of the sea. At present such differences are regarded as not only possible but even probable. The officers of the *Bonite* ought therefore to preserve their barometer with the most scrupulous care, in a fit state to make observations by which every variation may be registered for the purpose of comparison. Notice should never fail to be made of the exact height of the *bulb* of the barometer above the level of the sea.

Of the influence of different winds on the heights of the Barometer.—As soon after the memorable discovery of Toricelli as meteorologists directed their attention to the observation of the barometer, they perceived that *in general* certain winds produced a rapid ascent of the mercurial column, while opposite winds produced a contrary effect in a manner equally decided. The difficulty was to determine the *numerical value* of these influences. It was necessary, in order to evade entirely passing and fortuitous influence, and obtain the true measure of permanent causes, to operate in great numbers; to form an estimate from a long series of good observations made in the same locality; to group the winds by their precise directions; and to deduct the means of effects purely thermometrical.

Burckhardt undertook this labour, availing himself of twenty-seven years of observations which Messier had made at Paris from 1773 to 1801. If we designate by the letter H. the mean height of the barometer at Paris, that is to say, the height determined by the average of all the observations, the means corresponding to the different winds, according to Burckhardt's calculations, will be as follows:—

		mm.
South Wind,	H. less	3, 1
South-west,	—	2, 9
West,	—	0, 4
North-west,	more	1, 3
North,	—	2, 0
North-east,	—	2, 6
East,	—	1, 1
South-east,	—	0, 8

It will be seen, from the inspection of this table, that the direction of the wind occasions a variation in the state of the barometer at Paris of 3^{mm}, 1 above the mean, and of 2^{mm}, 6 below it, forming a total variation of 5^{mm}, 7; and that the opposite winds, combined two by two, give a mean height which, in extreme cases, scarcely differs by half a millimetre from the mean of all the observations.

M. Bouvard has presented to the academy the results of an investigation analogous to that of Burckhardt; it is founded on the observations of the barometer made at the observatory of Paris from 1816 to 1831, and leads in general to the same consequences. By assigning to the letter H. the signification which we gave it in the preceding table, we shall have the following barometrical heights, corresponding to the different directions of the winds: —

		mm.	
South wind,	H. less	3, 7	(2944 observ.)
South-west,	—	3 0	(2847 ——)
West,	—	0, 8	(3402 ——)
North-west,	more	2, 0	(1533 ——)
North,	—	3, 2	(2140 ——)
North-east,	—	3, 2	(1390 ——)
East,	—	1, 7	(1248 ——)
South-east,	—	1, 7	(890 ——)

The daily observations at nine o'clock in the morning, at mid-day, and at three in the afternoon, have all concurred in the formation of these numbers. Very nearly the same results will be obtained by employing only the maxima heights of nine o'clock, and the minima heights of three o'clock.

In this instance, as well as in the table of Burckhardt, half the sums of the heights corresponding to the opposite winds, are nearly equal to H, that is to say to the total mean. The highest mean effect of the wind is 6^{mm}, 9, which surpasses the result afforded by the observations of Messier by 1^{mm}, 2.

Both these tables tend to establish a fact with which meteorologists cannot be too strongly impressed, that in order to obtain in our climates the mean height of the barometer, it is indispensable to admit into the calculation an equal number of observations, corresponding to the winds from opposite directions.

The tables which we have just transcribed, suggest many scientific questions; they lead us to inquire how this influence of winds on the weight of the atmosphere, varies with the position of places, with their greater or less distance from the sea, with their latitude, &c. In the mean time, till data sufficiently numerous be obtained to enable us to attempt the solution of these various meteorological problems, I shall here present to the reader the results of two series of very accurate observations, which were communicated to the academy by MM. Schuster and Gambart. The first were made at the School of Artillery and Engineers at Metz, the others at the Observatory of Marseilles.

Observations at Metz, continued for Nine Years.

			mm.
South wind,	H.	less	2, 4
South-west,	H.	...	2, 1
West,	H.	...	0, 6
North-west,	H.	more	0, 3
North,	H.	...	2, 4
North-east,	H.	...	2, 1
East,	H.	...	1, 0
South-east,	H.	...	0, 8

The difference between the extremes is sensibly less than in the observations at Paris. At the same time, it would be premature to draw general conclusions from this fact, which may perhaps be purely accidental.

The following seems more decisive:—

Observations at Marseilles, continued for Five Years.

			mm.
South wind,	H.	more	0, 0
South-west,	H.	...	0, 7
West,	H.	less	0, 5
North-west,	H.	less	0, 9
North,	...	...	...
North-east,	...	...	...
East,	H.	more	0, 2
South-east,	H	...	0, 5

Although this table is incomplete, and founded on observations of only five years' continuance, and although the north and north-east winds are entirely omitted, there results from it no less important a consequence than this,—that if the direction of the winds exercise, at Marseilles, any influence on barometrical heights, that influence is very slight, and ought not always, in the case of winds of similar denominations, to have the same sign as in the north of France. Thus, while at Paris the south-west wind depresses the barometer considerably below the mean, its influence at Marseilles is positive; on the other hand, the north-west wind, which causes a considerable rise in the barometer at Paris, is that which produces the lowest depression at Marseilles.

When observations such as these have been made at many different places, they will probably put meteorologists in a condition to explain a phenomenon which has hitherto baffled all their efforts.

Of the Diurnal Variations of the Barometer.—Numerous memoirs have been published on the *diurnal variation of the barometer.* This phenomenon has been studied from the equator to the regions in the vicinity of the pole, at the level of the sea, on the immense plateaus of America, on the insulated summits of the highest mountains, and the cause has notwithstanding remained in obscurity. It is still necessary, therefore, to multiply observations on the subject. In our climates, the vicinity of the sea appears to manifest itself by a sensible diminution in the extent of the diurnal oscillation; Does the same thing take place between the tropics?

Observations on Rain.—Navigators occasionally speak of rains which fall on their vessels while traversing the equinoctial regions, in terms which lead us to suppose that it rains much more abundantly at sea than on land. But this subject still remains in the domain of mere conjecture; seldom has the trouble been taken to procure exact measurements. These measurements, however, are by no means difficult. Captain Tuckey, for example, made many during his unfortunate expedition to the River Zaïre or Congo. We know that the *Bonite* will be provided with a small udometer. It seems to us, therefore, expedient to recommend its commander to cause it to be placed on the stern of the vessel, in such a situation that it can neither receive the rain collected by the sails, nor that which falls from the cordage.

Navigators will add greatly to the interest of these observations, if they determine at the same time the temperature of the rain, and the height from which it falls.

In order to obtain the temperature of the rain with some degree of accuracy, it is necessary that the mass of the water should be considerable, relatively to the size of the vessel which contains it. A metal udometer will not answer for this purpose. It would be infinitely preferable to take a large funnel of some light stuff, very dense in the texture, and to receive the water which runs from the under side of it in a glass of small dimensions, containing a small thermometer. So much for the temperature. The elevation of the clouds in which the rain is formed cannot be determined but during the time of the storm; then, the number of seconds which elapse between the appearance of the flash and the arrival of the sound, multiplied by 337 metres —the degree of rapidity with which sound is propagated—gives the length of the hypothenuse of a right-angled triangle, whose vertical side is precisely the height required. This height may be calculated, if by means of a reflecting instrument we estimate the angle formed with the horizon by the line which, parting from the eye of the observer, terminates in that quarter of the cloud where the lightning first shewed itself.

Let us suppose, for an instant, that rain falls on the vessel whose temperature is below that which the clouds must possess, judging from their height and the known rapidity of the decrease of atmospheric heat; every one will understand the place which such a fact would occupy in meteorology.

Let us suppose, on the other hand, that during a day of hail (for hail sometimes falls in the open sea), the same system of observations had proved that the hailstones were formed in a region where the temperature of the atmosphere was higher than the point at which water congeals, and science would thus be enriched with a valuable result to which every future theory of hail must necessarily be accommodated.

We could adduce many other considerations to demonstrate the utility of the observations we have proposed; but the two preceding must suffice.

Rain in a perfectly Clear Sky.—There are phenomena of an extraordinary description, on which science possesses few observations; for this reason, that those who have had the privilege to witness them, avoid speaking of them from an apprehension that they might be regarded as undiscerning visionaries. In the number of these phenomena we may rank certain rains of the equinoctial regions.

Sometimes *it rains* between the tropics when the atmosphere is perfectly pure, and the sky of the most beautiful azure! The drops are not very thick, but they are of larger size than the rain-drops in our climates. The fact is certain; we are assured by M. Humboldt that he has observed the occurrence in the interior of countries, and by Captain Beechey that he has witnessed it in the open sea. With regard to the circumstances on which such a singular precipitation of water can depend, we are entirely ignorant of them. In Europe we sometimes see during the day, in calm and clear weather, small crystals of ice falling gently from the air, their size increasing with every particle of humidity they congeal in their passage. Does not this approximation put us in the way of obtaining the desired explanation? Have not the large rain-drops been at first, in the higher regions of the atmosphere, small particles of ice excessively cold; then have become, as they descended, large pieces of ice by means of agglomeration; and when lower still, been melted into large drops of water? It will be readily understood that the only object with which these conjectures are brought forward in this place, is to shew in what point of view the phenomenon may be studied, and to stimulate our young travellers, in particular, to observe carefully if, during these singular rains, the region of the sky from which they fall present any traces of halo. If such traces are perceived, however slight they may be, the existence of crystals of ice in the higher regions of the air will be demonstrated.

In the present day there is scarcely any country where meteorologists are not to be found, but it must be confessed that their observations are usually made at unsuitable hours, and with instruments either inaccurate in themselves, or improperly placed. It does not now appear difficult to deduce the mean temperature of the day from observations made at any hour; thus a meteorological table, whatever may be the hours noted in it, will be possessed of value, by the mere condition that the instruments employed will admit of being compared with the standard barometers and thermometers.

We think it proper to recommend these comparisons to the officers of the *Bonite.* Wherever they can be effected, local meteorological observations will be of value. A collection from the newspapers of countries will often supply what would otherwise be obtained with difficulty.

Magnetism.

Diurnal Variations of the Declination.—Of late years science has been enriched with a considerable number of observations on the diurnal variations of the magnetic needle; but the greater part of these observations have been made either in islands or on the *western sides* of continents. Corresponding observations made on the *eastern sides* would at present be very useful. They would serve, in fact, to submit to an almost decisive proof the greater part of the explanations of this mysterious phenomenon which have been promulgated.

The route of the expedition does not allow us to suppose that the *Bonite* can remain long at points situated between the terrestrial equator and the magnetic equator, such as Fernambouc, Payta, Cape Comorin, and the Pelew Islands. Had it been otherwise, we should have particularly recommended the erection of M. Gambey's beautiful instrument, in a firm position, at a distance from every ferruginous mass, and that the oscillations of the needle should have been attended to with the most scrupulous care.*

* At any rate we shall here present the problem, which observations made at the points mentioned would serve to solve. *In the northern hemisphere*, the point of a horizontal magnetic needle, which *turns towards the north*, moves from the *east* to the *west* from 8¼ o'clock in the morning to 1¼ in the afternoon, and from *west* to *east* from 1¼ o'clock A. M. to the following morning. Our hemisphere cannot be peculiar in this respect; the same effect produced on the north point here must be produced on the south point to the south of the

Inclinations.—In general it will be attended with little advantage to bestow much care on observing the diurnal variation of the horizontal magnetic needle, in places where the expedition is not stationary for a whole week. It is different, however, with the other magnetic elements. Wherever the Bonite stops, though it be only for a few hours, it is desirable to measure, if it can be accomplished, the declination, the inclination, and the intensity.

While attempting to reconcile the observations on the inclination, made at remote periods in different regions of the earth, not far distant from the magnetic equator, it has been ascertained, some years since, that this equator is advancing progressively and entirely from the east to the west. At present it is sup-

equator. Thus, *in the southern hemisphere*, the point of a horizontal magnetic needle which turns *towards the south*, will move from *east* to *west* from 8¼ o'clock in the morning till 1¼ in the afternoon, and from *west* to *east* from 1¼ P. M. till the morning of next day. This observation, at least, accords with reason. But let us compare the simultaneous movements of two needles, when referring them to the same point, *that* namely which is *turned towards the* NORTH. *In the southern hemisphere, the point turned towards the* SOUTH moves from *east* to *west* from 8¼ o'clock in the morning to 1¼ P. M., at the same time the north point of the same needle makes the contrary movement. Thus to put it definitively, *in the southern hemisphere, the point turned towards the* NORTH moves from west to east from 8¼ o'clock A. M. till 1¼ P. M., which is precisely the opposite of the movement made by the same north point, at the same hours, in our hemisphere.

Let us suppose that an observer, starting from Paris, advances towards the equator. As long as he continues in our hemisphere, the *north point* of his needle will make a movement every morning *towards the west*; in the other hemisphere, the *north point* of the same needle will move every morning *towards the east.* It is impossible that this change from a *western movement* to an *eastern movement* can take place in a sudden manner. There must necessarily be, between the zone where the first of these movements was observed and that where the second takes place, a line where, in the morning, the needle will neither move to the east nor to the west, that is to say will remain stationary.

Such a line must exist; but where is it to be found? Is it the magnetic equator, the terrestrial equator, or some curve of equal intensity?

Researches, continued *during many months*, in the places situated between the terrestrial and magnetic equator, such as Fernambouc, Payta, Conception, the Pelew Islands, &c., would certainly lead to the desired solution. But many months of assiduous observation would be requisite; for, notwithstanding the skill of the observer, some brief intermissions in the investigations of Captain Duperrey, undertaken at Conception and Payta, at the request of the Academy, have still left some doubts on the subject.

posed that this movement is accompanied with a change of form. The study of lines of equal inclination, regarded under the same point of view, is attended with equal interest. When all these lines shall have been traced upon the charts, it would be curious to follow them with the eye in all their displacements and changes of curvature; important truths may emanate from such an examination. It will now be understood why we require as many measurements of inclination as can be collected.

The question has been often agitated, whether, in a determinate place, the inclination of the needle would mark exactly the same degree at the surface of the ground, at a great height in the air, and at a great depth in a mine. The absence of uniformity in the chemical composition of the earth, renders the solution of this problem very difficult. If observations are made in a balloon, the measurements are not sufficiently exact. When the observer takes his station on a mountain, he is exposed to local attractions; ferruginous masses may then greatly alter the position of the needle, without there being any thing to make him aware of the fact. The same uncertainty affects observations made in the galleries of mines. Not that it is absolutely impossible to determine the influence of accidental circumstances in each place; but for that purpose it is necessary to have instruments of the most perfect kind; it is necessary to be able to go to a distance, and in all directions, from the station which one has chosen; and, finally, to repeat the experiments much more frequently than a traveller generally has an opportunity of doing. But, however this may be, observations of this kind are worthy of attention. Viewing the whole of them in connection, they will perhaps one day lead to some general result.

With regard to the declination, its immense utility is so much experienced by navigators, that any recommendation on the subject would be superfluous.

Observations on Intensity.—Observations on intensity are not of earlier date than the travels of Entrecasteaux and M. de Humboldt, and yet they have already thrown a bright light on the complicated, but at the same time highly interesting subject, of terrestrial magnetism. Observations of this nature ought, in the highest degree, to attract the attention of the officers of the

Bonite, for at present the theorist is arrested at every step by the want of exact measurements.

The aërial excursions of MM. Biot and Gay Lussac, formerly undertaken under the auspices of the Academy, were in a great measure designed for the examination of the following important question; Has the magnetic force, which, on the surface of the earth, directs the magnetic needle towards the north, exactly the same intensity at every height to which it may be elevated?

The observations of our two associates, those of M. de Humboldt in mountainous countries, as well as the observations of Saussure, of much older date, all seem to concur in shewing, that at the greatest heights which man has yet reached, there is no appreciable decrease in magnetic force.

This conclusion has recently been disputed. Some have remarked, that, in the ascent of M. Gay-Lussac, for example, the thermometer which indicated 87°.8 Fahr. (+ 31 *cent.*) on the ground at the time of departure, sunk as low as 15°.8 Fahr. (+ 9°.0 *cent.*) in the region of the atmosphere where the needle was made to oscillate a second time. But it is now proved that the same needle, occupying the same place, and under the influence of the same force, will oscillate so much the more quickly, according to the lowness of the temperature. Thus, in order to make the observations in the balloon and those on the earth comparable, it would be necessary, on account of the state of the thermometer, to make a certain diminution in the intensity indicated by the higher observations. Without this correction, the needle would appear equally attracted above and below; in spite of appearances, there was therefore a real decrease. This diminution of the magnetic force with the elevation seems likewise to result from the observations made in 1829, on the summit of Mount Elbrouz (in the Caucasus), by M. Kupffer. In this case an exact account was taken of the effects of temperature, and yet diverse irregularities in the inclination threw some doubts on the result.

We conceive, therefore, that the comparison of the magnetic intensity, at the base and at the summit of a mountain, is a matter which ought to be particularly recommended to the officers of the *Bonite*. Mowna-Roa, in the Sandwich Islands, seems to be a place very well adapted for the purpose. The observation may likewise be repeated on the Tacora, if the expedition stop for a few days at Arica.

LUMINOUS METEORS.

On Lightning.—M. Fusinieri has been lately studying the effects of lightning under an entirely new point of view.

According to this philosopher, the electrical sparks issuing from ordinary machines, which we see as they traverse the air, contain brass in a state of fusion and incandescent molecules of zinc, when they emanate from a brass conductor; if the sparks issue from a ball of silver they contain impalpable particles of that metal. In the same way, a globe of gold gives rise to sparks, which contain, during their passage through the air, melted gold, &c., &c.

In the centre of all these sparks the molecules are melted only; but in the circumference, the metallic particles undergo a greater or less degree of combustion, in consequence of their contact with the oxygen of the atmosphere.

When a spark issuing from a globe of gold traverses a silver plate, even of considerable thickness, there is seen on the two surfaces of the plate, at the point where the electric spark entered and emerged, a circular layer of gold, the thickness of which must be very inconsiderable, since the natural volatilization is sufficient to cause it to disappear entirely after a short time. According to M. Fusinieri, these two metallic spots are formed at the expense of the fused gold which the electric spark contains. The deposit on the first face is nothing extraordinary; but, by adopting the explanation of the Italian philosopher for the spot on the opposite surface, we are obliged to admit, that the gold disseminated through the spark has passed, at least in part, along with it through the whole thickness of the silver plate! It is unnecessary to add, that a spark issuing from a ball of copper, gives rise to the same phenomena.

The spark which emanates from a certain metal, does not merely lose a portion of the molecules with which it was at first impregnated, when it traverses another metal; but it becomes charged with new molecules at the expense of that metal. M. Fusinieri even asserts, that, at each passage of the spark, reciprocal changes are produced between the two metals; that when the spark, for example, leaves the silver to pass to the copper, it not only transports a portion of the first metal to the copper, but it likewise transports the copper to the silver! I will insist no longer, however, on these phenomena; I have cited them here only with a view to shew that the sparks of our ordinary machines contain ponderable substances.

M. Fusinieri affirms that similar substances exist in lightning, and that in this case also they are in a state of great division, of ignition and combustion. According to him, these transported matters are the true cause of the transient smells which thunder always occasions, and also of the pulverulent deposits which remain round the fractures through which the electrical matter has forced a passage. In these deposits, which have been too much neglected by observers, M. Fusinieri has detected metallic iron, iron in different degrees of oxidation, and sulphur. The ferruginous spots left on the walls of houses may be found, when strictly examined, to arise from the iron with which the lightning was charged, at the expense of that which occurs in almost every building; but what is to be said regarding the sulphurous spots on these same walls, and especially the ferruginous marks which are found in the open field on trees struck with lightning? M. Fusinieri conceives himself authorized to infer from these experiments, that the atmosphere contains, at every height, or at least as far as the region of stormy clouds, iron, sulphur, and other substances on the nature of which chemical analysis has been hitherto silent; that the electrical spark is impregnated with them, and that it transports them to the surface of the earth, where they form slight deposits round points that have been struck with the lightning.

This new method of regarding electrical phenomena, assuredly deserves to be followed up with that accuracy which is suited to the present state of science. Every one who witnesses the fall of a thunderbolt, would perform a very useful service by carefully collecting the black or coloured matter which the electrical fluid seems to have deposited at every stage of its progress, when it must have undergone sudden changes in rapidity. A careful chemical analysis of these deposits may lead to unexpected discoveries of high importance.

Falling Stars.—From the time that accurate observation has been directed to falling stars, it has been seen how much these long despised phenomena,—these pretended atmospheric meteors, —these so-called trains of inflamed hydrogen gas, are deserving of attentive examination. Their parallax has already placed them much higher than seems to accord with the sensible limits of our atmosphere according to the received theories.* While searching for the apparent direction in which these falling stars usually move, it has been ascertained that even though they are inflamed in our atmosphere, it is not from it that they originate, but that they come from without. This direction, *which is their most habitual one, seems diametrically opposed to the movement of the earth in its orbit!*

It is desirable that this result should be established by the investigation of a numerous series of observations. We have therefore requested the officers of the watch on board the *Bonite*, to note, during the whole of the voyage, the hour at which each of these falling stars appears, its angular height above the horizon, and particularly, the *direction of its motion.* By referring these meteors to the principal stars of the constellations which they traverse, the different questions which we have indicated may be resolved at a glance. Here, then, is a subject of research which will occasion no fatigue. It may suffice to attach our young countrymen to the subject, to remark how interesting it would be to establish the fact of the earth being a planet, from proofs derived from such phenomena as falling stars, the inconstancy of which has become proverbial. We might add, if it were necessary, that it is scarcely possible at present to see any other mode of explaining the astonishing appearance of those bodies observed in America on the night of the 12th and 13th November 1833, than by supposing that besides the large planets, there move round the sun

* Comparative observations made in 1823 at Breslau, Dresden, Leipsic, Brieg, and Gleiwitz, by Professor Brandes and many of his pupils, have assigned no less than 500 English miles as the height of certain falling stars.

The apparent speed of these meteors is found sometimes to be 36 miles per second. This is nearly double the rapidity of the earth's motion round the sun. Even although we were inclined to regard the half of this apparent velocity as an illusion arising from the effect of the earth's movement in its orbit, there would still remain 18 miles per second as the real velocity of the star, a degree of rapidity which exceeds that of all the superior planets except the earth.

myriads of small bodies which are not visible but when they penetrate into our atmosphere and there become inflamed; that these asteroids (to adopt the name which Herschel long since applied to Ceres, Pallas, Juno, and Vesta) move in a certain sense in groups; that others are insulated; and that the assiduous observation of these falling stars will be, at all events, the only means of enlightening us in regard to these curious phenomena.

We have just mentioned the appearance of falling stars noticed in America in 1833. These meteors succeeded each other so quickly, that they could not be counted; but a moderate calculation makes their number amount to a hundred thousand.* They were seen along the eastern side of America from the Gulf of Mexico to Halifax, from nine o'clock in the evening to sunrise, and even, in some places, during the light of day, at eight o'clock in the morning. *All these meteors issued from the same point of the sky*, situate near γ of the Lion; and that notwithstanding the altering position of this star in consequence of the diurnal movement of the sphere. This, then, is assuredly a very strange result, and we shall cite another which is not less so.

The shower of falling stars in 1833, took place, as we have already said, on the night of the 12th and 13th of November.

In 1799, a similar shower was observed in America by M. de Humboldt; in Greenland, by the Moravian Brothers; and in Germany, by various persons. The date is the night from the 11th to the 12th of November.

* The stars were so numerous, and appeared in so many different regions of the sky at once, that in attempting to reckon them, one would only expect to arrive at a very rough approximation. An observer at Boston compares them, when at the maximum, to half the number of flakes which are seen in the air during an ordinary fall of snow. When the phenomenon was considerably on the decrease, he counted 650 stars in 15 minutes, although circumscribing his observations to a zone, which did not include the tenth part of the visible horizon. This number, in his opinion, was not more than two thirds of the whole; thus there must have been 866, and in the whole of the visible horizon, 8660. This last number would give 34,640 stars per hour. But the phenomenon lasted for 7 hours, the number therefore that appeared at Boston must have exceeded 240,000; for it must not be forgotten that the data on which these calculations are founded, were not collected till the phenomenon was considerably on the decline.

In 1832, Europe, Arabia, &c., were witnesses of the same phenomenon, but on a smaller scale. The date of its appearance is again the night of the 12th to the 13th November.

This near approach to identity in the dates, authorizes us the more to invite our young navigators to watch attentively whatever may appear in the sky from 10th to 15th November, since the observers who were favoured with a clear atmosphere and expected the phenomenon last year (1834), saw manifest traces of it on the 12th and 13th of November.*

The Zodiacal Light.—The zodiacal light, although known for nearly two centuries, still presents a problem which has not

* Since my report was read to the Academy, M. Berard, one of the best informed officers in the French navy, has had the kindness to address to me the following extract from the journal of the brig *Loiret*, of which he was the commander:—

"On the 13th November 1831, at 4 o'clock in the morning, the sky was perfectly pure with abundance of red, but we saw a considerable number of falling stars and luminous meteors of large size. For upwards of three hours there could not, on an average, be fewer than two every minute. One of the meteors which appeared in the zenith, left an enormous train, forming a very broad luminous band (equal to half the diameter of the moon), in which many of the colours of the rainbow were very distinctly seen. We were then on the coast of Spain, near Carthagena, the thermometer in the air, 62.6 Fahr. (17°. 0 cent.); barometer, 30 inch. 3.4 lin.; temperature of the sea, 18°. 5 centi."

On the 13th November 1835, a large and brilliant meteor fell near Belley, in the department of Ain, and burned a barn. On the same night a falling star, more brilliant than Jupiter, was observed at Lille by M. Delezenne. It left behind it, in its passage, a train of sparks in every respect resembling those produced by a squib.

All these facts tend more and more to confirm us in the belief, that there exists a zone composed of millions of small bodies, whose orbits meet the plain of the ecliptic towards the point which the earth occupies every year, from the 11th to 13th of November. It is a new planetary world just beginning to be revealed to us.

It is doubtless unnecessary for me to say how important it would be at present to inquire whether other *trains of asteroids* meet the ecliptic in the different points of that in which the earth is placed towards the 13th of November. This investigation would require to be made, for example, from 20th to 24th of April; for in 1803 (I believe it was on the 22d of April), there was seen in Virginia and the Massachusets, from one o'clock till three in the morning, falling stars in such numbers and in all directions, that it might have been supposed to be a shower of rockets.

Messier relates that, on the 17th June 1777, towards mid-day, he saw a prodigious number of black globules pass across the sun for about five minutes! Might not these globules likewise be asteroids?

been solved in a satisfactory manner. The study of this phenomenon is chiefly reserved, by the very nature of things, to observers placed in the equinoctial regions. They alone can decide whether Dominique Cassini has sufficiently guarded against the causes of error to which an observer is exposed in our variable climates, and whether he has sufficiently taken into account the purity of the air, when he announced in his work that the zodiacal light is constantly brighter in the evening than in the morning; that in the course of a few days its length may vary from 60° to 100°; that these variations are connected with the appearance of solar spots, in such a manner that there must have been, for instance, a direct dependence and not merely a fortuitous coincidence, between the weakness of the zodiacal light in 1688, and the absence of every kind of spot or fœcula on the solar disc in that same year.

It appears to us, therefore, that the Academy ought to desire the officers of the *Bonite,* during the whole time they remain between the tropics, and when the moon does not lighten the horizon, to be on the watch, either after sunset or before sun-rising, and take note of the constellations which the zodiacal light traverses, of the star which indicates its point, and of the angular breadth of the phenomenon near the horizon, at a determined height. It is almost superfluous to add, that an account must be kept of the hours when the observations were made. The calculation of the results may be delayed without any inconvenience till the period of returning home.

We are not ignorant, as has been already hinted, that some very able observers regard these statements of Dominique Cassini as little deserving of confidence. They are unwilling to admit that sensible physical changes could operate simultaneously in such an immense extent as the zodiacal light embraces. In their opinion, the variations in intensity and length noticed by this great astronomer, were not real, and they think that we need go no farther for an explanation of them, than the intermissions of the atmospheric transparency.

It would not, perhaps, be impossible to find proof in the observations of Fatio, compared with those of Cassini, that atmospheric variations are insufficient to explain the phenomena described by the Parisian astronomer. With respect to the objection derived from the immensity of the space in which the physical changes must operate, it has lost all its force since the phenomena of the same nature exhibited by Halley's comet.

Our young countrymen ought therefore zealously to devote themselves to the observations which we have pointed out. The question is important, and no one can hitherto flatter himself with having definitively solved it.

Aurora Borealis.—It is now well ascertained that there are as many displays of polar aurora towards the southern hemisphere, as in the Arctic Regions. Every thing leads us to think, that the appearances of the southern aurora, and of that which we witness in Europe, follow the same laws. This, however, is mere conjecture. If a southern aurora be seen by the officers of the *Bonite* in the form of an arch, it will be important to notify exactly the *orientations* of the points of this arc's intersection with the horizon, and, if these cannot be obtained, the *orientation of the most elevated point.* In Europe, the most elevated point always appears to be situated in the magnetic meridian of the place where the observer is stationed.

It has been proved by numerous researches undertaken at Paris, that all kinds of aurora borealis, even such as do not appear above our horizon, and the existence of which, consequently, we can learn only from the reports of observers in the polar regions, alter decidedly the declination of the magnetic needle, as well as its inclination and intensity. Who, then, can presume to assert, as an argument to prove the great distance of the aurora borealis, that it never disturbs the magnetism of our hemisphere? The attention which our travellers, in every case, bestow on these phenomena, may at length throw some light on the question. Arrangements have been formed, in order that magnetic observations may be made at Paris during the whole time of the Bonite's absence, at periods so near each other, and in such a manner, that no irregularity can take place unperceived.

The Rainbow.—The explanation of the rainbow may be regarded as one of Descartes's most beautiful discoveries; but, at the same time, even after receiving the developments which Newton gave to it, it is still incomplete. When we look attentively at this magnificent phenomenon, we perceive under the red of the interior arch, numerous green and purple series, forming narrow contiguous arches, well defined, and perfectly concentric to the principal arch. Of these *supplementary* arcs (for that is the name given to them), the theory of Descartes and Newton makes no mention, and indeed it cannot even apply to them.

The supplementary arcs appear to be an effect of *luminous interferences.* These interferences cannot be produced but by drops of water of a certain smallness. In order that the phenomenon may appear with some degree of brightness, it is necessary that the drops should not only be of the proper size, but that at least the greater part of them should be equal in their dimensions, almost with mathematical accuracy. If, therefore, the rainbows of equinoctial regions are never attended with supplementary arcs, the circumstance would afford a proof that the drops of water which there issue from the clouds are of larger size, and more unequal dimensions, than in our climates. In our ignorance of the causes of rain, this fact would by no means be void of interest.

When the sun is low, the upper portion of the rainbow is, on the contrary, very much elevated. It is towards this culminating region that the supplementary arcs shew themselves in greatest splendour. Leaving that point, their colours rapidly become feeble. In the lower regions, near the horizon, and even considerably above it, no traces of them are ever seen, at least in Europe.

It must needs be, then, that the drops of water, during their vertical descent, have lost the property which they at first possessed; they must have laid aside the conditions necessary for efficacious interferences, which they do by having become much enlarged.

Is it not curious, it may be remarked in passing, to find in an optical phenomenon, in a peculiarity merely of the rainbow,—the proof that in Europe the quantity of rain must be so much the less the higher we place the vessel in which it is to be received!*

The increase in the size of the drops is, no doubt, to be as-

* In the observatory at Paris, there are two receptacles in which rain water is collected; one of them on the terrace, the other in the court, eighty-six feet lower than the first. In the course of a year the reservoir in the court received eight-hundredths *more* water than that placed on the terrace.

cribed to the precipitation of humidity on their surface, as they descend from the cold region whence they originated, and traverse atmospheric strata, gradually becoming warmer as they come nearer the earth. It is then almost certain that, if supplementary rainbows are formed in equinoctial regions, they will never reach the horizon, as is likewise the case in Europe; but the comparison of the angle of the height at which they cease to be seen, with the angle of disappearance as noticed in our climates, seems to afford the means of obtaining meteorological results, which no other method at present known can supply.

Halos.—In high latitudes, the vicinity of Cape Horn for example, the sun and the moon often appear surrounded by two luminous circles, which meteorologists name *halos.* The ray of the smallest of these circles is about 22°, the ray of the larger is almost exactly 46°. The first of these angular dimensions approaches near the minimum deviation which light undergoes while traversing a glass prism of 60°; the other would be given by two prisms of 60°, or by a single prism of 90°.

It seems, therefore, natural to seek for the causes of halos, as Mariotte has done, in the rays refracted by the floating crystals of snow, which, as every one knows, usually present angles of 60° or 90°.

This theory, besides, has received a new confirmation, since the power has been acquired of distinguishing between refracted and reflected light by means of chromatic polarization. It is in fact the colours of the first of these lights (that which is refracted) which produces the polarized rays of the halo. What, then, still remains to be known regarding this phenomenon? It is the following:—According to theory, the horizontal diameter of a halo and the vertical diameter ought to have the same angular dimensions; but we are assured, that these diameters are sometimes strikingly unequal. Measurement alone can establish this fact; for, if a judgment has been formed by the naked eye, the causes of illusion will not fail to explain how the most experienced natural philosopher might be deceived. The reflecting circles of Borda are admirably adapted to the measurement of the angular distances at sea. We may, therefore, without hesitation, recommend to the officers of the *Bonite*, to apply the excellent instruments with which they are provided, to determine the dimensions of all the halos *that appear to be elliptical.* They will themselves perceive that the inner edge of the halo, the only one which is distinctly defined, is much better adapted for observation than the outer edge; but it is necessary, with regard to the sun, not to neglect to indicate whether they have taken the centre or the edge as the term of comparison. We likewise regard it as indispensable, that the two rays diametrically opposite should be measured in each direction, for certain observers have mentioned circular halos, in which, if they are to be believed, the sun did not occupy the centre of the curve.

Trade-Winds.—Perhaps it will excite surprise to see the trade-winds announced as still affording a subject of important investigation; but it must be remarked, that the practice of navigators has often confined them to simple notices, with which science cannot be satisfied. Thus it is not true, whatever may have been alleged, that to the north of the equator these winds constantly blow from the *north-east;* and that to the south of it they blow uniformly from the *south-east.* The phenomena are not the same in the two hemispheres. In each place, moreover, they change with the seasons. Daily observations of the real direction, and as far as practicable of the force of the eastern winds which prevail in equatorial regions, would therefore be a useful acquisition to meteorology.

The vicinity of continents, on the western sides especially, modifies the trade-winds in their strength and direction. It sometimes even happens that they are replaced by a *west* wind. Wherever this change of the wind occurs, it is desirable to note the time, the bearing of the neighbouring country, its distance, and its general aspect. In order to shew the utility of this last recommendation, it is enough to say, that a sandy country, for example, will have a much speedier and more active influence, than one covered with forests or any other kind of vegetation.

The sea which washes the western side of Mexico, from Panama to the peninsula of California, between 8° and 22° north latitude, will afford an opportunity to the officers of the *Bonite* of observing a complete inversion of the trade-winds. They will find, as we are informed by Captain Basil Hall, a nearly permanent west wind, in a situation when the east wind of the equinoctial regions might be expected to prevail. In these latitudes, it would be curious to ascertain to what distance from the coast this anomaly exists; and at what longitude the trade-wind resumes, so to speak, its laws.

According to the most generally adopted explanation of the trade-winds, there ought constantly to be found, between the tropics, a *higher wind*, blowing in an opposite direction from that at the surface of the earth. Numerous proofs have been already collected of the existence of this counter-current. Careful observation of elevated clouds, particularly those named "*pommelés*," ought to afford indications of great value to meteorology.

The periods, strength, and extent of the monsoons will form, in short, a subject for study in which, notwithstanding a multitude of important observations on the subject, much yet remains to be gleaned.

Phenomena of the Sea.

*On the means of drawing up Sea-water from great depths, and discovering in what proportion the two chief constituent principles of atmospheric air are contained in it.**—Chemists have long since proved, that water becomes impregnated with the gases which rest on its surface. This absorption is effected by means of a true chemical affinity exerted on the different gases, and when its effects on oxygen and azote, the two principal constituents of atmospheric air, are carefully examined, it is found to be much stronger in regard to the first than the second. It hence follows, that the waters of seas and rivers, from being always in contact with the atmosphere, become at length impregnated with a mixture of gases, among which oxygen predominates. Indeed, the very accurate experiments of MM. Humboldt and Gay Lussac have proved that rain-water, the water of the Seine, and snow-water, contain a mixture of oxygen and azote, which amounts from 29 to 32 parts of oxygen in 100; while the proportion of oxygen in atmospheric air is constantly equal to 21 parts, and this in all seasons and climates. MM. de Humboldt and Provençal have besides determined the entire volume of mingled gases contained in water near the surface, and it was $\frac{1}{36}$ of the volume of water.

* This section of the instructions was communicated to M. Arago by M. Biot.

It follows as a necessary consequence of these properties, that the vast extent of sea which covers a large part of the globe, is impregnated with a mixture of gases, the proportions of which, near the surface, must be nearly the same as those just mentioned. I have ascertained that it is likewise so at the depth of a thousand metres; for sea-water, drawn from that depth, afforded me a mixture which contained 28 parts of oxygen in 100. This experiment I formerly made in the Mediterranean.

But here many important questions in terrestrial physics present themselves, which the apparatus I then employed did not enable me to solve. In proportion as we descend into the depths of the sea, the pressure of the superior portion becomes greater; and as a column of sea-water, ten metres in height, is nearly of the same weight as a column of air of the same base from the surface of the earth to the limit of the atmosphere, it follows that, at the depth of a thousand metres, the water sustains a pressure of a hundred atmospheres. How enormous, then, must this pressure be on the lower beds, if the mean depth of the sea, at a distance from the coasts, extends to several leagues, as the laws of gravitation seem to indicate!* It is likewise proved, by direct experiments, that water, when in contact at the surface with compressed gases, and also sustaining their pressure, absorbs the same volume of these gases as if they were subjected to the pressure of a single atmosphere merely; so that the weight absorbed becomes proportionably greater. If, then, the single fact of a uniform absorption, propagated from one bed to another throughout the whole mass of waters, be sufficient to account for the presence of a considerable volume of air, how greatly must the quantity be increased if it be in proportion to the pressure according to the varying depth! As this saturation must have been in gradual operation since the seas were formed, it must have modified by degrees the state of the atmosphere, and perhaps continues to modify it still, if the affinity which produces it is not satisfied. The influence of these phenomena on the state of the atmosphere, and consequently on the conditions of the existence of living beings on the surface of the globe, well entitles them to be studied in order to have the extent of their operation ascertained.

* Mécanique Celeste, tom. ii. p. *200*.

For this purpose, it is necessary to draw sea-water from a great depth, at a distance from land, and to bring it to the surface with all the air which it contains. This air must then be disengaged by boiling, its volume measured under the ordinary pressure of the atmosphere, and finally subjected to a chemical analysis. In these operations, the only difficulty is to draw the water from the desired depth, and bring it to the surface without allowing any of its contents to escape. Care must be taken not to employ vessels which are empty or filled with air merely, designed to open at the assigned depth and admit the water; for the pressure to which they will be subjected will cause the water to filter through the joints of the most perfect closing plates (*obturateurs*), or break the vessel if these resist. And, lastly, if the gaseous mixture contained in the deep-lying beds is subjected to the same pressure which they undergo, it will expand in the inverse proportion when the apparatus is brought near the surface, and will either escape by the closing plate (*obturateur*) or burst the vessel containing it. In order to avoid these effects, a hollow glass cylinder ought to be employed, closed at one extremity by a solid plate of metal, thus forming a true bucket provided with a handle, to which a cord is attached that it may be let down to the bottom of the sea. This bucket being empty, and open to the surrounding water, descends into the different beds without being injured by the pressure. When it has reached the required depth, a cord is drawn which is attached to its lower part, and by which it is reversed. This second cord is then employed to draw up the apparatus, and in order that it may not get entangled with the other, it is kept at the other end of the ship. But this cylinder of glass has a double bottom, one fixed, the other moveable. The latter is in reality the piston of an air-pump, which descends alone, by its own weight, when the bucket is drawn back; and, at the same time, the bottom fixes to a small hole furnished with a valve, which opens from without inwards by the pressure of the surrounding water, and allows it to enter into the empty space made by the descending piston. When the latter has descended, and the space filled, the valve in the bottom closes by its own spring, and the enclosed water is thus separated and kept apart till it arrive at the surface. But, if this water contain compressed air, nothing will counteract its expansive tendency when it is brought to the surface, where the pressure of the water from without is removed; it will then either escape or burst the apparatus. To guard against this, a free issue must be provided for all possible expansion either of the water or air. For this purpose the fixed bottom is furnished with a lateral canal which leads to a gas-bladder; the latter having been first filled with water, then emptied and pressed together before the apparatus was sunk. This bladder will receive all the air which the water may disengage on approaching the surface; and, if any be disengaged, it will return more or less inflated. Then, by closing the stopcocks with which the canal is provided, it may be separated from the vessel containing the water, its volume measured, and the enclosed air analyzed; after which what may still remain in the water may be examined, and likewise any other substance which the water may hold in solution. Such is the apparatus which has been entrusted to the commander of the *Bonite*; and the zeal as well as intelligence of that officer, affords us the assurance, that it will be usefully employed, under his directions, to solve the various questions relating to terrestrial physics indicated above; questions which, besides their purely scientific interest, have an additional importance attached to them, by the knowledge which their solution would supply respecting the permanence or changeableness of our atmosphere, and the conditions of the existence of living creatures found in the depths of the sea.

Marine Currents.

The Atlantic Ocean, South Sea, and the Mediterranean, are traversed by numerous currents, which are the more dangerous from carrying vessels out of their proper course, without the pilot suspecting their influence, and, in cloudy weather, he has no means of ascertaining it. Among phenomena relating to the sea, and considered in the double relation of theory and practice, there is certainly none more deserving of the highest degree of attention on the part of navigators of every country. The numerous memoirs and works specially appropriated to the subject, such as those of Ducoudray, Romme, and even the posthumous and able treatise of Major Rennel, which has just appeared, are very far, in my opinion, from having exhausted the subject. Of this the reader will be able to judge.

On the cause of Currents.—The most remarkable currents studied by navigators are, in the Atlantic :—

The current, which, after having gone round the bank of Agullas and the Cape of Good Hope, advances from south to north along the western side of Africa as far as the Gulf of Guinea.

The current termed equinoctial, which invariably flows from east to west on both sides of the equator, between Africa and America.

The current which, after having issued from the Gulf of Mexico by the Straits of Bahama, flows at a certain distance along the coasts of the United States towards N.E. as far as the bank of Nantucket, where it changes its direction.

Lastly, the current, by the action of which the waters of the ocean which bathe the coasts of Spain, Portugal, and Africa from Cape Finisterre as far as the parallel of the Canaries, are all directed towards the Straits of Gibraltar.

What can be the cause of these currents?

The trade-winds, according to some, by continually blowing in the Indian Ocean from east to west, must produce a *liquid intumescence* on the eastern coast of Africa near the equator. This accumulated water flows continually from north to south through the Straits of Mosambique. When it reaches the parallel of the Cape of Good Hope, the eastern wall or mound which had hitherto maintained it disappearing, the water necessarily flows westward. It is thus that the current of the Agullas is formed.

The equinoctial current of the Atlantic is attributed to the constant impulsion of the trade-wind on the waters in the vicinity of the equator to the north and south.

The Atlantic equinoctial current, in this respect resembling the equinoctial current of the Indian sea, must produce a great accumulation of water along the first coast which presents itself as a barrier to it; that coast is America. From this results a general movement of the Caribbean sea towards the strait which separates the eastern point of Yucatan from the western point of Cuba, as well as an elevation of the level of the sea in the Gulf of Mexico; and, lastly, that kind of cascade formed by the accumulated water in the Gulf, where it escapes from the Strait of Bahama, the continuation of which forms the *gulf-stream.*

With respect to the current flowing towards the Straits of Gibraltar, it may be caused by the lower level of the Mediterranean, and this depression of its surface may itself be occasioned by its excessive evaporation which the influx of the tributary rivers is insufficient to compensate.

These explanations are simple; they appear to rest on physical causes, the action of which must take place in the sense supposed; the most intelligent observers, Franklin, Rennel, &c., have adopted them; and yet I am about to prove that they are not so completely established by observation, measurement, and experiment, as to prevent us from entertaining legitimate doubts on the subject.

A continued and strong wind raises the level of the sea along the coast towards which it tends to direct the water; thus, at Brest, Lorient, Rochefort, &c., the tide is always highest, other circumstances being equal, during a *west wind.* On the opposite shores of the Atlantic, and along the coasts of the United States, on the contrary, it is the *east* wind which produces this effect. It is by *south* winds that the waters of the Mediterranean are accumulated in the ports of Genoa, Toulon, and Marseilles, and by *north* winds in those of Algiers, Bougie, and Tunis. These facts are not disputed, nor do they admit of being called in question. It only remains to determine the value of the accidental changes of level which winds may produce.

Franklin relates that, in an extensive piece of water three leagues broad, and about three English feet in depth, a strong wind caused the whole of one of the sides to become dry, while it raised the water on the opposite side three feet above its former level, the depth being six feet instead of three. In our own seas, I do not think that it will be necessary, in general, to state a higher number than this as the maximum effect produced by the most violent tempests.*

The Trades are constant winds, but their strength is very mo-

* Places are mentioned in the Mediterranean where gusts of the south-west winds (named the *Labeschades*) have raised the waters seven metres above their ordinary level; but this effect was purely local.

derate. The deviations from a uniform level which they can occasion ought likewise to be inconsiderable. But it seems difficult to believe, that the vertical fall of a metre, for example, or even two metres, can produce currents which do not entirely disappear after a passage of many hundred leagues.

I have stated that the trade-winds, on account of their want of intensity, seem but little likely to produce any considerable swelling in the waters of the ocean. I will even go further than this, and prove that, in point of fact, the seas from which currents appear to emanate, are exactly, or very nearly, of the same level as those which the currents traverse.

It has been indisputably proved by M. Lepère, in the observations made during the Egyptian expedition, that the level of the Mediterranean, near Alexandria, is lower by 8^{m}, 1 than the low water-level of the Red Sea near Suez, and 9^{m}, 9 than the high water level.

This is certainly a very great difference of level between two seas which may be considered as communicating with each other; for, on the one hand, the Mediterranean opens into the Atlantic by the Straits of Gibraltar; on the other, the Red Sea opens into the Indian Ocean by the Straits of Babelmandel; and, in the third place, the Atlantic and Indian Ocean become blended with each other at the Cape of Good Hope. It is very far from my intention to depreciate what is curious or interesting in such a result as this; but I must be allowed to say, that it throws no light on the disputed question of currents, for, to render the explanation admissible, there ought to be a sensible difference between the level of two contiguous seas, between that from which the current issues, and that into which it flows.

Does not precisely such a difference as this, it may be asked, exist between the Mexican Sea, in which the Gulf Stream originates, and the part of the Atlantic Ocean which washes the eastern side of the Floridas and Georgia?

The inhabitants of the Isthmus of Panama believe, but without proof, that the South Sea is higher than the Atlantic Ocean. Franklin, Rennel, &c. likewise admit a difference of elevation, but in a contrary sense. M. de Humboldt confirmed this latter opinion by some barometrical observations made at Cumana, Carthagena, and Vera Cruz, compared with others made at

Acapulco and Callao. At the three places first mentioned, the waters appeared to him to be three metres *above* the level of the South Sea, as taken on the western shores of Mexico and Peru. For as no one can doubt that the general mass of the Southern and Atlantic Oceans are of the same level; that portion of the latter near the Antilles, and that which is inclosed in the Gulf of Mexico will thus form a local elevation or intumescence of three metres.

Before citing a work which does not confirm this result, I ought to mention that my illustrious friend has himself remarked, with his usual caution, that his observations were not sufficiently numerous to establish the fact of so small a difference of level without any doubt.

Two engineers not long since crossed America at its narrowest point, in order to settle definitively the relative position of the two oceans. We may add that their object was not purely of a scientific nature, but had a direct reference to one of the grandest problems which commerce ever proposed, the possibility of a communication between the Atlantic and South Seas, across the Isthmus of Panama. Such was the object of the investigation, the results of which I am about to state, and which was intrusted by General Bolivar to Mr Lloyd, an English engineer, and a Swedish captain named Falmarc.

The date of this examination is 1828 and 1829, and it was made by means of one of Carey's levels. The point of departure is at Panama, on the Pacific Ocean, the level of the highest tides of the equinox corresponding to the third day of the full or of the new moon. Its other extremity is a place named Bruja, to which the influence of the tide extends. Bruja is on the Chagres, about twelve miles (five leagues) from the place where that river discharges itself into the sea of the Antilles.

At Panama, the mean difference of the level between high and low water, during strong tides, is 21.2 English feet. At Chagres, on the Atlantic, this difference does not exceed 1.1 feet.

By thus assuming, in each place, for the mean level of the ocean, a surface equally remote from the successive levels of the high and low sea, it follows, from the examination of Messrs Lloyd and Falmarc:

1*st*, That the mean level of the Pacific Ocean, at Panama, is from 3,52 English feet (1^m,1) *higher* than the mean level of the Atlantic Ocean at Chagres;

2*d*, That at the moment of high tide, the ocean on the western side of the isthmus, is from 13.55 feet (4^m,13) *higher* than on the western side.

3*d*, That at the moment of low water, on the same coasts, the Pacific Ocean, on the contrary, is *lower* than the Atlantic 6.51 feet (1^m,98).

These observations seem, then, to confirm the opinion long since adopted, that the mean level of the South Sea is more elevated than the mean level of the Atlantic Ocean; but the difference, instead of being enormous, as was supposed, is only eleven decimetres. It may even be allowed us to suppose, without injustice to the merits of Messrs Lloyd and Falmarc, that, in carrying on their operations in a wild country environed with difficulties; in traversing a line, the total extent of which, including sinuosities, is eighty-two miles (33 leagues), and that, in taking levels at 935 stations, they may have erred to the small extent of a metre. From this it follows, that there is nothing to prove that a sensible difference exists between the mean levels of the two great seas which communicate with each other by the Straits of Magellan and Cape Horn. *

The work of Messrs Lloyd and Falmarc, in so far as it applies to the explanation of the rapid current which precipitates itself from the Gulf of Mexico into the ocean by the Straits of Bahama, advances as a hypothesis that the South Sea and Atlantic Oceans, viewed as a whole, form a surface of the same level. We will escape from this difficulty by relating the results of some observations made in Florida a few years since by the French officers appointed by the American Congress to survey the line of a canal designed to unite the river St Marie, on the Atlantic, with the bay of Appalachicola, on the Gulf of Mexico.

* If, after the learned memoirs of M. de Humboldt, it is still necessary to return to the truly astonishing depression that the *Cordillera* of America presents in the Isthmus of Panama, before again assuming its full majesty in Mexico, I would remark that the most elevated point of the transverse line levelled by Messrs Lloyd and Falmarc, is only 633 English feet above the level of the sea.

According to the first calculation of measurements, the low tide in the Gulf of Mexico would be higher than the low tide of the Atlantic, by about 1^m,14 (3 feet, 52). A second calculation gives a similar difference between the two low tides of about 0^m,85 (2 feet, 63). The mean is 1^m,00 (3 feet, 08).

But even this slight inequality of level is greater than the real one. In fact, when we compare two seas subject to tides, it is evidently the mean levels, the points, that is to say, equally remote from the high and low seas, that ought to serve as data. In this instance, although I can perceive no cause for it, the comparison is made between two low seas. In order to state the matter accurately, therefore, it is necessary to elevate the point of comparison taken in the Gulf of Mexico to half the height of the tide observed in that gulf. The same thing must be done in regard to the eastern or Atlantic side of the Floridas. In the gulf, near the point where the level is terminated, the tide does not rise more than 0^m,3. On the other side of the Floridas, towards the mouth of the river St Marie, the tide is about 2^m,0. The low tide, therefore, is 0^m,8 more removed from the mean tide at St Marie than in the gulf. If, then, the mean levels are referred to, as must be done to obtain the real result, instead of 1^m,0, it will be found that the difference of the level of the two seas is 1^m,0 less 0^m,80; that is to say 0^m,2 ($7\frac{1}{2}$ inches.)

This quantity is evidently within the limits which ought to be assigned to the errors capable of being made in observations embracing the whole breadth of the Floridas. But even though the difference alleged were real, it may be doubted whether any one would be inclined to regard it as a sufficient explanation of the cause of a current which, issuing from the Straits of Bahama, at the rate of no less than five miles an hour (2 leagues), continues its progress into the very middle of the Atlantic, nearly in a straight line, to a distance of 500 leagues, without having its rapidity abated during the whole of that course.

Let us now consider the Mediterranean. Here the alleged lowness of the level, the presumed cause of the current flowing from the ocean to the Straits of Gibraltar, is said to be the result of an enormous annual evaporation, which the mass of waters contributed by the Nile, Rhone, Po, &c. are insufficient to compensate. Direct and demonstrative proofs of this want of compensation, are, it is true, completely wanting. In the absence of these, a new form will be immediately given to the argument, and it will be said (which is in reality the case), that in summer, at equal latitudes, the waters of the Mediterranean are from 3° to 3°5 Cent. (about 6° Fahr.) warmer than those of the ocean, from which it inevitably follows that the first undergo more evaporation than the others, and that nothing more is required to explain the current of the strait.

And this, it must be confessed, would be sufficient, if the cause indicated were to produce a *very sensible* difference of level in the two seas. Thus, whatever may have been said of it, the problem will be found to be reduced to one of numbers, or to a question of facts. It must be found out, either by calculation or experiment, *to what extent* the Atlantic Ocean is higher than the Mediterranean. The calculation, I have already stated, will be difficult to be made with precision, owing to the want of sufficient data. With regard to the experiment, the results of that which I am about to present, seem to me calculated to satisfy the most scrupulous minds.

Delambre found the direct means of inquiring into the comparative level of the two seas, by the great chain of triangles on the meridian of France, which extended from Dunkirk as far as Barcelona. The triangles comprehended between Rhodes and the Mediterranean, afforded him for the vertical height of that town, a result which agreed to a fraction of a metre with the height of the ocean, as deduced from that portion of the chain placed between Rhodes and Dunkirk.

It has been stated in opposition to this view, that the observations from which it was deduced, were not always made in favourable circumstances; that it is necessary they should be frequently repeated before they could be assumed as proving a difference of level; and that, moreover, the calculations had neither been made with sufficient care, nor on a plan sufficiently accurate. These objections are not without weight. The officers of the body of geographical engineers have likewise endeavoured to take advantage of the chains of triangles of the first order, drawn in different directions, which cover the whole surface of France, to submit the question of the respective levels of the two seas to a new examination. M. Delcros, among others, has devoted himself to this subject, and made extensive investigations. These, however, are still in MS., and I regret that I am unable to state the results. Besides this, the observations which M. Coraboeuf presented to the Academy of Sciences are as directly to the point as could be desired, and were conducted with a precision which left nothing to be desired.

This operation, carried on along the southern frontier of France, during the years 1825, 1826, and 1827, embraces, in the direction of the shortest distance, all the interval comprehended between the Ocean and the Mediterranean. Forty-five triangles of the first order, many of which have their summits on the highest points of the Pyrenees, join the fort of Sacoa, near St Jean de Luz, at different points of the plain of Perpignan, the elevation of which above the sea is so small as to be reduced to two secondary triangles. All the angles have been measured by M. Gambey's repeating circles, and by three series of repetitions at the least. The same is the case with the zenithal distances. Care has been taken, besides, to make these observations only between 10 o'clock in the morning and 3 or 4 in the afternoon, in order to avoid the effects of the irregular refractions, which appear near the horizon some hours after the sun has risen, and a few hours before setting. The extent of the atmospheric refraction, between each couple of stations, has been deduced from a comparison of the reciprocal distances to the zenith. As assistants in these important operations, M. Coraboeuf had Captain Peytier and Lieutenants Hossard and Testu of the Geographical Engineers.

The station of the *Crabère* occupies nearly the middle of the interval which separates the ocean from the Mediterranean. The eastern part of the chain of triangles has served to determine its height above the Mediterranean; the other part has afforded this same height above the ocean. It is necessary to remark, that the calculations can be made by a multitude of distinct combinations, among which M. Coraboeuf has made choice of three. He has ascended, in the first place, from the ocean and the Mediterranean to the Crabère, passing by the only series of summits of triangles which limit the chain towards the south. In the next place, by choosing exclusively the northern summits; and, lastly, by the diagonal directions, that is to say, by going alternately from a northern summit to a southern one. The following is the result of these various combinations:—

	Height of Crabere.		
	On the Mediterranean.	On the Ocean.	Diff.
Direction of the southern summits, .	2633m, 37	2632m, 95	0m, 42
Direction of the northern summits, .	2633 , 99	2632 , 07	1 , 92
First direction by diagonals, . .	2633 , 87	2633 , 61	0 , 26
Second direction by diagonals, . .	2632 , 79	2632 , 49	0 , 30
Mean,	2633 , 50	2632 , 77	0 , 73

The mean difference, 0m,73 (2 feet 3 inches), is too small, particularly when we recollect the extent of the line which was levelled, to prevent us from concluding that, when in a state of repose, the waters of the ocean, and those of the Mediterranean, form a surface of the same level. At all events, it can scarcely be doubted that if any difference in this respect exist, it is too small to be appreciated.

In this article I wish merely to prove that the subject of currents is far from being exhausted; that differences of level, to which hydrographers have recourse for an explanation of them, are either completely nugatory, or insignificant; that there is still room for further investigation. This object I conceive myself to have attained. I shall still, however, add a few reflections.

The theory of currents has made little progress hitherto, because those phenomena have chiefly been considered which appear at the surface of the sea. Currents produced by difference of saltness and temperature exist at all depths. There are currents, for example, in contact with the very bed of the sea, which transport the cold waters of the polar zones as far as the equator. Near the poles these waters move like the solid part of the earth which supports them, at a very slow rate, from west to east. As they pass by degrees to temperate and warm regions, they arrive at greater parallels of the earth, which henceforth move quicker than they; hence relative currents directed from the east to the west, the size of which is equal to that of the polar currents.

It is, if I am not deceived, by placing them in this point of view; by descending, in imagination, to the profoundest depths of the ocean; and by applying to the sea the theory which has already given such a satisfactory explanation of the trade-winds, that we shall succeed in throwing light on the subject which has just occupied us. It will thus, in my opinion, be equally possible to conceive how currents of inconsiderable rapidity cross such immense extent of sea; how they are reflected and changed in their course, *while yet at a distance*, by the sides of continents and islands; and how they turn aside on approaching banks, such as those of Agullas or Terra-neuva, above which there is not less than sixty fathoms of water!

Sea of Weeds.—Among the phenomena of the ocean, which, notwithstanding their antiquity, may yet become the subject of curious investigation, I should be inclined to place that of the *Sea of Plants* or the *Sea of Weeds.*

These names are now applied to a zone of the Atlantic Ocean situated to the west of the Azores. This zone, on an average, is from forty to fifty leagues in width; its extent in latitude is 25°; the space which it occupies being nearly equal in extent to the surface of France. It is entirely covered with plants (*Fucus natans*). The Portuguese call it *Mar de Sargasso;* Oviedo, *Praderias de Yerva* (Prairies). In 1492, the companions of Christopher Columbus were greatly alarmed by it, for they conceived that they had reached the remotest limits of the navigable ocean, and expected to be stopped by the sea-weed, as their fabulous St Barandan had formerly been by the ice of the Polar Regions.

By examining a multitude of observations on the subject, deposited in the archives of the English Admiralty, in order to determine the limits of the Sea of *Sargasso,* Major Rennel found that this great bank of fucus has undergone no change of place, between the years 1776 and 1819, either in longitude or latitude. This remarkable constancy in situation, M. Humboldt has shewn to have existed as far back as the end of the fifteenth century, in his remarks on the observations made by Columbus.

Three different explanations have been advanced to account for the existence of this sea of *Fucus natans.* Some are of opinion that there are, in these latitudes, numerous banks in the bottom of the ocean on which the fuci grow, and from which they are accidentally detached; others, that these plants vegetate, and develope themselves even on the surface of the water; but the opinion most generally received is, that the Sea of Weeds is the place where the Gulf-Stream continually deposits the plants with which it becomes charged on issuing from the Gulf of Mexico.

This last mentioned hypothesis has been adopted by Major Rennel, although it is very far from explaining why a great proportion of the floating weeds in the sea of Sargasso are, instead of being faded or decayed, in a state of great freshness. Indeed, English navigators never fail, when they speak of these regions, to mention the *fresh weed,* and *weed much decayed.* Christopher Columbus himself, as M. de Humboldt has remarked, was likewise struck with the mixture of *yerba muy vieja y otra muy fresca.*

The floating fuci of the sea of Sargasso are always destitute of roots and fruit. If we suppose them to be developed in the same region where they are found, we must regard them, as M. Meyer has done, as similar to fresh-water algæ, many of which multiply only by new branches. It will likewise remain to be explained by what means it is that the waters over such a great extent of sea escape so completely from the action of winds and currents, that the lapse of several ages has not dispersed the plants which were found collected there in the end of the fifteenth century, when the galleys of Columbus ploughed it for the first time.

It doubtless appears more natural to suppose, that, as the winds and currents by degrees draw the floating fuci beyond the ordinary limits of the Sea of Plants, their place at the surface is occupied by others detached from the bottom. According to this hypothesis the plants are stationary only in appearance; the sea will always appear equally covered above the region which nourishes them, but the individuals will be continually renewed.

What, then, is necessary at the present time to throw light on this curious point of physics? A few very simple experiments, which, however, are still wanting to science: Soundings made along the edges, and towards the centre of the sea of Sargasso, with the necessary length of line.

Temperature of Currents.—Every one is acquainted with the works of Franklin, Blagden, Jonathan Williams, M. de Humboldt, and Captain Sabine, on the Gulf-Stream. No one now doubts that this Gulf-Stream is an equinoctial current, which, after having made the circuit of the Gulf of Mexico, and issued from the Straits of Bahama, moves from south-west to north-east, at a certain distance from the coast of the United States, retaining all the time, like a stream of warm water, a greater or less degree of the temperature it had acquired between the tropics. This current divides into two branches. One of these visits the coasts of Ireland, Orkney, Shetland, and Norway, having the effect, it is said, of moderating the climate: the other gradually describes a curve, returns by its former route, crossing the Atlantic *from north to south*, usually to the west of the Azores, and sometimes at no great distance from the coasts of Spain and Portugal. After a very long circuit, its waters again join the equinoctial current from which they issued.

Along the coast of America, the position, breadth and temperature of the Gulf-Stream have been so well determined under each latitude, that a work has been published, without any appearance of empiricism, under the title of *Thermometrical Navigation*, for the use of navigators in these latitudes. It is very desirable that the returning branch should be known with the same certainty. Its excess of temperature almost disappears when it reaches the parallel of Gibraltar, and it can be accurately determined only by the mean of a great number of observations. The officers of the Bonite will greatly facilitate this investigation, if they determine the temperature of the ocean *every half hour*, and from the meridian of Cadiz, as far as that of the most western of the Canaries, with the precision of tenths of a degree.

We have just spoken of a current of warm water; our navigators, on the other hand, meet with a current of cold water along the coasts of Chili and Peru. This current, after leaving the parallel of Chiloé, moves rapidly from south to north, and conveys, as far as the parallel of Cape Blanc, the cold water of the vicinity of the southern pole. The temperature of this current was first noticed by M. de Humboldt, and it has been examined with very particular care during the voyage of the *Coquille.* The frequent observations on the temperature of the ocean which the officers of the Bonite will not fail to make between Cape Horn and the equator, will serve to extend or complete the important results obtained by their predecessors, especially by Captain Duperrey.

Major Rennel has described, with minute attention, the current which emanates from the south-east coast of Africa, and runs along the south bank of Agullas. According to the observations of Mr John Davy, the temperature of this current is from 4° to 5° *Cent.* (7° to 9° Fahr.) higher than that of the neighbouring seas. This high temperature is more deserving of the attention of navigators, from its being supposed to be the immediate cause of the cloud of vapour called the *Table-cloth*, which always envelopes the summit of the Table Mountain, whenever the wind blows from the south-east.

Temperature of the Sea at great depths.—It is not to be expected that a vessel such as the Bonite, dispatched on a special mission to the most distant quarter of the globe, will ever delay its progress for the express purpose of engaging in physical experiments. At the same time, when hours, and even entire days of dead calm are to be expected, especially when it is necessary to cross the line frequently, we conceive that this expedition will act wisely by providing thermometographical and sounding apparatus, for the purpose of sinking instruments in safety to the greatest depths of the ocean. There is now very little doubt that the inferior cold waters of the equinoctial regions are conveyed thither by submarine currents from the polar zones; but even the complete solution of this theoretical point, will be far from depriving the observations we now recommend of their interest. Who does not see, for example, that the depth at which the maximum of cold is found, and we may even say such and such differences of temperature, must depend, in every latitude, in a very direct manner, on the total depth of the ocean; so that we may expect that the latter may sooner or later be deduced from the value of thermometrical soundings!

Temperature of Shoals.—Jonathan Williams first made us acquainted with the fact, that water is colder on shoals than in the open sea. MM. de Humboldt and John Davy confirmed the discovery of the American observer. Sir Humphry Davy attributed this curious phenomenon, not to submarine currents, which, when arrested in their course, rise along the sides of banks and glide to the surface, but to radiation. By means of radiation, especially when the sky is clear, the superior beds of the ocean ought certainly to be greatly cooled; but every degree of cold, except in the polar regions, where the temperature of the sea is near zero, occasions an increase of density, and a descending movement in the beds cooled. Suppose an ocean without bottom, the beds in question sink to a great distance from the surface, and must slightly modify the temperature; but when the same causes operate on shallow water, the cooled beds accumulate, and their influence must then become very perceptible.

Whatever may be in this explanation, every one will perceive how much the art of navigation is interested in verifying the fact announced by Jonathan Williams, which some recent observations seem to contradict; how eagerly also meteorologists will receive the comparative measurements of the temperature of superficial waters in the open sea and above sand-banks; and, in particular, how desirable it is to determine by means of the thermometograph, the temperature of the bed of water which *rests immediately on the surface* of the shoals themselves.

Height of Waves.—The young officers of the Bonite will probably be greatly surprised if we assure them that none of their predecessors have fully answered the following questions: What is the greatest height of waves during tempests? What are their greatest transverse dimensions? What is the rate of their progress?

Observers have been usually satisfied with forming an estimate of the height. But, in order to shew how erroneous such estimations may be, and the influence which the imagination exercises in such matters, we may state, that navigators equally deserving of confidence, have assigned as the greatest height of waves, in some instances *five* metres, and in others *thirty-three*. What science now requires is not rude estimates, but actual measurements, of what it is possible to appreciate the exact numerical value.

These measurements, we are aware, are attended with great difficulties; these, however, are not insurmountable, and, at all events, the question is of too great interest to justify any hesitation about the degree of exertion necessary to solve it. We have no doubt that our young fellow-countrymen will themselves, upon reflection, devise some means for performing the operation which we require of their zeal; but a few brief considerations may assist in guiding them.

Let us suppose, for a moment, that the waves of the ocean are petrified and immovable; what, in that case, would be necessary to be done in a vessel likewise stationary, and placed in the trough of one of the waves, in order to measure the real height, —to determine the vertical distance of the crest of the wave and the trough? An observer would gradually ascend the mast, and stop at the point where the visual horizontal line, parting from his eye, seemed to touch the crest in question; the vertical height of the eye above the surface on which the vessel was floating, would be the height required. This operation, then, it would be necessary to attempt in the midst of all the commotions and disorders of a tempest.

In a vessel at rest, as long as the observer does not change his place, the elevation of his eye above the sea remains uniform, and can be very easily determined. In a vessel tossed by the waves, the rolling and pitching incline the masts sometimes to one side, sometimes to another. The height of the points of the top-masts varies incessantly, and the officer who has taken his station on the mast cannot ascertain the value of his vertical line, unless by the assistance of a second person placed on the deck, whose object it ought to be to follow the movements of the mast. If one limit their pretensions to ascertain this line within a third of a metre, for example, the problem appears to us completely solved, particularly if the moments chosen for observation are those when the vessel is nearly in her natural position, for then she is precisely in the trough of the wave.

It now remains to discover the means of determining whether the visual line resting on the summit of a wave be horizontal.

The crests of two contiguous waves are of the same height above the intermediate hollow. A visual horizontal line leaving the eye of the observer, when the vessel is in the trough, I suppose to be directed to the summit of the approaching wave; if this line be prolonged on the opposite side, it will likewise touch the summit merely of the wave already past. This last condition is necessary, and is sufficient to establish the horizontality of the first visual line; or with the instrument known under the name of the *Dip-sector*, having its ordinary circles provided with an additional mirror, there may be seen at the same time, in the same glass, and in the same part of the field, two images, situate at the horizon, one before, the other behind. The dip-sector, then, will shew to the observer, as he gradually ascends the mast, at what instant his eye reaches the horizontal plane touching the crests of two neighbouring waves.

We have supposed this observation to be made with all the precision that nautical instruments admit of. The operation will be more simple, and sometimes sufficiently exact, if the observer merely determine, with the naked eye, to what height he could ascend the mast, without ever perceiving, when the vessel is sunk in the trough, any other wave than the nearest of those approaching or receding. In this way the observation may be made by any one, and even during the severest tempests, that is to say, in circumstances where the use of reflecting instruments is attended with some difficulties, and when, moreover, perhaps no one but a sailor could venture with impunity to climb the mast.

The transverse dimensions of waves are easily determined, by comparing them with the length of the vessel as she passes through them; their rapidity is measured by well-known means. We have, therefore, in concluding the article, again to recommend these subjects of inquiry to the attention of the commander of the Bonite.

Visibility of Shoals.—The bottom of the sea at a given distance from a vessel, is more distinctly seen in proportion as the observer is elevated above the surface of the water: thus, when an experienced captain navigates an unknown sea filled with shoals, he sometimes places himself on the summit of the mast, in order that he may direct his vessel with greater security.

The fact appears to us so well established, that we have nothing to require of our young navigators regarding it in a practical point of view; but, by following the indications which we shall here point out, they may perhaps ascertain the cause of a phenomenon which affects them so nearly, and thence deduce more satisfactory means than casual observation has hitherto taught them to employ, for the purpose of detecting the situation of shoals.

When a pencil of light falls on a diaphanous surface, whatever may be its nature, a portion passes through it, and another portion is reflected. What is reflected is more intense in proportion to the smallness of the angle formed by the incident ray with the surface. This photometrical law is not less applicable to the rays which emanate from a rare medium, and meet the surface of a dense body, than to those which, moving in a dense body, fall on the surface of separation of that body and of the rare contiguous medium.

This being the case, let us suppose that an observer on shipboard wishes to perceive a shoal at a little distance—a submarine shoal, situate at thirty metres of horizontal distance, for example. If his eye is about the height of a metre above the sea, the visual line by which the light emanating from the shoal can reach him after issuing from the water, will form a very small angle with the surface of the fluid; if the eye, on the contrary, is very much elevated, suppose thirty metres, he will see the shoal under an angle of 45°. But the interior angle of incidence, corresponding to the small angle of emergence, is evidently less open than that which corresponds to the emergence of 45°. Under small angles, as has been seen, the strongest reflections take place; the observer, therefore, will receive a more considerable portion of the light which emanates from the shoal the higher he happens to be placed.

The rays emanating from the submarine shoal are not the only ones that reach the eye of the observer. In the same direction, and confounded with them, are rays of atmospheric light reflected exteriorly from the surface of the sea. If the latter were sixty times more intense than the former, they would totally conceal the effect. The shoal would not even be suspected, for it has been proved by the experiments of Bouguer, often repeated since, that the most experienced eye is not sensible of an augmentation of light of $\frac{1}{60}$. If there be a small proportion between these two lights, the appearance of the shoal will not be entirely lost; but it will be very feeble. When it is remembered that the atmospheric rays sent to the eye from the sea, have a greater degree of splendour if reflected under an acute angle, every one will perceive that two different causes concur to render a submarine object less apparent in proportion as the visual line approaches the surface of the sea, namely, on the one hand, the progressive and real weakness of the rays emanating from the object, which have to form its image in the eye; and, on the other, a rapid augmentation in the intensity of the light reflected from the exterior surface of the waters, or rather, if I may be allowed the expression, in the luminous curtain to which the rays issuing from the shoal must communicate their light.

On the supposition that the comparative intensities of the two superimposed pencils are, as every thing leads us to believe, the only cause of the phenomenon which we are now analyzing, we have it in our power to point out a better and more easy means of detecting submarine shoals, than has been hitherto followed. This means is very simple; it consists of looking at the sea, not with the naked eye, but through a plate of tourmaline cut parallel to the edges of the prism, and placed before the pupil in a certain position. A few words will render the mode in which the crystalline plate acts evident.

Let us assume that the visual line is inclined to the surface of the sea 37°. The light which is reflected from the outer surface of the sea under this angle, is completely polarized. Polarized light, as every natural philosopher knows, does not traverse plates of tourmaline suitably placed. A plate of tourmaline, then, may keep off entirely the rays reflected by the water, which, in the direction of the visual line, were mingled with the light emanating from the shoal, either obstructing it entirely, or at least greatly weakening it. When this effect is produced, the eye placed behind the plate, will receive only one kind of rays, those which emanate from submarine objects; instead of the two superimposed images, only a single image will be formed on the retina; and the visibility of the object which this image represents will thus be found greatly facilitated.

The entire and absolute obstruction of the light reflected from the surface of the water, cannot possibly take place but under an angle of 37°, because it is under this angle alone that it is completely polarised; but under angles from 10° to 12° greater or less than 37°, the number of polarised rays which the tourmaline can arrest, is still so considerable, that the same means of observation cannot fail to be attended with very advantageous results.

By engaging in the trials which we now propose to them, the officers of the Bonite will throw light on a curious question of photometry; they will probably confer on navigation a means of observation which may prevent many shipwrecks; and by introducing polarization into the nautical art, they will afford an additional proof of what those individuals expose themselves to, who unceasingly collect experiments and theories without any practical application of them, meeting every remonstrance with a contemptuous *cui bono?*

Water-Spouts.—Has electricity any influence in producing water-spouts? A distinct and categorical answer to this question would be possessed of great interest. The officers of the Bonite ought therefore to attempt to discover, when this phenomenon presents itself to them, whether it produces thunder and lightning.

Depressions of the Horizon.—The rather distinctly defined blue line, forming the apparent separation between the sky and the sea, to which sailors refer the position of the stars, is not in the mathematical horizon; but the degree at which it appears below it, and which is called *the depression*, may be calculated exactly, since it depends merely on the height of the observer's eye, and the dimensions of the earth. It is unfortunately not so easy to appreciate the effects of atmospheric refractions. It must even be said, that in the calculations of the tables of depression usually employed, the *mean refraction* only is taken into account, relative to a certain state of the thermometer and barometer. Officers of great skill, Captain Basil Hall, Captain Parry, and Captain Gauttier, have determined by observations, the errors to which navigators are exposed by following the common rule. It was sufficient for them to measure, either with the *dip-sector* of Wollaston, or with the ordinary instruments furnished with an additional mirror, and that in the most varied states of the atmosphere, the angular distance of one point of the horizon from the point diametrically opposite. Admitting, as we may be nearly at all times allowed to do, that the state of the air and of the sea are the same all around the observer, the difference of the distance measured at 180°, is evidently double the real depression of the horizon. The half of difference, compared to the depression of the tables, gives, therefore, the possible error of every angular observation of height made at sea.

The positive and negative errors observed by Captain Parry in the northern regions, have all been comprised between + 59″ and — 33.″ In the Chinese and East Indian seas, Captain Hall found the deviations greater; from + 1′ 2″ to — 2′ 58″. Finally, Captain Gauttier, in the Mediterranean and Black Seas, gives a still greater length, from + 3′ 35″ to — 1′ 49″. If it be recollected that the variation of a single minute in latitude nearly corresponds to a deviation of 2000 metres on the globe, every one will acknowledge how deserving of attention is the investigation which we have just mentioned.

By examining with care all the observations of MM. Gauttier, Basil Hall, and Parry, we come to the conclusion, *that the error of the* CALCULATED *depression is not* POSITIVE, *and that this depression does not exceed that observed,* EXCEPT *to the amount that the temperature of the air is higher than the temperature of the water.*

With regard to the negative errors, they present themselves indiscriminately in all the comparative thermometrical states of the sea and atmosphere, without the possibility of attributing these anomalies to any apparent cause, and, in particular, to the degree of the hygrometer.

MISCELLANEOUS OBSERVATIONS.

Rising of the Coast of Chili.—In the month of November

1822, after the earthquake which overturned the towns of Valparaiso, Quillota, &c., in Chili, great part of the country was found to be elevated from one to two metres above its former level. The earthquakes of 1834 appear to have been still more severe than that of 1822. It will therefore be of importance to examine whether they have not in like manner produced a sudden rise of the country. A beach on which the sea, by the effect of the tides, never ascends beyond one or two metres ought to furnish a multitude of appearances, such as "*embarcadères*," banks of oysters, muscles, and other shell-fish adhering to rocks, by means of which any doubt on the subject may be removed. A glance at the localities will do more in this respect, than the necessarily vague indications which we can furnish here. We can conceive, however, that we ought to mention the lake of Quintero, which communicates with the sea, as well fitted to afford indisputable proofs of changes of level. Recourse should likewise be had to the hydrographical charts of Vancouver, Malaspina, &c., for it is by no means probable that these risings would take place on the shore, without the bed of the sea participating in them.

Sudden or gradual elevations of the ground appear destined to perform such an important part in the history of the earth, that we must particularly invite the officers of the Bonite to take note of all recent phenomena of this nature that they meet with, and in an especial manner to direct their attention to the coast of Peru. *

Earthquakes.—According to an opinion pretty generally entertained in America, earthquakes are more frequent in certain seasons than in others. If this were fully ascertained to be the fact, it would be of extreme importance in the physics of the earth. A complete collection of the journals which have been published in Chili for twenty years, examined in relation to this

* At the moment when this sheet is going to press, I learn that some notes of Captain Fitzroy have just been read before a court-martial met at Portsmouth, for the trial of Captain Seymour, of the English frigate *Challenger*, shipwrecked on the coast of Chili. These notes, designed to account for the catastrophe, take notice of the changes which the currents have undergone near the port of Conception, since the earthquake of February 1835. Captain Fitzroy likewise states, that the island of Santa Maria has risen 10 English feet.

point, would certainly throw some light on the question. We recommend this object to the commander of the expedition, whether he may be inclined to perform the task during the voyage, or be contented with collecting materials for it.

To adopt popular opinions too hastily, is to run the risk of introducing into science, to its great injury, a multitude of confused notions, founded on phenomena imperfectly seen and inaccurately examined; but to reject these opinions without examination, is often the occasion of losing an opportunity of making some important discovery. From this consideration I do not hesitate to entreat our young countrymen to inquire, during their stay on the western coast of America, whether the phenomena which are said to have attended the earthquake which destroyed Arica and Saena on the morning of the 18th September 1833, have been observed in any other places. The following is an account of them by Mr John Reid, an English traveller.

"The continual baying of dogs, and braying of asses, announced the approach of danger. On the preceding day, the atmosphere had been of an alarming stillness. With the exception of a few rare gusts, coming sometimes from one side and sometimes from another, *and which were felt quite as well in the interior of an apartment as on the outside*; it might be said that during the whole of the day of September 18th, the immobility of the air at Saena was complete.

"The shocks had left a great number of empty bottles in the places which they occupied, but their corks were found scattered about in all directions.

"None of these empty bottles had been even overturned; others that were filled, on the contrary, were thrown out of their places and broken.

"The varnish which covered a new table belonging to Mr Reid, became so fluid, that the day after the earthquake the mahogany appeared surrounded with hanging glue.

"Some large jars of earthenware were sunk in the earth and contained water; and although the surface of the water was *three or four feet* below the mouth of the jars, yet a great part of the water was thrown out on the surrounding soil.

"At Saena it was remarked, that after a shock, whether slight or severe, all the dogs of the town went to quench their thirst at the first pool of water they could fall in with."

DECLINATION *and* INCLINATION *of the Magnetic Needle at Paris.*—On the 9th November 1835, at 1h 8′ P. M. we found that the northern extremity of the magnetic needle pointed to the west of the astronomical north, 22° 4′. On the 3d July 1835, at 9h. morning the inclination, was 67° 24′.

---

*Observations on the Sense of Touch, including an Analysis of Weber's Works on that subject.** By Dr GRAVES of Dublin.†

WEBER'S experiments on the sense of Touch are extremely interesting and original; some of them have been already published in English periodicals, but in so imperfect a manner as to furnish a very inaccurate and incomplete view of their results. If we touch the skin with the points of a compass one inch asunder, while the person so touched shuts his eyes, he at once perceives his skin to be touched in two places. By continually diminishing the distance between the two points, we finally arrive at a degree of approximation where the person feels his skin to be touched by but one body; he describes this body, however, as being a little longer in one direction than another, and it appears that this longer diameter corresponds with the line of junction between the two points of the compass. When these points are brought still nearer together, this inequality in the diameters is no longer felt, and the person has a defined perception of being touched by but one body. Now Weber has determined, by experiment, that the different portions of the surface of the body vary considerably in accuracy of touch, as *measured by the distance at which the points of the compass can be still distinguished from each other;* for it is evident that parts endowed with great power of touch, will continue to give notice of two points at a distance from each other, so small, that when examined at the same distance by less sensible portions of the

* *De Pulsu, Respiratione Auditu et Tactu. Annotationes Anatomicae et Physiologicae. Auctore Henrico Ernesto Weber. Lipsiae,* 1834.

† This interesting Memoir appeared in the March number of the Dublin Medical Journal, which did not reach Edinburgh until a month after its usual time.

skin, these two points excite but one sensation, and are by the touch erroneously judged to be but one. Thus the tips of the fingers and the point of the tongue were found to possess the most accurate sense of touch, for when the points were distant but half a Parisian line from each other (counting from the inner surface of each point), the feeling of the two distant points existed, and when they were within two-fifths of a line, although the person seemed to feel but one body, he nevertheless felt it to be longer in one direction than in another. The dorsum of the tongue was remarkably less sensible, for if the points placed in a line parallel to the median were less distant than three lines from each other, they were not felt to be distant. *Few persons who have not tried similar experiments will be prepared to credit the announcement of the very great difference which exists between the tactile accuracy of different portions of the skin.* On this subject the observations of Weber are quite novel, and open a new field for inquiry, not only to the physiologist, but to the practical physician and surgeon; for it is obvious that injuries or remedies applied to the skin must act with very different degrees of energy on parts so widely different in tactile sensibility from each other. I have repeated many of Weber's experiments, and confirmed his results. A little practice is necessary in order to accustom ourselves to judge concerning the sensation, compared as the points of the compass approach each other, and come within the *limits of confusion* (a term I have adopted to express the distance at which they produce the feeling of but one body, longer, however, in one direction than another). The sensation imparted is most curious. A few instances will suffice to prove the extent of the scale through which this *limit of confusion* ranges, when the *points are placed on the same horizontal line.*

Tip of the tongue 1 line. Margin of tongue, one inch from its tip, 2 lines. Skin on the zygomatic bone, 6 lines. Forehead, 6 lines. Hairy scalp, 8 lines. Middle of back, 12 lines. Near the upper border of the scapula, 18 lines. Inferior angle of the scapula, 24 lines. On the loins, 12 lines. Side of abdomen, 12 lines. Anterior surface of arm, 10 lines. Posterior do. 14 lines. Tips of the fingers, 1 line. Do. of toes, 3 lines.

The above results were obtained in examining the skin of others. The following, which Weber has arranged in the ascending scale as to the measurement of the *limit of confusion*, was taken from experiment on himself. He entitled it "Tabula graduum subtilitatis tactus in potissimis corporis mei partibus, quos per minimam distantiam crurum circini corpori impositorum, qua perpendicularis et horizontalis crurum situs, et intervallum interpositum sentiri poterat, metitus sum." The *limit of confusion* in the following table is therefore *the distance at which* the points of the compass could not only be perceived to be distinct from each other, but their direction, whether horizontal or perpendicular, could be judged of.

Tip of the tongue, ½ line. Inner surface of the finger tips, 1 line. Red part of lips, 2 lines. Inner surface of second phalanx of fingers, 2 lines. Outer or third phalanx of do., 3 lines. Top of nose, 3 lines. Inner side of extremities of metacarpal bones, 3 lines. Dorsum of the tongue one inch from its point, 4 lines. The portion of the lips which is not red, 4 lines. Edge of the tongue, one inch from its point, 4 lines. Metacarpal bone of thumb, 4 lines. Apex hallucis, 5 lines. Skin covering buccinator, 5 lines. Dorsum of second phalanx of fingers, 5 lines. Palm of the hand, 5 lines. Surface of eyelid, 5 lines. Centre of hard palate, 6 lines. Anterior surface of zygomatic process, 7 lines. Dorsum of the first phalanx of fingers, 7 lines. Outside of extremities of metacarpal bones, 8 lines. Mucous membrane of lips close to the gum, 9 lines. Posterior surface of its zygomatic process, 10 lines. Lower part of the forehead, 10 lines. Back part of the heel, 10 lines. Occipital skin, lower part, 12 lines. Back of hand, 14 lines. Neck, beneath lower jaw, 15 lines. Vertex of the scalp, 15 lines. Patella, 16 lines. Skin on the sacrum, 18 lines. Acromion, 18 lines. Glutaeus, 18 lines. Superior and posterior surface of forearms, 18 lines. Leg near the knee and near the foot, 18 lines. Dorsum of foot near toes, 18 lines. Sternum, 20 lines. Dorsal spine over five superior vertebræ, 24 lines. Cervical spine near occiput, 24 lines. Lumbar spine, 24 lines. Centre of cervical spine, 30 lines. Centre of dorsal spine, 30 lines. Middle of the arm where it measures most in circumference, 30 lines. Do. of the thigh, 30 lines.

This enumeration of relative distances, allowance being made for all probable inaccuracies in the experimental estimate of these distances, affords ample matter for reflection; and furnishes abundant proofs, if any were wanting, of the wisest adaptation of parts to the functions they are called on to discharge. Here is no unnecessary expenditure of tactile acumen, but a most rigid economy of the sense of touch, which is nowhere spread over surfaces indiscriminately, and without reference to their other physical qualifications. *This great difference was never before suspected to exist;* it was indeed known that the tops of the fingers, the tip of the tongue, and some other parts, enjoy the sense of touch in a pre-eminent degree, and are capable of judging much more delicately, concerning what they are placed in contact with, than other portions of the body. This was attributed partly to habit, partly to their shape, and many laid great stress on the facility with which these extremely moveable parts could be adapted and applied to bodies undergoing examination. Now, for the first time, has it been proved by Weber, that, quite independently of all these extraneous circumstances, the skin itself varies in the intensity of its tactile power; and that this arises not from the mere varying thickness of the epidermis, and general delicacy of conformation in the cutaneous tissue, but from an original difference in its organization. All these facts tend strongly to overturn the common hypothesis, that the sense of touch is diffused throughout the whole texture of the skin, and render it much more probable that it is performed only by certain small organs, extremely minute, and in size comparable to points, but differing much in their mode of distribution, being very crowded together and numerous in some parts of the skin, while in others they are more sparingly present, and are, as it were, thinly scattered. On this supposition alone, we can account for the signal differences in tactile discernment, which the different portions of the skin exhibit. The researches of Breschet, to which the attention of the English public was first drawn, by an able analysis by my friend Dr Costello, published in the Dublin Medical Journal for September 1835; these researches have rendered it certain, that the sense of touch is performed by a less simple apparatus than was generally imagined. M. Breschet considers that the nerve parts with its neurilema at the derma, as the optic nerve does in entering the sclerotic, and that the projecting papillæ take a new envelope from the outer surface of the derma; that the mere nervous pulp does not, of itself, constitute the sense of touch, but that, as in the sense of hearing or of sight, there is an apparatus, all the parts of which must be in unison to be perfect. If any one of the five constituent parts be wanting, touch cannot be exercised, and the derma, neurilema, and proper epidermic membrane, are to the papilla, what the complicated apparatus of sight and hearing are to the optic and acoustic nerves. The analogy goes farther, for the optic and acoustic nerves, on entering the structure of the eye and ear, undergo the same change as the tactile nerve entering the derma, with this difference, that the two former remain in their cavities, where light can penetrate to the one and sound to the other; but the nerve of touch must advance, as it were, to meet impressions. The following very curious phenomenon is recorded by Weber:—"If the points of a compass, distant from each other one or two lines, applied to the cheek, just before the ear, be then moved successively to several parts of the cheek, we shall find, on approaching the angle of the mouth, that the points will appear to recede from each other; this is produced by the great difference of tactile power in these parts. It is a general law, that the more sensitive portions of the skin regard any two points as farther asunder from each other, than equidistant points appear to be to a less sensitive portion. The same experiment may be tried by holding together the extremities of the fore-finger and thumb, and then passing the tips of both in a line from the ear to either the upper or the under tip; as they approach the latter, they will feel to the cheek as if they were becoming more and more distant from each other."

Another fact was observed by Weber—"If the legs of the compass be applied to two contiguous surfaces, enjoying the functions of voluntary motion, they will appear to be much more distant from each other, than when they are applied to one of these surfaces separately. Thus, if the points are distant half a line, they are not perceived to be distant when applied to one lip, but when one point is applied to the under lip, and another to the upper, they are at once felt to be two."

Another very remarkable conclusion announced by Weber deserves consideration: "Apply the legs of a compass to two portions of the skin, differing from each other remarkably, either in structure, in function, or in the use habitually made of them, and the legs will appear to be more clearly and distinctly felt than when they are applied to one and the same surface, even though it be the more sensitive of the two; thus the legs, when in contact, one with the inner surface and the other with the red outer surface of the lips, appeared much more distant from each other than when they were in contact with the red surface only, which has much greater tactile powers than the inner surface. The same observation applies equally to the neighbouring surface, differing much from each other in tactile power, viz. the margin and the dorsum of the tongue, the volar and the dorsal surfaces of the finger-points," &c. One result of Weber's experiments is of great importance in a physiological point of view:—" The tactile powers of any part of the skin are not, as is generally imagined, directly proportioned to its sensibility; thus the mammæ are easily tickled, and capable, when irritated, of producing great pain; in these respects they exceed any portion of the trunk, and yet the skin of or round the nipples is but very indifferently endowed with the faculty of touch, properly so called. Indeed the same remark applies to the arm-pits, the flanks, the soles of the feet, &c., and all ticklish parts of the skin in general, as they are possessed of a comparatively slight power of discriminating objects from each other by means of the touch. Who was ever made to laugh by tickling the points of his fingers? and yet they are possessed of a tactile accuracy far exceeding that of any other portion of the skin!"

This is a very curious subject of inquiry, and one not yet investigated. The reason of the matter is sufficiently obvious, for parts endowed with the greatest tactile acumen are necessarily much exposed, being so placed as to be brought with the greatest facility into contact with external bodies, consequently, if so disagreeable a sensation as that arising from tickling were easily induced by this contact, those parts would be almost useless as organs of touch. The experiments of Weber, considered with reference to the researches made by Breschet on the structural anatomy of the skin, render it extremely probable that the sense of touch, properly so called, resides in a peculiarly constructed apparatus, supplied with certain ramifications of the cutaneous nerves, while the function of sensation, comprising the power of perceiving painful or pleasing impressions, is much more generally diffused, and is the result of a much simpler organization. In fact, although the internal, mucous, fibrous, and serous surfaces, and the parenchyma of the different organs, are all capable of becoming actually painful, particularly when inflamed, yet it is very doubtful whether the sense of touch, properly so called, is ever exercised by those parts. No foreign substance is ever distinctly felt by the touch in the stomach and bowels; a sensation, painful or pleasing, is indeed excited by some matters immediately after they are swallowed, but all consciousness of their presence, by means of the sense of touch, soon ceases, and it cannot be again recalled by the utmost exertion of the will. A foreign substance lodged in the alimentary canal, or in the trachea, may give rise to the greatest possible degree of irritation; but though it thus acts upon the nerves of the parts immediately in contact with it, these nerves convey no idea to the sufferer of the shape or size of the body, or of any other of its physical qualities, concerning which we receive information through the medium of the sense of touch.

Weber's observations (pp. 67, 77) on the comparative tactile energy of the different portions of the trunk of the body, are extremely curious, and have disclosed a very remarkable difference between the sense of touch in the trunk and in the extremities. In the latter, where the points of the compass are placed across the axis of the limb horizontally, they are much more accurately distinguished than when they are placed in the longitudinal direction, or parallel to the axis of the limb (vertical), in other words, the *limit of confusion* is much sooner attained in the vertical than in the horizontal position of the points. Now, in many parts of the trunk the contrary obtains, and the vertical position is more accurate than the horizontal; this singular difference *Weber* explains by the different manners in which the nerves supplying the extremities and the trunk are distributed. The branches of the former generally run nearly parallel to the axis, while those of the latter pursue in most cases a transverse course; all parts of the trunk do not exhibit this difference. Whether this explanation is or is not admitted, the fact is undoubted. Our author next proceeds to shew what motion, whether it be of the touching organ or of the body to be touched, greatly augments the clearness and accuracy of the perception, a fact too familiar to require any elaborate illustration. As to the *idea of direction* which we derive from the sensation imparted to the skin by any minute substance, he justly observes, that it is always judged to be perpendicular to the surface of the skin at the point of contact. Of this there can be no doubt, and here we have a very striking analogy between the sense of vision and of touch, for it is a primary law, that rays of light impingent on the retina always produce a sensation, *i. e.* are seen in a direction perpendicular to that point; it would be well worth while examining whether the same law of perpendicularity is extended also to the ear. In the case of the eye this law is strikingly useful, as it enables many rays, originally diverging from the same luminous point, all to create a sensation in the same direction, although in converging they strike the retina from very different directions; in the eye all these perpendicular lines intersect at a common point, thence called the centre of visible direction, and this result derived from the spherical shape of the retina is attended with the most important consequences. No one has as yet attempted to investigate the question, whether any similar provision or contrivance exists with regard to the lines of direction, to which each part of the auditory nerve receiving vibrations refers sound; any given point of the hearing surface of the acoustic nerve receives impulses from the vibration essential to this sense, conveyed either through the fluid of the vestibule and semicircular canals, or through the solid bone surrounding the cochlea; the question arises, whether vibrations excited originally by the sounding body arrive by different routes simultaneously at the same point of the nerve, so as materially to reinforce and strengthen each other. Is there in this case any provision made to prevent vibrations, which arrive in different directions, from interfering with each other, with reference to the sensation they produce? Or are both, as impinging on a common point, referred to one common direction? If this were the case, the analogy between the perceptive properties of the retina and auditory nerve would be perfect, and nothing would remain to the philosophical examiner of the mechanism of the sense of hearing but to discover what relation these lines of common direction bear to the surface of the auditory nerve, and to each other; are they, as in the case of the retina, perpendicular to the nervous surface, and in what manner are they so arranged, that, in consequence of the shape of that surface in the convolutions of the internal ear, each line of direction resulting from the vibration communicated to any point, may be parallel to the various other lines of direction which result from vibrations, simultaneously communicated to all other points of the nervous surface?

These are extremely difficult questions, but it is by no means improbable that they may be hereafter satisfactorily resolved. But, to return to the sense of touch; in some parts of the surface, an exception seems to occur to the general rule of perpendicularity; thus, when a hair of the head is pulled, we can judge perfectly well of the direction in which it is pulled. The most obvious explanation of this fact, which refers to the discrimination of the line of traction to the bulb of the hair, Weber proves to be erroneous, and he shews that we judge of the direction in which the hair is pulled, by means of the muscles called into exertion to counteract the pull, and keep the head steady during its continuance. If these muscles be not called into play, which is the case when the head is held steadily by the hands of one person, while another, by surrounding the point in which the hair is pulled, with a firm pressure made by the fingers, thus prevents the least motion in the enclosed portion of the skin, then no matter in what direction the hair is pulled, the person cannot judge of it.

Weber's experiments on the faculty the skin possesses of estimating and comparing different pressures made on its surface, ought not to be altogether passed by in this report. One chapter he entitles, "*De Subtilitate Tactus in cognoscendo corporum pondere.*" If both the right and the left hand of the same individual are supported on cushions, and that he keeps his eyes shut, while unequal weights are placed one on each hand, he will, if the difference between the weights is considerable, be able to tell on which hand the heavier lies; slight differences of weight cannot be thus estimated, but they at once become perceptible if the hands be raised from the cushions; the muscles that now support the weight give great assistance in estimating its force. Thus we judge of the weight of any heavy body, partly by the pressure it produces against our surface, but chiefly by the quantity of muscular force it requires us to use in lifting or sustaining it. Weber has ascertained that in most men, the left side of the body and the left extremities enjoy a more accurate perception of weight than the right, so far as weight is estimated by pressure; of fourteen different persons experimented on, the left side of the body and the left extremities were found to be more sensible of weight, measured by pressure, than the right, in eleven; in two the contrary was observed, and in one only no difference between the sides could be detected. He offers no satisfactory explanation of this very remarkable and hitherto unobserved phenomenon, which is obviously of some value as making an original difference between the nervous power of the right extremities and right side of the trunk, as compared with the left, a difference which favours the idea, now indeed generally admitted, that we cannot explain the circumstance of man being right-handed and right-footed, except on the hypothesis of an original difference in the vital powers of the right and left halves of the body.

Weber next proceeds to make some observations, *De Subtilitate Tactus in sentiendo calore.*

I long ago maintained the opinion, that the perception of heat and cold is not a mere modification of the sense of touch. I am glad to find this view of the subject advocated by so high an authority as Lord Brougham, who, in his Discourse on Natural Theology, p. 3, note, remarks, that "there seems as little reason for arranging the sense of heat and cold under touch, as for arranging sight, smell, hearing, and taste, under the same head." Experiments made for the purpose of comparing the energy of this sense in different parts of the body, are attended with obvious difficulties; thus if the surface of the substance applied to the body be not exactly of the same extent in two cases, the result is not to be relied on, for, *cæteris paribus*, a larger body will feel hotter or colder than a smaller, and that in a very remarkable degree. Thus, let one vessel contain water heated to 98°, and another water at 104°; now if the finger be placed in the latter, and the whole of the other hand be immersed in the cooler, we shall be led to form a wrong judgment, and will pronounce the water at 98° to be hotter than that at 104°! In some cases the same error was made when the difference of temperature amounted to eight degrees, the hotter being at 106°. If the parts were kept a good while immersed, the person sometimes becomes sensible of his error, and judges rightly.

Weber has discovered a very remarkable fact, that *the left hand is more sensible of heat or cold than the right in most persons.* Thus, when the hands of a person lying in bed, and of exactly the same temperature, were plunged each in a separate vessel of hot water, the left hand was believed by the person to be in the hotter medium, even though the water it was in was really one or two degrees colder than the other. Weber has rendered it highly probable, that the greater sensibility which the left hand undoubtedly possesses in perceiving changes of temperature, is owing to the circumstance of its being covered, particularly on its palm, by a thinner epidermis, in consequence of being less used. Nothing is more striking than the accuracy of the skin in giving notice of changes of temperature, for a difference of one-third of a degree is detected clearly when the hand is immersed repeatedly and successively in two vessels of water, differing only so much in temperature. The skin detects best very minute changes of temperature when the medium examined does not fall short of, or exceed very considerably, the usual temperature of the body. Water at 98° can be much more certainly distinguished by the hand from water at 100°, than can water at 120° from water at 118.° As the ears perceive best a difference of tone in sounds, neither too acute or too bass, or immoderately loud, so the skin judges with most accuracy of medium temperatures, which produce no very violent or painful effect on its nerves. Weber is of opinion that the perception of temperature imparted to each nervous extremity in the skin, goes to unite itself to, and strengthen simultaneous impressions in the other ramifications of the same nerve, thus producing, by the conflux of a great number of impressions, a much stronger result and effect. This, at least, is certain, that a large conveys much stronger impressions than a small surface, and estimates changes of temperature with greater delicacy. Thus, if we place the fore-finger of one hand in water at 104°, and plunge the whole of the other hand into water at 102°, the latter will appear to us to be the warmer. If we plunge the finger successively into vessels containing hot water, we are unable to perceive very minute differences of temperature, which at once become perceptible when we use the whole hand. Nay, water, which can easily be borne by a single finger, will appear intolerably scalding to the whole hand. With regard to the power the skin possesses (by means of touch and its modifications) of comparing together two different temperatures or weights, various and multiplied experiments prove that this power is exercised with the greatest success when the perceptions compared are not simultaneous but successive. It is the same with the smell, taste, and hearing; apply to the tongue by means of camel's hair pencils, small portions of an acid and of a sweet substance; if the application of both be in quick succession, their taste is accurately distinguished and appreciated; but if they be applied simultaneously, the result is a less vivid perception of either, and a blending, as it were, together of the acid and the sweet. A similar result is obtained by applying the mouth of phials containing two different, but strongly odoriferous substances, to the nostrils; and musicians have long ago remarked, that when we wish to compare together two notes, it is done with much more accuracy by striking them in quick succession, than by striking them simultaneously. Vision appears to present an exception to the law which governs the other senses; for if we want to compare the lengths or the colours of any two lines, we place them close together, and look at them at the same moment. As Weber well remarks, however, the exception is here only apparent, for the truth is, *that we see nothing with perfect accuracy except its image falls on the retina at the extremity of the optic axis;* consequently, on examining two lines close beside each other, although we think we examine them simultaneously, yet we do not do so; our examination and comparison is made by causing the image of each to occupy the extremity of the optic axis several times in very rapid succession. The change in the position of the eye is here so light, and is performed with such ease, that we are unconscious of it.

Weber made many experiments on the accuracy of the sense of weight; of course this sense is more developed in some individuals than in others, and is capable of being rendered more exact by practice. Men accustomed to estimate weights by poising them in their hands, will distinguish perfectly between two only differing by a thirtieth part. In comparing two weights, one is poised, and then instantly the other *in the same hand;* the intervention of a few seconds between the poising of the first and of the second, does not prevent their accurate comparison. The interval may amount to twenty seconds, and yet a just estimate will still be made; but when it amounts to forty seconds, all accuracy is lost. The sight enjoys a still more accurate power of discrimination than the sense of weight, for a well practised eye will distinguish between two lines one hundred, and one hundred and one *lines* long respectively; in other words, will discover a difference amounting to one-hundredth part of the whole. According to the experiments of Delezennius, quoted by Weber, the sense of hearing is still more accurate, for a well practised musical ear will distinguish between two sounds differing from each other only $\frac{1}{320}$, calculating the number of vibrations the sounding bodies make in a given time.

A line can be perceived to be longer than another, even when an interval of fifty or sixty seconds elapses between looking at the first and at the second, provided that the lines differ $\frac{1}{10}$th in length. If they differ only $\frac{1}{20}$th, then an interval of thirty-five seconds may elapse without destroying our judgment; but if it be longer, our judgment becomes incorrect. When the difference between the lines amounts only to $\frac{1}{50}$th, an interval of three seconds between the examination of each is the longest that can be allowed without interfering with the correctness of the comparison. Having followed Weber with some accuracy through the body of his valuable treatises on the Touch, it may be worth while to dwell again, for a moment, on some of the chief conclusions he arrives at. We have a well established and definite idea of the distance of some parts of our bodies from others. Thus we feel the distance of the finger points from the wrist, and we remember that distance. It is the same with the arms as far as the elbow, and with the foot. These are all lengths which are firmly imprinted on the mind, and consequently there is a physiological reason for using them, as mankind have always done, as standards of measurement. When any two points on the surface of these parts are touched at the same time, we can with our eyes shut, and by means of the sense of touch alone, guess with great accuracy the distance the touched points are from each other, provided the points are situated somewhere near the sides or extremities of these parts, as at the tips or on the sides of the fingers. Here two points will be perceived to be distinct at distances much less than half an inch; but if the points be situated elsewhere, as on the back of the hand, then, although they be distant from each other half an inch, they will scarcely be felt as distinct, provided the line joining them is parallel to the long axis of the part; when it is transverse, the perception is much clearer, and continues at much smaller distances.

The discovery, that two equidistant points of contact on the same surface, excite very different ideas of the distance between them, according as the space lies lengthways on or across the limb, is one of the most striking and important which Weber has made, and can be most readily verified by experiment.

Nor am I aware that modern physiologists have obtained any results more curious than those relating to the different tactile acuteness enjoyed by different parts of the skin; a difference so great, that the points of a compass applied to the tip of the tongue, can be felt to be distant, when only distant half a line from each other; whereas, to use Weber's own words, "In medio brachio, in medio femore, in dorso scapulae, aliisque in locis sensus ille naturâ tam parum acutus est, ut apices circini $1\frac{1}{2}$ pollicibus Paris, a se invicem distantes, unam impressionem proferant, si nimirum ita ad has partes admoventur, ut linea utrumque apicem inter se conjungens secundum longitudinem brachii vel femoris posita sit."

On the Composition of the Water of the Lake Elton in Asiatic Russia, compared with the Water of the Ocean and with that of the Caspian sea. By Mr H. Rose.*

The Lake Elton, in the Steppe to the east of the Volga, two hundred miles to the south of Saratof is, on account of its extent, the most important of the salt lakes in the neighbourhood of the Caspian Sea. The quantity of the salt of commerce which is procured from it, is supposed to amount to nearly two-thirds of the whole quantity consumed in Russia. The lake is of an elongated form; the greater diameter stretches from east to west to the extent of about thirteen miles; the smaller, from

* Annalen der Physik, 1835.

north to south, reaches to about ten. It is so shallow as to be fordable; and, in truth, it is nothing more than a salt-water marsh which extends over great beds of salt, which are there constantly accumulating.

The water, which was analyzed by Professor Rose, was procured by M. de Humboldt, in his journey of 1829; it was preserved in a bottle well corked. Some crystals of sulphate of magnesia having collected in the lower part of the cork, these were afresh dissolved by heat, previous to the cork being removed; the water was then poured into a bottle having a glass stopper.

At the temperature of 54°. Fahr. the specific gravity of this water was found to be 1.27288; it did not exert any action on turnsol paper, and it supplied the following analysis:—

Magnesia,	10.22
Soda,	2.04
Potassa,	0.14
Chlorine,	16.97
Sulphuric Acid,	3.51

When, then, we subtract from the quantity of chlorine thus found, viz. 16.97, the proportions of it which are combined with the potassium and sodium, and affix to the remainder, viz. 14.55, the quantity of chloride of magnesium at 19.75 parts, we obtain as the analysis of this water—

Chloride of Potassium,	0.23
Chloride of Sodium,	3.83
Chloride of Magnesium,	19.75
Sulphate of Magnesium,	5.32
Water, and a very minute quantity of organized substance,	70.87
	100.00

The quantity of matter, therefore, which wholly resists the action of fire, and which is disengaged from the water, amounts to 29.13 per cent.

In this water there is no combination of bromine, nor of iodine, or at least these combinations exist only in the most minute quantities; no more are there any carbonates, nor phosphates, nor ammonia, nor lithium, nor metallic substances. Neither does any gypsum appear, nor any of the other salts of lime; which is the more surprising, since the brother of the author of this article, who accompanied M. de Humboldt in his journey, found crystals of gypsum in great quantities on the shores of the lake; and moreover, the whole of the specimens of crystalline salts which he procured, contained, on analysis, small quantities of lime, or a minute quantity of undissolved gypsum. But the absence of gypsum in this water, proves, without doubt, that it is insoluble in concentrated solutions of certain salts.

The water of lake Elton, then, is nothing more than a very concentrated brine, in which enormous masses of common salt have been deposited during a long space of time, and in which it still continues to be deposited during the summer months, because the water which enters it is not sufficient to replace that which it loses by evaporation. For little as the temperature of the water of this lake falls, great quantities of the crystals of sulphate of magnesia are found in it. Moreover, both its composition and its specific gravity must undergo great variations with the temperature. Accordingly, the banks of the lake in summer present only crystals of gypsum and of common salt; but in winter there are besides many crystals of sulphate of magnesia which are again dissolved in summer, so that pure common salt may be procured from the lake. The composition of this water is precisely what we should obtain by evaporating a very large quantity of common sea water at a very moderate temperature, so long as it deposited common salt.

The water of the other salt lakes, to the north-east and east of the Caspian, is somewhat similar to that of lake Elton, but has not exactly the same composition. That of lake Bogda is, according to Mr Erdman, altogether more like sea water. But of all the salt waters which have been analyzed, that of lake Elton most resembles the water of the Dead Sea. This latter has an inferior specific gravity, and also contains a smaller proportion of solid matter. According to the analysis of Gay-Lussac, undoubtedly of all others the most accurate, its specific gravity is 1.2283, at 63° Fahr.; and it contains 26.24 per cent of solid matter, which consists of metallic chlorides, viz., those of sodium, calcium, magnesium, and potassium, with some traces of gypsum. The absence of the sulphate of magnesia, and the presence of chloride of calcium, distinguish it from the water of lake Elton.

Next to those already named, the water of lake Ourmia near to Tauris in Persia, appears to be that, of those hitherto analyzed, which contains most salt. According to Dr Marcet its specific gravity is 1.16507, and it contains 22.3 per cent of solid matter, which consists of common salt, sulphate of magnesia, and sulphate of soda.

The similarity between the composition of these waters and that of the ocean and of salt pits is striking. The salts which are found in the waters of the ocean, according to known analyses, do not differ from those of which Mr Rose has demonstrated the existence in the water of Elton, except that it is generally admitted, after the principles laid down by Murray, that there is in sea water the simultaneous presence of sulphate of soda, and the chlorides of magnesium and calcium; although in all probability these salts, in extensive solutions, would mutually decompose each other. We cannot, it is true, determine with certainty in what manner the constituent parts of two salts are combined when these salts are of different solution or insoluble; but if it be admitted that the salts in the saline solutions are in the state of simple and not double salts, there is the greatest probability that in most cases the salts exist simultaneously in the solution, so that they separate by crystallization, by means of evaporation, at the ordinary temperature, or when it is somewhat elevated. That salt which is the least soluble is first separated. The propositions which Murray adduced in the maintenance of his hypothesis are not tenable. It cannot be denied that the sulphate of lime sometimes can maintain itself dissolved more easily in saline solutions than in an equal quantity of water; but usually it requires more time for its precipitation. In summer evaporation gradually effects the deposition from salt waters, first of gypsum, then of common salt; then of sulphate of magnesia, more or less pure, and sometimes mixed with common salt; and the chlorite of magnesia, or the most soluble of salts, remains in the sea water. Evaporation never produces sulphate of soda. The brother of the author of this article never found any trace of it on the banks of lake Elton; and in its waters are only found crystals of the sulphate of magnesia.

It is true that at different temperatures the combinations partly change in saline solutions in a singular manner, but still almost always from the sole cause that the salts are not then equally soluble. A mixture of common salt and sulphate of magnesia exhibits in this view the most extraordinary anomalies. If both be dissolved in a sufficient quantity of water, and if then, at the ordinary temperature, at least in summer, the water of solution be evaporated, the sulphate of magnesia and common salt are deposited. If the solution contain a large proportion of the latter and but little of the former, a part of the common salt is first deposited, then the other, while common salt still remains in solution; because in the heat of summer the sulphate of magnesia is to a very slight degree more difficult of solution than common salt. If the temperature descends to the freezing point, or ascends beyond 122° of Fahr., in both cases sulphate of soda is deposited, and it is formed from chloride of magnesia, because at the freezing point the sulphate of soda is, of the four salts which can be contained in the solution, viz. common salt, sulphate of magnesia, sulphate of soda, and chloride of magnesia, the most difficult to dissolve, and because at about 122° Fahr. it separates itself as an anhydrous salt. There is, therefore, ground for supposing that at the ordinary temperature the sulphate of magnesia and common salt simultaneously exist.

It was from these considerations that Mr Rose was induced in his indication of the constituent parts of the water of Lake Elton, to admit the combination with the magnesia of the whole quantity of the sulphuric acid which he had found, although this was in opposition to the opinion of Murray, which had also been adopted by Dr Marcet. And if some chemists in the analysis of sea-water by the manner of evaporation, and by treatment with alcohol, have discovered sulphate of soda, this is incontestably owing to the circumstance, that, in effecting the evaporation, they had employed a heat reaching almost to that of ebullition. Those who conduct the evaporation of this water at a temperature below 122°, cannot obtain any sulphate of soda.

As to the specific gravity of sea water, and the proportion of solid matter it contains, they both appear to be very variable. M. de Humboldt is the first who has attracted attention to these points; and he reckons that the proportion of solid parts varies from 3,22 to 3,87 per cent. Of recent researches, those of Mr Lenz, which have been made with the greatest care, and with due regard to those going before, fix the maximum of the specific gravity of the water of the Atlantic at 1,02856, and of the Southern Ocean at 1,028084.

From his numerous observations, Mr Lenz concludes that the waters of the Atlantic contain a greater proportion of salt than those of the Southern Ocean. The Indian Ocean, as the connecting medium of these two great masses of water, is consequently somewhat more salt on the side of the Atlantic than on the confines of the Southern Ocean; that is to say, on the west than on the east, whilst, at the same time, this difference is not considerable. One circumstance on this point is exceedingly curious, viz., the statement of Wollaston, that the water of the Mediterranean, fifty English miles to the east of the Straits of Gibraltar, at the depth of 670 fathoms gives, when heated to 302° of Fahr., a proportion of salt which amounts to 17,3 per cent., and a specific gravity of 1,1288; whilst more to the east, at two places a little removed, it possessed only the usual proportion of the waters of the ocean.

It is well known that enclosed seas have often a much smaller proportion of salt than others. This is especially notable in relation to the Baltic, in which this proportion diminishes as the distance of its communication with the German ocean increases. It even appears, according to the observations of Wilkie in 1771, that in the Sound, near to Landskrone, the west wind, and more especially the north-west, decidedly increases the specific gravity of its waters, whilst the east wind correspondingly diminishes it. The Caspian exhibits a similar phenomenon, but in a different direction. Here the water almost entirely loses its proportion of salt in the situation where the enormous watery mass of the Volga enters, and it is only after a continuance of southerly wind that the water becomes salt even at Astracan.

Mr Rose has analyzed some water which was given him by his brother, and which was taken from this sea in N. Lat. 45° 39, at the distance of 56 miles from the last of the islands which form the embouchure of the Volga, at a place where it was only three and a half fathoms deep. This water was so fresh that it could be drank like spring water. The analysis, although mperfect on account of the small quantity which was subjected to experiment, sufficed nevertheless to shew to how small a proportion the entrance of the Volga reduces the salt in the water at the northern part of this sea. The specific gravity, at 54° 5′ Fahr., was only 1,0013, and one hundred parts of water gave,

Chlorine,	0,0455
Sulphuric Acid,	0,0258
Lime,	0,0176
Soda,	0,0418
Magnesia,	0,0160

0,0455 of chlorine, combined with 0,0299 of sodium, the remaining 0,0016 of soda with 0,00205 of sulphuric acid; 0,02375 of sulphuric acid was then combined in the water in the form of gypsum, with 0,0169 of lime, so that 0,0007 of lime + 0,016 magnesia, were dissolved as bicarbonates. In 100 parts, then, of water, are dissolved—

Chloride of Sodium,	0,0754
Sulphate of Soda,	0,0036
Sulphate of Lime,	0,0406
Bicarbonate of Lime,	0,0018
Bicarbonate of Magnesia,	0,0440
Water, with a very small quantity of organized matter,	99,8346
	100,0000

The salts obtained by evaporation in a bottle of water, obtained near Astracan by the brother of Mr Rose, contained a much less proportion of sulphuric acid, as well as of lime and magnesia, the reason of which was, that during the evaporation, the earthy carbonates, as well as a great part of the gypsum, were precipitated, and could not easily be removed from the vessel. On the first action of the heat, the salt became black, and sank to the bottom; and 0,670 of a scruple, heated to a high temperature, afforded to Mr Rose 0,1275 of a scruple of sulphuric acid, 0,2593 of chlorine, 0,0549 of lime, 0,2320 of soda, and 0,0500 of magnesia. These parts formed, in the salt in the state of fusion, the following combination:—

Chloride of Sodium,	0,4293
Sulphate of Soda,	0,0080
Sulphate of Lime,	0,1322
Sulphate of Magnesia,	0,0692
Magnesia,	0,0265
	0,6652

Mr Rose remarks, that this analysis seems to indicate that a part of the magnesia perhaps exists in the water of the Caspian Sea in the state of sulphate of magnesia; but, in my analysis, I have admitted the combination of the magnesia with the carbonic acid, and that of the lime with the sulphuric acid, because it is impossible to determine, by the conclusions derived from analysis, what proportion of the two earths is combined with each of these two acids.

The water of that part of the Caspian which is nearest the mouth of the Volga is then comparatively free from solid contents. The excess of the specific gravity of the water of the Baltic, even where it is least salt, over that of pure water, is five times, and between the islands Laland and Femern more than seven times as great as the corresponding excess of the water which was analyzed from the Caspian Sea.

Eichwald also, in the account of his voyage on the Caspian, informs us, that the water, at the distance of four miles from the island formerly alluded to, was still so slightly brackish, that the vessel in which he sailed could there fill its water-casks. According to him, it is only in a lower latitude than 45°8′ N., where the water of this sea becomes deeper, that it becomes salt by degrees, and begins to assume the green colour peculiar to sea water; and, at the mouths of the great rivers, such as Terek and Ssoulak, it is found soiled, muddy, and scarcely salt.

The Caspian, not only to the north and north-east, but round all its banks, is surrounded with salt lakes. The water which these contain is so concentrated, that, as in lake Elton, the salt is deposited, simply by evaporation, in thick beds, which are easily broken up with iron bars. These lakes are very numerous upon the western side, especially upon the peninsula of Abscheron, near Bakou, where, according to Eichwald, they give out an odour like that of violets, and also upon the eastern side, especially round the Gulf of Balchan, upon the peninsula of Dardcha, and on the island of Tchelekan.

Farther Illustrations of the Propagation of Scottish Zoophytes. By John Graham Dalyell, Esq.* Communicated by the Author.

Overpassing for the present those nicer distinctions proposed by the arrangement and nomenclature of modern systems, I mean by *Zoophytes* to signify in general such animated products as resemble the form of plants, and consist wholly or partially of beings analogous to the hydra or polypus. No other name can be equally expressive of their nature.

Most of the lower animals propagate through the medium of an ovum, invested by an integument of different tenacity or induration, containing the elements of their progeny along with nutritious matter. This ovum is in itself inert, devoid of spontaneous motion, nor is it susceptible of being displaced by the struggles of the fœtus included, as may be seen during the internal revolution of certain planariæ and the vehement action of various sepiæ.

The development and escape of the fœtus from the ovum may sometimes ensue within the parent, as well as after extrusion of the ovum from its body.

Though necessary to premise these general principles, they are disturbed by many anomalies; whence the naturalist is compelled to resort to desultory illustrations in establishing analogies between certain animals now standing far apart in the *Systema Naturæ.*

I. A healthy and prolific Actinia commonly affixes itself to the side of its vessel horizontally, that full scope may be allowed to its organs. Then the distended tentacula of the lower half are occupied by the young in various stages; and interspersed among them, or separately, in other tentacula may be seen an opaque corpusculum of infinitely smaller dimensions, pursuing an irregular course through the liquid contents.

On amputation of the tentacula, one or more of these latter beings is discharged. All are opaque, red, solid, and of considerable specific gravity, and having a general resemblance to some

* The previous observations are contained in the Philosophical Journal for October 1834, and in the Report of the Proceedings of the British Association at Edinburgh.

of the *animalcula infusoria*. But under the microscope they prove of diversified form, many resembling flattened pease, some elongated or exhibiting irregular prominences, some almost spherical, others as if composed of two or even of three unequal spheres, and some which cannot be referred to any familiar figure. Short cilia or stout hairs environ their circumference, whatever be its outline, evidently instrumental, though not exclusively so, in their motion. This is also much diversified, being either progressive in a regular or irregular course, by describing an orbit; or when the body seems compounded of spheres, by horizontal revolution, as on an axis, at the point of union.

If extracted artificially, the corpuscula perish within a few days. But as the actinia is viviparous, discharging its young, having attained maturity, by the mouth, either in the course of nature or while disgorging the residue of its food,—so do the corpuscula sometimes, though very rarely, accompany them.

In this manner fourteen animated beings were produced at once by an *actinia equina* or *mesembryanthemum*, previously the parent of large and perfect progeny, seven or eight months in my possession. Six were the young with tentacula, and eight the corpuscula just referred to; which latter being separated and set apart in different vessels, could be identified with those extracted from the tentacula. All were sufficiently vivacious, sometimes moving, sometimes reposing; their excursions were longer or shorter, quicker or slower, regular or irregular, varying in space and duration, but always somewhat laboured, as if to overcome their own specific gravity, exceeding that of the surrounding medium.

Activity subsisted among the whole during eight days, when their shape had undergone some alteration. One in particular had become truncated in front; convex, clearer, and inflated behind, and exactly resembling the upper section of an ordinary sugar-loaf. Next, their motion relaxed. Though still mere specks of animated matter, inequalities might be recognised at one end, while the other was smooth and convex; and the sides had attained greater transparency. The cilia disappeared, and they became stationary. In eleven days from their origin, incipient tentacula were indicated in one, and in nineteen days eight or nine could be enumerated in another, which, now matured from its ciliated animalcular form, had affixed itself as a young actinia by the base. Others also adhered, and were acquiring a cylindrical shape, but their tentacula were of later evolution.

Thus, at an early stage, the actinia appears under a peculiar form, altogether different from its perfect shape; it is endowed with vigorous action, and provided with certain external organs, which are obliterated as it becomes stationary, and as others are unfolded.

II. Having passed *per saltum* to the *Alcyonium*, we find it consist of a compact gelatinous or fleshy matter, studded with innumerable cells sunk in its substance, which are inhabited by vivacious hydræ. Different species or varieties occur in the Scottish Seas, especially the *gelatinosum*, and a thin, green, flattened palmate kind, which has perhaps escaped the notice of naturalists hitherto.

A white, opaque, ovoidal, or nearly circular, flattened corpusculum, previously invisible, issues from the fleshy part of these products, whence it seems to be elicited, particularly by the influence of the light. On removal of a small specimen, that had already afforded many, from a dark situation to a moderate degree of light, at least 150 quitted their recesses within an hour. These beings are endowed with much greater activity than the corpuscula of the actinia; their courses are alike diversified: they swim through the water in all directions, regularly and irregularly; ascending to the surface, or descending to the bottom: pursuing a straight line, describing an orbit, or tumbling about among the neighbouring substances. Meanwhile, as if of soft consistence, their form alters, and the action of the cilia environing the body, is alternately accelerated and relaxed. At length, having become stationary, a margin diffuses around the body, and supervening transparence of the centre soon exposes an immature hydra within, which in nine or eleven days is displayed perfect from its cell. The inner surface of each tentaculum is now clothed by a double row of stout dark cilia in rapid motion, but in opposite directions, for as those of one side strike upwards, those of the other strike downwards. Farther diffusion of the basis adhering below forms additional compartments for other hydra.

The propagation of the Flustra *carbasea, foliacea,* and *truncata,* ensues after a similar fashion. A ciliated corpusculum, spherical, ovoidal or irregular, quits the leaf, pursues its courses in the water, becomes stationary, adheres, and a nascent flustra arises from the spot. Above ten thousand such corpuscula have been produced by a moderate sized specimen of the flustra *foliacea,* tinging the bottom of a vessel yellow from their multitude, and vitiating the water by their decay.

III. Many of the Sertulariæ propagate through the medium of a minute, flattened, smooth being, with a regular gliding motion, originating in the vesicles, which I have provisionally denominated *planula,* from its resemblance to the genus Planaria.

But a remarkable peculiarity occurs in the Sertularia *dichotoma,* or Sea thread, one of the most elegant and delicate of the tribe; where 1500 or 2000 living hydræ sometimes adorn a single specimen. Its vesicles are rarely found; when present they are in the proportion of about one to thirty hydræ, differing in nothing externally from the general aspect of others; replenished also by twenty or thirty greyish corpuscula, with a dark central nucleus. At first, all are immature and quiescent, but motion at length commences: the corpuscula become more distinct; several slender arms protrude from the orifice of the vesicle, which are seen in vehement action, and, after many struggles, an animated being escapes. But this has no relation either to the planula of the Sertulariæ, or the corpusculum of the flustra, alcyonium, or actinia. It might be rather associated with the Medusariæ. Before ascertaining its origin, I had named it *Animalculum tintinnabulum,* from its general resemblance to a common hand-bell, for the purpose of recognition. This creature is whitish, tending to transparency, about half a line in diameter; the body is like a deep watch-glass, surmounted by a crest rising from the centre, and fringed by about twenty-three tentacula pendent from the lip below. These are of muricate structure, or rough, and connected to the lip by a bulb twice their own diameter. The summit of the crest unfolds occasionally into four leaves, and four organs prominent on the convexity of the body appear at its base. When free the animal swims by jerks, or leaps through the water, or drops gently downwards; it is invited to move by the light, and it has survived at least eight days. Then it disappears, at least I have not been able to pursue its history longer. No other product has ever issued from the vesicles of the *Sertularia dichotoma* Fig. 1. enlarged.

IV. The only mode of propagation definitely ascertained of the *Hydra tuba*, the largest of the Scottish hydræ proper, is by the gemmation of the young from the body of the parent, and this is gemmation in the correct acceptation of the word. I kept a colony of these animals and their descendants during six years: numbers attained maturity; they fed rapaciously, grew and bred, succeeding at all seasons of the year. But, in February and March, the face or disk of some hydræ is invested by a pendulous flexible prolongation of an inverted conical form, obliterating the tentacula entirely. The apex being connected with the disk, this pendulous mass extends two or three lines in the course of time, and is gradually developed in twenty or thirty successive strata gradually broadening outwards. When more mature, the vehement clasping of extending arms at the extremity denotes, that each stratum is an animated being, which, after excessive struggling, is liberated, to swim at large in the water. This, also, may be associated with the Medusariæ. It is considerably larger than the preceding, two lines in diameter; of a whitish colour tending to transparence. The body resembles a flattened watch-glass; the margin dilating into from five to twelve horizontal broad flattened lobes; each cleft half-way down the middle, and with a black glandular looking speck in the centre of the fork. A crest resembling a quadrangular-clustered column rises from the convex surface of the body, and four organs may be sometimes observed on the same surface near its base. Motion is accomplished in jerks or leaps, somewhat as by the Medusæ proper, from percussion of the lobes on the water, the crest being downwards. Whether the pendulous mass or its individual parts be contained in one common involucrum, or in many specific integuments, is uncertain, but each of the animals composing it comes successively to maturity and departs. As the pendulous prominence disappears, the vigour of the hydra is restored, and the tentacula, liberated of the incumbrance effecting temporary obliteration, resume their natural form and functions. Weeks elapse in the course of this process, and during survivance of the animals. Fig. 2, 3. enlarged.

V. In addition to previous observations the *Tubularia indivisa*, I shall merely remark that here and in the *Tubularia polyceps*, a compound uterus of many cysts or sacs is generated on the face of the hydra. Each contains the elements of the progeny, expelled in a white, solid, spherical or ovoidal form, whence the external organs are speedily unfolded. But if accidentally retained, evolution succeeds within the cyst, and the tentacula are seen protruding from its orifice previous to expulsion. Probably, therefore, the progeny is expelled as a fœtus, invested by an amnios preserving its solid spherical or ellipsoidal form. Borne on the originating tentacula, it enjoys the faculty of transition, until reversed and rooted, which succeeds sometimes within two days of expulsion.

VI. The various species of Scottish Cristatellæ propagate under greater analogy to the higher animals, by an ovum with a harder shell and fluid contents. This, after escaping by decay of the *Cristatella mirabilis*, has required 200 or 230 days to mature the young, and 167 days for that of the *Cristatella lacustris*, when the ovum splits asunder horizontally to allow its exit.

The adult sertularia and the flustra enlarge by gemmation in its proper sense. Buds are generated at the extremities: each contains a hydra, which bursts the integument investing it, and protrudes from the cell to display its organs. Likewise new cells, formed by the enlarging leaf of the flustra, contain originating hydræ, which, reaching maturity, display their parts.

But from the preceding observations it appears exceedingly doubtful whether the name of *ovum* or *gemmule* can be appropriately applied either to the ciliated *corpusculum* or *planula* as some learned naturalists propose; perhaps each should be considered rather as an animal advanced a stage beyond that of the *gemmule* or *ovum*, and possibly bearing nearly the same relation to it as the larva or caterpillar bears to the ovum of insects. Neither can I at present view the various, protracted, interrupted, alternate motion and quiescence of these beings, otherwise than as resulting from animation; thence necessarily ascribing to them such characteristics as are inconsistent with the technical description and nature of an ovum. They repose permanently, also, on advancing still another stage, as the larva on entering its second state, which, though less conspicuous, does ensue with the actinia, and is amply demonstrated by the sertulariæ, flustræ, and alcyonia; and this repose terminating in apparent decay, is preparatory to the existence of that original hydra, which founds the specimen generating a thousand others from its basis.

Favourable conditions may enable more successful naturalists to prosecute the history of the two beings allied to the medusæ, and to ascertain whether any analogy subsists between the propagation from the sac on the face of the hydra of the Tubularia indivisa, and the propagation from the pendulous *nidus* on the face of the Hydra tuba.

Some of the *animalcula infusoria* may be probably found the progeny of zoophytes in an intermediate stage.

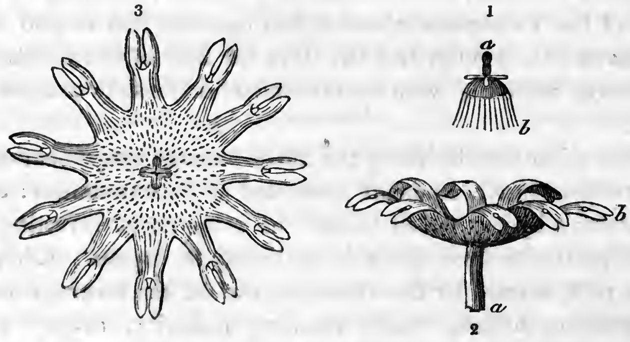


Fig. 1. Animal from the vesicle of the *Sertularia dichotoma*; crest *a*; tentacula, *b*. enlarged.

Fig. 2. Animal from the disc of the Hydra tuba ascending; crest *a*; arms *b*.
Fig. 3. The same quiescent—both figures enlarged.

Letter from M. Theodore Virlet *to M.* Arago, *on the Phenomenon of Dolomisation, and the Transformation of Rocks in general.*

I have just read in a journal an account of the discussion which took place at the meeting of the Academy of Sciences, on the 12th October, regarding M. de Buch's theory of dolomisation —a theory which must be allowed to be bold and ingenious, if reference be made to the period in which it has been advanced. It is known that I am far from acquiescing in all the opinions of this celebrated geologist; but as I did not hesitate, at a time when comparatively unacquainted with the science, to combat such of his opinions as I could not admit, I now think it due to the well known independence of my character, to support a fact advanced by him, which has been disputed, and which, besides, has direct reference to a question which has occupied much of my attention, viz. *the transformation of rocks in general.* This is one of the newest and most important questions of geology, and it ought to afford us the means of making rapid progress in the study of the composition of rocks, and lead to the solution of a multitude of facts hitherto regarded as inexplicable.

Some years ago, in describing to the Geological Society of France, the modifications occurring in a bed of hematitic iron, which I had an opportunity of observing near Sargans, in the Canton of St Gall, Switzerland, I was led, by the recollection of numerous analogical facts falling under my own observation, and what I have mentioned in my account of the Geology of Greece, to consider the phenomena of the transformation of rocks under two different points of view, and to divide the modified rocks into two very distinct classes.

1. Such as have been modified, whether by the prolonged action of heat, or by that of electro-chemical agents, or by both of these causes united, which have changed the combinations or primitive arrangement of the molecules in relation to each other.

2. Rocks which have been modified by chemical actions and reactions, with the assistance of foreign agents (such as the gases), which have acted directly upon them, and changed their primitive nature. It is in this class that dolomite ought naturally to be arranged.

The first manner of regarding the modification of rocks, which I was the first to propose, allows me to explain how certain beds placed in the midst of other beds, may be more modified than the latter, or may even undergo a complete modification, without the others, whether they were in contact, or even formed the lower part of the same deposit, experiencing any sensible change in their original state, and that without any of the beds being confounded with each other. The opinion which I advance on this subject, results as much from my own observations as from the manner in which I regard the first sandy deposits as being formed at the period when the waters began to condense on the surface of the earth; and although many may regard it as somewhat heretical, I have no doubt that it will soon be admitted by all accurate observers, viz. *That all stratified rocks, without excepting gneiss, the mica-slates, or clay-slates, &c. have been originally rocks of sediment, formed by mechanical aggregation, and that they have acquired the crystalline characters which now distinguish them, by a series of modifications which they have undergone posteriorly to their being deposited.*

It is conceived, on the contrary, according to the second kind of modification of rocks, that, in the greater number of cases, all the beds are confounded in such a manner as to present a single mass without distinct stratification; such, for example, as dolomite, certain deposits of sandstone and clay transformed into jaspers or trachytic porphyries, and other rocks which I have often had occasion to enumerate; for the chemical agents, by penetrating across a certain number of beds, or even the entire mass, have separated a part of the elements of the original rocks and substituted others, or else have formed new combinations, and finally united the whole mass of the deposit. It is to these considerations that I wish chiefly to direct attention, as they have reference to the phenomena of dolomisation.

I do not dispute, I even admit, that there are dolomites which should be called *primitive*, whatever may be their geological age; that is to say, which were the result of a series of simultaneous deposits of carbonate of lime and of magnesia, for magnesia was at least as abundant in nature as lime, particularly at the time when the old deposits were formed. These *primitive* dolomites, however, always present a distinctive character in being regularly stratified, like the other rocks to which they may be found subordinate; while the dolomites of which I now speak, and which I shall call *dolomites of transmutation* (such as are described by M. de Buch as occurring among the Alps, and many others which I could mention), are without stratification, presenting irregular masses, combined with other characters which individuals accustomed to observe modified rocks can seldom mistake. No one who has visited the dolomites of the Alps can entertain any doubt of the reality of the phenomenon of dolonisation, however difficult the explanation of it may at first appear, since chemistry teaches us that carbonate of magnesia is not volatile, or that it is decomposed at a red heat, an objection which has been urged by M. Thénard. It was in fact these considerations that caused me to be among the first to publish my doubts on the subject, at a time when no one undertook to ascertain, by chemical analysis, that the parts of the deposit which had not been modified, were not equally magnesian; that is, did not form beds of *primitive dolomite*—a circumstance which would have reduced the phenomenon of the change of limestones into dolomite, to a simple phenomenon of modified crystallization, analogous to that, for example, which has determined the change of the compact Jura limestone of Carrara, and that of the compact chalks of some parts of the Pyrenees, into granular limestones or statuary marble. One of my friends, M. Des-Génevez, who possessed a very extensive knowledge of chemistry, and whose early scientific works afford so much reason to lament his premature death, has unhappily been lost to the sciences before publishing the results of his chemical researches on dolomisation. These, he has many times assured me, had demonstrated to him that there existed an insensible passage from beds of unaltered carbonate of lime to dolomite or double carbonate of lime and magnesia. Thus the transformation of certain calcareous rocks into dolomite, posteriorly to their formation, appears to me to be a well established phenomenon, and requires, in my opinion, only to be properly explained in order to be admitted by all.

Who does not know how many facts, perhaps among the most difficult to comprehend previously, have already been explained by the excellent researches of M. Becquerel in electrical chemistry, and the important labours of M. Fournet, regarding the formation of veins? Numerous other facts, although not yet fully explained, have been brought forward and admitted without dispute. For example, I have proved that the emery of Naxos comes from veins, and consequently had been formed, like the greater number of specular iron ores, by means of volatilization and sublimation; yet the corundum and oxide of iron, the mixture of which constitutes emery, are not more volatile than the carbonate of magnesia, which forms the subject of dispute.

Since our chemical knowledge, then, does not always enable us to explain the phenomena whose existence we can prove, does it follow that we ought to call them in question? Has nature no mode of acting which surpasses our knowledge? And could she not proceed, for instance, by means of double chemical decomposition? On this supposition, the phenomenon will admit of easy explanation. It is well known that all the muriates are volatile, or at least susceptible of sublimation. Magnesia might then easily reach the state of a muriate, and occasion the formation of a soluble hydrochlorate of lime, which would be carried off by the infiltration of water; while the magnesia, on the contrary, would be combined with that portion of the carbonic acid set at liberty, and would thus serve to form the double carbonate of magnesia and of lime, which constitutes dolomite, properly so called. In this there is certainly nothing inadmissible or contrary to reason, inasmuch as the hydrochloric acid gas is one of the gases most frequently disengaged from volcanos, and the muriates ought to have been disengaged more abundantly in former times, if we admit, with geologists of the modern school, that the immense deposits of rock-salt which exist in saliferous formations, are deposited by volatilization, in the midst of the strata which they penetrate.

I am, therefore, of opinion that the modifications of rocks of the second class may henceforth be all explained by means of double decomposition—a process which has enabled one of my friends, M. Aimé, to produce in the laboratory crystallized specular iron ore, analogous to that of the Island of Elba, as well as pure iron equally well crystallized—a substance hitherto unknown to mineralogists; whence I conclude that the time is not perhaps far distant when we shall be able to produce with ease all the species of precious stones, without even excepting the diamond.

An Account of some Experiments and Observations on the Parr, and on the Ova of the Salmon, proving the Parr to be the young of the Salmon. By Mr John Shaw. Communicated by the Author.

That the facts which I am about to communicate regarding the Natural History of the Parr may appear not altogether undeserving of consideration, I may be allowed to observe, that my remarks have not proceeded from a hasty or imperfect observation, but from the experience of many years sedulously devoted to the study of the subject. The whole of my life, with the exception of a few years, has been spent on the banks of the streams where the salmon has been in the habit of depositing its spawn, and where, of course, the parr likewise abounded; my facilities of observation have, therefore, been as ample, as my efforts to discover the true history of this fish have been unremitting and laborious.

In opposition to the opinion held by most writers on the subject, I have always believed that the parr was the produce of the salmon, and that all attempts hitherto made to trace the early history of the latter fish have been unsatisfactory and fanciful.

To enable me to watch the progressive growth of the parr, I caught, on the 11th July 1833, seven parrs, and put them in a pond supplied with a stream of wholesome water. In this pond they continued to thrive remarkably well, taking flies, and sporting on the surface in fine weather in perfect health.

In the April following (1834) they began to assume a different appearance from what they had when first put into the pond, which was evident enough, even while they continued swimming at large in the water; but wishing to examine them more particularly, and at the same time to convince my friends of the fact of their having changed their appearance, I caught them with the cast-net on the 17th May 1834, and satisfied every individual present that they had assumed the perfect appearance of what is called the salmon-fry. They were now of a beautiful blue on the back, with a delicate silvery appearance on the sides, and at the same time the silvery scales came readily off on the hand when touched; the belly was white, and the average length was six inches, vertebræ sixty. There is one circumstance which occurred during the course of my experiments on these fishes which may be worth mentioning here, although I do not mean to attach much importance to the fact. About the first week in May, after they had undergone the change which I have mentioned, I was surprised to find that they were decreasing in number, and, on examination, I found that they had leaped out of the pond altogether, and were lying dead at a short distance from its edge. Whether this circumstance arose from their eager pursuit after the flies and other insects sporting on the margin, or whether they had leaped out of the pond in hope of making their escape to the sea, (it now being the period of their migration,) I shall not venture to offer an opinion. In March 1835, I again took twelve parrs from the river, which were distinctly marked with the characteristic bars of that fish. The average length of these individuals was about six inches. These also I put into a pond prepared for such experiments, supplied with a stream of pure water, and, according to my expectation, they had, by the end of April 1835, assumed the perfect appearance of the salmon-fry,—the bars being overlayed by the new silvery scales which the parrs of two years old invariably put on, previous to their departure for the sea.

From these experiments I think there can be no room to doubt, that the large parrs found in the river in autumn and the succeeding spring, (that is, at a period before the salmon-fry migrate,) are in reality the salmon-fry themselves, and that the small or summer parrs, (called by Dumfriesshire anglers the May parrs,) which still remain in the river, are those of one year old, and that they must remain another year, before they depart in the character of salmon-fry. The fact of the parr changing its appearance at a particular season, previous to its migration to the sea, is a circumstance which must be known to many who have made similar experiments, as well as to every angler of any observation, who has angled in rivers where this fish abounds, as there are many taken in April, before they have completely assumed their silvery coat, thus demonstrating the fact of themselves. The salmon fry has hitherto been erroneously supposed to grow to the size of six or eight inches in as many weeks, and to take its departure for the sea after this brief period has elapsed. The rapidity with which the parr of two years old assumes the appearance of the salmon-fry, has led to this error,—the parr taking about the same time to perfect its new dress, as the young salmon is supposed to take in attaining the growth at which it has arrived at the period of its migration.

As the continuance of the parr in the river for two years, as well as the fact that there are always two generations in the river at one time, is not generally believed, it may be necessary to detail the evidence by which the truth is established.

That this fish should not be found in the river in an earlier stage than the May or summer parr, had long appeared to me a very extraordinary and perplexing circumstance. I therefore made a very minute examination of the stream, where the old salmon had spawned the previous winter, and I found a very small but active fish in vast numbers, which I concluded must be the young parr, or samlet of that season. In order to prove the fact, I provided myself with a hoop, on which I stitched a piece of gauze, (the fishes being too small to be taken with any thing of a larger mesh,) and on the 10th of May 1834 I caught two or three dozen of them. They measured about 1 inch in length; their heads were large in proportion to their body, which tapered off towards the tail in form of a wedge, or small pin; and the small transverse bars peculiar to the parr were very distinctly marked. I then put them into two different ponds provided with a proper supply of running water, where they appeared to thrive remarkably well. In the succeeding May (1835), that is, after they had been in my possession twelve months, I again took a few of them from the pond, for the purpose of seeing what progress they had made. I found that they had increased in that time to the length of 3½ inches, on an average, and that they corresponded in every respect with the parr of the same age to be found in the river, but that they did not as yet indicate in the least degree the appearance of the perfect fry. Being convinced, nevertheless, from the result of my former experiments on the parr, that they would ultimately assume a different appearance, I retained them in the pond, and in the second week of May 1836 they had assumed (as in the former experiments) precisely the same appearance as the salmon-fry. They measure about 6½ inches in length, of a beautiful blue colour on the back; the sides bright and silvery; the dorsal and pectoral fins and tail tipt with black; the belly, ventral, and anal fins white; and on the most minute comparison with those at the time descending the river, not the slightest difference could be perceived, which proves what I have above stated, that the parr remains in the river two years before it assume the silvery appearance of the young salmon or smolt.

This active little fish, which has, as I have shewn, turned out to be the parr of a few weeks old, is nowhere to be found but in the streams in which the old salmon had deposited their spawn the previous winter, or in the immediate neighbourhood of these streams. Early in April 1835, I found them in the same stream as above stated, but so young and weak, from their having but recently emerged from the bed in which the egg had originally been deposited, as to be unable to struggle against the stream, where there was any considerable current. They therefore generally betook themselves into some small eddy, frequently where the horses, in passing the ford, had left the impression of their feet in the loose shingle of the stream, and in this shelter, where there was a slight current, a few inches deep, they continued to remain, with their little tails in constant action, till my approach was perceived, when they immediately darted under the stones and disappeared. At their first appearance, they are only to be found under the loose shingly stones two or three inches deep in water, with a very small current, and close to the edge of their parent streams, or on the gravelly bank sometimes called a scour or rack, which generally runs in an oblique direction across the river, and adapts the stream so admirably to the purposes of the breeding salmon, that there are few instances of such scours occurring in the river without their being much resorted to by the salmon during the breeding season. These little fishes, as above stated, are to be found in such situations during April, May, and even June; but as they increase in size and strength, they scatter themselves all over the shallower parts of the river, especially where the bottom is composed of fine gravel. The parrs of one year old, or summer parrs, are now to be found in every considerable little current, especially where the clear and shingly stream purls over the scour, and terminates in the head of the pool, where, during the whole summer, but more particularly in the months of August and September, they are caught by the angler with a small fly in prodigious numbers.

Having traced the progress of the little fish of one inch in length, through its several stages of the parr up to the period of its migration, I may now communicate the result of my experiments on the ova, to prove that I have not been mistaken in my opinion that this fish is produced from the ova deposited by the salmon the previous winter. On the 10th January 1836, I observed a female salmon of considerable size (about 16 lb.), and two males, of at least 25 lb., engaged in depositing their spawn. The spot which they had selected for that purpose was a little apart from some other salmon which were engaged in the same process, and rather nearer the side, although still in pretty deep water. The two males kept up an incessant conflict during the whole of the day, for possession of the female, and in the course of their struggles, frequently drove each other almost ashore, and were repeatedly on the surface displaying their dorsal fins, and lashing the water with their tails. Being satisfied that these were real salmon, there being at least ten brace of that fish engaged in the same process on the stream at the time, I took the opportunity of securing as much of the ova as I could possibly obtain. This I did three days after it was deposited, the males and female still occasionally frequenting the bed. The method by which I obtained the eggs was by using a thin canvass bag, stitched on a slight frame formed of small rod iron, in fashion of a large square landing-net, one person holding this bag a few inches farther down the stream than where the ova were deposited, and another with a spade digging up the gravel, the current carrying the eggs into the bag, while the gravel was in most part left behind. Having thus obtained a sufficient quantity of the ova for my purpose, I placed them in gravel under a stream of water where I could have a convenient opportunity of watching their progress. The stream was pure spring water. On the 26th February, that is, forty-eight days after being deposited, I found on close inspection that they had some appearance of animation, from a very minute streak of blood which appeared to traverse for a short distance the interior of the egg, originating near two small dark spots not larger at that time than the point of a pin. These two dark spots, however, ultimately turned out to be the eyes of the embryo fish, which was distinctly seen resting against the interior surface of the egg a few days previous to its exclusion. On the 8th April, which makes ninety days imbedded in the gravel, I found on examination that they were excluded from the egg, which was not the case a day or two previous. The temperature of the water at the time was 43°, the temperature of the water in the river 45°, and the temperature of the atmosphere 39°. On its first exclusion, the little fish has a very singular appearance. The head is large in proportion to the body, which is exceedingly small, and measures about *five-eighths of an inch* in length, of a pale blue or peach-blossom colour. But the most singular part of the fish is the appendage of a bag which adheres to the neck or upper part of the belly. It is of a conical shape, the base being attached to the fish. The bag is about two-eighths of an inch in length, of a beautiful transparent red, very much resembling a light red currant, and in consequence of its colour, may be seen at the bottom of the water when the fish itself can with difficulty be perceived. It also presents another singular appearance, namely, a fin or fringe, resembling that of the tail of the tadpole, running from the dorsal and anal fins to the termination of the tail, slightly indented. It does not appear that this little fish leaves the gravel immediately after its exclusion from the egg, but rather that it remains upwards of fifty days more under the gravel with this bag, as a supply of nourishment during that period, on the same principle as the umbilical supplies of other embryo animals. By the end of fifty days, or the 30th May, the bag disappeared, or rather contracted and formed the belly of the fish. The fin or tadpole-like fringe also disappeared by dividing itself into the dorsal, adipose, and anal fins, all of which then became perfectly developed. The little transverse bars, which for a period of two years (as I have shewn by experiment) are to characterize it as the parr, have also appeared. Thus, from the 10th January till the end of May, a period of upwards of 140 days, has been required to perfect this fish, and as yet it measures little more than one inch in length, and corresponds in every respect with the little fish on which I have made my former experiment, as well as with those to be found in the neighbourhood of the stream from which the ova were taken, and where at this moment they are to be found in great numbers. My observations have been confined to the two or three fish which I dug up the day on which I discovered they were hatched; the others remaining undisturbed, under the gravel, until of late, when by removing the shingle from off them, the perfect fish darted forth, and showed much activity. The circumstance of their being dug from the gravel a few days after their birth, does not appear to have affected their health or progress, as those which I now take from the shingle do not appear to be any farther advanced than those which have been subjected to the annoyance of my weekly inspection ever since the 8th April. It is evident, however, from the extreme difficulty the little fish has in dragging its unshapely magazine of sustenance along with it, that nature does not intend that it should be excluded from the gravel immediately on its exclusion from the egg; but rather that it should repose in its birth-place under the gravel, with this bag, as a source of supply until it be perfected.

It is well known to those who have paid any attention to the fact, that the salmon begins to spawn in autumn, and, in many rivers, continues to do so as late as the middle of February. It is also generally known, that the salmon-fry in most rivers migrate to the sea some time in May. But, be these facts as they may, I can speak with certainty so far as regards the river Nith, to which my experiments are chiefly confined. The salmon spawning, so late as the middle of February, and the ova remaining imbedded in the gravel for upwards of one hundred and forty days, proves that they cannot all migrate in May the same season the ova were deposited, as it must be the middle of June before these can possibly make their appearance; yet it must be recollected, that those which were deposited in the earlier part of winter, are now to be seen in thousands in the situations I have described, measuring, according to their respective ages, from one inch to an inch and a half in length, and certainly with no appearance of migrating.

The truth is, this little fish remains in the river all the first summer comparatively unobserved. It seldom takes the fly of the angler the first season, and when it does, it forms so contemptible a prize that it is generally returned to the river; its size not exceeding that of the common minnow. But by the time it arrives at the age of twelve or thirteen months, the larger parrs have disappeared as salmon-fry, which circumstance brings this fish more exclusively under the notice of the angler, and thence originates the provincial name of May or summer parr. All this considered, it must appear very extraordinary that it should never have occurred to the intelligent angler to inquire, what had become of the older generation of the parr, which was to be taken in such abundance in the beginning of April, while now (the end of May) there is no parr to be found in the river exceeding three and a half inches in length, and these comparatively scarce.

I have yet to communicate the result of another experiment made on the ova of the salmon, interesting as well from its novelty, as from its tendency to corroborate in part the results of the former. The experiment to which I allude was recommended by Sir Humphrey Davy, as having been tried by himself, as well as Mr Jacobi, with perfect success. On the 8th January 1836, I had an opportunity of practising this experiment, by taking a male and female adult salmon, whose apparent weight was from sixteen lb. to twenty lb. each, while in the act of spawning. Preparative to my experiment, I dug a trench in the gravel, through which I caused a current of water to flow two inches deep. I then had the two living fish held in this trench side by side, while with the hand I pressed the ova and seminal liquor out of their bodies, which mixed freely together in the stream. A few minutes after this process, I removed the ova to a stream of water to which no other fish had access, and on the 11th April, ninety-four days after the process of artificial impregnation, the young fish was excluded from the egg. They had precisely the same appearance in every respect as those in the former experiment, with the exception of being somewhat lighter in colour. Being, however, afraid of losing them in this open stream, I removed them into a pond, where I hope to be able to trace their progress still farther. It will appear from these experiments, that the ova artificially impregnated have taken four days more to perfect the embryo than those impregnated in the natural way. However, this slight difference in regard to time, may have proceeded from some imperfection in the process, or some little difference in the quality or temperature of the water.

That the female parr does not spawn is undeniable; and although the male parr of eighteen months old is to be found in the river, with the milt flowing from it in abundance, all the winter round till about the end of February, yet no instance has fallen under my observation of the roe in any female of the same age, or indeed of any age, having advanced to similar maturity. The female parr may be found in the river in autumn, in nearly equal numbers to the male, but the roe found in it has not the most distant appearance of approaching to maturity. I have also taken it at times during the whole winter, when the weather was mild, and still the roe had no appearance of advancing; and even up to the period of their migration, it is to be found with the roe in the same immature state. The male parr having got rid of the milt, and therefore having no strongly defined sexual distinction, has led many into the mistake of supposing all parrs to be mules. By a minute examination, however, there may be observed two very small reddish coloured vessels lying on each side of the swimming bladder, which runs from the neck to the vent; which vessels formerly contained the milt, but after its discharge have become very difficult of detection, from their minuteness and transparency.

It has sometimes been maintained that the female parr has been found in the act of depositing her spawn, but I am convinced that those who have held this opinion have mistaken the common trout for the par. Between the two the resemblance is so close (both being marked with the transverse bars) as to be a very probable source of error. If the parr was at all in the habit of depositing its spawn in the river, or in its tributaries, to which all small fish generally resort for that purpose, and if we consider that nine-tenths of the small fish found in this part of the river Nith are parrs, they must make some considerable appearance when assembled on the streams, and therefore could not escape observation. The apparent maturity of the organs of reproduction in the male parr, and the decided immaturity of the organs of the female of the same age, are facts on which I could not at present venture an opinion. However, from the specimens which I have at present in my possession of the parr three years old, that is, one year after assuming the dress of the salmon-fry, as I have already described, I am prepared to shew that it is *not a mature fish,* as it continues to increase in size at about the same rate it did previous to its disposing of the milt, that is, at the rate of three inches in twelve months, it being now nine and a half inches in length.* I have found this rule to hold good in regard to the growth of the parr, from observations on various individuals found in the river Nith. Assuming the parr to be one inch in length on its first exclusion from the egg, or rather from the gravel in which the egg is deposited, it will be found to measure at the same period the following year three and a half inches, and when two years old it begins to be distinguished by the peculiarities of the salmon-fry, and measures six inches. I do not mean to assert that the size of the Nith parr is to be the rule for other rivers, but as the parr in all rivers is admitted to be identical in species, a corresponding rule will be found to hold good, whatever stream the fish may inhabit. I have found the male parr on the streams in the winter during the time the old salmon were engaged in depositing their spawn, and on one particular occasion in January last, I caught upwards of three dozen of them. On examination I found these to be all males, and as they were all congregated on the bed or red which the salmon had formed, there is no doubt they were there for the purpose of feeding on the ova as well as the aquatic insects dug up by the female salmon, both of which I found in considerable quantities in their stomachs; but why there were no females found among them, appears to me very mysterious, as they are certainly to be found in other parts of the river the whole season, in pretty equal numbers to the male. I have had, on three different occasions, an opportunity of witnessing the return, or rather first migration, of the salmon-fry to the sea in small shoals. The first of these was in the first week of May 1831. I at that time had an opportunity of deliberately inspecting them, as the several shoals

* As this fish ought to have been in the sea twelve months ago, it cannot be expected to continue increasing in growth in its present artificial situation.

arrived behind the sluices of the salmon-cruive, and while they yet remained in the water, the indistinct transverse markings of the parr were still to be seen, especially when they happened to be swimming in a particular light, and occasionally as their positions happened to change, the bars became again imperceptible. I also examined a number of them in the hand, and by holding them at a particular angle to the eye, the bar could be distinctly perceived, but if the fish was viewed with the broad side presented directly to the eye, this peculiar appearance could not be recognised. Should all those methods fail, in shewing the characteristic bars of the parr on the salmon fry, it is only necessary to remove the scales from the sides, and the bars will be distinctly visible on the skin beneath. The next opportunity which I had of witnessing the salmon-fry in their progress towards the sea, was on the 3d May 1833. These had in every respect the same appearance as those I have already described. They passed down the river in families or shoals, of from forty to sixty and upwards, their rate of progress being about two miles an hour. The caution which they observed in descending the several rapids they met with in their journey was very amusing. They had no sooner come within the influence of the rapid current than they in an instant turned their head up the stream, and would again and again permit themselves to be carried to the very brink of the fall, and as often retreat, till at length one or two bolder than the others permitted themselves to be carried over by the current, when the whole, one by one, disappeared, and as soon as they had reached comparatively still water, they again turned their heads towards the sea and resumed their journey.

3d, The third and last opportunity I had of witnessing them migrating, was in May last (1836), when, as I have stated, I compared a few of them with those which had assumed the silvery dress of the salmon fry, after being in my possession two years, in the character of the parr. The river, during this month, being remarkably low, afforded me an opportunity of ascertaining more accurately the time during which they have continued to migrate, which has been nearly the whole of the month, but more especially during the second week, in the course of which the shoals were both larger and more frequent in their successive arrivals. They had all the appearance of the former, averaging from six to seven inches.

It must be admitted that my experiments and observations on what I consider the young salmon have been confined to a particular locality, and therefore may not be entitled to the same consideration as the opinions of a person of more extensive opportunity of research; yet, as the parr is allowed to be the same in all rivers, and, as it is universally admitted to frequent those streams only to which the salmon has access, there can be little objection to my facts on the score of confined locality. That the small parr of one inch in length, found in April amongst the loose gravel on the edge of the streams where the salmon had spawned the previous winter, is the young of the salmon, cannot, in my opinion, admit of a doubt. The facts which I have related in the foregoing pages will supply the deficiency of information so much complained of by most authors in treating of the early history of the salmon, that is, the progress of the fish from the egg up to the period of its migration; and when it is recollected that the vertebræ of the parr and those of the adult salmon correspond, and also that the parr in its new dress cannot be distinguished in any respect from the salmon fry, it may fairly be concluded that they are identical. That the parr is not the produce of the common trout must be evident to every one, from the circumstance of its changing its appearance at a stated season, and then migrating to the sea, a thing the common trout is never known to do. The diversity of species is also corroborated by the fact, that the common trout has never been observed to spawn in the stream where my observations were made. Unless the trout be of large size, which is not the case in the Nith, it uniformly prefers the tributary burns for depositing its spawn. Neither am I aware of an instance of the sea-trout or the herling spawning on the stream alluded to, these fish having generally proceeded early in the autumn either towards the source or into some of the tributaries.

A series of specimens illustrative of Mr Shaw's interesting observations on the natural history of the parr has been presented by him to the Royal Museum of the University.—Ed.

Abstract of a Meteorological Journal for the year 1835, *kept at the Elgin Institution.*

1835.	Half-past 8 o'clock A. M.			Half-past 3 o'clock P. M.			Half-past 8 A. M.	Direction of the wind at half-past 8 A. M., and the number of days that it blew in such direction in each month.														
	BAROM.	Thermom. Interior.	Thermom. Exterior.	AROM.	Thermom. Interior.	Thermom. Exterior.	Rain-Gauge.	N.	N.NE.	NE.	E.	E.SE.	SE.	S.SE.	S.	S.SW.	SW.	W.SW.	W.	W.NW.	NW.	N.NW.
January,........	29.614	40°.5	36°.2	29.646	41°.5	38°.5	2.500	...	...	1	...	...	4	...	1	1	4	...	15	...	5	...
February, ...	29.171	42.3	40.6	29.176	42.9	42.7	2.177	...	...	...	...	1	3	1	2	...	13	2	2	1	2	1
March,..........	29.572	41.8	42.6	29.651	42.7	44.6	1.607	...	1	...	...	1	3	...	1	4	14	...	2	...	4	1
April,...........	29.873	45.4	45.5	29.776	46.9	47.0	3.055	1	...	1	1	1	1	...	4	...	9	...	5	...	2	5
May,............	29.673	50.6	48.9	29.624	51.1	51.3	2.195	3	...	1	...	...	4	6	1	...	3	...	3	2	3	5
June, ...	29.857	59.1	59.2	29.822	56.6	60.7	1.215	2	...	...	...	...	1	2	1	...	5	...	1	6	9	3
July,............	29.807	60.8	60.8	29.709	62.0	63.1	1.003	1	...	1	...	...	5	4	3	...	8	...	4	1	4	...
August,.........	29.664	61.5	61.0	29.647	62.8	62.9	1.170	...	...	...	2	1	...	10	...	...	7	...	2	...	2	7
September,......	29.263	55.0	54.5	29.310	56.6	57.0	2.980	..	...	...	2	1	3	13	1	...	6	...	1	...	1	2
October,.........	29.379	47.5	44.2	29.334	48.1	47.0	3.013	1	...	...	...	...	...	9	...	1	13	...	3	1	3	...
November,......	29.526	44.7	40.4	29.596	45.0	42.5	2.010	1	...	...	5	...	1	7	8	...	6	...	...	...	1	1
December,......	29.766	41.8	37.2	29.831	42.2	38.4	1.155	...	...	...	1	...	2	3	8	...	13	...	1	...	3	...
Means,...	29.597	49.2	47.6	29.593	49.8	49.6	24.080 (Sum)	9	1	4	11	5	27	55	30	6	101	2	39	11	39	25

The *Barometer* used for making the observations inserted in this journal, is graduated by means of a vernier to thousandths of an inch. It hangs in a room fronting the north, where there is not the least current to produce undulations in the atmosphere around it; and accordingly, the circumstances attending each observation, in as far as regards the apartment where the instrument is kept, must be invariable. To the Barometer there is attached a *Thermometer*, which in the journal is called the *Interior* one. It is in the same favourable situation as the Barometer. The *Exterior Thermometer* is placed in the outside of the window of the same room, and is so situated as not to be affected by the sun's rays, either in the morning or afternoon, except in the longest days of summer; and, even then, it is several hours after being so affected, before the morning observation is taken. In the afternoon the sun's rays have no access to it, either directly or by reflection, till long after the observation is made. The *Rain Gauge*, which has its receiver about seven feet above the foundation of the building, is graduated to the thousandth part of an inch. The *direction* of the *wind* has been only taken once a-day, owing to the impracticability of combining two observations in such a manner as to be inserted with neatness in the summary of each month. But even by one observation, regularly taken at the same time every day in the year, we may form a tolerably accurate estimate of the winds that predominate in this quarter. And, on inspecting the journal, it will be seen at once that the SW., the mildest in this country, has blown 101 days out of the 365, being very nearly a third of all the rest. To this circumstance, there is little doubt, is to be attributed in a great degree, the superior balminess of the air in the lower parts of Morayshire, so universally talked of by those that cross the Spey, after traversing Banff or Aberdeenshire. The mean density of the atmosphere, by one year's observations, is found to be 29.595. The mean temperature in the shade, 48°.6. The mean monthly depth of rain, 2.006 inches.—Elgin Institution, 26*th January* 1836.

Kinfauns' Meteorological Table for 1835.

Extracted from the Register kept at Kinfauns Castle, North Britain.
Lat. 56° 23′ 30″—Above the level of the Sea 150 feet.

1835.	Morning ¾ past 9. Mean Height of		Evening, ½ past 8. Mean Height of		Mean Temp. by Six's Therm.	Depth of Rain in Garden.	No. of days	
	Barom.	Therm.	Barom.	Therm.			Rain or Snow.	Fair.
January, ...	29.738	35°.806	29.735	36°.032	36°.386	1.55	9	22
February,...	29.273	39.786	29.289	38.500	39.786	2.15	17	11
March, ...	29.612	41.290	29.629	38.290	40.516	2.00	15	16
April, ...	29.853	46.867	29.866	44.233	45.367	.50	6	24
May, ...	29.613	51.387	29.613	46.548	49.000	2.90	22	9
June, ...	29.837	58.633	29.844	54.333	56.267	.80	11	19
July, ...	29.735	60.774	29.745	55.129	58.064	1.20	14	17
August, ...	29.732	61.774	29.734	57.129	59.839	3.65	13	18
September,..	29.348	54.567	29.326	52.800	53.200	4.15	18	12
October, ...	29.413	45.032	29.446	44.484	44.645	2.30	12	19
November,..	29.628	41.633	29.631	40.233	41.100	2.80	18	12
December, ..	29.851	38.484	29.861	37.419	37.903	1.60	9	22
Average of the year,	29.636	48.003	29.643	45.427	46.840	25.60	164	201

ANNUAL RESULTS.

MORNING.

Barometer. Observations.		Wind.		Thermometer.	Wind.	
Highest,	2d Jan.	N.	30.50	11th August,	W.	70°
Lowest,	3d Feb.	W.	28.10	19th January,	N.	25°

EVENING.

Highest,	2d Jan.	N.	30.52	10th June,	S.	65°
Lowest,	24th Feb.	SW.	28.38	17th January,	W.	23°

Weather.	Days.	Wind.	Times.
Fair,	164	N. and NE.	37
Rain or Snow,	201	E. and SE.	84
		S. and SW.	119
	365	W. and NW.	125
			365

Extreme Heat and Cold by Six's Thermometer.

Coldest, . . . 21st January, ... Wind W. . . . . 20°
Hottest, . . 18th August, ... do. W. . . . . 78°
Mean Temperature for the year 1835, ... 46°84

Results of Two Rain Gauges.

	In. 100ths.
1. Centre of Kinfauns Garden, about 20 feet above the level of the sea, ...	25.60
2. Square Tower, Kinfauns Castle, 180 feet, ...	25.75

Meteorological Observations made at Castle Toward.

GENERAL MONTHLY RESULTS for the Years 1834 and 1835,

1834.	Atmospheric Variations. Wet and Stormy.	Fair.	Frosty.	Snow & Hail.	State of the Wind, taken at Noon. E.	SE.	S.	SW.	W.	NW.	N.	NE.	Atmospheric Pressure. Lowest.	Highest.	Mean.	External Thermometer in Shade. Lowest.	Highest.	Mean.	Fall of Rain.
																°	°	°	Inches.
January,........	21	10	2	...	5	2	5	4	9	4	1	1	28.90	30.40	29.65	36	48	42	10.00
February,.......	13	15	3	4	1	3	3	7	3	8	2	1	29.32	30.45	29.88	35	47½	41¼	3.50
March,..........	15	16	7	4	3	1	5	5	7	5	5	...	29.11	30.65	29.88	30	49	39½	4.30
April,...........	10	20	1	...	11	1	5	2	2	4	3	2	29.41	30.56	29.98	38	55	46½	0.40
May,............	12	19	...	...	12	...	12	3	1	3	...	...	29.29	30 70	30.00	45½	66½	56	2.75
June,...........	16	14	...	1	3	1	12	6	2	6	...	...	29.39	30.50	29.94	53	62½	57¾	4.60
July,............	14	17	...	...	15	2	3	4	4	1	2	...	29.60	30.48	30.04	50½	75¼	62⅞	3.40
August,.........	13	18	1	...	9	2	10	6	1	2	1	...	29.39	30.30	29,84	51¼	70	60⅝	3.50
September,.....	14	16	2	...	12	1	4	4	1	3	2	3	29.50	30.59	30.04	51	60	55½	3.30
October,........	20	11	2	4	1	1	6	8	2	7	6	...	28.92	30.69	29.80	37	58½	47¾	5.05
November,......	19	11	5	...	5	2	2	4	6	3	6	2	28.82	30.62	29.72	37½	53	45¼	4.80
December,......	16	15	6	1	2	4	5	2	7	7	3	1	28.81	30.83	29.82	34	57	45½	3.90
	183	182	29	14	79	20	72	55	45	53	31	10							49.50
1835.																°	°	°	
January,........	17	14	7	6	2	2	7	4	4	6	6	...	29.11	30.85	29.98	27½	49½	38½	2.60
February,.......	20	5	2	10	1	...	2	5	8	10	2	...	28.70	30.40	29.55	32½	50½	41½	6.00
March,..........	11	18	4	7	7	...	8	3	2	8	2	1	29.00	30.72	29.86	35	49	42	4.00
April,...........	20	10	8	2	6	...	2	6	2	10	3	1	29.62	30.53	30.07	38½	52	45¼	1.70
May,............	19	12	6	1	13	1	5	3	4	2	3	...	29.47	30.30	29.08	35	65	50	5.35
June,...........	10	20	...	2	10	1	...	1	8	6	3	1	29.39	30.54	29.96	41	79	60	1.70
July,............	18	13	...	...	10	3	5	5	4	4	...	...	29.52	30.38	29.95	46	72	59	4.40
August,.........	11	20	...	...	14	2	10	...	1	2	1	1	29.50	30.33	29.91	48½	72	60¼	4.10
September,.....	15	15	...	1	8	1	12	1	2	2	4	...	29.07	30.27	29.67	42	65	53½	5.60
October,........	18	13	3	1	5	...	7	4	1	8	4	2	28.92	30.42	29 67	35	57	46	3.50
November,......	17	13	3	1	5	3	6	5	1	2	4	4	29.01	30.62	29.81	29½	53	41¼	6.15
December,......	16	15	10	...	6	1	4	5	3	1	11	...	29.21	30.67	29.94	30	49½	39¾	4.00
	192	168	43	31	87	14	68	42	40	61	43	10							49.10

Abstract of Register of the Thermometer, Barometer and Rain-Gauge, kept at Regent Terrace, Edinburgh, in 1835.

1835.	Thermometer.		Register Therm.		Barometer.		Quantity of Rain.
	10 A. M.	10 P. M.	Min.	Max.	10 A. M.	10 P. M	Inches.
January,	38°.84	37°.48	34°.13	42°.29	29.731	29.770	1.08
February,	41.61	39.07	34.61	44.64	29.398	29.443	2.48
March,	42.77	39.19	35.00	46.13	29.643	29.705	2.28
April,	47.97	43.53	37.87	51.40	29.928	29.923	.79
May,	52.29	46.35	42.77	55.16	29.656	29.661	2.04
June,	57.90	52.07	46.80	62.50	29.897	29.906	0.02
July,	61.29	55.23	49.87	65.23	29.805	29.803	1.37
August,	62.19	57.55	50.06	67.13	29.719	29.737	1.99
September,	55.00	51.90	45.07	59.37	29.455	29.395	5.43
October,	47.32	44.23	40.42	50.84	29.508	29.548	2.09
November,	43.90	43.10	38.33	46.47	29.723	29.639	2.76
December,	40.39	39.19	34.93	42.68	29.893	29.897	1.89
Annual Mean	47.05		46.904		29.70		25.22

Meteorological Table, extracted from a Journal kept at Carlisle, (above the level of the Sea 45 feet). By Mr Joseph Atkinson.

1835.	Mean Height of Barometer.		Mean Height of a Register Therm.		Quantity of Rain.	No. of Days	
	10 A. M.	4 P. M.	Min.	Max.		Rain or Snow.	Fair.
January,	29.95	29.95	34°	40°	3.1125	22	9
February,	29.56	29.57	38	45	3.9405	22	6
March,	29.80	29.81	37	47	2.5875	13	18
April,	30.02	30.01	43	54	0.7375	19	11
May,	29.74	29.74	45	59	2.2350	23	8
June,	29.96	29.95	51	68	1.3200	14	16
July,	29.92	29.91	53	70	2.9783	14	17
August,	30.01	30.00	56	72	2.5490	15	16
September,	29.55	29.53	49	62	5.2125	24	6
October,	29.74	29.77	44	53	4.3650	22	9
November,	29.89	29.87	40	47	3.3197	27	3
December,	30.15	30.13	36	44	1.9718	19	12
Average of the Year,	29.925	29.92	43	55	34.3293	234	131

JOURNAL OF THE WINDS.

	No. of days.		No of days.		No. of days.		No. of days.
N.	8½	E.	14	S.	32½	W.	46½
N.NE.	12½	E.SE.	1½	S.SW.	12	W.NW.	5½
NE.	32	SE.	16½	SW.	60½	NW.	29
E.NE.	17	S.SE.	20	W.SW.	52	N.NW.	5

State of the Barometer, Thermometer, &c. at Whitehaven for 1835.

1835.	Barometer.				Thermometer.						Pluviometer.		Prevailing Winds.	No. of Rainy Days.	Stormy Days.	Snowy Days.	Thunder and Lightning.
	Max.	Min.	Mean.	Range.	Max.	Min.	Mean.	Range.	Mean Increase of Temperature.	Mean Decrease of Temperature.	Quantity of Rain.	Quantity of Snow.					
	Inches.	Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.	Inches.					
January, ...	30.79	29.10	30.093	1.69	49.5	21.0	37.855	28.5		4.806	4.643		S.	16	2	4	
February, ...	30.44	28.91	29.768	1.53	50.0	25.5	40.5	24.5	2.645		7.597		W.	25	14	1	
March, ...	30.67	28.99	29.970	1.68	55.5	28.5	41.630	27.0	1.130		4.247	1.504	SW.	17	11	5	1
April, ...	30.53	29.81	30.200	.72	59.5	31.0	47.100	28.5	5.470		1.340	.027	E.	18	7	2	2
May, ...	30.27	29.60	29.951	.67	64.5	35.5	51.0	29.0	3.900		3.964		SW. & S.	22	1		
June, ...	30.50	29.51	30.167	.99	77.0	43.0	58.540	34.0	7.540		1.601		NW.	9	2		
July, ...	30.35	29.76	30.103	.59	76.0	50.0	59.701	26.0	1.161		5.451		SW.	15	1		1
August, ...	30.40	29.65	30.064	.75	78.5	48.0	62.687	30.5	2.986		1.813		NE.	13			
September, ...	30.28	29.33	29.754	.95	70.5	43.5	55.666	27.0		7.021	6.507		SW.	21	8		5
October, ...	30.44	29.05	29.827	1.39	58.5	37.5	48.354	21.0		7.312	6.217		SW.	22	4		
November, ...	30.55	29.31	29.922	1.24	53.0	31.5	43.255	21.5		5.099	4.996		SW.	17	4		2
December, ...	30.61	29.33	30.199	1.28	50.5	22.5	40.088	28.0		3.167	4.228		SW.	20	1		
Means, &c.....	30.485	29.362	30.001	1.123	61.91	34.79	48.864	27.12			52.604	1.531	SW.	215	55	12	11
											1.531						
											54.135						

Annual Depth of Rain at Kendal in Cumberland, from 1829 *to* 1835 *inclusive.* By Mr WAKEFIELD. (Communicated to the Magazine of Popular Science, No. 1.)

	1829.	1830.	1831.	1832.	1833.	1834.	1835.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January,.........	0.747	0.429	1.619	2.278	1.628	14.758	5.349
February,.......	1.234	4.774	8.208	4.258	4.582	5.723	8.820
March,...........	0.867	5.045	6.208	3.549	2.070	5.171	5.049
April,............	3.511	5.656	2.433	2.235	3.754	1.043	1.589
May,..............	1.977	2.831	0.721	1.602	2.534	1.637	3.063
June,.............	4.204	5.289	2.682	4.643	7.715	6.699	1.254
July,..............	5.569	4.961	4.081	2.639	2.233	5.048	6.259
August,..........	9.383	4.218	3.899	4.433	1.966	6.167	3.107
September,......	5.243	8.027	6.393	2.295	3.527	4.908	7.815
October,..........	6.684	4.695	11.812	8.346	3.752	4.715	4.386
November,......	3.855	10.023	8.560	5.373	7.438	4.206	6.311
December,......	2.899	2.082	4.980	8.037	14.219	5.047	2.889
Whole Year,...	46.173	58.030	61.416	49.688	55.418	65.122	55.891

Mean depth of the Seven Years, . . 55.962 inches.
Total ditto, ... 391.738 do. or 32 feet $7\frac{3}{4}$ in.

On the Geology of Massa Carrara. By Professor FREDERIC HOFFMANN.

THE marble mountains of Carrara are situated in the north-western part of the Apuanian Alp, a mountain group, rendered very remarkable by the boldness of the forms of its rocks, and still more by its almost complete separation from the chain of the Apennines. This group extends about thirteen miles from south-east to north-west, and has an average breadth of about six miles. Towards the sea its acclivities are not very steep; and *Pietra Santa* and *Massa* are situated at the edge of an alluvial plain which stretches to the foot of the mountains, and is about $2\frac{1}{2}$ miles in breadth. This situation points out also very nearly the termination of the two narrow and deep penetrating lateral valleys of *Senavezza* and *Frigido*. To the north-west, in a sort of basin-shaped widening of the valley lies *Carrara.* The highest summits of this mountain chain, the *Pizzo d'Uccello* and the *Pania della broce*, are not quite 5800 Parisian feet above the level of the sea. On the side next the sea, talc-slate and mica-slate are the oldest rocks which make their appearance; they dip regularly to the south-west, and in their lower beds, or in the inner higher parts of the bottoms of the valleys of the *Sena* and *Frigido*, pass into a distinct gneiss. Limestone masses occur very frequently throughout the whole extent of the slaty rocks, and are particularly numerous and extensive in *Senavezza.* Most of them are interstratified with the general arrangement of the beds, and traversed by subordinate layers of slate which run in a similar direction to the general stratification. They almost all consist of a grey or nearly white crystalline-granular limestone, veined or striped by lighter or darker colours, and which is generally known by the name of *Bardiglio.* Besides these regular interstratified limestone masses, others appear in the slate having less regularity, and greater variety of structure. The largest of these is that which, completely surrounded by slate, forms the *Monte Altissimo* on the north side of *Senavezza*, rises to the height of upwards of 5000 feet above the level of the sea, and whose south side forms towards the valley of the Sena a rocky precipice 3000 feet in height. Here the limestone is placed, like a long, obtusely acuminated wedge, between the masses of slate. The gneiss turns suddenly round before this limestone mass, under a very high inclination to the north-east; while the latter dips to the south-west; on the bounding side of the valley the junction of the two rocks is displayed in a precipice of at least 1000 feet in height, and the bending of the gneiss round the included limestone is distinctly visible. On the opposite declivity of the *Altissimo*, and following the limestone are the strata of slate, which are nearly perpendicular, or dip towards the limestone on the south-west. Still more remarkable than this arrangement is the structure of the great limestone mass of the *Altissimo.* Where the slate appears in contact with the limestone, the latter is by no means a perfectly formed marble, but is an impure ash-grey, fine granular, almost compact limestone, which one would declare to be secondary limestone, if he did not find it in such unusual connexion. Petrifactions were not met with; an oolitic structure is sometimes, though rarely, observable. Usually the limestone is broken up into sharp-cornered fragments by innumerable fissures, and when its sandy looking granular structure is clearly exhibited, it looks like an impure dolomite. Stripes occur resembling those of the *Rauchwacke* limestone. Farther in the interior of the mountain the more or less considerable limestone and dolomite layer ceases, and at last the beautiful brilliantly white marble commences. The blocks of marble, still having a prismatic form, are always surrounded by more or less considerable stripes of green talc-slate abounding in iron-pyrites, which rock so completely encases the marble on all sides, and is so completely amalgamated with it, that it almost appears as if the pure granular limestone had been separated from the talc-slate in a liquid condition. Where the latter penetrates the limestone, and does not separate itself in distinct layers, impure coloured and striped *Bardiglio* is generally produced. In the midst of the mass of pure, fully formed marble, we not unfrequently see completely enveloped traces of dark grey nearly compact limestone, having irregular fissures, and so completely amalgamated with the including mass, that we might easily regard them as fragments of the impure limestone layer, upon which the altering action that produced the marble could not complete the change. To the east of both the chief branches of the valley of *Senavezza*, in the valley of *Versilia*, the talc-slate dips at first at a high angle to the south-west; but where the deep lateral valley of *Ruosina* commences, this position is changed. No gneiss appears under the slate, and the last dips, in a direction up the valley, to the north-east. At the *Ponte Stazzemese* an impure grey limestone reposes on the slate, and in all its relations reminds one exceedingly of the external layer of *Monte Altissimo.* Upon it follows, towards the *C. di Molina* regularly superimposed, the peculiar marble, known under the name *Mischio di Senevezza.* This *mischio* exhibits a great number of sugar-like, granular, white limestone fragments, included in a dark, iron red, compact claystone basis, in which not unfrequently fine needles of hornblende occur. At the planes of contact, the red colour of the uniting substance has also penetrated into the fragments, and fine scaly green talc coatings are often to be observed. This brings to our remembrance Savi's conjecture, that the peculiar marble may owe its origin to a highly ferruginous vein of wacke having penetrated into the limestone,—an occurrence which several analogies point out. The red wacke veins of the *mischio* penetrate like net-work the upper part, which runs in all cases parallel, and consists of a pure white saccharine marble. This is changed to Bardiglio in the superimposed mass, to which dark stripes of mica running through it give a very beautiful watered appearance. Immediately above the quarries, which are carried on to a considerable extent in the *Bardiglio,* the stripes of mica are more crowded together, and form a stratum of distinct, well characterized mica-slate. This, on the hanging side, becomes a black clay-slate, upon which, in considerable quantity, and with great regularity, a sandstone follows on the hanging side, which is divided into beds of from one to three feet in thickness, and often alternates with slaty marl; and upon this sandstone lies an extensive bed of limestone resembling *rauchwacke.* Near Stazzema, the sandstone becomes a fine granular quartz, and its plates of mica become real slaty stripes. It alternates, at last, in various ways with very considerable beds of pure clay-slate, and above the whole series of strata, at a height of 1000 feet above Strazzema, a distinct mica and talc-slate occurs, which proves that we have not yet left the region of the oldest formation of the chain. Similar conglomerated structures occur, in various ways, subordinate to this slate formation; but in no other place are they so striking, and in such remarkable and peculiar relations. This formation deserves particular attention, on account of its richness in metallic minerals in certain localities. A mass of talc-slate to the south of *Buosina* is penetrated in all directions by fine veins, and containing various sulphurets, viz. lead-glance, blende, antimony-glance, iron and copper pyrites. This circumstance is a strong argument for the original injection and sublimation of metals in mountains. Veins of iron-glance are abundant, and even cross in great numbers the veins of the sulphurets; large veins of red ironstone, magnetic iron, and iron-glance occur also in the slate, and a vein of this description has branched round the fissures of a bed of true *Bardiglio,* and has confused and altered it. Out of the slate formation of which we have spoken, there is only one limestone formation which presents itself in any considerable quantity in the Apuanian Alp. It rises to the height of 5728 feet above the level of the sea, in the rocky cone of Pania della Croce, and in some places forms walls which are frightfully perpendicular. On the great scale, and on the whole, this limestone formation occurs always above the slate. It is only along the declivity of the *Tambura* chain, which is directed to the south-west, that this position is reversed. There the slate reposes regularly on the limestone, and one cannot believe any thing else than that this steep elevated chain has been overturned in its whole extent at the south-western edge. The relations of the internal structure, and the occurrence of the characteristic alterations of this limestone, render it difficult to convey a distinct idea of it. On this account the author selected the description of distinctly displayed sections, such as those which occur in the mountain group of *Carrara.* Among these there is no one so perfect and varied as that at the north-west corner of the group, in the direction from *Castelpoggio* to *Tenerano.* *Castelpoggio* is placed on the lowest beds of the *Macigno* formation, which consists of red compact limestones penetrated by oxide of iron, alternating with red and grey slaty marls, which not unfrequently resemble old clay-slate. Here, as in other places, the *Macigno* formation is traversed by veins of quartz and calcareous spar, and very frequently blood-red jasper and hornstone occur in nodules or thin layers. The dip is to the south-west, under an angle of 48°. Under the *Macigno* commences next the *rauchwacke*, which has a blackish-blue colour, and is coarsely vesicular. Sometimes this rock loses its porosity, and becomes a marble of an inferior description, or it becomes a regularly stratified limestone, which not unfrequently alternates with slaty marls, that have completely the appearance of the layers of calcareous clay-slate which so frequently occur with the old limestone in the German transition series. At the junctions of the layers of limestone petrifactions occur, viz. small species of *Ostrea*, *Pecten*, *Cardium*, *Terebratula*, and many traces of corals. In the slates, bodies resembling scales of fishes occur, and with them an *Avicula* and a *Corbula.* Upon this limestone follows, about 600 feet under the pass, unstratified, vesicular, in general crystalline, limestone, which often seems to indicate dolomite, and in the lower part passes gradually into snow-white saccharine marble. Under the marble, and insensibly encroaching on it, appear the mica and talc slates. With these end the series of strata here observable; for at the top of the pass, an opposite dip of the strata takes place, and on the other side, at *Tenerano*, the superimposed *Macigno* again appears. What this section in the *Tecchia* exhibits within narrow limits, is seen on a larger scale in all the cross ravines of the mountain group of *Carrara*. In the central mass further in the interior of the mountain group, the more or less pure marble always occurs under the external limestone layer. Its largest and purest mass lies near *Turano*, and thence to the summit of the *Monte Sacro*. The talcslate is connected with it in the same way as on M. Altissimo, and divides the marble on the great scale into large distinct beds, which are parallel to the general dip; sometimes such separations of strata are indicated only by delicate scales of silvery mica, or talc. This, hitherto termed primitive limestone, passes gradually into compact vesicular limestone, abounding in petrifactions, so that there can be no doubt as to the connexion in their formation; nay, even a perfect alternation takes place of all these rocks. Between *Carrara* and *Calonata*, always proceeding in the descending order in regard to superposition, we still find the porous and compact fossiliferous limestone, then, where the valley of *Miseglia* begins, first a rock dipping under a high angle to the south-west, an intermediate link between the slaty marls of the secondary strata and the true clay-slate, and this on the lying side passes into a beautiful talc-slate; the talc-slate forms a large pure layer, and is succeeded by an underlying portion of beautiful white marble, veined with black. This marble is followed again by an alternation of stratified black limestone with slate, separated into plates one inch in thickness. The slate is here more like mica-slate, and the limestone is more rarely quite compact, and contains no petrifactions. Immediately under this alternation of limestone, a slate follows the great mass of the marble. Marble and dolomite are here one and the same thing, and ascend uninterruptedly to the summit of *M. Sacro*. If we go beyond this mass in an eastern direction, we meet with, first, a dark grey compact or fine dolomitic limestone, on which the whole mass of marble reposes, and occasionally numerous layers and nodules of flint appear in the *Bardiglio*-like limestone. In the upper part of the limestone region of the *Apuanian* Alp, we meet with numerous repetitions and proofs of the relations described. It is only the pass over the chain of the *Tambura* which presents peculiarities. When we have crossed the Tambura from the north-east, we leave, at the bridge under *Vagli di Sotto,* the last trace of the Macigno formation, which is cut through to this point from the valley of the *Serehio,* viz. a fine granular sandstone. On the other side, towards *Vagli di Sopra,* a compact dark limestone seems always to occur again, a rock which is so often subordinate to the *Macigno;* numerous veins of white, small, granular calcareous spar traverse it, which soon predominate over the mass itself, and convert it into marble. The numerous layers of slate in the mass of the marble have always, towards the upper part, the character of what Savi has called *Galestro*; they are iron red, often also green, and at the junctions of the layers there are the dull plates, which generally occur in old clay-slates and secondary slaty marls. Sometimes they contain more talc, become more shining, and then resemble the old talc-slates; sometimes they seem dull and crumbling, so that one would be inclined to believe himself transported to secondary rocks. Layers and nodules of jasper are also not awanting. The marble, which is always so distinctly divided into red beds on the great scale, is here everywhere intimately connected with the layers of Galestro. It is interramified and interwoven with it. In the midst of the marble, we meet with stripes and veins of rough vesicular limestone, which passes into compact and into dolomitic limestone. At the crest of the mountain chain lies a partly splintery, partly more or less altered smoke-grey limestone, in which *Guidoni* found a cast resembling a Turritella. Nearly the whole of the declivity of the Tambura chain towards the sea is decidedly dolomitic, although very often the pure marble-like granular limestone occurs in it. Under the *Pizzo d'Uccello,* the limestone is traversed by veins of calcareous spar and quartz, and contains also masses and layers of flint. Nearer Ajola, there are masses and layers of a fine granular felspar, a kaolin-like substance, seldom well characterized; but as to its mineralogical constitution, there can hardly be a doubt, as not far from Ajola it occurs, alternating with the limestone in regular layers of from one to three inches in thickness. In the larger portions of the felspar, iron-pyrites occurs frequently, and also ironstone in veins. In the neighbourhood of these substances, the limestone never alters its texture, and invariably preserves its regular stratification. The *Macigno* formation, which nearly surrounds the Apuanian Alp, is a grey sandstone, which constitutes nearly the whole of the northern half of the chain of the Apennines, and which has hitherto been considered a greywacke. Numerous limestones, divisible into groups, are subordinate to this sandstone. These prevail chiefly round the Apuanian Alp, far above the sandstone and its alternating slaty marls. The zone at the south-western edge only, from *Fosdinovo* to *Massa*, is entirely sandstone. The remains of Fuci (especially the *F. intricatus*) are characteristic of this rock, and also of the limestone and slaty marl. The sandstone formation everywhere reposes on the limestone; and where the *Macigno* is connected with the limestone, the two seem most intimately united. The great resemblance, and the direct union of the *Galestro,* and the older limestones reposing on the slate, and even the old mica and talc slates, prove this fact in the most perfect manner. Where the Macigno formation consists chiefly of limestone, it is often hardly possible to assign with precision the boundary of the two formations; for their dip is always the same, and the external resemblance so great, that one might reasonably consider them as formations passing the one into the other.

The *Macigno* formation, notwithstanding its enormous extent, and the peculiar character of its component rocks, is a geognostical equivalent of the great formation of the north of Europe, the *Chalk* and *Greensand.* Its relations in position hitherto observed, are in favour of this view. In Sicily, where this formation is displayed in all those peculiarities which distinguish it throughout the whole of Italy, the author was often surprised by the completeness through a great extent, of the uniform transition to the tertiary series. It is also rendered more probable by the repetition of the same circumstance in very numerous points on the Italian continent. The immediate neighbourhood of Geneva presents many such examples, and the observations of De la Beche on the environs of Nice, are of decisive importance on this subject. It is consequently very probable, that the limestone of the *Apuanian Alp*, which immediately follows the Macigno, is to be considered as a Jura limestone; and we must then, as Savi has recently done, refer the lower layers to the Macigno formation, a view which the author does not consider as the correct one. It is certainly very surprizing to find rocks like those described, clay and mica slates, talc-slate, and gneiss, which leave no doubt as to their being contemporaneous or intimately connected with fossiliferous limestone of the newer secondary formations. The slates follow not only directly in entirely uniform connection with those limestones, but they penetrate them, alternate with them, and are so intimately blended with their masses, that the author considers them even undoubted *members* of the *secondary series.* To which of the known members of the secondary class these slates are to be referred, we do not yet possess sufficient facts to enable us to determine. The chief difficulty seems to be particularly in the completely altered condition of the limestones. The marble, whose occurrence seems so remarkably connected with its intimate union with the slate is certainly a limestone altered by Plutonic agency. This we could not doubt, even though the numerous relations of its union with dolomite and vesicular limestone, its transition from compact and still unaltered limestone, its penetrating veins, &c. were not observable. If, however, these altering actions have been produced through the medium of the interlacing of the slates with the limestone, the conclusion that the slates must have undergone alteration and change cannot be avoided

The only distinct unaltered members of this slate formation, seem to be the *Macigno*-like sandstone and the slaty marl of *Stazzema*, and perhaps also the clay-slate occurring in its vicinity; as to whether these rocks at one time belonged to the series of the Jura or *Keuper* formation, the phenomena now under consideration afford no explanation. Mica and talc slates are decidedly products of a deeply penetrating altering action, and their connection with the gneiss inclines the author to believe that the probably long uninterrupted action to which all those altered rocks have been exposed, was accompanied by the uprising of a granitic mass, which in this district did not remain far under the bottom of the deeply-seated valleys. The whole process, according to the facts deduced directly from observation, took place very probably in the oldest period of the formation of the chalk, for even the oldest strata of the Macigno, those reposing directly on the Jura limestone, are distinctly altered and converted into *Galestro.* In the newer strata, we find not unfrequently enclosed fragments of shining mica and clay slates, which have been produced by the processes described. Mica and talc slates and the marble altered from limestone, appear to the south in the far projecting *Promontorio Argentaro,* in the mountain group of Campiglia, and in the island like separated *Monte Pisani*; and to the north of the Apuanian Alp, there is that mountain chain which surrounds the Gulf of Spezzia which presents a very perfect representation on a small scale of the phenomena we have described. Of the granite whose eruption at this last period of its formation probably produced all these appearances, we know almost nothing in this part of the mainland of Italy; it is said to occur at Campiglia, and there only. What is here concealed under the surface, is displayed rising above the sea in an immense mass in Elba; and a recent examination of that island made by the author proves that there the granite which forms mountains more than 3000 feet high, is intermixed with the older limestone of the Apennines (under the *Macigno*), and converts it by direct contact into marble. It is highly probable, that phenomena analogous to those ascribed to the action of the granite in the district described, have in the Apennines been produced by the out-break of the *Galestro,* whose appearance must have followed immediately the completion of the great Macigno formation.

1. *Observations on the Annular Solar Eclipse which occurred on the* 15*th of May* 1836, by William Galbraith, Esq. 2. *Observations on the Eclipse*, by Commander Alexander Milne. 3. *Observations made with Leslie's Photometer during the Eclipse*, by E. Sang, Esq.

An eclipse of the sun, it is well known, is caused by the body of the moon coming between the spectator on the earth's surface and the sun, thereby producing a greater or less degree of darkness according to the magnitude of the eclipse.

In the early ages of the world, men were either ignorant of, or at least not well acquainted with, the natural causes on which these phenomena depend; and consequently they were then regarded with superstitious awe, but now they are hailed with satisfaction as phenomena highly valuable for the purpose of improving our knowledge of astronomy, geography, and navigation. On this account some of our most zealous practical astronomers travelled hundreds of miles in order to observe this eclipse to the greatest advantage.

Those observers here who felt an interest in these departments of science, continued for some time previously to prepare to observe it as effectively as their circumstances would permit; and in several instances within our knowledge this was satisfactorily performed, and the agreement in the results almost as close as could reasonably be expected.

Throughout Europe the same vigilance will doubtless be manifested, from which a mass of observations must be obtained that will at this epoch fix with extreme precision the errors of the lunar tables which have already attained so high a degree of accuracy, by the labours of those astronomers and mathematicians who have done so much for the advancement of their favourite science.

My observations were made within the grounds of the observatory of Edinburgh on the Calton Hill, in Lat. 55° 57′ 22″.5 N., Long. 12^m $43^s.5$ W., as deduced from previous observations; and these I believe to be within the limits of admissible errors inseparable from observations. The instruments used were an inverting telescope, magnifying about fifty times, and a pocket chronometer of the usual construction, making five beats in two seconds. This train is not so convenient for estimating fractions of seconds as a clock beating exact seconds, or a chronometer making exactly two or three beats in a second, and consequently I was unable to mark parts of seconds satisfactorily. The chronometer was set to mean solar time a little before the commencement of the eclipse, and lost two seconds, as nearly as could be estimated, during the continuance of the observations, when compared with a clock whose error had been ascertained that day by direct observation. On allowing a proportional part of this rate to each observation, fractional parts have been introduced whose accuracy is not warranted to nearer than half a second of the truth of the time of the phase intended to be recorded.*

Having made these preliminary observations and explanatory remarks, I shall now state the final results in mean solar time.

	H.	M.	S.
The first contact of limbs took place at .	1	33	9.75
Annulus completely formed, . .	2	57	21.12
Annulus broke	3	1	11.03
Termination of the eclipse. . . .	4	19	22.40

From a mean of the times of the commencement and termination of the annulus, I infer that the time of greatest obscuration took place at 2^h 29^m $16^s.07$, and from the difference of those times that the annulus lasted 3^m $49^s.91$. It may be added, that all the observations here recorded having been taken with equal care, and no oversight having occurred or accident happened in the management of the instruments, they are all entitled, in the opinion of the observer, to equal confidence.

These observations, when compared with others made on different meridians, will give the difference of longitude between those meridians with great accuracy, though for ordinary practice the calculations are rather long and difficult to persons not much habituated to such operations. Taking the sun's and moon's places as given in the Nautical Almanac, which are derived from tables known to be liable to small errors only, the longitude of places may be obtained to a considerable degree of precision. On this principle, by the rules and formulæ which

* Different observers even at the same place will occasionally, in these kinds of observations, differ some seconds.

are well known, I have found the longitude of the place, by my observations of the beginning of the eclipse, to be $12^m\ 22^s.26$ W. and by the end $12^m\ 41^s.6$ W. The first of these deviates about 20^s from the truth, the second 2^s only, and the mean of both about 11^s, arising from the errors in the solar and lunar tables combined with the errors of observation. When the observations made at the Royal Observatory of Greenwich, and other places whose longitude is well determined, are obtained, a comparison of these will fix the longitude of my station with greater certainty; though from the results above it is clear that in distant countries, where corresponding observations cannot be often got, the longitude may in this manner be found, by repeated observations, to a very great degree of accuracy. In observing eclipses, the complete phase is generally that most attended to, because the precise time of peculiar intermediate states is not easily attained. I had originally proposed to mark the time when the ring was partially formed, and only broken occasionally by the lunar mountains, but I found some uncertainty in determining that state of the eclipse. By sweeping the eye along the serrated limb of the moon, I could not determine precisely the mean level of the moon's surface, or the circular line equidistant from the centre, and I was therefore doubtful whether the parts of the limb of the sun broken by dark intervening spots, were entirely mountains or not; and the short space of time allowed for consideration did not permit me to come at any conclusion by reason and reflection. The estimated time in some cases, from the light first breaking through what might be readily taken for valleys at the point of nearest contact, till the luminous arch reached the summits of the mountains, at both the formation and termination of the ring, taken, of course, in inverse order, was about five seconds, more or less, in my judgment, according to circumstances; but I have not recorded these among what I conceive to be decidedly good observations. I marked the time of entrance of some of the spots too, but as these observations also must be very vague, I have not communicated them.

These broken particles of light, already alluded to, intersected by the serrated peaks of the lunar mountains, produced a very interesting and brilliant effect, and had a good deal of the appearance of elongated particles of mercury arranged in a narrow circular groove, divided by small dark ridges lying between them. It might have been remarked sooner, that there appeared to the eye a darkish space very near the sun's limb about the point of contact, just before I was fully assured that the contact had absolutely taken place, and, something to my apprehension, to a certain degree resembling the rotatory or spiral motion of dust caused frequently by the wind in our streets in dry weather, but whether it was more illusory than real, I shall not at this time take upon me to determine. A comparison of my remarks with those of other observers, will, it is hoped, tend to verify or correct them.

I shall not attempt to describe the effect which the darkness had upon nature generally, because that though I could take a glance at the surrounding scenery occasionally, my mind was too much occupied with other pursuits to do it carefully. I saw Venus distinctly with the naked eye, but could not perceive either Jupiter or Sirius, because my eyes were too much affected with the constant glare of the solar rays, though viewed through a pretty deep-coloured glass, to distinguish any other objects accurately. I did what I could, though placed not in the most favourable circumstances, to observe the more striking phases of this interesting and important phenomenon with all the accuracy in my power, but with what success an extensive comparison of them with those of others will determine.

54. South Bridge, Edinburgh,
May 15. 1836.

Observations on the Annular Solar Eclipse. By Alexander Milne, Esq. Commander R. N., F. R. S. E., in a letter to the Editor, Professor Jameson, dated at Inveresk, near Edinburgh, May 1836.

The general aspect of the morning of the 15th May was unfavourable, and yielded but little prospect of a clear day for viewing the eclipse. The wind was strong from the westward, —the sky overcast, and a dull haze pervaded the western horizon. Towards ten o'clock, however, all these symptoms which foreboded disappointment vanished. The clouds had already become partially broken, and the sky soon afterwards became clear, while the sun shone forth warm and brilliant, dissipating the still lingering remains of the morning vapours.

The chief object to which I had proposed to direct my attention, and observations, was the effect which the progress of the eclipse produced on the thermometer, not only in the shade, but likewise when subject to the influence of the solar rays. With this view four thermometers were carefully compared with each other, that the observations might be reduced to one standard. One of these instruments was placed in the *shade*, subject to the aspect of a northern sky, and so placed as to be under no influence of local radiation, being solely affected by the aerial temperature;—whilst another was suspended also in a free circulation of air, but at the same time *in the solar rays.*

The other two instruments were subject to experiment under the direct influence of the solar heat, being enclosed in a box having a glass cover, to insure protection from aerial currents. The bulb of one was enveloped with a covering of black silk, with the intention of increasing the range of that instrument, and thereby more correctly detecting the loss, as well as increase of heat during the progress of the eclipse. These instruments were invariably kept directed to the sun.

These being prepared, and the sun clear, the observations were commenced, and continued at intervals as noted in the following table.

Mean Solar Time.	In the Box exposed to Solar rays.		In the air and sun.	In the north shade.	REMARKS.	Mean Solar Time.	In the Box exposed to Solar rays.		In the air and sun.	In the north shade.
	Black Bulb.	Plain Bulb.					Black Bulb.	Plain Bulb.		
10h 45′	95	77	81	...	Sky clear, although still some partial haze.	2h 43′	78½	75½	60½	57½
11 0	98	78¼	82	58		2 55	73½	72	59½	57
11 30	99	84¾	94½	...		3 0	69	66½	58½	...
11 45	107¼	102½	74	60		3 6	67½	65½	58	56
Noon.	121	106¾	72½	60½	Sky perfectly clear, and continued so until 4 h. 20 m.	3 10	68½	65½	57½	55½
12 15	121½	107	72	60		3 16	70	66½	57½	55½
12 30	120½	106¼	71¼	60		3 21	74	69¾	58½	55¾
12 45	118½	103¼	73½	60½		3 30	84	73½	59¼	55¾
1 0	121½	107	76¾	60¾		3 35	84½	77	60½	56
1 15	121 3/2	106½	75¾	61¼		3 43	88½	80	61¼	56
1 28	123	108	73	...		3 52	94½	84	61½	56½
1 40	120¼	106¾	70½	61¼	The Barom. at 100 feet above the sea remained stationary at 30.549.	4 2	97¼	86¼	60¾	56½
1 50	117½	104	67½	61¼		4 12	103	90¼	65¼	57¼
2 2	112	100¾	67	60½		4 20	103¼	92	65½	57½
2 12	105½	95	64¾	60		4 35	103	92	64¾	57½
2 23	100	92	64	59½		4 50	92	83½	61¾	57½
2 35	90½	84¼	62¼	58¾						

From the above tabulated results it will be observed, that from the first period of observation (10^h 45^m), the instruments in all their relative situations gradually rose in temperature, until 1^h 28^m, at which time they had obtained their maximum point, except the one exposed to the air and sun's rays, whose indications were influenced by passing currents of air by which it was at intervals depressed. At 1^h $33^m.7$ the moon's limb was observed to enter on the solar disc, and a few minutes after that observation the thermometers were noted. Those in the box under the direct influence of the solar heat, had already become depressed; while that in the northern shade remained stationary until near two o'clock, when it also indicated a depression. The three exposed to the sun's rays had continued their progressive descent from the commencement of the eclipse.

At 3^h the annular phase was central, and the instruments most clearly indicated an extensive depression from the time of the first contact of the limbs, and still continued to fall until some minutes after that hour, when those exposed to the solar rays attained their minimum temperature; not so, however, the thermometer in the shade, whose indications of its lowest point did not occur until 3^h 16^m.

The annular phase having ended at 3^h $2^m.5$, and the body of the moon withdrawn itself from the sun's disc, so as to permit the intercepted rays to fall to the earth, the instruments quickly indicated the change, and continued to rise as the eclipse passed off. They attained their second maximum at 4^h 24^m, the time at which the eclipse ended. The sky now became partially overcast with thin attenuated clouds, dispersed over the solar disc, and intercepting the solar radiation, farther experiments were therefore discontinued. It may, however, be mentioned, that during the time of the eclipse, and whilst the observations were recorded, the sun presented a uniformly clear disc, and the atmosphere remained as nearly as possible in the same state.

Abstract of greatest and least Temperature.	In the Box.		Air in Sun's rays.	North shade.
	Black.	Plain.		
Greatest 1 28	123	108	73	61¼
Least 3^h 6	67½	65½	57½	55½
Difference .	55½	42½	15½	5¾

It must, however, be remarked, that the greatest temperature in the shade as indicated at 1h 50m, is by no means the greatest temperature for the day, as it generally attains that point between the hours of two and three o'clock. With the object of attaining the hour of maximum temperature, some observations were instituted for the purpose. The sky and aspect of the day being in close resemblance to that of the eclipse, it was found that the same instrument in the shade indicated its maximum at 3h 20m, and having become stationary at 69¼. The temperature at 1h 28m of the same day being 66½, we may therefore attain from these observations an *approximate* temperature for the period of greatest obscuration, or of least temperature on the 15th. This we may assume at 64, whilst that actually observed was 55½, there being a difference of 8½. This difference, therefore, may be assumed as a close approximation to the loss of heat *in the shade* by the interception of the solar radiation.

Observations on this beautiful phenomenon were directed not only to the changes of temperature, but also to the progress of the phases, and the following are the results expressed in mean solar time, at the place of observation, which was at Inveresk, near Musselburgh, in latitude 55° 56′ 20″ N.; longitude 3° 2′ 40″ W.

	h.	m.	s.
First contact of the limb,	1	33	44
First do. of the annulus,	2	57	56.5
Last do. of the annulus,	3	2	34.5
Last do. of the limbs,	4	19	52

Occultation of the Spots on the Solar Disc.

		h.	m.	s.
Spot on sun's western limb,	(*a*)	1	40	3
Large well-defined spot towards sun's centre,	(*b*)	1	52	47
Small spot near lowest cluster,	(*c*)	1	13	44.5
Small spot under the large one,	(*d*)	1	14	45.5
Uppermost of two single spots above the large one,	(*e*)	1	33	9.5
Large spot of the upper cluster,	(*f*)	2	22	16.5
Emmersion of large spot,		3	33	33
Emmersion of spot (*e*),		3	34	41

On surveying the solar disc before the commencement of the eclipse, it presented a numerous collection of spots, chiefly confined to the west of the sun's centre, and dispersed vertically in an irregular form; some of them were single, having the appearance of a dark nucleus, with a well-defined edge, and surrounded with a faint cloudiness, which appeared parallel to the interior nucleus. In this encircling band which surrounded the largest spot, were observed several small dark specks, in colour resembling the interior nucleus itself; others were formed into groups and clusters, and dispersed on various parts of the sun's western disc.

Several seconds (6 to 10) before the formation of the annulus, a faint light was perceptible encircling the moon's limb before it advanced on the solar disc. This light gradually preceded the horns or cusps of the sun, as they approached each other, from the moon's progressive motion. Small detached portions of the sun's body were also observed around the lunar limb immediately before the formation of the annulus, an appearance probably owing to the rugged or uneven surface of the moon's body.

At this time the darkness which prevailed, was certainly not so great as was expected. Venus alone was distinctly visible, Jupiter could with great difficulty be distinguished, and none of the fixed stars could be observed. It was interesting, however, to trace during the continuance of the eclipse, and while the darkness gradually increased, the various characteristic changes which were produced. The peculiar dimness of the light emanating from the half-obscured sun, or when it alone exhibited a narrow ring, resembling the peculiar light afforded by the moon in tropical latitudes. The various rays of light as they penetrated through the foliage, gave faint and undecided shadows, while they presented the peculiar crescent form of the eclipse itself. These, however, were not the only points which accompanied this unusual and beautiful phenomenon. Nature, both animate and inanimate, was equally influenced by the approach and progress of the darkness;—for as the penumbra gradually overspread the place of observation, the birds took up for a moment their evening song; but speedily, as if alarmed by the unusual character of the twilight and its chilling coldness, ceased their notes, and became silent and motionless. Vegetable creation participated also in the general and universal change. The wild anemone, which shortly before was fully expanded, now yielded to Nature's laws and closed its leaves as on the approach of night; whilst a night-flowering plant in the greenhouse, in accordance with the laws of their peculiar constitution, opened its ball-formed flowers. But these phenomena continued only for a moment. The eclipse completed, and the sun's light and heat radiating once more on universal nature, every object seemed to participate in the cheering change. The flowers which had closed, soon yielded to the reviving influence, and expanded their flowers and leaves; while those whose natural time of bloom is only during darkness, closed them until the approach of the natural evening. The feathered tribes, as if released from some mysterious thraldom, were again immediately on the wing, and pursuing their wonted and busy occupations; the whole machinery of nature, in short, which for a few minutes seemed to stop, or at least to be retarded in its movements, again resumed its working, and was beheld in the possession of its wonted energies.

Observations made with Leslie's Photometer during the Annular Eclipse. By Edward Sang, Esq.

The predictions of great darkness which were current before the eclipse were not at all warranted by the computations; during the continuance of the ring, about one-eighth part of the sun's disc was to be exposed, an extent quite sufficient to afford light equal to the ordinary illumination about sunset. A bright sun at noon of a summer day gives an indication of about 150° of Leslie's photometer, and at three o'clock we might expect nearly as much; the eighth part of that is 17°—eight or ten times the illumination of a well-lighted room. If the sky had been covered with thick clouds, the light, during the greatest obscuration, might have been reduced to a single degree, which still, however, would have been sufficient to allow of reading a small print.

The observations made on the roof of the New Buildings, North Bridge, fully bore out these expectations. There the indications of an excellent photometer were noted every five minutes. Just before the eclipse began the light was 141°; at the time of the greatest obscuration it fell to 12½°; just before the eclipse terminated some thin clouds disturbed the observations; just before they appeared the photometer shewed 117°; after that it sunk to 86°, and rose six minutes after the eclipse terminated to 120½°.

The following table exhibits the results of the observations:—

Time.	Phot.	Remarks.	Time.	Phot.	Remarks.
0h 50m	136°	sky clear.	2h 40m	59°	
0 55	139		2 45	48	
1 0	140		2 50	35	
1 5	140		2 55	20	
1 10	141		3 0	12	eclipse annular.
1 15	140		3 5	16	
1 20	141		3 10	26	
1 25	141		3 15	37	
1 30	141		3 20	47	
1 33		eclipse began.	3 25	57	
1 35	141		3 30	64	
1 40	138		3 35	73	
1 45	138		3 40	80	
1 50	136		3 45	101	
1 55	129		3 50	107	
2 0	124		3 55	114	light haze.
2 5	119		4 0	117	
2 10	110		4 5	114	cloud.
2 15	106		4 10	113	cloud.
2 20	92	light cloud.	4 15	86	cloud.
2 25	89		4 19		eclipse ended.
2 30	77		4 20	105	
2 35	69		4 25	120	

From this table it will be seen, that so soon as the sun began to be concealed, the instrument indicated a perceptible diminution of light, and that its readings followed very closely the progress of the eclipse. The transparency of the air, however, was subject to considerable fluctuations, the effects of which are perceptible on the numbers. After the greatest obscuration the indications rise, but not with a rapidity equal to that of their descent, for this reason, that as the sun was getting lower, his rays had to traverse a greater thickness of atmosphere.

The diminution of heating power, as shewn by the photometer, was 129° of its scale, that is, 12°.9 of the centigrade thermometer, or 23¼ of the common Fahrenheit thermometer.

---

*Instructions for Making and Registering Meteorological Observations at various Stations in Southern Africa and other Countries in the South Seas, as also at Sea.**

The great importance of possessing an exact and carefully registered account of the variations of the barometer, thermo-

* Drawn up for circulation by the Meteorological Committee of the South African Literary and Philosophical Institution; we believe by Sir John Herschel. The "Instructions" are printed in No. 1. of Second Series of the South African Quarterly Journal, sent to us by our friend Dr Adamson of the Cape.

meter, and other meteorological instruments, and of the winds and weather throughout that extensive region of the Southern Hemisphere, which is either included within the boundaries of this colony, or readily accessible from it, has determined the South African Literary and Philosophical Institution to request the assistance of its correspondents, and of all who may have leisure and inclination for observations of the kind, towards the gradual accumulation of a continued and extensive series of meteorological Journals, and towards carrying into effect a concerted plan of contemporaneous observations, on stated days, from which it is conceived that much advantage will be derived. The institution therefore solicits the attention of its correspondents, and of the lovers of knowledge generally, to this object; and earnestly requests their co-operation in making, arranging, and forwarding to its secretary, resident in Cape Town, observations of the nature; and, so far as practicable, according to the plan of those hereafter detailed. Such observations alone can furnish the materials necessary for an accurate and scientific inquiry into the laws of *climate*, regarded as an object of local interest, and are the only data through which (taken in conjunction with the known laws of physics), the more general relations of meteorology can be successfully investigated.

It can scarcely be necessary to insist on the practical importance of this science to the agriculturist, to the navigator, and indeed in every branch of human affairs, or to dilate on the benefits which must accrue to mankind in general, from any successful attempts to subject to reasonable and well-grounded prediction the irregular and seemingly capricious course of the seasons and the winds; or on the advantages, purely scientific, which must arise from a systematic development of laws exemplified on the great scale in the periodical changes of the atmosphere, depending, as they do, on the agency of all the most influential elements, and embracing in their scope every branch of physical science. It is more to the present purpose to observe that, from what has already been done in this department of human knowledge, there is every reason to hope that no very distant period may put us in possession of the key to many of the most intricate meteorological phenomena, and enable us, though not to predict with certainty the state of the weather at any given time and place, yet at least to form something like a probable conjecture as to what will be the general course of the next ensuing season—perhaps to prepare us beforehand for violent and long-continued gales of wind, great droughts, or extraordinarily wet seasons, &c. in the same manner that our knowledge of the nature and laws of the tides, although confessedly imperfect, and, in a great measure, empirical, yet enables us to announce, beforehand, unusually high or low tides. No doubt such predictions of the weather, although only of a probable nature, would be highly valuable and useful, and would materially influence the practice of men in all operations thereon depending. In illustration of this, we need only refer to the value set by many farmers and others on weather-tables, founded on no sound principles, and ratified at best, if at all, only by a very partial and limited experience; or, to choose a better instance, we may cite the importance which is now attached by every seaman to the indications of the barometer, and the numerous cases with which nautical records abound, of great mischief, or even shipwreck, avoided by timely attention to its warnings.

Meteorology, however, is one of the most complicated of all the physical sciences, and that in which it is necessary to spread our observations over the greatest extent of territory, and the greatest variety of local and geographical position. It is only by accumulating data from the most distant quarters, and by comparing the affections of the atmosphere at the same instant at different points, and at the same point at different moments, that it is possible to arrive at distinct and useful conclusions. Hence arises the necessity of procuring regular series of observations made on a uniform system, and comparable with themselves and with each other, by observers at different stations, and of multiplying the points of observation as much as possible over the interior surface of continents—along sea-coasts—in islands—and in the open ocean.

The geographical position of this colony renders it perhaps the most interesting and important situation on the surface of the globe for observations of this nature: first, whether we regard it either as an advantageous station for observing the commencing action of the great counter-current of the trade-winds, where it first strikes the earth's surface, and, combined with the action of the heated surface of the African Promontory, gives rise to that remarkable alternation of south-east and north-west winds, which forms so distinguishing a feature of our climate—or consider it, secondly, as the farthest extremity of one of the two great *lobes* of land which form the terrestrial part of our globe, and as such, constituting at once a barrier to the currents and tides of two great oceans, and a limit to their climates—or, lastly, as a great nautical station, and one not devoid of difficulty and danger, in which every consideration of practical interest combines to stimulate the curiosity of the theorist, and give importance to the results of his inquiries.

As these pages may fall into the hands of many who have been little in the habit of observing systematically, or who may not be in possession of instruments of the nicest construction, attention to the following instructions is recommended as the means of rendering their observations most available for useful purposes, and comparable with each other, and, with those intended to be refered to as standards.

I. *General Recommendations and Precautions.*

1. The continuity of observations ought to be interrupted as little as possible by changes in the adjustments of instruments—in their places—exposure—mode of fixing—or of reading off and registering them. Whenever any alteration in these or any other particulars takes place, especially such as are likely to affect the zero points, or otherwise to influence the mean results, it should be noticed in the register.

2. So far as possible, registers should be complete—but if by unavoidable circumstances of absence, or from other causes, blanks occur, no attempt to fill them up by general recollection, or by the apparent course of the numbers before and after, should ever be made.

3. The observations should, if possible, all be made by one person—but as this may often be impracticable, the principal observer should take care to instruct one or more of his family how to do it, and should satisfy himself by many trials that they observe alike.

4. The entries in the register should be made at the time of observation, and the numbers entered should be those actually read off on the respective scales of each instrument, on no account applying to them previous to entry *any sort of correction, as for instance for zero, for temperature, capillarity, &c.* All these and the like corrections, being matter of calculation and reasoning from other observations, are to be reserved till the final discussion of the series, and for separate determination and statement.*

5. If copies be taken of the registers, they should be carefully compared with the originals by two persons, one reading aloud from the original and

* We regard this as of the highest importance.

the other attending to the copy, and then exchanging parts, a process always advisable wherever great masses of figures are required to be correctly copied.

6. A copy so verified, or the original, (the latter being preferred) should be transmitted regularly) if possible *monthly* from places within the limits of the colony) to the Secretary of the South African Literary and Philosophical Society, at Cape Town, which institution on its part will take care that such documents shall not merely be treasured as a dead letter in its archives, but shall be rendered available towards the improvement of Meteorological knowledge, to the full extent of their actual scientific value.

7. The register of every instrument should be kept in parts of its own scale, as read off, no reduction of Foreign measures or degrees to British being made—but it should of course be stated *what* scale is used in each instrument.

II. *Of the Times of Observation and Registry.*

Meteorological observations should be made and registered daily, at stated and regular hours. In fixing on these, some sacrifice of system must of necessity be made to the convenience and habits of the observer. The best hours in a scientific point of view would be those of Sun-rise, Noon, Sun-set, and Midnight, and these are the hours for which the registers are kept at the Royal Observatory. But these are not the hours adapted to general habits, and since the midnight observation is likely to be pretty generally neglected elsewhere than in an Astronomical Observatory, the following hours, for a division of the day into three parts, are proposed for what may be deemed the Morning, Afternoon, and Evening observations, viz.

Morning, . . .	8 A. M.
Afternoon, . . .	2 P. M.
Evening, . . .	8 P. M.

If, however, the habits or engagements of any one should not allow him to conform to these hours, rather than not observe he may select his own, specifying only what they are at the head of every page of his register, and adhering steadily to them in practice, only observing to make the extreme observations of each day equidistant from the middle one.

At the same time it will be borne in mind, that in what concerns the great Meteorological questions on which the most interesting features of the subject depend, the night is quite as important as the day, and has hitherto been far too much neglected. To any one, therefore, who may feel disposed to enter more zealously into the subject, and will not consider some personal inconvenience ill undergone for the sake of affording data of a peculiarly valuable description, this Committee would most earnestly recommend the adoption, in preference to all others, of the quaternary division of the 24 hours, as followed at the Royal Observatory above alluded to. And they leave it to the consideration of the Council, whether the keeping and transmission of registers on this principle might not advantageously be distinguished by some honorary reward, as that of a Medal for instance, should the funds of the Institution admit of it.

With a view, however, to the better determining the laws of the diurnal changes taking place in the atmosphere, and to the obtaining a knowledge of the correspondence of its movements and affections over great regions of the earth's surface, or even over the whole globe, the Committee have resolved to recommend, that four days in each year should henceforward be especially set apart by Meteorologists in every part of the world, and devoted to a most scrupulous and accurate registry of the state of the Barometer and Thermometer; the direction and force of the Wind; the quantity, character, and distribution of Clouds; and every other particular of weather, throughout the whole twenty-four hours of those days, and the adjoining six hours of the days preceding and following.* The days they have been induced to fix on and recommend for these observations are, the 21st of March, the 21st June, the 21st September, and the 21st December, being those or immediately adjoining to those of the Equinoxes and Solstices, in which the Solar influence is either stationary, or in a state of most rapid variation. *But should any one of those 21st days fall on Sunday, then it will be understood that the observations are to be deferred till the next day, the 22d.* The observation at each station should commence at 6 o'clock A. M. of the appointed days, and terminate at 6 o'clock P. M. of the days following, according to the usual reckoning of time at the place. During this interval, the Barometer and Thermometer should be read off, and registered hourly, and the precise hour *and minute* of each reading should be especially noted.

For obvious reasons, however, the commencement of every hour should, if practicable, be chosen, and every such series of observations should be accompanied by a notice of the means used to obtain the time, and, when practicable, by some observation of an astronomical nature, by which the time can be independently ascertained within a minute or two.† As there is scarcely any class of observations by which meteorology can be more extensively and essentially promoted, it is hoped that not only at every station of importance in this colony but over the whole world, and on board ships in every part of the ocean, individuals will be found to co-operate in this inquiry. Every communication of such observations addressed by channels as secure and as little expensive as possible to the Secretary of this Institution, will be considered as highly valuable.

III. *Of Meteorological Instruments, and first of the Barometer and its attached Thermometer.*

The Barometer is the most important of all Meteorological instruments. Its office is to measure the actual pressure of the atmosphere on a given horizontal surface at the time and place of observation. Its fluctuations are observed to have considerable relation to changes in the weather, and especially of the wind. Hence its use as a weather-glass.

* This is necessary by reason of the want of coincidence of *the day* in different parts of the globe, arising from difference of longitude. In order to obtain a complete correspondence of observation for twenty-four successive hours over the whole globe, it must be taken into account that opposite longitudes differ twelve hours in their reckoning of time. By the arrangement in the text the whole of the *astronomical day* (from noon to noon) is embraced in each series, and no observer is required to watch two nights in succession.

† For example, the first appearances and last disappearances of the Sun's upper and lower border, above and below the sea-horizon, if at sea or on the coast,—or on land the exact length of the shadow of a vertical object of determinate length on an horizontal level, at a precise moment of time, (not too near noon), &c.

A Barometer should be examined, before setting it up, for air-bubbles in the tube, and for the existence of air above the mercury in the upper part of the tube. This is done by gently inclining the instrument either way from the horizontal position a little up and down; when air-bubbles, if large, will be seen to run to and fro, and must be evacuated by inverting the instrument and by gentle blows on it with the hand, driving them up into the cistern. If this cannot be done, the instrument is useless. If air exists to an objectionable amount *above* the quicksilver, it will not tap *sharp* against the upper end of the tube when the barometer is quickly inclined from a vertical position, so as to make the mercury rise above its level, nearly to the top, and then gently *jerked* lengthways and backwards. If the blow is puffy and dead, or is not heard at all, the amount of air must be considerable, and may be expelled by inversion.

In fixing the barometer, choose a good light near a window, but not exposed to sunshine, in a retired apartment, little liable to sudden changes of temperature or to drafts of wind. Adjust the tube to a vertical position by a plumb-line, and fix it so as never to shift from that position. Before reading off, give a few taps on the instrument, enough to make the upper end of the column of quicksilver *shake* visibly, as the mercury is apt to adhere to the glass and give erroneous readings. In reading, bring the index always opposite to one part. The correct part to choose is the summit of the convexity of the mercury, to which the index should be made a tangent, but if this be difficult to hit, either from the construction of the index or the want of a proper fall of light, the line of junction of the mercury and glass may be taken. In that case, the tapping should never be omitted. Whichever mode of reading is once adopted should be stated, and always adhered to. A piece of white paper placed behind the upper part of the tube will generally enable any one to read off by the convexity of the quicksilver. In placing the index, notice whether it appears to shift a little up and down as the eye is raised or depressed; this is called Parallax, and is a source of uncertainty to be avoided by placing the eye in reading always on the *exact level* of the top of the mercurial column.

Barometric observations require corrections of three kinds, and to render them available and comparable with others, it is necessary that their amount should be ascertained, and distinctly stated. The first is called the Zero Correction. It includes several subordinate corrections arising from different sources, such as that originating in a faulty placing of the scale of inches, that due to the capillary depression of the Mercury in the glass-tube, and the constant part (which at a fixed station is nearly the whole) of the depression arising from the presence of air or vapour in the upper part of the tube.

To determine the zero correction, the Barometer must be compared with a standard instrument, such as that at the Royal Observatory for instance, or some other which has been compared with it, or with some standard of equal authority. Such comparison ought never to be omitted before forwarding the Barometer to its place of destination, nor should any opportunity be neglected of comparing it, when fixed in its place, with a good portable Barometer. In making such comparisons, all that is necessary is to record the readings of both the instruments, after at least an hour's quiet exposure, side by side, that they may have the same temperature. If compared by two observers, each should read off his own Barometer in his usual manner, and each should take a mean of several readings, then each should verify the other's results. By this means the zero of one standard may be transported over all the world, and that of all others compared with it ascertained.

The amount of the zero correction is often very large, as two or three-tenths of an inch, but its influence on the mean results of recorded observations, falls wholly on the determination of the heights of the station of observation above the mean level of the sea, and effects little, if at all, any conclusions of a meteorological nature which may be deduced from them. Hence, if proper care be taken to preserve a Barometer, once set up, immoveable, a long and regular series of observation with it has a value independent of any knowledge of this element, and it is fortunate that this is the case, as the zero correction is one extremely difficult to determine exactly *a priori*.

In transporting a *compared* Barometer to its place of destination, great care is necessary. It should always be carried *upright*, or considerably inclined, and *inverted*, and over all rough roads should be carried in the hand, to break the shocks to which it would otherwise be exposed. If strapped horizontally under the roof of a colonial waggon, or tied upright against the wood-work, with its head resting on the floor, there is not a chance of its escaping destruction. Strapped obliquely across the shoulder of a horseman, however, it travels securely and well, and with common care in this mode of transport, its zero runs no risk of change.

The next correction, and the most important of all, is that due to the temperature of the Mercury in the Barometer tube at the time of observation. To obtain this, every Barometer requires to have attached to, or fixed very near it, a Thermometer, called the attached Thermometer, which must be read and registered at each observation of the Barometer. It is preferable in practice to read off this Thermometer *first*, to avoid the error arising from breathing on, or standing long near it, while reading the Barometer itself. The zero of this Thermometer should be ascertained by comparison with a standard at the temperature of about 60° Fahr.

The third correction applicable to barometric observations arises from change of level of the mercurial surface in the cistern, owing to the transfer of a portion of its contents to or from the tube. In Barometers with small cisterns, and where the lower level cannot be adjusted at each observation, its amount may be large, and its effect being always to make the apparent fluctuation less than the real, *in a fixed proportion*, it ought, if possible, to be ascertained. The data necessary to be known are—first, the internal and external diameters of the tube—secondly, that of the cistern containing the mercury, at the surface, where the tube plunges into it. These particulars, as they must be known to the maker, ought to be inquired of him, and indeed ought to be engraved conspicuously on some part of the instrument.

Although all these corrections are necessary for the strict *reduction* of registered observations, they ought not to be applied to individual observations previous to registry. It is sufficient to know them. Their effect is in all cases easily and safely applicable to mean results, and to the conclusions therefrom deduced, and a world of troublesome and often mistaken calculations may be saved by so applying them.

Of the External Thermometer.—The External Thermometer should have a scale on which whole degrees are read off, and divisions large enough to admit of estimating tenths, or at least quarters of degrees, by the eye. It should be compared with a standard, and the difference stated, at one or more temperatures (the wider asunder the better) within the range of the climate in which it is to be used. In fixing it, choose a perfectly shaded but otherwise free exposure, and one where no *reflected* sunbeams from water, buildings, rocks, or dry soil, can reach it: and easily accessible for reading. There fix it firmly and upright. In reading it, avoid touching, breathing on, or in any way warming it, by near approach of the person. The quicker the reading is done the better.

Although read off at stated times, notice should be taken of all sudden and remarkable changes of temperature, as indicated by the external thermometer, whenever they occur. In the neighbourhood of the Cape, and in many other parts of the continent, hot winds frequently set in with great suddenness, often in the night, and singular alternations of hot and cold temperature occur, disturbing the regular laws of the diurnal fluctuation, and connected, doubtless, with many interesting meteorological phenomena peculiar to the climate of South Africa.

Of the Maximum and Minimum, or Self-registering Thermometer.—This should be placed horizontally in some place out of doors, shaded from direct radiation and rain, and otherwise freely exposed to air, and so fastened as to allow of one end being detached from the fastening and lifted up, so as to let the indexes within the boxes slide down to the ends of the fluid columns, a more convenient mode, when the steel index is free enough to allow it, than the use of a magnet.

Both the thermometers should be read off as early as possible every morning, and the indexes re-adjusted. But as double maxima frequently, and occasionally double minima occur, in consequence of sudden changes of temperature, it is recommended occasionally to inspect both of them, with a view to ascertain whether the motion of either the mercury or spirit has been reversed in an unusual manner, and such double maxima or minima, when remarkable, should be recorded as "supernumerary," with their dates and leading features.*

The Self-registering Thermometer is extremely apt to get out of order, by the indexes becoming entangled in the column of fluid. In travelling they should not for a moment be carried with the mercury bulb downwards; if this should happen, they are *sure* to arrive in a state unfit for use. To correct them is tedious, and always hazards fracture. With great care, however, it may be done, as follows:—

1*st*, The Spirit Thermometer. By many jerks, force the index down to

* The spirit thermometer is apt to undergo a gradual change of zero by the transfer (by distillation) of part of its spirit to the upper end of the tube. It should, therefore, often be compared with the mercurial one, and the difference of readings *applied* as a zero. In this *only* case is the application of a zero *before* registering permissible, and indeed essential.

the junction of the bulb and tube; then, by cautiously heating and cooling alternately the bulb, the tube, or the air vessel at the top, as the case may require, the disunited parts of the spirit may be *distilled* from place to place, till the whole is collected in one column in union with the spirit in the bulb.

2*d*, The Mercurial Thermometer. When the steel index gets immersed in the mercury, it cannot be moved by a magnet, and lets the mercury pass by its side. First cool the bulb (by evaporation of ether, if necessary) till the mercury is either fairly drawn down below the index, or a separation takes place in the column, leaving the index with mercury above it. Endeavour then, by tapping, warming the tube, or by the magnet, to loosen the index ever so little, then apply heat to the bulb, and drive up the index with its superabundant mercury quite into the air-vessel. This requires many trials and much patience. When there, hold the instrument bulb downwards, and suspend the index by a magnet at the top, allowing any globule of mercury to drop into the origin of the tube below; then heat the bulb cautiously over a very small clear flame of an oil lamp, till the mercury rises to the very top of the tube, and fairly unites with the globule there awaiting it. Let the bulb cool, and the mercury will sink in one united column; if not, heat it again. When this is accomplished, the index may be set loose, by withdrawing the magnet, and restored to its proper position in the tube.

A self-registering thermometer may be advantageously left (properly secured) for a whole year, or parts of a year, on elevated summits or other remarkable points, to ascertain their maxima and minima of temperature during absence. In such cases, take care to defend them from discovery, or accident from wild animals, birds, snakes, &c. In taking it up for reading off, observe not to derange the indexes, and do not leave it without seeing that the indexes are in contact, and the temperature that of the air at the moment.

Of Thermometers buried in the Earth.—Thermometers buried at different depths, for the purpose of examining the monthly changes of temperature of the soil, should have their balls and lower part of the scale well wrapped up in woollen cloth or pounded charcoal, and should be placed in strong earthen vessels, which may be entirely withdrawn from the ground so as to allow of inspecting and reading off the scale, without exposing the balls to any possibility of changing their temperatures while under examination. The vessels should be fitted with covers, to defend the scale from injury in burying and digging up.

A pipe of earthenware (composed of separate pieces), or one of wood, may be sunk ten or fifteen feet below the surface, into dry earth, and a thermometer, defended as above, lowered *by a chain.* The pipe being then obstructed at every two feet by some stuffing readily hooked up, the thermometer may be easily examined, and a register of its indications kept with very little trouble. In like manner, the temperature of wells may be registered.

Of the Temperature of the Sea.—The surface temperature of the water at sea should be registered, as a matter of course, with the same regularity and at the same hours as the barometer and thermometer. It is more conveniently (and with quite accuracy enough for the purpose) obtained by taking up a bucketfull of the water and stirring round the thermometer in it. Whenever a change to the extent of 2° Fahr. appears to have taken place since the last observation, a fresh bucketfull should be taken up and the observation repeated. It should also be noticed whether rain has fallen since the last observation. A sudden depression of 3° or 4° indicates the near approach of land. In a voyage from England, lately made by a member of this committee, the temperature of the surface water fell at once 9° Fahr. on approaching within a few miles of the entrance of Table Bay.

The temperature of the sea at considerable depths can hardly be regarded as a subject of ordinary meteorological inquiry and regular registry, though undoubtedly one of much physical interest, for which reason it is not considered necessary to dwell further on it.

Of the Hygrometer, &c.—In the absence of Daniell's Hygrometer, or of ether to cool it, the degree of dryness of the air may be ascertained by observing the temperatures marked by two thermometers suspended freely side by side (but not in contact) in the shade, and completely defended from all radiation *to* or *from* the *sky*, the one having its bulb and stem naked, the other with the bulb and lower part of the stem wrapped in linen or cotton, and thoroughly wetted with pure spring or rain water. The temperatures indicated by both should be noted when the wetted thermometer refuses to sink lower, and the conclusions left for subsequent calculations. The naked thermometer may be the "External Thermometer" itself, in which case a coated thermometer may be kept always suspended near it, completely screened as above mentioned, and wetted some minutes previous to the regular daily readings.

If a hair hygrometer be used, its points of absolute moisture and dryness should be frequently ascertained, as they are apt to change. The former may be found by keeping it some time in a close covered jar lined with wet blotting paper, and having water in it, and noting the point of moisture beyond which it refuses to go. The latter, by keeping it in the same manner in a jar perfectly air-tight, over fresh burnt quicklime, till it refuses to indicate a higher degree of dryness.

The best measure of the *momentary evaporating power* of the air, seems to be the depression of the wetted thermometer below the dry one. But the *actual evaporation* from a given surface, is quite another thing, and a question may very reasonably be raised, how far any useful approximation to a knowledge of the total evaporation from an extensive and diversified surface, unequally moistened, and variously exposed to the sun, defended by clouds, or refreshed by dews, *can* be obtained by any small or local experiments.

The Rain-gauge is an instrument of such extremely easy construction that any person who lives near a tin-man can procure one. In a climate so arid as that of Africa, however, it must be remembered that it will often need examination and cleansing, owing to long intervals of disuse in which insects and dust may lodge. It will often happen, too, that the slight rain of one day, if left unregistered, will be entirely lost by evaporation in the next—nay, that slight and transient showers may never enter it, being evaporated from it as they fall. The effect of copious dew, too, must be separated from that of rain, so that the mere registry of the contents of the gauge is not of itself a sufficient indication whether rain has fallen in the night or not. However, there are usually good reasons for decision on this point from other indications. Attention to the amount of dew is very necessary, not only because the meteorological questions involved are of a high degree of interest generally, but because in arid climates the dews are of almost as much importance to the maintenance of vegetation as the rain.

In stating the quantity of rain daily received in the gauge, the height of the receiver above the soil should be mentioned, experience having shown that the quantities of rain which actually fall on a given area on the ground, and at a very moderate height above it, often differ materially. In some localities and circumstances, the rain-drops receive accession from the air as they descend, in others they undergo partial evaporation. The former is generally the case in cool moist climates—the latter may be expected in this country.

Of the Wind.—The points most important to remark respecting the wind, are,

1st, Its average intensity and general direction during the several portions of the day devoted to observation and registry.

2dly, The hours of the day or night when it commences to blow from a calm, or subsides into one from a breeze.

3dly, The hours at which any remarkable changes of its direction take place.

4thly, The course which it takes in veering, and the quarter in which it ultimately settles.

5thly, The usual course of *periodical winds,* or such as remarkably prevail during certain seasons, with the law of their diurnal progress both as to direction and intensity—at what hours and by what degrees they commence, attain their maximum, and subside, and through what points of the compass they run in so doing.

6thly, The existence of Crossing Currents at different heights in the atmosphere, as indicated by the course of the clouds in different strata. In observing these, it is advisable to fix the eye by some immoveable object, as some point of a tree or building, the sun, or the moon, otherwise mistakes are apt to arise.

7thly, The times of setting-in of remarkably hot or cold winds,—the quarters from which they come, and their courses, as connected with the progressive changes in their temperature.

8thly, The connexion of rainy, cloudy, or fair weather, with the quarter from which the wind blows or has blown, for some time previous.

9thly, The usual character of the winds as to moisture on dryness, not as deduced from mere opinion or vague estimation, but from actual observation of the hygrometric state of the atmosphere during their prevalence.

Among these particulars it will be seen that some are of a nature susceptible of daily observation and registry, while others call for an exercise of the combining and inductive faculty on the observer's part, and cannot be made out otherwise than by continued attention and habitual notice of phenomena with a view to the investigation of their laws. The general impression left upon the mind as to any of the points of this kind above enumerated, by the occurrences of the past month, will therefore be more properly stated, in the way of summary remarks at the end of the Monthly Registers, than as entries under particular days.

Of the State of the Sky.—In describing the state of the sky as to clouds, &c. the observer will bear in mind that it is only in that region of the sky which is vertically above him that the true forms and outlines of the clouds are exhibited, and the area they cover, as well as the intervals between them distinctly seen. As they approach the horizon in any direction, their extent is foreshortened by perspective, their apparent magnitude diminished by distance, and their intervals covered in and hidden by their mutual interposition. In estimating therefore the quantity of clouds in the sky, regard must be had to this, and our judgment should rather be formed on a view of the region extending from the zenith every way half way down to the horizon, that from the aspect of the heavens below that limit. It would be better to notice both, and state, separately, the proportions in which each are covered, and the quarter of the horizon towards which the chief masses in the lower region lie.

The general aspect of Clouds, as classed under the heads Cumulus, Cirrus, Stratus, &c. should be noticed, and especially the height of this inferior surface, or the level of the *vapour plane*, should be estimated. In a mountainous region this is easy, so long as the vapour plane is below or not far above the summits of the hills, and in such regions the formation and dissipation of cloud in the neighbourhood of the mountain summits, under the influence of certain winds, form a subject of study of a highly curious and interesting nature.

The formation of Clouds at night, during calm weather, under the influence of a gradually descending temperature, is another point worthy of attention. It frequently happens, that, without any perceptible wind, the sky will suddenly become hazy in some one point, and the haze condensing and spreading, in all directions, without a wind, the whole heaven will become overcast in a remarkably short time. The same thing will sometimes occur nearly at the same hour for many nights in succession. Such phenomena should be noted whenever they occur.

Two or even three strata of clouds are very common in this district of South Africa. The lowest frequently resting immediately on the land and sea. The height and thickness of these strata, their connexion with cross or opposite currents of wind in the regions where they subsist, and the laws of their formation in gradual intermixture, deserve to be studied with care, and with reference to the hygrometic state of the air at the time and place, and for several hours before and after.

Of Thunder and Lightning, and of the Electrical state of the Air.—Connected with this part of the subject is the observation of shooting Stars and luminous Meteors. Remarkable ones should be noticed, and the moment of their appearance, their direction, duration, length of path, and *course among the Stars,* ascertained and noted, with the phenomena of their increase and decay of light, apparent size, separation into parts, trains left behind, &c. The *general* direction (if any) which they observe on particular nights, is a point also to be attended to. Such are the frequency and brilliancy of these splendid phenomena in the clear sky of this colony, that there can be no doubt of their affording an available method of ascertaining the differences of longitude of the most distant stations, if duly observed by persons furnished with means of ascertaining the time.

Thunder-storms of course will be noticed when they occur under the general head of the weather, but it is of consequence also to notice distant lightning, not accompanied with thunder audible at the place of observation (by reason of its great distance), * especially if it takes place many days in succession, and to note the quarter of the horizon where it appears, and the extent it embraces. In an actual thunder-storm, especial notice should be taken of the quantity of rain that falls, and of the fits or intermittances of its fall, as corresponding, or not, to great bursts of lightning, as also of the direction of the wind and the apparent progress of the storm with or against it.

Observations of the Electrical state of the Air in serene weather are unfortunately too much neglected. The apparatus they require is simple, and by no means costly, and may be constructed indeed by any one for himself with ease.

If the Committee in this their first Report do not dilate on this and other of the less usually practised observations of Meteorology, it is because they wish for the present chiefly to call attention to the accumulation of regular and daily observations of a more definite and numerical character. With this view they have drawn up, and by the liberal aid of Government, have procured to be printed skeleton forms, of which a copy is annexed, for immediate distribution among such Correspondents of the Institution, and others, as may be willing to undertake their filling up. These comprise, it is true, only the registers of the Barometer and its attached Thermometer, with that of the external Thermometer, and a column of Remarks for Wind and Weather, as being the most essential and indispensable elements of Meteorology; but it is in the power of any one who pleases to supply additional information, and to those who have leisure, instruments, and inclination for the task, the Committee would particularly recommend the regular observation of the Wet Thermometer, those of the Self-registering Thermometer and Weekly or Monthly Observations of Thermometers buried at different and progressive depths beneath the surface of the soil.

The printed forms provide for the arithmetical convenience of casting up the *means* for each month. In doing so, it is requested that care will be taken to verify the results by repetition, and (that usual sources of error may not escape notice) they recommend in every instance, before adding up the columns, to look down each to see that no obvious error of entry (as of an inch in the barometer, a very common error, or what is more difficult of detection, an error in the first decimal place) shall remain to vitiate the mean result. It is perhaps unnecessary to do more than mention the precaution of *counting* the days in *each* column on which observations occur, so as to admit of no mistake in the *divisor*, and to use throughout the decimal arithmetic in calculating the mean results. Care and exactness in these points will in

* Thunder can scarcely ever be heard more than 20 or 30 miles from the flash which produces it. Lightning, on the other hand, may be seen (or at least its reflexion on the clouds, forming what is called *sheet lightning*) at the distance of 150 or 200 miles.

most cases add greatly to the value of the communications, as it will be quite impracticable for the Committee, should observations flow in in masses, unreduced or erroneously reduced, to undertake the overwhelming task of recomputing them.

Although not, strictly speaking, a branch of meteorology, yet as the collection of observations of the Tides has been made a part of the duties of your Committee, they propose the following stations as points where it would be especially desirable to obtain regular observations of the time and height of high and low water, according to the rules and on the plan proposed by Mr Whewell, in his late researches on this subject, and they earnestly invite communications on this head from any residents at those ports who may have leisure and take interest enough in the important questions connected with the subject.

Cape Town,	Ascension,
Simon's Bay,	Mauritius,
Port Elizabeth,	Tristan d'Acunha,
Knysna,	Madagascar,
Saldanha Bay,	Mozambique.

In Cape Town and Simon's Bay, they have the pleasure to report, that a series of observations under the superintendence of Captain Bance and Mr Levien have already been undertaken at the instance of the Astronomer-Royal, and are now in active progress.

On a Method of Drilling, Turning, and Working Glass, by means of Turpentine. By Mr John Adie.*

58 Prince's Street, Edinburgh,
8th February 1836.

Dear Sir,

Agreeably to your request, I shall here state the method of drilling or turning glass, which I had the honour of communicating to the Society of Arts last Session.

It consists simply in using oil of turpentine with the common drill, file, or turning tool; and if care be taken that the cutting edge is never dry, it will not soon blunt, and the glass will be worked with great expedition.

I learned this curious fact when in London last spring, and that it was by accident discovered by a Frenchman. I am, dear Sir, yours, &c. (Signed) John Adie.

To the Secretary of the
Society of Arts for Scotland.

* Read before the Society of Arts for Scotland, 11th March 1835.

Temperature of Quadrupeds, Birds, Fishes, Plants, Trees, and Earth, as ascertained at different times and places in Arctic America, during Captain BACK*'s Expedition.* By Mr KING, Surgeon to the Expedition.

Date.	Hour.	Sun.	Wind.	Subject.	Thermometer, where placed.	Distance from Water. — Age.	Sex.	Habitat.	Temperature of subject.	Temperature of atmosphere.
1833, October 25.	Noon	Obsc.	NE. 2	Fir-tree 13 inches diameter	Centre	. . .	. .	Slave Lake	+ 32°	+ 12°
— 25.	. .	. .	. . . .	Earth of sand 1 foot	. . .	300 paces	. .	. . .	+ 28	+ 12
— 26.	1 P. M.	. .	N.NW. 3	Wood Partridge	Chest	. . .	Male	. . .	+110	+ 9
— 27.	Noon	. .	N.NW. 2	Tom-tit	. . .	. . .	. .	. . .	+100	+ 16
— 28.	11 A. M.	Clear	E. 1	Wood Partridge	. . .	. . .	. .	. . .	+109½	+ 5
— 29.	3 P. M.	Obsc.	E. 3	Wood Partridge	. . .	. . .	Hen	. . .	+109	+ 17
— 29.	. .	. .	. . .	Wood Partridge	. . .	. . .	. .	. . .	+110	+ 17
Novem. 4.	10 A. M.	. .	S. 2	Trout	Abdomen	. . .	. .	. . .	+ 36	+ 31
1834, January 5.	1 P. M.	. .	0	White Partridge	Chest	. . .	. .	. . .	+108½	— 3½
— 7.	2 P. M.	Clear	NW. 4	Redpole	. . .	. . .	Male	. . .	+ 99	— 7
— 7.	. .	. .	. . .	White Partridge	. . .	. . .	. .	. . .	+110	— 27
— 11.	. .	Obsc.	0	White Partridge	. . .	. . .	. .	. . .	+110	— 32½
— 21.	. .	Clear	NE. 1	Ptarmigan	. . .	. . .	Hen	. . .	+106½	— 41
April 2.	1 P. M.	Bright	E. 5	Squirrel	. . .	. . .	Male	. . .	+102	— 19
May 11.	. .	Clear	E. 2	Fir of 9 inches diameter	Centre	. . .	. .	. . .	+ 34	+ 40
— 11.	. .	. .	. . .	Birch of 5 inches diameter	. . .	. . .	. .	. . .	+ 32	+ 34½
— 12.	10 A. M.	. .	0	Fir } The same borings as yesterday. Holes had been kept well stopped.	. . .	. . .	. .	. . .	+ 33	+ 39
— 12.	. .	. .	. . .	Birch }	. . .	. . .	. .	. . .	+ 31	+ 39
— 12.	12 A. M.	. .	E. by S 3	Fir of 4½ inches diameter	. . .	. . .	. .	. . .	+ 33½	+ 41½
— 12.	. .	. .	. . .	Birch of 4 inches diameter	. . .	. . .	. .	. . .	+ 33	+ 42
— 13.	. .	. .	SE. 2	Fir of 3 inches diameter	. . .	. . .	. .	. . .	+ 61	+ 55
— 13.	. .	. .	SE. 2	Birch of 2½ inches diameter	. . .	. . .	. .	. . .	+ 55	+ 55
— 13.	. .	. .	. . .	Shrubby Birch	. . .	. . .	. .	. . .	+ 64½	+ 53

Temperature of Quadrupeds, &c. in Arctic America.—continued.

Date.		Hour.	Sun.	Wind.	Subject.	Thermometer, where placed.	Distance from Water. — Age.	Sex.	Habitat.	Temperature of subject.	Temperature of atmosphere.
1834, May	14.	12 A. M.	Clear	W. 3	Fir of 4 inches diameter	Centre	. . .	. .	Slave Lake	+ 57½°	+ 52°
—	14.	. .	. .	. . .	Birch of 2 inches diameter	. . .	. . .	. .	. . .	+ 52	+ 49
—	14.	. .	. .	. . .	Shrubby Birch	. . .	. . .	. .	. . .	+ 63½	+ 51½
—	15.	. .	. .	N. 2	Fir of 3½ inches diameter	. . .	. . .	. .	. . .	+ 33	+ 30
—	15.	. .	. .	. . .	Birch of 2 inches diameter	. . .	. . .	. .	. . .	+ 34	+ 30
—	15.	. .	. .	. . .	Shrubby Birch	. . .	. . .	. .	. . .	+ 40	+ 31
—	16.	. .	Dim	E. 4	Fir ef 4 inches diameter	. . .	. . .	. .	. . .	+ 48½	+ 48½
—	16.	. .	. .	. . .	Birch of 2½ inches diameter	. . .	. . .	. .	. . .	+ 43	+ 48
—	16.	. .	. .	. . .	Shrubby Birch	. . .	. . .	. .	. . .	+ 63	+ 61
—	17.	2 P. M.	Clear	N. 3	Squirrel	Chest	. . .	Female	. . .	+ 102	+ 27
—	18.	1 P. M.	. .	W.SW. 2	Wood Partridge	. . .	. . .	Hen	Slave Lake	+ 109	+ 30
—	20.	10 A. M.	. .	NE. 2	Trout	Abdomen	. . .	. .	. . .	+ 34	+ 32
—	29.	11 A. M.	. .	E. 9	White fish	. . .	. . .	. .	. . .	+ 43	+ 37
August	3.	4 P. M.	. .	. . .	Earth of sand	Surface	1 mile	. .	Sea-coast	+ 47	+ 60
—	3.	6 P. M.	. .	. . .	Sandpiper	Chest	. . .	. .	. . .	+ 107	+ 60
—	4.	Noon	. .	. . .	Earth of sand	Surface	200 paces	. .	. . .	+ 60	+ 56
—	4.	5 P. M.	. .	. . .	Earth of sand	. . .	1 mile	. .	. . .	+ 48	+ 53
—	4.	. .	. .	. . .	Earth of sand	2 feet	. . .	. .	. . .	+ 38	+ 53
—	4.	6 P. M.	. .	. . .	Brown Diver	Chest	. . .	. .	. . .	+ 106½	+ 50
—	4.	. .	. .	. . .	Northern Diver	. . .	1 week	. .	. . .	+ 95	+ 50
—	4.	. .	. .	. . .	Northern Diver	. . .	. . .	. .	. . .	+ 93	+ 50
—	7.	1 P. M.	. .	. . .	Earth of sand	Surface	500 paces	. .	. . .	+ 60	+ 54
—	7.	1 P. M.	Clear	. . .	Earth of sand	1 foot	500 do.	. .	Sea-coast	+ 34	+ 54
—	11.	3 P. M.	Obsc.	. . .	Musk Ox	Through ball-wound, blood oozing from heart.	1 year	Bull	. . .	+ 104	+ 41
—	11.	5 P. M.	. .	. . .	Lemming	Chest	1 month	Female	. . .	+ 93	+ 39

This Table is copied from Captain Back's interesting "Narrative of the Arctic Land Expedition," just published by Mr Murray of London.

General Table of Meteorological Observations at Fort Vancouver, from

	General Annual Means.	June.	July.	August.	September.	October.
Barometer, English inches, reduced to 32° F.	9 A. M. 30.043	30.035	30.070	30.005	30.049	30.026
	Noon, 30.035	29.999	30.020	29.953	30.024	30.013
	3 P. M. 29.985	29.953	29.990	29.899	29.968	29.955
	9 P. M. 30.041	30.006	30.008	29.957	30.011	30.014
	Max. 30.624	30.337	30.215	30.301	30.320	30.431
	Min. 28.994	29.665	29.798	29.689	29.623	28.994
	Range, 1.630	0.672	0.417	0.612	0.697	1.437
Thermometer of Fahrenheit corrected for ind. error by standard of Royal Soc. L.	7–8 A. M. 45°.8	56°.8	59°.6	56°.6	53°.9	46°.2
	Noon, 58.4	72.1	76.8	73.6	69.6	57.4
	3 P. M. 59.1	74.0	80.6	75.4	70.2	58.9
	Sunset, 54.8	66.5	72.8	68.9	63.5	55.3
	Max. 92	92	92	86	86	73
	Min. 18	48	54	49	46	36
	Range, 74	44	38	37	40	37
Daily Range of Thermometer,	Max. 33°	32°	31°	27°	32°	33°
	Min. 2	8	9	7	2	5
	Mean, 14.5	17.5	21.4	19.4	17.1	15.9
Days of Rain, on which any Rain fell,	157	4, 5, 6, 7, 9, 10, 11, 12, 20 = 9	5, 19, 22, 23 = 4	14, 15, 17, 25 = 4	14, 15, 17, 18, 27, 28, 29 = 7	1, 4th — 7th, 9, 10, 15, 24, 26th — 31 = 15
Depth of Rain in English in.	53.646	0.880	0.118	0.407	2.040	7.540
Days on which any Snow fell,	1	0	0	0	0	0
Depth of Snow in Engl. feet,	0.08	0	0	0	0	0
Prevailing Winds,	N. 26 S. 149	0 13	10 8	3 17	7 11	0 13
	NE. 14 SW. 178	0 46	1 34	0 28	4 17	0 19
	E. 101 W. 31	2 3	0 3	0 9	2 8	5 2
	SE. 136 NW. 81	4 4	1 21	2 9	10 13	16 13
No. of Observations,	Calm 159 Tot. 885	16 88	20 86	14 94	11 83	20 88
Days of Frost, Ther. observed below 32° F.	34	0	0	0	0	0
Days of Hail,	4	0	0	0	0	0
Days of Thunder,	2	0	0	1, 20 = 2	0	0
Evaporation of water in English inches in 24 hours,	Max. 0.90	0.70	0.90	0.50	0.45	0.29
	Min. 0.00	0.15	0.15	0.10	0.04	0.06
	Mean, 0.18	0.29	0.48	0.28	0.28	0.14
	No. Obs. 188	18	26	22	23	14
	Total, . .	. . .	. . .	8.60	5.23	2.77
Dew-point at Noon,	Max. 56					56°
	Min. 23					38
	Mean, .					47
Diff. between dew-pt. and temp. of air,	Max. 24	No obs.	No obs.	No obs.	No obs.	23
	Min. 1					5
	Mean, .					12
No. of Obs.	147					29

N. B.—The depth of Rain is ascertained by weighing the water at end of every month, received into a close bottle, by a funnel of known diameter, and calculating from thence the depth in inches by Dalton's formula in his Meteorological Essays.

Evaporation is measured by immersing a graduated style into a pewter basin five inches in diameter exposed freely to the air.

June 1. 1834 *to May* 13. 1835. Communicated by Dr M. Gairdner.

November.	December.	January.	February.	March.	April.	May.
29.972	30.122	29.874	30.125	30.135	30.167 15 obs.	29.936 12 obs.
29.968	30.122	29.859	30.118	30.149	30.193 18 obs.	30.000 10 obs.
29.945	30.057	29.850	30.077	30.124	30.095 15 obs.	29.909 8 obs.
29.970	30.123	29.871	30.108	30.168	30.218 5 obs.	
30.263	30.611	30.454	30.521	30.624	30.476	30.336
29.287	29.241	29.465	29.555	29.498	29.806	29.596
0.976	1.370	0.989	0.966	1.126	0.670	0.740
41°.9	33°.8	38°.6	37°.4	39°.0	40°.4 7 obs.	
50.9	41.0	44.7	47.4	51.6	57.9 13 obs.	
49.2	40.9	45.0	47.2	52.2	62.7 11 obs.	63°.6 8 obs.
48.9	37.8	43.0	42.5	46.3	56.4 14 obs.	56.1 12 obs.
61	5	56	58	64	73	
31	18	29	28	31	34	
30	32	27	30	33	39	
22°	20°	17°	26°	20°	29°	
5	3	2	3	5	5	No obs.
11.3	9.7	8.2	11.8	12.8		
1st — 5th, 8th, 11th — 21st, 27th — 30th = 21	1st — 8th, 21, 25th — 31 = 16	1st — 16th, 18th — 21st, 23d — 29th = 26	10th — 14th, 15th — 24th, 27, 28 = 8	1 — 7, 8 — 10, 16, 18, 19, 20, 22 — 31 = 21	3, 7th — 10th, 15th — 17th = 8	2, 11th — 16th, 19, 21, 26, 27 = 11
4.550	9.846	10.646	6.654	6.965	2.000	2.000
0	25 = 1	0	0	0	0	0
0	0.08	0	0	0	0	0
0 13	1 12	0 11	3 11	2 37	0 3	
2 4	0 1	0 3	1 11	5 4	1 11	
14 2	21 0	20 1	22 3	10 0	5 0	No obs.
25 4	28 0	27 5	14 7	9 4	0 1	
17 81	19 82	17 84	12 84	20 91	3 24	
25, 26 = 2	10 — 24th = 15	14, 17, 18, 22, 30, 31, = 6	4th — 9th, 25, 26 = 8	5, 11, 17 = 3	0	0
0	0	0	0	8, 23, 29 = 3	11 = 1	0
0	0	0	0	0	0	0
0.25	0.07	0.13	0.27	0.22	0.26	
0.03	0.06	0.00	0.00	0.07	0.07	
0.11	0.06	0.06	0.09	0.11		No obs.
8	2	26	24	21	4	
1.50	1.81	1.74	2.12	3.08		
50°	44°	49°	52	44°	41°	
35	28	36	23	32	36	
43	37	42	39	39		
13	14	7	21	21	24	No obs.
1	1	1	4	4	15	
7	6	4	10	11		
27	15	24	24	24	4	

Dew-point is ascertained by means of an instrument on the principle of Pouillet's, but using a thin transparent glass capsule, which, upon trial, I have found more delicate than a polished metallic surface.

Fort Vancouver is about 60 feet above the level of the sea.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr Graham, Prof. of Botany.

June 10. 1836.

Acacia lineata.

A. *lineata;* stipulis subnullis; phyllodiis lineari-spathulatis, subfalcatis, versus marginem superiorem uninervibus, oblique cartilagineo-mucronulatis, ramuloque rotundato pubescentibus; capitulis longe pedunculatis, subgeminis.

Acacia lineata, *Cunningham,* in Don's Syst. of Gard. 2. 403. n. 28.—*Bot. Mag.* 3346.

Description.—*Shrub* slender; branches diffuse, round, rough, tubercled by the persistent hardened bases of the phyllodia; young shoots densely pubescent, pubescence spreading or slightly reflexed, somewhat viscid. *Phyllodia* linear-spathulate, slightly falcate downwards, pubescent in rather a smaller degree than the young branches, marked by an obscure sessile gland on their upper edge near the base, terminated obliquely by a small blunt somewhat hooked callosity, single-nerved, the nerve being towards the upper edge, and slightly, but not always evidently branching, upon its lower side. *Peduncles* twice as long as the phyllodia, single or geminate, supporting single capitula of orange-yellow perfumed polygamous *flowers,* each of which rises from the axil of a concave, rounded, ciliated, nearly colourless *bractea. Calyx* segments 5, minute, linear, ciliated. *Corolla* 5-petalled, petals ovate, acute, clawed, spreading, twice as long as the calyx. *Stamens* very numerous, twice as long as the corolla. *Germen* oblong, pale green. *Style* oblique, longer than the stamens.

We received this plant from Kew, and it flowered in the greenhouse of the Botanic Garden in February 1836.

Begonia Fischeri.

B. *Fischeri;* caulescens, foliis oblongis, acutis, inæqualiter cordatis, dentato-serratis, utrinque glabris, nitidis; stipulis ovatis, integerrimis; floribus masculis 4-petalis, petalis exterioribus rotundis, concavis, marginibus plano-revolutis; floribus fœmineis 6-petalis, petalis ovato-lanceolatis, alis germinis inæqualiter rotundatis.

Begonia Fischeri, *Otto,* MS.

Description.—*Stem* erect, branched, fleshy, swollen at the joints, red, shining. *Leaves* unequally cordate, acute, indistinctly sinuated, slightly undulate, unequally dentato-serrate, glabrous on both sides, when young bright red behind, paler at the veins, and pink above, with a peculiar silvery lustre, which continues in the old leaves, the colour being then beautifully delicate yellowish-green, and the redness behind much less considerable; petioles nearly round, half as long as the leaves. *Stipules* large, ovate, acute, entire. *Peduncles* axillary, twice as long as the petioles. *Cyme* twice or oftener forked, branches divaricated. *Male flowers* in the forks of the cymes, 4-petalous, the outer petals rounded, with a sinuosity on one side, where the pedicel is attached, hollow in the centre, with flattened somewhat revolute edges, forming a perfect miniature of a barber's bason; inner petals obovato-cuneate, undulate. *Stamens* united only at their insertion. *Female flowers* 6-petalous, the petals ovato-lanceolate. *Germen* rather unequally winged, the wings unequally rounded.

This plant, which has small flowers, but exquisitely beautiful foliage, was received at the Botanic Garden from Berlin, under the name adopted, in 1835, and flowered in the stove in February and March 1836.

Begonia sanguinea.

B. *sanguinea ;* caule ramoso ; foliis inæqualiter cordatis, acuminatis, coriaceo-carnosis, glaberrimis, subtus sanguineis, margine crenulato revoluto ; germinis alis tribus æqualibus.

Begonia sanguinea, *Radd.* in Spreng. Syst. Veget. 2. 625.—*Link and Otto,* Icones Plant. Rarior. Hort. Reg. Bot. Berol. p. 25, t. 13.

Description.—*Stems* several from the crown of the root, subligneous, red with scattered oblong paler spots. *Leaves* (4–6 inches long, 2½–3½ inches broad) subpeltate, unequally cordate, acuminate, the apex soon withering, leathery-succulent, perfectly glabrous and shining on both sides, green above, blood-red below, the edge crenulate and revolute all round ; nerves about 10, radiating, the larger branched, the smaller subsimple. *Petioles* of very unequal length, round, resembling the stem. *Stipules* intrafoliaceous, large, ovate, acute, keeled, marcescent. *Peduncle* (10 inches long) terminal, becoming axillary, tapered, similar to the stem, but without spots, repeatedly dichotomous at the apex (primary branches about one inch long, the others gradually shorter). *Bracteæ* lanceolato-elliptical at each subdivision. *Flowers* white, rather small. *Male flowers* in the clefts of the cyme, or on the inner side, when the ultimate branches are reduced to two flowers, the outer being a female ; *petals* four, the two outer subrotund, slightly crenate, the two inner linear-elliptical, very narrow, entire ; *stamens* numerous, filaments free, excepting at the base, where monadelphous, anthers spathulate. *Female flowers* with five subequal *petals*, expanding later than the earlier of the males ; *stigmas* pale rose-coloured ; *germen* with three subequal wings.

I was at some pains to ascertain the relative position of the male and female flowers, when only these two were found at the extremity of the ultimate branch. It seems to me that the normal form is the conversion of the last dichotomous ramification of the cyme into the pedicels of two female flowers, and that the male flower, here, as elsewhere, is placed in the cleft, the loss of the inner female flower being an illustration of the opinion that internal parts, from pressure, more frequently abort than those which are external. As the common support of these two flowers generally turns half-round on its axis, their true position may not be obvious, unless examined when they are very young.

This plant, more remarkable for the colour and texture of its leaves than elegant in its form, was raised at the Botanic Garden, Berlin, from seed transmitted by Sello from Brazil in 1823, and communicated to the Botanic Garden, Edinburgh, in 1832. It flowers in the stove in April.

Bletia patula.

B. *patula ;* foliis radicalibus, lanceolatis, plicato-nervosis ; scapo elato, subramoso ; floribus patentissimis ; sepalis lanceolato-ellipticis, basi attenuatis, subæqualibus, patulis ; labello cucullato, lobis lateralibus rotundatis, intermedio emarginato transverse plicato, disco lamellis 6, subramosis, inæqualibus.

Description.—*Bulb* round, at first, when it pushes up the scape, very small, gradually enlarging to the size of a small orange ; in the second year green, shining, nearly smooth, and crowned by the withered bases, of the leaves, when these fall, marked with three or four circular bands, and furrowed at the apex, persisting for some years, and becoming gradually smaller without shrinking much ; young bulbs are formed at the base, or near the apex of those of the year preceding. *Leaves* arising from the apex of the bulb after the flowers, lanceolate, plicato-nervose. *Scape* (above 3 feet high) purplish and spotted at the base, with a few distant sheathing scales, greener above, subramous, terminal, but from its appearing in the very young state of the bulb seeming to be lateral, the old bulb only being conspicuous. *Raceme* (above 20-flowered) gradually elongating. *Flowers* large, very handsome, each springing from the axil of a small acute bractea, of nearly uniform reddish-lilac colour, only the base of the lip and its ridges being white. *Sepals* (1½ inch long) lanceolato-elliptical, nearly equal in size, the uppermost being rather the narrowest, all attenuated at the base, and spreading. *Lip* without spur, much broader than the sepals, the lateral lobes erect, the central broad-linear, notched, plaited transversely; disk with six waved, somewhat branched lamellæ, those nearest the sides being the shortest, and passing into diverging veins. *Column* more than half as long as the sepals, projecting into the centre of the flower, somewhat clavate, rounded on the upper, flat on the lower side, with a single tooth on each edge at its middle, a small terminal tooth, and two others on each edge immediately below the apex, the lower being rounded and decurrent. *Anther-case* rounded, emarginate, 2-celled, each cell divided longitudinally. *Pollen-masses* 4, parallel, each 2-lobed, laid along a thin plate, which is spread above the moist stigmatic surface. *Germen* (1½ inch long) twisted, spreading at right angles to the rachis.

This handsome species was received at the Botanic Garden from Dr Fischer, St Petersburgh, in 1830, without specific name, but marked as a native of Hayti. It has repeatedly since flowered freely in our stove, immediately succeeding *Bletia verecunda* in the end of March.

Drosera filiformis.

D. *filiformis;* scapis lateralibus, foliis lineari-filiformibus glanduloso-pilosis, dorso glabris canaliculatis, basi lanatis; staminibus 5; stylis 8, in paribus coalitis, basi.

Drosera filiformis, *Rafinesque*, in Need. Rep. 2. 360.—*Ibid*, in Desv. Journ. de Bot. 1. 227.—*Pursh*, Fl. Amer. Sept. 1. 211.—*Nutt.* Gen. 1. 142.—*Roem. et Schult.* Syst. Veget. 6. 763.—*De Cand.* Prodr. 1. 318.—*Torrey*, Fl. of North. and Mid. Sect. of United States, 1. 332.—*Spreng.* Syst. Veget. 1. 955.—*Beck*, Bot. of North and Mid. States, p. 42.

Drosera tenuifolia, *Willd.* Enumer. p. 340.—*Roem. et Schult.* 1. 763.—*Bigelow*, Plants of Boston, p. 124.

Description.—*Primordial leaves* deltoideo-subulate, glabrous; *secondary leaves* radical, linear, circinate, very woolly at their base, channelled and glabrous behind, in front rounded and covered with spreading greenish hairs, which support on their summit a red gland, and exude a viscid colourless juice. *Scape* lateral, with us always simple, green, glabrous, about as long as the leaves. *Spike* racemose, unilateral. *Pedicels* and 5-cleft persisting *calyx* covered with green glandular hairs. *Corolla* glabrous, rose-coloured, more than twice as long as the calyx, marcescent; petals 5, obovate, claws greenish. *Stamens* 5, scarcely longer than the calyx; filaments colourless; anthers erect, oblong, yellow; pollen granules round, yellow. *Styles* 8, spreading at their base in pairs, above erect and clavate, colourless. *Germen* round, green, ovules numerous, oblong.

This remarkable species was found by Mr James Macnab in a swamp about ten miles above Tuckerton, New Jersey, U. S., and introduced by him into the gardens about Edinburgh in 1834. It flowered freely in the stove at Dr Neill's, at Comely Bank Nursery, and with us. I cannot hesitate to agree with those who consider *Drosera tenuifolia* of Willd., synonymous with *D. filiformis* of Rafinesque, which, being the earliest published name, I retain.

Epacris microphylla.

E. *microphylla;* calycis foliolis obtusiusculis, tubum corollæ æquantibus; foliis cordatis, acutis, pedunculum superantibus, lateribus erectis; spica apiciflora; ramulis pilosis.

Epacris microphylla, *Br.* Prodr. Flor. Nov. Holland. 550.

Description.—*Shrub* erect; branches ascending, slender, tortuous, hairy. *Leaves* cordate, spreading, shining, rigid, sides folded forwards, base embracing the stem, middle bent back, apex ascending, acute. *Flowers* white, solitary in the axils of the leaves, collected into subterminal *pseudo-spikes*, on peduncles shorter than the leaves. *Calyx leafets* oblong, blunt, white. *Corolla* white; tube campanulate, equal to the calyx, nectariferous; limb 5-parted, segments ovate, blunt, spreading. *Stamens* alternating with the segments of the corolla; filaments adhering to the tube, the free portion erect, shorter than the limb; anthers bilocular, bursting along the face. *Germen* spheroid, with a short style rising from the depressed apex, and supporting a stigma of five erect blunt lobes.

We received this plant at the Botanic Garden, Edinburgh, in 1835, from Mr Westland's nursery, near Dorking, Surrey, where a large stock of scarce plants, and of *Epacridæ* in particular, are cultivated with a very remarkable degree of success. It flowered in the greenhouse during the month of March.

Mr Brown places this species next to *E. pulchella*, and, were it not for his authority, I should certainly have considered it only a variety of this. I do not find the difference in the form of the calyx segments, nor of the leaves, which Mr Brown observes, and would rest the chief distinction on the more slender, less tortuous branches of *E. microphylla*, the hairiness of the young shoots, the shortness of the peduncles in relation to the leaves, and in the pseudo-spikes being collected nearer the extremities of the branches.

Fritillaria ruthenica.

F. *ruthenica ;* caule subunifloro; foliis lineari-lanceolatis, imis superioribusque subternatis, illis obtusis, his, intermediisque sparsis, cirrhosis; floribus tesselatis cernuis.

Fritillaria ruthenica, *Wickstrom ?*
Corona verticillata, *Fischer.* MS.

Description.—*Stem* slender. *Leaves* linear-lanceolate, glaucous, channelled above, 3–5-nerved below, upper ones erect, the others spreading, lowest and uppermost subternate, those which intervene scattered at distances gradually increasing upwards, all terminated by cirrhi, excepting those composing the lowest verticel, which are more or less blunt. *Flowers* cernuous, in the specimen described two, the one terminal, springing from the centre of three leaves, the other a little below the apex, and subtended by two leaves, which are in contact on one side of the stem. *Peduncles* (half an inch long) resembling the stem. *Perianth* campanulate, tesselated, dark purple on the outside, somewhat paler within, segments terminated with a small tuft of minute hairs, and having along the centre of the inside of each a linear green mark, which leads to a subrotund nectary near the base, corresponding to a large gibbosity without, inner segments obovate, outer elliptical and narrower. *Stamens* half the length of the perianth; filaments filiform, very slender; anthers erect, equal in length to the filaments, yellow, elliptical; pollen granules yellow, minute, subglobular. *Pistil* longer than the stamens, but shorter than the perianth; germen green, 3-sided, slightly contracted at its origin; style green, about as long as the germen, trifid, with internal deeply grooved stigmatic surfaces.

This neat little species of *Fritillaria* was received by Dr Neill under the name here adopted, from the Botanic Garden, Berlin, in August 1835, and flowered in his greenhouse at Canonmills, near Edinburgh, in the following April. No doubt it will thrive in the open air as well as any of the species.

I refer to Wickstrom with doubt, because I have not an opportunity of consulting his original observations, and the definitions of the species by authors who quote him, scarcely apply to the plant before me. The MS. name of Fischer is attached to a specimen in my herbarium from the south of the Volga, which he with his usual kindness sent to me. It differs in no essential degree which I can perceive from Dr Neill's plant, and is not the *Fritillaria verticillata* of Ledebour, which is *Fritillaria leucantha* of Bot. Mag. 3083, which I have from Dr Fischer under the names of *Imperialis leucantha* and *Corona leucantha.*

Proceedings of the Royal Society of Edinburgh.

1836, *February* 1.—Sir Thomas M. Brisbane, President, in the Chair. The following Communications were read :

1. On the Mathematical Form of the Gothic Pendant. By Professor Forbes. Published in the Annals of Philosophy.

2. On the occurrence of the Megalichthys Hibberti in a bed of Cannel Coal in the west of Fifeshire; with Observations on the supposed Lacustrine Limestone of Burdiehouse. By Leonard Horner, Esq. F.R.SS. L. & E., and F. G. S. Published in the last Number of this Journal.

February 15.—Dr Hope, V. P. in the Chair.

The Keith Prize delivered to Professor Forbes, on which occasion, an address was delivered by Dr Hope, which appeared in last number of this Journal.

March 7.—Sir T. Makdougall Brisbane, President, in the Chair. The following Papers were read ;

1. On the Non-Hellenic portion of the Latin Language. By the Venerable Archdeacon Williams.

The line of argument went to shew, that the Umbri were one of the most ancient nations of Italy. That they, through their colonies, entered deeply into the composition of the primitive population of Rome. That, according to ancient authorities, these Umbri were the descendants of the "Veteres Galli." That these Veteres Galli were of the same race and blood as the present Cumbri of Wales, Cornwall, and Brittany. That hence it is probable, that the ancient language still preserved among these may have entered early into the composition of the language of the Romans. That the names of rivers, mountains, cities, lakes, districts, &c. in central Italy, and in all the countries over which the Sabellian tribes, and their cognate race the Veneti, diffused themselves, is likely to convert this probability into certainty. That the question concerning the ancient population of Italy has never yet been satisfactorily treated; that it never can be, unless the examiner is well acquainted not only with the language, but also the literature of Greece and Rome, and with at least one type or form of the several Teutonic and Celtic languages. That a slight acquaintance with other forms is also very desirable. That the writer professes to be conversant with Greek, Roman, and Cumbrian literature, and to a certain extent with the Anglo-Saxon, and that he knows something of the Gaelic and Basque tongues. That no examination of indexes can avail, owing to the peculiar character of the Cumbrian tongue, in which a person ignorant of the principles of its grammar might suspect that there was nothing fixed, while, on the contrary, it is the most fixed and indestructible of all languages. That the vocabularies of the Latin and Cumbrian languages are strikingly similar, although their grammars are radically different. That the work of comparing the two languages etymologically would be easy, had it not been for the long stay of the Romans in Gaul and Britain, which must be supposed to have made a deep impression upon the language of the natives. That nevertheless many Latin words exist, to the primary meaning of which the Cumbrian scholar alone possesses the key, and that a long list of words belonging to such a class must prove that some cognate branch of his language must have entered into the original composition of the Latin tongue. That the strength of the proof must depend upon the extent of the induction.

2. On the Sources and Composition of the different kinds of Gamboge. By Dr Christison. 3. On the Botanical Origin of Gamboge. By Dr Graham.

Gamboge was first made known by Clusius about the commencement of the seventeenth century, as a concrete juice from China. About the middle of the same century, Bontius conceived he had traced it to a particular species of *Euphorbia*, growing in Java and in Siam; from the latter of which countries the whole gamboge of commerce was at that time obtained. About the close of that century Hermann announced that gamboge was produced by two species of trees growing in Ceylon, which have been since often confounded together, but which are now designated by the names *Garcinia Gambogia*, and *Stalagmitis Gambogioides*. About the middle of last century, gamboge was referred by Linnæus to the former of these plants, and his reference was generally admitted. But about thirty years later, Professor Murray of Göttingen conceived he had traced it satisfactorily from the specimens collected by Koenig in Ceylon, and information obtained by the same botanist in Siam, to a new species which he called *Stalagmitis gambogioides*.

Dr Graham shows, from specimens and drawings sent from Ceylon, both by Mrs Colonel Walker to himself, and by David Anderson Blair, Esq. to the late Dr Duncan, that the plant producing Ceylon gamboge is neither *Garcinia gambogia*, as Linnæus thought, nor *Xanthochymus ovalifolius*, as conjectured by Dr Wight and Mr Arnott, nor *Stalagmitis gambogioides*, according to Murray and Koenig, but is a species described by Lamarck and Gärtner under the name of *Garcinia* or *Mangostana morella*, although it differs from all of these genera in the structure of its stamens, and, therefore, probably ought to be considered a new genus among those producing a gambogioid juice.

Dr Christison proved, that, at the present time, Ceylon gamboge is not an article of European commerce, and that the whole gamboge of the markets of this country comes, as in the time of Bontius, from China. After mentioning the analysis of fine gamboge made by Braconnot in France and John in Prussia, he stated the following as the mean composition of the several varieties of gamboge he has hitherto examined :—

Pipe gamboge of Siam : Resin, 72.2 ; Arabin, 23.0 ; Moisture, 4.8 ;=100.0. Cake gamboge of Siam : Resin, 64.8 ; Arabin, 20.2 ; Fecula, 5.6 ; Lignin, 5.3 ; Moisture, 4.1 ;=100.0. Ceylon gamboge sent by Mrs Colonel Walker : Resin, 70.2 ; Arabin, 19.6 ; Fibre of wood and bark, 5.6 ; Moisture, 4.6: Ceylon gamboge, adhering to a specimen of the bark sent by Mr David Anderson Blair : Resin, 75.5 ; Arabin, 18.3 ; Cerasin, 0.7 ; Moisture, 4.8 ;=99.3.

The proportion of the gum to the resin varied somewhat in each variety, but never differed more than 2 per cent. from the means given above.

The author added, that he had found the resin to be the active principle of gamboge.

He inferred from the composition of the different kinds of gamboge, and other circumstances detailed in his paper, that the cake gamboge of Siam is not entirely a natural production, but a manufactured article : that Ceylon gamboge, if freed from incidental fibrous matter, corresponds almost exactly with Siam gamboge : that, therefore, they are probably produced by the same plant : that Ceylon gamboge possesses precisely the same medicinal properties : and that this variety, if more carefully collected, may, in all probability, be applied with equal advantage to every economical purpose which is at present served by the finest pipe gamboge of Siam.

Proceedings of the Wernerian Natural History Society.

(*Continued from vol.* xx. p. 423.)

1836, *March* 12.—Robert Jameson, Esq. P. in the Chair. The Secretary read Mr Richard Parnell's account of the occurrence of the White-bait, *Clupea alba*, in the Frith of Forth, in considerable abundance ; and also his description of the Sprat or Garvey-herring, *C. sprattus*.

March 26.—David Falconar, Esq. formerly V. P. in the Chair. The Secretary read an account of the Tadpole fish, *Raniceps tri-*

furcatus, and of the Sea-snail, *Cyclopteris liparis*, observed by him in the Frith of Forth, and specimens of both were exhibited. Mr Wilson, for the Secretary, then read Remarks on the Vitality of the Toad, communicated by the Rev. Edward Stanley of Alderley Rectory.

Dr Thomas Aitken gave an account of the anatomy of a specimen of the Ursine Sloth, *Ursus labiatus*, which died a short time ago in a travelling menagerie, while at Edinburgh, demonstrating the peculiarities of the organs of respiration and digestion. The stuffed specimen of the animal was also exhibited.

Professor Jameson exhibited a series of birds from Northern India, collected by Mr Hamilton Stirling, which, he remarked, was remarkably interesting, as presenting many species which were not known to exist in that quarter. Mr William Jameson pointed out several of these ; of the rapacious order he noticed the Milvus govinda, and Accipiter dukhunensis, the former of which was considered to be probably the young of the Falco Cheela. With regard to the geographic distribution of the genus Milvus, it was stated, that it occurs in all the different continents of the Old World and New Holland, but that it has not as yet been detected in the New World, its place being there supplied by the genus Nauclerus. Specimens of the Gypaetos barbatus were again laid before the Society, Professor Jameson having many years ago exhibited this bird sent from Northern India by Lieutenant Tytler (which, since that time, has been discovered by other travellers), for the purpose of pointing it out under the form of the Vultur Niger, it in the young state being considered as a distinct species, and described under this name ; and also for the purpose of shewing that it from the nest upwards, undergoes the same changes as the European species, a character, before all others, marking them to be one and the same species. In regard to British birds in general, in connection with Indian ornithology, Mr Jameson stated, that more than one-third of them occur in India, either identical with, or undergoing certain slight modifications in the colour of the plumage, size, &c., characters which,if their habits and manners are the same, would lead him to consider them rather as marked varieties than as new species. To the diurnal rapacious birds Mr Jameson particularly directed the attention of the Society, and stated, that of the 18 diurnal birds of prey found in this island, the following striking distribution was presented, viz. In common with Europe 3 ; Europe and Asia 2 ; Europe, Asia, and New Holland, 1 ; Europe, Asia

Africa, and New Holland 3; Europe Asia, and North America, 5; if, however, the Circus cineraceus exists in North America, which is not at all improbable, we will have 6; Europe, Asia, and South America, 1; Europe and North America 3. To these conclusions, Mr Jameson remarked, he had come, principally from an examination of the magnificent collection in the Museum of the University of Edinburgh. After some other general observations on the identity of particular species of rapacious birds, in which it was stated, that the Falco cherrug of Gray, is the female of the Falco islandicus; the Circus pallidus, Sykes, the young male of the Circus cyaneus; the Circus variegatus, the Circus rufus, &c.; Mr Jameson exhibited specimens of the Gallus bankiva in its various stages, and remarked, that it is probably one of the originals of the domestic fowl, which seems to have originated not from one, but from many species; Bucco grandis; Phasianus albo-cristatus in its various stages; Parus (Leiothrix) furcatus, Temminck; Cinclus Pallasii, Temminck. With regard to the characters assigned to the genus Leiothrix by Swainson, of which the Parus furcatus is the type, and which has been justly separated from the true Pariadæ, some observations were made, shewing that several of these are quite inapplicable to the type of the genus. In exhibiting the Cinclus Pallasii, Mr Jameson remarked, that the genus was confined for many years to but one species, the existence of the Pallas dipper being called in question, and that not found out of Europe. Now, however, we have three, and probably a fourth (a bird existing in the collection of the University of Edinburgh, which may be placed in this genus, or rather forms a connecting link between the genus Cinclus and that of Pitta), some of which are found in all the great continents of the world, with the exception of New Holland. That the Cinclus Pallasii is the same as the Cinclus Americanus, an opinion advocated by L. Bonaparte, can only be maintained by those who have not had an opportunity of comparing the two species, being very apt to be misled by the meagre description of the former by Temminck; one character alone, distinguishes the two species, viz. the Pallas Dipper is more than a third larger than the American; moreover, the latter never assumes the colour of the former, at least Mr Jameson was unable to detect, in a series of specimens of the Cinclus Americanus, in the Museum of the University of Edinburgh, the slightest approach to the tinge of colour assumed by the Pallas Dipper. A specimen of Wryneck (Yunx torquilla) was exhibited, which was killed in February last in Fifeshire.

Mr Stirling being about to return to India, was elected a corresponding member of the Society. The meeting authorized the purchase of a complete coloured copy of the "Zoological Journal," for the Society's library.

April 16.—Dr R. K. GREVILLE, formerly V. P. in the Chair. A notice was read on the dolomization of the marble limestones of Skye, with analyses of the same, shewing their magnesian character. The author also stated his views in regard to the geognostical relation of the Plutonian rocks of Skye, which he referred to the porphyry and trap formations. He noticed, besides, the rock of St Kilda and the granite of Arran, both of which exhibit several of the characters of the porphyry series, and may probably, in a geognostical sense, be considered porphyries rather than granites. The blunders in observation, and the wholesale appropriation to himself of the geology of Scotland (in despite of all the published and unpublished accounts of Scottish, English, and German geologists,) by Dr MacCulloch, were noticed; and it was remarked by several members, that a better spirit was now generally abroad, and that few were disposed to follow in the path of the author of the Geology of the Hebrides.

Mr Torrie, Assistant Secretary, read a communication from Edward Hamilton Stirling, Esq. on the Calaite or Mineral Turquois Mines of Nishapur in Persia.

James Robertson, Esq., Mining Engineer (who is appointed by the government at Teheran to conduct mining operations in Persia) was elected a corresponding member.

April 30.—ROBERT JAMESON, Esq. President, in the Chair. Mr Torrie read a letter from M. de Moligny, dated Besançon, 8th September 1835, giving an account of a tremendous fall of the mountain called the Dent du Midi. It was therein stated, that a space of about two square leagues, extending from the base of the mountain to the Rhone was covered by the debris, in some cases to the depth of ten or twelve feet.

Professor Jameson took notice of a shower of hail which had occurred immediatelybefore the time of meeting this day, mentioning that he and several other members of the Society had examined the hailstones, and ascertained that they were crystallized in the form of double six-sided pyramids, and at the same time of larger size than usual.

The Society adjourned till November next.

Proceedings of the Society for the Encouragement of the Useful Arts in Scotland.

The Society for the Encouragement of the Useful Arts met in the Royal Institution, on Wednesday the 9th March 1836, at 8 o'clock P.M. Mr Professor FORBES, Vice-President, in the Chair.

The following Communications were laid before the Society:—

1. Suggestions as to the probable good effects of employing Caoutchouc in solution in the manufacture of Ropes; and for brushing over the standing rigging, sails, &c., of ships. By Mr Thomas Johnston, 137 George Street, Glasgow.

2. Letter from the Secretary of the Manchester Mechanics' Institution regarding copy of the Society's Proceedings; and sending a copy of Lord Brougham's Address to the members of that Institution on 21st July 1835.

3. Suggestion of a new experiment whereby the Rotation of the Earth may be demonstrated. By Edward Sang, Esq. Vice-Pres. Soc. of Arts.

4. Embossed Maps for the Blind; and specimens of Printed Music for their use, upon a new principle of Notation, applicable to Music in general; with Observations. By Mr James Gall jun. 24 Niddry Street, Edinburgh.

5. Observations on the Dublin and Kingston Railway. By David Stevenson, Esq. 1 Baxter's Place, resident engineer for Granton Harbour.

6. Report by Mr Patterson of the Fire-engine Establishment on Mr Adam Hope's new Nose-pipe for Fire-engines, was read and approved of.

7. The committee on Mr Macpherson's bath-heater gave in their report, which was read and approved of.

8. The Report of the Prize Committee, containing the List of Prizes offered by the Society for Session 1836–7, was read, and printed lists of the prizes distributed.

9. The Report of the Prize Committee awarding the Prizes for Session 1834–5 was read, and the prizes were delivered to the successful candidates by the Vice-President, with appropriate addresses, in the order of that Report, which is as follows:—

(1.) To Mr D. Landale mining-engineer, Wemyss, Fifeshire—for his Method of carrying Low-pressure Steam to the distance of 193 yards, to work a steam-engine for draining a coal-mine; read 22d April 1835. The Society's Gold Medal, value Fifteen Sovereigns.

(2.) To Mr William Meikle, Townend, Strathaven, Lanarkshire—for his New Reed Instrument, called the "Caledonica," and for his improved Oboe and Bassoon; exhibited and described 28th January 1835.—The prize "for the best set of experiments on any branch of Practical Mechanics," being the Society's Gold Medal, value Ten Sovereigns.

(3.) To Mr Robert Muir (of Muir, Gowans, and Co. printers), 42 Argyle Street, Glasgow—for his communication "On the best Composition for Rollers employed in applying the Ink to the Types in Letterpress Printing, especially with a view to preserve their adhesive and elastic properties in as uniform a condition as possible during damp and other variable states of the atmosphere;" read, and specimen roller exhibited for trial, 11th March 1835.—The Society's Silver Medal, value Ten Sovereigns.

(4.) To Mr John Weir, venetian-blind maker, 35 Gordon Street Glasgow—for his new and improved method of Raising and Lowering the Venetian-Blind; and for his improved Pincers for drawing Nails; exhibited and described 25th March 1835.—The Society's Silver Medal, value Five Sovereigns.

(5.) To Mr David Kemp, smith, Fox's Lane, Leith—for the ingenuity displayed by him in his Latch-Lock of a new construction —and his Chest or Desk-lock; exhibited 10th December 1834.—The Society's Silver Medal, value Five Sovereigns.

(6.) To J. Stewart Hepburn, Esq. of Colquhalzie, Crieff—for his essay on the causes of Obstructions in Water Pipes and Syphons by disengaged air; and the construction of an Air Extractor for removing them; read, and model and drawings exhibited 5th February 1834.—The Society's First Honorary Medal.

(7.) To David Stevenson, Esq. 1 Baxter's Place, Edinburgh, resident engineer for Granton Harbour—for his Observations on the Manchester and Liverpool Railway; read, and illustrative drawings exhibited, 25th February 1835.—The Society's Second Honorary Medal.

(8.) To George Martin, Esq. 108 George Street, Edinburgh—for his Remarks on the Glasgow and Garnkirk Railway; with illustrative sketches; read, and exhibited 22d April 1834.—The Society's Third Honorary Medal.

(9.) To Mr James Dowie, boot and shoemaker in ordinary to the King, 57 Frederick Street, Edinburgh, M.S.A.—for his notice of an improved form of Boots and Shoes adapted to the comfort of the wearer; read, and specimens exhibited, 20th May 1835.—The Society's Fourth Honorary Medal.

The Committee reported that there had been no competition for the following prizes:—

1st Prize (Keith Medal) of	. .	20	Sovereigns.
3d — of	. . . . .	8	—
4th — Lithographic Prizes	of	12	
5th — Lithographic Prizes	of	8	—
6th — of	. . . . . .	10	—

March 30.—Edward Sang, Esq. Vice-President in the chair. The following communications were laid before the Society:—

(1.) Description and Drawing of a Wind-Safe for Grain and Hay Stacks. By Mr Thomas Johnstone, 137 George Street, Glasgow.

(2.) Description of a Plan by which the Blind may be taught to write a common Current Hand, as small and as elegant in its forms as that generally used by those who see. By Mr James Gall jun. printer, Edinburgh, M.S.A.

(3.) Description of a New System of Arithmetical and Geometrical Notation for the Blind, in which no Apparatus is required further than Common Pins and a Cushion (or any thing into which the pins may be thrust, such as a soft Chair, or a bed), in order to perform the longest and most difficult calculations in Arithmetic and Algebra,—and in Geometrical Problems the same are necessary, with the addition of a pair of Wooden Compasses, one leg of which is a straight edge, and the other a scale for measurement. By the same.

(4.) Remarks on Pointing Naval Ordnance; and Suggestion of a new method of ascertaining the precise moment when a Gun arrives at horizontal position. By T. B. College, Post-office, Edinburgh.

(5.) Donation.—On the manner in which Friction affects the motions of Time-keepers. By Edward Sang, Esq. teacher of mathematics, Edinburgh, M.S.A. &c., July 1835. From the Author.

(6.) The Committee on an Alphabet and mode of Printing for the use of the Blind, reported progress.

(7.) The Committee on Mr Macgregor's Escapement reported.

The following Candidates were admitted as ordinary members, viz :—

1. Mr James Mackay, Goldsmith, 24. Forth Street; 2. J. Stewart Hepburn, Esq. of Colquhalzie, by Crieff; 3. Mr Alexander Stewart, painter, 88 Prince's Street.

April 13.—Mungo Ponton, Esq. Councillor, in the Chair. The following communications were laid before the Society :—

(1.) On a Systematic Method of Measuring Surface and Solidity. By Mr John Sang, land-surveyor, Kircaldy.

(2.) Description and Drawing of a Simple plan for enabling Blind persons to communicate in Writing with their seeing Friends. By Mr John St Clair, teacher of music, Monteith's Close, 61 High Street, Edinburgh. The Writing-Board was exhibited.

(3.) On the Use of Steam in the Economizing of Fuel. By Andrew Fyfe, Esq. M.D., M.S.A.

(4.) Notice of a New Pocket Box Circle, for making Observations at Sea or on Land. By William Galbraith, Esq. teacher of mathematics, Edinburgh, M.S.A.—The Instrument was exhibited.

(5.) Notice of an Improvement in the Construction of Wollaston's Goniometer. By Edward Sang, Esq. teacher of mathematics, Edinburgh, Vice-Pres. Soc. Arts.—The Apparatus was exhibited.

The following candidates were admitted as Ordinary Members, viz.

1. Cunninghame Borthwick, Esq. 27 Albany Street; 2. James Graham, Esq. of Leitchtown, 2 Rutland Square; 3. John Thomson, Esq. professor of music, 32 Royal Circus.

April 27.—Edward Sang, Esq. Vice-Pres., in the Chair. The following communications were laid before the Society :—

(1.) Drawing and Description of a Machine for cutting Mortises in Joinerwork, &c. By Mr J. Kirkwood junior, wright, Glasgow. A specimen of the work was exhibited.

(2.) Description and Drawing of a New Method of raising and lowering a Slide-Rest, by means of Inclined Planes moved with Screws. By Mr James Bell, philosophical instrument maker, 54 South Bridge Street, Edinburgh. Communicated by Mr Alexander Bryson, M.S.A.—The Tool was exhibited.

(3.) Six Specimens of Lithographic Printing, from Transfer Drawings, in competition for the 2d Prize for 1835–6.—As also, Three Specimens of Lithography executed entirely with the Pen, but too small for competition for the above Prize. By Messrs Maclure and Macdonald, lithographers, 160 Trongate, Glasgow.

(4.) Twelve Specimens of Lithographic Printing, chiefly from Transfer Drawings. By Mr Samuel Leith, lithographer, late of Banff, now of Leith and Smith, lithographers, Hanover Street, Edinburgh.

(5.) On a Fire-Engine for Ships. By T. Borthwick, Esq. 1 Broughton Place, Edinburgh.

(6.) On the Construction of a High Pressure Steam-wheel. By the same.

(7.) A new System of Short-hand Writing. By Mr J. Kerr, teacher of stenography, and land-surveyor, 63 South Bridge, Edinburgh.—Specimens in illustration, and for comparison with other systems now in use, were exhibited.

Mr John Sang, land-surveyor, Kirkcaldy, was admitted as an Ordinary Member.

The President then took occasion to express his own regret, and that of the Society generally, that no competitors had appeared for the other and particularly the lithographic prizes offered last session. He stated, that the Society have no wish to store up the funds committed to their charge, but that it is their anxious desire to afford every encouragement to competitors; in proof of which he alluded to the greater liberality of the offer for next session. The adjudgment of the prizes he also stated to be free of every objection. They are awarded, in fact, by a committee appointed for that especial purpose after all the communications on which they are to decide have been submitted to the Society; and the appointment of that committee is by the Society at large, and not merely by the Council; so that candidates may rely on an impartial consideration of the merits of their inventions.

The Committee proposed that, while thanks were due to all those gentlemen who had sent them communications, the special thanks of the Society should be given to the following gentlemen for their valuable communications, viz.—

1. To Mr Professor Forbes, Edinburgh, F.R. SS. L. & E. Vice-President Soc. Arts—for his communication on the application of

the Compressibility of Water to practical purposes; read 22d April 1835.

2. To the Rev. Edward Craig, A.M. Oxon. Counsellor Soc. Arts—for his notice of arrangements for Measuring the Angles of Crystals viewed under a high magnifying power; read and exhibited 20th May 1835.

3. To Mr John Dunn, optician, 50 Hanover Street, Edinburgh, Curator Soc. Arts—for his new Klinometer, which serves as a portable surveying instrument, or Theodolite; exhibited 22d April 1835.

4. To Mr James Edgar jun. 6 Newington Place, Edinburgh—for his model and description of a method of constructing Wooden and other Bridges; read and exhibited 11th February 1835.

5. To Mr John Adie, optician, 58 Prince's Street, Edinburgh—for his notice of a new method of Cutting, Drilling, and Working Glass, Porcelain, &c., by means of Turpentine; communicated to him in London.

The following gentlemen were admitted as ordinary members, viz—

1. Mr James Gall jun., printer, 24 Niddry Street; 2. Mr William Galbraith, teacher of mathematics, 54 South Bridge; 3. Mr Walter Sibbald, ironmonger, 8 Meadow Place; 4. John R. Skinner, Esq., W.S., 5 Roxburgh Place.

May 25.—Mr Professor Forbes, Vice-Pres. in the chair. The following communications were laid before the Society:—

1. A new Anemometer, by which the most minute changes in the force or velocity of the wind, or current of air, may be measured. Invented and constructed by Mr R. Adie, optician, Liverpool. Communicated and described by Mr John Adie, optician, Edinburgh.

2. On the Construction of a Fluid Engine, acting by Atmospheric Pressure. By Mr T. Borthwick, 1 Broughton Place, Edinburgh.

3. On the Effect of Atmospheric Pressure on the Steam-Engine, as usually constructed. By the same.

4. On condensing by contact. By the same.

5. On Substitutes for Paddle-Wheels in Steam-Boats for Canal Navigation. By the same.

The following Reports of Committees were read and approved of, viz.—

6. On Smith's Instrument for Cutting Coats.

7. On Gall's Maps for the Blind.

8. On Gall's New Notation for Music.

9. On Gall's Plan of Teaching the Blind to Write.

10. On Gall's New System of Arithmetical and Geometrical Notation for the Blind.

11. On St Clair's Writing Board for the Blind.

12. On Dr Fyffe's Use of Steam in economizing Fuel.

13. On Mr Galbraith's Pocket Box Circle.

14. On Mr Sang's Improvement in the construction of Wollaston's Goniometer.

15. On Kirkwood's Morticing Machine.

16. On Bell's Method of raising and lowering a Slide-Rest.

17. On Whitelaw's additions to the Turning Lathe in slow turning.

18. Professor Forbes pointed out to the Society the probably extreme practical importance of a most extraordinary fact in optics communicated lately to the Institute by M. Cauchy. He was led to anticipate by theory, and verified it experimentally, that near the limit of total reflection in a prism refraction takes place, with a *vast increase* of intensity of the incident ray of light. If this be confirmed (and the proof is easy, though Mr F. has not had leisure to put it to the test), the most astonishing results, the Vice-President pointed out, would ensue; since light, however trifling, may be magnified indefinitely.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Shower of Falling Stars in Russia, on the night between the* 12*th and* 13*th November* 1832.—The following extract of a letter from Monsieur le Comte de Suchteln to Monsieur Feodorou, was communicated to the "Royal Academy of Sciences" at Paris, in which mention is made of numerous meteors which were seen in the neighbourhood of Orenburg, in the night between the 12th and 13th November 1832. "In the night between the 12th and 13th November 1832, between three and four o'clock in the morning, the weather being calm and serene, and the thermometer being at 55° of Fahr., the heavens appeared to be bespangled by a great number of meteors, which described a great arch in the direction of from north-east to south-west. They burst like rockets, into innumerable small stars, without producing the slightest noise, and left in the sky, what was long of disappearing, a luminous band, having all the various colours of the rainbow. The light of the moon, which was then in her last quarter, obscured this appearance. It sometimes seemed as if the heavens were cleft asunder, and in the opening, there appeared long brilliant bands of a white colour. At other times flashes of lightning rapidly traversed the vault of heaven, eclipsing the light of the stars, and causing these long luminous bands of varied colours to appear. These phenomena continued to succeed each other without occasioning the slightest percep-

tible noise. They were in their greatest splendour between five and six o'clock in the morning, and continued without interruption till sunrise. They were observed principally by the sentinels and by the officers, when going their rounds; also by the ecclesiastics, and by the subordinates, in the cathedral, and by many other persons. Monsieur Milordou, the principal priest of the cathedral, stated, in the account which he gave of this occurrence, that the interior of the cathedral was sometimes suddenly illuminated by the light of this brilliant phenomenon. Monsieur Itschitow, Lieutenant-Colonel of the 3d Battalion of the Line of Orenburg, also confirmed these statements in his Report, which, as an additional ground of confidence, contained the accounts of the sentinels in the several positions in which they had been posted. During the same night, and almost at the same hour, a not less remarkable appearance was witnessed at Hitzkaja-Saschtschita, about seventy-five miles to the south of Orenburg. Two columns of a white colour rose from the horizon equidistant from the moon, which at the time had not risen far; about the middle of their height they appeared brilliant and much curved. Several horizontal bands sprung from this point, the most brilliant of which extended towards the moon, in which they appeared to unite, so that in this way they appeared to form a great H. In the town of Ufa, the seat of the government of the same name, situated 380 miles to the north of Orenburg, a phenomenon similar to that which was observed at Hitzkaja-Saschtschita, was perceived, but, according to the accounts which have been given, it was not quite so brilliant."

GEOLOGY.

2. *Disengagement of Inflammable Gas in the Interior of Coal Mines.*—Monsieur Combes has presented a notice to the Royal Academy of Sciences at Paris, which may serve as a sequel to the remarks of Mr Buddle, an English engineer, upon the evolution of hydrogen gas in coal-mines. It seems quite certain, as stated in this notice, that the evolution of carburetted hydrogen in coal-mines, has frequently a relation to the pressure exercised externally at the surface, so that there is no disengagement where the external pressure is considerable, and

it becomes more and more abundant in proportion as this pressure is diminished. Besides, the pressure under which the gas commences to be evolved from the pores of the coal, varies in different mines. It is sometimes scarcely superior to the common pressure of the atmosphere, as without doubt is the case in many of the mines in Northumberland, in which, according to Mr Buddle, the atmosphere becomes explosive when the barometer is low, whilst scarcely a trace of inflammable air is to be found when the barometer is very high. At other times, it exceeds considerably the pressure of the atmosphere. Monsieur Combes states the following as a proof of this. We give the statement in his own words. "In the year 1830, I caused the shaft of a coal-pit at Latour, near Firmini, Department of the Loire, to be emptied of the water it contained. This pit had for many years been abandoned, on account of the immense quantities of inflammable air which had been generated in the galleries, which had occasioned so many deplorable accidents, that the working could not be continued, on account of the imminent risk. The pit was 230 feet deep at the place where it reached the roof of the galleries in the coal, and it was filled with water to within 65 feet of the surface. This free portion of the pit contained only atmospheric air, without a single trace of carburetted hydrogen. When the water was pumped out to the depth of 193 feet from the surface, the roof of the galleries being still covered with water to the extent of 37 feet, the gas began to be disengaged through the column of water still in the pit, with a noise such as a copious spring would have made by falling from the upper part of the shaft. After this event, the air in the pit continued to be in the highest degree explosive. One day two workmen incautiously descended into the pit to discover the spring, which they supposed issued from the upper part of the shaft; they took a common lamp along with them, and when they had descended about 45 feet, their lamp set fire to the gas; when, fortunately enough, only the upper layer exploded. One of the workmen was severely scorched. When they had ascended to the surface, and a wisp of burning straw was thrown into the pit, a very great explosion was the immediate result. Thus, in this mine, the inflammable gas was evolved under a pressure at least equal to two atmospheres, and probably much more; the shaft was in fact opened upon the most elevated portion of the workings, and all the galleries communicating with it had a rapid descent, following the inclination of the bed, which was at an angle of 18° or 20°. The escape of the gas through this depth of water continued without interruption, with the same intensity, during several months. I may add, that, after I had caused a horizontal floor of planks of fir to be constructed, and covered over with about six feet of stiff clay, well pressed down, and sunk to the bottom of the shaft, the gas escaped much more sparingly across the fissures of schistous rock, but still in very considerable quantities. In those beds which retain the hydrogen only under such great pressure, it is manifest that the quantity will vary to a very trifling extent, with the variations of the barometer. Nevertheless, in certain circumstances, the air in the mine is more charged with gas, as during the time of a storm, when the barometer is low, than in calm and fine weather, with a dense atmosphere." In the work to which this notice is, as we have said, a kind of supplement, Mr Buddle, when treating of the explosion which took place on the 3d of August 1830, in the coal-mine of Jarrow, points out two causes which are the cause of explosions in the coal-mines in the north of England; 1*st*, The numerous fissures and rents in the encasing rock, which thus form cavities filled with gas, whence it issues in greater or less quantities, according as the pressure of the atmosphere is more or less; 2*dly*, Blind cavities in the coal-seam itself, or in the surrounding rock, whence the gas suddenly escapes, when the galleries reach and open them up. M. Combes confirms the existence of this second cause of disengagement, from the occurrence of accidents which have happened in French mines; and, among others, by an explosion which happened on the 10th of April 1824, in the coal-mine of Ronchamp, in Haute-Saone, when twenty miners were killed, and sixteen most severely injured. According to the reports of the engineers of the mine, inflammable gas had previously very rarely, and in very small quantities, manifested itself in this mine. A trifling disengagement had taken place in a try-work which was begun at the bottom of the pit of St Louis, close to a fault. In a report which M. Thirria gave in to the Director-General of bridges, roads, and mines, he remarks, "It was imagined that the gas might be got rid of by means of a ventilator, which should propel it into a gallery of transport, in which the current of air was so strong, that it sometimes blew out the lamps of the workmen. The engineers supposed that the gas, instead of having been forced out of the mine by the current of air, was forced only into some old abandoned workings which were situated at the extremity of the gallery of transport, behind pillars and walls, and some rubbish; and that it was there ignited, owing to the falling down of a part of the roof of this cavity, whereby a great quantity of the gas was suddenly expelled, or possibly owing to some of the workmen having taken a lamp into it. However this may be, the gas has begun to shew itself in the works, in the neighbourhood of the fault of the pits of St Louis; and this circumstance recurring frequently in the mines, it becomes important to recommend it to the peculiar attention of miners, and to point out to them the means of its prevention."

3. *Analysis of a Clay Ironstone, forming a bed twelve inches thick, in the Coal Formation at Wardie, to the westward of Newhaven, near Edinburgh; by William Gregory, Esq. M. D.*—No. 1. from a depth of twenty fathoms and five feet, contains in the calcined state, as given to me in 100 parts, Matter insoluble in acid (sand), 19 6; Peroxide of iron, 72 5; Alumina (clay), 3 5; Lime (a trace), 0 0; Moisture and loss, 4 4; = 100.—No. 2. from a depth of twenty-six fathoms and four feet, contains in a calcined state in 100 parts, Insoluble matter, 37 8; Peroxide of iron, 56 4; Alumina, 2 5; Lime (a trace), 0 0; Moisture and loss, 3 3; = 100.—No. 1. when calcined contains, therefore, about 50 per cent. of pure iron, calculated in the metallic state, and No. 2, 40 per cent. nearly.—No. 3, in its natural state contains in 100 parts, Insoluble matter, 19 3; Protoxide of iron, 45 9; Alumina, 1 5; Water and loss (a trace of lime) carbon, &c. 33 3 = 100.—The metallic iron here is 32 2 per cent.; the reason of the difference is, that by the calcination a quantity of water was expelled, so that in numbers *one* and *two*, the quantity of iron is increased in proportion to the weight of the mineral analyzed. All the ores are remarkably good, and there can be no doubt that, with the addition of lime and other necessary fluxes, they will work admirably. I have scarcely seen any ores of the coalfield containing so much as 45 per cent. protoxide;

and it is probable that Nos. 1 and 2 contain a good deal more than this. All, I have no doubt, in the natural state contain, as No. 3 does, some carbonaceous matter, but the quantity of this is not large.* Dr Gregory adds, that the quality of the ironstone of Wardie is not surpassed and scarcely equalled in any iron-work in Scotland.—*Communicated by Captain Boswall of Wardie.*

4. *Volcanos of Kamtschatka.*—In the second volume of Erman's travels, which is soon to appear, the following heights of volcanos are given: The summit of the volcano of Kliutschewsk, 4804 metres above the level of the sea; the summit of the volcano of Tolbatschinsk, 2534 metres; the summit of the volcano of Schiveloutsch, 3214 metres. Kliutschewsk is the highest point in the Peninsula of Kamtschatka. The limit of perpetual snow is 1618 metres.

5. *Temperature of the Mines at the Leadhills and of some Springs on the Rhine.*—On occasion of a late visit to the district of Leadhills (says Professor Forbes, in a note to the editor), I suggested to my friend and former pupil, Mr Irving of Newton, the importance of determining the temperature of the springs in the bottom of Leadhill mines at this particular epoch; the workings having been discontinued since the end of March, any supposed influence of animal heat and lights is avoided, and yet the pumping of the water has been regularly carried on. Mr Irving immediately and zealously undertook the inquiry, and descended to the deepest part of the mine on the 16th of May, and found the temperature of the water in the bottom to be 49°. This was at a depth of ninety-five fathoms below the Barrow Road or entrance to the Susanna vein. A spring at the upper level had a temperature of 44°. The temperature of the air at seventy-five fathoms, where there was free circulation, was 53°, and at about half that depth, also in a current, it was only 50°. The facts are therefore entirely in accordance with published observations on the increase of temperature with depth, and are, I presume, the first of the kind made with care in Scotland.

* The ironstone of Wardie is also interesting not only on account of the coprolites it contains, but also from its having afforded many new and rare fossil fishes, discovered there by Lord Greenock, of which descriptions and figures have been published in Agassiz's great work on fossil fishes.

Mr Irving has promised to pursue these experiments, and I have procured for him some of the standard instruments especially designed for this purpose furnished by the British Association.—In relation to an interesting class of facts noticed in M. Bischoff's paper on the temperature of springs in the last number of your Journal, namely, that many springs have a mean temperature a few degrees above that of the air at the place, I beg to add my testimony from observations made in the volcanic country of the Rhine. I am scarcely yet prepared from the testimony of my own observations to consider the fact to be so general as the German naturalist supposes. The following extract from my journal will shew that in the particular cases in question I had arrived at the same conclusion, but that I was disposed to attribute the effect to a local cause:—" *3d August* 1832.—Near the lower quarry [of Bell near Obermennig], and in the bottom of the valley, rises a very fine mineral spring, very similar in every respect to that of Tonistein [near Laach]. It contains more iron, and the quantity of carbonic acid [gas] evolved is perfectly enormous. Both springs have their origin in nearly similar circumstances, namely, at a junction of tufa with clayslate. Their temperature, it is remarkable, is almost the same, that of Tonistein being 55.°5, that of the present one (which is near Obermennig, and is called the Kesselbron, according to Hibbert) 54.°2. Its height must be considerably above the other. Rising as these springs do from valleys of common clayslate merely filled up with accidental eruptions [of trass or volcanic mud], may we not conclude that the progress of secular refrigeration has not yet reduced the temperatures of the lower strata of the difficultly conducting mass of tufa to the mean temperature of the place, which these *extremely copious* springs certainly exceed by several degrees."

6. *Progressive Rise of a portion of the bottom of the Mediterranean.*—M. Theodore Virlet lately addressed a note to the French Academy of Sciences, in which he directed the attention of geologists to the probability of the speedy appearance of a new island in the Grecian Archipelago, in consequence of the progressive rise of a sunken solid rock (composed of trachytic obsidian ?) in the gulf of the volcano of Santorin. The following are the author's observations on this subject:—" Towards

the end of the last century, at the period when Olivier visited Santorin, the fishermen of the island asserted that the bottom of the sea had recently risen considerably between the island of Little Kaïméni and the Port of Thera; in fact the soundings did not give a greater depth than fifteen to twenty fathoms, where formerly the bottom could not be reached. When Colonel Bory and the author visited the island in 1829, they were able not only to confirm the truth of Olivier's statement, but also to ascertain, by various soundings, that the rise of the submarine land had continued, and that at the point indicated the depth was not more than four fathoms and a-half. In 1830 the same observers made new soundings, which enabled them to determine the form and extent of the mass of rock, which in less than a year had been elevated half a fathom. It was found to extend 800 metres from east to west, and 500 from north to south. The submarine surface augmented gradually to the north and west, from four to twenty-nine fathoms, while to the east and south this augmentation amounted to forty-five fathoms. Beyond this limit the soundings indicated in all directions a very great depth. I have lately been informed that Admiral Lalande, who, since 1830, has twice returned to Santorin, ascertained that the rock still continues to rise; and that, in September 1835, the date of his last visit, the depth of water amounted to only two fathoms, so that a sunken reef now exists which it is dangerous for brigs to approach. If the rock continues to rise at the same rate, it may be calculated that, in 1840, it will form a new island, without, however, those catastrophes which this phenomenon seems to presage for the gulf of Santorin, being a necessary consequence of the epoch of its appearance at the surface of the water. Since the eruptions of 1707 and 1712, which produced the new Kaïméni, volcanic phenomena have completely ceased in the gulf of Santorin, and the volcano seems at the present day quite extinct. Nevertheless, the rise of a portion of its surface seems to demonstrate continual efforts to make an eruption during fifty years; and that, whenever the resistance shall not be strong enough to offer a sufficient obstacle, the volcano will again resume its activity."

7. *Remains of Quadrupeds in the Oolitic System of Rocks.*—Hitherto the only fossil remains of the mammalia known to na-

turalists, older than the chalk formation, were those of a species of Opossum, or Didelphis, found in the oolitic rocks of Stonesfield. Some years ago, Hugi, professor at Soleure, discovered, in quarries in that part of the oolitic system named Portland Stone, remains of true quadrupeds. Very lately, these neglected observations of Hugi have, we are told, been confirmed and strengthened by additional discoveries in the same quarter, made by a Mr Gressy. He, in a communication to the Natural History Society of Strasbourg, enumerates the following animal relics as having been found inclosed in undoubted beds of Portland stone, along with remains of crocodiles, emys or fresh-water turtles, &c., viz. bones of Paleotherium, the Anoplotherium gracile, of a kind of hedgehog, and a pachydermatous or ruminating animal, the size of a sheep.

8. *Temperature of the different Tertiary Deposits at the Epoch of their Formation.*—M. Deshayes has communicated to the French Academy of Sciences a notice on the determination of the temperature of the tertiary periods of Europe from the knowledge of the fossil shells which these formations contain. The author commences by detailing rapidly some facts relative to the distribution of molluscous animals in proceeding from the north to the south, and principally from the North Cape to the Gulf of Guinea. "If" (says he) "we take as a whole the small number of species living in the north, we can divide them into two perfectly distinct categories: the one series peculiar to the cold seas and never passing their limits; and the other, including a smaller number, living also in the temperate seas of Germany, France, and England, with the species belonging to these seas. In examining the mollusca of our temperate seas, in which there exists a larger number of species than in the seas of the north, it is easy to divide them into three series. In the first are included the species I have just indicated, those which extend to the seas of the north; the species of the second series extend to the southern seas; and those of the third are peculiar to temperate seas. In the intertropical region similar phenomena present themselves. There we find a greater number of species than in the two preceding regions; and if some among them occur also in the temperate region, a large proportion are peculiar to the equatorial regions. These, then, are the general

facts, and we may already draw from them the general consequence that each group of species represents the mean temperature of each of these regions. But there are certain species of more local and others of more general occurrence. Thus, to give an example of the latter, the *Buccinum undatum* is found from the North Cape to the Senegal, modified according to the temperature; and it is easy to distinguish the varieties peculiar to the three principal conditions of temperature. Other species, more sensible to the influences of temperature, are much more local, and they are precisely those which it is the most important to know. The following are some of them. The *Buccinum glaciale* and *Cardium Groenlandicum* do not extend beyond the polar circle, and are found in Norway and Greenland. The *Terebratula psittacea* lives between the fifty-fifth and seventy-fifth degree. In my opinion these species and several others represent the mean temperature of the north of Norway. The *Tellina Baltica*, *Patella noachina*, *Natica clausa*, *Patella testudinalis*, several species of the genus *Astarte*, and some other species, seem to me to represent the mean temperature of the north of England, the south of Sweden, and Denmark. In the English Channel, on the coasts of France and England, there exist several species peculiar to our temperature, such as the *Psammobia vespertina*, *Pecten irregularis*, &c. The coasts of Spain and Portugal are less known than those of New Holland and North America. Among the large number of species known in the intertropical zone, there are a great many which are peculiar to it, and which, accustomed to a high and little varying climate, do not occur living in any other part of the surface of the globe; they express then with fidelity the temperature of the seas in which they live. These facts relative to the coincidence of the temperature with the presence of certain species, necessarily preceded the remarks which I have to make regarding the temperature of the geological epochs of tertiary deposits. I ought to add that, in order to determine this interesting question, it was necessary to compare carefully all the known living species with all those found in the various tertiary formations of Europe. The following are the principal results obtained by the aid of this long investigation:—1. The tertiary formations of Europe do not contain a single species in common with the subjacent secondary formations; 2. The tertiary formations are the only ones containing fossil remains of species which live at the present day; 3. The analogous species are more numerous in proportion as the formation is more modern, and reciprocally; 4. Constant proportions (3 in 100, 19 in 100, 52 in 100), in the number of analogous species, determine the age of the tertiary formations; 5. The tertiary formations are in superposition, and not in parallelism, as was at first supposed; 6. The tertiary formations ought, according to their zoological characters, to be divided into three groups. We shall now give the conclusions relative to the temperature of the three series of tertiary strata at the epoch of their formation. The most superficial tertiary formations were deposited when the temperature of Europe was nearly similar to what it is at present. The proofs are the following:—The tertiary formations of this age in Norway, Sweden, Denmark, St Hospice near Nice, and of a part of Sicily, contain, in a fossil state, all the identical species of the corresponding seas. These same formations of the Mediterranean side of France, of Spain and Piedmont, of Italy, of Sicily, of the Morea, and of Barbary (Algiers) contain a great portion of the species which live in the Mediterranean, but contain also some whose analogues no longer exist, or are distributed in small quantity in the hot regions of the Atlantic Ocean and in the Indian Seas. These observations have induced me to believe that the Mediterranean has experienced a slight depression of temperature, since the chain of the Atlas on the one side, and that of the Apennines on the other, assumed their present *relief.* During the second tertiary epoch, to which belong a great number of small basins, scattered especially near the centre of Europe, the temperature has been very different from that which at present exists in these places. In fact, the species peculiar to the Senegal and the sea of Guinea, those which best represent the temperature of that part of the equatorial zone, are found in the fossil state in the beds of this second period. The temperature of the third period, at first a little more elevated than our own in the Mediterranean basin, has become similar to that which we experience; in the north the species of the north are fossil; in the south the species of the south. Thus, since the commencement of the tertiary formations, the temperature has been constantly diminishing, passing in our climates from the equatorial to that of our own day; it is easy to determine the difference. Natural philosophers, by resting on their beautiful theories of heat, have doubtless been able to suppose *a priori* the changes of temperature of which I have spoken; it is, nevertheless, curious to perceive their conjectures confirmed by a science for a long time neglected, and which no one thought of directing to this new object."

HYDROGRAPHY.

9. *The Level of the Caspian much below that of the Ocean.*—In 1814, Messrs Engelhardt and Parrot attempted to determine, by means of the barometer, if, as was long ago supposed, the waters of the Caspian Sea are less elevated than those of the Mediterranean and the ocean. The mean of three determinations gave a difference in this respect of 98 metres. But subsequently M. Parrot having thrown some doubt on the result of these observations, made in 1814, M. Erman has taken up the subject, and the following is the result of his investigations: Barometrical observations made for seven years at Kasan, compared with corresponding observations made during the same period at Dantzig, give 31.8 metres as the height of the former town above the level of the Baltic. This result is confirmed by six years' observations at Mitau. Hence, with the assistance of levelling, M. Erman concludes that the height of the junction of the Kasanka with the Volga, is only 8.8 metres above the Baltic. Thus, in order that there should be a coincidence between the levels of the Caspian and the Baltic, it would be necessary that, in the extent of 205 German miles between Kasan and Astracan, the descent of the river should not be more than 8.8 metres, which seems inadmissible. The descent of the Volga from Torjok to Kasan, in an extent of 155 miles, has been measured. Supposing that, in the remainder of its course, the river follows the same law, M. Erman has ascertained that the depression of the Caspian Sea, compared with the Baltic, would be eighty-four metres—a result which does not differ much from that (ninety-eight metres) obtained by Messrs Engelhardt and Parrot.

10. *Spring at the Summit of a Mountain.*—M. Durieu, who lately made a scientific journey in the kingdom of the Asturias, mentions the following important fact in physical geography:—

' A beautiful spring flows from the highest point of the peak of the *Sarrantina*; but, as this peak is not commanded by any neighbouring summit, we must necessarily suppose that the other branch of the syphon must be placed at a great distance to the east, to receive, on the flanks of the mountains covered by perpetual snow, and having a much greater elevation, the water which issues from the extremity of the shorter branch, and which an extraordinary accident or a concealed natural cause, has forced to ascend to the top of a pointed peak."

BOTANY.

11. *Fossil Ferns.*—The following general conclusions regarding the geological and geographical distribution of fossil ferns, are contained in a recent memoir by Professor Göppert. The beds of the coal formation contain the largest number of fossil ferns, viz. 182, while the muschelkalk, and the chalk and tertiary formations, contain the smallest number. The total number of these fossil vegetables at present known amounts to 253, of which ninety-two have been found in Silesia, twenty-nine in Bohemia, fifty-six in the other countries of Germany, forty-nine in France and Belgium, eighty-nine in Great Britain, three in Denmark and Sweden, one in Italy, eleven in North America, one in Holland, and four in the East Indies. The ferns that are the most widely distributed on the globe are the following:—*Alethopteris Serlii* (in England, France, Silesia, Pennsylvania), *Neuropteris angustifolia* and *N. abutifolia* (in England, Bohemia, Silesia, Pennsylvania), and *N. Lohsii* (in England, France, Belgium, in the districts of the middle Rhine, in Bohemia, and Silesia). Most of the ferns of the Jura formation occur in England. The number of fossil ferns amounts nearly to a third of the total number (800) of fossil vegetables at present known. But it is very probable that we are acquainted with but a small portion of these fossils. Several genera of ferns belong exclusively to one or to two formations. Thus, the following occur only in the coal formation:—*Gleichenites*, *Balantites*, *Beinertia*, *Bockschia*, *Danæites*, *Diplacites*, *Glockeria*, *Glossopteris*, *Steffensia*, *Woodwardites*; and the same formation contains also the most of the species of the genera *Asplenites*, *Adiantites*, *Aspidites*, *Alethopteris*, *Cheilanthites*, *Cyatheites*, *Hemitelites*, *Neuropteris*, *Odontopteris*, *Trichomanites*, and *Hymenophyllites*

The two genera *Anomopteris* and *Scolopendrites* are peculiar to the *grès bigarré*. The genus *Asterocarpus* occurs in the coal formation, and also in the *Keuper*, *Pachypteris* only in the Jura series, and most of the species of *Acrostichites* and *Polypodites* in the same formation. Fossil ferns of all formations, without even excepting those of the chalk and the *Molasse*, present a striking resemblance to the tropical species of ferns, but none to those of temperate and cold climates. One of the principal conclusions to be drawn from the geological distribution of fossil ferns is, that each formation has particular species, which differ essentially from those of other formations. To this there are very few exceptions. Silesia is remarkable for its extremely rich fossil flora, for no less than 230 species have already been found in that country. The fossil flora of England resembles greatly that of Silesia. Excepting the genus *Stigmaria*, which is common to the transition and the coal formations, no species has been found in two formations. Finally, it is remarkable that dicotyledons and junci occur both in the most ancient and in the most modern deposits, a fact which tends to prove that there is little foundation for the opinion that at the earliest epochs cellular plants only existed, afterwards monocotyledons, and then at a later period dicotyledons.

ANTHROPOLOGY.

12. *Prospects of the Negro Population in South America, and of the gradual extinction of the original inhabitants of the New World.*—We behold (says the Foreign Quarterly) with a conviction which no arguments can weaken, with a vividness of perception which no efforts of our own can soften, the certainty of an impending and tremendous conflict between the white and the negro, the coloured and the Indian population, the fearful nature of which it is as easy to foresee as it is awful to contemplate. Such is also the opinion of Dr Poeppig, who, in his account of Chili, has the following observations:—" No country in America enjoys, to such a degree as Chili, the advantages which a state derives from an homogeneous population and the absence of castes. If this young republic rose more speedily than any of the others from the anarchy of the revolutionary struggle, and has attained a high degree of civilisation and order, with a rapidity of which there

is no other example in this continent, it is chiefly indebted for these advantages to the circumstance, that there are extremely few people of colour among its citizens. Those various transitions of one race into the other are here unknown, which strangers find it so difficult to distinguish, and which, in countries like Brazil, must lead, sooner or later, to a dreadful war of extermination, and in Peru and Columbia will defer to a period indefinitely remote the establishment of general civilisation.* * * If it is a great evil for a state to have two very different races of men for its citizens, the disorder becomes general, and the most dangerous collisions ensue, when, by an unavoidable mixture, races arise which belong to neither party, and in general inherit all the vices of their parents, but very rarely any of their virtues. If the population of Peru consisted of only Whites and Indians, the situation of the country would be less hopeless than it must now appear to every calm observer. Destined as they seem by Nature herself, to exist on the earth as a race, for a limited period only, the Indians, both in the north and the south of this vast Continent, in spite of all the measures which humanity dictates, are becoming extinct with equal rapidity, and in a few centuries will leave to the whites the undisputed possession of the country. With the Negroes the case is different ; they have found in America a country which is even more congenial to their nature than the land of their origin, so that their numbers are almost everywhere increasing in a manner calculated to excite the most serious alarm. In the same proportion as they multiply, and the white population is no longer recruited by frequent supplies from the Spanish peninsula, the people of colour likewise become more numerous. Hated by the dark mother, distrusted by the white father, they look on the former with contempt, on the latter with an aversion which circumstances only suppress, but which is insuperable, as it is founded on a high degree of innate pride. All measures suggested by experience and policy, if not to amalgamate the heterogeneous elements of the population, yet to order them so that they might subsist together without collision, and contribute in common to the preservation of the machine of the state, have proved fruitless. * * * The late revolutions have made no change in this respect. The hostility, the hatred, of the many coloured classes will continue a constant check to the advancement of the state, full of danger to the prosperity of the individual citizens, and perhaps the ground of the extinction of entire nations. The fate which must, sooner or later, befall the greater part of tropical America which is filled with negro slaves, which will deluge the fairest provinces of Brazil with blood, and convert them into a desert, where the civilised white men will never again be able to establish himself, may not indeed afflict Peru and Columbia to the same extent; but these countries will always suffer from the evils resulting from the presence of an alien race. If such a country as the United States feels itself checked and impeded by its proportionably less predominant black population; and if there, where the wisdom and power of the government are supported by public spirit, remedial measures are sought in vain; how much greater must be the evil in countries like Peru, where the supine character of the whites favours incessant revolutions, where the temporary rulers are not distinguished either for prudence or real patriotism, and the infinitely rude Negro possesses only brutal strength, which makes him doubly dangerous in such countries, where morality is at so low an ebb. He and his half descendant, the mulatto, joined the white Peruvian, to expel the Spaniards, but would soon turn against their former allies, were they not at present kept back by want of moral energy and education. But the Negro and the man of colour, far more energetic than the white Creole, will in time acquire knowledge, and a way of thinking that will place them on a level with the whites, who do not advance in the same proportion so as to maintain their superiority." When we consider all these circumstances, when we see Buenos Ayres even now harassed by perpetual wars with the Indians, when we think of the frightful crimes that have already taken place at Para, we cannot but anticipate the consequences that must ensue if the Negroes should rise in a general insurrection, and be joined by the native Indians. We wonder at the blind infatuation of the Brazilians, who, in defiance of their own laws, still import 100,000 new slaves every year from Africa, and we feel our minds depressed by the melancholy persuasion, that the future fate of these fine countries will prove even more tremendous than the awful denunciation which threatens to visit the sins of the fathers upon the children, even to the third and fourth generation.

13. *Historical and Statistical Researches on the Causes of the*

Plague. By M. DE SEGUR DUPEYRON.—M. de Segur Dupeyron had already attempted to prove that the plague comes to Europe chiefly from Egypt. This proposition not having been generally admitted, he has thought it necessary to develop further this part of his researches. In examining the correspondence of the consuls preserved in the archives of the Foreign Office, M. de Segur has found that two circumstances are indicated, which, in certain cases, may produce the plague in Egypt. These circumstances are, 1. Famine ; 2. Malignant Fevers. But famine in Egypt is generally caused by the too great or too small rise of the Nile. After a small inundation, little of the land having been irrigated, but a small portion can be sown ; and after a great overflow the waters require a long time to retire, and the seed-time passes before all the grain has been deposited in the earth. Hence M. de Segur has searched in Arabian authors for information regarding the height attained by the Nile, in the greatest possible number of overflowings, and he has inquired if, in the years corresponding to the too small and too great inundations, the plague has not existed somewhere. His investigations have been limited to the period comprised between the middle of the tenth century, and the middle of the fifteenth, because the works which he has been able to consult, notice only the inundations during that epoch. Of fifty to fifty-five plagues that have occurred in Europe during these five centuries, forty coincide with the too great or too small risings of the Nile. As the great work on Egypt contains a table of the heights of the river from 1737 to 1800, the author has been able to ascertain if in that number of years the plague occurred in the country after unfavourable inundations, and he has found that of fourteen plagues which took place during the period, thirteen coincide with the bad inundations which produce famine. After an examination of the correspondence of the consuls in Syria, and in the islands of the Archipelago, M. de Segur endeavours to prove, 1. That the plague prevailed in Syria and the Archipelago only after it had previously manifested itself in Egypt ; 2. That the famine in Syria and the Archipelago has been followed only by malignant fevers, and never by the plague, unless when it existed in Egypt ; and he concludes, that the famine in Syria and the islands of the Archipelago may be regarded as the origin of the same maladies it produces everywhere; but that in Egypt it gives rise to results which follow it in no other country, since it is almost always accompanied by the plague. In conclusion, M. de Segur remarks, that in Egypt there is a particular cause which increases the malignant fever, so as to give it all the characters of the plague, and this cause exists only in that country.

NEW PUBLICATIONS.

1. *The Physical and Intellectual Constitution of Man considered,* By Edward Meryon, F. R. C. S., &c. London: Smith, Elder. & Company. 1836. 8vo, pp. 240.

This agreeably written and interesting little volume we recommend to the particular notice of our readers.

2. *A Geological Sketch of the Tertiary Formation in the Provinces of Grenada and Murcia in Spain, &c.* By Brigadier Charles Silvertop. London: Longman & Rees, 8vo, pp. 236, with plates.

Silvertop is one of that active and enterprising corps of military officers who of late years have devoted their time to the advancement of geology. Like Murchison, their geological chief, Silvertop is not a mere museum or closet, or society geologist, but an indefatigable and successful labourer in the field, as is shewn by the interesting work now before us, which we doubt not will be prized by all true lovers of practical geology.

3. *Ascent to the Summit of Mont Blanc in* 1834. By Martin Barry, M. D. F. R. S. E., Member of the Wernerian Society, &c. Blackwood & Sons, Edinburgh, 8vo, pp. 119, with plates. 1836.

Of this successful ascent to the highest point of Europe some account has already appeared in this Journal; but the volume before us contains many valuable and interesting additional details, and also a luminous history of the observations of all preceding travellers who have reached or attempted to reach the summit of Mont Blanc.

4. *The Earth; its Physical Condition and most remarkable Phenomena.* By W. M. Higgins, F. G. S., Lecturer on Natural Philosophy at Guy's Hospital. London: Orr & Smith. 1836. 12mo, pp. 512.

This amusing and generally correct view of the physical history of our globe will take its place among the more esteemed of our popular works on this subject.

TO CORRESPONDENTS.

Several papers received will be noticed or inserted in next number of this Journal.

List of Patents granted in Scotland from 18th March to 16th June 1836.

1. To Francis Brewin of the Kent Road, in the county of Surrey, Esq. for "a certain new and improved process of tanning."—Sealed at Edinburgh 18th March 1836.

2. To James Morison of Paisley, North Britain, mauufacturer, for "improvements on the jacguard machine, and on what is called the ten box lay, and on the reading and stamping machines used in making shawls and other figured work."—18th March 1836.

3. To Luke Herbert of Paternoster Row, in the city of London, civil-engineer, for "certain improvements in mills or machines for grinding and sifting farinaceous and other substances."—23d March 1836.

4. To John Brunton of West Bromwich, in the county of Stafford, engineer, for "certain improvements in the construction of retorts for generating gas for the purpose of illumination."—25th March 1836.

5. To Miles Berry, of the office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer and mechanical draftsman, for "a certain improvement, or certain improvements, in the system, or mode, or method, of working engines for exerting mechanical power," communicated by a foreigner residing abroad.—6th April 1836.

6. To Joseph Chesseborough Dyer of Manchester, in the county of Lancaster, machine-maker, and James Smith of Deanstone, in the county of Perth, cotton-spinner, for "certain improvements in machinery used for winding upon spools, bobbins, or barrels, slivers or rovings of cotton wool, and other fibrous substances of the like nature."—7th April 1836.

7. To William Hale of Greenwich, in the county of Kent, late of Colchester, in the county of Essex, civil-engineer, for "certain improvements on machinery applicable to vessels propelled by steam or other power, which improvements, or parts thereof, are applicable to other useful purposes."—11th April 1836.

8. To John Birkby, late of High Town, but now of Upper Rawfolds both of Liversedge near Leeds, in the county of York, card-maker, for "improvements in machinery for making needles."—11th April 1836.

9. To Frederick Chaplin of Bishop Storford, in the county of Herts, tanner, for "an improvement in tanning hides and skins of certain descriptions."—11th April 1836.

10. To Charles De Bergue of Clapham Rise, in the county of Surrey, engineer, for "certain improvements in machinery used for spinning and doubling yarn or thread, manufactured from cotton or other fibrous material." —11th April 1836.

11. To Frederick Edward Harvey of the Horsely Iron-works, in the parish of Tipton, and county of Stafford, mechanical draftsman, and Jeremiah Brown of Tipton, in the same county, roll-turner, for "certain improvements in the process and machinery for manufacturing metallic tubes, and also in the process or machinery for forging and rolling metal for other purposes."—22d April 1836.

12. To William Maugham of Newport Street, Lambeth, in the county of Surrey, chemist, for "certain improvements in the production of chloride of lime, and certain other chemical substances."—25th April 1836.

13. To Thomas Ridgway Bridson of Great Bolton, in the county of Lancaster, bleacher, for "a certain improvement or improvements to facilitate and expedite the bleaching of cotton, linens, and other vegetable fibres." —25th April 1836.

14. To Joseph Lidel of Arundel Street, Panton Square, in the county of Middlesex, Professor of Music, for "certain improvements in piano fortes," communicated by a foreigner residing abroad.—28th April 1836.

15. To ANDREW SMITH of Princess Street, in the parish of St Martin in the Fields, and county of Middlesex, engineer, for "certain improvements in engines for exerting power for driving machinery, and for raising and lowering heavy bodies."—28th April 1836.

16. To JOHN BURN SMITH of Salford, in the county of Lancaster, cotton-spinner, and JOHN SMITH of Halifax, in the county of York, dyer, for "a certain method or methods of tentering, stretching, or keeping out cloth to its width, made either of cotton, silk, wool, or of any other fibrous substances, by machinery."—28th April 1836.

17. To ROBERT COPLAND of Brunswick Crescent, Camberwell, in the county of Surrey, Esq., for "improvements upon patents already obtained by him for combinations of apparatus for gaining power."—6th May 1836.

18. To WILLIAM PRESTON of Sunnyside, in the county of Lancaster, operative calico-printer, for "certain improvements in printing of calico and other fabrics."—10th May 1836.

19. To HENRY SHARPE of Broad Street Buildings, in the city of London, merchant, for "improvements in sawing wood and other materials," communicated by a foreigner residing abroad.—10th May 1836.

20. To JAMES CROPPER of the town and county of the town of Nottingham, lace manufacturer, and THOMAS BROWN MILNES, of Lenton Works, in the county of Nottingham, bleacher, for "certain improvements in machinery or apparatus for embroidering or ornamenting bobbin-nett, or lace or cloths, stuff, or other fabrics made from silk, cotton, wool, flax, or hemp," communicated by a foreigner residing abroad, and improvements made by themselves.—10th May 1836.

21. To JACOB PERKINS of Fleet Street, in the city of London, engineer, for "improvements in the apparatus and means for producing ice, and in cooling fluids."—13th May 1836.

22. To WILLIAM GOSSAGE of Stock Prior, in the county of Worcester, chemist, and EDWARD WHITE BENSON of Wichbold, in the same county, chemist, for "an improvement or improvements in the process of making or manufacturing ceruse or white lead."—20th May 1836.

23. To HENRY ADCOCK of Summer Hill Terrace, Birmingham, in the county of Warwick, civil-engineer, for "certain improvements in the loading and unloading of ships, brigs, schooners, and other vessels, at docks and quays, and in streams and rivers, and for the more facile transit and stowing of merchandize taken from ships, brigs, schooners, and other vessels at docks and quays, with a view to abridge human labour and economise expenses.—24th May 1836.

24. To JOHN WHITING of Rodney Buildings Row, Kent Road, in the county of Surrey, Doctor of Medicine, for "an improvement or improvements in preparing certain farinaceous foods."—24th May 1836.

25. To SAMUEL DRAPER of Basford, in the county of Nottingham, lace-maker, for "improvements in producing plain or ornamental weavings."—24th May 1836.

26. To GEORGE, MARQUIS OF TWEEDDALE, for "an improved method of making tiles for draining, soles, house-tiles, and flat roofing tiles."—25th May 1836.

27. To THOMAS GRAHAME of Nantes, in the Kingdom of France, but now of Suffolk Place, Pall Mall, in the county of Middlesex, gentleman, for "certain improvements in passing boats and other bodies from one level to another."—26th May 1836.

28. To JEREMIAH HORSFALL and JAMES KENYON, both of Addingham, in the county of York, cotton spinners, for "certain improvements in engines used for carding cotton, wool, and other fibrous substances."—13th June 1836.

29. To FRANCIS PETTIT SMITH of Hendon, in the county of Middlesex, farmer, "for an improved propeller for steam and other vessels."—15th June 1836.

30. To GEORGE HOLWORTHY PALMER, of the Canal Grove, Old Kent Road, civil-engineer, for "an improvement in the purification of inflammable gases, and an apparatus by which the improvement is applied, such apparatus being also applicable to other useful purposes."—16th June 1836.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

On Volcanos and Craters of Elevation. By Leopold Von Buch.

The tour which I made in the autumn of 1834, with Professor Link and M. Elie de Beaumont, and afterwards also with M. Dufrenoy, enabled us to determine more exactly some of those relations of elevation-craters, of which I formerly treated in the Academy, seventeen years ago, and also in my work on the Canary Islands.

Volcanos are the constant chimneys, the canals uniting the interior of the earth with the atmosphere, which spread around themselves the phenomena of eruption from craters that are of small extent, and are only once in operation. *Craters of elevation*, on the contrary, are the remains of a great display of power from within, which can and actually has raised islands of several square miles in extent, to a considerable height. They are conical and very extensive circular enclosures (umgebungen), with strata, which internally seem horizontal, but which on all sides dip to the exterior in a mantle-shaped manner. From such enclosures proceed no eruptive phenomena, there is no canal connecting them with the interior, and it is only rarely that we find in the vicinity or in the interior of such a crater traces of volcanic activity still in operation. This difference seems to me more an observation than a hypothesis. It is the separation of appearances whose causes cannot be ascertained, or even investigated without such a distinction.

That from the middle of such an elevation-crater, whose action was only for a short period of time, a new cone should arise, generally of trachyte, which becomes a permanent volcano, and spreads its eruptive phenomena over a wide circle around it, is strikingly and pre-eminently exemplified by the Peak of Teneriffe.

But, as in many other volcanos, melted substances which flow in the form of streams of lava, are raised to the edge of the volcanic crater; it has been supposed to be a fundamental truth, that a mountain of this description must have been produced by means of such a rising up, and the subsequent hardening of the flowing lava; that thus, Vesuvius, even Etna, and many similar mountains, have gradually been raised from a low level to their present height; and the certainty that volcanic cones must have attained their present elevation by this process, has led some geologists to suppose and assert a similar gradual increase of height in the case of the enclosures of craters of elevation; although streams of lava never occur in elevation craters. Our journey has afforded us complete proof, that *a volcanic cone can never be produced by the continued building up of streams of lava*, that its height can be increased only by the sudden elevation of solid masses, and that the whole cones of *Etna*, *Vesuvius*, *Volcano*, and *Stromboli*, owe their first elevation to a sudden projection above the surface.

We have to thank the unceasing activity in observing of M. Elie de Beaumont, for the chief proof of this important fact, a proof which I may term truly striking, inasmuch as it directly seizes hold of the subject, and leaves so few difficulties unexplained. He has ascertained by the careful measurement of about thirty streams round Etna, and of a great many on Vesuvius, that a stream having an inclination of 6° or more cannot possibly form a continuous mass; it falls so rapidly that it can acquire only an inconsiderable thickness—not amounting to more than a few feet. It is only when the inclination is not more than 3°, that the mass can spread, and can accumulate to a considerable height. Now as the last third of Etna rises with an inclination of 29° to 32°, it is clear, that when even a stream of lava flows from the great crater, which very rarely happens, it can produce very little effect either as to the increase or the external form of the mass. Even at the bottom of the *Val di Bove*, which is a great subsidence on the declivity of the volcano, the inclination of the streams is still 8° or 9°, and hence their thickness is so inconsiderable that their course is recognised by their black colour, and not by their bank-like continuation. The form also of Etna is quite regular, rising up on all sides with a uniformly advancing outline. The many, almost innumerable cones of eruption of the declivity and round the base, stand like warts on the vast colossus; and the streams which flow from it so completely disappear at a short distance, that we must necessarily regard it as an absurdity to ascribe to them even the slightest influence in altering the form of the mountain.

A few of Beaumont's determinations may shew the truth of the conclusion drawn: The great stream of lava which in 1669 destroyed many villages, and flowed past the walls of Catania to the sea, burst forth at the foot of the *Monti Rossi*, with an inclination of 2° 58′, and so proceeded eastwards from *Monte Pilieri*. There, where the road from Nicolosi to Torre di Grifo crosses this stream, its medium inclination up towards its source is 3° 45′, and down towards the sea 2° 34′. Near Catania, where the breadth of the stream is much less considerable, its inclination is 5° or 6°, but soon diminishes towards the sea. The medium inclination of this rapid stream is therefore only from 2° to 3°. The lower part of the stream of lava which in 1832 threatened Bronte, has an inclination of 1° 51′, and still presents to the eye a very distinct slope. The slightly elevated streams which descend very rapidly from the woods at Zaffarana have a medium inclination of 6° 23′. From the base towards *Aci Reale* where they form high banks, their inclination is 2° 13′. The highly inclined and broken up streams of lava between *Randazza* and *Lingua grossa* vary from 4° to 4½°.

A stream which descends from the *Piano Arenoso*, under the summit of Etna, to the steep *Val di Bove*, has an inclination of 24°, but then it has only marked its course by a narrow line of very loosely cohering slag, like all other streams which descend from a similar height, with so great an inclination.

The same is the case on Vesuvius. The broad stream of lava which is crossed before the Hermit's Hill is reached, descends with an inclination of 3°. The streams of 1804 and 1822, which pass the hill of Camaldoli, from the Torre del Annunziata, have an angle of not quite 4°. The last cone of Vesuvius, on the contrary, rises with an inclination of 28° to 30°. Very frequently, much more so than in other volcanos, streams flow down this steep declivity. We look out for them, in order to ascend on their surface to the summit. But we never see them of great extent; they hardly ever have a greater thickness than four feet, and at the edge of the crater their mass is like a ray on the slope. They pierce for themselves rapidly a deep and narrow furrow in the loose materials, and cannot extend at all in breadth. On the 12th August 1805, Humboldt, Gay-Lussac, and myself witnessed this phenomenon. We were standing at 9 o'clock in the evening on the balcony of a window in sight of Vesuvius. Suddenly a line of fire shot like lightning from the summit to the base, and remained fixed on the mountain like a burning thread. We proceeded rapidly in a boat to Torre del Greco; but the stream had already obstructed the great road. After such a sight, and after the experiments mentioned, we require no further arguments to convince us that mantle-shaped masses, or masses spreading out over a considerable space, cannot be streams of lava which have flowed on steep declivities; and the observations of M. Elie de Beaumont give certainty to this conclusion.

Even though the beds of which Vesuvius, Somma, Etna, and Stromboli are composed, were sent from the interior of the earth in a liquid state, yet they cannot have been originally formed in the condition in which we now find them, viz. as the surrounding masses of a steep cone; but must have acquired their present form from a cause acting on themselves, viz. the elevation round an axis, which axis was opened up in the form of a crater after the elevation.

It is indeed very remarkable and striking, that this was not observed at the first glance of the Vesuvius of our day. Hamilton, it is true (*Campi Phlegrei*, p. 63), makes the very well founded remark, that probably this volcano may have been first formed at the time of the earliest of all known eruptions, that which destroyed Herculaneum and Pompeii; for the very accurate Strabo gives a description of the mountain, which does not at all correspond with its present form, or with that it presented at the time of any of the eruptions. He says: *dempto vertice, qui magna sui parte planus, totus sterilis est. Geog. lib.* v,—a mode of expression which cannot apply to a steep and sharp cone like *our* Vesuvius.

Vesuvius, or Somma, according to Strabo.

Strabo would certainly not have omitted to mention the double hill; Spartacus would not have pitched a camp for ten thousand gladiators in the small crater of the steep Vesuvius; Pliny would not have forgotten to enumerate in his list of volcanos a mountain so like Stromboli as the present cone of Vesuvius, if it had been in existence.

Somma and Vesuvius after the time of Pliny.

Hamilton, however, was of opinion, that this cone had been gradually produced by the continued eruptions of ashes and lavas. Its height has, on the contrary, been constantly decreasing, and will go on diminishing. It is extremely probable that Vesuvius has become a true volcano by this elevation in the interior of the crater of Somma, or in the interior of Strabo's Vesuvius; or that it is only since that time that a permanent communication has been opened with the atmosphere: for the Somma itself possesses so perfectly all the characters of a crater of elevation, that we may regard it as a model of this volcanic form, and nothing is visible in this mountain which indicates a resemblance to a real volcano, or which is similar to a real stream of lava. Not only are the beds of leucitophyre, of which it consists, spread over a great part of the circumference, as we now see it, but they are inclined to the exterior, with angles varying from 20° to 30°, without any diminution of their often considerable magnitude—a state of things completely at variance with the phenomena presented by streams of lava having so high an inclination. But the elevation of the whole of the vast mountain, in its full extent, is proved in a still more striking manner by the mode in which the Neapolitan tuffa is disposed round the declivities. The tuffa is a white porous rock, composed chiefly of pumice, and extends over the whole plain between the Apennines and the sea. It is found from Capua to the hills of Sorrento, from Nola to beyond Naples, and almost always in horizontal strata, reposing immediately one above another, and thus the surface is perfectly level. These white strata approach the Somma without interruption, but when they reach its base they immediately ascend, and follow the inclination of the acclivity, at a high angle. At a certain height, which remains perfectly the same round the mountain, they stop, and then we observe, rising with a high inclination, the black *leucitophyre* beds of the walls of Somma, which continue to the summit. The boundary of the tuffa round the mountain is rendered distinctly visible from a distance by the little platform which results from this slight difference of inclination between the beds of tuffa and of leucitophyre; and indeed these relations which are of such high importance for the history of the whole, are beautifully exhibited, and in a manner as clear as it is picturesque, in the superb view of the volcano, and its neighbourhood, obtained from the town of Naples.* The height to which the tuffa ascends is about 1900 feet above the sea, both on the acclivity on the Somma and Ottajano side, and on that above Pompeii and Torre del Greco. Its limit is the long hill on which the well known house of the hermit is built. The upper part of the Somma, that without tuffa, rises

* The boundary of the tuffa is well marked in the two cuts on the preceding page.

1500 feet higher. In the whole plain of Naples the horizontal strata of tuffa rise only at one point to the height of 1419 feet, near Camaldoli of Pouzzoli, and there only for a very limited space. Their usual height in the plain never exceeds 800 feet above the sea, and that is not half the height to which they are elevated on the Somma. At the volcano, therefore, they are no longer in their original position, but have actually been elevated round an axis, which is the axis of the crater itself.

It is not generally stated, that the strata of white pumice-tuffa surround not only the side of the mountain towards the Apennines, or towards S. Anastasia, Somma, and Ottajano; but also all the part towards the sea; and it may escape many observers, that the Hermit's Hill, which is cut through by the path, belongs to the general covering of the plain of Naples, and not at all to the rocks of Vesuvius. It is easy to be convinced of this being the fact, by examining the direct connection which subsists between the mass composing the little eminence, and the lower strata at the base of the mountain. It is equally certain, that similar strata appear in the ravines above Torre del Greco, and that the products which occur at the foot of the hill of Camaldoli of Annunziata, are to be included under the same head. As Professor Link and myself descended on the 21st of October 1834, from the Lava which had burst out on the 8th August to Bosco tre Case, we saw also, on that side, considerable strata of white tuffa, almost directly above Pompeii. Hence it seemed evident, that it must have been from such strata that the fragments of pumice which cover Pompeii were separated, and which, hitherto so inexplicable, there lie mixed with Vesuvian leucitophyre. Leucitic rocks are not found in connection with pumice. The latter is a product of the conversion of trachyte into obsidian. But both these are substances which never occur in Vesuvius; and the volcano has never been known to throw out the smallest fragment of pumice. Hence the pumice of Pompeii remained an enigma. If, however, as is now nearly certain, it has been torn from such strata, similar to those existing above Bosco tre Case, it becomes extremely probable that Vesuvius, when it rose from the middle of the crater of Somma as a permanent volcano, projected around on the side towards the sea, not only the upper portion of the surrounding

Somma, but also a considerable mass of the upper strata of tuffa, in order to open a place for its own beds; and then by these appearances, Strabo's description will be confirmed as perfectly consonant with truth, and will be recognised as a most important document in the history of volcanos in general.

The Neapolitan pumice-tuffa is not a direct product of volcanic eruption, but is a tertiary formation as much as the limestone of Syracuse and Palermo. It has been formed in the sea and distributed by the sea with regularity over the surface. This has not been doubted since the time of Hamilton; but it is too often forgotten when separate eruptive phenomena of the Neapolitan district are described, and when the origin of the tuffa is ascribed to such particular eruptions. But there is almost no district that is connected by this tuffa, in which marine productions do not occur in the strata; and these occur of such beauty and perfection, and with such completely preserved shells, that we might think it impossible that they had ever been at any period, exposed to the violent movements of volcanic action. The Neapolitan collections, especially those of the Academy, of Monticelli, and of Dr Leopold Pilla, contain excellent specimens of this description from various localities; and others from many different places, we find figured but indifferently in Hamilton's work. Among the last there is in Plate xlv. a large beautiful oyster included in the tuffa, from a quarry at Baiæ. Dr Pilla has a similar oyster contained in the tuffa of Posilipo, from where the new road is cut through the hill. Hamilton has figured a whole collection of a Cerithium, probably the *Cerithium vulgatum*, found in a quarry at the summit of Posilipo, and similar to the species which occurs so frequently in Ischia, and at the Faro of Messina. The 47th plate is entirely a representation of such a Cerithium conglomerate, which was found in the Fossa Grande, under the Hermit's Hill, near the Somma, and which is so well known as a rich locality for minerals. Fig. 6. of Plate xlii. of Hamilton's work represents a group of shells of a Pectunculus, from a tuffa quarry under Capo di Monte; and Monticelli has a similar pectunculus from the Somma in his collection. Dr Pilla has discovered a small Echinoneus, in considerable quantity, in the tuffa above the village of Somma, which is very similar to the Echinoneus subglobosus, figured by Goldfuss, Plate xlii. fig. 9, which I have also found in limestone near Syracuse. The Cardium edule from the Somma is also to be seen in the collection of Dr Pilla. These facts seem quite sufficient to prove, that all strata of tuffa are not thrown out directly from a volcano, but *that they are a marine formation*, similar to the tertiary limestone, and that, on that account, they are equally distributed over the whole surface.

Since the Somma pierces through and elevates the strata of tuffa, it cannot, of course, have existed as a mountain previously to the formation of the tuffa. Still volcanic activity was not on that account entirely without perceptible operations in this district. It is remarkable, and in the highest degree worthy of attention, that the tuffa of the Hermit's Hill, and also the strata in the valleys called the Fossa Grande and the Fossa della Vetrana, contain between the fragments of pumice many blocks and pieces of leucitophyre. In the tuffa of Naples there are no traces of that substance. It would be important to know at what distance from the mountain the leucitophyre is no more found, but such investigations have never been undertaken. These pieces must therefore belong to the strata which were at first spread over the submarine surface by volcanic agency, and which were at a later period raised up as walls of the crater of elevation. But, associated with them, and also surrounded by and included in the tuffa, we find masses of dolomite and other rocks of older formation, which contain the greater number of those beautiful crystals, by which Vesuvius has become more celebrated among collectors of minerals than any other mountain in the world. It has been calculated, that of all the mineral species known, more than the half occur on the declivities of Vesuvius, and that by much the greater number of these belong to the fragments met with in the tuffa. They are generally termed the ejected masses of Vesuvius; and though we have no examples of such masses having been thrown out from the volcano, yet it has been imagined that they have been so at an earlier period. It becomes quite evident how erroneous, nay how completely absurd this opinion is, when we reflect that the including tuffa is identical in its formation with that of Capua and Naples, and that it was spread around previous to the elevation of the Somma. The included masses must therefore have been in this district before the Somma, and much less Vesuvius, existed. They therefore cannot be ejected masses of Vesuvius, or even of the Somma. Most probably they are the products of a submarine deeply-seated volcanic action; and as an argument in favour of this opinion, we have the great analogy which subsists between the substances now under consideration, and the crystals which have been produced by the action of primitive rocks, that have been sent from beneath, on limestone, and the contact edges of both rocks, as for example at Monte Monzoni, in the valley of Fassa, in the valley of Ala in Piedmont, and also at Arendal in Norway. At all these localities a great many of the Vesuvian minerals occur, and in part of equal beauty, viz. vesuvians, garnets, epidote, unattached crystals of augite, and others. It is only numerous species of the zeolite family that are peculiar to Vesuvius. These have been formed at a later period than the first mentioned minerals, and it would seem under very different circumstances; meionite, nepheline, and sodalite, frequently cover vesuvian, crystals of hornblende, and garnets, but are never covered or enveloped by these minerals.

Should appearances so varied and so intimately connected together not be sufficient to prove the elevation of the Somma through the strata of tuffa, and the elevation of Vesuvius in the middle of the crater of the Somma, still more decided evidence is to be found in the neighbourhood of Naples, a district so rich in important volcanic phenomena; evidence which seems to place the question beyond all doubt. Thus the elevation of such strata of tuffa with a crater actually occurred before our eyes. The *Monte Nuovo near Pozzuoli*, formed on the 19th September 1538, *is a true crater of elevation*, and by no means an erupted hill. The disintegrated strata of tuffa in the middle, and the blocks, ashes, and dust scattered around by the gases of the interior, by which Pozzuoli itself was nearly entirely buried, and every thing involved in darkness; might well lead the contemporaneous observers to the conclusion that the mountain itself had been produced by these ejected masses; and so much the more, because its surface was seen covered by them. But the aspect of the crater teaches us quite a different view of the subject. When, on the 11th October 1834, the distinguished French geologists Elie de Beaumont and Dufrenoy, and myself, made the circuit of the crater, and descended into its interior, we saw, with the greatest distinctness, on the declivities, the terminations of the strata, whose component rock could hardly be distinguished from the ordinary tuffa of Posilipo. The inclination of the strata is to the exterior all around, as may be easily observed. In the interior of the crater, and on its bottom there are black slags in large masses; and on the outer surface the external covering is formed by large scattered porous blocks of altered trachyte and other similar fragments. Had the internal walls of the mountain been formed of ejected masses, they would not be white, fine-grained, and compact, but would only resemble shapeless conglomerates, composed of extremely large and earthy fragments, to which they have no similarity whatever.

Not long afterwards, we ascended the crater of Astruni, one of the largest, and probably also the most beautiful, of all the craters in the Phlegrean fields. The rock, which makes its appearance on the interior declivities, is by no means black and slaggy, as we might expect it to be in such a crater; on the contrary, it is rather striking from its great whiteness. Slags lie in it as at Posilipo. It is composed of the strata of tuffa, which are inclined round the axis to the exterior, and which are well seen in Hamilton's representation, plate 20. This crater is not level at the bottom, like Monte Nuovo, but presents in its centre several hills, which rise to a height of 200 feet, and unite to form a dome-shaped whole. These hills are composed of *trachyte*. The rock is not a lava, for we can nowhere observe the slightest trace of a stream. The mass also is every where continuous and compact, being composed of large rocky portions, which are separated only by clefts into large blocks. The trachyte consists of a grey, coarsely splintery, much shattered basis, in which are imbedded numerous and often considerable crystals of glassy felspar, and in smaller quantity, black, small crystals, resembling augite. The whole rock is like what we would expect to find in a hill in the *Siebengebirge*. How beautifully does not this phenomenon explain the whole cause or history of the volcanic operations! In *Monte Nuovo* we have a mountain with a crater of elevation in its centre, but without a solid nucleus. In the crater of *Astruni*, the compact masses rise up in a dome-like form; but they are not broken up—no lasting communication with the interior, in short, no volcano is produced. These masses remain, as it were, a model of the great trachytic unopened domes, which are so numerously scattered over the surface of the globe, and of which we have examples in the *Puy de Dome* and *Chimborazo*. *Vesuvius*, finally, does not rise up alone, but exhibits also the required permanent communication which we see opened at its summit, and thus affords an example of a true volcano. It is probable that even the spectacle, and the opportunity of investigating so extraordinary a series of events, will in a short time be presented to us. In the beautiful and extensive elevation-crater of Santorin, which is composed of strata of tuffa, trachytic masses have been raised up as in Astruni, but only in detached and but slightly-continuous rocks. The bottom of the crater has, near the land, a depth exceeding 600 feet. For a series of years this bottom has been rising gradually from the middle of the crater. The depth of the sea is constantly diminishing, and at the present time, the raised up land is very near the surface. We have here evidently an example of a trachytic dome, which will probably rise much higher than the surrounding edges of the island; and it is quite possible, nay, extremely likely, when we remember the violent movements to which the northern part of the Morea is exposed, that the mountain which is now being elevated will at length break out, and form a volcano. M. Virlet was the first to make known this unexpected and remarkable fact,* and it is too instructive and important to allow us to pass it over without quoting the words of the author. The island, says Virlet, is rising between Micro-Kameni and the haven of Thira in Santorin. Twenty years ago, it was fifteen yards under the surface of the sea; in 1830, Colonel Bory and M. Virlet found that the depth was only from three and a half to four yards. Since that time, the depth has been so much diminished, according to the public journals, that the appearance of the island may be speedily expected. Its summit extends 2400 feet from east to west, and 1500 feet from

* Bulletin de la Soc. Geol. de France, iii. p. 109; and Edin. New Phil. Journal, vol. xxi. p. 175.

north to south. The slope sinks rapidly, and at a short distance the depth is very great. The island is rising, says Virlet, like a vast graft out of the sea.

Thus islands of elevation and craters of elevation are quite a general consequence of volcanic activity; but they are not volcanos, though, nevertheless, as it would seem, they exercise a much more important influence than the largest volcanos on the alteration, and especially on the increase, of the surface of the earth. Such islands are doubtless still rising from the sea. It has been frequently conjectured, that all coral islands of the South Sea which contain a shallow lake *(lagune)* in the middle, may be regarded as islands of elevation; and a new and extremely remarkable account given by Poeppig in the excellent and talented narrative of his journey, presents us with an example in which Nature seems, as it were, to have been surprized in the very act of forming such an island.

Captain Thayer, of the American schooner Yankee, visited the haven of Talcahuano in Southern Chili. Poeppig saw him there, and obtained permission to examine his journals. It appeared from this authentic source, and from the relation of the Captain, that, on the 6th September 1825, in south latitude 30° 14′, and east longitude 178° 15′ from Greenwich, an entirely unknown small island was descried from the ship. A thick smoke rose from the middle of the island. Boats were sent to examine it. As they approached a completely barren rock was seen, which rose only a few feet above the surface of the sea. It consisted of a broad ring which included a small pond, and which, being broken at one point, seemed to admit the sea. The sailors sprang out into the water in order to drag the boat over the shallow, but in an instant they sprang back in the highest degree alarmed, because the hot water had burnt their feet. The smoke was seen to issue from several fissures which traversed the surrounding ring. Only at one point sand was found; all the rest consisted of solid rock. The crater had a diameter of 800 paces, and sloped so rapidly externally that at a distance of 100 fathoms no bottom could be reached. Nevertheless, at a distance of four English miles the temperature of the sea water was 10° to 15° higher than had previously been remarked in these latitudes. This is the first time that one of the flat South Sea islands containing a sea lake has been seen smoking and giving out vapour; and it is not extraordinary, for, as such a phenomenon only indicates violent volcanic action, and not the existence of a volcano, the effects of the agency of the fire disappear after a short time, and can only be seen by those whom accident has conducted at so transient a moment to such an inland. The solid masses of which it was composed were widely different from those of Ferdinand's island (Graham's island) off Sicily, or of Sabrina, near St Miguel in the Azores, whose solid nucleus did not reach the surface, and whose loose layers of erupted slags and lapilli were speedily destroyed by the sea. In a few years corals surround such South Sea islands as the one we have instanced, they become a resting place for fatigued birds, and are gradually covered by vegetation.

We may therefore feel assured that discoveries in the great ocean will never be exhausted. Islands will continue to rise from the depths of the seas, and the various conditions of their vegetation will relate their histories.

That elevation craters rise out of the sea is, however, quite accidental, and does not belong to their internal constitution, or to the conditions of their appearance. These relate chiefly to the great restraining covering, which obstructs the extrication of the imprisoned vapours from the interior, and which must therefore be removed and broken up before the vapours can escape into the atmosphere. Hence craters of elevation can, in like manner, be formed on the solid land, or, on already existing and raised up islands, and of these we have examples, sometimes of the greatest distinctness, in almost every land. The *Laacher-See* near the Rhine, Cantal, and Montdor, are among the most remarkable elevation craters occurring on continents; and some which are precisely similar have been actually formed before our eyes. When the island *St Marie* in the Azores was discovered, Prince Henry bestowed it on its discoverer, the navigator D. Vincent Cabral, and it was cultivated and peopled. Some time afterwards Cabral reached the north-west side of the neighbouring island *St Miguel.* He found a flat productive land of great extent well suited to cultivation, and hence in the highest degree adapted to the foundation of new colonies. Nearly a year was occupied in St Marie with preparations for this new settlement. When Cabral, provided with every thing that was necessary, again arrived at St Miguel, he was greatly astonished to find that the part of the island previously visited by him had during the summer been completely overturned and destroyed. Instead of the plain he saw a high mountain; the whole district was devastated and covered by slags and large blocks, and the cultivation of the land was now quite impracticable. The mountain is 2000 feet in height. It surrounds an enormous crater, which at the upper edge has a circumference of fifteen English miles. Two lakes are situated in its interior, *Lagoa Grande* and *Lagoa Azul*, and the whole internal flat surface is termed the *Vale de las sete citades*. The circumference of the interior of the crater amounts to nine English miles, and the declivities are composed of beds of pumice. This, then, is a mass which of itself would have formed a considerable island; an island which would have equalled in size and in height most of the Sandwich islands, or those which surround Otaheite. It lies, as is well worthy of observation, in the same direction, viz. from N. W. to S. E., as both the other similar craters of elevation which occur in St Miguel, and in the general direction of the Azores. It is the direction of a great rent on which the volcanic phenomena have manifested themselves. St Marie, the most remote island of this group, and a little out of the direction, forms the edge of the fissure. It is entirely composed of clay-slate and limestone, presenting no example of volcanic rocks, and is the only one of these islands which has this geological constitution (*Captain Boid on the Azores*, p. 10). Since then craters of elevation can be formed both on the solid land and on islands which have already been raised up, it cannot surprise us that in such a position the beds composing their walls or occurring in their vicinity should contain land productions. No other conclusion can thence be drawn, but that such land productions have been carried thither by the sea, or that the elevated surface was not covered by the water of the sea.

That the greater number of elevation craters, and almost all volcanos, are surrounded by trachyte, or rocks resulting from it, or are composed of such substances, was some years ago considered by me as a very certain inference. But the discoveries of Gustavus Rose regarding felspar, have thrown a new light on this subject, as well as on the whole science of geognosy, and from this source a new and unexpected view has been obtained of the rocks of volcanos. By means of the conclusions resulting from these discoveries, and the more extensive application of the more exact determinations which have been rendered necessary, a new and entirely unexplored field of investigation has been opened up. I may regard it as one of the not wholly unimportant fruits of our autumn tour, and one produced by the discoveries of Rose, that the rock of Etna appeared to us for the first time in its true form and in its true composition. The great abundance of crystals of felspar contained, as I believed, in the lavas of Etna, had induced me to assume that the entire volcano consisted, like other volcanoes, of trachyte. And indeed the newest accounts of the latest observers describe the declivities and the interior of the mountain as formed of trachytic rocks. The discoveries of Rose teach us to adopt another opinion. The entire want of obsidian and of pumice in Etna was of itself an extremely remarkable and striking phenomenon, since, in every other case, trachyte indicates pumice, as, on the other hand, pumice indicates obsidian and trachyte. After many careful investigations in the district of Catania, and on Etna itself, M. Elie de Beaumont and myself were, after much doubt, at length convinced that felspar does not occur in Etna, and therefore that there is no trachyte. All the streams of lava, as well as all the beds in the interior of the mountain, consist of a mixture of *augite* and *labradorite,* and in this respect resemble the *dolerite* of the basaltic series. But M. de Beaumont is inclined to think, that the preponderance of labradorite may render a new name necessary for the Etna rock. From such a rock obsidian and pumice were certainly not to be expected. Etna is hence more related to basaltic rocks than to trachyte. It does not at all resemble the Lipari Islands, for some of these are entirely composed of trachyte. The others, on the contrary, viz. Lipari, Volcano, and Salinas, consist almost entirely of pumice, or of tufaceous rocks, in which pumice appears as a chief ingredient. It is only the constantly active Stromboli that is again different from these last. It would appear from collections, and from the investigations of Professor Friedrich

Hoffmann, that the rock forming the beds and lava-streams of that volcano is a very fine-granular mixture of augite and labradorite—an Etna dolerite. Now, since the Vesuvian leucitophyres are distinguished from this rock only by the addition of leucite, and the diminution in quantity of the labradorite, it is clear that these relations prove a resemblance and a connection of the whole eastern part of the series of volcanic phenomena in Italy, viz. of Etna, Stromboli, Vesuvius and Somma, Rocca Monfina near Sessa, Monte Albano near Rome, and Monte Mario on the Tuscan frontier. Trachyte and trachytic products occur, on the contrary, towards the west. They form the Lipari Islands, and the Ponza Islands lying far out at sea. There is an essential geognostical distinction in the position of these rocks.

Another important distinction in the rocks composing volcanos presents itself when albite takes the place of felspar. A rock is then formed, which can no longer be named trachyte; for it is not a mere alteration of the trachyte, but is a very constant compound, which invariably occurs, having entirely different relations. In Europe it is rare; for the Italian islands, as well as the greater part of Mont d'Or and Cantal, the Siebengebirge near Bonn, and also the mountains of Iceland, are composed of rocks in which the real and true felspar cannot be mistaken—therefore of trachyte and of trachytic masses. But this is not the case on the other side of the ocean. According to Rose's investigations, we may assume, with considerable certainty, that not one of the almost innumerable volcanos of the Andes consists of trachyte, but that all contain *albite* in their component masses. So general an assertion may seem extremely bold; but it loses this appearance when we reflect, that, by means of Humboldt's Travels, we had already obtained information regarding nearly the half of these volcanos and their products, in both hemispheres. We are indebted to Meyer for our knowledge of the volcanos lying to the south, viz. in Bolivia and northern Chili, and which were entirely unknown until his journey. Poeppig has extended this information to the most southern limits of Chili. Since also it would appear from Erman's discovery, that the northern volcanos of Schevelutsch in Kamschatka are composed of a similar rock, the volcanos of North America are probably similarly constituted. A distribution so extensive and so remarkable seems sufficiently to justify the name of *Andesite*, under which this compound of predominating albite and little hornblende has already been sometimes mentioned.

The object of the present memoir is to shew anew, *that elevation-craters are not volcanos; that the distinction between the two is well grounded and important; and that even the cones of volcanos can be formed only by sudden elevation, and never by the building up of streams of lava.*—POGGENDORFF's Annalen, vol. xxxvii. p. 169.

On the Temperature of the Earth's Surface during the Tertiary Period. By M. ELIE DE BEAUMONT.

IN the last Number of this Journal (page 177), we inserted an abstract of M. Deshayes' Memoir on the Temperature of the Tertiary period, communicated to the Academy of Sciences of Paris; and we now give an account of some interesting observations on the same subject, read by M. Elie de Beaumont to the Philomathic Society of Paris. The author stated that he had for some time been occupied with the investigation of the temperature of our latitudes during the different geological periods, and that, in regard to the Plastic clay and the *calcaire grossier*, he had concluded that the temperature was rather less elevated than that deduced by M. Deshayes. According to the latter naturalist, the basin of Paris must have possessed, at the epoch of the *calcaire grossier*, a temperature at least equatorial—that is to say, at least amounting to 27½° C. (82° F.) M. Elie de Beaumont is of opinion, that, agreeably to the results obtained some years ago by M. Adolphe Brongniart, the climate of our countries, during the more ancient tertiary period, must have resembled exceedingly, in its general relations of temperature, that of Lower Egypt, of which the mean temperature at Cairo is 22° C. (72° F.) He founds his calculation on the following considerations: At the epoch of the Plastic clay and the *calcaire grossier*, the arborescent ferns and cycadeæ which had previously covered our continents, and of which some still occur at the present day between the tropics, had certainly ceased to exist in our latitudes, since, according to the researches of M. Adolphe Brongniart, we find no remains of them in tertiary strata. At the same period, the coral reefs which, during the silurian, and probably also during the carboniferous epoch, extended in the sea to Iglolik to the north of America, in latitude 69½°, and which, during the Jurassic epoch, extended to Kirkdale in Yorkshire, in latitude 54½°, had also ceased to figure in our latitudes, and since then have not reappeared. A lowering of the temperature of the winters seems to M. Elie de Beaumont the sole cause which can be assigned for this triple disappearance. The temperature of the winter season in our latitudes must have already been pretty low at the period of which we are speaking, since the tree-ferns and the cycadeæ could not continue to exist on our continents, and since the species of polypi, which have the power of grouping themselves in reefs, could not continue to live in our seas. On the other hand, the plastic clay and the *calcaire grossier* of the environs of Paris, and even the beds formed still more recently on the surface of France or the neighbouring countries, present abundant remains of palms, of crocodiles, and of large pachydermata. The temperature of the winter season at the epoch of the *calcaire grossier*, must then have been sufficiently elevated to allow these organic forms to prosper; and it might have been still a little further lowered without causing them to disappear. By combining this consideration with the preceding one, we obtain two limits, between which must be comprised the winter temperature of the countries in which we live at the epoch of the deposition of the *calcaire grossier*. These two limits approach each other pretty closely, and the winters of Cairo fall precisely between them. In fact, palms and crocodiles flourish in Egypt, and hippopotami and other large animals live there. On the other hand, tree-ferns and cycadeæ do not exist; and coral banks, which border the coast of a great part of the Red Sea, stop at the port of Tor in Arabia, about 2° of latitude south from Cairo. As to the temperature of the hottest seasons of the year, it is at present almost the same in all the countries which are not very near the poles; and M. Elie de Beaumont is of opinion, that this *normal maximum* of terrestrial temperatures, cannot have varied considerably since the earth was covered with vegetables. For if the temperatures of the winters, and those of the warmest periods of the year, were, in the basin of Paris, at the epoch of the deposition of the *calcaire grossier*, the same as those of the present day at Cairo, the mean temperature must also have been the same, that is 22° cent. (72° F.)

M. Deshayes founds his higher calculation on the great number of fossil shells collected in the basin of Paris. This number amounts to 1200, while in the seas of Senegal and of Guinea not more than 900 are known; but it must be remarked, says M. Elie de Beaumont, that the 1200 species of fossil shells found in the basin of Paris, did not live simultaneously; they are obtained from several layers formed successively, and of which the richest would be very far from affording so large a number; and probably also, he adds, we are better acquainted with the fossil shells of the environs of Paris, than with the living shells of equatorial seas.

M. Elie de Beaumont then enters into some details on the manner in which he conceives the diminishing temperatures of successive geological periods may have resulted from the gradual cooling of the internal mass of the earth. We know that a *constant relation* exists between the excess of temperature presented by the surface of the earth, above that which the sun and the atmosphere tend to communicate to it, and the gradual augmentation of the temperature of deep places. At the present day, when we descend into the interior of the earth, we find that the temperature augments about 1–30th of a degree cent. for every yard, and the excess of temperature of the surface is about 1–32d of a degree. At the epoch of the coal formation, the augmentation of temperature may, without doubt, have been about 1–3d of a degree for every yard; but important geological considerations are opposed to the supposition that it was more considerable. The excess of temperature of the surface, then, could not be higher than 1–3d of a degree cent., a quantity too small to account *directly* for the difference of the climates of the present day. The explanation of this difference, so well ascertained by geologists, can be found only in the accessory effects which an augmentation more rapid than at the present day could produce in the temperature of deep places. These accessory effects may, according to M. Elie de Beaumont, be reduced to three, which have all concurred to render the polar climates much less different from the equatorial climate than they are at present.

1. In the more ancient geological periods, the polar ice could not exist, and its absence would probably of itself be sufficient to elevate the mean temperature of the pole to 0° (32° F.), whereas, at the present day, it is perhaps 25° below 0° (—13° F.)

2. Since the polar ice did not exist, the sea must have presented, from the surface to the greatest depth, a much more equal temperature than at the present day. This temperature must have been every where a certain number of degrees above the maximum of the density of the water of the sea. In such a sea, the temperature of the surface could never be lowered more than a very small quantity below the temperature of the mass. This sea must have been covered by fogs in the parts near the poles, whenever the sun was removed from the horizon.

3. Since the temperature of deep places increased ten times more rapidly than at the present day, thermal sources and jets of hot vapour must have been much more frequent; nearly all the springs must have been thermal; and each time the sun left the horizon of the poles, the surface must have been covered by fogs, which prevented nocturnal radiation, and the radiation in winter. These fogs, which existed only during the absence of the sun, moderated the cold of the nights and the winters, without changing the temperature of the summers. They, therefore, elevated the mean temperature, and rendered the climate more mild, more uniform, and more equatorial. They were united with the action of a sea, which was warmer and more difficult to be cooled at the surface, to produce in the temperature of the pole a *positive anomaly*, diametrically contrary to the *negative anomaly* produced by the permanent ice of the present day.

Account of one of the most important Results of the Investigations of M. VENETZ, *regarding the Present and Earlier Condition of the Glaciers of the Canton Vallais.* By JOHANN VON CHARPENTIER. With later additions by the Author. *

Mr VENETZ, engineer of bridges and roads in the Canton Vallais, has been occupied for several years with the study of glaciers and their accompanying phenomena. His investigations have led him to results which are of great importance for the history of the changes which have taken place on the surface of the earth. My object in here indicating one of these results is chiefly to direct the attention of geologists anew to an important geological phenomenon which seems now to be nearly forgotten by the philosophers who devote themselves to the history of the earth, and this owing to an opinion entertained that the subject has been exhausted and fully explained,—I mean the erratic blocks or the fragments of the alpine rocky masses which occur from the ridge of the Alps to that of the Jura, from the southern slope of the first to the plains of Upper Italy, and generally in the valleys and at the base of all *high* mountain chains, with the exception of those which lie in equatorial districts, and on which the masses of perpetual snow cannot be converted into glaciers.

The most distinguished geologists, even those who have raised geology to the high rank it now holds among the sciences, have ascribed to the agency of water the removal or the transport of these blocks to the places where we now find them. At the time such authorities so expressed themselves, this mode of removal was considered so much the more certain and clearly proved, because the opinion was announced for the first time at a period when in fact most geologists attributed extravagant effects to the action of water, for the inequalities of the earth's surface were then regarded as exclusively a consequence of that action.

The investigations of M. Venetz on the Vallais glaciers, have suggested to him the examination of the transported blocks in

* Read at Lucerne to the Meeting of the General Swiss Society of Naturalists on the 29th July 1834.

the valley of the Rhone, and his observations on these blocks, and on the particular circumstances in which they occur, have convinced him that their removal cannot have been effected by water, however enormous may have been its mass, and powerful its operation.

It is my present intention to communicate, as shortly as possible, some of the facts which contradict the opinion of the removal of these blocks by water, and I shall neither enter into details nor instance localities, as M. Venetz is occupied with a separate publication on glaciers, in which he will communicate with care all his observations.

In all localities where we find these blocks, their accumulations present confused heaps of fragments of all dimensions, from the size of a grain of sand to that of thousands of cubic feet. There is, therefore, no separation according to the volume or relative weight of the blocks; a separation which must necessarily have existed, had they been transported by the agency of water. In that case, the largest blocks would occur nearest the district where the flood had commenced, and the fragments of rock would diminish in size, as the places where they have remained lying, are more and more removed from that first point. The largest blocks ought therefore to be found in the valleys of the Alps, and the smallest on the Jura. There is, however, as we have already said, no trace of such a separation; for blocks occur on the Jura of as large a size as any that are met with in the valleys of the Alps.

Water which carries bodies of any description along with it, produces also, at the moment its progress commences, another description of separation or division, viz. a separation into layers or strata which takes place according to the volume and weight of the masses which have been broken loose. Thus the water forms superimposed layers of blocks, gravel, sand, and mud. But there is nothing to be seen in the deposits of erratic blocks of that stratified separation which, according to what has been said above, must have existed, had these blocks been carried by water to their present localities and positions. It is true that we sometimes find strata of gravel, sand, and mud, in the vicinity of these deposits; but such stratified displays are much too partial and much too limited in extent, to give foundation to the belief that the whole mass of these blocks has been conveyed by water. I shall in another place speak of the origin of these small stratified accumulations of sand and mud.

Although most of the blocks now under consideration exhibit a rounded form, yet we occasionally find them having not only a flattened figure, but even presenting no rounding, and having edges and angles which have suffered no smoothing whatever. It would be impossible to understand how these blocks could be pushed forwards to the foot of the Alps, and driven up to the ridges of the Jura, without having lost their fresh condition and the sharpness of their edges and angles.

The deposits of erratic blocks have generally a prevailing extension in one direction, so that we cannot better compare them than to dams or walls; or they sometimes form small rather conical hills, which are either isolated or stand in a row. These deposits never occur, on the contrary, in the form of spreading and flat alluvial masses or plateaux.

The dams already mentioned run horizontally, frequently several behind one another, on the declivities and at the foot of the mountains, and their direction is, in the first case, parallel to that of the valley. Those, however, which run at the foot of mountains enclosing a valley, turn away from the mountains at their lower end, cross in an oblique direction through the valley, and would unite in the middle of the latter, if they were not interrupted by the stream running there.

The surface between two such dams always consists of fixed rock, which is covered only by a little earth, or by some scattered blocks. Sometimes two or more dams are so near one another, that they form together a single dam, with two or more crests.

This internal and external constitution of such deposits, cannot be explained by assuming that their component materials were transported by water to their present situation. Water would have deposited the blocks, especially on the flat surfaces of valleys, and at the base of the Alps, as a spreading flat alluvial mass.

It would also be impossible to conceive how, by such an assumption, we could explain the passage of the materials of these dams through the lakes, without these lakes being for the most part filled up.

This view is equally insufficient to account for the extraordinary position of immense single blocks, which we sometimes find planted vertically in the soil, in the valleys or on the sides of a mountain, and split up throughout their whole extent from top to bottom,—a phenomenon which would force us to believe that these blocks had fallen perpendicularly from a certain height, on the very spots where we now see them, and had been rent asunder by the fall, into the several fragments lying near one another.

It is further remarked, that the blocks which are derived from one of our large valleys, are never mixed with those having their origin in another neighbouring valley. This fact, which had been already observed by Escher, does not harmonize with the effects of the action of water, even though that action had taken place in both valleys at the same time. It is impossible to understand how the stones carried away by both floods, should not be mixed, at that point, at least, where the flowing of the water had continued sufficiently long to admit of the deposition of stones; and more especially where the Jura was encountered, when a reverberation or whirling must have taken place, well calculated to produce this mixture. I have also to add, that the blocks proceeding from a lateral into a principal valley, are not mingled, but that the blocks of the two valleys form separate dams.

All chains of mountains which have afforded erratic blocks, present, on all the fixed rocks which have not subsequently suffered from weathering and decomposition, the remarkable phenomenon of rounded and polished surfaces Apparently these are the consequence of friction, and as we every where see how the rocks in the beds of mountain-streams and rivulets become smoothed by the stones carried down by the water, it has been concluded, that the smooth and polished surfaces of the rocks of our great valleys, have been produced by that great flood which is believed to have transported the blocks now under consideration, so that these last must to a certain extent have acted in the manner of emery. This explanation is also grounded on the fact, that the polished surfaces occur only to the height reached by the blocks, and that where the blocks cease, the rocks are no longer smooth and polished, but present rough surfaces, which are true fracture-faces.

But a great flood of water by no means explains the circumstances accompanying the phenomenon. For how can we understand the polishing, by means of stones transported by water, of the overhanging surfaces which form the roofs of the rocky vaults, known among the natives by the names, *Barmes* or *Balmes?* How can we explain the existence, behind projecting rocks, of polished surfaces, which must have been protected by these very rocks from the stream, and from the friction of the stones carried along by the water.

But let us set aside these difficulties and objections, and let us assume, for the moment, that the polished surfaces have been actually caused by the friction resulting from streams of water. In this case, these ought to be more striking in the depths of the valleys, and towards their lower extremities, than higher up on their sides, or nearer their origin. They ought not to occur at all on mountain-ridges, and on the passes of the Alps. But exactly the contrary is the case. The smooth and polished surfaces occur not only from the foot of the Alps to their highest ridges, but become more striking the higher we ascend; and we see them on all the high alpine passes, as on the St Bernard, the Simplon, the Grimsel, the St Gotthard, &c.

I might here cite a number of facts which are in like manner opposed to the supposition of a rush of water, if I did not think those already given were sufficient to prove that the power which conveyed the blocks was not a flood.

M. Venetz thinks that this power must have resulted from the action of glaciers, and that the deposits of alpine blocks are nothing else than *moraines* or glacier-walls. I am well aware how very extraordinary, improbable, nay, fantastic, such an opinion must at first sight appear. How, indeed, can we convince ourselves, that formerly all our great valleys were occupied, throughout their whole extent, by enormous glaciers, which, at the mouths of the valleys, were spread out in the form of vast fan-shaped plains of ice, and in this manner must have covered nearly the whole land between the Alps and the Jura, and which ascended to the summit of the latter, nay, in some instances, even extended beyond it? How can we reconcile such an hypothesis with the numerous facts which prove that, in former times, the temperature of our districts was much higher than it is at present? How can we believe in the former existence of such prodigious glaciers in a tract of country which at one time produced palms, as is proved by the occurrence of casts of a chamærops in the rocks of Lausanne and Vevay?

I acknowledge that these and many other objections presented themselves, when, about five years ago, M. Venetz communicated this hypothesis to me for the first time. I remained incredulous, until at length the facts which were zealously sought for and examined to enable me to combat this opinion led me to a very different result from the one I had anticipated, and, above all, shewed me that the former existence of enormous glaciers may harmonize perfectly with the facts which prove that our climate was at an early period considerably warmer. Before I endeavour to reconcile facts which are apparently so incompatible, I shall first shew that all the phenomena exhibited by deposits of these blocks and all the accompanying circumstances can be quite satisfactorily explained by the progressive movement of erratic blocks by glaciers.

Wherever stones are deposited by glaciers, they are collected together and heaped up without order, and without any separation according to size and weight. The largest blocks are mixed with gravel and grains of sand, and all are transported to an equal distance. Thus the alluvial masses formed by glaciers present exactly that want of separation of the individual stones according to size and weight which we observe in deposits of erratic blocks, and there is thus nothing remarkable in finding on the Jura blocks of the same size as those occurring at the foot of the Alps and in the valleys.

The flowing off of the water is sometimes prevented by glaciers and *moraines*, so that at their sides small lakes are formed, in which stones, sand, and mud are deposited by the streams. It is therefore not surprising to find associated with the alpine blocks small layers of such materials, which apparently have been deposited and stratified by means of the water.

Although most of the stones transported by the glaciers are rounded, or at least have the angles and edges more or less smoothed by rubbing against one another, yet we find occasionally on the ridges of the glaciers single large blocks which have reached the foot of the glacier without any rubbing whatever, and which are therefore in a perfectly fresh condition. By these facts we can explain in what way some large alpine blocks have been removed and deposited at great distances without having suffered any rubbing or smoothing of their angles or edges.

Moraines have the form of dams or walls with one or with several crests. In some cases they have a conical form, or present a row of small conical hills. When a glacier has, as is generally the case, several moraines, these are parallel to one another; and the surface of the separating spaces is of naked rock, or rock covered by a little earth, a few stones, or strewed blocks. The external form of the *moraines*, and the relative position of several of them belonging to the same glacier, are almost identical with the respective form and position of deposits of erratic blocks.

Glaciers never produce *moraines* in the form of spreading and flat or fan-shaped alluvial masses, such as are deposited from running water; for glaciers penetrate to the fixed rock and carry before them all the existing earth, stones, and blocks, a fact well known to all those who have observed glaciers during their progress, and which is easily explained by their manner of advancing and increasing in size. This property of glaciers to penetrate to the fixed rock, and thus form and clear for themselves a path, explains to us perfectly why our lakes have not been filled by the enormous mass of blocks, rubbish, and sand which have taken their course through, or, to speak more correctly, over them, a result which must necessarily have occurred had those materials been transported by water.

The internal mass of a glacier consists of ice or rather of frozen snow in a pure state, without any mixture of earth or stones. When blocks fall through a fissure to the bottom of the glacier, they are rolled or pushed forwards. If they remain hemmed in between the walls of the fissure, they appear again after the lapse of a certain period of time on the surface of the glacier, but at a point further down the valley than where they fell in.* When, however, a block falls quite near the lower end of a glacier through a fissure to the bottom, and at a time when the glacier is retiring, it remains nearly at the same point and in the same position which it occupied when it fell. These facts, which will be confirmed by all who are intimately acquainted with glaciers, explain to us why we find so few blocks in the bottoms of valleys, or at the foot of the Alps; in other words, in all those plains which have formed the bottom of the great ancient glaciers; and at the same time, they point out how the blocks mentioned above, which are placed in so remarkable a situation, and are split up in their whole length, have reached their present position. These blocks are nothing else than such masses of rock which have fallen to the bottom of the glaciers at the moment when the latter were about to commence their retrograde movement.

It has been well known, since the time of Saussure, that the moraines of two glaciers, when they meet at a very acute angle, do not become mixed. This fact explains perfectly why the blocks derived from one of our large valleys do not mix with those which have their origin in the neighbouring valley, a circumstance which cannot be explained by the supposition that the transport of the blocks has been effected by a flood.

We know that the fixed rocks which are in contact with the glaciers are rubbed and polished by their agency. As they endeavour to extend themselves, they follow all the bendings of the rocks, penetrate into all their sinuosities and hollows, polishing their surface, even where it is directed downwards or is overhanging, an effect which could not in any degree be produced by water carrying stones along with it.

Since the glaciers proceed from the ridges of the Alps, their destroying action must have lasted a much longer time on these ridges than in the valleys and at the foot of the mountains. There is, therefore, nothing wonderful in the traces of rubbing and smoothing being displayed to a much greater extent and in a more remarkable manner in the high valleys and on the high

* This fact, which at first sight appears very extraordinary, is known to all who have frequent opportunities of observing glaciers. It is completely explained in a memoir communicated by M. Venetz to the Swiss Society, during their meeting at Bern in 1816.

passes of the Alps, than in the lower part of the valley. If these rubbings had been produced by a rush of water or a flood, the case would have been reversed.

If I were not afraid of fatiguing the reader, I would further quote a multitude of phenomena, which, in this manner, stand also in connection with erratic blocks; and I could still add many circumstances, presented at almost every step on our mountains and in our plains, and which, taken together, support the opinion, that, in former times, all the alpine valleys, and part of the plains at the foot of the mountains, were occupied by enormous glaciers.

Among others, may be mentioned, the cylindrical perpendicular scoopings which are observable on the surface of isolated masses of rock in the bottoms of the valleys; the fissure-shaped scoopings which, in the German Switzerland are termed *karren* or *karrenfelder* ; traces of scoopings on isolated masses of rock, distinctly resulting from waterfalls; the enormous extension of all old beds of river, which distinctly prove, by the regular stratification of the matter of which they consist, that the mass of water which formerly flowed into them must, for a long period, have been more considerable than the present quantity, even at its greatest height; &c.

I shall only further add, that the occurrence of deposits of alpine blocks on the sides of the Jura, and even on some parts of its ridge, by no means renders necessary the supposition that the old glaciers must have filled with their mass the whole space between the Alps and Jura, or that, in other words, their thickness was, to a certain extent, equal to the height of the ridge of the Jura above the plain. Such an opinion would be not only improbable, but would be opposed to what takes place before our eyes. For when a glacier in a valley runs at about a right angle to the direction of that valley, it sometimes happens that it extends across the valley, and rises on the side of the mountain opposite to a more or less considerable height, depending on the mass of the glacier, and which is in inverse ratio to the steepness of the acclivity it has to ascend.

Nor does the occurrence of alpine blocks at a great distance from Switzerland, render necessary the belief in an altogether improbable extent of the old glaciers; for these last blocks have apparently been transported by water, and not by glaciers. This is proved more particularly by their becoming smaller the farther they are removed from Switzerland, and by their being deposited in a stratified manner. For example, blocks of five or six feet in diameter occur near Lyons, 200 feet above the Rhone; while, in the plain of the Crau they are not more than five or six inches in diameter. In order, however, to carry such masses so far, a much larger mass of water would be requisite than that now presented by the Rhone, a mass which can only be accounted for by the occurrence of much larger glaciers than those now existing.

It only remains for me to indicate as shortly as possible the the mode of reconciling the former existence of such enormous glaciers with the facts which prove the higher temperature of our climate at an early period.

I think, that, at the time when the flat districts of Switzerland possessed a climate sufficiently warm for the production of palms, the Alps did not exist. The Jura alone formed a high land, separated to the south from the sea by a low land, a sort of flat coast, which included all the land formed of molasse and analogous rocks. Neither the Jura range nor the coast land at its base was elevated to such a height above the surface of the sea as at the present day. The low part of Switzerland formed a real coast lowland.

At some distance from this coast land, some islands stood at no great height above the surface of the sea. Their vegetation consisted chiefly of ferns, equisetaceæ, and some monocotyledonous plants, as the casts in the slates of Erbignon, Salvan, Gétroz, the Col de Balme, and other places, seem to prove.

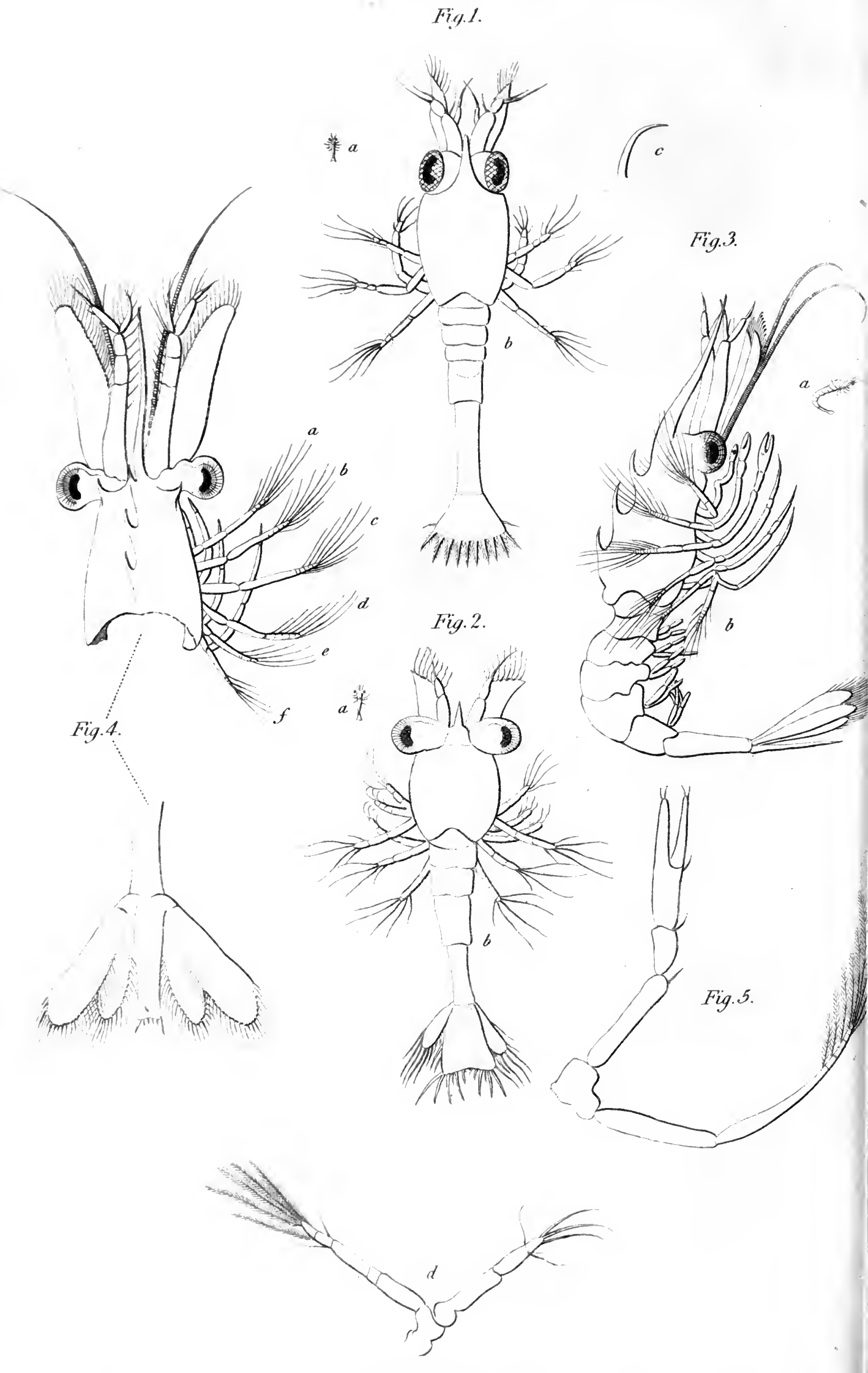
During this state of things, a great and probably last elevation of the Alps took place—an event which the labours of Von Buch and Elie de Beaumont have proved to be one of the most certain facts in geology.

The power which effected that great operation extended not only to the Alps, but also, though in an inferior degree, to the Jura, and to the coast land lying at its base, and very probably raised all of them to a much higher elevation than they at present have.

Every thing leads us to conclude that the Alps were at one time elevated to a much greater height than they at present possess. So vast an occurrence must have caused displacements, rupturings, fractures, and the formation of hollow spaces; the upraised masses must have fallen together, and sunk until all the unsupported parts again assumed a fixed position, and the whole mass acquired its present stability.

The elevation to so great a height above the sea, in combination with the diminution of the temperature of the earth itself, must have produced a great change in the climate of these districts. The climate which was capable of producing the *Chamærops*, and other plants of warmer lands, must have been converted into the climate of high northern latitudes. The atmosphere was cooled; the Alps were covered with snow, which, as it constantly fell into the valleys, formed those vast glaciers which gradually covered the whole of lower Switzerland, and pushed their *moraines* to the summit of the Jura. These glaciers then began to diminish and to retire, when the sinking already mentioned took place; and as the height of the Alps, the Jura, and the intermediate flat countries, became gradually less considerable, the climate gradually became warmer, until at last it acquired its present temperature.

The *Chamærops* formerly flourished in the neighbourhood of Vevey and Lausanne, That district probably formed, as already mentioned, a coast lowland, and must have had a mean temperature of 17°.5 cent. (64° F.) The mean temperature of those alpine valleys in which glaciers are not indeed formed, but are preserved, is 6° (43° F.) This is, for example, the mean temperature of Chamouny. If we assume that the temperature diminishes 1° cent. for every 160 metres diminution of height, then the land which had a mean temperature of 17°.5 must have been elevated 1840 metres ($17.5 - 6 \times 160 = 1840$), in order to reduce the temperature to 6°. But as the present height of Vevay, which is the same as that of Geneva, amounts to 372 metres, that district must have undergone a sinking of 1468 metres. If we assume the same amount of sinking for the Alps, Mont Blanc must have had a height of 6278 metres, an elevation not nearly equal to that of the highest summits of the Himalaya, of the Nevados of Illimani and Sorata, or of Chimborazo.



Hinder Member of Fig. 1.

Memoir on the Metamorphoses in the Macrourae or Long-tailed Crustacea, exemplified in the Prawn (Palæmon serratus). By W. V. Thompson, Esq., F. L. S., Deputy Inspector-General of Hospitals. (Communicated by Sir James MacGrigor, Bart. M. D., F. R. S., &c.) With a Plate.

Having in my Zoological Researches, and in subsequent memoirs, fully developed the metamorphoses in the *Brachyurae* or crabs, I am happy to have it in my power to make known similar changes in the *Macrourae*, and which have been traced in the prawn in a very satisfactory manner, as well as in the shrimp, and some others of this tribe of the Crustacea; in all of which the larva, although different from that of the crabs, is nevertheless a *Shizopoda,* generally of a totally different aspect from the parent animal, and provided at first with a very limited number of cleft members, commonly two or three pair, perfectly analogous to those of the *Zoe;* these larvæ, it must be farther observed, do not, in their subsequent stages, become *Megalopæ*, like those of the crabs, but appear to undergo a successive development, probably embracing *several* stages.

For several seasons previous to 1828, I had repeatedly met with animals in the harbour of Cove, so exactly similar to Slabber's metamorphosed Zoe (Zool. Res. pl. 1. fig. 1, *b*), and scarcely doubting the accuracy of his observations at that time, that I wished for an opportunity of verifying the fact by similar attentions bestowed upon some zoea, the result of which turning out so contrary to my just expectations, (as given Zool. Res. mem. 1st, p. 8), it still remained a desideratum to know what this nondescript really was; the most probable conjecture being that it might prove the intermediate stage of some of the *Macrourae.*

The summer of 1828 put me in possession of this interesting information; for by hatching the ova of the prawn, it was discovered to be the first stage of that animal! numbers of them being excluded while actually under observation (as represented Fig. 1.); thus proving that the *Macrourae* are also subject to metamorphosis, but that the larva bears no farther relation to zoea than in its members, which are similarly cleft, with the outer divisions adapted to natation. Slabber (as suggested Zool. Res. p. 8), no doubt committed some serious mistake; for, making allowance for the greater accuracy of the delineations I have given, there is every reason to suppose the two animals identical, or that both are derived from the same individual species of *Palæmon*, viz. *serratus*.

As it would be highly desirable to trace the metamorphoses of these larvæ into the perfect animal, it was attempted to preserve them alive, but without success.

At the same season of the preceding year, an animal was captured, (of which Fig. 2. is a representation), the correspondence of which with the above larvæ, in colour and size, renders it more than probable that it is the same in a more advanced stage, having acquired an additional pair of cleft members, and a pair of scales at each side of the tail, as in the animal figured Zool. Res. pl. i. fig. 6. *a*, from the equatorial region of the Atlantic, which we may therefore consider as the advanced larva of some oceanic type of this tribe.

Another animal, which can be no other than the prawn in a still more advanced stage, was taken July 25. 1824, (Figs. 3, 4, 5): in this remarkable stage, it is now no longer doubtful to what perfect type it belongs, as it presents the general appearance of the prawn, but still retains the natatory division of the members, now increased to six pair; the subabdominal fins, although conspicuous, are as yet rude; and the frontal spine or rostrum exhibits only three serratures, placed far behind, over the dorsum of the corselet. Still it is so remarkably prolonged in front, as nearly to equal the antennæ in length.

We have here three distinct stages, in each of which the animal differs considerably; and from the disparity in size between the second and third, it is most probable that there is at least one intermediate stage between these two, in which the larva may be supposed to present a shorter rostrum than the last, with fewer or no serratures, and a less number of cleft members. This being kept in view, will prevent such observers as may meet with animals of this description from unnecessarily burthening the class with new genera and species. Indeed it must be deemed a fortunate circumstance on this account, that the discovery of the metamorphosis preceded the knowledge of the major part of the larvæ of the *Crustacea*, which must otherwise have formed a very formidable group of *Shizopoda*, well calculated to bewilder future zoologists.

For the benefit of such zoologists as may wish to witness for themselves the hatching of the ova of the *Palæmon*, it may be observed, that the females, which are vastly more numerous than the males, carry their ova attached in groups to the inner branch of the subabdominal fins, and that these ova are at first of an oval figure, a pale yellowish-brown colour, and small size, but that as they increase in size they become more round, change to dark brown, then to reddish-brown, acquiring gradually greater translucency, and a pale flesh colour, with black eyes. Females arrived at this stage of gestation, being kept in frequently renewed sea water for a few days, will assuredly confirm what I have above stated.

EXPLANATION OF THE FIGURES IN PLATE I.

Fig. 1. Larva of Prawn (*Palæmon serratus*) when first hatched; *a*, of the natural size, and *b* magnified. A side view of rostrum, *c*. One of its cleft members more magnified, *d*.

2. Intermediate stage of the same? Natural size, *a*, and magnified, *b*.
3. Supposed last stage of the larva of the Prawn. Natural size, *a*, and magnified, *b*.
4. Another sketch of the fore-part and tail of the same. A natatory division of the *Pedimanillæ*, *a*. *b*, *c*, Do. of the two chelate members. *d*; *e*, *f*, Do. of the three posterior members.
5. One of the second pair of chelate members more highly magnified.

Considerations respecting a New Power which acts in the Formation of Organic Bodies. By M. Berzelius.

When new compounds are formed in unorganized substances in consequence of action between different bodies, it is the result of the mutual tendency of these bodies to comply, in a more perfect way, with their affinities. On the one hand, those substances whose affinities are the strongest combine; and, on the other, those which have the weaker affinities are expelled. Previous to the year 1800, it was not supposed that any other determinate causes of these phenomena existed, than the power of this affinity itself, along with heat, and, in some circumstances, light. At that date the influence of electricity was detected; and shortly afterwards we were led to confound the electrical agency over bodies with the chemical, and to consider affinity as nothing more than the manifestations of opposite electricities, heightened by light and heat. But still, even this system supplied no other means of explaining the origin of new compounds than the supposition that, by the approximation of bodies thus put into contact, the electrical influence succeeded in more completely neutralizing them.

Starting with these views, which are deduced from the effects which occur in unorganised bodies, and then studying the chemical actions which organised bodies present, we observe that in the organs of these latter the most different kinds of products are elaborated, notwithstanding that the matter whence they all proceed consists in general only of one identical liquid, circulating in the vessels with more or less velocity. The vessels of the animal body, for example, without interruption, receive blood from the heart, and, nevertheless, at their extremities secrete milk, bile, &c. without the admission of any other liquid which is capable, in the way of double affinity, of affecting any decomposition whatever. There is clearly here a fact, of which the science of unorganised matter can give no explanation.

At this epoch M. Kirckhoff, discovered that starch, dissolved in a diluted acid, is transformed, at a certain temperature, first into gum, and then into sugar. It was then inquired, according to the prevailing views with respect to effects of this kind, what that substance was which the acid had taken from the starch in reducing it into sugar; but it was found that no gas had escaped, as the acid reappeared, by means of alkalies, in its original quantity; that no new combination had been formed; and that the liquid contained nothing else than sugar, in quantity equal, and even superior, to the quantity of the starch employed. The cause, then, of this change was as problematical as that of the secretions in organised bodies.

M. Thénard soon afterwards discovered the peroxide of hydrogen, a liquid whose elements are very feebly retained together. Upon this substance acids produce no change; but alkalies, on the contrary, occasion a tendency to decomposition, a species, in short, of fermentation, in which, of consequence, there is a separation of oxygen, and water remains behind. But what is peculiarly interesting is, that the same effect is produced not only by the action of such bodies as are soluble in this liquid, but also of various solid bodies, some organised and others unorganised; as, for example, by the peroxide of manganese, silver, platina, gold, and even by the fibrine of blood. The substance which produces the decomposition undergoes no alteration; nor does it become an element of the new compound, and therefore it operates by an inherent power, which, though unknown as to its essence, is, nevertheless, demonstrated by its effects.

Shortly before this discovery of M. Thénard, Sir H. Davy had noticed a phenomenon, the connection of which with the preceding was not immediately recognised. He had proved that platinum, heated to a certain extent, and brought into contact with a mixture of the vapour of alcohol, or of ether, and atmospheric air, possessed the power of producing the combination of these bodies, whilst other substances, such as gold and silver, had not this property.

A short time after this, Mr E. Davy found that a preparation of platinum in a state of extreme mechanical division had the power, at ordinary temperatures, and after being moistened with alcohol, of becoming incandescent by the combustion of alcohol, at the same time changing this liquid by oxidation into acetic acid.

After this followed the discovery of Döbereiner, which was the most important of them all. He demonstrated the property which spongy platinum has of setting fire spontaneously to a current of hydrogen gas projected into atmospheric air; a phenomenon which the researches of Thénard and Dulong extended to many other bodies, both simple and compound, but with this restriction, that, whilst platinum, iridium, and some other metals of similar character, acted at temperatures below 32° Fahr., those other bodies, such as gold, and still more silver, required much higher temperatures, and glass a heat of 300° or more.

Thus, this property, which at first was considered as acting in a way that was altogether singular, appeared to be a general property, though acting differently in relation to different bodies; and it became possible to deduce from it certain applications. We now know, for example, that, in the act of fermentation, in the transformation of sugar into alcohol and carbonic acid, the change which is effected by the insoluble substance which is called *ferment* or *yeast*, and which may be replaced, though with less certainty, by animal fibrine, by albumen, by cheesy matters, &c. &c. cannot be explained by any chemical action between sugar and yeast, and that no phenomenon in unorganised bodies approaches it so nearly as the action of platinum, of silver, and of fibrine, in the decomposition of the peroxide of hydrogen into oxygen and water. It was, therefore, only natural to suppose that the mode of acting of yeast was analogous.

The transformation of starch into sugar, by means of sulphuric acid, had not hitherto been arranged and connected with the preceding facts; nevertheless, the discovery of *diastase*, a substance which acts upon starch in a similar manner, but with much greater energy, directed attention to this analogy; and the parallel was completely demonstrated to our satisfaction by the ingenious researches of M. Mitscherlich regarding the formation of ether. Among the many theories respecting the formation of ether, one, as is well known, made the power of the sulphuric acid to transform the alcohol into ether to depend upon its power of combining with water, admitting that the alcohol, considered as a compound of one atom of etherine (C^4H^3) and two atoms of water, was converted into ether, by yielding the half of its water to the acid. This theory, as simple as it was ingenious, was in perfect harmony with our knowledge of the actions of the affinities of bodies; but, notwithstanding, it did not explain why other non-acidulous bodies, as strongly disposed for water, did not also produce ether. The researches of M. Mitscherlich now prove that sulphuric acid, properly diluted, and taken at such a temperature that the refrigeration produced by the addition of the alcohol may compensate for the heat which is produced by the mixture, decomposed the former into ether and water, both of which, owing to the temperature surpassing the boiling point of water, separated themselves by distillation from the mass, and presented, when completely condensed, a mixture of the same weight with that of the alcohol employed. The method of operating in this experiment, as well as the fact of the distillation of the water conjointly with the alcohol, was, it is true, known before M. Mitscherlich, but to him belonged the merit of foreseeing the consequences. In a word, he demonstrated, that at this temperature the sulphuric acid acted upon the alcohol, in virtue of the same power which determines the action of alkalies upon oxygenated water, since the water, separating itself entirely from the mixture, had not obeyed any affinity for the acid; and he hence concluded, that the action of the sulphuric acid and the diastase upon starch, whence resulted sugar, must be of the same nature.

It is proved, therefore, that many substances, simple and compound, solid and in a state of solution, possess the power of exercising upon compound bodies an influence essentially distinct from chemical affinity, an influence which consists in the production of a displacement, and a new arrangement of their elements, without their directly and necessarily participating in it, some special cases only excepted. Assuredly such a power, which is capable of effecting chemical reactions in unorganised substances, as well as in organised bodies, although still too little known to be accurately explained, must play a far more important part throughout nature than we have hitherto been led to suppose. In defining it a new power, I am far from wishing to deny that some connection exists between its influences and the electro-chemical ones, with which we are more familiar; on the contrary, I am very much disposed to recognise in it a peculiar manifestation of these same influences; but notwithstanding, so long as we have not ascertained the real nature of this power, it will be more simple, so far as regards our future researches, to consider it as independent, and to confer upon it, for the facility of comprehension, a particular name. Accordingly, I shall designate it, thereby following a well known chemical etymology, the *catalytic power* of bodies; and the decomposition it produces I shall call *catalysis*, in the same way as we have designated by the term *analysis*, the separation of the elements of a compound, by means of the ordinary chemical affinities. This power seems *definitely* to consist, in a faculty of bodies, by their simple presence, and without any chemical participation, to rouse up the play of certain affinities which at that temperature remained inactive, so as to determine, in consequence of a new arrangement of the elements of the compound, a new state of perfect electro-chemical neutralization. As this agent acts generally in a manner analogous to heat, it may be demanded, if being differently graduated, sometimes by a different mode of using the same catalytic body, sometimes by the introduction of different catalytic bodies in the same liquid, it would produce, as we often see in the action of different temperatures, different catalytic products; and if, on the other hand, the catalytic power of a body can exert itself upon a great number of compound bodies, or whether, as our experiments appear to indicate, only upon certain bodies, to the exception of others? But in the present state of our knowledge it is impossible to decide these questions, as well as many others which might be agitated on the subject; and their solution must be left for future research. It is sufficient, for the present, to have demonstrated the existence of this power by a number of examples; which power, as now explained, sheds a light altogether new upon chemical agency in organized bodies. We shall give only one example: round the eye of the potato we find a portion of *diastase* accumulated, which is totally wanting in the tuber itself, and in the developed germ: in this point we recognise a catalytic centre of action, in which the insoluble starch of the tuber is changed into gum and sugar; and this portion of the potato becomes the secreting organ of those soluble substances, which go to form the juices of the nascent germ. It is not at all likely that the action now mentioned should be the only one of its kind in vegetable life; on the other hand, we may decidedly presume that in vegetables, as well as in the animal body, a thousand catalytic effects take place between the solids and the fluids, whence really result the great number of different chemical compounds, whose production at the expense of the same physical fluid which we call blood, or vegetable juice, is to be explained by no other known cause. —From *Jahrbuch für* 1836.—*Von H. C. Schumacher, Berzelius, Bessel, Gauss, Moser, Olbers, and Paucker. Stuttgart,* 1836.

Account of a Method of separating Small Quantities of Arsenic from Substances with which it may be mixed. By James Marsh, Esq. of the Royal Arsenal, Woolwich. (Communicated to the Society of Arts of London).*

Notwithstanding the improved methods that have of late been invented of detecting the presence of small quantities of arsenic in the food, in the contents of the stomach, and mixed with various other animal and vegetable matters, a process was still wanting for separating it expeditiously and commodiously, and presenting it in a pure unequivocal form for examination by the appropriate tests. Such a process should be capable of detecting arsenic not only in its usual state of white arsenic or arsenious acid, but likewise in that of arsenic acid and of all the compound salts formed by the union of either of these acids with alkaline substances. It ought, also, to exhibit the arsenic in its reguline or metallic state, free from the ambiguity which is sometimes caused by the use of carbonaceous reducing fluxes. It appeared to me, that these objects might be attained by presenting to the arsenic, hydrogen gas in its nascent state: the first action of which would be to deoxygenate the arsenic; and the next, to combine with the arsenic, thus deoxygenated, into the well-known gas called arsenuretted hydrogen. Being thus brought to the gaseous state, the arsenic would spontaneously (so to speak) separate itself from the liquor in which it was before dissolved, and might be collected for examination by means of any common gas apparatus; thus avoiding the trouble, difficulty, and ambiguity of clarification and other processes whereby liquors, suspected of containing arsenic, are prepared for the exhibition of the usual tests, or of evaporation and deflagration, which are sometimes had recourse to in order to separate the arsenic from the organic substances with which it may have been mixed.

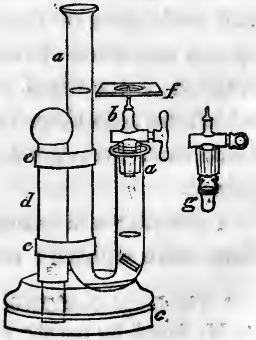
I had the satisfaction of finding, on trial, that my anticipations were realized; and that I was thus able, not only to sepa-

* The Large Gold Medal of the Society of Arts of London, was presented to Mr Marsh for the above valuable communication, which will appear in the 51st vol. of the Society's Transactions.

rate very minute quantities of arsenic from gruel, soup, porter, coffee, and other alimentary liquors, but that, by continuing the process a sufficient length of time, I could eliminate the whole of the arsenic in the state of arsenuretted hydrogen, either pure, or, at most, only mixed with an excess of hydrogen.

If this gas be set fire to as it issues from the end of a jet of fine bore into the common air, the hydrogen, as the more combustible ingredient, will burn first, and will produce aqueous vapour, while the arsenic will be deposited either in the metallic state, or in that of arsenious acid, according as it is exposed partially or freely to the air. The former condition is brought about by holding a piece of cold window-glass opposite to and in contact with the flame, when a thin metallic film will be immediately deposited on its surface; and the latter, by receiving the flame within a glass tube open at both ends, which, in half a minute, will be found to be dimmed by a white pulverulent sublimate of arsenious acid. By directing the flame obliquely upon the inside of the tube, it strikes against the glass and deposits the arsenic, partly in the metallic state. In this case, if the tube, while still warm, be held to the nose, that peculiar odour, somewhat resembling garlic, which is one of the characteristic tests of arsenic, will be perceived. Arsenuretted hydrogen itself has precisely the same colour, but considerable caution should be used in smelling it, as every cubic inch contains about a quarter of a grain of arsenic.

The requisite apparatus is as simple as possible; being a glass tube open at both ends, and about three quarters of an inch in its internal diameter. It is bent into the form of a syphon *aa*, the shorter leg being about five inches, and the longer about eight inches in length. A stop-cock *b*, ending in a jet of fine bore, passes tightly through a hole made in the axis of a soft and sound cork, which fits air-tight into the opening of the lower bend of the tube, and may be further secured, if requisite, by a little common turpentine lute. To fix the apparatus, when in use, in an upright position, a hole is made



in the wooden block *c* for the reception of the lower part of the pillar *d*, and a groove is cut in the top of the same block to receive the bend of the tube *a a*. Two elastic slips *e e*, cut from the neck of a common bottle of India rubber, keep the tube firm in its place.

The matter to be submitted to examination, and supposed to contain arsenic, if not in the fluid state, such as pastry, pudding, or bread, &c., must be boiled with two or three fluid ounces of clean water, for a sufficient length of time.

The mixture so obtained must then be thrown on a filter to separate the more solid parts: thick soup, or the contents of the stomach, may be diluted with water and also filtered; but water-gruel, wine, spirits, or any kind of malt-liquor, and such like, or tea, coffee, cocoa, &c., can be operated on without any previous process.

When the apparatus is to be used, a bit of glass rod about an inch long, is to be dropped into the shorter leg, and this is to be followed by a piece of clean sheet zinc, about an inch and a half long and half an inch wide, bent double, so that it will run down the tube till it is stopped by the piece of glass rod first put in. The stopcock and jet are now to be inserted, and the handle is to be turned so as to leave the cock open. The fluid to be examined, having been previously mixed with from a drachm and a half to three drachms of dilute sulphuric acid (1 acid and 7 water), is to be poured into the long leg, till it stands in the short one about a quarter of an inch below the bottom of the cork. Bubbles of gas will soon be seen to rise from the zinc, which are pure hydrogen, if no arsenic be present; but, if the liquor holds arsenic in any form in solution, the gas will be arsenuretted hydrogen. The first portions are to be allowed to escape, in order that they may carry with them the small quantity of common air left in the apparatus; after which the cock is to be closed, and the gas will be found to accumulate in the shorter leg, driving the fluid up the longer one, till the liquor has descended in the short leg below the piece of zinc, when all further production of gas will cease. There is thus obtained a portion of gas subject to the pressure of a column of fluid of from seven to eight inches high: when, therefore, the stop-cock is opened, the gas will be propelled with some force through the jet, and, on igniting it as it issues (which must be done quickly by an assistant), and then holding horizontally a piece of crown or window-glass *f*, over it, in such a manner as to retard slightly the combustion, the arsenic (if any be present) will be found deposited in the metallic state on the glass; the oxygen of the atmosphere being employed in oxydizing the hydrogen only during the process. If no arsenic be present, then the jet of the flame as it issues has a very different appearance; and, although the glass becomes dulled in the first instance by the deposition of the newly formed water, yet, such is the heat produced, that in a few seconds it becomes perfectly clear, and frequently flies to pieces.

If the object be to obtain the arsenic in the form of arsenious acid, or white arsenic, then a glass tube, from a quarter to half an inch in diameter (or according to the size of the jet of flame), and eight or ten inches in length, is to be held vertically over the burning jet of gas, in such a manner that the gas may undergo perfect combustion, and that the arsenic combined with it may become sufficiently oxydized; the tube will thus, with proper care, become lined with arsenious acid in proportion to the quantity originally contained in the mixture.

When the glass tube is held at an angle of about forty-five degrees over the jet of flame, three very good indications of the presence of arsenic may be obtained at one operation; viz., metallic arsenic will be found deposited in the tube at the part nearest where the flame impinges,—white arsenic or arsenious acid at a short distance from it,—and the garlic smell can be readily detected at either end of the tube in which the experiment has been made.

As the gas produced during the operation is consumed, the acid mixture falls into the short limb of the tube, and is thus again brought into contact with the zinc, in consequence of which a fresh supply is soon obtained. This gas, if submitted to either of the processes before described, will give fresh indications of the presence of the arsenic which the mixture may have originally contained; and it will be easily perceived that the process may be repeated as often as may be required, at the will of the operator, till no further proofs can be obtained.

When certain mixed or compound liquors are operated on in this apparatus, a great quantity of froth is thrown up into the tube, which may cause a little embarrassment by choking the jet. I have found this effect to take place most with the contents of the stomach, with wine, porter, tea, coffee, or soup, and, indeed, with all mucilaginous and albuminous mixtures. The means I adopt to prevent this effect from taking place, or, at least, for checking it in a great measure, is to grease or oil the interior of the short limb of the apparatus before introducing the substance to be examined, or to put a few drops of alcohol or sweet-oil on its surface previously to introducing the stopcock and its appendages. I have, however, found, if the tube be ever so full of froth in the first instance, that, in an hour or two, if left to itself, the bubbles burst, and the interior of the tube becomes clear, without at all affecting the results.

In cases where only a small quantity of the matter to be examined can be obtained, I have found a great convenience in using the small glass bucket *g*. Under such circumstances, the bent glass tube may be filled up to within an inch of the short end with common water, so as to allow room for the glass bucket, which must be attached to the cork, &c. by means of a little platina wire; a bit or two of zinc is to be dropped into the bucket, with a small portion of the matter to be examined, and three or four drops of diluted sulphuric acid (acid 2, water 14); and the whole is then to be introduced into the mouth of the short limb of the tube. The production of gas under this arrangement is much slower, and, of course, requires more time to fill the tube, than in the former case; but the mode of operating is precisely the same. Indeed, it is of great advantage, when the quantity of arsenic present is very minute, not to allow the hydrogen to be evolved too quickly, in order to give it time to take up the arsenic.

A slender glass funnel will be found of service when as much as a table-spoonful, or even a tea-spoonful of matter, can be obtained for examination. In this case, the tube is to be partly filled with common water, leaving a sufficient space for the substance to be examined; a piece of zinc is to be suspended from the cork by a thread or wire, so as to hang in the axis of the tube; and the fluid to be operated on, having previously been mixed with dilute sulphuric acid, is then to be poured through the funnel carefully, so as to surround the zinc, avoiding, as far as possible, to mix it with the water below, and the stopcock and its appendages are to be replaced in the mouth of the tube; the production of the gas then goes on as before stated, and the mode of manipulating with it is exactly the same as described in the foregoing part of this paper.

It will be necessary for me, in this place, to explain the methods I employ after each operation, to determine the integrity of the instrument, so as to satisfy myself that no arsenic remains adhering to the inside of the tube, or to the cork and its appendages, before I employ it for another operation.

After washing the apparatus with clean water, a piece of zinc may be dropped in, and the tube filled to within half an inch of the top of the short limb; two drachms of diluted sulphuric acid are then poured in, and the stopcock and cork secured in its place; hydrogen gas will in this case, as before, be liberated, and fill the tube. If the gas as it issues from the jet be then inflamed, and a piece of window-glass held over it as before described, and any arsenic remains, it will be rendered evident by being deposited on the glass; if so, this operation must be repeated till the glass remains perfectly clean, after having been exposed to the action of the gas.

When I have had an opportunity of working with so large a quantity of mixture as from two to four pints (imperial measure), I then have employed the instrument here figured, which is, indeed, but a slight modification of one of the instantaneous light appearances, now so well known and used for obtaining fire by the aid of a stream of hydrogen gas thrown on spongy platinum. It will, therefore, be of importance only for me to describe the alteration which I make when I employ it for the purpose of detecting arsenic. In the first place, I must observe, that the outer vessel *a*, which I use, holds full four pints, and that the jet of the stopcock is vertical, and its orifice is twice or three times larger than in the instrument as generally made for sale, and also that there is a thread or wire attached to the cork of the stopcock *b*, for suspending a piece of zinc *c*, within the bell-glass.

With an instrument of this description, I have operated on one grain of arsenic in twenty-eight thousand grains of water (or four imperial pints), and have obtained therefrom, upwards of one hundred distinct metallic arsenical crusts.

Similar results have been obtained with perfect success from three pints of very thick soup, the same quantity of port wine, porter, gruel, tea, coffee, &c. &c.

It must, however, be understood, that the process was allowed to proceed but slowly, and that it required several days before the mixture used ceased to give indication of the presence of arsenic, and, also, a much larger portion of zinc and sulphuric acid was employed from time to time, than when working with the small bent tube apparatus, in consequence of the large quantity of matter operated on under this arrangement.

With the small apparatus, I have obtained distinct metallic crusts, when operating on so small a quantity as one drop of Fowler's solution of arsenic, which only contains one-120th part of a grain.

The presence of arsenic in artificial orpiment and realgar, in Scheele's green, and in the sulphuret of antimony, may be readily shewn by this process, when not more than half a grain of any of those compounds is employed.

In conclusion, I beg to remark, that although the instruments I have now finished describing, are the form I prefer to all that I have employed, yet it must be perfectly evident to any one, that many very simple arrangements might be contrived. Indeed, I may say unequivocally, that there is no town or village in which sulphuric acid and zinc can be obtained, but every house would furnish to the ingenious experimentalist ample means for his purpose; for, a two-ounce phial, with a cork and piece of tobacco-pipe, or a bladder, with the same arrangement fixed to its mouth, might, in cases of extreme necessity, be employed with success, as I have repeatedly done for this purpose.

The only ambiguity that can possibly arise in the mode of operating above described, arises from the circumstance, that some samples of the zinc of commerce themselves contain arsenic; and such, when acted on by dilute sulphuric acid, gave out arsenuretted hydrogen. It is, therefore, necessary for the operator to be certain of the purity of the zinc which he employs, and this is easily done by putting a bit of it into the apparatus, with only some dilute sulphuric acid; the gas thus obtained is to be set fire to as it issues from the jet; and if no metallic film is deposited on the bit of flat glass, and no white sublimate within the open tube, the zinc may be regarded as in a fit state for use.

Mean Temperature of Montreal, Lower Canada, for the period of Ten Years, viz. from 1826 *to* 1835 *inclusive.* By ARCHIBALD HALL, M.D. In a Letter to the Secretary of the Wernerian Natural History Society.

MONTREAL, LOWER CANADA,
February 8. 1836.

SIR,

I HAVE taken the liberty of transmitting to you, through the hands of Alexander Shakel, Esq. A. M. of this city, the following statement, manifesting the mean temperature of the city of Montreal, and which, I conceive, might prove an addition to other data in your Society's possession of a similar nature.

The mean temperature, as given below, may be relied on as substantially correct, being deduced from the meteorological tables of the afore-mentioned gentleman, who has been in the habit of making daily observations for the last fifteen or twenty years; and moreover, his thermometer, having never during that time been removed from the station it at present occupies, indicates a temperature upon which the same modifying powers have always exerted a corresponding degree of influence.

The means of the months are deduced from the series of two daily observations, the one at 7 o'clock A. M., when it may be assumed that the temperature is at *minimum*, the other at 3 o'clock P. M., when it may be stated to be at *maximum*.

I have also appended the mean temperature of the *seasons*, and the hottest and coldest days in each year, with the temperature of each, as recorded in the journal. With respect to the correctness of the former, it may be urged in opposition, that they have not the same tri-monthly duration here as in other latitudes; our spring and autumn, *e.g.* seldom exceeding six weeks or two months. My sole object in allotting to each their quarterly duration was to facilitate a comparative examination of the same periods of the year in other climates.

MEAN TEMPERATURE FOR THE MONTHS OF THE YEARS OF

Months.	1826.	1827.	1828.	1829.	1830.	1831.	1832.	1833.	1834.	1835.
January,....	17°.5	12°.4	17°.8	13°.5	12°.0	13°.8	16°.0	18°.7	11°.3	17°.1
February,...	21.4	19.3	26.9	14.2	17.7	20.9	16.2	14.9	27.0	13.7
March,......	28.9	32.0	33.6	31.0	32.6	36.0	30.1	27.0	29.4	29.2
April,........	42.9	46.2	44.6	46.2	52.9	47.2	41.9	47.0	49.1	40.2
May,.........	65.4	57.4	63.0	64.5	59.6	62.5	58.1	61.8	56.8	55.8
June,.........	72.3	69.2	76.2	68.9	67.2	75.0	68.1	64.8	65.3	65.5
July, ...	76.9	73.0	73.9	71.9	75.1	74.9	70.7	72.2	76.3	70.8
August,......	73.8	69.0	76.3	71.8	73.1	73.1	71.6	67.6	69.6	67.8
September,..	63.0	63.0	62.9	57.0	60.8	61.2	63.1	61.1	62.7	56.7
October,.....	49.6	47.4	46.3	50.3	53.7	50.5	49.2	45.3	45.3	49.0
November,..	33.9	28.6	28.5	34.5	41.6	37.1	33.3	33.5	34.5	38.8
December,..	20.0	19.2	18.6	28.8	27.1	9.6	18.6	24.8	13.8	10.8

MEAN TEMPERATURE OF THE SEASONS.

	Years.									
	1826.	1827.	1828.	1829.	1830.	1831.	1832.	1833.	1834.	1835.
Of Spring, beginning Mar. 20, ending June 20, inclusive,	55°.2	54°.1	55°.8	56°.3	56°.2	58°.3	51°.5	54°.0	52°.2	49°.8
Of Summer, beginning June 21, ending Sept. 21, inclusive,	78.5	68.6	73.4	67.9	70.8	70.7	69.6	68.0	77.4	66.8
Of Autumn, beginning Sept. 22, ending Dec. 20, inclusive,	40.1	36.1	37.8	40.3	44.3	37.7	38.8	38.3	36.4	33.3
Of Winter, beginning Dec. 21, ending Mar. 19, inclusive,	1826-7 17.3	1827-8 23.0	1828-9 15.7	1829-30 22.1	1830-1 21.9	1831-2 17.6	1832-3 17.0	1833-4 22.2	1834-5 17.1	...

MEAN TEMPERATURE OF THE YEARS

1826, ...	47°.1	1831, ...	46.8
1827, ...	44.7	1832, ...	44.7
1828, ...	47.3	1833, ...	44.8
1829, ...	46.0	1834, ...	45.0
1830, ...	47.8	1835, ...	42.0

Mean Temperature of the City of Montreal, in Lat. 45° 31′ N., Long. 73° 35′ W., ... 45°.7

MAXIMUM AND MINIMUM TEMPERATURE IN EACH YEAR.

1826,	Max.	July 12,	96 +
	Min.	Feb. 1,	28 —
1827,	Max.	July 8,	86 +
		... 11,	86 +
	Min.	Feb. 12,	20 —
1828,	Max.	June 27,	98 +
	Min.	Dec. 20,	20 —
1829,	Max.	June 6,	94 +
		July 11,	94 +
	Min.	Jan. 4,	23 —
1830,	Max.	July 17,	93 +
	Min.	Jan. 31,	20 —
1831,	Max.	June 1,	97 +
	Min.	Dec. 22,	17 —
1832,	Max.	July 2,	89° +
		... 5,	89 +
		... 8,	89 +
		Aug. 31,	89 +
	Min.	Jan. 29,	17 —
		Feb. 25,	17 —
1833,	Max.	June 23,	90 +
		Aug. 21,	90 —
	Min.	Jan. 19,	25 +
1834,	Max.	July 25,	96 +
	Min.	Jan. 25,	16 —
1835,	Max.	Aug. 12,	98 +
	Min.	Dec. 17,	25 —

A few remarks, and I shall conclude.

By a reference to the means of the years, the fact will be apparent, that a gradual decrease of temperature has marked them, as they have successively passed away, a circumstance not very consonant with the almost universally received opinion, that countries become gradually warmer in the ratio of their cultivation, population, &c.

The last year (1835) holds a conspicuous place in point of lowness of temperature, its mean being 2°.8 lower than the mean of the means of the ten years. It may also be remarked, that 1830 stands in a diametrically opposite point of view, its mean being 2°.1 higher.

With respect to the relative means of the months, it is not unworthy of notice, that December 1835 was only excelled in point of frigidity by the same month of 1831, the difference between the two being only 1°.2. In like manner do July 1834 and August 1828 stand pre-eminent for heat.

Perhaps few other climates possess the same range of temperature as the one peculiar to this country; in proof of which it is only necessary to refer to the maximum and minimum temperatures in each year. The highest temperature recorded in the journal is 98° +, which only occurred twice during the period of the ten years. The lowest temperature to be met with occurred only once, in February 1. 1836, when the thermometer indicated 28° —. By deducing the means of the maximum heat and cold, as shewn above, we shall have 92°.8 + as the mean of the former, and 21°.4 — as the mean of the latter, thus indicating a thermometrical range of 114°.2. I have the honour to remain, yours, &c.

ARCHIBALD HALL, M. D.

Second Report of the Meteorological Committee of the South African Literary and Scientific Institution.—Read 11*th July* 1835.

THE Meteorological Committee having proceeded to draw up and circulate a compendious body of instructions for making and registering Meteorological Observations—the same which forms a part of their first Report to this Institution—and having, moreover, distributed in various quarters copies of the printed forms alluded to in p. 16 of that Report—have received in consequence communications from various parts of this colony, in most instances expressing great willingness to co-operate in the observations recommended, but in almost every case complaining of the want of meteorological instruments, and in some, requesting a supply. Your Committee are not without hopes of being enabled in some instances to supply the deficiency. Meanwhile they have to acknowledge the receipt of a regular return, according to their printed form, from Captain Wolfe, Commandant of Robben Island, of the state of the Barometer, interior and exterior Thermometer, Wind, and Weather, at the hours agreed upon, during the whole of January, February, March, and April, of the present year, with the promise of their future regular continuance. In this communication the observations appear to have been made with such regularity, and the instructions of the Committee generally so well attended to, as leads them to regret that the barometer employed should (as appears by the numbers set down) be one capable of being read only to the nearest tenth of an inch, and to render them very desirous to supply a better. A spare barometer belonging to the Royal Observatory has been accordingly placed at their disposal by the Astronomer Royal, and so soon as it shall be furnished with a new tube, and otherwise repaired, will be forwarded to Captain Wolfe, with a request that his series of observations may be continued with this instrument, instead of that at present used;—Robben Island being in many respects a highly advantageous station for acquiring an insight into the Meteorology of this point of the coast, much more so than Cape Town itself.

From Worcester, your Committee have received a register of the Thermometer only (having no Barometer), from P. J. Truter, Esq. Civil Commissioner for the district, for the month of January of the present year. Having only one Thermometer, which is used both for ascertaining the interior temperature and that of the outer air, the Committee would recommend that he should be supplied with at least one other, and be requested, until a barometer can be procured, to fill up the column of the in-door Thermometer with observations of the hygrometric state of the air, as ascertained by the depression of temperature produced by wrapping the bulb in wet linen or cotton, and suspending it freely, in the manner recommended in p. 12 of their Instructions.

The Committee have also received from the Astronomer Royal, and from Sir J. Herschel, hourly Observations at the Solstices of December 1834 and June 1835, and the Equinox of March 1835, made according to the plan proposed in their printed Instructions. The comparison of these observations has shewn, that, in this locality at least, even at stations so near together as Feldhausen and the Royal Observatory, the fluctuations of atmospheric pressure are very far from nicely corresponding, and that, so long as any wind subsists in a mountainous district, the atmospheric strata can by no means be regarded as horizontal. The calm, however, having been complete and uninterrupted for ten successive hours on the night of the 22d ult., afforded an excellent opportunity for determining the difference of level of the two stations, which appears to be 129 feet 8 inches, subject to a trifling correction for the zero points of the barometer, which remains to be more exactly ascertained.

Communications have been received by the Committee from Sir E. Ryan, Chief-Justice of Calcutta, containing a Register of the Barometer and Thermometer, kept by himself during his passage from Table Bay to Calcutta, in the months of December, January, and February 1834–5; from —— McHardy, Esq. Surgeon on board the Mountstuart Elphinstone, containing a similar register made in the voyage of that ship from Table Bay to London, during parts of the months of September and October 1834; from Captain Wauchope of H.M.S. Thalia, containing extracts from a Journal of the Barometer and Thermometer, &c. observed on board of H.M.S. Eurydice, off Saldanha Bay, during a heavy gale in 1819, as also in Table Bay during a violent north-wester in 1817; and, lastly, from H. W. Innes, Esq. Surgeon on board the Sherburne, containing a similar register kept during the approach to and after the arrival of that ship in Table Bay in January 1835.

Of the two former of these communications (those of Sir E. Ryan and of Mr McHardy), it must be observed, that they both, but especially the first, afford strong corroborative, and indeed quite decisive, evidence of that important meteorological fact, of a considerable depression of the barometer in approaching to the equator from extra-tropical latitudes. Sir E. Ryan's barometer, previous to his sailing, was compared, through the medium of a portable barometer in possession of Sir J. Herschel, with the Mural Circle Barometer of the Cape Observatory, the difference of which, from the standard of the Royal Society, had been previously ascertained by two distinct comparisons, agreeing perfectly *inter se*, made by the intervention of the above-mentioned portable barometer, which had been brought to the Cape by Mr Henderson, and again transported by him to London. By these comparisons, it was found that Sir E. Ryan's barometer required a correction of —0.116 in. to reduce it to the Royal Society's standard. This correction being applied, and the reading so corrected being reduced to the freezing temperature, and classed into groups in zones of 10° in breadth, proceeding northwards and southwards from the equatorial zone (between the latitudes 5° N. and 5° S.) according to the observed latitudes of the ship at noon of each day, give as follows:—

Limits of the Zone of Latitude.	Number of Days' Observations.	Mean Pressure observed in inches.	Mean corresponding observed Latitude.
Equatorial Zone.			
Lat. 5° N. to 5° S.	7	29.821	0° 41′
... 5 ... to 15 ...	10	29.849	9 50
... 15 ... to 25 ...	8	30.030	19 12
... 25 ... to 35 ...	10	30.125	31 0
... 35 ... to 40 ...	24	29.934	38 25

The observations of Mr McHardy, though extending only to latitudes south of the equator, and, though evidently made with far less care, and with an instrument in which the fluctuations arising from the motion of the ship are very imperfectly destroyed, yet, when reduced and grouped in a similar manner, afford a result agreeing in their general tenor very satisfactorily with those of Sir E. Ryan. To render them comparable, as the zero of Mr McHardy's barometer is unknown, a correction of — 0.188 has been applied to all his reduced observations, by which the equatorial indications of the two barometers are made to agree, and the following Table exhibits their results when so reduced, grouped, and corrected:—

Limits of the Zone of Latitude.	Number of Days' Observations.	Mean Pressure in inches.	Mean corresponding Latitude.
Lat. 0° N. to 5° S.	8	29.821	1° 42′
... 5 ... to 15 ...	5	29.802	9 20
... 15 ... to 25 ...	6	29.960	19 41
... 25 ... to 35 ...	16	30.085	31 20

The total depression concluded from the latter series of observations agrees very nearly in amount with that stated by Sir J. Herschel, as the result of his own observations during his voyage from England. The general fact may now therefore be looked upon as unequivocally established, and it is hoped that it will henceforth attract the attention of all voyagers; and that observations will be diligently accumulated for the purpose of ascertaining the law of variation of atmospheric pressure in all latitudes both within and beyond the tropics, and in either hemisphere, since it is very possible that the same exact law may not be found to apply to both, and that the Atlantic, Indian, and Pacific Oceans may offer differences depending on their different extent and relation to the continents adjacent to them.

If, in a report like this, it be allowed to speculate on the causes of meteorological phenomena, it appears extremely probable that the equatorial depression in question arises from the same cause which produces the trade winds, viz. the rarefaction and consequent ascent of the equatorial air, which, although constantly supplied from the extra-tropical latitudes, is yet not supplied *instanter*, nor without a due dynamical motive force, which, in a free elastic fluid, *can* be no other than an excess of pressure on the side from which the supply is drawn, or (which comes to the same thing) a diminution of it, in the nature of a "suction," on that side towards which the superficial currents rush; which excess and diminution obviously arise from the overflow of the unsustained portion of atmosphere above the equatorial zone into the regions beyond. The inquiry, therefore, connecting itself as it does, with all the greater phenomena of meteorology, assumes a high degree of interest, and will no doubt be studied with the perseverance and exactness it merits.

A series of observations of the heights and times of high and low water at Simon's Bay, extending from January 26. to June 30, has been obligingly submitted to the consideration of the Meteorological Committee, by J. Deas Thomson, Esq. and the Astronomer-Royal. It has not yet been possible to compare them with any theory, and indeed it would be premature to attempt it here, as they will require to be combined with the mass of knowledge now accumulating on this subject in Europe, to render them in any degree available. One remarkable result, however, may be mentioned here, which offers itself on a very cursory inspection of the heights, as compared with the declinations of the Sun and Moon, viz. that while the monthly fluctuation of the mean sea-level, arising from the *moon's* alternate occupation of the northern and southern hemisphere, is scarcely perceptible, amounting hardly to two inches, its annual variation, due to the similar approach of the sun to the northern and southern solstice, is much more considerable, and forms indeed a prominent feature in the Tides of this coast, amounting to no less than eight inches, or nearly a fifth of the average difference between high and low water—as the following brief Table will shew—in which the interval embraced by the observations is divided, not as usual into lunations from full to full or from new to new Moon, but into periods marked by the moon's passing from south to north of the equinoctial. By this division the effect (if any) of the moon's change of declination compensates itself, and leaves the solar effect in evidence. The cause of the prominence thus given to this part of the sun's agency, appears to lie in the length of its period compared with the moon's, which gives time for the waters of the whole ocean to accommodate their general level to the actual force, by bodily transfer from one part of the globe to another, and by assuming, at each instant (what the tides of short period have never time to do), very nearly the figure of equilibrium due to this particular modification of the disturbing forces.

Observed Mean Positions of the Mid-water mark on the Float of the Tide-gauge at Simon's Bay, during successive intervals of the Moon's Transit from North to South of the Equinoctial.

Limits of intervals.			No. of Tides observed.	Heights of Mid-water on the Gauge.	Remarks.
Jan. 26	to	Feb. 16,	22	4 feet 4.38 in.	period incomplete.
Feb. 17	to	Mar. 15,	27	4 ... 2.35 ...	
Mar. 16	to	April 11,	27	3 ... 10.83 ...	
April 12	to	May 9,	28	4 ... 0.22 ...	
May 10	to	June 5,	27	3 ... 9.52 ...	
June 6	to	June 30,	25	3 ... 8.37 ...	period incomplete.

At the meetings of the Institution of Wednesday September 3, and October 1, Sir J. Herschel stated that he had examined the Meteorological Journal kept at the Port-Office by Mr M'Cleod, under the direction and superintendence of Captain Bance, during 58 months, commencing with October 15. 1828, in which are registered the heights of the Barometer with the temperature of the instrument, for the hour of 9 A.M., noon, and 3 P.M., with the usual notices of wind and weather, and that having reduced and interpolated them by graphical projection, he had been led to the following conclusions:—

1st, That the atmospheric pressure at Cape Town is subject to a considerable and very regular annual fluctuation, amounting (when reduced to a temperature of 32° Fahr.) to 0.287 in.

—the highest level being attained about the 16th of July, and the lowest about the 16th of January, on an average of five years.

2d, That the barometric pressure is also subject to a regular diurnal fluctuation, whose average amount, on a mean of the whole year, may be stated at 0.027 in.; the highest pressure taking place at or about 9 A. M., and the lowest (so far as can be gathered from observations made only at the hours above named) at 3 P. M.

3d, That this daily oscillation is itself subject to an annual alternate increase and diminution—the limits being 0.0198 in. and 0.0322—the former, or lesser diurnal variation, corresponding to the middle of January, and the latter or greater to the beginning of July.

4th, That these fluctuations are maintained with such regularity, that there is not a single month in the fifty-eight examined, in the mean of which the daily oscillation does not appear; and that in the annual oscillation (with exception of one remarkable anomaly, produced by the tremendous storm of July 1831) not only does every year exhibit the fluctuations in question, but its progress is marked by similar stages, or phases of increase and diminution; the most remarkable of which is a temporary suspension of the regular rapid rise of the mercury towards its maximum, usually taking place about the latter end of May or beginning of June.

5th, That, contrary to usually received notions, the rainy season at the Cape corresponds to a *generally* elevated state of the barometer, although it is true that particular storms of wind and rain are often marked by a temporary depression.

Sir J. Herschel further observed, that the amount of the annual barometric variation at the Cape corresponds pretty nearly with the amount of a depression of the mercury, which he stated to have been observed by himself in his voyage hither, at and near the equator, below its habitual state in the extra-tropical regions—a depression then noticed, as he at that time supposed for the first time, but which it appears had also been (very recently) noticed and made the subject of inquiry and numerical computation by Professor Schow of Copenhagen, in a paper published in the Annales de Chimie for June 1833.

Sir J. Herschel also farther stated, that the mean annual barometric fluctuation at Calcutta, on the average of between two and three years' observations made by Mr Prinsep, examined by him, appears to be much greater than that at the Cape, and, what is very remarkable, in a contrary direction, the maximum of Calcutta corresponding to the minimum at the Cape. And he attributes this to an actual bodily transfer of a portion of air from hemisphere to hemisphere, by the alternate heating and cooling of the two hemispheres, as the sun crosses from side to side of the equator. The effect of this cause, which he considers to be general over the whole earth, will be to modify the regular and constant effects of the Trades, by a set of periodical winds differing materially in their character from local monsoons, and to this cause he is disposed to attribute the observed annual oscillation of the extreme north and south limits of the Trade winds.

The northern hemisphere, he further observed, being, by reason of its greater quantity of land, more superficially heated than the southern, it should be expected that the mean pressure beyond the southern tropic should exceed that beyond the northern, and he suggested this as a subject worthy of examination by meteorologists properly situated in both hemispheres.

Lastly, he observed, that severe gales, occurring whether in summer or winter, appear to depend on causes entirely extraneous to the regular periodical fluctuations of pressure, and are probably dependent on causes of a local and transient nature—but that *a correspondence of extraordinary seasons* in distant parts of the globe, may be expected to accompany great occasional deviations from the usual law of these fluctuations in any given place, and that it is far from impossible that an assiduous attention to this point may ultimately enable us to predict their occurrence.

The series of observations at the Port-Office being still in progress, the foregoing results are not considered as final; but whatever modifications future years' observations may necessitate, will be from time to time inquired into and reported.

Note, by M. Alphonse de Candolle, *concerning M. Marcel de Serres's Essay upon the Question, Whether the Examination made, in the Coal formation of Canada and Baffin's Bay, of Plants analogous to those which now flourish in Equatorial Regions, proves a Change in the Inclination of the Ecliptic.**

Having become acquainted with the memoir of M. Marcel de Serres, when it was passing through the press, I take the liberty of offering some reflections which suggested themselves during its perusal.

The author commences by attributing to me the theory, more or less ingenious, which he combats; but if I have had the humble merit of making it known to Frenchmen, I must here repeat, what I formerly distinctly announced, that I received it from Messrs Hutton and Lindley, the conductors of "*The Fossil Flora of England.*"

As to the gist of the question, I must observe that M. Marcel de Serres has denied two laws in physiological botany, in which there are now but few doubts; and which, if opposed, at least merit to be combated with new facts. The one of them is the permanency of species; that is to say, the hereditary nature of forms, in which there is an accurate resemblance of individual plants to those from which they have been produced, whatsoever their external circumstances may be: and the other is, that those plants which may be referred to the same genera or families can live only under analogous conditions, as to soil, heat, light, and humidity. Let any one descend into a deep mine, and he will there find some of the inferior ferns and lycopodineæ only: or let him reflect upon the important action of light on the respiratory and exhaling functions of vegetables, and it will then be scarcely possible for him to believe that the plants which are organized, as are those of our equatorial regions,—plants which do not cast their leaves, and which open their stomates, under the direct influence of the sun, twelve hours out of the twenty-four, can support a darkness of several months'

* Marcel de Serres's paper is inserted in vol. xix. p. 64 of the Edin. New Phil. Jour.

continuance! Well, then, either the arctic plants of the ancient world were organized in a manner different from our present equatorial plants, or they were subjected to similar physical conditions. They could not have existed without one or other of these alternatives. Their fate would have been the same as that which is experienced now-a-days by a plant of a warm climate when it is exposed to cold, to prolonged darkness, or to excessive humidity: it dies, and revives no more. But the former alternative is false, for observation has demonstrated the close analogy of these ancient species with our present equatorial plants. There remains, therefore, only the second alternative, that they were really subjected to similar conditions as it regards heat, light, &c. So far as heat is concerned, the similarity of the ancient polar regions is not disputed, because every one admits the necessity of a certain temperature for analogous plants; but is it not necessary to make the same concession respecting light, which is of as much consequence to vegetables as temperature itself?

I leave it to the natural philosopher to reconcile the phenomena which I think we must necessarily admit, with the known laws of our sphere; namely, that a more uniform and stronger light was at one time shed over the polar regions. I do not mean now to contend for the hypothesis of a change in the inclination of the terrestrial axis, but only for the fact that light was then otherwise distributed. Perhaps it may one day be discovered, that terrestrial magnetism and the high temperature of the globe produced formerly a light which now is unknown; or possibly it may be discovered that there was a time when the auroræ boreales were much more frequent and intense than they at present are; but this is nothing more than mere hypothesis. What, however, appears to be an incontrovertible fact is this, that the fossil vegetables of Baffin's Bay were formerly under the influence of a different light than are those which vegetate in that region at the present day.

Inquiry in relation to the alleged influence of Colour on the Radiation of Non-luminous Heat. By A. D. BACHE, Professor of Natural Philosophy and Chemistry, University of Pennsylvania. *

IN the following essay I propose to submit a few remarks on a paper by Dr Stark of Edinburgh, first published in the Transactions of the Royal Society of London for 1833, together with an experimental inquiry into the alleged influence of colour on the radiation and absorption of *non-luminous* heat.

The experiments were commenced soon after the paper referred to reached this country, and in them was adopted what seemed to me the less exceptionable of two methods used by Dr Stark, which actually bear upon the question of the radiation of non-luminous heat. It was my intention to examine the matter more fully than had been done by Dr Stark, and to procure a more satisfactory induction by experimenting on a considerable variety of substances. In this I had the kind assistance of my colleague, Professor Courtenay.

While these experiments were in progress, the remarks of the Rev. Professor Powell of Oxford on the paper of Dr Stark appeared in the Edinburgh New Philosophical Journal. They confirmed me entirely in the view of the inapplicability of most of the experiments made by Dr Stark, to the determination of the question of the influence of colour on the radiation or the absorption of heat. Of this class were the absorption of heat, *radiant* heat being understood, as tested by the inverse of Count Rumford's method for comparing the conducting powers of substances used for clothing; also, as tested by the effect of heat from the *flame* of an Argand gas-burner, thrown by a mirror upon the bulb of an air-thermometer which was variously coated. Of the same class were the experiments on radiation, as tested by the method used by Count Rumford, as above referred to, the enveloping materials of the inner thermometer being wools of different colours, and coloured wheaten paste.

Not included in this class are the methods of ascertaining the rate of cooling of a thermometer, of which the bulb was coated

* Silliman's Journal of Science, vol. xxx. p. 16.

with different pigments, and of a glass globe filled with warm water and variously coated. I gave the preference to a modification of this latter method, from the greater extent of radiating surface which may, without inconvenience, be commanded by it. The glass globe used by Dr Stark was one inch and a quarter in diameter; it was coated at different times with Prussian blue, red lead, and white lead, and in a room at 50° Fahrenheit, the fall from 120° through 25° was in seventeen minutes, eighteen minutes, and nineteen minutes.

I am constrained to differ from Professor Powell in his remarks upon the method just referred to, and, with great deference to so high authority, would state why I consider them inconclusive. Professor Powell deems it necessary, or at least highly important, to the determination of the question, that the radiating coatings of the globe should be equalized in respect to thickness, conducting power, density, &c.; and refers to the experiments of Professor Leslie, in which equal quantities of different radiating substances were dissolved and spread upon a surface for comparison. That equal thickness of substances possessing different radiating powers should be compared together, seems to me to be disproved by the law established by Sir John Leslie's own experiments, namely, that radiation takes place not only from the surface, but in a thickness which is appreciable in good radiators. Thus when the different coatings of jelly were applied in succession upon one of the sides of the cube in Professor Leslie's experiments, the radiation increased with the thickness up to a certain point. The effect of conducting power appears by this same experiment to be so small that an increase of the thickness in the bad conductor was actually more than compensated by the increased radiating power. The influence of density on conducting power is well known, but the effect of either, as controlling the radiating power of a substance, or as modifying it, is, I apprehend, yet to be appreciated. If these views be correct, and they are, I believe, founded upon the authorities so ably illustrated by Professor Powell in his report on radiant heat to the British Association, the radiating powers of substances would not be rightly compared by equalizing their thickness upon a given surface, nor by equalizing their weights, but by ascertaining for each substance that thickness beyond which radiation does not take place. This will be placed in a clearer point of view in the sequel.

I do not, however, consider the question at issue as the less difficult to determine, "no substance can be made to assume different colours without at the same time changing its internal structure;" * and I believe, with Professor Powell, that "a very extensive induction is perhaps the only means open to us of ascertaining this (the circumstances and properties wherein the coatings differ), considering how totally ignorant we are of the peculiarities on which their colour depends." This *very* extensive induction I do not pretend to have made, but I have multiplied our experiments so much beyond the number made by Dr Stark as to be able to shew that the supposed influence of colour on the absorption and radiation of heat remains yet to be demonstrated, and thus to prevent the admission, as proved, of what is more than doubtful. The principal object was to select a considerable variety of pigments of the same colour, differing chemically, and of different colours chemically allied, and, as subsidiary, to ascertain the effect of changes of colour produced by chemical means on different substances, and the effect of the material used to apply the pigment to the radiating body. Several tin cylinders were procured, two inches high, and one and a half in diameter, closed at the bottom, and having fitted to the top a slightly conical tube to receive a perforated cork, through which to pass the stem of a thermometer. One of these vessels having been selected, was coated in successive layers with a pigment. Water which was boiling in a porcelain capsule was then poured into the cylinder, which was suspended by means of two lateral hooks to cords attached to the canopy covering the lecture table. A thermometer introduced through a cork had its bulb nearly in the middle of the axis of the cylinder, and the thermometer, by displacing part of the water, proved that the quantity contained was the same in each case. A temperature was selected for beginning the experiments sufficiently below that which the introduction of boiling water produced, to permit the rate of cooling to have become uniform; and one for

* Professor Leslie's Essay on Heat.

ending, which was high enough to prevent uncertainty from the slowness of the fall of temperature. The instant of the arrival of the mercurial column at any degree on the scale, and of its leaving the same, was noted, and a mean taken for the time of being at that temperature; a precaution which, though superfluous in such experiments as these, will, I am persuaded, be found of importance where minute accuracy is desired in investigating the motion of heat. One of us observed the thermometer, the other noted the time by a pocket chronometer.

The time of cooling of the cylinder coated with colouring matter having been ascertained, an additional layer of the same substance was put upon it, and the cooling again observed. The time of cooling diminished of course until that thickness was attained beneath which no radiation takes place, the time then slowly increased with each additional coat, the conducting power entering as an appreciable element into the rate of cooling. To shew the decided nature of the results, I subjoin an account of one series towards the beginning of our experiments, when a want of experience rendered us cautious in applying the successive coatings, lest we should pass the thickness of determinate radiation. The necessity for thus feeling our way rendered the labour of the experiment very considerable.

Cylinder coated with Prussian Blue.

Time of cooling from 180° to 140° Fah.

1. Thick coating,	1011½ seconds.
2. Do. added,	965
3. Additional coat, do.,	910¾
4. Do. do.,	829½
5. Do. do.,	805
6. Do. do.,	842

Another series in a further advanced stage of our experiments is subjoined:—

Cylinder coated with Litmus Blue.

Time of cooling from 180° to 140° Fah.

1. First thick coating,	985 seconds.
2. Additional coat,	855
3. Do. do.,	827½
4. Do. do.,	834½

Besides the necessity of making several experiments to obtain a single result, it sometimes occurred that particular results required to be repeated for verification when apparent discrepancies occurred; this was done to ascertain if they were real or not.

As it was obvious that the experiments must necessarily extend through a considerable time, during which the circumstances attending the cooling of the cylinders could not be expected to remain uniform, a standard for comparison was provided, and a cylinder of which the coating was not changed, and which was observed in regular turn with the other cylinders. At first a vessel without coating was used for this purpose, but as it was found liable to tarnish, a cylinder was substituted having a coating of aurum musivum, which was one of the smoothest and most uniform of the coloured substances used. The number obtained on the different days from a mean of the trials made of the cooling of the standard cylinder were applied to compare the results of one day with those of another. This assumes that the times of cooling of the different vessels would be affected proportionally by a given change in the circumstances of the experiment. This inability to preserve the circumstances constant is the real objection to this method, and one which most affects the certainty of the results.*

The following example shews the application of this method. The observed times of cooling of the standard cylinder, from 180° to 140° in two experiments on the 31st of October, were 969½ and 968½ seconds, mean 969. Three experiments on the 1st of November gave 898, 892, 893½ seconds, mean 894½. Cylinder No. 4, coated with cochineal (crimson), gave for the time of cooling from 180° to 140° on the 1st of November, 848½. To compare this with a result obtained with the same cylinder on the 31st of October, we have $894\frac{1}{2} : 969 :: 848\frac{1}{2} : x$, the equivalent number for October 31st, 916.3 seconds.

The results obtained with the same cylinder on different occasions of experiment having been thus rendered comparable, the comparison of experiments with different cylinders was ef-

* If the circumstances could be retained the same, three observations of the temperature at equal known intervals would give a numerical expression for the radiating power of the coating.

fected by determining the time of cooling with the same coating upon different cylinders. Thus, Nos. 1 and 2 having been coated with carbonate of lead, and their times of cooling through forty degrees having been ascertained, all the results with the various other coatings applied to these cylinders were comparable. The numbers thus obtained will not be strictly proportional to the radiating power of the substance used, for the whole surface of the cylinders, including the ends, was not coated, and the contact of the air and its consequent circulation exert a most important influence on the rate of cooling. This latter element has been shewn by the experiments of Petit and Dulong to be independent of the nature of the surface, and as the amount of uncoated surface remains constant, the greater effect of radiation will appear by the more rapid rate of cooling, and the less by the less rapid rate.

I proceed now to examine the degree of approximation which may be expected from the results of the experiments.

First, A comparison of different observations on the same day, under the same circumstances of the cylinders, and nearly or quite the same as to the temperature of the room, will shew how far accuracy is possible under the most favourable suppositions. The following table presents the results of this kind obtained during the entire series of experiments, with the ratios of the time of cooling :—

In the following table, ten of the ratios are about 1.01 to 1, six 1.02 to 1, three 1.03 to 1, one 1.04 to 1, and two 1.05 to 1. It is therefore fair to infer, that the single ratio of 1.14 to 1 results from an error of record or observation, and the table fully shews, that *under the same circumstances the results could readily be reproduced within about two per cent.*

Nature of Coating.	Time in Seconds.	Ratio.
CYLINDER, No. 3.		
No coating.	1281¾ 1300	1.000 1.014
Chalk.	909¼ 939¼	1.000 1.034
Prussian blue.	909½ 932½	1.000 1.025
Litmus blue.	920½ 956	1.000 1.038
CYLINDER, No. 5.		
Aurum Musivum.	892 893½ 898	1.000 1.001 1.007
Ditto on another occasion.	937½ 959	1.000 1.023
Do.	943½ 957	1.000 1.014
Do.	818 820½	1.000 1003
	850 860 897	1.000 1.012 1.055
Do.	851 872½	1.000 1.025

Nature of Coating.	Time in Seconds.	Ratio.
CYLINDER, No. 1.		
Sulphuret of antimony.	849½ 972¾	1.000 1.145
Ditto additional coating on another occasion.	871½ 878½	1.000 1.008
Red lead.	886½ 894½	1.000 1.009
Do. blackened by sulphuretted hydrogen.	911½ 924½	1.000 1.014
CYLINDER, No. 4.		
Gamboge.	932 942½	1.000 1.011
Chromate of lead.	938½ 954½	1.000 1.017
Vermilion.	845 850	1.000 1.006
Sulphate of baryta.	740½ 778	1.000 1.051
CYLINDER, No. 1.		
No coating.	1396½ 1425½ 1445½	1.000 1.020 1.035
Do. another occasion.	1313½ 1315½	1.000 1.002
Do.	1303 1320	1.000 1.013

Date.	No. Cylinder.	Nature of Coating.	Reduced time of cooling.	Ratio.	No. Cylinder.	Nature of Coating.	Reduced time of cooling.	Ratio.	No. Cylinder.	Nature of Coating.	Reduced time of cooling.	Ratio.
Oct. 21.	II.	Not coated.	1406	1.00	III.	Prussian blue.	914	1.00				
24.			1422½	1.00			953¼	1.04				
24.		Do.	1422½	1.06		Do.	953¼	1.05				
25.			1314½	1.00			910¾	1.00				
28.	I.	Ammoniacal sulphate of cop-	853½	1.01	V.	No coating.	1342	1.02				
29.		per,	849½	1.00			1311½	1.00	V.	Litmus blue.	855	1.00
31.			862	1.01			1359½	1.04			827	0.97
31.	II.	Ammoniacal sulphate of cop-	930	1.13	IV.	Cochineal.	877½	1.03	V.	Aurum Musivum.	968½	1.08
Nov. 1.		per (not the same as above).	826½	1.00			848½	1.00			894½	1.00
1.	II.	Ammoniacal sulphate of cop-	808½	1.00	IV.	Chromate of lead.	907	1.00	V.	Aurum Musivum.	894½	1.00
6.		per (not the same as above).	831½	1.03			944¼	1.04			948½	1.06
6.	I.	Red lead.	890½	1.00	IV.	Alkanet.	980½	1.00	V.	Aurum Musivum.	948¼	1.00
11.			912¼	1.02			926¾	0.95			950¼	1.00
15.	V.	Aurum Musivum.	865½	1.06	VI.	Black lead.	870	1.09				
17.			819¼	1.00			799	1.00				
17.	III.	India ink.	788½	1.00	V.	Aurum Musivum.	819½	1.00				
18.			836½	1.06			869	1.06				
20.		India ink.	834	1.00	V.		816	1.00				
21.			890	1.07			861¾	1.06				

Second.—The correction for the altered circumstances of temperature of the room, &c. may be tested by comparing the experiments made with different cylinders having the same coatings on different days. In the table on the preceding page, is given the various results of this kind furnished throughout the series of experiments. The date is given in the left hand column, and applies to all the results on the same horizontal line with it. A comparison of the numbers in the columns marked ratio, and on the same horizontal lines, will shew how far the same reduction to a standard would have been given by different cylinders: in other words, how far the influence of currents of air, local temperature, and radiation from or to adjacent bodies, might have interfered with the particular results.

Of the ratios thus brought into comparison, it will be found that in one case the results are identical; in four others that they differ one per cent.; in two others two per cent.; in four others three; in one four; in three five; in two seven; and in one ten per cent.; omitting this latter, the accordance is much less satisfactory than was shewn by the former table, and the average amount of error is nearly four per cent.

Having now shewn the probable limits of accuracy in the experiments, I proceed to compare together the reduced times of cooling of the same cylinders with different coatings. In the table will be given the observed time of cooling through forty degrees, and the time of cooling of the standard from whence the reduced times are deduced. As the colours of the substances were not in all cases what would be expected, the colour is designated in a separate column.

Cylinder, No. 1, variously coated.

Nature of Coating.	Colour.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced Time of cooling.	Remarks.
			Seconds.	Seconds.	Seconds.	
Carbonate of lead.	White.	Oct. 24.	864	1014	864	Smooth.
Vermilion.	Red.	25.	806	937	872	Smooth, with minute cracks.
Golden sulphuret of antimony.	Brown, nearly black.	31.	868.5	969	909	Rough, peels easily.
Red oxide of lead.	Orange.	Nov. 6.	890.5	948.2	952	Smooth.

Cylinder, No. 1, variously coated—continued.

Nature of Coating.	Colour.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced time of cooling.	Remarks.
			Seconds.	Seconds.	Seconds.	
Red oxide of lead, additional coat,		Nov. 11.	932.7	950.2	995	For comparison with following.
Do. blackened by hydro-sulphate of potassa,	Brown.		917.8	...	966	Red shews through.
Plumbago.	Black.	17.	787	819.2	974	Uniform, but not glossy.
Gamboge.	Olive.	20.	808.7	816	1005	Smooth, but in streaks.

The radiating power being greater, as the time of cooling is less, we have the order of radiating power of the different coloured substances as follows:—white, red, brown, orange, black, green. Omitting in this enumeration the blackened surface of the red oxide of lead, which had passed in thickness the maximum radiating thickness, and is only comparable with the result which precedes it, the change of colour effected by changing the surface sulphuret of lead (black or rather brown), increases the radiating power in the ratio of 1.03 to 1, which is within the average of error.

The following results, given in order of time, and reduced by the standard, were obtained with cylinder No. 2.

Nature of Coating.	Colour.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced time of cooling.	Remarks.
			Seconds.	Seconds.	Seconds.	
Ammoniacal sulphate of copper.	Bluish-green.	Nov. 6.	808.5	948.2	856	Streaked, and peels off rough.
Indigo.	Blue.	11.	928	950.2	990	Very smooth.
Carbonate of lead.	White.	14.	883.2	956	937	Smooth.
Do. Do.		15.	910	856.5	982	For comparison with following.
Do. blackened by hydro-sulphate of potassa.	Black.	15.	874	...	944	
Peroxide of manganese,	Dark brown.	18.	747	869	872	Uniform, but not smooth.

The variety of colour is here small; the radiating powers rank bluish green, dark brown, white, blue; omitting the second experiment with the carbonate of lead, which is only comparable with the one in which the surface was blackened by hydro-sulphate of potassa. Comparing these two results, the change of surface appears to have increased the radiating power in the ratio of 1.04 to 1.

The coatings applied to cylinder No. 3 were more varied than those of either of the foregoing.

Cylinder, No. 3.

Nature of Coating.	Colour.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced time of cooling.	Remarks.
			Seconds.	Seconds.	Seconds	
Carbonate of magnesia.	Yellowish-white.	Oct. 11.	859.5	862	1011	Rough in specks projecting.
Carb. of lime (chalk).	White.		879		1034	Do.
Carb. of lead.	White.		877		1032	Smooth, and somewhat shining.
Prussian blue.	Blue.	25.	805	937	871	Rough.
Litmus.	Blue.	31.	831	969	870	Not uniform.
Bichromate potassa.	Reddish-brown.	Nov. 1.	854	894.5	986	Streaked, and not smooth.
Alkanet.	Crimson.	11.	926.7	950	989	Uniform.
Do. rendered blue by potassa.	Blue.		938.2		1001	
India ink.	Black.	17.	776	819	959	Not smooth.
Do.		18.	836	869	976	More uniform (mean, 697).
Carb. of lead in oil of lavender.	White.	21.	843.5	862	992	Uniform, but not glossy on surface.
Do. blackened by hydro-sulphate of potassa.	Black.		850		1000	

The effects of changing the crimson of alkanet to a blue was apparently to decrease its radiating power about one per cent., or the change of colour in reality did not alter the power. The carbonate of lead lost also slightly, or rather was not affected, by the change not only of its surface, but of a considerable part of its mass, for the oil of lavender having evaporated, the hydro-sulphate of potassa penetrated the coating. The substance by means of which the coating was applied seems not to have sensibly affected the radiating power; the carbonate of lead applied with gum differing in radiating power but four per cent. from that applied with oil of lavender.

The colours rank from the foregoing table, blue, two varieties; black, brown, crimson, white, black, blue, white, three varieties. There is no certainty that the litmus and alkanet, changed to blue by potassa, were originally the same in colour. The surfaces were very different in regard to uniformity and smoothness; the alkanet was perfectly uniform, but not at all glistening; it may be described as of a uniform minute roughness. In this table, we have the greater number of whites at the bottom of the scale of radiation, and of blue and black at the top; but this is all that can be said, for a white, a black, a blue, are in close proximity near the middle of the scale.

The results with the cylinders Nos. 4 and 5 were few in number. They are subjoined.

Nature of Coating.	Colour.	Date.	Observed time of cooling.	Time of cooling of standards.	Reduced time of cooling.	Remarks.
			Seconds.	Seconds.	Seconds.	
Cylinder, No. 4.						
Cochineal.	Crimson.	Nov. 1.	848.5	894.5	962	Not uniform.
Chromate of lead.	Yellow.	... 6.	931.7	948.5	996	Very smooth and uniform.
Bi-sulphuret of mercury (vermilion).	Red.	... 11.	843.7	950.2	888	Uniform and smooth.
Sulphate of baryta.	White.	... 15.	759.2	865.2	889	Rough.
Ditto.	...	... 21.	829	861.7	975	Smooth, freshly precipitated.
Cylinder, No. 5.						
Gamboge.	Olive.	Oct. 29.	845.5	934	917	Smooth.
Bi-sulphuret of tin (aurum musivum).	Yellow.	... 31.	969	969	1014	Very even.

The order from cylinder No. 4 is red, white, crimson, white, yellow; the influence of the roughness of surface is here plainly shewn, by which the place of the white material, sulphate of baryta, is entirely changed; this is a quality difficult to appreciate, and yet here we find it exceeding in influence any other property of the coating.

A review of these results will shew that we have been able to establish, among the separate series, no order of colour. We have the different orders as follows:—

From No. 1.	No. 2.	No. 3.	No. 4.
White	Green	Blue	Red
Red	Brown	Black	White
Brown	White	Brown	Crimson
Orange	Blue	Crimson	White
Black	White to black, an increase of 4 per cent. in radiating power.	White	Yellow
Green		Black	
White to black, an increase of 3 per cent. in radiating power.		White	No. 5.
		No effect from changing white to black, or purple to blue.	Green
			Yellow

A more satisfactory comparison, in respect to the number of substances employed, will be had by using the means, heretofore described, for comparing together the results obtained with different cylinders. For example, Nos. 1, 2, and 3, were each coated with carbonate of lead, and through the numbers given by these coatings, those found for the other coatings can be compared. Nos. 1 and 4 were coated with vermilion, and Nos. 1 and 5 with gamboge.

The following table presents the comparison, the substances being arranged in the order of their radiating powers:—

No.	Nature of Coating.	Colour.	No. of Cylinders.	Date.	Time of cooling.	Remarks on Surface.
					Seconds	
1	Litmus blue, .	Blue, . .	3	Oct. 31.	728	
2	Prussian blue, .	Blue, . .	3	... 25.	729	Rough.
3	Ammoniacal Sulphate of Copper,	Greenish-blue,	2	Nov. 6.	789	Rough.
4	Peroxide of Manganese, . .	Brownish-black,	2	... 18.	804	Not shining but uniform.
5	India Ink, . . .	Black, . .	3	... 17.	804	Not smooth.
6	Bi-chromate of Potassa, . .	Brown, . .	3	... 1.	810	Streaked, streaks smooth.
7	India Ink, . . .	Black, . .	3	... 18.	817	Smooth.
8	Alkanet, . . .	Crimson, .	3	... 11.	828	Not shining but uniform.
9	Carbonate of Lead in oil of lavender,	White, . .	3	... 21.	830	Smooth, not shining.
10	Sulphuret of Lead,	Black, . .	3	... 21.	837	
11	Alkanet blue, .	Blue, . . .	3	... 11.	838	
12	Carbonate of Magnesia, . . .	White, . .	3	Oct. 13.	846	Rough.
13	Carbonate of Lead in gum, . .	White, . .	1	... 24.	864	Smooth.
14	Carbonate of Lime,	Dingy white,	3	... 11.	865	Medium.
15	Vermilion, . .	Red, . . .	1	... 25.	872	Smooth.
16	Sulphate of Baryta,	White, . .	4	Nov. 15.	873	Rough blueish-white.
17	Golden Sulphuret of Antimony,	Brown, . .	1	Oct. 31.	909	Smooth, in streaks.
18	Indigo, ...	Blue, . . .	2	Nov. 11.	912	Smooth.
19	Cochineal, . . .	Crimson, .	4	... 1.	944	Smooth.
20	Red Lead, . .	Orange, . .	1	.. 6.	952	Smooth.
21	Sulphate of Baryta,	White, . .	4	... 21.	957	Medium.
22	Plumbago, . .	Black, . .	1	... 17.	974	Not shining but uniform.
23	Chromate of Lead,	Yellow, . .	4	... 6.	977	Smooth.
24	Gamboge, . . .	Olive-green,	1	... 20.	1005	Smooth, in streaks.
25	Bi-sulphuret of Tin, . . .	Yellow, . .	5	Oct. 31.	1085	Smooth.

The results thus exhibited are decidedly unfavourable to the specific effect of colour in determining the radiating powers of bodies. Blue is above black at the beginning of the table, and occurs again in the eighteenth place. Although the first seven numbers are blue or black, the ninth, tenth, eleventh, and twelfth, are white, black, blue, and white respectively. Red occupies the eighth and nineteenth places, and then an intermediate one, namely, the fifteenth. White is in the greater number of cases in the middle part of the table, ranging close to black.

The alleged advantages of dark clothing during cold weather thus seem to have been too hastily inferred; and it appears that, provided the person is not exposed to the sun, the particular colour of the clothing is not of real importance.

If colour is not a determining quality, neither does roughness appear to be so, for though generally the smooth surfaces are lower on the list, this is not universal.

The rough sulphate of baryta is lower on the list than the smooth carbonate of lead.

Plumbago occupies a low place, and India ink a comparatively high one.

The best radiators do not appear to belong to any particular class of bodies; litmus blue and Prussian blue are side by side, while sulphuret of lead, and the bi-sulphuret of tin, are fifteen numbers apart.

If the results be admitted as decisive of the radiating powers of the bodies used, they shew, that each substance has a specific power not depending upon chemical composition nor upon colour. I do not claim to found such a conclusion upon the experiments; their object has been before stated, and if they prevent the introduction of an inference from an imperfect induction as a law of science, the labour bestowed upon them will be amply recompensed.*

Notes on the Natural History and Statistics of the Island of Cerigo and its dependencies. By Robert Jameson, Esq. Assistant Surgeon, 10th Regiment of Foot, Corfu.

Cerigo is the most southern of the Ionian States. It is situated to the south of the Morea, at the mouth of the Gulf of Kolokythi and Furnus, its nearest point to the mainland is only distant about twelve miles. The island is in shape triangular, with its base towards the south. Its greatest length is about nineteen and greatest breadth twelve miles; the whole island forms an area of about 116 square miles.

Under the denomination of dependencies are Cerigotto to the S. S. E. distant about twenty miles, and several islets and rocks at

* The scientific reader need not be reminded that these remarks do not bear upon the radiation or absorption of heat accompanying light.

a greater or less distance from the coasts of Cerigo; among the most important are the Calami on the west coast, Diacofti on the east, the Ovo, Dragonares, and Kouphenisi on the south; besides these there are others off the north of Cerigotto, viz, the Nautilus (native name Tracolithra), Porrelli (Laguneres), and Porri (Trasonisi); the nearest is the Nautilus, which is reckoned to be four and a half miles distant. The most important of the dependencies, Cerigotto, is said to have a surface of twelve square miles.

Cerigo is a mountainous land, being less so, from the nature of the rocks, as we proceed from south to north. Before proceeding further, we may mention a division of the island into districts by the natives, and used by Government, to prevent repetition hereafter. There are five districts, viz. Citta, Livadi, Milopotamo, Castrisso, and Potamo. The district of Citta includes all that portion of the island to the south of a line drawn nearly straight across, commencing between the creeks of Felloti and Meledoni on the west, and terminating a little below Diacofti on the east coast; Potamo is the part to the north of an irregular line from Calumi on the west to the roadstead of Pelagia on the east; Castrisso occupies the remainder of the east coast, and the greater part of that adjoining the Potamo boundary line; the rest is almost equally divided between the districts of Livadi and Milopotamo, whose boundary line begins near Mortidea on the coast.

Cerigotto may be divided into three districts, a southern called Apolitaria, a western Camerelles, and an eastern Asprolachos.

In all parts of Cerigo, except the district of Potamo, the mountain chains run N.W.—S.E. and N.N.W.—S.S.E., while in Cerigotto and that district they run N.N.E.—S.S.W. There are three principal chains in Cerigo. The southernmost, to which I shall give the name of St[a] Elessa, after its highest summit, runs through the south-west of the island; the next forms the high land in the centre of it, and is called Lachnos; the third I shall term St Georgio, after its highest point, extends along the east side of the island. Some suppose that the mountain called St[a] Elessa is the highest, while others maintain it to be the mountain of St Georgio, neither exceeding 1400 feet.

Cerigotto is traversed by three mountain chains, an eastern called Kaoos; a central, Kalives; and a western Cephali. The highest mountain on the island, which belongs to the central chain and is called Turcovigla, is said to be 1100 feet above the level of the sea.

In Cerigo and Cerigotto, except at the north end of the former, the mountains have the same general forms, viz. round-backed and table shapes, sometimes they have conical or peak-shaped summits. St[a] Elessa is a good example of the table form, St Kindinus, of the same chain, of the conical; but the form most generally met with is the round-backed, while peaks occur less frequently.

The greater part of the west and south coasts are precipitous, while the east is flat or shelving, and the mountains have a gradual ascent.

Separating the three principal chains of Cerigo, are two principal valleys of a moderate breadth. Lateral chains with their valleys terminate in these principal valleys. In these principal valleys the bounding mountains have a gradual ascent, but in the lateral and less important they often present immense mural precipices.

From the physiognomy of Cerigo just described rivers of any importance could not be expected to occur, but there are several streams which continue to run throughout the whole year. One rises from the central chain of mountains near the village of Milopotamo, and after taking a winding direction, and turning many mills in its course, discharges itself into the sea on the west coast. Another rises near the village of Milata, runs along for a few hundred yards and disappears in the limestone, but at one of the points of Paleopolis, on the sea coast called Cashi, about two miles distant from the place of disappearance, there is a pool of water through the whole year, which is supposed to be a partial reappearance of the Milata rivulet; a third runs through the village of Carova, turns also many mills. There are other less important rivulets, but not worthy of particular notice. In the course of some of those streams you meet with cascades, but they never exceed thirty feet in height.

I have already mentioned that the shape of the island is triangular. The west side and base are much indented when compared with the east, and it is singular that the two best and only ports worthy of notice are situated, the one called Kapsali, at the south-west angle, and the other, San Nicolo, at the south-east angle; the other part used as a port is called Santa Pelagia, and is merely an open beach situated on the north-east coast.

Port Kapsali, being the site of the city, is the only place where vessels obtain pratique, or are allowed to perform quarantine. Two lateral chains are sent off from the St[a] Elessa range, one from Mount Elessa, running nearly north and south, forming the west side of the harbour, and the other from Mount Kindinus, a subordinate range of which forms the east side; but the harbour is partially divided by a mass of rock projecting from its upper part, making two ports within the great harbour, the east, called (from its appropriation) Quarantine, and the west Kapsali, being double the size of the former. In Kapsali, the water is deep enough for ships of any burden; while Quarantine is shallow, and only fit for minor vessels. Unfortunately, however, Kapsali is much exposed to the violent SW. and SE. gales which blow frequently during the year.

Port San Nicolo is an indentation on the east side of the bay of the same name, of a truncated triangular shape, the apex being its mouth: its basis is notched at the two angles, forming inner harbours, the west one being small when compared with the east. The greatest length of the port is 280 yards, and of the largest notch 150 yards: the broadest part of the port measures 150 yards, and of the notch 90 yards. The deepest part of the port is 20 yards, and of the notch 6 yards. Even close inland of the notch, the depth of water varies from 5 to 17 feet, and in the port from 12 to 40 feet. This harbour is sheltered from every wind that blows, has good anchorage-ground, and is much liked by mariners who frequent the Levant.

In Cerigotto there are four ports, but all so bad as even to be dangerous for boats. The largest is on the north coast, and called Potamo, on account of a streamlet discharging itself into it. The next in importance is Camerilla, facing the SW., a small creek, half filled up, and its entrance obstructed by rocks, which have fallen from the precipices, by which it is on all sides surrounded. Two others still remain, called Stavrolo and St Georgio, which can only be approached by boats in north-east winds.

Sketch of Formations.

Nearly the whole of the northern district of Cerigo consists of primitive slaty rocks, while the higher parts of the middle and southern districts are chiefly transition, with their valleys more or less covered by tertiary and alluvial deposits.

Primitive District.—It consists chiefly of mica-slate, which passes into clay-slate on the south-west of the district. Among the varieties of mica-slate, the common and undulated were most frequently met with; but the fine slaty was noticed in the neighbourhood of the clay-slate, and the talcose in several parts near the north end of the island: the common variety contains felspar in several places. The clay-slate is of a blackish-grey, and sometimes of a bluish-grey colour.

Associated with the mica-slates and clay-slates, the following minerals were noticed occurring either in beds or veins. *Limestone.*—Of it we observed several beds, but the most extensive is about half a mile to the south-east of the village of Potamo, where it is sometimes of a white and at other times of a grey colour, pretty highly crystallized, but full of flaws. *Serpentine.*—It occurs in small beds, of a green colour, on the north-east coast, where it is occasionally mixed with limestone, forming what is called Verde Antico. *Quartz.*—It occurs of various colours, in beds and veins in different parts of the district; but it is most abundant at the north end. Near Ortholithas there are veins of a richly coloured pale blue variety. *Iron-ores.*—Several kinds were noticed, of which the most common were specular iron-ore, compact red iron-ore, and compact brown iron-ore, occurring sometimes in the form of veins, as the specular iron on the acclivity of mica-slate opposite Santa Pelagia, and the red iron-ore near Potamo village, in the clay-slate; the brown iron-ore occurs in beds in clay-slate in a lateral valley a little to the south of the village of Potamo.

Stratification.—The direction of the strata was everywhere remarked to be ENE., WSW., and the dip NNW. at an angle between 18° and 25°. In some places, the strata were a good deal contorted.

Transition district.—Only two rocks were met with in this formation, viz. limestone and greywacke, the latter occurring in small quantities, and quite subordinate to the former. *Limestone.*—This rock may be divided into an upper, middle, and lower portion, from general characters which it possesses: the lower is compact, always stratified, and the predominating colour is red; the middle portion is grey, and presents the slaty structure; and the upper is seldom stratified, has black as its predominating colour, and is more frequently crystalline; also the lower parts are more venigenous than the upper. The limestone throughout its whole extent has a splintery fracture, and is translucent on the edges. All the parts agree in being traversed by veins of calcareous-spar in every direction, but in some places more than others. Everywhere this rock is traversed by numerous fissures and caves, some of which are of considerable extent. The two most remarkable caves are called Santa Sophia, the one situated in the district of Cetta, the other in that of Milopotamo. That in Cetta is superior to the other in dimensions: it is situated in a ravine about a quarter of a mile from the sea, and two or three miles to the east of the town of Cerigo. Its mouth is nearly an isosceles triangle, of considerable dimensions. On entering, we found the floor and roof covered with stalactitic matter, assuming various grotesque forms, and dividing it into several chambers. The first is about seventy or eighty feet broad by about fifty or sixty long: the second is about forty-six feet long by thirty-five or forty broad, in which the calc-sinter assumes as many shapes as if it had been worked by artists of superior skill. Here you find something like an altar, there a throne, and in other places pedestals and pillars of various descriptions, more or less ornamented by different kinds of natural architecture. In most of the caves you descend in going into them, but in this you ascend. What arrested our attention most was the insufferable heat felt on entering the second chamber, increasing as we advanced. On suspending a thermometer, it stood at 70° Fahr., while the external air shortly afterwards proved to be only 50°. From experiments made, it would appear that the heat of this cave in winter is much greater than that of the atmosphere. The average annual temperature of the atmosphere for six years was 65°.3′. Shepherds and goatherds avail themselves of the unusual heat, by sheltering their flocks in the outer chamber of the cave at night, which circumstance may assist in making the temperature greater than it would naturally be. The other cave is in a cliff overhanging the sea, near the village of Milopotamo, but nothing so remarkable presented itself in it as to be worthy of notice. The floors of both were covered by a dark-coloured earth, but no fossil remains were observed.

Many of the fissures are filled up by an indurated clay of a reddish-brown colour, containing angular pieces of the limestone, and imbedded in it pieces of bones of various animals; among others we observed teeth and bones of oxen, deer, sheep or goats, and birds, also several bones belonging apparently to the Rodentia, but none of the order Carnivora could be detected. The most important bone fissure is situated at Vrulea on the south coast, a few feet above the water's edge, and about three or four miles to the east of Port Kapsali, where we find the clay everywhere thickly studded with pieces of bones, and in a few places shells of the genus Helix were noticed; the clay is so hard and tough, that specimens could only be obtained by blasting. There is another considerable deposit of osseous breccia in fissures on the west side of a hill a little to the north of the town called Turcovano, where Spalanzani says, "Che un' intera montagna é piena d'ossa umane e belvene impietrite;" how far he is right, will be evident from the above remarks. As to the occurrence of these remains in the crevices, and how deposited, Dr Buckland has given ample explanations in his work: at present, however, in many of the fissures, hawks, pigeons, rabbits, &c. nestle, and I may remark that often they are so covered by brushwood, as to be dangerous not only for quadrupeds but also for travellers.

In the limestone, fossil organic remains are of rare occurrence, and what we met with were chiefly trochites.

The coasts and highest lands of the south and middle divisions of Cerigo, the east coast of the northern division, except in a few points where the mica-slate juts down on the coast, the extreme end of the island, all the high lands of Cerigotto, some of the islets, as Ovo, Drajoneres, &c. consist of this limestone. We find the same limestone resting on the clay-slate where the northern district joins that of Milopotamo, and on mica-slate at the junction of it and Castrisso.

Greywacke.—Lying immediately upon the limestone is a rock which I have called greywacke. It may be divided into upper and lower; the lower is composed of fragments of various sizes held together by a clayey basis, and consisting of quartz, flinty-slate, jasper, and limestone, the latter being the most abundant; in the upper the particles are smaller, and the rock assumes the slaty structure. But sometimes the particles disappear altogether, and then it is either slaty or merely a clayey amorphous mass of a blue or red colour, which is used here for covering the roofs of houses. The greywacke and its slate sometimes alternate, but I did not observe that to be the case in Cerigo. They are met with, covering the lower parts of the limestone, as at the foot of a mountain-chain or the lower lateral chains. In several parts of the mountain-ranges we see them very distinctly, extending from their acclivities into the valleys, and forming hilly ground.

The following minerals were noticed in this formation during a hasty examination. *Quartz.*—Various subspecies and varieties of this mineral occur in the limestone and greywacke. Common quartz was met with abundantly in the lower parts of the limestone, forming veins; these veins were sometimes of a red but oftener of a green colour. The subspecies flinty slate occurs in veins in the greywacke, as well as (although in smaller quantity) its variety called Lydian stone. In the lower part of the limestone, veins of common jasper of a red colour are met with. *Manganese Ore.*—The black and grey species of this ore occur along with the jasper just mentioned. Both the red and black compact iron-ores were also observed occurring in veins.

Although in this sketch we have only mentioned a few minerals which occur imbedded, yet it is important to know that the rocks of the transition class are the repositories of the principal ore-mines and roofing-slate quarries now worked, also of several mines worked in ancient times, such as those of silver on Mount Laurion, which Zenophon says yielded an annual income of 100 talents to the State; we are therefore entitled to infer, that some deposite of valuable ore may one day be discovered in Cerigo. If there was a market, various quarries might be opened in the clay-slate, limestone, and greywacke-slate, not only from being easily worked, but also from their accessible nature and vicinity to the shore.

Stratification.—The direction of the limestone strata is N.E.—S.W., dipping to the N.W. at an angle varying between 15° and 30°. Contortions of the strata are uncommon.

Secondary Rocks appear to be entirely wanting in Cerigo and its dependencies.

Tertiary Rocks.—Several deposites met with in this island are referable to the tertiary class. The oldest is a blue marly clay, containing numerous fossil marine shells; the genera most frequently met with were ostrea, natica, buccinum, eburna, turritella, cerithium, pleurotoma, and murex: it varies in thickness from a few feet to many yards. In a transverse valley a short distance to the north of the town of Cerigo, and through which the principal or Potamo road runs, we have a good example of it; there at the upper part it alternates with the lower parts of the next deposite, which is a sand or sandstone; again, in the escarpment of the valley of Tholaris, this clay contains beds of brown coal. Resting then on the clay is a calcareous sand or sandstone, of a brown or yellow colour, in which were imbedded marine shells, although neither so abundant nor of the same character as in the former; the shells belonged chiefly to the genera solarium, tellina, and ostrea. The next rock in succession is a limestone which passes into a sandstone in its lower parts, and into a marl in its upper, the central part is more or less compact and generally inclines to a yellowish colour, while the upper is of a white or grey colour: it varies like the clay in thickness from 50 to 200 feet, abounding in marine remains; the lower beds seemed to be characterised by the number of ostreæ and fossil corals they contained, while the upper may be said to be characterized by their terebratulæ, and the middle by their abounding in large pectunculi and pectenes. Besides these, many other fossils were met with, sometimes confined to particular parts of the deposit, and at other times found diffused: in the lower we met with specimens belonging to the genus mytilus; in the middle, among the crustacea, echinarchinus, of the mollusca, the genera mytilus, spondylus, donax, cardium, venus, voluta, conus, and in the upper various species of encrinites, the claws and pieces of the body of crabs. In all the deposits the genera balanus, trochus, &c. were met with.

In this limestone we very often met with caves, but as it was customary among the Cerijotts from a very early up to a late date, to excavate the solid rock for tombs, temples, and dwelling-places, it becomes difficult to distinguish between the natural and artificial caverns.

From the mineral characters and from the organic remains these deposites contain, we may refer them either to two formations, viz. the plastic clay and coarse limestone, or, as no fresh-water productions were observed, to the latter only; but in such a determination, local circumstances must be taken into account.

Sometimes it happens that the upper layer of the marl contains rolled fragments of various minerals, but the most abundant is limestone, forming the basis of a calcareous sandstone, in which we find the same kind of remains as just mentioned.

As to the distribution of these deposits we may remark, that the clay, sandstone, and a portion of the limestone formation, occur in Livodi, Calamos, Tholaris, also in several lateral valleys in Citta; and in the larger creeks along the east coast between Mirtidia and Felloti, as well as the Paleopolis, the coarse limestone formation is complete. In fact, parts of the limestone formation occur in all the creeks on the west, in a few places on the east coast, in the two divisions of the east principal valley already mentioned, in a small portion of the west principal valley, and in several large lateral valleys in the district of Citta, also in another near the north end of the island. Sometimes we find these deposits at considerable heights above the level of the sea in the smaller valleys, but the most frequently met with is the calcareous sandstone covering several of the higher parts of the mountains, unaccompanied by any other deposit.

From a very early period, the more compact portions of the coarse limestone have been quarried for building-stones. Castri, in the bay of San Nicolo, was the site of the ancient sea-port town called Scandea, a little to the east of which there are extensive quarries, supposed to have been worked by the Greeks for the construction of their town; and at present there are quarries in the creek of Felloti, and on the high ground of Perati, where the limestone is very compact.

In noticing the distribution of the clay and limestone, Cerigotto is not mentioned, because there we have only the limestone at the north harbour, and the valleys are covered by a deposit of a different nature. This deposit is a slaty, white or grey marl, varying in compactness, containing layers of *menilite* of a few inches thick; only in one part, viz. at the lower end of the west valley, where the marl is less slaty and more compact, we observed marine and fresh-water fossil shells of the genera Lymnea and Ostrea. From the occurrence of the menilite, we are inclined to refer this to the lower division of the gypsum formation.

Although somewhat foreign to our subject, we may state that deposits, containing shells similar to the clay and limestone of Cerigo, were observed in the Gulfs of Kolokythi and Koroni, also between the town of Modon and Cape Gallo; but, as the remarks were made in a cursory way, I may be mistaken in my conclusion.

In Cerigo and Cerigotto, we meet with a conglomerate, covering the rocks just mentioned. It consists of a basis or ground of marl, holding together rolled fragments of limestone, quartz, felspar, and sometimes serpentine. At Santa Pelagia we see, apparently the same conglomerate passing into a sandstone containing abundance of pectunculi. No other fossil organic remains were observed in it, consequently we were unable to determine its age; but in a few places it contains caves, in which were imbedded, in calcareous sinter, the bones of animals belonging to the class mammalia. In the creek of Nessachia, on the north-west coast of Cerigo, there is a small cave in this rock, situated about twenty feet above the level of the sea. It is divided into a right and left chamber. The former is so high throughout its whole extent that we can stand upright in it, and the floor is lined with calcareous sinter, in which are imbedded pieces, seldom, however, whole bones of oxen, goats, and sheep. On the other hand, the latter is low, and its floor is covered by a black mould several feet deep, in which the teeth and bones of all parts of the body belonging to oxen, deer, and sheep, were found imbedded and in a tolerably perfect state. These bones are of a white colour, dry, and adherent to the tongue; but to what age we are to assign them is difficult to determine. A more minute examination of the cave may elucidate the subject. At present this cavern, although by land almost inaccessible to man, is occasionally inhabited by flocks of sheep and goats when grazing in the neighbourhood; but, as the opening into it is scarcely large enough to admit these animals, others of larger dimensions could not enter; therefore, the above remains must have come there by some other means, and perhaps at an antediluvian date. Several other caverns were met with in this conglomerate in the district of Citta, but no fossil remains were observed in them.

The conglomerate occurs lining the sides, and partially filling up many of the minor valleys of the transition limestone. In the northern principal valley of this limestone, it covers the upper compartment called Oscelles, commencing in the Creek of Nessachia, and terminating suddenly at Tholaris. It also occurs superimposed on the tertiary limestone along the east coast, forming small arid flats, and in the south minor valleys of the district of Citta; also in various parts of Cerigotto.

Resting on this conglomerate, in various parts of the island we find a gravel composed of pieces of limestone, quartz, jasper, &c. in which, in Calamos and the Paleopolis, we met with pieces of ox and deer's bones. This deposite would require to be more minutely examined than we had leisure for. Having entered generally into geognostic details, we shall now make a few agricultural remarks.

Soils.—Until lately, very little attention was paid here to this important subject. At present the Ionians are making many improvements, and throwing into cultivation lands which have always been considered barren. It could not be otherwise under a chief so talented and accomplished as His Excellency the present Lord High Commissioner. Indeed, ever since Sir Howard Douglas's arrival in these islands improvements have been projected, and are successfully being carried into effect in every department. We trust that the people over whom he so paternally presides will appreciate the important services he is rendering to them. There are several great bars to the improvement of agriculture in Cerigo, but one of the greatest is, that many of the labourers leave their own island in spring to work elsewhere, for in the Morea, Candia, and Asia Minor, they make more money than if they remained at home to cultivate their grounds. The consequence is, that the agricultural labours are performed in their absence by the old men, women, and children. Another, and on what seems to hinge all others, is the want of a port to carry on commerce with other people, so as to export the produce of the island not consumed by the inhabitants themselves.

Having premised these remarks, we may now state, that, on account of the diversity of formations in Cerigo, we meet with various kinds of soil. In the district of Potamo the soil is for the most part too siliceous to be fertile; indeed, it is often extremely sterile, except in the valleys, and more especially those which are watered, where there is a mixture of other substances rendering it fit for cultivation; but in the valleys at the south end of this district, where the clay-slate and limestone appear, vegetation is extremely luxuriant. Again, in the limestone ranges, the soil is scanty and unproductive, owing, as in Potamo, to a want of a due mixture of other substances. In some valleys, or parts of valleys, the soil is entirely marly or clayey, in others calcareous, on both of which vegetation may be almost said to cease in hot weather; in others gravelly; while in the finest of all there is a mixture of marly clay and sand, owing to a breaking up and mixing of the tertiary deposits, forming the richest soil in the island, as at Livadi and Milopotamo. It is not long since the natives began to use manure generally; and even now some soils are never manured or fallowed. These are in the valleys which the natives believe to be sufficiently strengthened after bearing by the substances brought off the mountains during the rains.

Crops.—The average annual number of acres cultivated for five years, was 11,832 in Cerigo, pasture land 4988, and uncultivated 66,503½; in Cerigotto the average for three years was 280 acres cultivated, and 164 pasture land. In the districts of Livadi and Milopotamo, where the soils are clayey or loamy, we find the greatest quantity of wheat grown, and in the former the most pulse, and in Potamo the most maize and oats. An annual average for five years gives, of wheat, for Livadi 864 bush., for Milopotamo 740, and Castrisso, 364; pulse, for Livadi, 1224 bush., Cetta 25; in Potamo, of maize and oats 18,080 bush. Barley is also grown in the different districts in small quantities. The wheat is sown in autumn, and the harvest is in June; an annual average of five years for the whole island gives 2994 bush. raised on 453 acres, and there is imported about 8000: the flour is rich, but destroyed in a considerable degree by the imperfect manner in which the various processes are performed before it is ground. Thrashing is performed in the open air, by the grain being thrown into a circular thrashing floor of a few feet in diameter, into which bullocks are driven to tread on it, and winnowing is done in as imperfect a mode. This island enjoys a considerable advantage over most of the others, by having good millstones, which consist of buhrstone, imported from the island of Milo, where it occurs very abundantly, and there is thus little chance of having grit in the flour, a common occurrence in the sister isles. On the streams already mentioned there are numerous water-mills; and wind-mills are erected on the convenient heights throughout the island.

Maize is sown about May or June, and is ripe by September or October. It is one of the principal articles of food, although in early times it seems to have been a great rarity, "as it was then only eaten as a dessert at the public tables." The annual average quantity of maize raised for five years on the island was about 50,000 bushels, and a small quantity was imported. On Cerigotto, on an average of three years, there were 1870 bushels grown, including a few of barley and cambuchio.

Pulse.—The average quantity grown in Cerigo is 3161 bushels on 1508 acres, and there are 648 imported.

Olives.—Persons coming from Corfu to this island are immediately struck with the comparatively diminutive size of the olive trees, which is generally attributed to the high winds prevalent in Cerigo. Olive grove property, as in the other islands, is much divided. Every second year, or what we may call a season, the olives from which the oil is pressed are collected as they ripen in winter and part of spring. It would seem that in almost every island the natives have a different way of expressing the oil, and that of natives of Cerigo, curious from its apparent antiquity, is as follows: the olives are placed on a nearly flat stone, and another heavy one of a square shape is rolled backwards and forwards on them, so as to press the fruit; when thus bruised, the mass is put into a large bag (made of the fibres of a scoperta), which is closed, and thrown into a vessel containing hot water, and allowed to remain there till heated; it is then taken out and placed on a shallow trough with a plugged hole on the one side. The trough is elevated about two feet above the chamber floor, a man treads on the bag thus filled, from which the oil is expressed along with the warm water; as soon as the trough is nearly full, the plug is withdrawn, when both the substances escape into a vessel placed beneath, having near its bottom a plugged orifice. By the time this vessel is filled, the greatest part of the oil has separated from the water, and floats on its surface, from its specific gravity being much less; therefore when the orifice near the bottom is opened, the water escapes, mixed with only a small quantity of oil, into a hole dug in the ground outside of the chamber, where this oil also, when the mechanical mixture ceases, is collected by skimming it off by means of a branch or bunch of straw. In this manner a man, assisted by a woman and child, will make a barrel or more in a day.

The oil in quality ranks next to that of the island of Paxo, and is much esteemed, but unfortunately the produce is scarcely sufficient for the consumption of the island. On an average of three seasons, the seasonal quantity made was 1438 barrels, and the ground occupied by the olive trees was 514 acres. From a general average each tree is supposed to yield one-fourth of a barrel of oil, which will give for the whole island 5752 fruit-bearing trees, and the number to each acre will be about 11½.

Wines.—In vines there are 1372 acres cultivated, and the soils used are those of a poor clayey nature; hence we find much land in Castrisso and Potamo laid out in this way. There are several varieties of vines yielding wines of different strength and flavour; but a few years ago some vines were introduced from a celebrated wine district in the island of Candia, yielding a wine superior to any of those of a similar description belonging to the island. The manufacture of wine is so primitive that I cannot forbear giving a description of it. In the heat of the day the grapes are collected, and thrown into a stone cistern, where they are trampled on by the peasantry, so as to bruise the fruit to express the juice; on a level with the bottom is an opening, through which the juice passes into a large stone basin, from whence it is taken and put into casks, properly situated for fermentation. Racking, sulphuring, or fining are little or not at all understood. Some of the other islanders put resin, &c. into their wines as a preservative, and here they use gypsum, imported from Candia, which imparts a harsh disagreeable taste. Wines are of a pale and dark colour; the pale are of a very light yellow, or colourless, and to the taste similar to Sauterne, but much stronger bodied; the dark are similar in colour to port, with a stronger body than the pale, and less acid taste than the French wines. From the refuse of the wine-press an odoriferous spirit is made, and is much in use throughout the Archipelago.

Cotton.—Sufficient quantities of cotton and flax are grown for home consumption, amounting in the former to 3732, and in the latter to 2750 lb. on a quinquennial average.

Tobacco.—A small quantity is annually raised on the island, but is much inferior to that grown on the continent of Greece.

Indigo.—In mentioning this substance, I deviate somewhat from my object. The cultivation and manufacture of it was attempted in these islands on a small scale in 1832, with unlooked for success, by J. Falconar, Esq., at the request of the then Lord High Commissioner, Sir Frederick Adam. Mr Falconar found that a soil possessed of moderate properties, and rather stiff than otherwise, was adapted for the plant. The ground should be slightly tilled, and the seed sown somewhere between March and the middle of April, and the weeding ought to take place when the plant is a month old. By the middle or end of July the plant is matured, and the manufacture of the indigo commenced. In India the greatest expense at first is the erection of a manufactory, on account of the expense of materials, which is not here so. Towards maturity a steadiness of temperature seems to be favourable to the plant, which is said not to be the case in India; for here, from tables kept for two years, it would appear that the difference of temperature between six P. M. and midnight averages in July and August scarcely 6° Fahr., and between midnight and sunrise, which is the coolest part of the twenty-four hours, it is about 12°. Perhaps this may be assigned as the cause of the experimental crops here yielding much more matter, even under the disadvantageous mode of manufacture followed, than is obtained in Bengal. Further trial of the manufacture of this substance is worthy of serious attention of the Ionians.

Some of our kitchen garden plants have been introduced with success. Potatoes are gaining ground; the Cerigotts gradually acquiring a taste for them; at present more are consumed than reared on the island. Cerigo onions are everywhere highly prized, causing an abundant exportation; the garlic and leeks raised are also in repute. Cucumbers are grown in considerable numbers, on which, when in season, the poorer inhabitants almost entirely subsist. An attempt has been made to introduce tropical vegetables, and at present in a sheltered spot the banana plant is thriving.

From the small quantity of wood, the island has a very rugged bleak appearance, there being scarcely any thing but olives, unless in some of the more fertile spots, where we occasionally meet with the lemon, orange, walnut, almond, fig, cypress, oak, elm, locust, pomegranate, and palm. Fig-trees are said to be reared with greater facility than any others, growing where there is any soil for them to take root; the brushwood almost entirely consists of mastick and myrtle.

(*To be continued.*)

On the Formation of Hail. By M. De la Rive.

Electricity, whose presence in the formation of hail is rendered probable by the thunder and lightning which always accompany and characterize storms, has for long been regarded as playing an important part in this phenomenon, as in almost all other meteorological occurrences. Volta especially, by means of the opposite electricities with which he supposed the clouds which were placed the one over the other were charged, explained the augmentation in the size of the hailstones, which, according to him, passed from one of these clouds to the other, as light bodies situated between two jars filled with opposite electricities, would be alternately attracted and repelled between them.

In thus frequently traversing the humid atmosphere which separates the two clouds, and in slightly penetrating the clouds themselves, he maintained that each hailstone condenses upon itself an increasing quantity of water, which is congealed, thus forming the concentric layers which are observed in its structure; till finally becoming too heavy, it could no longer be sustained among the clouds, and fell upon the earth in a more or less inclined direction, according to the strength of the wind. It is to this tossing to and fro between the two clouds, and to the dashing of the hailstones against each other, that he ascribed the peculiar noise which is heard in the air some time previous to the descent of hail, and which has been compared to the noise which the quick and violent shaking of a sackful of nuts would produce. As to the formation of the nucleus of the hailstone, Volta attributed it to the great degree of cold produced by the evaporation which takes place at the upper surface of a cloud, the rapidity of which is increased by the direct effect of the solar rays which strike upon the cloud, and are absorbed by it.

The theory of Volta was attacked in a very powerful manner by M. Arago, in a very interesting article, which appeared in the Annuaire of the Board of Longitude for 1828. After confirming some objections which had previously been advanced by M. Bellani, the illustrious Frenchman suggested many others. How, for example, can it be admitted, that the great evaporation produced by the heating of the cloud by the action of the solar ray can be the real cause of greater cold, when this evaporation takes place only in virtue of the larger quantity of heat which is supplied to the liquid? Who, again, can conceive that the electrical power exercised by bodies so light as the clouds can sustain and neutralize the action of the weight of the hailstones, amounting sometimes to half a pound? or, finally, how can we suppose that two clouds can continue so strongly electrical that they can move heavy masses when they are so near each other, and separated only by an extremely humid stratum, through which the electricity might freely pass from the one cloud to the other?

Such are some of the objections to which the theory of Volta is liable, and which M. Arago points out in the article just referred to. It was the difficulties in which the theory is involved that led the *Academie des Sciences* in the year 1830, to appoint the best explanation of the phenomena of hail as the subject of the great prize which fell to be delivered in 1832. The conditions on which it was to be conferred were severe. The competitors were to supply a theory supported by direct experiments, and upon varied observations made, if possible, in the very regions in which the hail was formed, and which might replace the vague hypothesis with which we have been compelled to be satisfied up to the present time. The essayists were also recommended to avail themselves of all the accurate information which had hitherto been collected on the radiation of caloric, on the temperature of the atmosphere at different elevations, on the cold produced by evaporation, and upon electricity, &c.: finally, they were required, whilst treating of the formation of hailstones, to follow out the consequences of the theory they should adapt to its numerical applications, regarding the physical constitution of these hailstones, also respecting the enormous bulk they sometimes acquire, and as to the season of the year, and the times of the day in which they were most commonly observed. But in 1832 the prize was not conferred, because none of the memoirs presented were considered worthy of the honour; and the *Academie* again appointed this subject as the question for competition for the year 1834. Again, however, none of the essayists fulfilled the conditions proposed; the prize continued unadjudicated, and from that time, we believe, the subject has been entirely withdrawn.

It is under these circumstances that M. Lecoq, without aiming at the prize, appears to have complied, if not with all, yet, doubtless, with the most difficult of the required conditions; inasmuch as he has produced observations which were made in the very regions in which the hail was formed, and which besides, as we shall presently see, are abundantly calculated to throw light upon the theory of the phenomenon, and especially to demonstrate by facts the truth of the objections previously offered against the theory of Volta. We shall now allow M. Lecoq to speak for himself, and shall then conclude this article by some considerations on the phenomena of Hail, as influenced by electricity.

The year 1835 was quite remarkable for the number and intensity of the storms which prevailed in the south and middle of France. Electrical clouds rested permanently above the high mountains of Auvergne, and if sometimes the heat of the sun succeeded in dissolving them, it was only for a few hours, and very rarely for a whole day. The clouds accumulated with rapidity, the thunder rolled in the distance, a tempest announced the storm, and the rains descended in torrents. Violent hail-showers had already destroyed the harvest in the district of the Puy-de-Dome, and every day brought with it fresh disasters.

On the 28th of July the sun rose from an azure sky, no cloud appeared on the horizon, no vapour floated in the atmosphere, so that a beautiful day was anticipated. At 10 A. M. the heat became intense, and at mid-day it was almost intolerable, and then some thin flakes of vapour floated in the air at a great distance; the wind was north, but so feeble, that it in no degree tempered the heat. At one o'clock the wind had increased; the white and floating clouds had descended considerably, and half an hour later, covered a great part of the horizon; they had a greyish tint, which became darker and darker, till they were nearly quite black. At two o'clock they formed an immense covering over the whole of Auvergne; and it was then easy to anticipate that a frightful storm was at hand. We waited with anxiety for the issue of that majestic and terrible scene which was preparing. Silence and consternation everywhere reigned, speedily flashes of lightning illuminated the massive vapours which covered the old volcanos of Auvergne, while the sun still shone upon a portion of La Limagne. We then heard a distant and low-muttering sound which resembled a kind of rolling, and almost about the same time we saw a vast cloud advance from the west to the east, pure white in some places, but principally on its edges, and of a deep grey colour in the centre; it approached with great rapidity, and seemed to be hurried forward by a violent west wind, which we had not previously felt at Clermont. This cloud was evidently underneath all the others; its borders were festooned and deeply slashed, and protuberances, in the shape of long nipples, were suspended from the lower portion. At a quarter past two, the anterior part of this cloud had approached very near to Clermont, and the noise which we had long indistinctly heard was now very intense; and I then very clearly distinguished a very rapid motion in the edges of the cloud; these edges seemed to me to be undulating, but in the position in which I was, what appeared to be undulations must have been the product of a very violent agitation. I then imagined that I could distinctly perceive hailstones in the edges of the cloud, and I predicted to some persons who were with me the immediate descent of hail. Accordingly, two minutes after having seen this whirlwind kind of motion, there was a fall of hailstones, which instantly broke all the tiles of the houses, and all the panes of glass exposed to the north and west; for the hailstones being at the same time propelled along both by the north and the west wind, necessarily took the mean direction.

The first hailstones which fell succeeded each other very slowly, then all at once their number increased so rapidly that in ten minutes the soil was covered with them; some drops of water escaped at the same time from the electrical cloud, and then the distant rolling sound which we had so long heard entirely ceased; and the cloud freed from its swelling appendages, was carried away by the wind; after some hours the sun illuminated, with its pale and feeble light, that scene of desolation which night was speedily about to envelope.

It is not necessary that I should describe in detail the terrible effects of these hailstones. Suffice it to say, that some branches of trees two inches in diameter were cut asunder by them; some polished stones which formed part of the cornice of houses were broken on their edges, and some phonolite slabs, which were employed instead of the tiles which cover the roofs, were broken by the shock of the masses of ice. Finally, a considerable part of the beautifully stained glass in the windows of the Cathedral of Clermont was in a few minutes broken, although it had been exposed for at least four centuries without having been injured by any storm.

The hailstones fell very obliquely, so much so, that many persons were struck by them in their rooms, after they had entered through their windows; others were surprised in the fields and were wounded, though I have not heard that any were killed. I anticipated that I should have discovered a marked rotatory motion in the hailstones, but I could not convince myself of it at the moment of their fall, for they almost all broke instantly on the pavement.

The fall of the hail was scarcely over when I went, accompanied by M. Bouillet into the Botanic Garden, with the intention of examining the hailstones. Here we found many which, from having fallen upon the plants, were quite entire, and presented very remarkable forms. Their medium size was about the size of a pullet's egg, and some were as large as a turkey's. We were, however, informed that some of larger dimensions had fallen at Montferrand. Their form was an elongated spheroid, with the two extremities apparently equal: they were generally studded over with crystals, some of which still bore the shape of hexagonal prisms, terminated by six-sided prisms; but more frequently the angles had melted away, and the prisms had become cylindrical. Some of these superadded crystals projected as much as eighteen lines at the moment of their fall, and some according to appearance were two inches high. Other hailstones were only rough on their surface, and presented an infinite number of small elevations, like the masses of sulphuretted iron which are found in certain clays and lignites.

The crystals were grouped at the two extremities of the great axis of the ellipsoid, which, to all appearance, were the two poles of the hailstones, and their equator, so to speak, was deprived of a large proportion of them: in all the crystals were largest at the two extremities. The interior structure of the hailstones was nearly always the same. The centre was formed of small grains of white hoar-frost, and was opaque and fibrous; this was surrounded by many layers of transparent ice, which were sometimes so distinct that they could be counted; they increased in thickness as they approached the circumference, and they appeared harder toward the exterior than in the interior.

Their weight was not great, for the heaviest we weighed was only four ounces. However it is probable that the heaviest did not fall within our observation, for other individuals found some of them as heavy as five ounces and a half; and I have been assured that some weighed as much as eight ounces, and even as several pounds. Laying aside all exaggeration, I am inclined to believe that some amounted to eight ounces, though I much doubt if any were heavier.

Painfully distressed with the disastrous consequences of the storm, the opportunity escaped me of collecting the hailstones and afterwards analyzing the water, which I subsequently much regretted, and the more so, as persons worthy of credit have assured me, that many of them deposited a blackish residuum, which had a disagreeable smell, and that the water which was obtained on their melting had a very decided odour.

Having learned that the storm had raged in the Department de la Creuse, I suspected it might have prevailed to a still greater extent, and therefore requested M. Dejean, the Prefect of the Puy-du-Dome, to solicit information from the Prefects of the neighbouring departments, which he did with the greatest kindness.

I thus learned that the storm began at ten o'clock in the morning over the sea; the hail commenced by desolating a part of the Ile-d'Oleron, particularly the communes of St Pierre and St George's. The cloud then crossed from the west to the east over the department of La Charente Inferieure, in which the district of Marennes particularly suffered. The communes of St Aynant, St Jean-d'Angle, St Symphorien, St Sornin, St Just, Arvers, &c., were also visited by the storm, the hailstones varying from the size of a hazel-nut to that of a walnut.

The cloud passed over La Charente without discharging any hail; at least I have not obtained any information that it did from the Prefect of this department; but in Haute-Vienne, and exactly on the confines of La Charente, the hail fell in many places in the neighbourhood of Rochechouart. From thence, and pursuing a perfectly straight line from west to east, it crossed the department of Haute-Vienne. At noon it arrived at La Creuse, district of Bourganeuf. The communes of Faux-Mazuras, Manzac, Soubrebord, Morterolle, Vidaillac, St Hilaire, La Pouge, and St George's, were more or less invaded by it. The storm continuing to follow the same direction, reached the district of Aubusson, and there produced great devastations. From mid-day till 2 P. M. enormous hailstones fell in the communes of St Amand, Lupersat-Ars, St Avit-le-Pauvre, St Sulpice-les-Champs, La Rochelle, St Maixent, St Ulpimien, Maynat, Beissat, Alleyrat, St Silvain-Letrueq, St Aynat, La Chaussade, St Michel-de-Vesse, Chavanat, Malleret, and Banise. At half-past one o'clock the storm reached the western limit of the department of Puy-du-Dome; a quarter of an hour later, there descended upon the communes of Gelles, Proudine, St P.-le-Chastel, St Oure et Roure, enormous hailstones which, in a few minutes, covered the earth to the thickness of three inches. At two o'clock masses of real ice fell upon the lava which extends behind the Puy-du-Dome, and were broken to pieces on the angles of the volcanic rocks. Shortly after the cloud passed the Puy-du-Dome, it did much damage in the commune of Arcines, and from about a quarter till half-past two, it finished its disastrous course, upon Clermont and Montferrand; and thus in four hours and a half the tempest-cloud traversed a space of about ninety leagues.

In La Charente Inferieure, some communes of the district of Jonsac were visited by hail-showers between three and four o'clock P. M., as some others had been at four o'clock A. M. At St Yrieix (Haute-Vienne) there was also a fall of hail between three and four o'clock P. M.; and finally, half an hour later than that at Clermont, in the same department, the storm descended upon the communes of St Germain, L'Embron, Ardes, St Gervais, Collonges, Mauriat, Beaulieu, Lebrénil, Jumeaux, Auzat-sur-Allier, Orsonnette, La Monge, and Estel.

Hence we may conclude that the line of the hail was accompanied with lateral clouds, which the north wind carried generally to the south. The cloud which carried the hail was at first narrow, it then increased in size, and attained above the department of La Creuse, its greatest width; it then contracted again till it reached the middle of the department of the Puy-du-Dome, in which its extremity, cut in a straight line, presented an edge of half a league in breadth: its shape was that of a spindle, of which each truncated extremity was situated on the one side upon the Ile-d'Oleron, and on the other over Clermont, and the widest part was above La Creuse. In all the more ample accounts I have obtained, it is stated that the colour of the cloud was grey and white; that its edges revolved, that it extended from west to east, and with great rapidity, under the enormous cloud which hid the heavens from every eye. The wind also was every where the same, that is to say, there were two currents, the one placed above the other, which crossed each other at right angles, and in the direction of the four cardinal points, or from north to south, and from west to east.

The intensity of the storm went on, steadily increasing. In La Charente Inferiure the hailstones were small, round, and not very numerous; their numbers and volume increased in the department of Haute-Vienne, where some of them assumed the oval form; but it was especially in the department of La Cruese and in the district of Aubusson, that the hailstones attained all their size, and that oval form which they preserved, as far as Clermont; and their bulk was, to all appearance, very considerable, for the documents I have received from this department nearly all assign eight and ten ounces to them, and some as much as two and three pounds, so we are safe in concluding, that many of the larger were six or eight ounces.

It appears that the others were not covered with those long crystals which were found in those which fell at Clermont; these latter, during their long course, were the only ones which allowed the water of the cloud which sustained them to crystallize around them.

The hail was everywhere of short duration; it seldom lasted for half an hour, and almost everywhere it was followed by rain, which, however, was not very copious.

The tempest-cloud was exceedingly low when it left the department of La Creuse, for it passed along below the summit of the Puy-du-Dome, on which no hail fell, whilst a great quantity fell on the Little Puy-du-Dome, at a height of 3700 feet. Several persons who were upon this mountain at the time, were struck by the hailstones without experiencing any injury from them, although they were all studded with long and many pointed crystals. The animals which were feeding at this elevation were also assailed by these hailstones without manifesting any signs of fear, whilst some accidents happened on the road to Limoges, at an elevation of 620 feet less: the horses being hit hard, took fright, and the carriages were overturned.

The hailstones at the top of the Little Puy-du-Dome were carried along with great horizontal rapidity, and a few only fell on the summit of the mountain; they passed along with a hissing noise in a stratum of air which was extremely cold.

After the storm, I satisfactorily ascertained the height at which the hail was formed, by an attentive examination of the trees and plants, at the base and on the sides of the Great Puy-du-Dome.

At a certain elevation, the leaves, though exposed to the action of the hailstones, were not much injured, and were in no degree torn; for not having acquired at that time any great vertical velocity, they struck without tearing, and so fell under the trees. Somewhat lower down, descending towards Clermont, the leaves of the trees were lacerated; lower still, the branches were broken, and we have already stated some of the devastations of which Clermont was the scene; for the hailstones had then traversed a vertical course of from 2000 to 2500 feet.

The storm of the 28th July was assuredly one of the most awful that has been witnessed for many years. During the following days numerous very heavy showers were the preludes of new storms; and on the 2d of August a part of the zone which had been destroyed by the hail of the 28th July, was again whitened with fresh hailstones; but nothing had been left for further destruction.

It happened that on the 2d of August I was a witness, so to speak, of the formation of the storm, and of the congelation of the hailstones.

Leaving Clermont at six in the morning, I ascended the high ground which commands the town on the west. I traced the limits of the hail of the 28th July, that I might determine by following the edges of the injured surface, the shape of the cloud which had conveyed the dreadful scourge. At 10 I reached the base of the Puy-du-Dome; the day being splendid, and the sun most powerful.

Some white clouds extended themselves over the Mont-Dore; the Puy-du-Dome stood out majestically from the azure sky. Some shepherds whom I had interrogated respecting the effects of the hail of the 28th, urged me to retreat without loss of time to the hamlet of La Barraque, if I wished to avoid the storm which, according to them, was assuredly and speedily coming to assail us. The hope of seeing, in all its details, one of those magnificent scenes of which the atmosphere is the theatre, induced me, on the contrary, to attain, as quickly as possibly, the summit of the Puy-du-Dome, and before mid-day I was seated on the top of this enormous pyramid, and extending my observations over the immense horizon. The west wind, which had prevailed all the morning, speedily brought along with it some low clouds, which passed a few yards above my head, but the sun again appeared. I then saw other clouds detach themselves from the Mont-Dore, and approach very near me, impelled by a very violent south wind, but which I did not feel till near one o'clock. When I thus saw great clouds proceeding in different directions, I could not for an instant doubt the formation of hail, and my hopes were soon changed into reality.

So long as the two strata of clouds were not superimposed on each other, there was no appearance of hail. All I noticed was, that those which came from the south, and which were the most elevated, were congregating in little groups, which seemed to precipitate themselves on each other, so forming great black clouds, so large and weighty that the wind could scarcely move them, though they nevertheless proceeded towards the north. The lower part of the cloud would then elongate itself, presenting an enormous projection, torrents of water would speedily escape from it, which inundated spaces which were very circumscribed. As soon as a large quantity of water had escaped from the cloud, it became lighter, was again carried along by the wind, and disappeared at the horizon. This phenomenon was repeated many times during the course of an hour: but by this time the west wind had collected a great number of clouds, which formed an immense curtain, extending over the whole vault of the heavens. The south wind pushed under this stratum of vapour additional white clouds, which came with great velocity. The wind became violent and very cold on the summit of the Puy-du-Dome. The lower stratum of the clouds was not like the upper, uniform, but was composed of numerous coloured flocculi, which advanced in the same direction, but at unequal distances, and with different velocities. The brightest flashes of lightning illuminated them from time to time, and the thunderbolts, like furrows of light, passed from one cloud to another, sometimes an extended flash seemed even to traverse, at the same moment, the space which separates the Puy-du-Dome from the Mont-Dore. All these phenomena occurred in the lower strata of vapours, and I never saw the electric spark traverse the stratum of air which separated the two layers of clouds. I perceived the hail in the distance precipitate itself from the lower clouds and fall to the earth; I saw it distinctly at the distance of fifty yards from the summit of the Puy-du-Dome, and before my face. The cloud whence it escaped had indented edges, and exhibited in these edges a whirlwind kind of movement which it is difficult to describe: it seemed as if each hailstone was forced forward by an electric repulsion. Some escaped from beneath, others sprang out from above, so that they flew off in all directions, and would assuredly have reached the earth in many different courses, if the south wind, which was beneath the west wind, had not blown them all towards the north. After five or six minutes of this extraordinary agitation, in which the anterior edges only of the cloud seemed to participate, the hail ceased, order was re-established, the hail cloud, which had continued to advance very rapidly, continued its route towards the north, allowing us to perceive in the distance some sprinklings of rain, which scarcely reached the earth's surface, appearing rather to be dissolved in the lower strata of the atmosphere.

I waited for a second scene similar to that which I have just described, till a prodigious flash of lightning illuminated all the lower mass of clouds, one of whose edges rested upon the summit of the Puy-du-Dôme. I imagined that I was all of a sudden plunged into the most vivid light, and I experienced a general uneasiness, which probably arose solely from the terror with which I was seized. I descended the Puy-du-Dôme with the greatest rapidity, fearing to be hurt by the hailstones, or at least to be drenched by the storm, and I made for an asylum in a hollow grotto at the base of the Puy-du-Côme, which had on other occasions afforded me shelter. The summit of the Puy-du-Dôme was enveloped in the tempest-cloud, and it would have been imprudent to have remained longer there.

After having remarked the direction of the storm, and rested for a moment from my fatigue and alarm, I reached the Puy-du-Côme, a magnificent observatory, where I was still near the clouds. It was now two o'clock, and the state of the skies made me fear other heavy showers, which I was solicitous of avoiding. I then directed my steps towards the Puy-des-Goules, between two and three miles from the top of Puy-du-Côme, and I ascended it about three o'clock. The heavens were very much in the same state, the two strata of clouds were still apparent, and the south wind, which was very cold, scudded with great strength along the sides of the mountain. It brought along with it another hail-cloud, which appeared to be heavily charged, and in which I was enveloped for about five minutes. The hailstones were numerous, and the largest was scarcely the size of a filbert. They were formed of concentric layers, more or less transparent, and were roundish or slightly oval; they were all carried along in a horizontal direction with great velocity, from which the attraction of the mountain seemed to make them swerve, and many fell upon its sides. Very many struck me without doing me any injury, and they fell as soon as they touched me. The greater part of the cloud passed over my head, and I distinctly heard the hissing noise of the hailstones, or rather a confused noise, the result of an infinite number of partial sounds, which I could attribute to nothing else than the friction of each hailstone against the air. The cloud which passed over my head, and in which all the hail was formed, allowed none to escape beyond a half league from the spot on which I was standing. Some, however, fell on the northern side of the mountain which intercepted its progress, and I collected a certain number of the hailstones in a phial. I subsequently submitted the water to many chemical tests, and I obtained a sensible precipitate, with nitrate of silver, and muriate of barytes.

All the hailstones appeared to be subjected to a very rapid rotatory motion, but in different directions, so far as I could judge by examining their movements at the moment of their fall on the crown of my hat, which I held as much horizontal as possible to receive them. Many other clouds, charged with hail, still rose from the south, and now on one point, and now on another, it hailed without interruption from one till four o'clock on the chain of the Puys, from Mont Dore, as far as Riom and Volvic.

Between four and five o'clock the hail ceased; the clouds now formed only a single stratum, but they often presented that phenomenon I had noticed in the morning, viz. that they grouped together, and then poured out, along with flashes of lightning, enormous quantities of water. The south wind also had now ceased, the west alone blew, and carried along with it these frightful waterfalls. One of them discharged itself in my view at Barraque, on the great road to Clermont. I was distant from it about forty yards, and not a drop of water fell on me. A heavily loaded carriage which was at a little distance, disappeared in the twinkling of an eye, under the mass of water which the heavens poured down upon it. After the passage of the waterspout, it was overturned in a ditch, and the postilions for a time did not try to right it, so intense was the darkness in the midst of the storm. Large pieces of pavement and great blocks of granite were carried along by this waterspout, which still hurried away before me, and reached Clermont half an hour before I could arrive. The storm of the 2d of August was not so rapid in its progress as that of the 28th of July, and it traversed a much shorter line. It began upon the mountains of Cantal, and terminated upon the confines of Auvergne and Bourbonnais. M. L. de Buch, who, that day, was at Cantal, ineffectually attempted at ten in the morning to reach the summit of Puy-Griou, on account of the violence of the wind. My brother-in-law M. Nivet, who was at the Mont Dore, did not perceive the wind at the Pic de Sancy till midday, and I myself, upon the top of Puy-du-Dome, did not perceive its violence till one o'clock, and it was then only that the hail clouds arrived.

Perhaps I may have dwelt somewhat two much in detail upon these phenomena of which I was an eye-witness, but I believe I have collected some facts which are new to meteorology, a science which is not very rich in them even at the present day. I shall conclude by endeavouring to sum up, without, however, considering them as quite general, the observations which I have collected on the two occasions I have specified.

Conclusions.

1*st*, It appears that hail is formed during the prevalence of winds of *impulsion*, and not of those of *inspiration*, which, however, are generally more violent than the former. The storm of the 13th July 1788, concerning which M. Tessier made a report to *L'Academie des Sciences*, goes to confirm this opinion. Its velocity was nearly the same as that of July 28. 1835.

2*d*, Two strata of clouds, placed the one over the other, and two winds from different quarters, seem necessary for the production of hail.

3*d*, The hailstones do not pass from one cloud to another as Volta supposed; on the contrary, they advance with very great horizontal rapidity, and are urged forward by an extremely cold wind.

4*th*, Electricity nevertheless plays an important part in these phenomena, and according to all appearance, the superior cloud supports the inferior, heavily loaded with hailstones, and probably in a state of opposite electricity. There is probably also electrical repulsion among the hailstones which form the anterior extremity of the cloud, and which there present the whirlwind-like phenomenon which is so remarkable, and which I have twice observed in the most distinct manner.

5*th*, The hailstones do not strike against each other during their horizontal transport; and the noise which is heard, that rolling murmuring which is perceived at so great a distance, is owing to the combination of the individual sounds produced by each hailstone cutting the air with such swiftness. The clashing of any hailstones during their progress causes them immediately to descend.

6th, We are led to suppose that the hailstones are subjected to a rapid rotatory motion, but my opportunities have not yet enabled me distinctly to see it.

7th, The formation of hailstones, and their increase, appears owing to cold produced by the evaporation at their surface, on account of their great velocity. The hot air into which the anterior edge of the cloud penetrates, leaves a portion of water deposited upon them, a part of which is evaporated, and thereby congeals the other part, and thus forms concentric layers round the nucleus; the wind unceasingly transports the hailstones into new portions of air which is saturated with moisture, and the upper cloud supports them in their progress. But the lower cloud rapidly increasing in density by degrees falls down, and separates itself, more especially on its anterior portion, from the electrical cloud which supports it, till it reaches that point in which the action of this latter is almost nothing, the hailstones being all electrified in the same manner then strongly repel each other, and present that violent agitation which is perceived at the surface of the earth, and which repels in all directions those hailstones which the wind reunites by imposing upon them its own direction.

8th, The presence of long crystals at the opposite poles of the hailstones of the 28th of July 1835, would indicate that those which were placed at the equator were destroyed during their descent by the rotatory motion, or that this same movement hindered them from being formed upon the equatorial portion on account of their velocity, whilst they were easily grouped upon the poles.

9th, The water procured from the melting of the hailstones was far from being pure.

From this short review, we may see how necessary it is, especially in meteorology, to guard against too readily generalising facts. We must first observe, and then observe again, and we must wait till favourable occasions again place us in circumstances where we may see well, and study well, before we propose theories which, like those of Volta, can be supported only upon the reputation of a great name.

CLERMONT, 1*st February.*

The account which we have just been reading, and the precise results which the author deduces from the facts of which he was an eye-witness, appear to be of a nature calculated to throw much light on the still rather obscure subject—the formation of hail. As, moreover, they seem to agree with the view we have taken with regard to this phenomenon, we may be allowed, in concluding this article, to explain briefly the opinions which we have formed on the point.

Electricity, then, always accompanies the formation and fall of hail; but we are often asked if, in correct reasoning, this is a sufficient motive for admitting that the hail owes its existence directly or indirectly to the electricity:—Is it not possible that the same cause which determines the formation of the hail, at the same time developes the electricity, and that these two phenomena, instead of being connected as cause and effect, have no other alliance than that which depends on their having a common cause? The new opinions upon electricity with which science has within these few years been enriched, and more especially those which have obtained concerning the different circumstances in which this element may be developed, seem to confirm this conjecture; they seem also to derive a new degree of strength from the observations which have been made by M. Lecoq, and we shall therefore endeavour to develope our views.

The propagation of heat in any body is always accompanied by a development of electricity; and so far as there is any difference of temperature between the different parts of a body, so far is there a rupture of the natural electrical equilibrium. Now, if we consider a vertical column of atmospheric air during a serene calm, when no wind or cloud affects its physical condition, it will represent a body in which the temperature goes on decreasing from the base, which rests upon the earth, to its summit, which is the limit of our atmosphere. The difference of the temperature at the two extremes of this column must be very considerable, since it is admitted that the temperature of the atmosphere at its upper limit is at least —50° Cent. This difference ought also to be greater in summer than in winter, and in hot than in cold countries, since the temperature of the base of the column of air is determined by that of the soil upon which it reposes, whilst the temperature of its summit, being that of the limit of the atmosphere, is every where and at all times the same. This difference of temperature, which extends itself uniformly between all the points of the vertical mass of air, is necessarily the result of a continual propagation of heat from below upwards, and should consequently be accompanied by a development of electricity, the intensity of which should be increased in proportion as it ascends; that is to say, in proportion as the difference of temperature becomes greater. Now this is precisely what we learn from direct observation; we in truth find that the atmosphere, when it is calm and serene, is charged with a positive electricity, whose intensity is continually greater as we ascend. As to the negative electricity, which should accumulate at the base of the column, it is absorbed by the earth; for many observations of various kinds, and amongst others those of De Saussure and of Volta, demonstrate that the earth is endowed with a negative electricity.

Taking, then, this view of the matter, which reposes solely upon experiment, and perfectly accords with what we discover in a body heated at one of its extremities, the permanent electrical condition of the atmosphere will essentially depend on the manner in which the heat is distributed, and propagates itself throughout the air, and not upon evaporation, vegetation, or any other cause variable in intensity, and unequally distributed, to which the atmospheric electricity has been erroneously, as we think, attributed. It would be easy to demonstrate that this explanation can account with the greatest accuracy for the variations which atmospheric electricity undergoes; and in particular that it is not in opposition to the fact observed by De Saussure, and after him by other naturalists, that this electricity is stronger in winter than in summer. In truth, the greater intensity of atmospheric electricity during the winter is owing to this circumstance, that the electroscope, by means of which we perceive it, is at this period of the year put into communication with a larger portion of the atmosphere, on account of the moisture with which the air is then almost always saturated.

Should the atmosphere cease to be serene, or should a mass of air loaded with humidity, and carried by the wind, happen by its mixture with another mass of air to produce clouds, immediately a new distribution of temperature, and consequently of the electrical condition of the column of air, would be effected. To comprehend this result, we must remember that the solar rays which heat the surface of the earth traverse the atmosphere without sensibly heating it; and that it is the heat emanating from the earth which essentially determines the calorific state of the atmosphere. Now, when a vertical column of atmospheric air is divided into two sections by a layer of vapours, or by a cloud more or less thick, the terrestrial heat, not being able at least wholly to traverse this layer or this cloud, is sent back towards the earth whence it came, instead of penetrating through and beyond it. The portion of the column comprehended between the cloud and the soil preserves, therefore, this heat, whilst the portion included between the cloud and the limit of the atmosphere receives little or no heat; and the more the former of these two portions becomes hot, the more the second must be cold. Thus the column, instead of exhibiting a gradual decrease of temperature from its base to its summit, is found to be divided into two portions, having each a uniform but very different temperature. The cloud, more or less thick (or there may be many of them superposed on each other) which separates the two portions, is then very warm on its inferior surface, and very cold on its upper surface. It must of course, be very strongly electrified, negatively on the one side and positively on the other; and this electrical condition may be constantly destroyed by the neutralization of the two opposite electricities, which operate across the cloud itself, without however ceasing to exist, since the cause which produces it continues to act, and is ready to reproduce it as rapidly as it disappears. Here, then, we perceive the two strata of clouds of which M. Lecoq speaks; the wind ere long separates them; the atmosphere is speedily filled with clouds, some of which are negatively electrified, and the others positively, without including those which are electrical through the influence of others.

But why, it may be inquired, does this distribution of temperature, which produces so great an accumulation of electricity in the atmosphere, for the most part also produce the phenomenon of hail? In answer to this question, we must recollect, that the thicker the layer of clouds which intercepts the heat from the earth, the colder is the upper portion of the atmospheric column. Its temperature ought to be decidedly inferior to that of ice, for if the cloud completely interrupted all the terrestrial heat, it would be reduced to the temperature of the upper limit of the atmosphere, which is less than —50° Cent.; but there is no necessity it should be so low as this. It is enough that its temperature should be sufficiently low to congeal the drops of water at the upper part of the cloud, and so to freeze them as to render them capable, when impelled by the wind, as noticed by M. Lecoq, to traverse the layers of clouds, and descend towards the earth in consequence of their weight, at the same time condensing and freezing upon their surface the vapours through which they pass. Thus, the same cause which favours the abundant accumulation of atmospheric electricity in the cloud, will also be that which most assuredly effects the formation of hail. In particular, the immense heat which is usually experienced before a thunderstorm precisely indicates the existence of an invisible stratum, or of a cloud which, placed in some part of the atmosphere above the observer, intercepts the terrestrial heat, and sends it back whence it comes, instead of allowing it to proceed into free space; consequently, the higher the temperature is raised on the surface of the earth at any given time, the more it must be depressed on the other extremity of the column, or on the other side of the cloud, and consequently there must be the greater tendency both to the development of electricity, and to the formation of hail.

Again, the hailstones once formed, enlarge more or less, according to the length of the course which the wind causes them to pursue through the atmosphere; according to the quantity of water which they meet with in this course, and, finally, according to the temperature more or less low which they possess at the moment of their formation. In winter, if they be small, it is owing to the absolute quantity of water which the atmosphere contains being much less; and since at the moment of their formation the temperature is not lower than in summer, they must clearly condense a smaller quantity of water upon their surface, since they encounter less in their passage: they then form what we call *hoar-frost* (gresil).

We believe, therefore, that hail is formed in the most elevated regions of the atmosphere; where we besides know, from the appearance of halos, that small crystals of ice are often floating. The cloud which carries these small nuclei of hailstones on its upper surface descends obliquely towards the earth through the combined effect of its weight and of the dominant wind. In proportion as it descends, the hailstones increase in size, or diminish and are dissipated, according as they meet in their course clouds, or a dry atmosphere. In the former case, the cloud which conveys them becoming always more and more weighty, at length descends lower than all the others, as has been often remarked, and finally disperses itself upon the ground.

We shall not attempt at present to develope more in detail the ideas which we have now expressed. We should even have waited till a greater number of observations than those which we have already made had furnished us with a more solid groundwork, had not the publication of M. Lecoq presented a favourable occasion for their publication.

New Researches on the Organic Elements, and intimate Structure of Animal Bodies By G. R. Treviranus. Bremen, 1836.*

Having detailed in the preface the various expedients which were employed to render the microscopical examination of the tissues free from error, the author proceeds to detail in the first chapter his observations on the intimate structure and ultimate organic elements of cellular membrane. The result at which he arrives is, that in the vertebrated animals, cellular membrane is composed of cylinders or tubes, which he terms elementary cy-

* The above analysis of Treviranus's very interesting volume, with the observations, are from the last number of the Dublin Medical Journal, communicated to that work by Dr Graves.

linders. In the following instances, he found the diameter of these cylinders, and of the globules of the blood, expressed in decimals of the French millimetre, to be

	Cylinders.		Globules.
" Human Male,	0,002		0,004
Rabbit,	0,0011		0,0049
Hen,	0,003	Greater diameter,	0,01
	,0004	Lesser diameter,	0,006"

From this it appears that the diameter of the elementary cylinder is in general greater than that of the globules of the blood; I say generally, for some exceptions are mentioned, *e. g.* the tortoise and the frog. The elementary cylinders of cellular membrane are no doubt organized tubes, formed by a proper tunic, and we shall hereafter see that they are of great importance, not only being the groundwork of cellular tissue, but also as forming a system continuous with the lymphatics, and probably extremely influential in the circulation. For some valuable remarks on the microscopical observations of Mascagni, Milne Edwards, and others, I must refer to the treatise itself.

According to Treviranus, the cerebral mass, both cortical and medullary, consists of hollow cylinders containing a soft pulpy matter. These cylinders, extremely minute in the cortical substance, are somewhat larger in the medullary, and still larger in the nerves. He does not determine whether this enlargement be owing to an absolute increase of size in the same cylinders, or to their being joined by other cylinders, which thus coalesce into bundles; the latter, however, he seems to think the most probable supposition. In following the nerves towards their periphery, he found that they have a tendency to subdivide again; and he was able to prove, that in some parts at least of that periphery, the final nervous ramifications consist of cylinders derived from a continual subdivision of the larger nervous tubes into their primitive elementary cylinders. Thus in the retina, Treviranus has shewn, beyond the probability of error, that the following is the mode in which the nervous matter is disposed: after the optic nerve has penetrated the sclerotic and choroid, its cylinders or nervous tubes spread themselves out in every direction, either singly, or collected into fasciculi; each single cylinder and each fasciculus, on arriving at a certain point, quits its former course, and bends inwards towards the opposite and inner side of the retina; immediately on making this deflection, it passes through the meshes of the vascular network, derived from the central vein of the optic nerve. Before arriving at the inner surface of the retina, it passes likewise through a second vascular network, derived from the ultimate ramifications of the central artery of the retina. Each nervous cylinder, or tube, obtains at this place a sheath-like covering from the vascular coat of the retina, *and thus enveloped, terminates in the form of a papilla behind the vitreous humour.*

To ascertain the reality of this structure many precautions must be used, all of which are minutely detailed by Treviranus. As very fresh eyes only must be employed, these investigations will not always lead to very satisfactory results; the cylinders or tubes alluded to, lose their configuration very soon after death, and are resolved into minute globules. The papillary structure of the inner surface of the retina is consequently difficult to be verified in the human eye, unless the observer be very much accustomed to such investigations. Treviranus asserted positively, that the medullary or nervous layer of the retina is not prolonged, as some have believed, to the zonula. Near the border of the medullary lamina of the retina, he could distinctly perceive that it is covered by two membranes, the external homogeneous (membrana Jacobi) and the internal vascular. Both these coverings extend beyond the nervous lamina, and coming into contact with each other, acquire longitudinal plaits, and proceed between the vitreous humour and the choroid in the form of corpora ciliaria to the zonula. Between these there is no trace of proper medullary or nervous substance, and those who believe they have seen such, have been deceived by the white appearance of the vascular lamina. Treviranus remarks, that it would be in fact quite useless for the medullary or nervous portion of the retina to be prolonged quite beyond the limits of the space which lies within the reach of the rays of light.

The papillary termination of the extreme nervous cylinders which compose the retina, is confirmed by a similar distribution of the other nerves devoted to the senses, *e. g.* the acoustic and olfactory. In order to examine this structure of the retina it is necessary to select a clear day, when having taken small bits of the retina of an animal recently killed, and cut off with an oblique edge, unmoistened by water, and uncleansed from the particles of vitreous humour that adhere to their surface, you must place them on the object-glass of a compound microscope, which magnifies 300 times, gives an image of great clearness, and possesses a well illuminated field of vision. On the outer surface of these bits of retina, one may distinguish the radiated course of the medullary cylinders, and on their slanting edges may be seen the inward progress of the same, together with the regularly arranged, though crowded papillæ which cover the inner surface. The vascular network and the sheaths of the papillæ cannot be accurately observed on such fresh bits of retina, and consequently, to distinguish these, a particular mode of dissecting the retina, previously hardened by spirit of wine, is recommended by Treviranus. He gives the following measurements of the semidiameters of the papillæ in different animals expressed in decimals of a Paris line:

Frog, . . .	0,0014
Tortoise, . . .	0,0014
Starling, . . .	0,0004
Swan, . . .	0,0008
Rabbit, . . .	0,0007
Hedgehog, . . .	0,0002
Man, . . .	0,0006

These semidiameters, observes Treviranus, obtained by means of the most accurate micrometrical measurement, are in general several ten thousandth parts greater than the number which I have already proved to represent the maximum which the radius of the image of a luminous point on the retina must possess, in order to excite the sensation of one indivisible object. Now we cannot with certainty affirm it to be requisite for clear and distinct vision, that the concentrated rays from such a point should strike the retina only on one papilla; such, however, is probably the fact.

Each papilla is convex, and undoubtedly most sensibly affected, when the middle of the image, or that part where all the rays meet, coincides with the centre of the papillary elevation;

when the extremities of two pencils of rays impinge on one and the same papilla, they will not be both as distinctly seen as if one alone fell upon that papilla. We may therefore assert, that we are not very far from the truth in estimating the radius of the image of a physical luminous point on the retina as varying, when distinctly seen, within the limits of the decimals 0,001 and 0,0001, the Parisian line being unity. This inference, drawn from the size of the papilla, agrees pretty well, although not exactly, with the results of the optical calculations made by Treviranus, and of experiments performed by Mayer. It is worth noting, as connected with this part of the subject, how much more visible a black object on a white ground is, than a white object on a black ground at the same distance. Thus, a white square whose side equals a Parisian line, when fixed on a black ground, becomes invisible at the distance of about twenty-seven and a half inches when looked at in a weak sunshine; by ordinary day-light it ceases to be visible at twenty-four inches. Now a black object of the same dimensions, on a white ground, can be seen, under the same circumstances, at double that distance, viz. forty-eight inches.

Although it is probable that the papillæ of the retina, like all other nervous papillæ, are capable of becoming turgescent when their nervous energy is excited, yet their extreme minuteness prevents this turgescence from being of an amount capable of exerting any appreciable influence in diminishing the distance between the seat of the image on the papillæ and the lens. When we look attentively at an object it may become clearer, and may be actually seen with greater distinctness on account of this turgescence. Treviranus thinks that when both eyes are turned to an object with attention, it will be seen with greater distinctness for two other reasons also,—first, because under these circumstances the image falls exactly at the extremity of the optic axis in each eye; and, secondly, because the iris of each eye adapts itself to its task by regulating the size of the pupil, so as to accommodate it to the distance of the object, and the angle which the extreme rays from the object make with the axis of vision. Treviranus's researches concerning the ultimate structure of muscles, tendons, fibrous membranes, &c. are extremely interesting, and the discoveries he has made concerning

x 2

the presence or absence of the elementary cylinders, their size, appearance, and arrangement in the different tissues of the body, tend to throw much light on their individual natures as well as on their mutual relations. These matters, however important, I am reluctantly obliged to pass over in silence, in order to leave room to dwell particularly on his seventh chapter, which treats of the capillary vessels, and the roots or origins of the lymphatics.

"It has been believed by some, that the capillaries consist merely of passages percolating the cellular tissue, but destitute of any proper tunics. The following questions naturally occur concerning these vessels. Have the capillaries proper tunics? Is their diameter large enough to admit the entrance of the red blood? or if their trunks are sufficiently large, have they ramifications too small for this purpose? What is their connexion, or have they any direct communication with the vessels carrying red blood on the one hand, and the lymphatics on the other?

"One may easily distinguish with a strong magnifier that these vessels, which are large enough to admit a stream composed of several rows of globules, are marked with transverse as well as longitudinal lines externally. It is not always easy to determine whether the transverse lines are not mere folds or plaits; the longitudinal are formed by the outlines of the elementary cylinders of the cellular membrane which surrounds the tunics of the vessels. The transverse lines vanish when the vessel becomes too narrow to admit more than one row of the globules of the blood. Many vessels lose at this stage their cellular envelop, and retain only a single homogeneous tunic; others, on the contrary, retain a cellular coat. The simple vessels with but one tunic occur chiefly in membranes that consist entirely of a horny material, as, for instance, the epithelium which covers the free side of the corpus ciliare, the ciliary processes and ciliary ligament as far as the edge of the cornea. Similar vessels may be detected on the posterior surface and lateral edges of the capsule of the lens, and in the pecten of birds. In these situations they sometimes occur of considerable dimensions. Thus in the fox, this epithelium exhibits simple vessels with a diameter of from 0,04 to 0,05 (millimetre). In the amphibia vessels of this description may be detected in many other membranes.

Thus in the mesentery of a living frog spread upon glass, a microscope with a magnifying power of 300 exhibits branches of veins measuring from 0,014 to 0,028 (millim.) in diameter, which are perfectly transparent, and can only be distinguished at their edges, being as simple as if they were merely covered with a pellicle of pure water."

We now come to a part of Treviranus's treatise, which is of peculiar importance, and which I dwell on with more than ordinary pleasure, because it contains the fullest and most satisfactory proofs of the correctness of the opinion, *that the lymphatics are the veins of the white tissues,* an opinion which I was the first to promulgate.

"In some parts of the body I observed circumstances attending these minute and simple vessels, which render it probable that they terminate in tubes of such a degree of fineness that they are incapable of admitting the globules of the blood, and *consequently contain a serous fluid only.* This is the case with those branches of the central artery of the retina, that proceed to the posterior surface and lateral edges of the capsule of the lens. Even at their origin from the central artery, these vessels become at once incapable of carrying red blood, the very presence of which in these parts would be destructive to the transparency so essential to vision."

Treviranus then proceeds to detail minutely the situations in the eye, lungs, and other parts where he has observed these prolongations of the arteries containing none of the coloured blood; and indeed, from their diameter being much smaller than that of the globules, it is evident that they are incapable of admitting them. He then examines their structure, as compared with that of the minute lymphatics, and endeavours, I think, successfully to solve the long-disputed question as to the origin of the lymphatics, and their connexion with other portions of the circulating system. In the tortoise and cold-blooded reptiles, it is easy to make out with the aid of the microscope the continuity of the roots of the lymphatics, with the elementary cylinders of the cellular membrane. The latter gradually coalesce, and assume new appearances as they are about to become lymphatics. Now we have already seen, that extremely minute ramifications of the arteries are the bearers of the serous fluid to the white tissues of the body, from which, again, the lymphatics return the colourless blood; in fact, they are the veins of the white tissues. In most parts, and this refers to the cellular membrane generally throughout the body, the blood, after being used, and before it can be allowed to join the general circulation with safety, requires to be elaborated in glands in which a gradual change in the white blood is effected, and a gradual reunion with the red blood is promoted. In some few organs, as the brain and the ball of the eye, no lymphatics have been discovered. In these we may suppose, that some reason exists which prevents the white blood, after being used, from being necessarily injurious, and here, consequently, it is at once mixed with the blood of the veins, and the minute transparent vessels which receive it in the first instance do not therefore undergo the changes above mentioned, as preparatory to the formation of lymphatics. On the contrary, they proceed at once to join the minute veins. It is also very remarkable, that, in the organs in which no lymphatic glands are found, we have but one order of tissues, the white. This is the case in the eye-ball and brain.

Many reflections arise from the consideration of this fact; but I must return to our author, who has expended much labour and research on the lymphatic terminations in the alimentary canal, with respect to which, he has convinced himself that the villous flocculi, so abundant on the mucous membrane, are composed of minute tubes packed together, and continuous with neighbouring lymphatics. He thinks that some of his observations render it very doubtful whether the old opinion, that some of the lacteal terminations have open mouths, or rather perforations in their extremities, may not after all be correct.

But not only do we find lymphatic terminations in the villi of the mucous membrane and in its general surface, but also in its crypts and follicles, which Treviranus considers to be organs of absorption as well as of secretion. To conclude, Treviranus agrees with Müller and others, that the ultimate terminations of all the ducts of glands are, in the form of very minute tubes, collected into lobules, and terminating each in a cul de sac; the whole mass is connected by cellular membrane, and in this way is formed the parenchyma of every secreting organ. In some there is reason to believe, that the blood from which the materials for secretion are derived does not directly reach the parietes of the cul de sac, but only circulates in the interposed cellular membrane. In other glands, again, and the lungs present a striking example of this arrangement, the blood circulates in the parietes of the extreme ducts, which are, in the case of the lungs, the air-cells. I have already mentioned, that no elementary cylinders can be detected in certain tissues. It is very remarkable that this is the case with respect to the air-cells of the lungs, whose tissue, consequently, does not consist of condensed cellular tissue, or, as I once thought, and as Magendie supposed, of serous membrane. With respect to the bones of man and animals, the observations of Treviranus fully establish the existence of the laminated structure. The laminæ are arranged into several layers. In the bones of man these layers are folded and multifariously perforated, which is not the case in other animals. These folds in the laminæ, or their layers in the human bones, appear in the longitudinal section to resemble fibres, for which, when examined with a microscope, they are indeed easily mistaken, and the perforations in the laminæ produce an appearance of cells. The perforations are entirely distinct from the cavities or interstices discovered in the substance of human bones by the microscope, and which are found in consequence of the laminæ not lying in complete apposition with each other, for they leave between them interstices that are filled with fluid. The researches of Treviranus, Deutsch, and Purkinjè, have completely established the foliated structure of bones, and have also explained the circumstances which misled Scarpa, and induced him to form the opinion that the osseous texture is cellular. To conclude, it is necessary to observe, that Treviranus himself allows he has been anticipated in some of his investigations by the celebrated Ehrenberg, who has applied his practised powers of microscopical observation to similar researches. Treviranus, however, differs from Ehrenberg in several very important particulars, and has likewise pointed out the sources of Ehrenberg's errors.

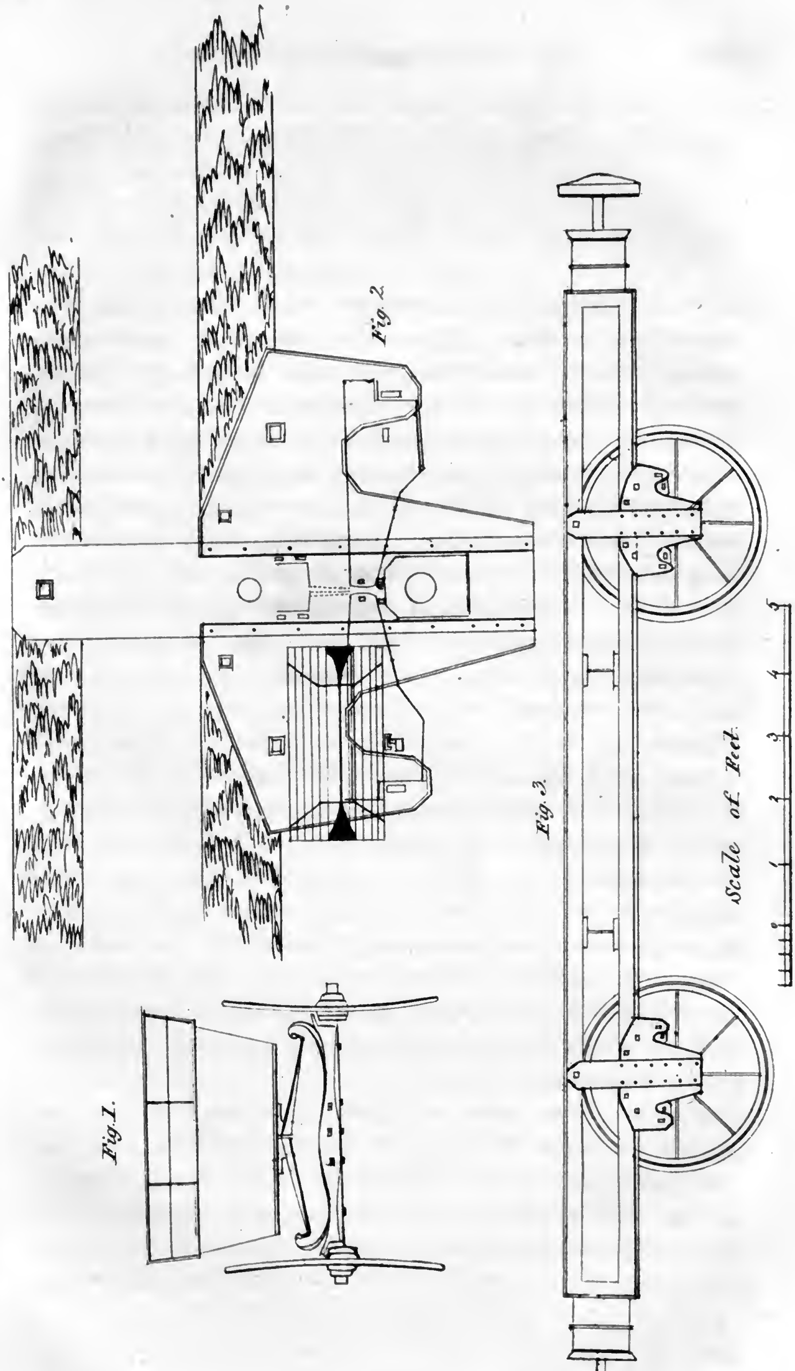
An Account of a recently invented Patent Spring, called, "The Safety Spring," and applicable to Carriages and Carts of every description *. By the Rev. R. J. Barlow. With an illustrative Plate.

When springs were first brought into practice, they were imagined to be useful merely to give ease to the traveller, and a certain degree of security to fragile articles; reflecting persons, however, quickly discovered them to be a great means of saving the carriage and lessening the draught, which latter is clearly proved in the works of Drs Helsham and Arnott. To save the road upon which we travel, has, since the formation of railways, become a consideration of the utmost importance, and so perfectly convinced are scientific men of the value of springs for that purpose, that the eminent engineer Mr Stephenson does not permit a single waggon to be run upon the Manchester and other lines under his direction without springs, although the weight and expense thereby added to each waggon is very considerable.

Hence, it is evident, that besides the comfort and convenience of springs, their chief advantages consist in saving the horse or engine, the carriage itself, and the road upon which it travels; and consequently the only argument against their being universally adopted by the Ordnance Department, and for farming carts, and common stage waggons, must arise from their being so expensive, so liable to break, and so ponderous when employed for heavy waggons, all which evils are in a great measure obviated by this invention, the peculiar properties of which may be thus briefly enumerated.

A greater degree of ease than those now in use;—almost perfect security against breaking under any circumstances;—a saving of weight upon railways to the amount of three-fourths, upon the common roads to the extent of two-thirds;—much cheaper;—a direct up and down motion, which prevents the swinging and rolling of the carriage, and consequently secures it against being

* Communicated to the Whitby Philosophical Society by the Rev. R. J. Barlow, the patentee, of Linden Grove, near Stokesby, Yorkshire, September 1836.



Forrester & Nichol, lith. Edin:

overturned under any extent of load ;—simple, capable of being repaired by the most indifferent mechanic,—may, upon emergency, be increased in strength for bad roads and heavy luggage ;—preserves the graceful appearance of the C spring so completely as to deceive the eye, and in all other cases is lighter and more elegant than those now in use.

That this spring is easier than those in general practice has been proved by comparing them with some of the best London manufacture for the space of a year, during which they were tried upon the worst description of roads : again upon the Whitby railway, where they have been in use for some months, they are found to have a much more pleasant motion than any hitherto employed. This is attributable solely to the spring being acted upon instantaneously, and completely without friction, which prevails to an enormous degree in the old springs, and renders them stiff or wooden to a great extent.

The superior security of this spring may be proved in this manner. The levers are constructed of two pieces of one-fourth inch plate iron, distant from each other two or three inches, and connected by one or more small blocks of wood, or, as in the case of the C spring, by one solid piece, all firmly rivetted together ; by this means the iron receives the strain edgeways, and, like the blade of a saw or knife, supported in such a position, it may, with little weight, be made equal to any load.

The spring itself never exceeds eight or ten inches in length, and consists of several steel plates of a lozenge shape, inserted in a kind of case called a stop (from its regulating the quantity of motion and stopping it at a certain given limit). This stop, by its tongue running through the centre, divides the plates into upper and under series, and contains, at each end, a rack or rest for every plate, which being supported at the extremities, the whole spring is pressed in the centre directly like an elliptic spring, and since every plate is supposed to be capable of bending more than it is permitted, it is not possible that the spring can ever break, because it is checked before it reaches the breaking point. Let it not, however, be imagined, that, being thus checked, the motion must be unpleasant, for if the spring be proportioned to the weight, it will never collapse but with such a shock as might endanger the carriage. It should also be mentioned, that whereas all springs are found to break, or set and lose their shape and original position if too heavily laden, this safety spring will, on the contrary, always return to the same height, when the load is taken off, be it ever so great; for, as has been shewn, it is impossible to break the spring, and when it has gone home, the strain then comes entirely upon the levers, which are made beyond any, even the utmost calculated weight or strain.

The difference of weight between these springs and the old ones, has been accurately determined at the Whitby Railway, and is as follows:—old springs for a 3 tons carriage, 372 lb.; new springs for a 3 tons carriage, 90 lb., being, as stated above, a saving of three-fourths in weight; but it is further to be remarked, that in the old springs, double the load requires double the weight of springs; whereas in this invention, the spring alone requires increase, directly as the weight, a few pounds additional to the levers being sufficient; thus, for instance, on the Whitby line, 3 tons take springs of 90 lb., but 156 lb. is sufficient for 6 tons, the levers being increased by only 6 lb.

The saving of expense is evident from the simple nature of the invention, because all the parts can, without loss of steel or iron, be cut in the cold state by heavy machinery, after which little hand labour is necessary: again it is to be considered that there is never more than one-third of the material employed, and that one-half of that is iron instead of steel.

The direct up and down motion will thus appear. In all cases, such as public coaches, phaetons with perches, and gigs, where the springs can be conveniently placed so as to run, not across, but along the axle, should the weight by a jerk be thrown to one side, the lever or levers on that side will work the springs, and those on the opposite side being freed from duty, will fall at the same time, by which means the carriage is compelled to descend at both sides alike, and therefore will move directly up and down only, so far as the springs are concerned; whereas with the present springs, when the weight is thrown to one side, the opposite side of the spring being relieved from pressure, kicks up, and tends much to make the carriage swing and overturn.

The facility of increasing the strength for bad roads or heavy luggage, will be understood by supposing the stop and its racks to be so arranged, as to be capable of receiving at the top and bottom one or more plates. This will materially increase the strength, and may be performed by an ordinary servant. In the levers no change is requisite, as they are always capable of working a spring of much greater power than would suit the carriage under ordinary circumstances.

Explanation of Plate II.

Fig. 1. exhibits the back of a phaeton hung so as to have the up and down motion, and avoid the side swing.

Fig. 2. represents the frame of a railway carriage, as seen with the patent springs and double guide-plates of one-fourth inch plate iron, made, as shewn, of several pieces rivetted together, or cut out of a single sheet. It is to be noticed, that the spring box plays within the guide-plate, and thus the dirt and dust are kept from the oil; or the piece rivetted on may be cut off, so as to allow the spring box to play outside, if preferred.

Fig. 3. exhibits, on a larger scale, the same kind of guide-plate, which is expressed as if transparent, to render the inner works visible. Thus the shape of the spring box, the *position* of the syphon, the shape and action of the levers are apparent; and there is also displayed on one side a single spring of a 6 tons waggon inserted in its stop or case, the dark line being the tongue and rest.

In the plan above drawn, the carriages and waggons are hung 8 inches lower than usual.

It may be necessary to add, that the small quantity of motion in the spring (not exceeding half an inch) is multiplied many times by the lever, before it is communicated to the carriage.

Notes regarding some of the Plants observed during the last year, in excursions from Edinburgh, especially some new stations for those of rare occurrence, or concerning the geographical distribution of a few which are more common. By Dr Graham.

The stations for plants in the neighbourhood of Edinburgh continue to be examined with great care; and an additional stimulus has been given to this lately, by the establishment of the Botanical Society, the zealous members of which gallop and creep in succession over every acre within the range of our local flora. Scintillations from this body have also taken place in various directions over Scotland. In August, Dr Gilbert Macnab and Mr Ashby made a very rapid journey through Forfarshire, Aberdeen, Sutherland, and Orkney. The success which attended the four days' journey with my pupils to the Mull of Galloway last year, excited a desire in some friends to accompany me to that district this year again; and by appointment we met at Stranraer on the 1st of August. At Lochnaw Castle we crossed the peninsula which separates Loch Ryan from the Irish Channel, walked a few miles along the west coast at this point, and afterwards examined, with considerable care, every promising part of the coast from the station of *Ononis reclinata* north of West Tarbet, round the Mull of Galloway, and by Drummore, Sandhead, Glenluce, Glasserton, Whithorn, Isle of Whithorn, Cragleton Castle, and Garlieston, to Wigton, where the party broke up on the 9th. I afterwards visited the shores at Kirkcudbright and Balmaehead. From these sources the very brief observations which I am now to make are derived.

Acinos vulgaris.—*Near Aberlady,* Dr Balfour; *Queensferry Hills,* Dr Dewar. ——Aira cristata.—Extremely common on the dry hilly pastures near Edinburgh. We remarked that it was very scarce in similar situations in Galloway.——Ajuga pyramidalis.—Plants were given to Dr Macnab, and two stations pointed out to him in Orkney by Dr Alexander R. Duguid, but the season was too far advanced for him to gather it.——Allium oleraceum.—*Near Montrose,* Dr Balfour and Dr Macnab.——Alyssum calycinum.—I last year noticed the discovery of this plant in two Scottish stations; I I may notice now in confirmation of the propriety of restoring this genus to the British Flora, that Mr Watson informs me the same species has been found in *Leicestershire.*——Artemisia maritima.—*Mouth of the Tyne,* near Tyningham, in great abundance, Dr Balfour. In various places to the eastward of *Burrow Head in Galloway,* but nowhere that we observed so abundant as near *Cragleton Castle,* and at *St Mary's Isle.*——Asperugo procumbens.—Long ago observed near Guillon Links, but not for several years, till this season, when it was first noticed near *Luffness,* by Mr Forbes. It grew there in abundance in a neglected field.——Asplenium alternifolium. —Very sparingly *near Dunfermline,* Dr Dewar. *Dunkeld,* Dr Macnab.—— Bartsia viscosa.—This plant, so abundant on some parts of the west coast of Scotland, was not seen last year in Galloway, and only sparingly this year near *Port William.*——Bromus arvensis.—Two stations only in the neighbourhood of Edinburgh were known for this plant last year. It has been since observed in *many fields around Edinburgh*, and particularly *near Newhaven.*——Bromus velutinus, β.—This variety, no doubt the same as that found by Mr Johns in Cornwall, as I have ascertained by comparison with specimens which he kindly gave to me, was gathered near *Sandhead, Galloway.*——Calamagrostis epigejos.—*Below the Bridge of Dee, Braemar*, Dr Macnab.——Campanula rapunculoides.—*Near Montrose*, Mr A. N. Balfour.——Campanula trachelium.—*Near Donnibristle*, Rev. A. Robertson.——Carex capillaris.—Observed by Dr Macnab in great abundance at the mouth of the river Naver, and to form a large proportion of the turf on the knolls in Durness.——Carex extensa.—It was observed only in one station in *Galloway* last year. This season, as we went farther to the eastward than we did then, we found it in great profusion in many places among the stones close to the highwater-mark.——Carex fulva.—*Subalpine situations near Edinburgh*, not unfrequent, though not before observed in the district.——Carum verticillatum.—This plant, extremely abundant in some parts of the west coast of Scotland, was not observed in Galloway last year, nor this, excepting in the *neighbourhood of Wigton and Kirkcudbright.*——Cladium mariscus.—As this plant is stated on the authority of the late Mr J. Mackay to be "plentiful in Galloway," every pool in our route this year and last were examined for it, and information sought every where regarding it. It was neither seen nor heard of; and a suspicion was necessarily excited that it does not grow, or at least is not "plentiful" in the district. Can Galloway have been inadvertently given, instead of Galway, where it does grow? If so, the only known station for this plant now in Scotland is in *Sutherlandshire.*——Eleocharis multicaulis.—This plant has before been found in the neighbourhood of Edinburgh, but Dr Dewar has found it more abundantly *near Dunfermline.*——Erodium maritimum.—*Coast near Port William*, Galloway, abundant. New I believe to Scotland.——Fedia dentata.—In many places *around Edinburgh*, in Fife, and in *Galloway.*——Genista tinctoria.—In great profusion in Galloway in many places, *from Glasserton to Balmae Head.*——Habenaria chlorantha.—Very common in *subalpine grounds near Edinburgh.* On the *banks of the Reservoir, towards Airdrie*, and in *Galloway.* Whether specifically distinct or not, it is easily distinguished from the H. bifolia, by its more robust growth, greenish-yellow flowers, broader, more connivent segments of the perianth, and its diverging anther-lobes.——Helosciadium nodiflorum.—In almost every ditch throughout our whole route in *Galloway;* but not, as far as I know, hitherto observed on the east of Scotland.——Hieracium umbellatum.—*Valley of Clova*, Dr Macnab.——Juncus maritimus.—One station only was visited by the party in Galloway last year. This year we found it in very many, along a great part of the coast in our whole route, and in extreme abundance.——Jungermannia Mackaii.—*Near Drummore*, Dr Greville. The plant has not before been observed in Scotland.——Lamium intermedium.—In many situations *around Edinburgh*, and *near Sand Head, Galloway.*——Lepidium Smithii.—Road-side near *Whitekirk, Berwickshire*, Dr Macnab.——Linnæa borealis.—*Neighbourhood of Edinburgh.* A single round patch of this plant, six or eight feet across, was observed this summer, probably the first time, near Edinburgh, for Dalhousie Woods, it is believed, have been erroneously marked as a station.——Lycopus Europæus.—In great profusion along the shores to the southward and eastward of Glenluce.——Lysimachia vulgaris.—*Near Parkhall, Stirlingshire,* Mr Learmonth. I do not know any station nearer Edinburgh for this plant, which was pointed out to me on the banks of Loch Lomond by Mr Joseph Hooker.——Malva moschata.—I doubt very much whether this plant ought to be considered other than an outcast of gardens in the neighbourhood of Edinburgh, and in many other parts of Scotland; but *along the shores of Galloway* it is certainly native.——Melilotus leucantha.—In vast abundance in a deserted quarry *on the banks of the Union Canal near Edinburgh.*——Nuphar pumila.—In a loch on the south side of the road leading from Aviemore to Inverness, and 2½ miles from the former, Dr Macnab.——Œnanthe pimpinelloides.—We found both last year and this extremely common in the marshes on the shore nearly along the whole of our course in *Galloway,* but it had not been observed on the east coast, till Mr Campbell gathered it in great abundance *near Dunbar.*——Polygonum maritimum.—In great abundance in many places along the *south shore of Galloway.*——Rhinanthus major.—*Sandy fields on the coast between Arbroath and the Sands of Barry,* frequent, Dr Macnab.——Scirpus Savii.—In far greater abundance than it was found last year in Galloway, by the road-side, *near Drummore, and at other places on the south coast.*——Senecio saracenicus.—*Near Bathgate,* Dr Simpson. *Near Dunfermline,* Dr Dewar. I have not seen the former station, but I have gathered the plant in the second, and see no reason to doubt its being wild. It is in sparing quantity, but is very far removed from any garden.——Silene Anglica.—*In a field near Culloden, in great abundance,* Mr John Forbes.——Silene conica.—*Montrose,* Dr Macnab.——Silene noctiflora.—*Aberlady; Fife,* Mr Stark; Dr Dewar.——Sium latifolium.—*Wet ground on the shore, a mile east from Dunbar,* Mr Campbell.——Solanum nigrum.—This is a scarce plant in Scotland, and it may be doubted whether it is ever met with there, except when it has been introduced. We found it densely covering the ground on some large defined patches, where sea-weed had been dried, *near Sandhead and Port William.*——Specularia hybrida.—Sparingly in *a field at Dirleton,* Dr Balfour. Abundantly in a *neglected field at Luffness.*——Symphytum officinale, var. flore fere nigro.—This very remarkable variety, which was some time ago sent to the Botanic Garden by Mr Smith of Ayr, and retains its peculiarity in cultivation, we found in considerable quantity by the road-side *between Loch Ryan and the Irish Channel.*——Thalictrum alpinum.—I have gathered this plant on the shore at Durness, but only on the mountains to the eastward, and considered it an evidence that a western longitude brought alpine plants to the shore in the same latitude; but this year it has been found by Dr Macnab, growing with Draba incana on the shore, so far east as the *mouth of the river Naver.*——Trifolium ornithopodioides.—*Shore near Inverkeithing,* Miss Robertson. Very probably this plant is more generally diffused than we at present know it to be. I and many of my sharper-eyed friends must many times have trode over the ground where Miss Robertson has this summer found it.——Triticum loliaceum.—*North Queensferry*, Dr Dewar. I am not aware that this plant has been mentioned as native of the west of Scotland. We found it in much greater abundance than I ever before saw it to grow, in many places along the *south shore of Galloway*, from Drummore eastward.——Tulipa sylvestris.—Observed this spring near Edinburgh, in a field which certainly never had been cultivated. If brought there by accident, it must have been many years ago, for it now forms a large dense patch.——Veronica Buxbaumii.—*Near Dunfermline*; *near Inverkeithing*, Dr Dewar. I have not seen either station, but am inclined to believe with Dr Dewar that it has not been introduced, because it has only been lately cultivated, and probably it has not been so yet, near either of these stations.——Viola flavicornis—In several stations near Edinburgh, abundantly. First observed by Mr Forbes.

After returning from Galloway, I went with Sir William J. Hooker to Ben Lawers, and the mountains in the vicinity of Killin. The alpine plants were in worse condition than I ever saw them—no doubt in consequence of the singular and particularly bad weather of this summer (as in courtesy to the season it must be called), during which warmth and perfect drought prevented vegetation in May, and cold, rain, and wind destroyed it in July.

Proceedings of the Royal Society of Edinburgh.

1836, *March 21st.*—Rev. Dr. Chalmers, Vice-President, in the Chair. The following Communications were read:

1. Observations and Experiments on the Coloured and Colourable Matters in the Leaves and Flowers of Plants, particularly in reference to the Principles upon which Acids and Alkalies act in producing Red and Yellow or Green. By Dr Hope, V. P. R. S. E., F. R. S. [Continued from 18th Jan.]

After premising some general remarks respecting the object of research, and enumerating the various authors who had written upon the subject, Dr Hope explained various terms which were to be used in the discourse. To the various coloured matters presented by the leaves and flowers of plants, De Candolle had applied the denomination of Chromule, which term he meant to adopt.

There resides in the same parts of plants, in addition to the chromule, some matter probably destitute of colour, which becomes red by the action of acids, and yellow or green by the action of alkalies. To it Mr Ellis gave the name of colourable matter, which the author changes to Chromogen. When an acid, added to a vegetable infusion, causes a red colour, and an alkali a yellow or green, it has beenthe universal opinion that both sets of agents act upon one and the same colourable principle. The leading object of this paper is to shew that the Chromogen, or colourable principle, is not an individual substance; and that there are two distinct principles, one which forms the red compound with acids, which he denominates Erythrogen; and another, which affords a yellow compound with alkalies, which he calls Xanthogen.

To establish that opinion, Dr Hope made many experiments on the leaves and flowers of plants, with various reagents, principally water, alcohol, acids, and alkalies: and has exhibited the results in the compendious form of tables. The first table presents the result of experiments on the leaves of many plants; and the general result from them, in regard to the special object of inquiry, is, that in addition to the green Chromule, denominated Chlorophyle by many writers, they all contain Xanthogen, and that none of them, excepting those which have some tint different from the green, contain Erythrogen.

The second table exhibits the result of the action of the reagents upon white flowers, all of which, to the number of about thirty, gave proofs of their containing Xanthogen, but no Erythrogen nor tinted Chromule of any kind.

The third table displays the results with yellow flowers, from which the general inferences are, that the yellow Chromule varies in its nature in different flowers; that all those subjected to experiment contained Xanthogen, and none of them Erythrogen.

The fourth table exhibits the experiments with red flowers, and affords the general conclusions, that while the red chromule shews considerable variety of character, red flowers contain both Xanthogen and Erythrogen in abundance.

The fifth table exhibits the results with twenty blue flowers, and presented the general observations, that the blue Chromule varies in its character in different blossoms, particularly in shewing very different degrees of solubility in water and alcohol, and in some producing coloured, and in others colourless, solutions in both menstrua; and that they contain both the colourable principles of Xanthogen and Erythrogen.

The sixth table relates to ten orange flowers, which equally shews that the orange Chromule differs much in one plant from another, and that they contain both colourable principles.

The seventh table relates to twenty purple flowers, and afforded the same conclusions as the preceding.

The eighth table exhibits the experiments made upon the tinted Chromule found in other parts of plants, beside the corolla of the flowers, *e. g.* the calyx, bractea, the coloured leaves of plants, fruits, and surface of the roots, all which comported themselves as the corresponding coloured Chromules of the flowers do.

Litmus presented a solitary example, but a very interesting one in this inquiry, of a substance abounding largely in Erythrogen, but containing no Xanthogen.

A table was also presented, exhibiting the general facts relative to the power of sulphurous acid in decolorizing the Chromules of plants. This acid, whether employed in its gaseous or liquid form, does not decolorize the Chlorophyle of leaves. It does not affect white flowers. It did not decolorize any one of about a score of yellow flowers submitted to its action. Of thirty or forty red flowers it decolorized all, with the exception of two or three. Of twenty blue flowers, two, the *Commelina cœrulea,* and the blue *Centaurea Cyanus,* resisted its blanching power. It decolorized some of the orange-coloured flowers, but rendered others of them of a bright yellow: it decolorized all the purple flowers that were tried, with the exception of purple *Centaurea Cyanus,* which it rendered blue, and the purple *Scabiosa atropurpurea,* which it reddened. It affected the tinted Chromule occurring in other parts of plants than blossoms; it completely blanches the internal leaves of the red cabbage, which are of a bright purple red, while it renders the external bluish-purple leaves green. It turned to green various leaves, which possessed red tints, whether general over the whole leaf, as the red beet, or partially diffused, as in some species of Dracæna. It decolorizes some fruits, while others resist its action.

Along with the general facts, the various hypotheses respecting the origin of the different Chromules, and sources of the differences amongst them, the autumnal coloration of leaves, &c., were brought under consideration.

Lastly, a detail was given of the influence of light in the production of different Chromules, shewing that it is indispensable for the production of the Chlorophyle of leaves, and the tinted Chromules formed during the autumnal coloration of the same parts; and that it is not indispensable for the formation of some of the finest tints of flowers and fruits, if essential for any.

The paper terminated with the following general conclusion: That there exist in plants two distinct colourable principles, two species of Chromogen, one which generates red compounds with acids, and denominated Erythrogen; and another which forms yellow compounds with alkalies, called Xanthogen. That these two principles occur together in red and blue flowers, and in the leaves of a few plants which exhibit the former of these tints; that all green leaves, all white and all yellow flowers, and white fruits, contain Xanthogen alone; that Litmus abounds in Erythrogen, but has no Xanthogen; that the Chromules of different tints may be generally considered distinct vegetable principles, or compounds having their own proper hue; sometimes intimately blended, or chemically combined with Chromogen; at other times having no connexion with it; that they are also occasionally, but not frequently, compounds of Chromogen with acids or alkalies.

The second part of the paper will comprehend an inquiry into the special character and properties of the two colourable principles, the Erythrogen and Xanthogen.

2. On Dyspnœa, and other Sensations experienced on the summit of Mont Blanc. By Martin Barry, M. D. Communicated by Dr Alison.

3. Mr Smith of Jordanhill made a verbal communication regarding a change in the Relative Levels of the land and sea on the west coast of Scotland; recent shells having been found even at the height of 70 feet.

4. Professor Forbes communicated a memorandum respecting the Polarization of Heat.

April. 4.—Dr Hope, Vice-President, in the Chair. The following communications were read:

1. On Paracyanogen and its Compounds. By James F. W. Johnston, Esq.

2. On the newly discovered microscopic Entozoon infesting the Muscles of the Human Body. By Dr Knox.

April 11.—Dr Abercrombie, Vice-President, in the Chair. The following communications were read:

1. On the Origin of the Adjective. By Professor Pillans.

2. On Single and Correct Vision by means of Double and Inverted Images on the Retinæ. By Dr Alison.

April 18.—Sir T. M. Brisbane, Bart. President, in the Chair. The following communications were read:

1. Observations on the Chemical Nomenclature of Inorganic Compounds. By Dr Hope.

2. Account of a Visit to the Governor of the Chinese Frontier Town of Mai-ma-tchin, bordering with Kiachta, in Siberia. By a General Officer in the Russian Service. Communicated by Mr Robison, Sec. R. S. Edin.

May 2.—Sir T. M. Brisbane, Bart. President, in the Chair. The following papers were read;

1. An Attempt to ascertain the Relative Positions of the Athenian and Syracusan Lines before Syracuse, from the Description of Thucydides. By Professor Dunbar.

2. Researches on Heat. Second Series. By Professor Forbes.

3. Dr Knox exhibited some specimens to prove that the teeth of the Cachalot are devoid of enamel.

PROCEEDINGS OF THE BRITISH ASSOCIATION AT BRISTOL IN AUGUST 1836.*

GENERAL OFFICERS.

President.—The MARQUIS OF LANSDOWN; but owing to his unavoidable absence, his place was taken by the MARQUIS OF NORTHAMPTON.
Vice-Presidents.—Rev. W. D. CONYBEARE, F.R.S.—JAMES C. PRICHARD, M.D. F.R.S.
General Secretaries.—Rev. WILLIAM V. HARCOURT, F.R.S.—FRANCIS BAILLY, F.R.S.
Assistant General Secretary.—Professor PHILLIPS of King's College.
Treasurer.—JOHN TAYLOR, Esq. F.R.S., F.G.S.

LOCAL OFFICERS.

Treasurer.—GEORGE BENGOUGH, Esq.
Secretaries.—Professor DAUBENY.—V. F. HOVENDEN, Esq.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President.—Rev. W. WHEWELL, F.R.S.
Vice-Presidents.—Sir D. BREWSTER.—Sir W. R. HAMILTON.
Secretaries.—Professor FORBES.—W. S. HARRIS, Esq. F.R.S.—F. W. JERRARD, Esq.

The following Memoirs were read, and statements made, and more or less extensively discussed:—1. Notice regarding the progress made in the construction of a Lens of Rock-salt, by Sir D. Brewster. 2. Account of the recent Tide Observations made at the ports of London and Liverpool, by Mr Lubbock. Mr Lubbock stated, That through the indefatigable exertions of Mr Dessiou, considerable progress had been made in the reduction of the observations made at Liverpool by Mr Hutchinson. The diurnal inequality or difference between the superior and inferior tide of the same day, which in the Thames was very inconsiderable, if not insensible, was found at Liverpool to amount to more than a foot; a matter upon which the learned gentleman laid considerable stress, as calculated to lead to important practical results. The object of these reductions was to compare the results of theory with these observations, and with those of Mr Jones and Mr Russell made at the port of London: The principal objects of comparison were the heights of the several tides, and the intervals between tide and tide; and these were examined in their relations to the parallax and de-

* Report prepared from accounts in Athenæum, Felix Farley's Bristol Journal, and private communications.

clination of the Moon and of the Sun, and in reference to local, and what may in one sense be called accidental causes, as storms, &c. Of this latter, one of the most curious, as well as important, is the effect of the pressure of the atmospheric column. The author stated, that M. Daussy had ascertained, that at the harbour of Brest a variation of the height of high-water was found to take place, which was inversely as the rise or fall of the barometer, and that a fall of the barometer of 0.622 parts of an inch, was found to cause an increase of the height of the tide, equal to 8.78 inches in that port. To confirm this interesting and hitherto unsuspected cause of variation, had been one principal object of the researches of the learned gentleman; and, at his request, Mr Dessiou had calculated the heights and times of high-water at Liverpool for the year 1784, and compared them with the heights of the barometer, as recorded by Mr Hutchinson for the same year: and by a most careful induction, it had turned out that the height of the tide had been on an average increased by one inch for each tenth of an inch that the barometer fell, *cæteris paribus;* but the time was found not to be much, if at all affected. Mr Lubbock then proceeded to examine the semi-menstrual declination and parallax correction, and stated that the result was a remarkable conformity between the results of Bernouilli's theory, and the results of observations continued for nineteen years at the London Docks. But to render the accordance as exact as it was found to be capable of being, it was necessary to compare the time of the tide, not with that transit of the Moon which immediately preceded it, but with that which took place about five lunar half days previously. To explain this popularly, Mr Lubbock stated, that, however paradoxical it might appear to persons not acquainted with the subject, yet true it was, that, although the tide depended essentially upon the Moon, yet, any particular tide, as it reaches London, would not be in any way sensibly affected, were the Moon at that instant, or even at its last transit, to have been annihilated; for it was the Moon as it existed fifty or sixty hours before which caused the disturbance of the ocean, which ultimately resulted in that tide reaching the port of London. The author then exhibited several diagrams, in which the variations of the heights of the tide, as resulting from calculations founded upon the theory, were compared with the results of observations. The general forms of the two curves which represented these two results, corresponded very remarkably; but the curve corresponding to the actual observations, appeared the more angular or broken in its form, for which Mr Lubbock satisfactorily accounted, by stating, that the observations were neither sufficiently numerous, nor sufficiently precise, from the very manner in which they were taken and recorded, to warrant an expectation of a closer conformity, or a more regular curvature. When it is recollected that the observations are at first written on a slate, and then transferred to the written register, by men otherwise much employed, and whose rank in life was not such as would lead us to expect scrupulous care, it was not to be wondered at, if occasionally an error of transcript should occur, or even if the observation of one transit was set down as belonging to the next. When to these circumstances it was added, that the tide at London was in all probability, if not certainly, made up of two tides, one having already come round the British Islands, meeting the other as it came up the British Channel, it was altogether surprising that the coincidence should be so exact; and it was one among many other valuable results of these investigations, that it was now pretty certain, that tide tables constructed for the port of London by the theory of Bernouilli, would give the height and interval with a precision quite sufficient for all practical purposes, and which might be relied on as sufficiently exact, when due caution was used in their construction, and the necessary and known corrections applied. In conclusion, Mr Lubbock stated that the observations for the port of London had now been continued from the commencement of this century, and those for Liverpool, as we understood, about twenty-five years.

3. Mr Whewell gave an account of the proceedings of the committee appointed to fix lines of the relative level of sea and land. He commenced by saying, that as in the discussion of the relative level of land and sea, the tides of the ocean were an important element, he should preface the remarks upon that subject, which he intended to submit, by making a few observations upon the very valuable communication of his friend Mr Lubbock. This communication he highly eulogized, and pointed out to the Section the importance of many of the conclusions, should they prove hereafter to be generally applicable: but he expressed strongly his fears that this would not be the case. Observation had, in the instance of the tides, far outstripped theory, for many reasons, which it would be impossible to detail; but among the most prominent were the complexity of the problem itself involving the astronomical theories both of the sun and moon; the masses of these bodies; the motions of disturbed fluids, and local causes tending to alter or modify

the general geographical effect of the great tide-wave at any particular place. It was upon a careful review of these considerations, that he was led to fear that it would be still many years before theory would become so guarded and supported by local observations, as to afford a sufficiently correct guide to be implicitly relied on in these speculations. He instanced the tides of the Bristol Channel, which, in consequence of their excessive magnitude, afforded magnified representations of the phenomena by which the deviations become more remarkable. At the port of Bristol, the tide rose to a height of fifty feet, while towards the lower part of the Channel it only rose twenty, and along other parts of the coast not quite so high. The most striking of Mr Lubbock's conclusions was that by which it appeared that the ocean assumed the form of the spheroid of equilibrium, according to the theory of Bernouilli, but at five transits of the moon preceding the tide itself. By the calculations of Mr Bent, however, it would appear, that although the observed laws of the tides at Bristol might be made to agree with Bernouilli's theory of equilibrium tides, by referring them to a certain anterior transit,—so far as the changes due to parallax were concerned, as also as far as those due to declination were concerned,—yet it turned out that this anterior period itself was not the same for parallax as for declination. The two series of changes have not therefore a common origin or a common epoch; so that in fact there is no anterior period which would give theoretical tides agreeing with observed tides; and, therefore, at least the Bristol tides do not at present appear to confirm the result obtained by Mr Lubbock from the London tides. Mr Whewell then illustrated these views by diagrams, by the aid of which he explained to the Section the luni-tidal intervals, and the curve of semi-menstrual inequality—(this latter term, and the doctrine connected with it, was introduced into the subject of the tides by the Professor himself). Professor Whewell then proceeded to the question more immediately before him—the proceedings of the Committee appointed to fix the relative level of the land and sea, with a view to ascertain its permanence, or the contrary. He observed, that the Committee had not taken any active practical steps for the important purposes for which they were appointed, because they had met with many unexpected difficulties requiring much consideration. It was, however, intended to appoint a Committee for the same purposes, who should be furnished with instructions founded upon the views at which the former Committee had by their labours and experience arrived. One method proposed was, that marks should be made along various parts of the coast, which marks should be referred to the level of the sea; but here the inquiry met us in the very outset—what is the proper and precise notion to be attached to the phrase the *level of the sea?* Was it high water-mark or low water-mark? Was it at the level of the mean tide, which recent researches seemed to establish? In hydrographical subjects the level of the sea was taken from low water, and this, although in many respects inconvenient, could not yet be dispensed with, for many reasons, one of which he might glance at —that by its adoption, shoals, which were dry at low water, were capable of being represented upon the maps as well as the land. The second method proposed appeared to be one from which the most important and conclusive results were to be expected. It consisted in accurately levelling, by land survey, lines in various directions, and by permanently fixing, in various places, numerous marks of similar levels at the time; by the aid of these marks, at future periods, it could be ascertained whether or not the levels, in particular places, had or had not changed, and thus the question would be settled whether or not the land in particular localities was rising or falling. Still further, by running on those lines, which would have some resemblance to the isothermal lines of Humboldt, as far as the sea coast, and marking their extremities along the coast, a solution would at length be obtained to that most important practical question,—what is the proper or permanent level of the sea at a given place? Until something like this were accomplished, Mr Whewell expressed his strong conviction of the hopelessness of expecting anything like accuracy in many important and even practical cases. As an example, he supposed the question to be the altitude of Dunbury Hill referred to the level of the sea: if that level of the sea were taken at Bristol, where the tide rises, as before stated, fifty feet, the level of low water would differ from the same level on the sea coast at Devonshire, where the sea rises, say eighteen feet; and supposing, as is most probable, the place of mean tide to be the true permanent level by no less a quantity than sixteen feet, which would therefore make that hill to appear sixteen feet higher, upon a hydrographical map constructed by a person taking his level from the coast of Devonshire, than it would appear upon the map of an engineer taking his level at Bristol. In the method proposed, the lines of equal level would run, suppose from Bristol to Ilfracombe in one direction, and from Bristol to Lyme Regis in the other, and by these a common standard of level would soon be obtained for the entire coast.—Professor Sir William Hamilton rose to express the sincere pleasure he felt at the masterly expositions of Mr Lubbock and Professor Whewell. One conclusion to which Mr Lubbock had arrived was to him peculiarly interesting, viz. that by which it appeared that the influence of the Moon upon the tides was not manifested in its effects until some time after it had been exerted, for a similar observation had recently been made by Professor Hansteen respecting the mutual disturbances of the planets.—Mr Lubbock rose to say, that the agreement between the results calculated from the theory of Bernouilli and those obtained from actual observation, were much more exact than Professor Whewell seemed to imagine; in truth, so close was the agreement, that they might be said absolutely to agree, since the difference was less than the errors that might be expected to occur in making and recording the observations themselves.—Mr Whewell explained that he wished to confine his observations to the Bristol tides, as these were the observations to which he had particularly turned his attention; and, with respect to which, he should be able, at the present meeting, to exhibit diagrams to the Section, which he felt confident would amply bear out his assertions respecting these tides.—Mr Lubbock stated, that so near, indeed so exact, had been the coincidence between the observations made at London and Liverpool, and the theory, that he was strongly inclined to believe that that coincidence would be found at length to be universal.—Professor Stevelly inquired, from Mr Lubbock, whether he did not think it quite possible that local causes might exist, which would be fully capable of producing the deviations from the theory of Bernouilli; as, for instance, in the case of Bristol, so ably insisted upon by Professor Whewell, where the causes of the extraordinary elevation are the land-locking of the tide-wave as it ascends the narrowing channel, and the reflexions of other tide-waves from several places. Now, particularly in the case of reflex tides, may it not so happen, and does it not, in fact, happen in several places, that they bring the actual tide to a given port at a time very different from that at which the influence of the Moon and Sun, if unimpeded, would cause it to arrive, and thus separate, as Professor Whewell had stated, the origin or epoch of the variations due, suppose to parallax and declension, and even cause other deviations from Bernouilli's theory?—Mr Lubbock replied, that unquestionably it might so happen; but, in his opinion, the discussion of a few observations, like those made at Bristol, could not be expected to point out very exactly the origin or epoch of either of the variations of parallax or declination, with sufficient exactness, to furnish secure data for determining that they did not correspond to any one common previous transit of the Moon.—Professor Whewell exhibited some diagrams, which tended to illustrate his view of the question; and, in particular, he drew the attention of the Section to the circumstance, that the diurnal inequality, which was now beginning to be observed, decided the question, inasmuch as its epoch could not by any means be attributed to the same previous transit of the Moon to which the others were referred—Mr Frend congratulated the meeting upon the prospect now held out of determining precisely that most important practical question, the true level of the sea.

Mr Lubbock next made a communication respecting the formation of an empirical lunar theory.

Professor Sir William Hamilton read his report on Mr George B. Jerrard's mathematical researches, connected with the general solution of algebraic equations.

Professor Phillips read his Report of the Experiments instituted with a view to determine the Temperature of the interior of the Earth.

Professor Forbes gave an account of the experiments he had directed to be made on subterranean temperature at the Lead Hills in Scotland.

The Rev. Mr Craig read a paper on Polarized Light.

Section B.—Chemistry and Mineralogy.

President—Rev. Professor Cumming.
Vice Presidents—Dr Dalton, Dr Henry.
Secretaries—Dr Apjohn, Dr C. Henry, W. Herapath, Esq.

Mr Watson read a paper on the Phosphate and Pyrophosphate of Soda.

Mr Ettrick noticed a new form of Blowpipe, by which the blast of the common blowpipe was made as equable as that produced by water-pressure.

Mr Herapath then drew the attention of the section to the composition of Bath Water, as recently determined by him, and detailed the methods of analysis which he adopted, and the results at which he arrived.

Dr Hare next described his apparatus for the analysis, on the plan of Volta, of Gaseous Mixtures.

Mr Herapath read a paper on the Theory of the Aurora Borealis. He stated that he always found this phenomenon to be low in the atmosphere, and in connection with clouds. Hence he inferred that it is occasioned by electricity passing from the clouds.

Section C.—Geology and Geography.

President—Rev. Dr Buckland.

Vice Presidents—R. Griffith, Esq., G. B. Greenough, Esq.
(For Geography) R. I. Murchison, Esq.

Secretaries—W. Sanders, Esq., S. Stutchbury, Esq., T. J. Torrie, Esq.
(For Geography) F. Harrison Rankin, Esq.

A memoir was read by Mr E. Charlesworth, being a notice of Vertebrated Animals found in the Crag of Norfolk and Suffolk. The principal object in bringing forward this subject, was to establish the fact of the remains of mammiferous animals being associated with the mollusca of the tertiary beds above the London clay, in the eastern counties of England. These remains are confined to a part of the Crag formation, which appears to extend from Cromer in Norfolk, to within a few miles of Aldborough in Suffolk, and the depth of which was very great, wells having been sunk in it without reaching its bottom. The bones of fish, and a large portion of the testacea met with in the stratum, differ widely from those of the coralline beds, and from that part of the Crag deposit which skirts the southern coast of Essex and Suffolk. Among the mammalia, which the author states really belong to the Crag, is the *Mastodon angustidens*, of which several teeth have recently been obtained in Norfolk from localities adjoining the parish of Withingham, the spot from which Dr W. Smith states the specimen to have been procured which is figured in his " Strata Identified." Mr Charlesworth conceived the discovery of the remains of the mastodon in this formation, as affording an argument to prove the relative ages of these rocks, as no remains of this animal have been found in America in beds more ancient than the diluvial. The remaining genera of mammiferous animals can be identified with those now existing, or with such as are found in diluvial and lacustrine deposits. The author next notices the discovery of the mineralized remains of birds, chiefly bones of the extremities of natatorial tribes, a solitary instance of a similar discovery in America being the only one recorded. He was not prepared to speak concerning the different kinds of fish, but he stated their distribution—species of Squalus being found near Orford, and what Agassiz conceives to be Platex, at Cromer. Among the most remarkable is the Carcharias megalodon, the teeth of which are found in Suffolk, equal in size to specimens from the tertiary formations of Malta. He also alluded to the difference of the testacea in different parts of the Crag, from which he was inclined to infer there were several eras in its formation. No traces of the existence of reptilia have yet been detected, which would rather support the opinion of Dr Beck and Deshayes, that the climate during the Crag epoch was analogous to that of the Polar regions.—Professor Sedgwick stated, that he had been long aware of the existence of remains of mammalia in the Norfolk Crag, although this had been disputed by Mr Conybeare, in his work on the Geology of England and Wales. He was rather inclined to consider the Crag as all of one epoch; and Mr Lyell had found existing species as numerous in the lower as in the upper Crag. With regard to Mr Charlesworth's idea of the extinction of the mastodon in England before the formation of the diluvial beds, Professor Sedgwick conceived that it was reasoning from a negative fact, and that until more extensive search had been made, no such inference could be fairly drawn. He also mentioned that remains of the beaver were found in the alluvions of Cambridgeshire, and that it might have existed in England a thousand years ago. He was confident that no cause still in existence could have produced the diluvium on the Crag; its whole appearance suggested the idea of a great rush of waters.—Mr Conybeare was perfectly willing to correct his opinion respecting the existence of the remains of mammalia in the Crag. He was of opinion that the tertiary strata of America had not been sufficiently examined to justify the conclusion that it did not contain remains of the mastodon. He started a question—which of the species of mastodon found in other countries did the British one resemble?—Mr Greenough mentioned, as a singular peculiarity of the diluvium of Norfolk, its containing large masses of chalk, which contain organic remains differing in some respects from those of the chalk *in situ*. The town of Cromer seemed to be built on an immense block of chalk, contained in the diluvial formation.—Mr Murchison dissented from Mr Greenough's opinion. He conceived the formation of chalk was under the diluvium, and had been elevated and disrupted. He had seen at Hazeborough large platforms of chalk laid bare after a storm; near that place were needle-shaped rocks of chalk, and at Cromer the foundation of the town must rest on part of the same mass. There were strong reasons for believing that the Norfolk diluvium contained recent shells only. Mr Lonsdale, on examination, could discover no others.—Mr Charlesworth mentioned, that Dr Beck considered the shells of the tertiary period to be extinct species, and that at the formation of the Norfolk Crag the climate must have been very cold, like the Arctic regions. He considered the diluvial formation to have been sufficiently searched to warrant an opinion that it does not contain the remains of the mastodon. Many singular organic remains have been found there, which have been transported, as of saurians, which must have come from Yorkshire. In alluding to the fact of shells similar to those of the Crag being found at Bridlington, he was informed by Mr Sedgwick that the formation at that place was probably part of the Crag.

A paper by Mr J. B. Bowman, was now read, on the Bone Caves at Cefn, in Denbighshire. A description of these has been already published in the *Edinburgh New Philosophical Journal.* The caves are in the carboniferous limestone. The roof of the lower cave is covered with stalactites, which are often broken off or blunted. The diluvium on the floor contains fragments of slate, and the upper portion animal remains in great abundance. Among these are some of a very minute size, and also elytra of beetles. A black matter is also found, with veins of reddish clay. The bones are often in fragments; the teeth are somewhat worn; sometimes the teeth are of young animals, but no indentations have been found upon them. No skulls have been discovered, nor any coprolites. The bones frequently contain gelatine, and have often manganese upon them; hair was also discovered. The stalactites seem confined to the anterior part of the cave; in the posterior part a fine sand is found.

After this, a desultory conversation took place on the exhibition of two models by Mr Ibbotson, one of the country round Neufchatel, in Switzerland, on the scale of half an inch to the mile; and the other of a part of the Under Cliff in the Isle of Wight, on the scale of three feet to the mile.

Mr Greenough mentioned a new mode of engraving medals lately adopted in France, and which he conceived could be advantageously employed in laying down the varieties of surface on maps. —Mr Griffiths spoke of the great importance of models like Mr Ibbotson's, as being so well calculated to display the geological structure of a country. He suggested the importance of possessing maps, both of outline and of features, and he alluded to the magnificent map of Ireland, under the Ordnance Survey, the scale of which, being six inches to a mile, enabled the geological observer to trace the geological features with a facility before unknown.—It was mentioned, that the new map of Austria was on a scale of twenty-two inches to the mile, but this Mr Greenough considered inconveniently large.—Mr Ibbotson stated, that models could be easily multiplied by employing a metal mould, and using *papier maché*, or some preparation of caoutchouc; and that they might be dissected to exhibit the internal structure, and that the materials of the strata themselves could be used as colouring matter.—Lord Northampton and M. de la Beche gave their testimony of approval.—Several gentlemen then spoke of the application of combinations of letters to geological maps, to express the more minute geological phenomena; but the general opinion was, that in geological maps simplicity should, as much as possible, be preserved, and that the best mode would be to have two maps of the same district, one without names, for the geological map, and the other with the necessary writing. Maps of this kind had been given to the Geological Society by the Archduke John of Austria.

Section D.—Zoology and Botany.

President.—Professor Henslow.
Vice-Presidents.—Rev. F. W. Hope, Dr J. Richardson, Professor Royle.
Secretaries.—John Curtis, Esq., Professor Don, Dr Riley, S. Rootsey, Esq.

Dr Richardson commenced the proceedings of the Section, by reading the introductory portion of his report "On the Zoology of North America." It did not appear probable that the progress of colonization had, as yet, extinguished any one species of animal from the country. The great similarity which existed between the animals of North America and those of Europe, as regarded their generic distinctions, connected with the dissimilarity of their species, rendered them well adapted to inquiries connected with their respective geographic distribution. Hitherto the trivial names bestowed by the colonists upon many of those of North America, had tended to mislead naturalists. The observations in the present report would principally refer to the western parts of North America, including New Mexico, the Peninsula of Florida and California, down to the well-defined limits of the South American zoological province. Dr Richardson then proceeded to describe the physical structure of this country, of which the Rocky Mountains formed a most remarkable feature. The altitude of many of their peaks rose above the limits of perpetual snow, and their sides were flanked by zones of different temperature, affording passages for animals from the Arctic circle to the table lands of Mexico, without any great alteration of climate throughout the whole extent. The temperate zones of both hemispheres might, in this way, be connected, were it not that the Cordilleras were greatly depressed at the Isthmus of Panama, and that a plain extended from sea to sea a little further to the south. As yet we possess no information of the elevation of the backs of these mountains, independent of the heights of some of the peaks, and the elevation of the base of the range is equally unknown. The depths of some of the transverse valleys are considerable, and these afford passages for the migration of animals. Most of the principal rivers flowing to the east cut across the chain, and one actually rises to the west of the crests of the range. On the Atlantic side are prairies, composing plains gently inclining to the east, and there is an extent of land which may be likened to a long valley, which stretches from the Arctic sea to Mexico, without any transverse ridges dividing it, but merely affording three distinct water-sheds. The greatest width of the plain is about 15° of longitude, in the 40° to 50° of north latitude. This configuration gives great facility for the range of herbivorous quadrupeds from north to south, and for the migration of low-flying birds; whilst the Mackenzie furnishes a channel by which the anadromous fish of the Arctic Sea can penetrate 10° or 11° of latitude to the southward, and the Mississippi enables those of the Gulf of Mexico to ascend far to the north. The most remarkable chain east of the Mississippi, is that of the Alleghanies, which is about 100 miles broad, rises from a base between 1000 and 1200 feet, and attains an elevation from 2000 to 3000 feet above the sea. The strip of land between them and the coast is 200 miles broad in the Carolinas; becomes still broader in Georgia, and, sweeping round the northern extremity of the chain, joins the valley of the Mississippi. This strip influences the distribution of animal life, by extending southerly to the 5° of latitude, thus forming also a barrier to the progress of anadromous fish from the Atlantic to the bottom of the Gulf of Mexico. With reference to physical geography, Newfoundland appears as a prolongation of the Atlantic coast line, and its zoological and botanical productions correspond to those of Labrador. When the canals already projected shall have opened a communication between the several great inland seas which exist in North America, an interchange will take place between the fish of widely diverging waters. The great proportion of water to land forms a striking feature of the north-east continent. This may be zoologically divided into two districts, viz. the northern or barren grounds, and the southern or wooded. The temperature is here materially influenced by the inland sea of Hudson's Straits, and thus its capability of supporting animal life much affected. On the west of the Rocky Mountains, the northern corner appears to be similar to the eastern side or barren grounds. The general character of the country bordering the Pacific is mountainous. With respect to the climate of North America, the eastern coast has a lower mean temperature than the western, at least in the higher latitudes. Probably the isothermal, and even the isothæral lines of the banks of the Columbia and New Caledonia correspond nearly in latitude with those of the east coast of Europe. But on the eastern side, down to the 56th parallel of latitude, the subsoil is perpetually frozen. Even in the 45th parallel, on the north side of the great Canada lakes, there is upwards of six months of continuous frost, and the grallatorial and most of the graminivorous birds can find nothing to support them in the winter season; and, consequently, the migration of the feathered tribes is here much more general than in the countries of Europe lying under the same parallel. The principal cause of this great difference between the climates of the eastern and western districts may be ascribed to the configuration of the coast land, which detains the ice in its bays and gulfs, and this, in melting, materially depresses the summer heat. The decrement in the mean annual heat, corresponding to the increase in latitude, is greater in North America than in Europe, and there exists a wider difference between the temperatures of summer and winter. Dr Richardson then concluded this introductory portion of his report, by details concerning the temperatures which had been observed at different places in the country under consideration.

Mr Rootsey exhibited specimens of sugar, malt, and an ardent spirit, which he had extracted from mangel wurzel, and considered that this root might, under certain circumstances, be grown to great advantage in this country, for the purposes of manufacturing the above articles.

Mr G. Webb Hall read a communication "On the Acceleration of the Growth of Wheat." After pointing out the advantages which might accrue to agriculture from the attention given by scientific men to certain subjects with which it was connected; and the absolute necessity which now existed for making the most extensive and careful investigations concerning many points of great importance to the success of agriculture, he proceeded to call the attention of the Section to a statement of facts, by which it would be seen that the usual period allotted to the occupation of the ground for a crop of wheat might be very materially abridged. At an average, this might be estimated at ten months, though twelve, and even thirteen, were not unusual, and eight might be considered as the shortest period for the ordinary winter wheat. By a selection of particular seed, and a choice of peculiar situation, wheat sown early in March has been, on different occasions, ripened before the middle of August, a period scarcely exceeding five months. Mr Hall considers it an unquestionable law of vegetation that the offspring of a plant of early maturity itself seeks to become so likewise, even when placed in unpropitious circumstances, and that it recedes with reluctance from the condition of its parent. Hence the seed of a crop which has been ripened in five months has a better prospect of producing another crop equally accelerated than that from a crop which has been longer in ripening. He also asserted, that the acceleration of a crop was farther promoted by thick sowing, which likewise might be considered advantageous in checking and stopping the mildew.—Dr Richardson referred to the remark of Humboldt, that in South America the wheat crop was ripened in ninety days from the period of sowing, and stated, that about Hudson's Bay this period was only seventy days. He suggested the probable advantage that might arise from importing seed from the latter country for the purpose of furthering Mr Hall's views; but this gentleman stated, that he had found that seed imported from a distance (and he had tried some from Italy) was liable to become diseased.—As connected with the subject of the acceleration of the growth of seeds, Professor Henslow mentioned the results of experiments which he had tried upon seeds of a species of Acacia, sent by Sir John Herschel from the Cape of Good Hope, with directions that they should be steeped in boiling water before they were sown. Some of these were kept at the boiling temperature for three, six, and fifteen minutes respectively, and had yet germinated very readily in the open border; whilst those which had not been steeped did not vegetate. It was suggested that these facts might lead to beneficial results, by shewing agriculturists that they may possibly be able to steep various seeds in water sufficiently heated to destroy certain fungi or insects known to be destructive to them, without injuring the vital principle in the seed itself.—Mr Hope mentioned a practice common in some parts of Spain, of baking corn to a certain extent, by exposing it to a temperature of 150° or upwards, for the purpose of destroying an insect by which it was liable to be attacked.—Dr Richardson mentioned, that the seeds sold in China for the European market were previously boiled, for the purpose of destroying their vitality, as the jealousy of that people made them anxious to prevent their exportation in a state fitted for germination. Upon sowing these seeds he had nevertheless observed some few of them were still capable of vegetating.

Mr Curtis exhibited some specimens of the terminal shoots of a Pinus, which had been attacked by the *Hylurgus piniperda*, and made a few remarks on the habits of the insect.

Dr Daubeny communicated to the Section the partial results which he had obtained from a series of experiments he was carrying on at Oxford, respecting the effects which arsenic produces on vegetation. He was led to undertake these experiments from having received a communication from Mr Davies Gilbert, in which he stated that there was a district in Cornwall where the soil contained a large proportion of arsenic; and that no plants could grow in it except some of the Leguminosæ. By analysis, this soil yielded him about 50 per cent. of arsenic, in the form of a sulphuret; the rest being composed principally of sulphuret of iron and a little silica. He had already ascertained that a little of the sulphuret mixed in soils produced no injurious effect on *Sinapis alba*, barley, or beans; and that they flowered and seeded freely when grown in it. Although the want of solubility in the sulphuret might be assigned as a reason for its inactivity; yet it was certainly taken up by water in small quantities, and imbibed by the roots of plants.

Upon watering them with a solution of arsenious acid, he found that they would bear it in larger proportions than was presupposed. The injurious effects of arsenious acid on vegetation in the neighbourhood of the copper-works of Bristol and Swansea was noticed by Mr Rootsey; and Mr Stevens mentioned the circumstance of the trout in some streams of Cornwall having been destroyed by the opening of some new mines in their neighbourhood, from which arsenical compounds were discharged, though the vegetation did not appear to be injured by them; and it was further stated, that horses were considerably injured, and rendered subject to a remarkable disease, by the effects of arsenical compounds in the same districts.

Section E.—Anatomy and Medicine.

President.—Dr Roget.
Vice-Presidents.—Dr Bright, Dr Macartney.
Secretaries.—Dr Symonds, G. D. Fripp, Esq.

Dr O'Beirne read a Report of the Dublin Committee on the Pathology of the Nervous System.

A short description of a case of Aneurism of the Arteria Innominata, furnished by Sir D. H. Dickson, was then read.

Section F.—Statistics.

President—Sir Charles Lemon, Bart.
Vice-Presidents—H. Hallam, Esq., Dr Jerrard.
Secretaries—Rev. J. E. Bromby, C. B. Fripp, Esq., James Heywood, Esq.

A very curious and interesting report was read, entitled, "A few Statistical Facts, descriptive of the Former and Present State of Glasgow," by James Cleland, LL. D.

Section G.—Mechanical Science.

President—Davies Gilbert, Esq.
Vice-Presidents—M. I. Brunel, Esq., John Robison, Esq.
Secretaries—T. G. Bunt, Esq., G. T. Clark, Esq., William West, Esq.

The discussions were opened by some observations of Professor Moseley on the theory of Locomotive Carriages.

Dr Lardner next laid before the meeting many details in regard to Railroads. Afterwards Mr Russel of Edinburgh read an important memoir on the traction of boats in canals at different velocities.

Tuesday, August 23.

SECTION A.—MATHEMATICS AND PHYSICAL SCIENCE.

Mr Russel gave an interesting statement of a series of experiments regarding the Laws of the Motions of Waves excited in Water.

Professor Powell read a paper respecting the Refractive Indices of several Substances.

A paper was then read, contributed by Sir D. Brewster, "On the Polarizing Structure of the Crystalline Lens of the Eyes of Animals after Death."

The Rev. J. W. M'Gauley read an account of "A Series of Experiments in Electro-Magnetism, with reference to its application as a Moving Power."

SECTION B.—CHEMISTRY AND MINERALOGY.

Mr Exley read a very interesting memoir on a new theory of chemical combination, deduced from mathematical data, and demonstrated mathematically.

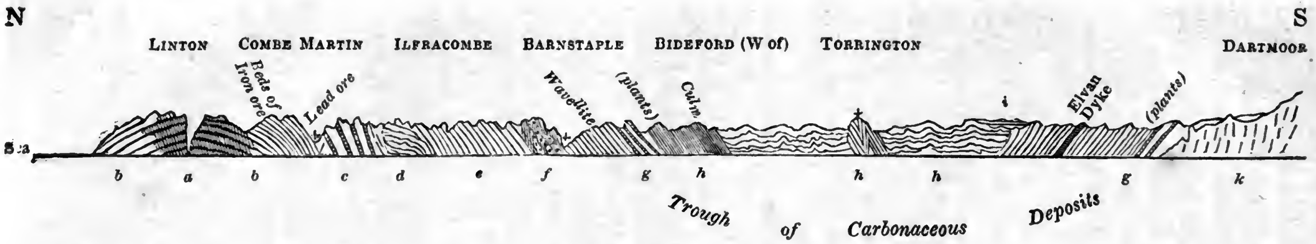
Dr Charles Henry read an account of some experiments made with a view to determine the mode in which certain gases act in preventing the action of spongy platinum upon a mixture of oxygen and hydrogen. The gases he examined were carbonic oxide and olefiant gas. He found that carbonic oxide was the most powerful, and that carbonic acid was always the result. Hence it is evident that oxygen and hydrogen are prevented from combining by the superior attraction of the carbonic oxide for the oxygen. Olefiant gas he found not to be decomposed, and hence the attraction which prevents the combination is not sufficiently powerful to form any other. This explanation is corroborated by the fact, that it requires a very great proportion of the olefiant gas to produce the effect.

Mr Herapath then read a paper on Arsenical Poisons.

SECTION C.—GEOLOGY AND GEOGRAPHY.

The first paper was "A Classification of the Old Slate Rocks of Devonshire, and on the true position of the Culm Deposits of the central portion of that county," by Professor Sedgwick and Mr Murchison.—The authors began by observing, that this was a mere outline of a more detailed memoir on the physical structure of Devonshire, which they were about to lay before the Geological Society of London. In the published geological maps of that county, the whole system of the older slate rocks was represented under one colour, without any attempt at subdivision; and one colour also represented different limestones, without any discrimination. The object of the authors was, to remedy these defects,—to ascertain and represent the true position of the successive deposits and their natural subdivisions, so as to compare them with corresponding deposits in other places. They also wished to determine the true place of the remarkable carbonaceous deposits of central Devon, which had been previously regarded as belonging to the lowest portion of the grauwacke formation. A section was exhibited of part of that county, from the north coast to one of the granite peaks of Dartmoor immediately south-west of Oakhampton. A diagram of the Section will be found in the opposite page.

In the ascending order this section exhibits—1. A system of slaty rocks, containing a vast abundance of organic remains, generally in the form of casts. These rocks sometimes pass into a fine glossy clay-slate, with a true transverse cleavage; sometimes into a hard quartzose flagstone, not unusually of a reddish tinge; sometimes into a reddish sandstone, subordinate to which are beds of incoherent shale. In North Devon they are very rarely so calcareous as to be burnt for lime, but in South Devon, rocks of the same age appear to be much more calcareous. 2. A series of rocks characterized by hard thick-bedded red sandstone, and red micaceous flagstone, subordinate to which are bands of red, purple, and variegated shales. The red colour occasionally disappears, and the formation puts on the ordinary appearance of a coarse, siliceous grauwacke, subordinate to which are some bands of imperfect roofing slate. In this series are very few organic remains. It is several feet in thickness, occupying the whole coast from the west end of the Valley of Rocks to Combe Martin. 3. The calcareous slates of Combe Martin and Ilfracombe, of very great aggregate thickness, abounding in organic remains, and containing in a part of their range at least nine distinct ribs of limestone burnt for use. This limestone is prolonged into Somersetshire, and appears to be the equivalent of that on the flanks of the Quantock Hills—4. A formation of greenish and lead-coloured roofing slate of great thick-



Ascending Series of Devonian Rocks.

Cambrian Rocks.

(*a*) Slaty schists, with some calcareous courses and organic remains.

(*b*) Purple, red, and grey sandstones, with beds of iron-ore in upper members—peculiar fossils near their junction with the overlying limestones. Veins of lead and copper.

Upper Cambrian or Devonian Rocks.

(*c*) Calcareous group of Combe Martin and Ilfracombe—fossils very abundant—slaty cleavage.

(*d*) Slates with quartzose veins and beds—incoherent schists, &c. Manganese mines.

Silurian Rocks.

(*e*) Slaty sandstones and schists — cleavage passing through the beds of organic remains.

(*f*) Ditto, with concretionary limestones, and many well known Silurian fossils, chiefly of the lower part of the system.

Culm Deposits = Coal-field of Pembroke.

(*g*) Culmiferous or black limestone, with portions of stone coal, and fossils distinct from any found in the inferior groups. Wavellite occurs in the beds below this limestone.

(*h*) Culm beds with underlying and overlying successions of sandstone and shale often highly pyritous, with many nodules of iron-ore, frequently containing coal plants, and never affected like the older rocks by slaty cleavage.

(*i*) New red sandstone resting unconformably on the carbonaceous deposits.

(*k*) Granite of Dartmoor and Elvan Dyke, both erupted through the culm deposits.

ness, and occupying a well defined zone in North Devon, its upper bed alternating with and gradually passing into a great deposit of sandstones of various colours and micaceous flagstones. These siliceous masses alternate with incoherent slates, and are in some places surmounted by great masses of red unctuous shale, which, when in a more solid form, generally exhibit cleavage oblique to the stratification.—5. The Silurian system resting conformably on the preceding, and of great thickness, on the north-western coast, containing many subordinate beds and masses of limestone. In its range towards the eastern part of the county, it gradually thins off, but its characters are well preserved, and it everywhere contains vast numbers of characteristic organic remains.—6. The carbonaceous system of Devonshire, in a direction east and west across the county, in its southern boundary so close to Dartmoor that its lower beds have been tilted up and altered by the granite. It occupies a trough, the northern border of which rests, partly in a conformable position upon the Silurian system, and partly upon older rocks, probably of the division No. 4. Its southern border also rests on the slate rocks of Launceston. It everywhere exhibits a succession of violent contortions. In some places it is overlaid by patches of green sand, and west of Bideford by conglomerates of the new red sandstone. The lowest portion of this vast deposit is generally thin bedded, sometimes composed of sandstone and shale, with impressions of plants, sometimes of indurated compact slate, containing wavellite. These beds are surmounted by alternations of shale and dark-coloured limestone with a few fossils. Subordinate to these, there are on the western side of the county thin veins and flakes of culm or anthracite; but this is wanting on the eastern side, and the calcareous beds are more expanded. The higher beds of this deposit are well exhibited on the coast west of Bideford. These often contain impressions of vegetables. Though in a state of greater induration than the ordinary coal-measures of England, and even in many places destitute of any trace of coal, still these beds do not differ from the great unproductive coal-field of Pembrokeshire. The authors consequently concluded, that from the order of superposition,—from mineral structure—from absence of slaty cleavage peculiar to the older rocks on which this deposit rests, and from the specific character of its organic remains, it may without hesitation be referred to the regular carboniferous series. In the course of the details, the authors alluded to a remarkable elevated beach, occupying two miles of coast on the north side of

Barnstaple Bay, a more special account of which is being prepared for the Geological Society.

Mr De la Beche objected to the conclusions of Messrs Sedgwick and Murchison, although he did not dispute the correctness of the section of the country which they had exhibited to the meeting. He conceived that he had traced the carbonaceous rocks passing into what had been termed the Cambrian system, although he was not prepared to say that it really was that system. He was also unable to make that separation of the contorted rocks, suggested by the authors of the paper. He spoke of the overlying greenstones in different places, and considered that these were of different ages; also of the changes produced by granite on rocks of every kind in contact with it. He alluded to the former opinions of the rocks called by the general name, Greywacke, which opinions have, of late years, been totally altered. He attached very little importance to mineral characters: unless the consideration of the imbedded organic remains was made of the first importance, we were sure of falling into error. Are the organic remains in these carbonaceous rocks of Devon really the same as those of the general carboniferous system? He stated, that he conceived there was evidence to prove that there was a regular band of rocks surrounding Dartmoor, which had been thrust up through the hollow in the middle. He could nowhere discover any line of separation between the carbonaceous and the older rocks, so that he was unable to reconcile the deposits of coal with those of other parts of England, and as to the age of these older rocks all were agreed. In the Alps, organic remains of the coal formation are found in beds, alternating with oolites, so that we must not limit too strictly the range of these organic remains, as we should be certain of all the conditions under which coal plants can be accumulated. We should recollect, that the remains of the vegetation of a mountain may be entombed at its base, so as to be shifted from its original habitat; and that, although the disposition of organic remains may hold true for a certain extent of the earth's surface, we have no right to consider such a disposition universal.—Mr Sedgwick remarked, that he could with certainty distinguish four calcareous zones in North Devon—viz. one at Linton, a second at Ilfracombe, and two others at Barnstaple. The difference of the limestones of South Devon was also very remarkable; that of Plymouth being essentially distinct from that of Dartmoor. These carbonaceous strata also extended several miles into Cornwall.—Mr Conybeare considered that the public had exaggerated the difference of opinion then before the meeting. He was rather inclined to coincide with Messrs Sedgwick and Murchison in considering the strata in dispute as referable to the general carboniferous system, and from the general resemblance of the formations to those of Pembrokeshire, the probability was much strengthened.—Professor Phillips conceived that it had been satisfactorily proved, that there existed a coal basin in the interior of Devonshire, although, at first sight, from the unprofitable nature of the contained coal, being the kind called Culm, some hesitation might have taken place as to assigning it its true position. But doubts must vanish on inspecting the organic remains: and here he might observe, that it was a mistake to suppose that Dr Smith, the founder of English geology, had ever intended to limit the range of these remains as some had accused him of. We might readily assume, and observation has confirmed, that some organic remains of one stratum may be found in contiguous strata, associated with fossils of different kinds, so that organic remains alone are insufficient to point out distinctions in strata. But the general appearance of the limestones of Devon was precisely similar to those of the north of England, in regard both of mineral character and imbedded fossils. From their appearance, he had expected their interstratification with shales, and Mr Murchison had confirmed this supposition. The Devon limestone corresponded indeed with the upper bed of the Yorkshire limestone; in the former he had detected a shell, a species of Anodon, which he had not observed in the latter; but the species of Posidonia found in both exactly correspond. Perhaps one cause of mistake might have been the little attention paid to the black limestone of Craven, by Mr Conybeare, and to this limestone there was a most striking resemblance in the black variety of Devonshire. He alluded to the extraordinary anomaly of coal plants having been found in the Alps, associated with oolites, but this might be an exception from the general law, and exceptions there must be; still it must be allowed, that organic life must have a constant relation to the state of the actual surface. He came to the conclusion, that the Devon district would not offer any anomaly in geological arrangement, but that it would correspond in arrangement with the other parts of the country, and that a fruitful source of error is the hitherto vague term Greywacke, which has been applied indiscriminately to a great variety of rocks, so as to include many of different ages throughout this county.—Dr Buckland congratulated the meeting on the difference of opinion among the geologists present, such a difference producing discussion, which was the sure means of arriving at truth. He considered, that the true solution of the question at issue would be in the middle course; that, no doubt, it could not be easily granted, that the series under consideration was carboniferous, when no true coal was contained in it; but, were we to adopt the new term, *culmiferous*, we should get rid of the difficulty. This culmiferous series he regarded as the lowest portion of the coal formation, and as resting upon the Silurian rocks. He alluded to the difficulty of making geological maps; these must be constantly modified, according to the extent of investigation: errors of omission must be committed by every pioneer in geology, which can only be corrected by the researches of succeeding observers.

After the discussion was closed, Mr De la Beche exhibited a part of the Ordnance Geological Map of Devon, and such parts of that of Cornwall as have been finished; and pointed out the general parallelism of certain great lines of dislocation both in the metalliferous and non-metalliferous districts. He stated that he considered such lines to have been produced at the same geological epoch, and attributed the fact of the occurrence of the ores of useful metals in some situations and not in others, to conditions which were to be found in the one and not in the other. The conditions most favourable to the occurrence of the tin and copper ores of Cornwall and Devon, are the proximity to the junction lines of the granitic and slate systems of those counties; the intermixture of granitic and porphyritic dykes with the slates, or with the masses of granite; the occurrence of great lines of dislocation traversing the lodes or mineral veins, and termed *cross courses*, &c. The author pointed out numerous other conditions, and then noticed the beneficial effects of the proximity of the granitic or porphyritic dykes provincially termed *elvans*, and which alike traverse the granitic and the slate systems. In support of this view, he instanced more particularly the mines in the vicinity of Marazion, where the lodes or mineral veins traverse lines of elvans obliquely, and where very rich bunches of ore have been obtained at such junctions. Indeed the miners of that part of the country are perfectly aware of the value of these junctions, and carry their work on as much as possible within their favourable influence. The author directed the attention of the Section to the fact, that all the great mines of Cornwall are situated amid the above conditions, and to the advantages which geology could thus confer upon the community, by pointing out to them those places where the chances are favourable to mining operations, and by inducing them to avoid those bubble speculations at this moment so unfortunately common.—Mr Hopkins was called upon to make some observations regarding the direction of the fissures mentioned by Mr De la Beche, but he did not enter very fully into any discussion, as he proposed, on the following day, to bring the general consideration of fissures before the Section. He observed, however, that there must have been one great axis of disturbance, to which the smaller fissures must either have been parallel, or have circulated around it; indeed, Mr De la Beche had supposed the great line of fissures from Blackdown to Cornwall had been curved by the intervening granites. He stated, that there must be a connexion between the width of lodes and their mineral contents; also, that in the production of fissures there must have been several periods of elevation.—Mr Fox then mentioned a remarkable experiment which he had made upon the yellow sulphuret of copper, having changed it by electricity into the grey sulphuret. In a trough a mass of clay was placed, so as to divide it into two portions, in one of which was sulphate of copper in solution, in the other dilute sulphuric acid. On the electric communication being made by placing the yellow sulphuret in the solution of sulphate of copper, and a piece of zinc in the acid, the change of sulphuret took place, and crystals of native copper were also formed upon it.—Mr Fox observed, that native copper is not found in the mines of Cornwall combined with yellow copper, but with black copper-ore; and that the grey ore is generally found nearer the surface than the yellow, and also in and near the cross courses.—Mr Taylor bore testimony to the importance of geological information to mining agents, who now were informing themselves, not only in practice, but in theory. He spoke of the exertions of the late Mr Phillips, in drawing up a geological map of Cornwall, so far back as 1800. He suggested the propriety of tracing the lines of fissures into the coal districts, and also wished the directions of the lead lodes of the mountain limestone to be ascertained, as likely to lead to general results.

Section D.—Zoology and Botany.

Dr Richardson resumed the reading of his Report on the Zoology of North America. In touching upon the geographical distribution of the Mammalia, he remarked the great similarity which existed between them and the European species; whilst there was the greatest dissimilarity to those of South America. The boundary line separating the Faunas of North and South America, was not at the Isthmus of Darien, but at the tropic of Cancer. No Quadrumana occur to the north of the Isthmus of Darien; though in Europe there is a species which ranges as far north as the rock of Gibraltar, in latitude 36°.—In the order Carnivora, and family Cheiroptera, all the North American species belong to that tribe which possesses only one bony phalanx in the index, and two in each of the other fingers, to which tribe also all the European bats belong, except an Italian species of Dinops. None of the sixteen species recorded as natives of North America have been found elsewhere; two only have been traced over any great extent of country, and one of these (resembling the European Pipistrellus) ranges through 24° of latitude, and is the most northerly species in America. There must be still many bats to be discovered in that country, as those of Mexico, California, and the whole track of the Rocky Mountains are entirely unknown. Of the family Insectivora, ten species were enumerated; and it was stated that North America differs more from Europe in this family, than in any other of the order Carnivora. Three of the European genera do not exist in North America, and the three genera found in North America do not exist in South America. The North American species of Sorex, however, closely resemble those of Europe.—Of the family Marsupiata, inhabiting the New World, only three species reach into North America, the rest being confined to the south of the Isthmus of Darien. Two of these occur no higher than Mexico; but the third (the Virginian opossum) ranges to the great Canadian lakes on the north, and to Paraguay on the south.—About forty species of the family Carnivora have been noticed; and this family includes a greater number than any other which are common to both North America and Europe; though possibly a closer acquaintance with some which are at present considered identical, may enable us to establish some distinction between them. The generic forms of North America are the same as those of Europe, excepting in a very few cases, which belong to the South American group. A few of the more northern forms also cross the Isthmus of Darien to the south.—In the family Plantigrada, two of the four bears of North America are undoubtedly peculiar to the New World; and one of these is the most northerly quadruped it contains. The American Glutton, or Wolverine according to Cuvier, is identical with that of the Old World. Among the Digitigrada, the range of the Mustelæ is limited southwards to the northern or middle districts of the United States. Whether any of the American and European species of this genus be really identical, is involved in great uncertainty. Of the three Otters of North America, one appears to be identical with that of Europe; and another, if correctly identified as the *Lutra Brasiliensis*, has a most extensive range, from the Arctic Sea through great part of South America. Eight species of the genus Canis are found in North America; but there is great difficulty in distinguishing the species, and in identifying them with any of those of Europe. The domestic dog breeds with the wolf and fox, and their offspring is prolific. Eight species of the genus Felis were mentioned by Dr Richardson, three of which extend from South America into the south-western territories of the United States; and some of the others are still doubtful as North American species. The nine species of Amphibia found in North America, are mostly common to the northern seas of the Old and New Worlds: the genus Otaria alone being confined to the North Pacific; and even these range to the Asiatic coast. The specific identity of some of the seals is involved in very great doubt. In the order Rodentia, there have been between seventy and eighty species discovered; and here North America surpasses every quarter of the globe in the abundance and variety of form which these animals assume. The squirrels are not yet satisfactorily determined. The marmots are numerous, except in the subgenus Spermophilus. There is only one which may possibly be common to the New and Old World. There is only one of the restricted genus Mus, which is unequivocally indigenous to North America; and this closely resembles the European *M. sylvestris*. Other species have been introduced from the opposite side of the Atlantic.

Mr Bowman read a communication respecting the Longevity of the Yew-Tree; and mentioned the result of his observations upon the growth of several young trees, by which it appeared that their diameters increased during the first 120 years, at the rate of at least 2 lines, or the one-sixth of an inch per annum; and that under favourable circumstances the growth was still more rapid. In the church-yard at Gresford, near Wrexham, North Wales, are eighteen yew-trees, which are stated by the parish register for 1726 to have been planted in that year. The average of the diameters of these trees is 20 inches. Mr Bowman then remarked on two yew-trees of large dimensions, from the trunks of which he had obtained sections. One is in the same churchyard as those above mentioned, and its trunk is 22 feet in circumference at the base, 29 feet below the first branches. This gives us a mean diameter of 1224 lines, which, according to De Candolle's rule for estimating the age of the yew, ought also to indicate the number of years. From three sections obtained from this tree, Mr Bowman ascertained that the average number of rings deposited for one inch in depth of its latest growth, was 34⅔. Comparing this with the data obtained from the eighteen young trees, he estimated the probable age of this tree at 1419 years. The second of these trees is in the church-yard of Darley in the Dale, Derbyshire, and its mean diameter, taken from measurements at four different places, is 1356 lines. Horizontal sections from its north and south sides gave an average for its latest increase at 44 rings per inch nearly, which gives 2006 years as its age, by the mode of calculation adopted by Mr Bowman. He then proceeded to state his opinion of the reason why so many old yew-trees were to be met with in church-yards: he considered that they might have been planted there at a period anterior to the introduction of Christianity, under the influence of the same feelings as those which prompted the early nations of antiquity to plant the cypress round the graves of their deceased friends.

Mr Ball exhibited the skulls of a species of Seal common in Ireland, with the view of eliciting information, as he considered it to be new to the British Fauna, and very distinct from the two already recorded. The present species was never known to become tame, whilst the *Phoca vitulina,* generally considered the more common species of our coasts, was very easily tamed.—Professor Nilsson, of Lund, at once pronounced this species to be his *Haliochœrus griseus,* forming a distinct genus from *Phoca,* and described by him in the year 1820. It had been previously recorded by Fabricius, under the name of *Phoca gryphus.* It is common in the Baltic and North Sea, and to be met with in Iceland, and attains a size of eight feet in length. In Sweden it was emphatically termed the Sea-seal, in contradistinction to those which inhabited gulfs. He remarked that the name of *Phoca vitulina* had been applied by Linnæus, and subsequent authors, to three distinct species, to which he had himself given the names of *barbata, variegata,* and *annellata.* Of these he had ascertained that a specimen, captured in the Severn, and now in the Bristol Institution, belonged to the *annellata.*—Dr Scoular remarked that the species which Professor Nilsson had identified as his *Haliochœrus griseus*, predominated in Ireland over the *Phoca vitulina*, though it had been hitherto neglected; and that the great difference in the teeth of these species, justly entitled them to be considered as forming distinct genera.—Dr Riley exhibited the stomach of the specimen alluded to, as having been caught in the Severn, in which he had found from thirty to forty pebbles, and states that other instances had occurred of a similar nature; and that it was a popular notion that they assisted the seal in the way of ballast whilst catching his prey, which it did by rising vertically upwards, and seizing it from below. But Sir Francis Mackenzie then asserted that he had repeatedly seen the seal chase salmon into the nets, and that it was not usual for it to capture its prey in the way described. Neither he, nor Professor Nilsson, nor Mr Ball, had ever found stones in the stomach of this animal.

Dr Hancock read a paper on a new species of Norantea, from Guiana, termed by the natives Corocoromibi.

Mr Hope exhibited a remarkable specimen of the *Lucanus camelus*, Fabr., from North America, the right side of which had the configuration of the male, and the left of the female sex.

Mr Hope read a communication, expressive of the probability that some of the early notions of antiquity were derived from observations made on the habits of insects.

Mr P. Duncan offered a few remarks upon the subject of Mr Hope's speculations.

Mr G. Webb Hall commented on the effects of lime as variously applied to different soils.

Section E.—Anatomy and Medicine.

The first paper read was entitled, "Observations on Remedies for Diseases of the Brain," by Dr Prichard, of Bristol.

The second paper read was by Dr Houston, on a human fœtus without heart or lungs.

The third paper was by R. Carmichael, Esq. on Tubercles.

Section F.—Statistics.

Mr Kingsley presented and described several forms of tables, for more accurately displaying the revenue and expenditure of the United Kingdom, and procuring accuracy in Parliamentary Returns of the state of Savings Banks, &c.

Baron Dupin addressed the Section on the subject of a paper he had laid upon the table, entitled, "Researches relative to the Price of Grain, and its influence on the French Population."

Section G.—Mechanical Science.

The sitting of the Section occupied but a short time, during which two papers were read, one of some interest, by Mr Henwood, on Naval Architecture, and a second by Mr Coosham, on certain improvements in Napier's rods. Dr Daubeny also exhibited an ingenious instrument for taking up sea water from any given depth, for the purpose of chemical analysis, being an improvement of an admirable invention for that purpose sent out in the Bonite.

Evening Meetings.

In consequence of the incessant rain, the intended Promenade and Horticultural Exhibition at Miller's Gardens was abandoned, and notice given that the Geological, Statistical, and Mechanical Sections would meet in the evening.

In the Geological Section, Dr Hare of Philadelphia entered upon a history of the many modifications of the Pile of Volta, and in particular drew attention to a form of it devised, and long since described by himself, but which he conceived had not in a sufficient degree attracted the attention of European philosophers. Dr Hare concluded by the exhibition of some striking experiments illustrative of the igniting or deflagrating efficacy of his Voltaic arrangements.

Professor Phillips followed with an account of the distribution over the northern parts of England of Blocks or Boulders. The Association, he observed, had formerly proposed a question regarding this distribution, and the present was a partial attempt at its solution; and it was interesting both to the geologist and the geographer, as it involved the effects of running water in modifying the surface of a country. In glancing over the north of England, we find a great variety of rock formations, from the oldest slates to the newer tertiary; the country generally slopes to the east, with the exception of the group of Cumbrian mountains, which form a local conical zone. One striking feature in its physical geography, is an immense valley running north and south, and passing through a great variety of formations; the Wolds of York being chalk, the strata near Whitby of oolite, the vale of York new red sandstone, while the carboniferous rocks are displayed in Northumberland and Durham. All the country from the Tyne to the Humber is covered with transported boulders, many of which are of rocks quite different from any near the spots where they occur, and some even not recognizable as British rocks. Could Mr Lyell's ideas regarding the office of icebergs be true, that they had been the means of transporting gravel to distant places? Boulders of the Shap Fell granite had been found in the south-eastern part of Yorkshire; in the interior, there were great accumulations of them in many places, their directions seemed all to converge to a certain point, in what is termed the Pennine chain, but on this chain no boulders have been observed, except at one point, from which you look towards Shap Fell; towards the north they have been drifted nearly as far as Carlisle, but there is no trace of them towards the west. We also find boulders from Carrick Fell carried to Newcastle and the Yorkshire coast, and these have been drifted over the same point of Stainmoor. Mr Phillips gave several conflicting opinions of different geologists, to account for this extraordinary transportation: the bursting of the banks of lakes; the alternate elevation and depression of mountain chains; and the supposition that the entire country had been under the sea, when the distribution of boulders had taken place.—Mr Sedgwick then rose, and remarked, that the direction of transport of the blocks may have been modified by the surface over which they were carried; and that Sir James Hall had been the first who had observed the Shap Fell boulders. These boulders Mr Sedgwick had noticed on the shores of the Solway Firth, mixed with gravel from Dumfries-shire. He alluded to the action of water upon the crests of mountains, and to the occurrence of transported blocks at considerable elevations. It was well known that mountain lakes were gradually filling up; and he had shewn in a paper to the Geological Society the relation of a lake to the age of the valley containing it. With the diluvial gravel over the country we find associated organic remains,—a strong proof that the land must have been dry when the transportation took place.—Mr Murchison had observed these boulders associated with recent shells at various elevations,—consequently, the land must have been at one time under the sea, and have been subsequently elevated. There must have been a relative change of the level of land and sea; and Professor Esmark, in Norway, had been the originator of the idea of the icebergs transporting gravel. He referred to the valley of the Inn, in the Tyrolese Alps, as illustrating this alteration of level: boulders of granite had been found on calcareous mountains composing one of its sides, elevated five or six thousand feet above the sea level; and this valley could not have been scooped out.—Dr Buckland was of opinion that the land must have been dry before the action of the water that had transported these blocks. There was a great number of organic remains mixed with the gravel, derived from animals existing on dry land; and this was not only true in England, but confirmed by observations made on the continent of Europe.

In the Statistical Section Dr Lardner delivered a lecture on Steam Communication with India.

In the Section of Mechanical Science, Mr Whewell gave a short account of the present state of the science of the Tides. Though there can be no doubt, that the tides are to be reckoned among the results of the great law of universal gravitation, they differ from all the other results of that law in this respect, that the facts have not, *in their details,* been reduced to an accordance with the theory; and the peculiar interest of the subject at the present moment arises from this, that the researches now going on appear to be tending to an accordance of theory and observation; although much in the way of calculation and observation remains to be still effected before this accordance reaches its ultimate state of completeness. With regard to observation, the port of Bristol offers peculiar advantages; for, in consequence of the great magnitude of the tides there, almost all the peculiarities of the phenomena are magnified, and may be studied as if under a microscope. With regard to the theory, one point mainly was dwelt upon. By the theory, the tides follow the moon's *southings* at a certain interval of time, (the *lunitidal* interval,) and this mean interval will undergo changes, so as to leave less than the mean when the moon passes three hours after the sun, equal to the mean when the moon passes six hours after the sun, and greater than the mean when the moon passes nine hours after the sun; and the quantity by which the lunitidal interval is less than the mean when the moon is three hours after the sun, is exactly equal to the quantity by which the lunitidal interval is greater than the mean when the moon passes nine hours after the sun. And this equality of the defect and excess of the interval at three hours and at nine hours of the moon's transit, is still true where the moon's force alters by the alteration of her parallax or declination. Now we are to inquire whether this equality of excess and defect of the interval in all changes of declination, &c. is exhibited by observation. It appears at first sight, that the equality does not exist; that is, if we obtain the lunitidal interval by comparing the tide with the *nearest* preceding transit. But, in truth, we ought not to refer the tide to such a transit, because we know that the tide of our shores must be produced in a great measure by the tide which revolves in the Southern Ocean, and which every half day sends off tides along the Atlantic. The tide, therefore, which reaches Bristol, is the result of a tide wave, which was produced by the action of the sun and moon at some anterior period. It is found, that if at Bristol we refer each tide to the transit of the moon, which took place about forty-four hours previously, we do obtain an accordance of the observations with theory in the feature above described,—that although the moon's force alters by the alteration of her declination, the defect of the lunitidal interval for a three hours' transit of the moon is equal to the excess of that interval for a nine hours' transit. And thus, in this respect at least, the tide at Bristol agrees exactly with the tide which would be produced, if, forty-four hours before the tide, the waters of the ocean assumed the form of the spheroid of equilibrium due to the forces of the moon and sun, and if this tide were transmitted unaltered to Bristol in those forty-four hours.

Wednesday Aug. 24.

Section A.—Mathematical and Physical Science.

The first paper read was by Mr W. Snow Harris, "On some phenomena of Electrical Repulsion."

Professor Challis read his "Supplementary Report upon the Mathematical Theory of Fluids."

Professor Stevelly gave his "Illustration of the meaning of the Doubtful Algebraic Sign in certain formulæ of Algebraic Geometry."

Professor M'Cullagh made a communication respecting the laws of Double Refraction in crystals of quartz.

Mr R. Addams then made a communication on the Interference of Sound, and illustrated his subject by several experiments.

Section B.—Chemistry and Mineralogy.

Dr Daubeny read an interesting report on the present state of our knowledge with respect to Mineral Waters.

Mr Mushet exhibited some specimens of metallic iron prepared by exposing the iron-ore to long continued heat, with a small quantity of fuel, and thus reducing it to the metallic state without fusion.

Mr Johnston described Paracyanogen and its compounds.

Mr West next read a short paper, the object of which was to suggest a new mode of determining the presence, and estimating the amount, of those materials which constitute but small fractional portions of the atmosphere. His proposition was, that instead of operating upon a limited volume of air, as is usually done, a very large quantity of it should be made by mechanical means to pass through appropriate fluids—such as barytic water for carbonic acid, and nitrate of silver when the object was to determine the presence of muriatic acid.—Dr Dalton stated that he had for many years turned his attention to the amount of carbonic acid in the atmosphere, and that he had satisfied himself that its average quantity was one part in 1100. He altogether rejected the results of Saussure, and contended that the quantity of this gas in the atmosphere was constantly the same in town and country; and that even in a crowded theatre it seldom rises to one per cent.—Dr Thomson gave it as his opinion, that a fall of rain diminished the amount of carbonic acid in the air, and expressed surprise that Dr Dalton should maintain an opposite tenet.

The business of the day was concluded by Dr Hare reading a pamphlet on the Berzelian nomenclature, which he addressed some years since to Professor Silliman.

Section C.—Geology and Geography.

Mr Stutchbury read a paper by himself and Dr Riley on some newly-discovered Saurian Remains, from the magnesian conglomerate of Durdham Down. This communication chiefly related to the specimens exhibited to the meeting, and contained a number of minute anatomical details, which testified in a high degree the industry of Dr Riley and Mr Stutchbury, who had examined the specimens; but it would be impossible, without plates, to convey to the reader any accurate notion of these highly-interesting organic remains. They belong to two new genera established by Dr Riley and Mr Stutchbury, the *Palæosaurus* and *Thecodontosaurus;* and were found in the magnesian conglomerate which at Durdham Down reposes on the carboniferous limestone. They must have been deposited upon the spot where they were found without violent action, as they bear no marks of attrition. Perhaps the most interesting fact mentioned was the peculiar structure of the vertebræ of the newly-discovered saurians, which presented a remarkable contrast to those of the recent crocodiles. He shewed a singular gradation from the recent saurians to sauroid fishes, by means of this arrangement of vertebræ, which thus becomes an excellent guide in the discrimination of the saurian animals; and he concluded his communication with a quotation from Agassiz, respecting the progressive development of animal life.—Dr Riley alluded to the extraordinary structure of the cerebral column of these extinct saurians, as likely to illustrate the supposition of Dr Gall, that the spinal column of vertebrate, would be eventually found to correspond with the ganglionic system of invertebrate animals.—Dr Buckland was particularly struck with the singular structure of these vertebræ, as indicating in the animal a nervous power of the most extraordinary character.

A paper was read by Mr Hopkins, containing theoretical views respecting the geological phenomena of elevation. The principal object of the author in this paper was to investigate the effects of an elevating force acting simultaneously at every point, on portions of the crust of the globe of considerable superficial extent; and to shew that the theoretical inferences deduced from this hypothesis are in striking accordance with the phenomena he had observed in the limestone and coal districts of Derbyshire. He also proved that in that district the direct cases of dislocation were not such as could result from the influence of the jointed structure as the determining cause of those directions. He pointed out how the theory he had discussed will account for nearly all the phenomena of mineral veins, which can be attributed to mechanical causes; as well as for the formation of systems of anticlinal lines, of faults, and of the phenomena of elevation.—Mr Sedgwick considered this as the most important communication as yet made to the Section. We should now be enabled to indulge in the same speculations in Geology, as in her elder sister science Astronomy, and from the beginning now made, it was impossible to predict how far investigations like Mr Hopkins' might eventually be carried. The observations of Mr Hopkins held true in Cumberland, Derbyshire, and Flintshire; and some of his cases of complicated dislocation were admirably illustrated in Caernarvon and Stainmore. Mr Sedgwick had himself paid particular attention to the joints of rocks, and had found them connected both with their strike and dip. He had also observed some singular phenomena in the Westmoreland slates; he had seen in them two sorts of joints, and a cleavage which was in a different direction from the jointing.

In South Wales the planes of splitting were in one direction with very few exceptions.—Mr Phillips expressed his high satisfaction at the result of Mr Hopkins' paper, and expressed a hope that the phenomena of geology might, to a certain extent, be explained by such simple laws as regulate the other branches of physical science. With regard to the structure of rocks, which promised to throw so much light upon the subject, he proposed a new term for it, the *symmetrical* structure. In the examination of rocks under the three classes of Calcareous, Arenaceous, and Argillaceous, he had remarked, that the regularity of the structure increased with the antiquity of the rock, which was well exemplified in the older slates and limestones. For this there must be a cause, and this must be a central heat, which has acted most upon the older formations, and least upon the new. Illustrations of the effects of heat upon strata may be obtained from those in contact with dykes, which produce symmetrical structure in rocks or clays through which they pass. Internal heat must then have caused the regular structure so generally observed in rocks. The direction of the fissures pointed out by Mr Hopkins in Derbyshire, corresponded with the observations of Mr De la Beche in Cornwall, and of Mr Conybeare in Glamorganshire. The phenomena of the direction of the joints were well worth investigation, as there was much uncertainty involved. They evidently pointed out the weaker points, or places of least resistance, where the disturbing force would operate with most effect; and they may have been the result of consolidation, as we find them in conglomerates, as well as in homogeneous rocks; still it might be a question, if they were formed before or after dislocation.

Section D.—Zoology and Botany.

Col. Sykes made a communication to the Section "On the Cultivated and Wild Fruits of the Deccan."

Mr Mackay read the Report which he had been last year requested to prepare, "On the Geographical Distribution of the Plants of Ireland." This contained a catalogue of 195 of the more remarkable species, with a comparative view of such as were common to the neighbourhoods of Dublin, Edinburgh, and the south coast of Scotland. And Mr Mackay then entered into some details illustrative of the more remarkable points of difference in the vegetation of Ireland and Scotland. This difference might be partly ascribed to the more southerly situation of Ireland, and the height of its mountains being inferior to those of Scotland. Its greater exposure to the influence of the western ocean gives it a moister climate. Scotland is, in consequence, much the richer in alpine plants, and Mr Mackay enumerated fifty-five species of the more remarkable alpine and other plants natives of that country, which do not occur in Ireland. Many plants on the western coast are natives of the mountains of Spain and Portugal. A list was then given, in which twenty-one species were enumerated as natives of Ireland, but which had not been found in any other parts of Great Britain, and it was very remarkable that several of these were also to be met with on the western side of the Pyrenees. In conclusion, Mr Mackay proposed to continue his observations, hoping to present the Association with a more perfect list on a future occasion.

Mr Royle read a communication on Caoutchouc.

Mr P. Duncan detailed some observations on Marine Luminosity.

Dr Hancock read a paper " On the Cow-fish, *Manatus fluviatilis*, of the inland waters of Guiana."

Dr Macartney made some observations on the preservation of animal and vegetable substances from the attacks of insects. He employed a concentrated solution of equal parts of alum, nitre, and salt, mixed with an equal quantity of proof spirits and a little oil of lavender or rosemary. A forcible injection of this liquid into the arterial system would perfectly preserve a dead body for three or four months fit for dissection, and portions of one which had been thus injected, if rubbed over with pyroligneous acid, might be preserved for any length of time. He recommended a coat of plaster of Paris to be daubed over succulent plants as a mode of preserving them, and, when dry, this might be easily removed. He noticed the entire preservation of some bodies found in the bogs of Ireland.

Mr Hope exhibited a collection of North American insects, principally Coleoptera, collected from the raw turpentine sent over to this country, in which they had become entangled. They were extracted from the turpentine whilst it was slowly melting at the warehouse, and then placed in spirits of turpentine to cleanse them thoroughly. In this way they may be prepared in as great beauty and perfection as when newly captured.

Section E.—Anatomy and Medicine.

Dr Macartney read the report of the Dublin Committee, appointed by the British Association, " On the Motion and Sounds of the Heart;" and the report of the London Committee, " On the Sounds of the Heart," was read by Dr Clandining. Dr Symonds then read a letter from Dr Spittal, of Edinburgh, stating, that in consequence of the death of Professor Turner, and the absence of one of the members on the Continent, the Committee had not been able to prepare a report. After that a paper was read, "On the Gyration of the Heart," by F. A. Greeves, Esq.

The President then read a communication from Dr Brewster, entitled, "A singular development of Polarizing Power on the Crystalline Lens, after Death," and also a letter from the same, "On Cataract, or a disease resembling Cataract," which, if resisted in its earlier stages, the Doctor believed, from personal experience, might be overcome. For detecting this disease, which generally manifested itself between forty and sixty, the Doctor gave instructions, and further stated, that by attention to diet and regimen, and taking care not to study by night, he had been cured in about eight months. If the affection had not been checked in time, he entertained no doubt it would have ended in cataract.

Dr Carson then communicated some "Observations on Absorption."

SECTION F.—STATISTICS.

A paper on Statistical Desiderata, by W. R. Greg, Esq., of Manchester, was presented by the Rev. E. G. Stanley.

Mr John Taylor, Treasurer to the Association, read a paper on the Comparative Value of the Mineral Productions of Great Britain and the rest of Europe. A calculation, he said, was made by Mr C. F. Schmidt, in 1829, of the value of the mineral productions of Europe, at Continental prices; and, from the accuracy of the statements coming within Mr Taylor's own knowledge, he was disposed to believe in the others. It should be borne in mind that the continental prices differed greatly from those in England, and, consequently, that the amounts were comparative, and not absolute value. The value of the mineral products of Europe, including Asiatic Russia, were,—gold and silver, 1,943,000; other metals, 28,515,000; salts, 7,640,000; combustibles, 18,050,000; making in round numbers a total of about 56 millions, exclusive of manganese. Now to this amount Great Britain contributed considerably more than one-half, viz. 29 millions, in the following proportions:—Silver, 28,500; copper, 1,369,000; lead, 769,000; iron, 11,292,000; tin, 536,000; salts, 756,250; vitriol, 33,000; alum, 33,000; coal, 13,900,000. He then gave a sketch of the history of mining in Great Britain, dwelling strongly on its vast increase since the introduction of the steam-engine.

Evening Meeting at the Theatre.

The Secretaries having read abridged reports of the proceedings of the Sections, a very interesting letter was read from Sir John Herschel to Sir William Hamilton.

Thursday, August 25.

Section A.—Mathematical and Physical Science.

Mr Peacock read a communication from Mr Talbot, "On the Integral Calculus."

Dr Apjohn made a communication "On the use of the Wet-bulb Thermometer, in determining the specific Heat of Air."

Professor Sir W. R. Hamilton then made a communication "On the Calculus of Principal Relations."

The Rev. Mr Scoresby gave an account of two very delicate Magnetic Instruments.

Professor Forbes read a paper "On Terrestrial Magnetic Intensity at great Elevations from the Earth." The author began by giving a rapid review of Saussure's observations connected with this subject. It was well known (he said) to men conversant with these researches, that this enterprising philosopher and naturalist ascended Mont Blanc, nearly at the summit of which he resided for many days, making and recording numerous meteorological experiments, at an elevation of about 11,000 feet above the level of the sea; but when his observations upon the magnetic needle were properly corrected, for the depression of temperature well known to exist at these great elevations, the result of them was, that at this great elevation there was no alteration of magnetic intensity which could be safely pronounced to be beyond the limits of the errors of observation. Subsequently, Gay-Lussac ascended in a balloon to the altitude of about, or perhaps beyond, 23,000 feet, yet his observations also, when due allowance was made for alteration of temperature, gave no alteration of the magnetic intensity. But the researches of M. Kuppfer seeming to conduct to a quite opposite conclusion, and the result, as stated by him, being such as, if the observations were correctly made, would give a diminution of the magnetic intensity for stations whose elevation above the earth was considerable, which could by no means be accounted for by ordinary errors of observation, Professor Forbes deemed this a matter of so much importance to science, that he determined to make an extended series of observations at various levels among the Pyrenees and Swiss Alps. Accordingly, having last summer provided himself with a sufficient number of magnetic needles fit for making proper observations upon magnetic intensity, and their times of vibration at Paris having been accurately ascertained, he commenced his tour for this purpose in the neighbourhood of Barège and Bagnières; and, from a multitude of observations which he had made and recorded, he now wished to select a series of forty-five observations made at thirteen different stations, the elevation of which above the level of the sea varied from 6000 to 10,000 feet. Before he detailed these observations and their results, he described the principle upon which they were conducted, and which appeared ingenious, and well calculated to lead to satisfactory results. In each instance, the observations were made at three distinct stations —one on the summit of the mountain peak, or most elevated spot; and two at a lower, but equal level on each side of the hill, so chosen, that a vertical plane would pass through the three stations, and be perpendicular to the axis or length of the hill. It is obvious then, that, speaking generally, any disturbing effect exercised upon the needle by the materials of the hill at one of the lower stations, would be opposite in kind to thatexercised at the other of the two lower stations; and, therefore, the mean between these observations, made at the two lower stations, would give the magnetic intensity at a point immediately beneath the upper station. By a comparison of this mean intensity, therefore, with the intensity at the upper station, it could be readily proved whether or not the intensity diminished as you ascended to a greater elevation. The result of the entire of this laborious course of experiments was, that, with the exception of one solitary instance, the station being in the Pyrenees and in the neighbourhood of iron mines, there was no diminution of the magnetic intensity at the higher stations, at least beyond the limits of the necessary errors of instruments and observations: even at the station where some diminution did manifest itself, the quantity of that diminution was very much smaller than that which resulted from the views of M. Kuppfer.

Professor Powell read a paper " Respecting the impermeability of water to radiant heat."

A paper by Sir David Brewster, " On the action of crystallized substances upon Light," was then read by the Secretary, Mr Snow Harris.

Dr Williams gave an account of an improved Ear Trumpet.

The President then said, that as there were yet a great many interesting communications to be brought forward, the Section would reassemble at eight o'clock in the evening.

THURSDAY EVENING.

Mr G. W. Hall made a communication upon "The Connexion observed at Bristol between the Weather and the Tide." He commenced by stating, that long and carefully-continued observation of the weather at Bristol, together with a direct interest in becoming possessed of rules for anticipating its changes, led to the following theory, which was strikingly correspondent with facts:—1st, That the barometer very generally, indeed, almost invariably, undulates at times corresponding with the changes of the moon, and at these times it more frequently falls than rises. 2dly, That the weather is ordinarily unsettled at these periods, continuing so for about two or three days; and for the most part the wind becomes high at these times. 3dly, That as the weather settles (if it become at all settled, since it not unfrequently remains in an unsettled state), so will it continue until the next change of moon, or rather until the recurrence of its disturbing influences. 4thly, That these variations occur as regularly at the quarters of the moon as at the new and full, and are then as fully marked. 5thly, That the period, about five days, which determines the state of the weather, is derived from the spring and neap tides, or the full influence of the sun and moon upon them.—The only origin of these rules, he stated, was actual observation. Very striking changes of temperature and weather from intense frost to spring mildness, and then frost recurring, first led to marking this correspondence; and so closely has it been observed, and so fully established, that operations upon a large scale, which are dependent upon the weather, have been frequently and successfully conducted in accordance with these rules. He considered the severe frost of 1813–14, which continued about twelve weeks, with partial thaws intervening, and the severe weather of succeeding winters, with their intermissions, to be closely connected with the above rules. The partial rains also of very dry summers have been found to take place at the same seasons of change, insomuch that for amusement he had frequently traced back the periods connected with the age of the moon, from the thaws that took place in severe weather, or the rains occurring in long-continued drought. Residing on the banks of the river, and taking much interest in the operations of Professor Whewell respecting the tides, and his description of these, Mr Hall stated, that he had been led closely to compare them with the weather; but difficulties to him insurmountable had occurred, when considering the variations of weather in different places at the same time; yet, regarding those in the neighbourhood of Bristol, his conviction was unwavering. Perhaps the varying time at which the tide reaches various places, so fully described by Professor Whewell in his lecture on Tuesday evening, might assist in solving this difficulty; and if the attention of others were directed towards it, his end would be attained.

Mr Ettricke then gave a description of "An Instrument intended to observe Minute Changes of Terrestrial Magnetism," and of other philosophical instruments.

Mr R. Addams then made a communication respecting the Vibration of Bells.

Mr Rootsey then read papers "On the Music of the Greeks, and a System of Mnemonic Logarithms."

SECTION B.—CHEMISTRY AND MINERALOGY.

Dr Daubeny stated, that he had ascertained that the sublimation of carbonate of magnesia was entirely a mechanical process, and he inferred that no support could hence be given to Von Buch's well-known theory of dolomization.

Dr Dalton then gave an exposition of his views upon the subject of Chemical Notation, and the atomic constitution of chemical substances.

Mr Johnston explained the use of some chemical tables which he exhibited.

Dr Thomson read a very valuable paper on Mixtures of Sulphuric Acid and Water, in which he shewed that the theory of Irvine respecting specific heat cannot be true.

Mr Jones detailed the results of an elaborate Analysis of Wheat, and mentioned that he had formed a new and peculiar volatile fluid by the action of sulphuric acid on wheat.

SECTION C.—GEOLOGY AND GEOGRAPHY.

A paper was read by the Marquis Spineto on the geographical position of Memphis, in Egypt. The state of that city during the time of its long prosperity was first considered; then the causes of its destruction; and lastly, the opinions of different travellers regarding its position. Its particular site had been described by ancient historians as on an island in the Nile, evidently formed of the mud of that river; and that it had been protected from inundations by various extensive works erected by its kings. When its splendour decayed, these works went out of repair, and hastened the ruin of the city, which strewed with its fragments the place on which it had stood. Finally, it was submerged under drifted sand, and its true position became a problem to modern travellers. Of late, however, the site has been determined by the French, who, in one of their exploring expeditions, had examined the stratification of the place supposed to be Memphis, and they ascertained the spot by the succession of drifted sand, ruins, and mud. Its latitude they fixed at 29° 20′ N. and longitude at 31° 30′ E. from Greenwich.—Mr Murchison spoke of the great value of geographical papers to the geologist, and of the one just read, as an excellent example of this kind.—Dr Buckland took this opportunity of mentioning the establishment of Mr Van der Maelen, at Brussels. That gentleman had devoted, in the most praiseworthy manner, his time and fortune to the advancement of science, by making large geographical and geological collections, for the purpose of diffusion over the world, by means of exchange with societies or individuals. Dr Buckland advocated such a mode of obtaining maps and specimens to the different provincial societies of the United Kingdom.

The next paper was on the change in the chemical character of minerals induced by galvanism. 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 electro-magnetic condition of metalliferous veins, and also on the electric conditions of various ores to each other, seem to have supplied an answer, inasmuch 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, 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 also taken from 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 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, shewing a remarkable analogy to the relative position of tin and copper ore with respect to each other, as they are found in mineral veins.

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 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 his time to electricity, and he had latterly been occupied in improvements in the voltaic power, by which he had succeeded in keeping it in full force for twelve months by water alone, rejecting acids entirely. Mr C. then proceeded to state, that having observed in a cavern in the Quantock Hills near his residence, that part of it which consisted of slate was studded with crystals of *arragonite*, while the limestone part was covered with crystals of *calcareous spar*, he subjected portions of each of these substances in water, to long continued galvanic action (ten days action), and obtained from the slate crystals of arragonite, from the limestone crystals of calcareous spar. In order to ascertain if 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 whole 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 variations of his experiments he had obtained crystallized quartz, the blue and green carbonates of copper, chrysocolla, phosphate of copper, arseniate of copper, acicular carbonate of lead, sulphate of lead, sulphuret of iron, white antimony, and many other minerals.

Professor Phillips then gave an interesting description of a bed of magnesian limestone, which exists near Manchester.

Evening Meeting.

Mr Murchison exhibited a map of England, coloured to represent some phenomena of physical geography, and for the purpose of answering a question proposed by the Association. On a former evening Mr Phillips had given an account of the boulder stones found in the north of England, and which had been traced even as far as Worcestershire. Mr Murchison, in his researches in Wales and the neighbouring counties, had not observed these carried to the country bounded by the Severn, nor had he observed any of the silurian gravel carried to the central parts of England. From this he concluded that Siluria must have been formed subsequently to this central part, which might have been an island or part of the continent. In this country of Siluria he had found the deposits of gravel perfectly local; nor could he perceive in this gravel any recent shells; on the borders of the South Wales Coal Basin were marks of diluvial action—fragments of coal strata being thrown off as from a centre. Another proof of the newer elevation of this part of Britain, are the marks of large lacustrine expanses at recent periods. Out of this tract not only do we observe the boulders of granite extending from north to south, but we find fragments of recent marine shells in the diluvium of Lancashire, Cheshire, Salop, and part of Stafford, all diminishing as we approach the Severn. But he was of opinion that these boulders could not have been so diffused when the surface had been dry land, but that the operation must have been effected under the sea, as proved by the presence of these marine shells, and by the fact of boulders having been found on the summits of the sides of valleys, which could not have been brought to those positions save by the agency of currents of the ocean. This later period of the elevation of Siluria, must have produced also the present course of the Severn. In concluding his remarks, Mr Murchison mentioned the possibility of icebergs assisting in the transport of diluvium.—Mr Conybeare mentioned the fact of chalk boulders being found upon Flat Holm, near Bristol, which stones must have been brought down by the Avon.

Section D.—Zoology and Botany.

Dr Moore announced his having procured a fish in Plymouth Harbour, new to Great Britain, the *Trigla cataphractes*, and Mr Yarrell confirmed the accuracy of the observation, and stated the species to be common in the Mediterranean.

Dr Richardson then read the concluding portions of his report. The order Edentata is eminently South American, and only three or four species are met with in North America. The fossil species of Megatherium and Megalonyx, however, are found in both Americas.—The order Pachydermata is remarkable for the size of most of its species, and the number of the extinct species is more than double the recent ones in the New World. Only two genera and three or four species belong both to North and South America. Fossil elephants and mastodons occur in the most distant parts of North America. Although the present race of horses is certainly of European origin, yet fossil bones of this quadruped are met with in Kotzebue's Sound.—Thirteen species of Ruminantia were enumerated, two of which are common to the old and new continents, and have a high northerly range. The North American deer are very imperfectly known. The reindeer reach to Spitzbergen and the most northerly of the American islands, and range southwards as far as Columbia River on the Pacific coast, and to New Brunswick on the Atlantic. Although the musk-ox ranges from the barren lands over the ice to Parry's Islands, it is not found either in Asia or Greenland.—There appears to be nine species of Cetacea, known as North American, and those on the east coast are mostly inhabitants of Europe also, under the same parallels of latitude, especially those of the Greenland seas. On the western side the species are common to Asia also.—The report then proceeded with an account of the Ornithology, which Dr Richardson said it would be unnecessary to touch upon at so great length or with so much detail as the Mammalia, since the species were so much better known, a great majority of them being migratory, and therefore those which lived in the less frequented regions were, at stated seasons, visitants of the more civilized districts. Local lists, however, were still wanting to enable naturalists to trace their geographical limits with precision, and, more especially, our knowledge was very imperfect of those of California and Russian America. Of about 500 species, there were one-fourth to be found in Europe, but not more than one-eighth in South America. Of the former, or those common to North America and Europe, thirty-nine were land-birds, twenty-eight waders and sixty-two water-birds. Several of the generic forms were peculiar, but only two of the families, viz. the Trochilidæ and Psittacidæ, were not to be found in Europe; and the Hoopoe is the only European representative of the whole order to which the former of these families belongs.—No vultures are common to both worlds, but nearly half the other birds of prey are so, and many of these range over South America also, and indeed the whole world. One-fourth of the Corvidæ are inhabitants of Europe; but the other land-birds, common to both continents, are in much smaller proportions, and not more than two out of sixty-two Sylviadæ are European. The number of species common to North and South America is very uncertain. Some of the most numerous families characteristic of the former country have few or no species in South America. It is remarkable that only one Trochilus has been described as common to North and South America, although this family is peculiarly characteristic of the latter country; and there are twenty-two species which have been described as natives of Mexico. Dr Richardson then detailed several particulars respecting the migration of birds, stating it to be his opinion, that the spring movement was for the purpose of finding a convenient place for incubation and rearing the young. The lines of route were influenced by the supply of food to be obtained, and thus the northerly and southerly courses were often over different tracts; and he pointed out the three great lines of route which were to a certain extent determined by the physical features of the country. The absolute number of birds to be found in different countries decreases on receding from the Equator towards the North Pole; but of those which stay to breed in any place, the number increases from the Equator up to the 60th degree of north latitude, where the forests begin to grow thin. But the progress of civilization has already had an influence on the migrations of certain species, by affording them an abundant supply of provisions, where they were before without any. Thus the starlings proceed further north as the culture of the Cerealia continues to extend in that direction, and the introduction of certain tubular flowers into the gardens of Florida, has enticed species of humming birds thither from the south. Some details were then given of the distribution of the various families of birds, and a table in the report exhibited the absolute number of species, as well as the number of such as breed in Philadelphia, Massachusetts, and Suskatchewan.

Mr Phelps read a communication "On the formation of Peat."

Mr Mackay then read a communication he had received from Mr Nuttall, "On the management of the Pine Tribe."

Dr Lloyd read a communication on the Marsileaceæ.

An abstract of a paper from Mr P. Teale was read, "*On Alcyonella stagnorum*;" and very beautiful preparations and specimens of it were placed on the table. It was found in great abundance from August to November, in 1835, in a small pond near Leeds. It was supposed to be new to Great Britain.

Dr Riley mentioned a circumstance in the osteology of the two-toed ostrich, which had escaped observation. He shewed, that the third toe was really present in a rudimentary state concealed by the integuments. It consists of two phalanges, and is articulated with a well-defined condyle of the tarsal bone, and projects on the same plane with the other two.

Section E.—Anatomy and Medicine.

Dr Hodgkin read a paper on the connexion between the veins and absorbents.

Dr Reid of Dublin then read to the section a paper, entitled, "A short Exposition of the Functions of the Nervous System."

Section F.—Statistics.

Professor Forbes described the result of his application of Quetelet's principle, of describing the increase of stature, weight, and strength by curves. He had carefully experimented on English and Scotch students, between the ages of fourteen and twenty-five, in the University of Edinburgh. The general laws of the curves were nearly those established by Quetelet. In the comparison of nations, the Irish appeared to be the first in all physical developments, the Scotch ranked next, the English were the lowest of the three nations, but they were above the Belgian. It was generally remarked, that the data for the Irish and English were not sufficiently accurate to justify any general conclusions.

A paper from Dr Collins on periodicity of birth was read.

Baron Dupin exhibited two maps of Britain, coloured on Guerry's plan, to illustrate Criminal Statistics, and their relation to density of population and education. The latter was both the more prominent, and, in relation to subsequent discussion, the more important branch of the Baron's observations. He drew a distinction between moral and physical education, describing the latter as an indifferent instrument capable of being applied either to good or evil. He then briefly glanced at the proportion between juvenile offenders in England and France, stating as a general result, that the young criminals of England more frequently reformed than those of the Continent.

Friday, August 26.

Section A.—Mathematical and Physical Science.

Mr Whewell read a paper on a new Anemometer. In this communication Mr W. explained a method of tracing or registering the course of the winds through a given period by the motion of a pencil, on an appropriate scale, so as to obtain eventually a true type of the winds, which has never yet been arrived at by other instruments.

Professor Phillips read a Notice of the probable Effects of elevated Ground in the Direction of the Lines of equal Magnetic Dip.

Professor Stevelly read a Paper on the Mathematical Rules for constructing Compensating Pendulums.

Telescopes.—Sir D. Brewster read a paper describing a contrivance by which he was enabled to render distinct the dark lines of the spectrum under the most unfavourable circumstances, and obtain other useful effects. The method was to introduce a cylindrical refractor between the eye and the eye-glass of the telescope, the effect being, as he shewed, to give a linear form to a most irregular image.

Mr Russell read a paper on certain Elements of the resistance of Fluids that appear to be intimately connected with the Application of Analysis.

Dr Hare read a communication relating to the prevailing theories of electricity; he endeavoured to explain many interesting phenomena attendant on the electric spark and the divergence of electrified bodies.

Dr Carpenter described a System of teaching the Blind to read, similar to Mr Lucas's.

Mr Hodgkinson read an Account of some Experiments, at the request of the Association, to determine the comparative Strength and other Properties of Iron, made with the hot and cold blast, at the Carron, Devon, and Buffrey Works, under similar circumstances.—In the Carron and Buffrey works, the strength was rather in favour of the cold blast. In the Devon iron the advantage was much in favour of the hot blast; but it is proper to remark, that the cold blast iron was very white in the break, and that from the hot was grey.

Section B.—Chemistry and Mineralogy.

The following papers were read:—Some improvements on the Voltaic Battery. By Mr Crosse.—Observations on Atmospheric Electricity. By Mr Crosse.—On a new compound found during the destructive distillation of Wood. By Mr Scanlaw.—On a peculiar compound of Carbon and Potassium. By Professor E. Davey.—On a new gaseous Bicarburet of Hydrogen. By Professor E. Davey.—On the conducting power of Iodine. By Dr Inglis. —On Fluorine. By Mr Knox.—On detecting the Strength of Spirits, by diluting with Water. By Mr Black.—Communication on the Aurora Borealis. By Dr Traill.

Section C.—Geology and Geography.

Sea Rivulets in the Island of Cephalonia.—Lord Nugent read a communication respecting some sea rivulets in the Island of Cephalonia. The water, he said, entered the earth through fissures in the rock on the seashore, and it was not discovered where it emerged, but it was supposed to flow into the sea, on the side of the island nearest Ithaca. Some observations were made by the Marquis of Northampton, Mr Murchison, Dr Daubeny, and the Chairman, but no solution of the problem was given.

Tertiary Deposits.—Mr Charlesworth read an elaborate paper on some fallacies in Mr Lyell's test in determining the ages of tertiary deposits by the per centage of existing species, which may be considered as a continuation of his paper on Crag Formations.

Professor Forbes made a communication on the connection of the Pyrenean hot springs with the geology of the district, in which he gave an outline of the physical structure of the Pyrenees.

The Rev. Mr Clarke stated the existence of two hot springs at Longfleet, near Poole, maintaining their temperature of fifty-four degrees in all seasons of the year.

A communication by Professor Traill of Edinburgh was read, giving an account of various localities of fossil fishes in Pomona, Orkney. A map and specimens were exhibited, and observations made by Dr Buckland, Mr Greenough, and others. A drawing of a remarkable fossil fish from Clashbinnie, Forfarshire, was laid before the Section by John Robison, Esq. of Edinburgh.

Section E.—Anatomy and Medicine.

The first paper read was entitled "Observations on the Pathological Condition of the Bones in Chronic Rheumatism;" and "On the Condition of the new Circulating Channels in a case of Double Popliteal Aneurism. By Mr Adams."

The third paper read was a Report on "Fracture of the Neck of the Thigh Bone. By Dr Evanson."

Mr Hetling read a paper "On a new Instrument for the Removal of the Ligature of Arteries" at pleasure.

The last paper read was on the Chemistry of the Digestive Organs, by Mr R. T. Thompson.

Mr Gordon, dentist, of Park Street, exhibited (although in an unfinished state) some beautiful models, in ivory, representing the head, neck, heart, and lungs of the human body.

At the meeting of the General Committee, held on Saturday, August 27, it was determined that the Meeting of the Association for 1837 should be held at Liverpool, in the month of September. The following are the Officers appointed: The Earl of Burlington, *President;* Dr Dalton, Sir Philip Egerton, Rev. E. G. Stanley, *Vice-Presidents;* Dr Charles Henry, Mr Parker, *Secretaries.*

SCIENTIFIC INTELLIGENCE.

GEOLOGY AND HYDROGRAPHY.

1. *Subsidence of the Coast of Greenland.*—In a letter from Dr Pingel of Copenhagen, to the President of the Geological Society of London, it is stated that the first observations which led to the supposition that the west coast of Greenland had subsided, were made by Arctander between 1777 and 1779. He noticed, in the firth called Igalliko, (lat. 60° 43′ N.), that a small, low, rocky island, about a gun-shot from the shore, was almost entirely submerged at spring tides, yet there were on it the walls of a house fifty-two feet in length, thirty feet in breadth, five feet thick, and six feet high. Half a century later, when Dr Pingel visited the island, the whole of it was so far submerged that the ruins alone rose above the waters. The colony of Julianahaab was founded in the mouth of the same firth in 1776; and near a rock, called the Castle by the Danish colonists, are the foundations of their storehouse, which are now dry only at very low water. The neighbourhood of the colony of Frederickehaab (lat. 62° N), was once inhabited by the Greenlanders, but the only vestige of their dwelling is a heap of stones, over which the firth flows at high water. Near the well known glacier which separates the district of Frederickehaab from that of Fiskenass, is a group of islands called Fulluartalik, now deserted; but on the shore are the ruins of winter dwellings, which are often overflowed. Half a mile to the west of the village of Fiskenass, (lat. 63° 4′ N.), the Moravians founded, in 1758, the establishment called Litchtenfield. In thirty or forty years they were obliged once, perhaps twice, to move the poles upon which they set their large boats, called umiak or women's boat. The old poles still remain as silent witnesses, but beneath the water. To the north-east of the mother colony Godthaab, (lat. 64° 10′ N.), is a point called Vildmansnass, by St Egede, the venerable apostle of the Greenlanders. In his time, 1721–1736, it was inhabited by several Greenland families, whose winter dwelling remains desolate and in ruins, the firth flowing

into the house at high tide. Dr Pingel says, that no aboriginal Greenlander builds his house so near the water's edge. The points mentioned above, the writer of the letter had visited; but he adds, on the authority of a countryman of his own, highly deserving of credit, that at Napparsok, ten Danish miles (forty-five English) to the north of Nÿ-Sukkertop, (lat. 65° 20′ N.), the ruins of ancient Greenland winter-houses are to be seen at low water. Dr Pingel is not aware of any instance of subsidence in the more northern districts; but he suspects that the phenomenon reaches at least as far as Disco Bay, or nearly to 69° north latitude.

2. *Quantity and Proportion of the constituent Parts of Atmospheric Air in Water.*—Dr Thomson finds that 100 cubic inches of Clyde water contain 3,113 inches of air. This air, extracted from the water by boiling, afforded 70,9 volumes of azotic gas, and 29,1 volume of oxygen gas; = 100,0. Thus the air extracted from water by boiling is much richer in oxygen gas than common air, containing rather more than 29 per cent., while common air contains only 20 per cent. by volume. But this excess of oxygen diminishes so rapidly when the air thus obtained is inclosed in a vessel over water, that after four days it does not contain more than common air.

3. *Use of Nicol's Calcareous Spar Prism in discovering Shoals in the Ocean.*—M. Arago remarks, that the bottom of the sea, or the surface of a shoal *at a given distance from the ship*, is more distinctly seen from the masthead, or, generally speaking, from a considerable height, than from the deck. This phenomenon he explains by remarking, that the reflected light from the surface of the sea, which is always intermixed with that from the bottom, or the shoal, possesses a less and less degree of intensity in proportion as the angle of reflection, reckoned from the surface, is larger. In order fully to remove this reflected light, he proposes, when it is wished to look into the sea to discover cliffs or shoals, &c. to observe them by means of a tourmaline, in which the axis is held horizontally, if possible, under a polarizing angle of 37°, reckoning from the surface. The entire and absolute obstruction of the light reflected from the surface of the water cannot possibly take place under a smaller

angle than 37°, because it is under this angle alone that it is completely polarized; but under angles of 10° or 12° greater or less than 37°, the number of polarized rays which the tourmaline can arrest is still so considerable, that the same means of observation cannot fail to be attended with very advantageous results. Poggendorf proposes to use for this important purpose, instead of the tourmaline, Nicol's *Calc Spar Prism,* because, from its being colourless, it is much better fitted for the purpose. By engaging in such experiments, "mariners," Arago remarks, "will throw a light on a curious question of photometry: they will probably confer on navigation a means of observation which may prevent many shipwrecks; and by introducing polarization into the nautical art, they will afford an additional proof of what those individuals expose themselves to who unceasingly collect experiments and theories without any practical application of them, meeting every remonstrance with a contemptuous *cui bono.*" Ere long, we doubt not, ships generally, at least all those vessels specially occupied in geographical and hydrographical researches, will before leaving port be provided with Nicol's valuable little instrument.

4. *Thermal Springs in the Columbia Territory.*—In a letter to Professor Jameson from Dr Gardiner, dated 18th September 1835, is the following notice:—" I have ascertained the existence of three more *thermal springs* in the Columbia territory, in addition to those I formerly noticed.* Specimens of the water of these have been sent to me; one only have I been able to analyze; but the quantity of water was too small to enable me to determine the numerical proportions of the ingredients. These were carbonic acid in large quantity, carbonates of lime and magnesia, muriate and sulphate of soda, and silica; the total amount of solid matter was = 26,0 in 10,000 parts of water. Its specific gravity was 1007.1.—The silica was in large quantity. It is situated on Bear River, which discharges itself into Great Salt Lake, in the desert between Snake River and Mexico, and is in the midst of a country from which I have specimens of obsidian and pumice. You may readily

* Ed. Phil. Jour. vol. xx. p. 206.

conceive how much all these make me regret the impossibility of my visiting those regions, so interesting to the geognost."

5. *Discovery of Carbonate and Sulphate of Lithia in a Spring.*—A mineral spring recently discovered at Rosheim, between Strasbourg and Schelestad, has been found to contain two substances hitherto unknown as ingredients of spring water, viz. sulphate and carbonate of lithia.

6. *Siliceous Sinter of Iceland.*—M. Robert, in his account of the geology of Iceland, mentions, that in the vicinity of the Geysers, the siliceous concretionary deposits constitute a mass of no less than four leagues in length.

7. *Native Mercury in Granite.*—M. Alluaud sen. of Limoges, has communicated to the Philomathic Society of Paris a notice regarding the mercury of Peyrat-le-Chateau, in the department of Haute Vienne. The district is composed of different varieties of granite passing into each other, and also into pegmatite, gneiss, and similar rocks. It is in the decomposed quartzose granite of the esplanade of the ancient castle that disseminated liquid mercury has been found at various isolated points.

8. *Fall of part of Dent du Midi.*—M. Elie de Beaumont read to the Geological Society of France a communication from M. Lardy upon the fall of a part of the *Dent du Midi*, one of the high Alps. This fall took place on the 26th of August 1835. M. Lardy states, that on Tuesday the 25th of August there was a violent storm in the evening all round the Dent du Midi; and it was asserted that its peak was often struck by the thunderbolt. Next day, the 26th, between ten and eleven o'clock in the morning, a very considerable portion of this peak suddenly broke off from its eastern edge, and precipitated itself with a dreadful crash upon the glacier which is situated upon the southern side of the *Dent*, and in its descent drew along with it an immense proportion of this glacier. This enormous mass of stone and ice fell into the deep ravine which separates the *Dent du Midi* from the *Col de Salenfe*, into which the torrent of St Barthelemy runs. Speedily there issued from this gorge, through which this torrent flows to the valley of the Rhone, as it were, a mountain of black and viscid mire, on the

surface of which there floated vast masses of rock of all dimensions, some of them as much as twelve feet high. This liquid mass, like a flow of lava, directed itself towards the Rhone, across the forest of pines which covers this part of the valley, drawing along with it every thing it met in its way. Trees of the largest size were overturned, and crushed like reeds. On reaching the bank of the river, it precipitated itself into it, thus forming an expanse of mud, which was fearful to behold. The fragments of rock contained in the mud were also impelled into the Rhone, whose waters were thrown to the opposite bank, and forced to reascend their channel to a considerable distance. The great road covered by this mire and these stones became impracticable, and it was necessary to construct, by means of fagots, &c. a new road on this elastic soil. For many days the communications between the high and low Valais were effected by means of a bridge which was far from being stable, thrown across the torrent at the commencement of the gorge. It is impossible to conceive any thing more frightful than this ravine of from sixty to a hundred feet deep, with a breadth of from two to three hundred feet, and which augments in size as far as the Rhone, choked up with this really frozen mire, with its surface studded with great blocks of stone, and the trunks of trees. A small portion only of the peak fell to the northern side of the *Dent du Midi*, which, descending by a slope, covered a part of a glacier which is on that side.—After reading this notice from M. Lardy, M. Elie de Beaumont gave some additional details regarding the phenomena, a part of which he had witnessed. He particularly insisted on what appeared especially curious as to the mode in which the muddy currents, produced by the fall, spread themselves over the great cone of debris of the torrent of St Barthelemy, and which is at an inclination of from five to seven degrees. These torrents of mud did not contain perhaps a tenth part of water, and yet they displaced blocks of limestone several yards long, and even floated them on their surface for considerable distances, almost as easily as a river floats ice. Notwithstanding its comparatively small dimensions, this phenomenon appeared to M. Elie de Beaumont to possess a peculiar interest, as leading to conclusions respecting the mode by which the transport of diluvian blocks or boulders is effected.—M. Huot, who was also an eye-witness of a part of this phenomenon, added some facts to the communication. The nature of the soil, which was composed of calcareous schist and of a black marl, must have necessarily facilitated the eboulement. A cloud of dust rose to a great height for many days after the event, which, from a distance, had all the appearance of a volcanic eruption. It was remarked, that a person might walk upon the fluid mass almost at the same instant as it was extending itself in all dimensions. At the extremity of the valley, a deep valley of erosion was hollowed out in the Taswey.

9. *Trade in Chromate of Iron.*—Many years ago chromate of iron was discovered by Professor Jameson in the serpentine rocks of the Shetlands, and afterwards on the mainland of Scotland. This observation was in 1820 mentioned in one of the editions of his System of Mineralogy. Afterwards, his pupil, the now well known and distinguished geologist, Dr Hibbert, found chrome-ore in such quantities in the Shetland groups, that the quarries of it opened after his visit have realized a very considerable return to the proprietors. It is exported from the Shetlands as a raw material. Since the year 1826 this ore of iron has become in Norway, where it was discovered by the celebrated geologist Esmark, an article of trade. It was, until the year 1831, exported in the rough state, and with but comparatively little profit to the proprietors of the ground, to Altona, Hamburgh, Petersburg, England, and Holland. In the year 1830 not less than 1133 ship-pounds were sent to Havre de Grace alone. It being considered more profitable to export the prepared chrome in place of the crude ore, a company has been got up at Drontheim for this purpose, which purposes to supply the cotton printers in Britain, France, and Germany, and also the porcelain manufactories, with prepared chrome.

10. *Ehrenberg's new Discovery in Palæontology—Tripoli composed wholly of Infusorial Exuviæ.*—At the Royal Academy of Sciences of Paris, July 11th, the following letter was communicated, dated Berlin the 3d of July, from M. Alexander Brongniart:—" I have to-day 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 name 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 carcasses 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-Foria 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 had 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.——Professor Jameson and Mr Nicol have examined carefully characteristic specimens of polishing-slate (Polierschiefer), and found in them numerous remains of lacustrine infusoria, thus confirming the discovery of Ehrenberg, in so far as it applies to the Polier-slate.

11. *Dinotherium giganteum.*—It is said there has been lately found in the neighbourhood of Eppelsheim, at a depth of 28 feet, the head of the *Dinotherium giganteum*, the most colossal fossil quadruped hitherto discovered. The head is in a state of complete preservation; it measures 6 feet 3½ inches (French) in its greatest length, and it weighs about 500 pounds. There was found near to this head a humeral bone six feet long, which was considered to belong to the same animal.

12. *Mass of Green Malachite of extraordinary size.*—A few months ago there was met with in the mines at Nischne-Tagilsk, in Russia, a mass of green malachite measuring 16.2 French feet in length, 7.5 French feet in breadth, and 8.6 French feet in height, and weighing about 1300 Russian pounds.—*Poggendorf's Annalen*, No. 1, 1836.

13. *The Coal Formation of the United States.*—1. Valuable beds of bituminous or common black coal occur *low down* in the carboniferous limestone, as well as in the higher accumulation of the common coal-bearing measures. 2. Beds of limestone, with marine animal remains, also occur in the true coal measures. 3. The great deposits of *anthracite* or *glance-coal* belong not to the transition series, as formerly maintained, but to the coal-formation properly so called. 4. There is an alternating series of *red sandstones and red shales*, crowded with *productæ* and crinoidal remains, and occasionally with caryophylleæ, pectens, and spiriferæ, supporting productive coal-measures, and resting upon a great body of mountain limestone, which limestone rests upon old red sandstone.—*Weaver in Lond. and Ed. Phil. Mag.* Aug. 1836.

14. *Difference of Temperature between Granite and Slate in the Cornish Mines.*

GRANITE.

Depth.		Number of Observations.	Temperature.
Surface to 50 fathoms,	Average 31 fathoms,	7	51° 6′
50 to 100	79	15	59 0
100 to 150	133	11	63 4
200 and beyond	277	3	81 3

SLATE.

Depth.		Number of Observations.	Temperature.
Surface to 50 fathoms,	Average 35 fathoms,	21	57° 0′
50 to 100	73	19	61 3
100 to 150	127	29	68 0
150 to 200	170	21	78 0
200 and beyond	221	5	86 6

These observations were in all cases made on jetting and running streams of *water*, on their issuing from the *unbroken* rock; a mode which Mr Henwood thinks more likely to approach the true temperature of the earth at and near the place of observation, than if the thermometers were inserted in holes bored in the sides of the galleries, as these are much affected by the air circulating in the vicinity, which, also, being itself exposed to so many disturbing causes, cannot be regarded as giving more than a very distant approximation to the real temperature of a given spot.—*W. J. Henwood, Esq. in Records of Science*, Sept. 1836.

15. *The Uniformity of the Form of Mountain Slopes in the Higher Alps, with an attempt to explain the facts, and an exposition of the resulting consequences.*—Everywhere, at the height of about 5000 feet above the level of the sea, in that line in which in the Alps the region of forests terminates, and gives place to that of pasturage, there appear, as if by enchantment, great smiling plains of about a mile broad, and sometimes many leagues long, which are bounded by vertical walls of rocks, by fields of snow, or by slopes covered with debris, or rolled pebbles. On the contrary, if we descend lower, to the region where the forests cover the soil, there we everywhere find rapid declivities, cut into the shape of gorges and of funnels. This constant difference in the form of the slopes is owing to the difference of the vegetation by which they are covered. In those regions where the forests exist, not only do their roots protect the rapid declivities of the rocks from the action of atmospheric influences, but their trunks also arrest the descent of the debris which fall from the more elevated rocks. These debris in the long run undergo a change, and are converted into vegetable mould, which is ever making an addition to the soil which covers the rock. Under these circumstances the valley which the mountain forms continues to be straight, having all the characters of a narrow defile. On the contrary, in those places in which the forests are wanting, the sides of the mountains being freely exposed to all the vicissitudes of the weather, are unceasingly undergoing the process of decomposition, the bottom of the valley is gradually filled up with the debris which are constantly descending, whilst the lateral slopes are ever gradually receding from each other. Attentive observation, continued for several successive years, and directed to the decomposition and rolling down of the slopes which are not protected by forests, would supply the data for an approximate calculation of the length of time which would be necessary for the formation of the entire valley. From these considerations we may at once perceive how

injurious is the process of denuding the slopes of the Alps of their woody covering, a process which unfortunately is every day, and every where, rapidly accomplishing; since the necessary consequence is, that thus the disintegration rapidly proceeds, and the way is opened up whereby avalanches, and immense blocks of rocks, descend to the lowest and most cultivated parts of the valleys.—*Escher in Bib. Universel.*

16. *On the Chalk and Calcaire grossier of Meudon.*—M. Deshayes has lately communicated to the *Philomathic Society* of Paris, some observations which he made in a quarry at *Bas Meudon*, in which may be seen, as stated by M. d'Archiac to the Geological Society of France, the immediate contact of chalk and of the *calcaire grossier*, both of the formations, according to M. Deshayes, being clearly distinguished by the fossils they contain,—fossils which are altogether different in the two formations (terrains). In this quarry is to be found, immediately above the hard chalk, or *caillasse*, a bed of *calcaire grossier*, then different alternating strata of limestone, marl, and plastic clay; then the thick bed of plastic clay, and finally the upper *calcaire grossier*. The fissures, also, which penetrate into the chalk, proceeding from the upper surface of the bed of *caillasse*, are filled with a limestone which contains the shells of the *calcaire grossier*, which appears to M. Deshayes to confirm his opinion concerning the fixed distinction betwixt the two formations (terrains) which are superposed upon each other. M. Deshayes then, from these circumstances, maintains, that it ought not to be admitted as a general proposition, that the oldest tertiary formation is the lacustrine or plastic clay formation; he thinks that the distinction between the chalk and the tertiary formation, a distinction which is accurately determined by the difference of the fossils peculiar to these two geological epochs, leads us to recognise at Meudon, tertiary marine strata, analogous to the *calcaire grossier*, and yet situated underneath the plastic clay, and in immediate contact with the chalk. M. Elie de Beaumont, who has also examined the same quarry, stated to the Society that his observations had led him to quite a different conclusion. He thinks that the separation between the chalk formation and the tertiary strata is especially determined by the marked traces of distinct erosion which the waters have every

where produced on the surface of the chalk formation, as well as by the lacustrine deposits of clay, sand, pebbles, &c. which have been formed at the termination of this watery revolution. M. Elie de Beaumont found at Meudon the traces of such erosions immediately underneath the thick bed of plastic clay, particularised above by M. Deshayes, and accordingly he thinks that it is with this bed that the tertiary deposits commence; that the marine strata, which are situated lower down, ought to be considered as belonging to the upper part of the chalk formation; and that the zoological distinctions which M. Deshayes wishes to establish, and which in this case are supported upon a very few species only, the greater part of the shells collected here being indeterminable, are not sufficient to prevail against the grand geological characters which the general arrangement of the two formations presents, and that the only conclusion which we can admit here, is one which must be admitted for several chalk formations in the south of France, viz. that many species of animals, whose remains are found abundantly in the old tertiary formations, existed at the time of the deposition of the chalk formation.—*Proceedings of Phil. Soc.* June 1836. L'Institut.

BOTANY.

17. *Delightful Smell on approaching tropical lands from sea.*—The coast of Chili, says Poeppig, " appeared nearly to resemble the desolate region of Terra del Fuego." Even the peculiar smell was wanting which is usually perceived on approaching the coasts of countries between the tropics, and of which even animals are so sensible that they become restless, appearing to have a presentiment of the termination of their long confinement, and often boldly leap overboard to reach the shore, which they suppose to be close at hand. On this passage Poeppig says in a note,—" Whoever has made a voyage to the tropical countries of South America, or the West Indies, will always remember with pleasure the sensation which he experienced on approaching the land. Perhaps no sense is then so strongly affected as the smell, especially if you approach the coast in the early hours of a fine summer's morning. On the coasts of Cuba, the first land I saw in America, on the 30th of

June 1822, all on board were struck with the very strong smell, like that of violets, which, as the day grew more warm, either ceased, or was lost amidst a variety of others, which were perceptible as we drew nearer the coast. During a long stay in the interior of this island, I became acquainted with the plant which emits such an intense perfume as to be perceived at the distance of two or three milss. It is of the species *Tetracera*, and remarkable for bearing leaves so hard that they are used by the native cabinet-makers, and other mechanics, for various kinds of work. It is a climbing plant, which reaches the tops of the loftiest trees of the forest, then spreads far around, and in the rainy seasons is covered with innumerable bunches of sweet-smelling flowers, which, however, dispense their perfume during the night only, and are almost without scent in the daytime."

ZOOLOGY.

18. *Extract of a Letter from M. Gay to M. de Blainville, dated Valdivia, 5th July* 1835, *regarding the habits of Leeches of Chili, and the tendency exhibited by reptiles in the same country to become viviparous.*—It is a remarkable circumstance that here all the leeches live in the woods and never in the water; and, indeed, I cannot botanise without having my legs injured by their punctures. They crawl on plants, trunks, and shrubs, and never approach marshes or rivers; and the only one it has been my fortune to discover in such localities, is a very small species of "*Branchibolelle*," which lives in the pulmonary cavity of the *Auricula Dombeii*; it was in dissecting this molluscous animal that I met with it. In the neighbourhood of Santiago, I have discovered another species, which lives on the gills of the Astacus. A fact not less interesting, and which merits your attention, is the tendency exhibited by reptiles to become viviparous in these southern regions. The greater number of those I have dissected presented this remarkable circumstance. Thus, not only the harmless adder of Valdivia gives birth to living young, but also all the beautiful Iguanas allied to the genus *Leposoma* of Spix, and which, on account of their beautiful colours, I have, in the mean time, named *chrysosaurus*. The species which I have examined, even those which at Santiago deposit eggs, have all, without exception, presented this pheno-

menon, and I may therefore be permitted to generalise. The Batrachians have also afforded me some examples of this description, although in general they are all oviparous. Nevertheless a genus resembling the *Rhinella* of Fitzinger, and of which several species, rather prettily marked, form part of my collection, is constantly viviparous, and therefore increases the proofs of a fact which is rendered more remarkable by the circumstance that all the examples occur in a radius of two or three leagues only.

19. *On the Changes which the Stomachs of Crabs undergo during the period of casting their Shells.*—A very accurate account of these changes is given by Dr K. E. V. Baer, in the sixth number of Müller's Archiv 1834. Crabs, it is well known, change their shells at a certain season of the year; and it is a very old opinion that they change their stomachs at the same time, a new stomach being formed round the old, which is digested by the recently developed organ. Baer has proved that the crab's stomach consists of two coats; one inner, which in every respect may be compared to a callous, horny epidermis, and which is destitute of vitality; and an outer or containing coat, transparent, but sufficiently strong and vascular. The inner coat, as it is well known, consists of various and very curious parts, some resembling boney plates, others compared to teeth; now at the period when the crab changes its skin, it likewise casts the inner coat of the stomach, and on this account this process, analogous to the moulting of birds, and to the renewing of the hair in quadrupeds, is in the crab attended with very great constitutional disturbance, and a total interruption of the digestive function. Baer relates very accurately the changes which the stomach undergoes preparatory to the casting of its inner coat. It would be beside our present purpose to follow him in this description, however interesting. Some things he mentions are, however, specially worthy of remark; in the first place the softer parts of the old epidermis or inner coat of the stomach are very rapidly digested in the stomach, as soon as it has recovered its functions, and has, which it does quickly, formed a new lining on its inner surface. But there are other harder parts that cannot be so readily digested and dissolved, and which are otherwise disposed of. The hard and hollow bones, populary termed

the teeth, are got rid of by being discharged through the external orifice corresponding to the mouth. There are other solid plates of the epidermic portion of the stomach, which are not of a shape calculated to irritate the new and tender epidermis, and consequently they can be retained with impunity, and are destined to perform a new and curious function, for according to Baer, these plates, for some time preparatory to the act of casting the shells, rapidly increase in weight and in solidity, so as at the period we are speaking they may be considered as forming considerable reservoirs of earthy matter, to be gradually dissolved and digested in the newly lined stomach, at the very time earthy matter is required by the animal for the formation of its new shell. These plates are popularly called *crab-stones*, and when submitted to the digestive process soon lose their roughness, and become smooth and polished before they are entirely dissolved. These crab-stones are chiefly composed of carbonate of lime, and Baer has proved, by repeated analysis, that the fluid contents of a crab's stomach contain (at the time these stones are in them) a considerable portion of lime, carbonic acid, and muriatic acid. It is interesting to observe, that the chemical investigations of Dulk render it highly probable, that the chief solvent in the crab's stomach is the same acid which plays so important a part in human digestion and in dyspepsia, viz. *free muriatic acid.—Dublin Medical Journal.*

ANTHROPOLOGY.

20. *Dreadful Effects of the Immoderate Use of Coca.*—Dr Poeppig, in his Travels having mentioned the *Coca* plantations, gives a very long account of the remarkable plant, which has now become an indispensable necessary of life amongst the Indians of the Andes; and, as an article very extensively cultivated, deserves great attention. The coca (*Erythroxylon* Coca, Lam.) is a bush from six to eight feet high, somewhat like a blackthorn, which it resembles in its numerous small white blossoms, and the lively green of the leaves. These leaves, which are gathered and carefully dried, are an article of brisk trade, and the use of them is as old as the first knowledge of the history of Peru. It is a stimulant which acts upon the nerves in the same manner as opium. Unhappily, the use of it has degenerated into a very

which seems incurable. The Indians of America, especially those of the Peruvian Andes, notwithstanding the civilization which surrounds them, have a vague sense of their own incurable deficiency, and hence they are eager to relieve themselves, by violent excitements, from such melancholy feelings. This accounts not only for the use of the coca, but also for the boundless love of spirituous liquors, which possesses scarcely any other people in the world in an equal degree. To the Peruvian, the coca is the source of the highest gratification; for under its influence his usual melancholy leaves him, and his dull imagination presents him with images which he never enjoys in his usual state of mind. If it cannot entirely produce the terrible feeling of over-excitement that opium does, yet it reduces the person who uses it to a similar state, which is doubly dangerous, because, though less in degree, it is of a far longer duration. This effect is not perceived until after continued observation; for a new comer is surprised indeed at the many disorders to which the men of many classes of the people are subject in Peru, but is very far from ascribing them to the coca. A look at a determined *coquero* gives the solution of the phenomenon; unfit for all the serious concerns of life, such a one is a slave to his passion, even more than the drunkard, and exposes himself to far greater dangers to gratify his propensity. As the magic power of the herb cannot be entirely felt, till the usual concerns of daily life, or the interruptions of social intercourse, cease to employ the mental powers, the genuine *coquero* retires into solitary darkness or the wilderness, as soon as his longing for this intoxication becomes irresistible. When night, which is doubly awful in the gloomy forest, covers the earth, he remains stretched out under the tree which he has chosen; without the protection of a fire near him, he listens with indifference to the growling of the ounce; and when, amid peals of thunder, the clouds pour down torrents of rain, or the fury of the hurricane uproots the oldest trees, he regards it not. In two days he generally returns, pale, trembling, his eyes sunk, a fearful picture of unnatural indulgence. He who has once been seized with this passion, and is placed in a situation that favours its development, is a lost man. Dr Poeppig heard in Peru truly deplorable accounts of young men of good families, who, in an accidental visit to the woods, began to use coca to pass away the time, soon acquired a relish for it, and from that moment were lost to the civilized world, and, as if under some malignant spell, refused to return to the towns. We are told how the relations at length discovered the fugitive in some remote Indian village, and, in spite of his tears, dragged him back to his home. But these unhappy persons were as fond of living in the wilderness, as averse to the more orderly mode of life in the towns; for public opinion condemns the white *coquero*, as it does an incorrigible drunkard among us. They, therefore, take the earliest opportunity of escaping to the woods, where degraded, unworthy of the white complexion, the stamp of natural superiority, and become half savages, they fall victims to premature death, through the immoderate use of this intoxicating herb.

21. *Effects of compressed Air on the Human Body.*—Dr Junod has communicated to the Academy of Science 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 eighteen minutes an agreeable warmth is felt in the interior of the thorax. The whole economy 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 a 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 a 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 fifteen or twenty minutes there is a true dyspnœa. The pulse is full, compressible and frequent; the superficial vessels are turgid. The eyelids and lips are distended with superabundant fluids, and hemorrhage 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.—*Lond. and Edin. Phil. Journ.*, August 1836.

22. *Manner of obtaining Blood in cases where the Vein does not yield it readily; by Dr Burdach.*—The plan is applicable to all cases where open veins do not give a sufficient quantity of blood, or in bleeding fat persons where the veins are not very apparent. To produce the effect, says M. Burdach, it is merely sufficient to apply a ligature also on the other arm, as is you were about to open a vein in it. After an interval of from two to ten minutes, the vessels of both arms will be swollen and full of blood. As soon as the person feels numbness, the ligature is to be relaxed, and compression made with the thumb, that the blood of the open vein may flow in a jet; the flow of blood is to be kept up or stopped by tightening or relaxing both ligatures.—*Gräfe et Walther Journal der Chirurgie. Dublin Medical Journal.*

23. *Poisoning by Arsenic cured by the Hydrated Tritoxide of Iron.*—A remarkable case of this description is recorded in the *Gaz. Med. de Paris* (22d August 1835) by M. Monod. The subject of it was a hair-dresser, thirty-five years of age, who, in a paroxysm of delirium tremens, swallowed a drachm and a half of white oxide of arsenic. Half an hour afterwards the antidote was given to him, suspended in water, and he drank in twelve hours all the tritoxide produced by the decomposition of five ounces five drachms of the trito-sulphate of iron. He had no

violent colic, and twenty-hours afterwards experienced scarcely any uneasiness.—*Ibid.*

24. *Anatomical and Physiological Remarks on Hunchbacks.* —Although the observations of Dr Stern, recorded in Müller's Archiv, cannot be said to throw any new light upon this subject, yet they have served to place in nearer juxtaposition, and have elicited some curious points of comparison on the organization of this deformed class of persons. Even the most inattentive observer must have remarked the sort of family likeness, both of mind and body, that runs through the individuals labouring under this deformity, a likeness arising not merely from the existence of a hump in all, but from a similarity in complexion, in the general form of the head, and in the care-worn, superannuated appearance of the face. Their limbs, too, have all the same disproportioned appearance, and seem evidently fashioned to serve a trunk of larger proportions. But though the growth of other parts, as for example, the extremities, has not been suppressed equally with the growth of the trunk, yet neither has the development of these parts proceeded regularly, and it is to this curious phenomenon that the memoir of Stern is directed, for he proves that the different bones of the extremities in hunchbacks do not bear their due proportion to each other. Thus the *thigh-bone* is somewhat shorter than it ought to be, even in proportion to the diminished stature of the individual, while the bones of the feet are very large, and suited to a much taller person; of all the bones the humerus is proportionally the longest, and to this is owing the great comparative length of the upper extremities in people thus deformed. The skulls of hunchbacks present a very curious proportion between the cranium, properly so called, or brain case, and the skeleton of the face. In fact the former equals in size that of a well grown adult, while the bones of the face remain undeveloped and small, as in childhood; this gives their physiognomy a very curious expression, for in their heads old age and wisdom seem associated with several of the elements of childhood and simplicity. In the form of the lower jaw, in the great size of the mouth, and the compressed flatness of the lips, in the sharp elongated nose, we recognise a striking likeness between all hunchbacks. It is curious thus to find that a disease of one or

of a few vertebræ, occurring *at an early period of life*, serves not merely as a foundation for a permanent deformity in the spine itself, but proves that means of modifying the size and shape of even distant organs, such as the bones of the face and of the extremities; an occurrence of this sort taking place before our eyes, and long after birth, teaches us what we may expect from injuries or diseases of any important organ during the growth of the fœtus; prepares us for expecting that certain malformations of central organs necessarily give rise to secondary disturbances in the development of some given parts lying more in the circumference. It would be extremely interesting to determine what influence the *situation of the hump* has in disturbing distant development; is the general formation of the face and limbs, when the hump arises low in the back, different from that which distinguishes those whose humps occupy a situation higher up? It is curious enough, and contrasts strongly with the effects on the facial development produced by a hump on the back, that some infants are born with the face fully formed, but wanting the brain and spinal marrow!—*Ibid.*

25. *Socrates not poisoned by Hemlock.*—Dr Christison, in his memoir on hemlock, in the Edinburgh Philosophical Transactions, states, that the poison which terminated the existence of Socrates was not our hemlock, or, at least, that the description given of the symptoms produced by the action of the poison do not correspond with those of the hemlock known to us. Plato says, "When he felt his limbs grow weary, he lay down on his back, for so the man had told him to do, and at the same time the person who administered the poison went up to him, and examined, for a little while, his feet and legs, and then squeezing his foot strongly, asked him whether he felt him do so? Socrates replied, that he did not. After this the man did the same to his legs, and proceeding upwards in this way, shewed that he was cold and stiff. As he approached him he said to us, that when the effects of the poison would reach the heart, Socrates would depart; and now the parts about the lower belly were cold, when he uncovered himself (for he was covered up), and said, which were his last words, Crito, we owe Esculapius a cock, pay the debt, and do not forget it. It shall be done (replied Crito), but consider whether you have any thing else to

say. Socrates answered not, but in a short time was convulsed." The following were the phenomena observed when *conein*, or the essence of hemlock, was administered :—" Six drops were allowed to fall into the back of the throat of a young active puppy ten weeks old. In thirty seconds there was sudden convulsive respiration, and some stiffness of the hind legs, immediately followed by great feebleness of those legs. In a few seconds the fore-legs became also very feeble. In sixty seconds from the time the poison was introduced the breathing ceased. Slight convulsive tremors followed for a single minute more." The author concludes that the ancient poison is still unknown to us.

26. *Death of Mr David Douglas,* extracted from a Letter by J. Goodrich and J. Diell, published in the Ke Kumu Hawaii, a paper printed at Honolulu*, Nov. 26. 1834, *and inserted in Silliman's American Journal.*—" From Edward Gurney, an Englishman, we received the following account of the tragical scene: About ten minutes before six o'clock in the morning, Mr Douglas arrived at his house on the mountain, and wished him to point out the road to Hilo, and to go a short distance with him. Mr Douglas was then alone, but said that his man had gone out the day before (this was probably John, Mr Deill's coloured man). After taking breakfast, Edward accompanied Mr Douglas about three-fourths of a mile, and after directing him on the path, and warning him of the traps, went on about half a mile further with him. Mr Douglas then dismissed him, after expressing an anxious wish to reach Hilo by evening, thinking that he could find out the way himself. Just before Edward left him, he warned him particularly of three bullock traps, about two miles and a half a-head, two of them directly on the road, the other on the side. Edward then parted with Mr Douglas, and went back to skin some bullocks which he had previously killed. About eleven o'clock two natives came in pursuit of him, and said that the European was dead, and that they had found him in the pit in which the bullock was. They

* Mr Douglas was born at Perth, Scotland, and had travelled in various parts of the world as a naturalist, connected with the Horticultural Society of London. He was engaged in his scientific pursuits when he met with the fatal accident. It took place on the 12th of July 1834.

mentioned, that as they were coming up to this pit, one of them observing some of the clothing on the side exclaimed *lole*, but in a moment afterwards discovered Mr Douglas within the cave, trampled under the feet of the bullock. They went back immediately for Edward, who left his work, ran to the house for a musket and ball, and hide, and on coming up to the pit, found the bullock standing upon Mr Douglas's body. Mr Douglas was lying upon his right side. He shot the animal, and after drawing him to the other end of the pit, succeeded in getting out the body. His cane was with him, but the bundle and dog were not. Edward, knowing that he had a bundle, asked for it. After a few moments' search, the dog was heard to bark, at a short distance a-head, on the road leading to Hilo. On coming up to the place, he found the dog and the bundle. On further examination, it appeared that Mr Douglas had stopped for a moment and looked at the empty pit, and also at the one in which the cow had been taken; that after passing on up the hill some fifteen fathoms, he laid down his bundle, and went back to the pit in which the bullock was entrapped, and which lay on the side of the pond opposite to that along which the road runs, and that whilst looking in, by making a misstep, or by some other fatal means, he fell into the power of the infuriated animal, who speedily executed the work of death.

NEW PUBLICATIONS.

The Northern Flora, or a Description of the Wild Plants belonging to the North and East of Scotland; with an account of their places of growth and properties. By Alex. Murray, M.D. Part I. 8vo. Pp. 183. Adam & Charles Black, Edinburgh; Brown & Co. and Clark & Son, Aberdeen; and Smith & Elder, London.

In his preface the author justly remarks, that the Flora Scotica of Lightfoot, and the more recent work of Sir William Jackson Hooker, belong rather to the southern and western parts of the kingdom than to Scotland in general. The late eminent Mr Don of Forfar (father of Professor Don of London) long ago made known the botanical riches of the Clova Mountains; and some partial lists of the plants of the Orkney Islands, and of the county of Sutherland, likewise appeared: but, with these exceptions, the botany of the north of Scotland, properly so called, has in a great measure been unexplored, or at least its treasures have remained unpublished. The present work is intended to supply the deficiency. In his arrangement Dr Murray has not adopted the Natural Orders, but has followed the Linnæan System. This first part proceeds as far as Cuscuta in Pentandria Digynia, (this genus, however, being, by mistake, apparently included in the order Monogynia), but does not embrace any of the Umbelliferæ. In his descriptions of species, Dr Murray has deviated widely from the Linnæan canon. He does not first give the decidedly specific characters in a certain number of words, and then a detailed description in a separate paragraph; but he combines the characters requisite for distinguishing one species from another, with any other points deemed useful or interesting. If this plan detract from the rank of the publication as a strictly botanical work, it will perhaps render it more popular. The number of rare alpine species described by Dr Murray, even in this portion of his labours, will surprise and delight the lowland botanist. He will here learn, that amid the Clova Mountains of Angus, he may, in the course of one summer day, fill his vasculum with such plants as Veronica alpina, Saxifraga rivularis, Gentiana nivalis, Alopecurus alpinus and Phleum alpinum. In the extreme north, again, almost at John O'Groat's, he may pick the Pinguicula alpina and Primula Scotica. Regarding this last, we may remark, that it was first brought to Edinburgh from the Orkney Islands by Dr Neill in 1806, as an insular variety of Primula farinosa; but, on being cultivated by Mr Don, at the Botanic Garden, was pronounced by that acute botanist to be specifically distinct. Dr Murray most justly bears testimony to the "discoveries of Mr Don having been lately confirmed and extended;" and we regret, therefore, that he should have seemed to throw the slightest doubt on the fact of Hierochloe borealis (Holcus odoratus, Lin.) being a native of Forfarshire, by using the expression, "*said* to have been found there by Mr Don." The far more showy and more easily observed plants, Sonchus cœruleus and Lychnis alpina, originally discovered by Mr Don, long baffled the search of more recent herborizers, but were at length found in plenty by Dr Graham and his students. There seems no reason to doubt, therefore, that though the less conspicuous holy-grass has hitherto eluded the keen eyes of the Professor of Botany, and his zealous pupils, it, too, will one day repay their indefatigable toils in some "narrow mountain valley,"—the habitat assigned to it by Mr Don. And which "narrow" as it may sound at the fire-side, will probably be found of very respectable amplitude, the botanist of Forfar being noted for underrating alpine distances.

But we have not room to enlarge. Let Dr Murray go on with equal care and zeal, and we confidently predict, from the specimen now published, that his work will prove one of the most interesting of local Floras.* In an appendix we find "Notes from the Ancients," by Mr Francis Adam, on some of the native plants described in this part of the Flora, curious and amusing enough; and also "Observations on the Agricultural properties of several of our native plants," by the Rev. Mr Farquharson—observations which will be found interesting and important to all enterprising and improving agriculturists and sheep-farmers.

List of Patents granted in Scotland from 7th July to 10th September 1836.

1. To David Fisher of Wolverhampton, in the county of Stafford mechanic, for an invention of "an improvement in steam-engines."—Sealed 7th July 1836.

2. To Hamer Stansfeld of Leeds, in the county of York, merchant, for an invention, communicated by a foreigner residing abroad, of "improvements in machinery for preparing certain threads or yarns, and for weaving certain fabrics."—8th July 1836.

3. To Thomas Rock Shute of Watford, in the county of Hertford, silk-throwster, for an invention of "improvements in spinning and doubling organzine silk."—8th July 1836.

4. To Robert Walter Swinburne of South Shields, in the county of Durham, agent, for an invention of "certain improvements in the manufacture of plate-glass.—12th July 1836.

5. To Edward Jelowicki of No. 8 Seymour Place, Bryanstone Square, in the county of Middlesex, Esq., for an invention, communicated by a foreigner residing abroad, of "certain improvements in steam-engines."—15th July 1836.

6. To Benjamin Simmons of Winchester Street, in the burgh of Southwark and county of Surrey, engineer, for an invention of "certain improvements in chemical retorts, stills, and other apparatus, and in the machinery connected therewith, and by the use or employment whereof various processes can be speedily, conveniently, and economically performed."—18th July 1836.

7. To John Isaac Hawkins of Chase Cottage, Pancras Vale, in the Hampstead Road, in the county of Middlesex, engineer, for an invention, communicated by a foreigner residing abroad, of "an improvement in the art of manufacturing iron and steel."—18th July 1836.

8. To John Archibald of the parish of Alva, in the county of Stirling and kingdom of Scotland, manufacturer, for an invention of "certain improve-

---

* While this sheet is in the press we learn that the "saxifrage more exclusively confined to the mountains, but found on the coast of Banffshire," mentioned at p. xii. of Dr Murray's preface, has recently been ascertained to be one of the varieties of the very variable Saxifraga hypnoides, Linn.—a species which occurs sparingly on Arthur's Seat hills at Edinburgh. We have also heard that, since the publication of the first part of the Northern Flora, Elyna caricina (Schoenus monoicus) has been found in Braemar, by Mr George Dickie, surgeon.

ments in machinery, or apparatus for carding wool, and doffing, straightening, piecing, roving, and drawing rolls or cardings of wool."—21st July 1836.

9. To William Wainwright Potts of Burslem, in the county of Stafford, china and earthern-ware manufacturer, William Machin of Burslem aforesaid, china and earthen-ware manufacturer, and William Bourne of Burslem aforesaid, manager, for an invention of "an improved method or process, whereby impressions or patterns in one or more colours or metallic preparations are produced and transferred to surfaces of metal, wood, cloth, paper, papier machée, bone, slate, marble, and other suitable substances prepared, or otherwise not being used or known, as earthen-ware, porcelain, china, glass, or other similar substances."—29th July 1836.

10. To Walter Hancock of Stratford, in the county of Essex, engineer, for an invention of "an improvement or improvements upon steam-engines." —29th July 1836.

11. To John M'Dowall of Johnstone, in the county of Renfrew, Scotland, engineer, for an invention of "certain improvements in machinery for sawing and cutting, and likewise in the mode of applying motive power thereto."—2d August 1836.

12. To Henry Walker Wood of No. 29 Austin Friars, in the city of London, merchant, for an invention of "certain improvements in certain locomotive apparatus."—4th August 1836.

13. To John Burns Smith, of Salford in the county of Lancaster, spinner, and John Smith of Halifax in the county of York, dyer, for an invention of "a certain method or methods of tentering, stretching, or keeping out cloth to its width, made either of cotton, silk, wool, or any other fibrous substances by machinery."—11th August 1836.

14. To Henry Gore of Manchester, machine-maker, for "an invention of certain improvements in the machinery or apparatus for spinning or twisting cotton and other fibrous substances."—11th August 1836.

15. To Samuel Hall of Basford, in the county of Nottingham, gentleman, for an invention of "improvements in propelling vessels, also improvements in steam-engines, and in the method or methods of working some parts thereof, some of which improvements are applicable to other useful purposes."—15th August 1836.

16. To Thomas Earl of Dundonald of Regent's Park, in the county of Middlesex, for an invention of "improvements in machinery and apparatus applicable to purposes of locomotion."—15th August 1836.

17. To Joshua Bates of Bishopsgate Street in the city of London, merchant, for an invention, communicated by a foreigner residing abroad, of "certain improvements in machinery for cleaning and preparing wool."—19th August 1836.

18. To John Sharp of the burgh of Dundee in the county of Forfar in North Britain, flax-spinner, for an invention of "certain machinery for converting ropes into tow, and certain improvements in preparing hemp or flax for spinning; also certain improvements in certain machinery for the preparation thereof for spinning, part of which improvements are also applicable to the preparing of cotton, wool, and silk, for spinning."—24th August 1836.

19. To James Champion of Manchester, in the county of Lancaster, machine-maker, for an invention of "certain improvements in machinery for spinning, twisting, and doubling cotton and other fibrous substances."—31st August 1836.

20. To John Springall of Oulton in the county of Suffolk, iron-founder, for an invention of "an improved mode of manufacturing certain parts of ploughs." 2d September 1836.

21. To Richard Thomas Beck, of the parish of Little Stonham in the county of Suffolk, gentleman, for an invention, communicated by a foreigner residing abroad, "of new or improved apparatus, or mechanism, for obtaining power and motion to be used as a mechanical agent generally, which he intends to denominate Rotæ Vivæ."—10th September 1836.

22. To Henry Scott Junior, and Robert Stephen Oliver, hatters in the city of Edinburgh, for an invention, communicated by a foreigner residing abroad, "of a certain improvement or improvements in the manufacture of hats, caps, and bonnets."—10th September 1836.

INDEX.