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THE

PHILOSOPHICAL MAGAZINE
AND
ANNALS OF PHILOSOPHY:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE, THE LIBERAL AND FINE ARTS, AGRICULTURE, MANUFACTURES, AND COMMERCE.

NEW SERIES.

No 39.—MARCH 1830.

WITH AN ENGRAVING

Illustrative of Mr. De la Beche's Paper on the Formation of Conglomerate and Gravel Deposits.

BY

RICHARD TAYLOR, F.S.A. L.S. G.S. M. Astr. S. &c.

AND

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

ALERE FLAMMAM

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TO CORRESPONDENTS.

We hope to insert the Communication on the Measurement of the Hills of Swaledale, Yorkshire, in our next.

The "Lines to Caleb Mainspring" have been received. We do not recollect that we have ever before been favoured with a poetical communication; and must therefore obtain the opinion of some who are more familiar with the Muses. As Prize Chronometers and Trial Numbers are, we believe, themes which have not before been *sung*, (though we are aware there are musical clocks,) we give the following sample of our Correspondent's *numbers*.

> " Variety is pleasing—but a pest
> The same dull rate of sameness;—so to find
> Six * eighty right is wrong.—Caleb, you jest.
> I've added up your ratiocination,
> And find the product to be botheration."

* 6″·80, the mean monthly rate of Caleb's Common Watch.

*** *The Editors request that all Communications intended for immediate insertion may be sent to the care of Mr. Richard Taylor, Printing Office, Red Lion Court, Fleet Street, London, at furthest by the* 15*th day of the month, or they will be too late to appear in the ensuing Number.*

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THE VARIOUS BRANCHES OF SCIENCE, THE LIBERAL AND FINE ARTS, AGRICULTURE, MANUFACTURES, AND COMMERCE.

NEW SERIES.

N° 41.—MAY 1830.

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TO CORRESPONDENTS.

In reply to "BARNACLES," we inform him that the Right Ascension and Declination of all the principal stars (nearly 3000 in number) are given in the Catalogue of the Astronomical Society: but we do not know of any work that contains the "Distances" of the stars.

In our next Number we hope to give Dr. SCHMIDT's Paper on the Dimensions of the Earth; the Rev. Mr. VERNON's Examination of Crystallized Oxide of Zinc; Mr. ALISON's Narrative of an Ascent to the Summit of the Peak of Teneriffe; and Mr. W. HERAPATH's Paper.

Mr. J. PRIDEAUX's Paper and Correction have been received, and will meet with early attention.

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THE VARIOUS BRANCHES OF SCIENCE, THE LIBERAL AND FINE ARTS, AGRICULTURE, MANUFACTURES, AND COMMERCE.

NEW SERIES.

No 42.—JUNE 1830.

BY

RICHARD TAYLOR, F.S.A. L.S. G.S. M. Astr. S. &c.

AND

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TO CORRESPONDENTS.

We have been obliged, by great press of matter, again to postpone the insertion of Mr. NIXON's Communication; Mr. ALISON's Ascent to the Peak of Teneriffe; the conclusion of Mr. DE LA BECHE's Paper; and the continuation, which we promised in our last, of the controversy between MM. CUVIER and GEOFFROY-SAINT-HILAIRE, respecting the Unity of Organization in Animals.

The Communications which have been received from Mr. GALBRAITH, Mr. WITHAM, Mr. PRIDEAUX, Mr. MACVICAR, and Mr. S. SHARPE, will receive our early attention.

Our Mathematical readers, we apprehend, would not consider the Paper containing a "Remark on the Diophantine Analysis," to be suited to our pages.

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PHILOSOPHICAL MAGAZINE,
OR
ANNALS
OF
CHEMISTRY, MATHEMATICS, ASTRONOMY, NATURAL HISTORY, AND GENERAL SCIENCE.

BY
RICHARD TAYLOR, F.S.A. L.S. G.S. M. Astr. S. &c.
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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. VII.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE AND ANNALS OF PHILOSOPHY.

JANUARY—JUNE, 1830.

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ALERE FLAMMAM

TABLE OF CONTENTS.

NUMBER XXXVII.—JANUARY.

Dr.

NUMBER XXXVIII.—FEBRUARY.

NUMBER

NUMBER XXXIX.—MARCH.

NUMBER XL.—APRIL.

Mr.

NUMBER XLI.—MAY.

M. Gay-

NUMBER XLII.—JUNE.

Specific

PLATES.

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JANUARY 1830.

I. *Further Examination of the Deposit of Fossil Bones at North Cliff in the County of York. By the Rev.* WILLIAM V. VERNON, *F.R.S. F.G.S. Pres. Y.P.S.**

AT a former meeting of the Society, I gave an account† of the discovery of some remarkable fossil bones in a marl-pit near North Cliff; and at the same time I mentioned the desire of the Council to complete the investigation of so interesting a subject, by undertaking a more accurate examination of the bed in which they were found.

The pit having been partly filled up before it attracted the attention of any scientific observer, and not having been sunk to the bottom of the deposit, I was then only able to furnish such information on some material points as I could collect from the report of the farmer by whom the marl was worked, and from the borings which I had caused to be made through it. But by the wish of the Council, and with the permission of a member of this Society‡, to whom the care of the property belongs, I have since directed the sinking of another pit near the former, and have personally superintended the operation. Six or seven hundred loads of marl were removed; the deposit was penetrated to its lowest bed; the depth at which the several bones were found was measured, and the nature and the location of all the contents of the pit were carefully observed. I have represented in the following table the chief details of

* Read before the Yorkshire Philosophical Society, Oct. and Nov. 1829; and communicated by the Author.

† Published in the Phil. Mag. and Annals, Sept. 1829, vol. vi. p. 225.

‡ William Worsley, Esq.

 this

this examination: details of which the value may be judged from the regret expressed by Cuvier, in his great work, at the deficiency of similar *data*, and his acknowledgement of the uncertainty which it has thrown on some of his conclusions.

Mineral Contents.	Feet.	*Organic Contents.*
Yellow Sand.—In this and the gravel below it a few pebbles of quartz and sandstone	3	*No Bones, Shells, or Vegetable Remains in the Sand or Gravel.*
Gravel—composed of chalk pebbles and sharp flints.	4½	
Gray Marl — indented by the gravel in some places to the depth of three feet, and containing large rolled pebbles of quartz		*No Shells or Vegetable Remains in the Gray Marl.*
	7	Elephant (Prim.) Numerous small fragments of the tusk and tooth.
	8	Do. Calcaneum.
Mountain limestone and carboniferous sandstone with chalk and flint.	9	Do. Three cervical Vertebræ.
	10	Do. Astragalus.
		Deer. Branch of horn.
		Horse. Radius: lower end.
Black Marl— containing minute pebbles of chalk, very few flints; and at the bottom two or three pieces of a fine-grained calcareous sandstone, similar specimens to which may be found in one of the adjacent beds of the red marl, but not including a single fragment of remote rocks. No specimen of these was found below the gray marl.	10	Rhinoceros. Radius: upper end.
	11	Bos. Metatarsal bone.
		Wolf. Radius.
	12½	Elephant. Humerus, and the head of it detached.
	13	Horse. 1st Phalangial bone.
	½	Do. 2nd Do.
	14	Do. 3rd Do. (col^d Helix.)
		Elephant? Four caudal Vertebræ.
	14	Duck. Ulna. Clavicle. Tibiæ: lower end.
	15	Bos. Bison. Occiput, part of the frontal and maxillary bones and the horns.
	15	Wolf. R. lower jaw. Condyle of another, R. Humerus, Radius and Ulna, articulating.
	18	Bos. Two molar teeth: upper jaw.
		The Black Marl abounds in Shells; chiefly Planorbis complan. Lymnæa palus., and in Vegetable Remains including jointed stems.
		Horse. Rib.
	22½	
Strong blue Marl.—Some clay nodules found in this	24	
Flint Gravel in Marl.	25	*No Bones, Shells, or Vegetable Remains in these alternations.*
Strong blue Marl.	½	
Flint Gravel in Marl.	26½	
Red Marl.		

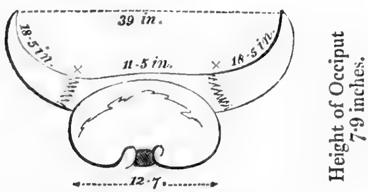
The

The principal objects of research appeared to me to be these: First, to ascertain whether the organic remains were distributed in any order of succession; whether, for instance, the remains of extinct species, such as *Elephas primigenius* is presumed to be, lay below those which correspond with existing types, and below the shells which Mr. Phillips has lately identified with recent native specimens. Secondly, to determine whether the deposit contained any decisive evidence that it had been originally formed by a sediment from tranquil waters, and subsequently penetrated by the irruption of a diluvial torrent.

To the first of these inquiries the answer furnished by the table is beyond expectation distinct. The discovery of the head of an ancient variety of the bison is in this respect a fortunate occurrence. When I compared it with the description given in the "Ossemens Fossiles" of the cranium of the aurochs or bison, and perceived in it the strongly marked characters which distinguish this from every other species of ox*, the obtuse angle which is formed by the frontal and occipital bones at the junction of their planes, the curvature of this crest and the distance at which it is placed behind the horns,—I was much gratified to find that one of the desiderata of geology was here for the first time supplied.

The following quotation from Cuvier will explain the interest which attached to the finding such a specimen in such a situation: he is speaking of the bones of the ox found at Kirkdale, and in other ancient deposits.

"Il seroit de la dernière importance en géologie de savoir à quelles espèces ont appartenu les os de chaque gisement, de determiner, par exemple, si ce sont des os d'aurochs ou



* Circumference of core of horn near the base 15·5.
Distance of its base in advance of the crista occipitalis 4 inches.
Angle included between the frontal and occipital slopes 105°.
Extreme breadth of occipital condyle................ 6·0 inches.
Extreme breadth of occipital foramen .... 2·1
Extreme length of ditto .... 1·6

 des

des os de bœuf ou de buffle, qui ont accompagné les éléphans, les rhinocéros lorsq'ils vivoient dans nos climats et l'on comprend aisément quelles conséquences on déduiroit d'un tel fait aussitôt qu'il seroit bien établi. Malheureusement il reste encore plusieurs sources d'incertitudes; il n'est pas toujours facile de déterminer une espèce d'après les os des extrémités, lorsque l'on n'a pas son crâne." The most skilful of comparative anatomists was not able to distinguish the species of this genus with certainty but by the peculiarities of the cranium, and no cranium had yet been discovered with the bones of the extinct elephant, rhinoceros, and lion.

The consequences alluded to in the foregoing passage follow from the consideration of the different climates which the different species of the ox inhabit in their natural state: the buffalo is naturally an inhabitant only of the tropics; the ox or urus and the aurochs or bison are, and always have been, as far as the history of animals extends, natives of a cold or temperate climate. If therefore it is the bison, the remains of which are found with those of the extinct species of elephant, it is to be presumed that the latter animal also was a native of such a climate, and by consequence that the temperature of these latitudes has undergone little alteration since the remains in question were inhumed.

To this conclusion I think myself intitled to come, from the discovery in the present instance of a bison's head beneath the remains of the elephant and rhinoceros; and I consider such a conclusion confirmed in the highest degree by the situation where it is shown by the table, that those land- and fresh-water shells were found which have been so fully identified with species and varieties now existing in this country. These shells, twelve in number, to which Mr. Phillips has since added another (*P. spirorbis*), proved to be embedded with the bones to the very bottom of the black marl; but in the upper *gray* marl not a trace of them was to be seen: so far are they from being of recent deposition, that they are not even mixed with but distinctly covered by unequivocal relics of diluvian action.

But I am anticipating the second point to which I stated that my inquiries were directed; namely, the question whether any part of this deposit had been formed by a sediment from tranquil waters.

Now as far as this examination went, it proved that for the first seventeen feet from the stratified red marl upwards, not a pebble had been deposited which might not have been brought by the tides of the Humber, or the influx of streams from the near adjoining hills. The black marl exhibited all the

the characters of a tranquil sediment; scarcely a stone was found in it which might not have been carried by the gentlest current; the whole mass was impregnated with minutely divided animal and vegetable matter, and gave out a fetid odour; so loose was its texture, though it might be pressed into a clay tenacious enough for brick-making, that it worked very lightly, and crumbled, as it was thrown out, into mould: the tenderest shells were perfectly preserved; the bones showed no signs of having been rolled; their edges had lost none of their sharpness; their angles were in no degree smoothened. Out of a hundred bones, and fragments of bones, I could find but two which were rubbed, and these were worn and polished at the points of the extremities alone.

The fragments had been brought into the state in which they were found by various causes; some had been apparently fractured before they were floated in, others had been broken in their place by unequal pressure; some had split asunder by a longitudinal cleavage, others had fallen to pieces by chemical decomposition: the latter effect had taken place chiefly in the upper and wetter marl.

Nothing like an entire skeleton was found; but in some instances bones lay together which articulated with one another: thus the humerus, radius and ulna of the wolf were not far separated; and the coronary phalangial bones of the horse were found in regular order; the second a little below the first, and the third a little below the second. The magnitude* of these phalangial bones is remarkable when compared with those of the ancient British horse; I have seen some which were taken out of a *barrow* in the East Riding of Yorkshire, by the Rev. Mr. Stillingfleet, and I think they are not more than half the size.

Upon the whole, all the appearances hitherto described may be very well accounted for by the common operation of a river like the Humber, in forming alluvial tracts. We may conceive the lowest alternations of the marl to have been deposited by successive tides; we may conceive a *mere* to have been by degrees banked up, in which some molluscous kinds lived, and into which others were washed from the surrounding marshes, together with the remains of the animals which frequented them, some of which may have perished by violence and some by natural decay; and lastly, we may conceive the same ground, by some demolition of its embankments, to have been after-

* Length of 1st coronary 3·5; Length of 2nd coronary 2·25; Greatest breadth of head 2·75; Greatest breadth of head 2·60; Breadth across the ends of the last bone .... 3·40; Versed sine of ditto .... 2·75.

wards

wards overflowed by more frequent tides, in consequence of which the former vegetable products have failed, and the deposit of land- and fresh-water shells has ceased.

At all events I think it appears that so far no violent and overwhelming torrent has been concerned in the formation of these beds.

But above the black marl and about the level of high water in the Humber, at this level the first marks begin to be observed of a more powerful action. From this point to the surface large stones are embedded in the deposit transported from distant rocks, from the western hills of Yorkshire and the mountains of Cumberland. These stones correspond so entirely with those of all our great beds of diluvial gravel, that if a heap of the former were laid by a heap of the latter, the two heaps could not be distinguished from each other. They may be supposed perhaps to have been washed out of those beds; but surely they have not been washed out of them by the Humber; for if so, why are they not rather deposited in the lower parts of the marl, than at a point to which none but the finer particles suspended by a flood could be supposed to reach. The Humber does not now deposit such stones.

This last remark is equally applicable to the bed of gravel and sand which lies above the marl. There is no argillaceous matter mixed with it or over it. It is otherwise in the lower part of this deposit: there, as might be expected from the inundations of so muddy a river, the flints are mingled and covered with marl. So that whether we make a comparison with the warp-land deposited by the Humber in the present state of things, or with the deposit of marl formed in the ancient state of things, we find nothing in this sand and gravel analogous either to those alluvial sediments, or to the usual alluvium of similar rivers.

I have been greatly confirmed in the view which I have taken of this subject, by inspecting a recent alluvial deposit near the mouth of the Tees. Here one of those subterranean forests which have been so often observed on our coast near the confluence of the rivers with the sea, has been intersected by a cut which is designed to connect two reaches of the Tees, and to shorten the navigation from Newport to Stockton. The following is a section of this deposit.

Feet.

10. Blue clay, containing no stones, but stained with black spots of animal or vegetable matter, and having the same fetid odour as the black marl at Cliff.

6. Peat containing trees: a horse's head was found in it, some teeth of sheep, and many hazel nuts.

1—6 Com-

Ft. Inch.
1—6 Compressed plants of recent marshes, in high preservation, being generally uncarbonized, and of a greenish straw-colour: among them a jointed plant closely resembling that found at Cliff.
1. Blue clay without spots.
Red rock marl.

The trees are*, oak, birch, and hazel; they have fallen in various directions: the roots, from which the trunks have been broken, in some instances are standing upright, but want, as I was informed, the tap root. No marks of tools have been observed upon any of the trees; the opinion of the superintendant of the work is, that they had been drifted or rather had sunk into their present situation from an adjoining rise, which he thinks was the ancient bank of the Tees. There can be no doubt that the argillaceous bed above them has been formed by the river, which would even now cover the surface at high tides if it were not banked out.

Here I conceive we have a deposit which may be compared in some respects to that of Cliff; and here also I found abundance of diluvian pebbles, distinguished only from those above described by the circumstance that the mountain limestone contains the Madrepores which are found in that rock in the county of Durham. But the material difference between the two cases is this; that the diluvian stones are here embedded in the red rock marl, which is the substratum whereon this alluvium also reposes; and they had penetrated the marl, which is here extremely hard and solid, to the depth of at least four feet, to which depth I saw it worked. So that at Stockport we have the same gravel buried under the alluvium which at Cliff is laid above it, and we have here no diluvian stones in the upper clay.

To what a height this gravel has been carried in the vale of Cleveland, I had an opportunity of observing under the northern escarpment of the hills near Stokesby, where I found a bed of it containing pebbles of mountain limestone, and large boulders of millstone grit mixed with specimens of the older rocks, at an elevation which I calculated to be about eight hundred feet above the level of the sea.

In reviewing the whole of the subject I cannot refrain from remarking, that however visionary it may appear to hope for geological data sufficient to fix the chronology of the Deluge,

* One of the fragments of oak was more than thirty feet long and about three feet in diameter. The wood sunk in water, but was capable of being worked, and was not much discoloured; its tendency to split on exposure was said to be removed by scalding it in boiling water.

should

should accurate and multiplied observations be extensively made upon the depth of sediment which rivers have deposited above the diluvium, and upon the depth of sediment which the same rivers are still accumulating,—some approximation at least may be arrived at towards the solution of this question; and it is even possible that similar observations on antediluvian alluvia may be attended with interesting results in confirming the testimony of history as to the æra of creation itself.

In the mean time it is already become one of the best established inferences in geology, that since the formation of the animals which now subsist upon the earth a general and overwhelming inundation has occurred. In the wreck of that great catastrophe no new *genus* has been discovered. The fossil *species* which it has entombed do not differ more from the existing species than the existing species from one another. The antediluvian *Bison, Reindeer, Glutton, Wolf,* and *Fox* cannot be distinguished by any specific difference from the recent animals, nor, as far as yet appears, can the *Horse* or *Stag;* and the distinction of one species of *Bear* is allowed to be doubtful.

The indigenous European quadrupeds wanting to this list are not very numerous; and of these it is highly probable that more will be found when the repositories of antediluvian bones shall have been fully searched. Fifteen species of molluscous animals have now been compared with their recent analogues, and, as might be expected of more stationary animals, the specific identity has been found to be still more absolute. Against this positive evidence that the whole belong to one epoch, interrupted only by a transient convulsion, there is nothing to set but the disappearance of some of the antediluvian species; and this is surely no unnatural consequence of the catastrophe which is admitted to have occurred.

I am, therefore, very far from agreeing in the opinion that "the body of evidence seems to render a new creation presumable*," subsequent to the diluvian epoch; and I am equally far from thinking that there is any evidence at all against the creation of "Man and the Monkeys" having preceded the *geological* deluge. The only inference which can be drawn from the absence of the bones of monkeys is that which has been long since indicated by Cuvier, that the antediluvian animals of Europe were not the same as the animal population of the torrid zone; and with respect to human bones, it must be remembered that it is only lately, by the industry of living naturalists, that these deposits have been examined with any attention

* Sketch of a Classification of European Rocks.—Phil. Mag. Dec. 1829.

even

even in Europe; but that in Asia, the cradle of the human race, they have never been at all explored. From the absence of human bones in the antediluvian deposits of Europe, the only consequence which can justly be inferred is, that the regions of the earth which we inhabit were not peopled before the Deluge; and this consideration furnishes a strong presumption that no long interval elapsed between that event and the creation.

So that the present state of our knowledge upon this subject presents us with two geological probabilities: first, that there has been since the creation of the present order of animated beings a general deluge, which destroyed a great multitude of those animals and extinguished several species; and secondly, that this deluge followed the creation at no very considerable interval of time, and before mankind had overspread the earth. These are links between natural and civil history which it is of the highest interest to trace; and the confirmation and extension of them is the most important object to which the inquiries of geologists can be directed.

II. *On the Presence of Iodine, Potash, and Magnesia, in the Bath Waters. By Mr.* Charles Cuff.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE annexed detail of experiments upon the Bath waters, I made at the suggestion of Sir G. S. Gibbes, M.D. with a view to ascertain the presence of iodine, bromine, magnesium, or potassium; the two latter bodies having been stated by Mr. Walcker, in his recent analysis, as existing in them*, though unnoticed by other experimentalists except Dr. Bryan Higgins, who assigns twenty-two grains in the gallon of muriate of magnesia. At a more convenient season I purpose renewing this inquiry into the saline contents of these celebrated springs. My present object referring solely to the points stated above, I submit the result of my experiments to the attention of others who may have time or inclination to pursue the investigation. I remain, Gentlemen, yours, &c.

Bath Institution, Nov. 18, 1829. Charles Cuff.

Twenty-four imperial gallons of the water from the King's Bath were evaporated in a *new* tinned iron boiler to about half

* See Phil. Mag. and Annals, N. S. vol. vi. p. 148.

a pint, and the liquid separated from the solid matter which had precipitated during the evaporation.

The solid matter, inferred to be the sulphate of lime (with silex) and iron, known to be the chief proportion of the solid contents of the water, was not examined, as the inquiry related only to the easily soluble salts, which the liquid would necessarily contain. The water on being further reduced in a Wedgewood-ware basin, by cautious evaporation yielded well-formed crystals of chloride of sodium, and a small portion of a finely crystalline substance, which on examination proved to be sulphate of lime.

The taste of the concentrated water had hitherto been purely saline, but when reduced to half an ounce, and separated from the crystals which had formed, it was also bitter.

The mother liquor was tested as follows:

A. Oxalate of ammonia	No change.
B. Muriate of baryta...	Dense precipitate insoluble in nitric acid.
C. Carbonate of ammonia	No change, but yielded a precipitate by boiling, and also by the addition of phosphate of soda.
D. Muriate of platina..	Yellow precipitate.
E. Solution of starch...	No change, but a very decided violet precipitate on adding a few drops of diluted sulphuric acid.
F. Nitrate of silver......	Copious precipitate not *wholly* soluble in pure ammonia.
G. Chlorine gas.........	No change.

Experiment A. shows that all the lime had been removed.
B. that sulphuric acid was present.
C. indicated magnesia,
D. potassa,
E. and F. *Iodine.*
G. showed that there was no bromine.

Comparative experiments with a solution of iodine upon the starch test used, render it probable that the proportion of iodine in the whole quantity of water evaporated was much less than a grain.

III. *On the General Existence of Iodine in Spring Water.* *By* R. Henderson, M.D.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

MY attention has of late been directed to the statements of Prof. Daubeny, respecting the existence of bromine and iodine in many of the mineral waters. I beg leave to observe that

that I have found traces of iodine not only in warm and saline springs, but also in every common spring in which there are traces of the chlorides of calcium and sodium. It appears that Mr. Murray had, previous to Prof. Daubeny's communication, ascertained the existence of iodine in the Gloucester and Cheltenham springs; also in the saline waters at Ingestre and at Bex. Iodine appears to exist in the warm springs of Keddleston, Matlock, Buxton, wells near Bristol and Bath; the quantity is so small as not to be appreciable, and requires the evaporation of many gallons before the slightest change is induced by the amylaceous test. Sir H. Davy attributed the colouring principle in sea-water partly to the presence of iodine; as the chloride of sodium is so generally diffused and iodine appears to be an accompanying principle, and therefore probably may be traced in every spring. The strongly charged saline springs of Cheshire contain it in much larger proportion than the warm springs above mentioned; and in all the well-waters I have examined, by the evaporation of fifty or sixty gallons to a few ounces, a faint peach-blossom colour is produced by the test. I evaporated fifty-four gallons of the condensed water of a steam-engine in the neighbourhood where I reside, and which may be considered as nearly equal to distilled water in purity;—no traces appeared, and therefore, independent of the vessels employed in the evaporation, iodine, as to its state of combination, I have not been enabled to detect. The quantity existing in every mineral water is too minute to produce any effects on the constitution, and therefore, the ascertainment of its existence may be considered more as interesting to the chemical inquirer than of any practical utility *. Thus silex and alumina have been stated as existing in some warm springs. Unless there is an uncombined alkali in the waters, I have never discovered either the one or the other. I believe the error has arisen from very finely divided silex, or alumina, when diffused in water, requiring many days for its deposition; and hence, previous to the analysis of any mineral water, it should be preserved in a quiescent state many days, in order to admit of the deposition of every particle of matter in a state of mechanical suspension. And I perfectly accord with the observations of the late Dr. Murray of Edinburgh, that the salts ought to be considered as existing in the waters in their natural state, in that state of combination in which their

* The continued or frequent exhibition, however, (to use a medical phrase) of waters containing even such minute proportions of iodine, may, we conceive, at length produce appreciable and perhaps important effects upon the system; and in this point of view the subject of Dr. Henderson's paper may become practically useful.—Edit.

 solubility

solubility is the greatest; and that in the process of evaporation, the dried salt is not presented to us in that state of arrangement in which it previously existed in the fluid mass; and thus in many instances what we conclude to be the chloride of sodium is the chloride of calcium.

In the year 1825, Mons. Peschier of Geneva published some interesting researches on titanium: he has discovered it as existing in considerable proportions in mica, talc, chlorite, steatite, asbestus, marl, and eisspath. The results induced me to examine the Mona marble of Anglesea. Although I have detected titanium [?], as soon as my series of experiments are concluded, I shall have great pleasure in transmitting them to the public through the medium of your valuable Journal.

I am, Gentlemen, yours respectfully,

Ambleside, Cumberland, Nov. 8, 1829. R. HENDERSON, M.D.

IV. *Proposal of another Number as a Test of the Chronometers on Trial at Greenwich.* By J. L. T.

THE observations of Caleb Mainspring in the last Number of the Philosophical Magazine, on the trial number hitherto applied for awarding the prizes for the best chronometers, have induced me to propose another number to be substituted for it; the application of which, although certainly more laborious than that of the former one, would be by no means very difficult, especially after a little practice. C. M.'s remarks prove that the present trial number is not an absolute test of the quality of a chronometer; it is, indeed, clear from the derivation of the number, that almost every day's differences might undergo slight alterations, the accumulated sums of which would be considerable, without altering that number. The object which the Admiralty must have in view, is to find out those chronometers which will most exactly retain the rates derived from a certain, longer or shorter, interval, during an uncertain subsequent period. The usual and most important application of chronometers for determining the longitude at sea, by means of a rate, *previously* determined in port or on shore, does not admit of any regard to the alterations, however regular to all appearance, of that rate which in other applications of chronometers might be admitted and taken into account. The Admiralty can, therefore, not look to any other differences but the first, and should award the prizes to those chronometers only, which present the greatest uniformity in the first differences of their daily differences from the mean time

time of the same place. It is submitted whether the following quantity derived for each chronometer from the daily comparison with mean time will be a good criterion of this uniformity. Let the first differences of the daily differences of each chronometer from Greenwich mean time (in fact, the numbers printed in the Greenwich reports for each day) be d', d'', d''', &c., and let the mean of all for the whole period embraced by the trial be d; and then let the sum of the squares of the differences of the number for each day, and the mean of all, be calculated; viz. $(d'-d)^2+(d''-d)^2+(d'''-d)^2+$&c. It appears to me that this number would be fit to judge by of the quality of a chronometer, and every alteration in any one number would produce a change in it. Without contending that the reciprocal of this number would express the exact weight of the different results obtained by these chronometers during a subsequent period, it may be affirmed that the order of the chronometers, with regard to quality, would be the reverse of the order of those quantities as to magnitude. As the object of these remarks is only to submit a proposal to the examination of those who are competent and willing to judge of its propriety, I shall only add an example of the manner in which this trial number of a chronometer is to be calculated. The calculation would in most cases require little accuracy, as the numbers will vary greatly for the different chronometers; and only in cases where they would, for the best ones, be nearly the same, would it be necessary, by a more accurate repetition of the calculation, to decide the uncertainty left in the previous approximate calculations.

Observed daily Rates.	Difference between the mean and each Day's Rate.	Squares of the preceding Differences.
9·5	+·4	·16
8·7	—·4	·16
8·3	—·8	·64
9·2	+·1	·01
9·6	+·5	·25
9·1	+·0	·00
8·4	—·7	·49
9·7	+·6	·36
9·8	+·7	·49
9·7	—·4	·16
9·1 mean.	·0	Sum 2·72 trial number.

J. L. T.

V. *Re-*

V. *Register of the Pluviometer at Bombay, in the Year* 1828. *By Mr.* Benjamin Noton, *Assay-Master of the East India Company's Mint, at Bombay**.

(The Register was kept by Howard's Rain-Gauge.)

1828.	June.		July.		August.		Septemb.		October.			
Date.	Rain.		Rain.		Rain.		Rain.		Rain.			
	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]		
1	0	0	0	16	0	40	0	74	0	0		
2	0	0	0	42	0	26	0	15	0	0		
3	0	50	0	58	0	37	1	87	0	11		
4	0	0	2	16	0	6	0	15	0	1		
5	0	0	0	0	0	13	0	30	0	0		
6	0	0	0	78	0	20	1	14	0	0		
7	0	0	3	14	0	20	0	56	0	0		
8	0	0	1	25	0	23	0	7	0	0		
9	0	0	0	76	1	57	1	84	0	19		
10	0	0	0	80	2	80	4	22	0	0		
11	0	0	0	12	2	19	1	0	0	0		
12	0	62	7	26	0	48	0	8	0	0		
13	0	0	3	66	0	18	0	0	0	69		
14	0	2	0	12	0	73	0	2	0	48		
15	1	15	0	17	0	66	1	25	0	0		
16	3	47	1	80	0	0	1	0	0	65		
17	0	4	5	50	0	0	0	30	0	42		
18	0	0	7	92	0	6	3	64	3	76		
19	0	86	2	93	0	2	0	20	0	9		
20	1	35	3	80	0	0	0	63	0	0		
21	0	2	0	38	0	0	0	60	0	0		
22	0	0	0	81	0	0	0	0	0	0		
23	0	0	0	24	0	0	0	0	0	0		
24	8	67	0	36	1	70	0	63	0	0		
25	4	58	0	5	0	7	0	40	0	0		
26	0	23	0	18	0	0	0	29	0	0		
27	1	40	0	11	0	8	0	0	0	0		
28	0	27	0	10	0	6	0	0	0	0		
29	0	10	1	60	0	52	1	0	0	0		
30	0	25	5	50	1	90	0	0	0	0	Total	
31	0	0	0	19	2	35	0	0	0	0	inch.	dec[ls]
Total	23	53	52	75	17	22	22	8	6	40	121	98
Years	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]	inch.	dec[ls]
1817	45	72	23	67	9	34	24	87	0	19	103	79
1818	22	54	17	69	28	45	10	39	2	7	81	14
1819	15	95	30	66	20	24	10	11	0	14	77	10
1820	18	82	28	37	19	49	10	66	0	0	77	34
1821	15	18	20	60	28	52	18	29	0	40	82	99
1822	29	21	26	59	33	83	22	16	0	82	112	61
1823	21	76	15	96	19	70	4	28	0	0	61	70
1824	3	89	8	7	17	86	1	78	2	37	34	33
1825	24	45	25	17	12	94	9	68	0	0	72	24
1826	17	75	26	97	8	40	23	50	1	23	77	85
1827	49	15	10	29	10	51	10	16	0	92	81	3
1828	23	53	52	75	17	22	22	8	6	40	121	98

* Communicated by the Author.

VI. On

VI. *On Artificial and Natural Arrangements of Plants: and particularly on the Systems of Linnæus and Jussieu.* By William Roscoe, *Esq. F.L.S.**

Ordines naturales valent de *Natura* Plantarum;
Artificiales in *Diagnosi* Plantarum. Linn.

That nature has impressed upon the individuals of her vegetable kingdom characters sufficient to enable us, not only to distinguish them from each other, but to form them into their proper families and combinations, cannot be doubted. Nor will it be denied that the arrangement of a system of vegetables, founded upon true natural distinctions, would be in the highest degree gratifying. It is not therefore surprising that so many attempts have been made to accomplish this most desirable object; but attractive and splendid as it may be, and certainly as it is known to exist, it is not likely to be ever fully disclosed to our view.—"The majesty of nature" glances before our sight, but as often as we attempt to retain her, she eludes our efforts.—Her vegetable productions are so numerous, their characteristics often so difficult to ascertain, they are related to each other by so many ties, that it is in vain to expect that we shall ever be able clearly to define them, and accurately to seize upon the true distinctions; so as to combine the whole in the precise order in which they were primarily disposed by her hand. In the mean time, the necessities of human life, no less than the objects of science, require that some mode should be adopted which should enable us to distinguish plants from each other, and to designate them by their appropriate names, although we may not be able precisely to ascertain their natural connections and relative situations: and for this purpose it became indispensably necessary to have recourse to art; not to overthrow or oppose nature, but to assist us where she deserted us, to guide our steps till we could again recover her track, and to furnish us with a lamp till we were again illuminated by the beams of day.

Happily for the world, the formation of such a system was undertaken by the illustrious Swede whose name it bears; and certain it is, that it could not have fallen into abler hands.—With the conviction of the real existence of natural genera and orders, no one was more deeply penetrated; and to interfere with these relations as little as might be consistent with his primary object of a complete arrangement of the vegetable world, was his constant solicitude. For the creation of this system he

* From the Transactions of the Linnæan Society, for 1810. The renewed interest which the subject has excited, and the estimation in which the venerable author is justly held, have induced us, though late, to transfer this Paper to the pages of our Journal.—Edit.

he did not, however, wholly depend upon the materials supplied by his predecessors. The systems of all of them were discarded, or only so much of each of them retained as appeared to suit his purpose; but the most valuable part was supplied from his own resources. To whatever period we may assign the discovery of the sexual system, it was he who first demonstrated it in unambiguous and decisive terms, and who applied this great discovery to the formation of an arrangement of plants, which comprehends and defines every individual of the vegetable world. In executing this great task, he has placed the science of botany upon a firm and immoveable foundation; and if he has at any time erred in the application of his own principles, it has been rather from an unconquerable reluctance to interfere, more than was necessary, with the dispositions of nature, than from the pride of erecting a system which should contravene her works.

That the system thus formed is an artificial, and not a natural one, must be admitted; and that it was always so considered by Linnæus, is evident from all his works. Yet this characteristic is not to be taken without some limitations. And in the first place it may be observed, that by the mode of arrangement which he has adopted, the major part of all known vegetables are formed into their great natural combinations in such a manner as scarcely to be susceptible of further elucidation.—Again, the *genera* of Linnæus are uniformly natural; or at least display such trivial exceptions as to oppose no objection of any moment; and this purity in his genera may be considered as of the utmost importance to the character, not only of his own, but of any system. It is therefore only with respect to the place which each genus occupies in his system, that any solid objection can be made; and if this be so situated as to be readily discovered, even although it may not in every instance be found amongst its nearest congeners, it is a defect which may be remedied by an accurate reference, and which as it is occasioned, so it must be excused, by the universality and facility of the system. It would perhaps be too much to say that such an arrangement could not have been effected with less violation of natural affinities; but certain it is that with these affinities he was well acquainted, and the preservation of them was constantly in his view; insomuch that, notwithstanding its acknowledged defects, it may, by a due attention to its exceptions, be studied as a natural system with considerable advantage; whilst, at the same time, it affords an universal key through every department of the vegetable world.

The approbation with which the arrangement of Linnæus was received on its promulgation, and the subsequent adoption of it into general use, may be considered as the most unequivocal

equivocal testimonies of its excellence. It is true, exceptions have been taken against particular parts, and alterations suggested in departments of minor importance, even by the very editors of his works. To have expected perfection in the first outline of a science, the materials of which are continually increasing, would be unreasonable; and these alterations, instead of derogating from, do homage to the system which they correct. The period however is now arrived which is to try its stability.—A rival has of late risen up, and has already become truly formidable.—Under the patronage and by the influence of a neighbouring nation, this rival now comes forward, and demands universal homage. Its advocates are not only numerous, but learned; not only acute, but earnest.—That their influence is daily increasing cannot be doubted; and the crisis is now arrived when their opinions must be either submitted to, or resisted.

Notwithstanding the favourable reception given to the sexual arrangement of plants, it is well known to have made but little progress through the southern nations of Europe; and the French in particular refused implicitly to admit the novel doctrines of the Swede. In Botany, Tournefort continued to be their guide. In Zoology, Buffon directed their steps; and their example induced the Italians, and in some degree the Germans, to follow the same track. From various circumstances, and particularly from the great accession of individuals of the vegetable kingdom to which the arrangement of Tournefort is wholly incompetent, his authority has declined; but Linnæus has not always gained the followers that Tournefort has lost. Other leaders have risen up, and proposed arrangements and nomenclatures of plants wholly different from those of Linnæus; and in particular, the successive efforts of the distinguished family of Jussieu have raised a standard to which many of the most eminent botanists of the present day think it an honour to resort.

The system of the Jussieus, as originally proposed by Bernard, and afterwards illustrated and amplified by Antoine Laurent de Jussieu, has higher pretensions than that of Linnæus, and professes not only to unite together in their natural orders such plants as are related to each other, but to form a complete arrangement, in which every known plant may be found in its proper situation, and every unknown plant may when discovered take its place among its congeners. A system, in short, which unites all the advantages of a natural arrangement with the elucidation of a technical one; and comprises within itself all that is requisite to botanical science.

ence*. If such a system could be established, it is evident that it must render that of Linnæus of no value; or, rather, must exhibit it as calculated only to mislead the student, and amuse him with words, instead of communicating to him substantial knowledge.

In the execution of his task the younger Jussieu had peculiar advantages. Since the time of Linnæus the accessions to the science have been immense; not only from the introduction of new genera and species, which to him were wholly unknown, but from the greater attention which has been paid to the examination of the individuals of the vegetable kingdom; the modes of their existence, œconomy, and reproduction, and various other particulars connected with botanical studies. To enumerate merely the writers on these subjects whose works are entitled to approbation, would be to form a considerable catalogue. That the mass of information thus obtained has thrown great light on the physiology of plants, cannot be doubted; and no undertaking could be more commendable, or more worthy of the talents of the illustrious scholar who engaged in it, than that of endeavouring to apply such knowledge to general use, and showing the affinities and connections which nature has established between the individuals of her vegetable kingdom. The great utility of such a work is obvious; its foundations are deeply laid in the principles of nature; and in order to make a proficiency in such study, it is necessary to examine far beyond the exterior phænomena which are requisite for an artificial arrangement. Hence the science acquires new dignity; and, instead of being conversant merely with exterior forms and nominal distinctions, becomes acquainted with the laws and operations of nature in one of the most important of her functions; that by which she elicits from unorganized matter the means of support for animal life.

Of the ability with which Jussieu has executed his task, and the impulse which he has given to these pursuits, every botanical student is well informed; nor is it possible to recommend his writings, and those of several of his countrymen who have adopted, and perhaps improved upon his system, too earnestly to their attention, as elucidating the natural characters and relative connections of a considerable portion of the vegetable kingdom. This, however, is not the whole to which these authors lay claim. It is not sufficient that we admit, in its

* "His genuina mox substituitur scientia, quæ vegetantium non modo *nomina*, sed et *naturam* inquirens integram eorum organisationem cunctos caracteres prospiciat, &c."—*Jussieu, Introduc.* p. 67.

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fullest extent, the expediency and utility of studying the natural arrangements of plants, but we are now required to adopt this new system as a general arrangement and nomenclature, in the stead of that of Linnæus; to discard his labours, as of an inferior and a succedaneous kind; and to hail the moment when the great event, which he is said to have himself considered as the destruction of his own system, has actually taken place.

It is true the triumph of the new system has not yet been announced, even by its warmest promoters, in distinct and unambiguous terms; but the very arrangement of a *Genera Plantarum*, like that of Jussieu, offers it to universal use; and the manner in which it is spoken of, both by him and his followers, sufficiently demonstrates that this is its ultimate object, to the total exclusion of that of Linnæus. In the very introduction to his work, Jussieu has himself sufficiently disclosed his views, by the objections which he has brought against the system of his illustrious predecessor; the tendency of which is not merely to show that it is imperfect when considered as a natural arrangement, but that even as an artificial one it is not entitled to a preference. In arranging these objections Jussieu has observed, "1. That the distinctions of the Linnæan system are sometimes founded on the minuter organs of vegetables, requiring the use of glasses and instruments. 2. That the method is arbitrary; the distinctions of his classes being derived from some one part only; and that from a deficiency of real characters he is compelled to adopt such as are inconstant, which he uses frequently and promiscuously, to the exclusion of those which are substantial. 3. That in determining by the number of stamina, not only genera nearly related to each other are frequently divided, but that even species are separated*." To these he adds many other objections of minor importance, and afterwards asserts, that "if a preference is to be given to that method which is the most easy, and the most agreeable to the order of nature, that of Tournefort is the most perfect; that the arrangement of the Linnæan system is sometimes perplexed, its designations difficult, and its connections of plants not related still more frequent; that it is indebted

* "Systema tenuissimis interdum innititur organis, oculo armato et acu divellente tunc difficilius observandis. 2. Præterea arbitrarium, systematico errore, dum multiplicatis classibus omnes earum designationes ex unicâ parte molitur depromere; tunc solidorum caracterum penuriâ essentialibus promiscuè addit inconstantes, quos etiam, utpote numerosiores frequentius usurpat, prioribus plerumque neglectis. 3. Staminum numero sic discrepant non tantum genera cognatissima, sed et species congeneres ab invicem demovere nesciæ, &c."—*Jussieu, Introd.* p. 40.

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for its general reception among botanists to the conciseness and certainty of its characters, the number of individuals arranged under each order, and the improved nomenclature by generic and specific names*." To this, however, he adds, "that all such systems are arbitrarily constructed, that they exhibit a factitious science, terminating not in the knowledge, but merely in the defining and naming of plants; and that, in short, they can only be considered as a prelude to the science of botany, affording a succedaneous arrangement of plants, until, by repeated labours, they can be reduced into a proper and natural series†."

From these and other observations to be found in the writings of Jussieu, it is not difficult to perceive that the system there proposed was intended to replace that of Linnæus; which from that time was presumed to be no longer necessary to the student; and these pretensions have been enforced by subsequent writers, who have adopted the arrangements of Jussieu. In his Discourse on the Study of Botany, prefixed to his "*Tableau du Règne Végétal*," M. Ventenat has not only collected the authorities of several preceding botanists in derogation of the system of Linnæus, but has even made use of the authority of Linnæus against himself. In this, indeed, he has in some degree followed the example of Jussieu, who has availed himself of several passages from the writings of Linnæus to prove his acknowledgement of the superiority of a natural method‡; but this concession has been carried by both these writers to an extent which Linnæus certainly never intended, and which it will not in any candid construction bear. If we admit the interpretation put upon the writings of Linnæus, he has himself acknowledged the futility and proclaimed the downfall of his own system, and has consequently released his followers from engaging in its defence.

* *Jussieu, Introd.* p. 41.

† "Hæc autem systemata arbitrariò constructa, scientiam exhibent factitiam, non naturalem, et plantis non penitus cognoscendis, sed tantùm compendiosè definiendis ac certò nominandis addictam. Habenda sunt igitur quasi præludia botanica, aut repertoria aptè digesta, indicisque non alphabetici, alii aliis commodiores, in quibus, secundum signa in faciliorem propriæ investigationis laborem mutuique Botanicorum commercii nexum admissa pacto ordine disponuntur plantæ, donec feliciùs iterata meditatione in seriem verè naturalem distribuantur."—*Jussieu, Ibid.*

‡ "Classes quo magis naturales, eo *ceteris paribus* præstantiores sunt. Summorum Botanicorum hodiernus labor in his sudat, et desudare decet. —Methodus naturalis hinc ultimus finis Botanices est et erit."—*Linn. Phil. Bot. n.* 206.—"Primum et ultimum in Botanice quæsitus est methodus naturalis.—Hæc adeò a Botanicis minùs doctis vili habita, a sapientioribus verò tanti semper æstimata, licet detecta nondum &c."—*Linn. Class.* p. 485. *ap. Jussiæi Introd.* p. 43.

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"This system," says Ventenat, "has had its partisans and its critics. Some have said with Royenus,

> 'Si quid habent veri vatis præsagia, Floræ
> Structa super lapidem non ruet hæcce domus;'

whilst others have not hesitated to assert with Alston, that the sexual system is full of difficulties, and that it is the least natural of all those that have been invented for the classification of plants.

"At this period," continues M. Ventenat, "when experience has enabled us to appreciate the value of the sexual system, and envy and adulation are alike removed, we may assert, without fear of being suspected of partiality, that Linnæus has himself acknowledged the inconveniencies attending the sexual system. This man of genius did not suffer himself to be seduced by the delusions of self-love; and he has frankly acknowledged that his principles had sometimes compelled him to deviate from the track of nature.—Let us not however attach to the sexual method greater importance than was given to it by its author. Those who have read his works ought to know that artificial methods were only considered by him as introductory to the natural method.—In fact, the celebrated naturalist of Upsal was all his life a zealous defender of natural combinations, as may be proved, in the first place, by different axioms interspersed in his works. 2. In the *Eulogia* which he has conferred on those botanists who have endeavoured to follow the traces of nature. 3. In the fragments which he has left us of natural orders, and at which he never ceased to labour*." After quoting a passage from Linnæus in justification of these sentiments†, he adds, "It is remarkable that this great man, after having in his public lectures demonstrated plants according to the sexual system, in his private conferences with his most distinguished pupils developed the principles by which he had been guided in the establishment of his natural orders, and by his learned dissertations prepared the way which led to the perfect knowledge of vegetable productions‡."

Now if, by these and similar observations, it be meant merely to prove that Linnæus was fully convinced of the importance of studying the natural affinities of plants, and that he consi-

* *Ventenat, Discours sur la Botanique.* V. *Tableau du Règne Végétal,* t. i. pp. 17, 18.

† "Dici et ego circa methodum naturalem inveniendam elaboravi; bene multa quæ adderem obtinui; perficere non potui, continuaturus dum vixero. Interim quæ novi proponam. Qui paucas quæ restant benè absolvit plantas, omnibus MAGNUS ERIT APOLLO." *Class. Pl.* p. 485.

‡ *Ventenat, Discours,* p. 19.

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dered it as the highest department of the science, there can be no difficulty in acceding to them; but if they be intended to show that he was of opinion that any arrangement of plants on a natural system was to be preferred to, and might supersede the use of, his own artificial arrangement, (and if this was not the object in view, the introduction of the concessions of Linnæus is of no avail,) it may justly be observed that these authors have either mistaken or not fairly represented the meaning of Linnæus.—That natural affinities are to be studied, and that this department of the science cannot be too diligently cultivated, was his decided conviction. He has even frequently contemplated the possibility of an arrangement which should include in their natural orders the whole vegetable kingdom; but in alluding to such an event, it was always as a mere possibility, of the completion of which he had scarcely a distant hope: still less would he have been inclined to admit that any such arrangement, even if it could be formed, could supersede that which he had with so much assiduity demonstrated, and to which he invariably adhered to the close of his life. To collect together detached sentiments from his writings for the purpose of proving that he preferred a natural method to his own, as a general arrangement, is to pervert his opinions, to render him the adversary of his own labours, and the suicide of his own fame. To the firm and inflexible conviction of the practical superiority of his own method, all the passages cited by these writers are strictly reconcileable; but if any doubt remained on this subject, it would readily be dissipated by a reference to his works. Even in the brief introduction to his own fragments of natural orders, he has placed it in so clear and perspicuous a light, that it is impossible to mistake it. "Natural orders," says he, "cannot constitute a method without a key. In distinguishing plants, the artificial method is alone of any avail; a natural method being scarcely, or rather not at all, possible. Natural orders are useful in acquainting us with the nature of plants, but an artificial method is requisite to their discrimination*." And to this he has added, in language that must for ever remove all ambiguity on this head, "Those persons who, instead of a natural method, have arranged plants in fragments of such a method, and reject an artificial one, seem to me to resemble those who, having a convenient and well roofed house, overturn it, in

* "Ordines naturales non constituunt methodum absque clave.
"Methodus artificialis itaque sola valet in diagnosi, cum clavis M. naturalis vix ac ne vix possibilis sit.
"Ordines naturales valent de natura plantarum—Artificiales in diagnosi plantarum."

order

order to build one in the place of it of which they are unable to finish the roof*."

That Linnæus has in many parts of his works highly commended those who have distinguished themselves in investigating the natural relations of plants, is certain; but to suppose that by this he meant to approve of those who pretended to have formed a natural arrangement, is to attribute to him an opinion which he has disavowed in the most pointed terms. "A real botanist," says he, "will investigate the natural order of plants when it can be discovered;" but, "he will not boast of having discovered a system perfectly conformable to the laws of nature†." And among his diagnostics of pretended botanists he particularly includes that of "presuming that they are acquainted with a natural method‡."

[To be continued.]

VII. *On the Vegetation of the First Period of an Ancient World, that is, from the First Deposit of the Transition Series to the Top of the Coal-field; the Magnesian Limestone forming its upper Limits: with Remarks on the Probability of its Vegetable Origin, &c. By* HENRY WITHAM, *of Lartington, F.G.S. &c.* §

BEING firmly persuaded that the great objects of geology will be much advanced by a serious attention to the history of the vegetation of the different epochs, from the most remote period of organic creation down to the present day; being most anxious to promote a spirit of inquiry in this country, as ardent as that of our continental neighbours,—I have devoted a certain portion of my time to the examination of different coal-fields, to endeavour to corroborate by proof the assertions of that ingenious French naturalist who has lately favoured the world with many judicious remarks upon this dark and difficult, but interesting branch of science.

* "Qui loco methodi naturalis disponunt plantas secundum ejus fragmenta, *respuuntque artificialem,* videntur mihi iis similes, qui commodam et fornicatam domum evertunt, inque ejus locum reædificant aliam, sed tectum fornicis conficere non valent."

† "*Botanicus verus,* ordinem naturalem, ubi patet indigitet."—*Règn. Végét.* 27.

"Nec naturalissimam structuram oratorio sermone ebuccinat."—*Phil. Bot.* p. 294.

‡ "Botanophili Fallaces—Methodum naturalem sibi notam crepant."—*Règn. Végét.* 27.

§ Read before the Wernerian Society, Dec. 5th, 1829; and communicated by the Author.

shall

I shall therefore now state to you the result of my limited investigation.

To the ardour of M. Adolphe Brongniart, in the researches he has so successfully made in collecting materials for the physical history of the formations which compose the crust of our planet, every geologist must feel interested; as well as to Cuvier, Sternberg, Boué, Brown, D'Urville, and others, for their able and unremitting exertions in this dark field of early existence. It has been reserved for this eminent young naturalist to present to the public a classification so natural, and generally so clear, as greatly to facilitate the labours of those who interest themselves in such pursuits, and greatly to aid them in recording such particulars as may accidentally come under their immediate notice*.

Impressed with the importance of this subject, I first of all availed myself of the kindness of Mr. Dolphin, head-agent to Messrs. Hall and Co., who solicited me to explore a vein called Jefferies Rake, in the Derwent mines, near Blanchland, in the county of Durham. Having travelled up the adit about three quarters of a mile, we began to descend by the assistance of ladders. At the depth of about fifty-five fathoms below the surface, in a bed of sandstone nearly forty fathoms thick, we were gratified by a sight of some magnificent specimens of an ancient Flora, belonging to M. A. Brongniart's first period of vegetable creation. The two varieties appear to belong to his third class, "the Vascular Cryptogamia." The first were *Stigmaria* (Lycopodiaceæ); the second were fine specimens, of great circumference, of *Sigillaria* (Filices). Two of these last-named specimens, which were situated in the space cleared out to get at the lead ore, stand erect, and their roots are firmly embedded in a thin stratum of bituminous shale, much carbonized. I should think the height of one of these prodigious fern stumps may be about five feet, and its diameter probably exceeds two. The other, which has been kindly presented to me, may be seen in my museum, No. 14, Great King-street. It has, I understand, been the opinion of some gentlemen who have visited these ancient relics, that they have been washed into, and deposited in their present situation by some aqueous revolution.

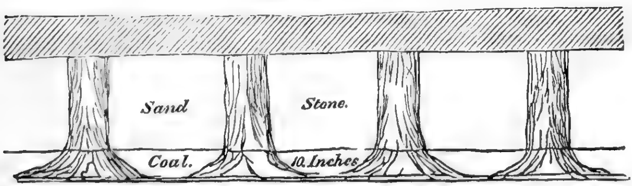
To this conclusion I must object, for two reasons. First, because the roots are firmly embedded in the shale, as if they had remained undisturbed in their original earthy envelope; and Secondly, because you may discover in each cheek of the vein, other trunks of these members of this ancient Flora, in the solid rock, the position and the appearance of which are more

* See Phil. Mag. and Annals, vol. vi. p. 133.

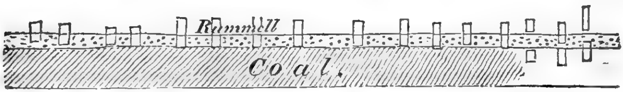
more consistent with the supposition that they grew on the spot where they were found. The confused heaping, fracturing and violence, which characterize diluvial action, are not seen here.

In proceeding towards the east, I received much valuable information from my intelligent friends Mr. Buddle, an eminent coal viewer upon the rivers Tyne and Wear, and Mr. Hutton of Newcastle, whose anxiety in pursuit of this branch of the science is so well known. In the great Newcastle coal-field, the fossil plants are generally in horizontal position, or parallel with the strata, in the greatest possible confusion; much broken, and the parts far separated. Indeed, the confusion is the most serious difficulty the observer has to contend with. It is difficult, however, to trace the operation of a current of water sweeping off the weaker vegetables, and depositing them where we now find them so beautifully preserved. Notwithstanding this, there are to be found in considerable abundance, in various positions, large and strong trunks of plants which appear to remain in their natural position, and which have been able to withstand the force of such torrent, if it can be proved that any such did exist. These vertical plants I have generally found to be the *Sigillariæ*. The *Saginariæ*, the *Stigmariæ*, and *Calamites* (speaking generally), on the contrary, do not appear to have been sufficiently strong to have resisted any revolutionary influence. Below the main seam (which according to Mr. Forster's section of the strata is 150 yards below the surface), in a sandstone there are numbers of fossil plants standing erect, with their roots in a small seam of coal lying below. These stems, as you will perceive by the following diagram, are truncated, and lost in this seam, leaving room to believe they may have formed part of this combustible mass or bed.

High Main Seam.

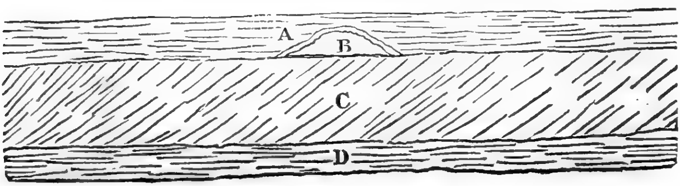


Again, in some of the seams, when the coal is worked away by the miners, the roof often falls. This is to a considerable degree owing to the number of vegetable impressions breaking the coherence of the stratum and bringing these fossils along with it. It must be observed that in almost every instance they are surrounded by a coating of very fine coal, of about half or three-quarters of an inch thick, having a polished surface, with very little attachment to the surrounding matter. This, I doubt not, is the cause of the fall; the fossil dropping out sometimes as much as three feet in length, leaving a hole in the roof almost perfectly circular. Often it falls in these large pieces; but sometimes the nature of the shale of which its substance is composed, causes it to fall in portions of different thickness. It is to these falling pieces that the miner's expressive term *Kettle Bottoms* applies.



These fossil plants run from two to eight feet in circumference. The occurrence of numerous impressions which you may observe in the specimens of parts of different plants in the shale, forming the substance of these fossils, is to me, I must confess, very difficult of explanation. Some years ago a friend of mine found a kettle bottom at old Kenton colliery, eighteen inches in diameter, coated with fine coal, the substance of which was entirely mineral carbon or charcoal, with a mixture of earthy matter and pyrites. A portion of this specimen is in the collection of the Geological Society. It is much to be regretted that hitherto none of these interesting fossils have been followed into the strata. We do not know how far they extend, or to what height they are standing.

Again, in the coal districts of Scotland, amongst the troubles which affect the roofs of coal, there is one, of a very singular form, known by the name of *Pot Bottom* or *Cauldron Bottom*, and are from the size of one foot to five feet in diameter. One of these is represented in the annexed diagram.



A. Roof of coal, argil with sand.
B. Pot or cauldron bottom.
C. Bituminous coal.
D. Pavement of coal. Fire clay.

In

In working the bed of coal, the miner generally knows that he is approaching one of these by the coal becoming twisted and more difficult to work, and this continues till this trouble in the roof is passed. The general form is similar to that represented in the figure, when of course the mouth of the pot is always inverted, the sides of it are generally lined with coal from one-eighth of an inch to an inch in thickness, and the pot or cavity is filled up with stone of the argillaceous kind, or fire-clay, having generally less mixture of sand in it than is in the roof-stone around. The under surface of the stone which fills the pot is irregular and waving, not smooth like the roof adjoining.

Although the coal which lines the pot is connected with the main bed of coal, it is of a texture altogether different, having a bright appearance like jet, and it breaks into very minute cubical pieces; sometimes it has no bitumen in it, and is of the nature of glance coal. The sides of the pot are generally as smooth as glass, with small furrows or grooves in a vertical direction, so that there is very little tenacity between the sides of the pot and the stone which fills it up: this circumstance renders these troubles very dangerous, particularly when they are of a large size, as they fall without giving any warning. The peculiar singularity attending this trouble, is the twisted texture and alteration which are found in the bed of coal immediately under and adjoining it, without any mixture of the stone in it which fills up the pot.

There is sometimes no lining of coal, and it generally happens that a piece of the stone, which fills up the pot, adheres to the upper part of the cavity, so that the trouble may go further up into the strata than is imagined. This trouble requires to be minutely investigated, and the pavement upon which the coal rests should be examined under the trouble, to ascertain if it is in any way altered in its structure, as is the case with the coal.

I am indebted to my much respected friend Mr. Bald, civil-engineer, for this latter information; and I am happy to say that it is his intention, at an early period, to devote his attention to these singularly curious objects.

Were further proof of the vegetable origin of coal wanting, the fact of finding impressions of the *Sagittariæ* in the solid coal, the thin layers of incoherent carbonaceous matter, having much of the silky aspect of charcoal, alternating with layers of good bituminous coal, and bearing the form of the *Calamites* most perfect, should go far to establish the vegetable origin of these combustible beds.

Having now troubled you with the few facts I have been able to collect in the coal districts further south, to which I have added some remarks on the troubles of the Scotch basins,—I shall add some short observations on the neighbourhood of this city. Here again, I have been fortunate in obtaining many specimens of vascular cryptogamic plants, whose natural substances have been transubstantiated into the sedimentary deposits in which they were entombed, with the exception of their bark or outer coating, which is always much carbonized. The prevailing plants of this district, like those of the Newcastle field, appear to be the *Sigillariæ*, the *Saginariæ*, with a number of *Calamites*.

I beg leave here to mention, that in the neighbourhood of Burnt Island in Fifeshire, one of these vegetable fossils, the *Stigmaria* of Brongniart, the *Lepidodendron* of Sternberg, with strong impressions of its leaves, occurs in a limestone. This is a circumstance by no means of common occurrence. This limestone is devoid of any testaceous or coralline remains, and in appearance and composition, by analysis, varies little from the limestone of the Portland oolite. A deposit of limestone also occurs at Hatton, near East Calder, containing terrestrial vegetable impressions.

I now take the opportunity of introducing an account of that fossil member of early vegetation, discovered in the year 1826, in the quarry of Craigleith.—The length of time which has been allowed to elapse without attempting to obtain the necessary information respecting this singular plant,—add to that, the peculiarity of its structure and composition,—has induced me to take much pains upon this point. I therefore laid a well cut transverse and also longitudinal section of this fossil tree before Mr. Hincks, botanical curator to the Philosophical Society of York. His opinion is, that it is a monocotyledonous plant; as a pithy substance fills up the interstices between the vessels, and that there has been no bark or concentric arrangement of layers. He also observes a striking resemblance to certainly monocotyledonous stems, which he has before examined. "On the whole," Mr. Hincks says, "having made the examination of this curious specimen submitted to me, with the greatest care, I can scarcely admit of a doubt upon the subject*."

* Since writing this paper I have received a kind communication from Mons. A. Brongniart, through Mr. Philips of York, to the following effect. "Please to inform Mr. Witham that I have received his specimen of the Craigleith fossil plant. It has much surprised and interested me. Having

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The internal structure, its singular colour when contrasted with the block of sandstone in which it was found, induced me to request my friend Mr. Nicol to analyse it. The following was the result:

60 per cent of carbonate of lime.
18 per cent of oxide of iron.
10 per cent of alumine.
9 per cent of carbonaceous matter.

The height of this gigantic plant was thirty-six feet; three feet diameter at its base, and lying in nearly horizontal position corresponding with the dip. No branches were found. This therefore, with a few others I could here mention, and which I trust will ere long be submitted to your consideration, form but trifling exceptions to the general distribution of early vegetation.

Thus in these great coal-fields (exclusive of the many varieties of plants found in the bituminous shale, which I am happy to say will shortly be submitted to the public, in a work intitled "The Fossil Flora of Great Britain," by Mr. Lindley, Professor of Botany in the London University, and my friend Mr. Hutton of Newcastle) we find the opinion of M. A. Brongniart most completely verified; namely, that the vascular cryptogamic plants had a vast numerical proportion; and in fact, of 260 species discovered in this terrain or period, 220 belong to this class. "Should however," adds M. A. Brongniart, "more precise observations or new discoveries make known in the old formation, some plants of more than one of the classes which we have admitted, or even some species of one of the classes which have appeared to us to be wanting at this epoch, still the essential relation of these classes to each other would be but slightly modified. Thus it might be proved, that certain, yet little known genera of the coal formation, are true dicotyledonous plants. Yet it would not be the less certain, that the vascular cryptogamic plants were by much the most numerous vegetables, during the first period of vegetation." The same remarks he makes respecting the lias, and other formations. Thus whatever new discoveries may be made in the vegetables of this period, from the first deposit of the transition rocks to the top of the coal-field, yet the essential characters can be but slightly modified, and this period will always remain perfectly distinct.

The more gentlemen will therefore interest themselves in

had so little time for examination, I cannot now give a final, but only a conditional opinion. It is, that I believe it to be a section of a monocotyledonous plant."

promoting

promoting the examination of these ancient relics, the more likely are they to perceive the time fast approaching, when we shall be able with greater certainty to ascertain each deposit by the peculiarity of its vegetable fossils.

The essential character therefore of this first period of vegetation is proved to be the predominance of vascular cryptogamic plants; and we have here a most striking example of the great development which the species in question had attained in this first period of vegetable creation, when the two principal agents, heat and moisture, had evidently exerted an extraordinary influence.

Geologists have entertained, and do entertain, very different notions respecting the origin of coal.

It appears very probable, from the singular development of the vegetation of the first period, that these different combustible beds may have been deposited as a kind of peat of greater or less extent, formed from the remainder of vegetables, and on which other vegetables still grew. This opinion is, I should think, greatly confirmed by the description just given of the Newcastle coal-field. It appears also the more probable, as it is well known that many plants of the families composing this early vegetation, grow abundantly in localities of this kind. The *Equisetum* (Horsetail), the *Osmunda regalis* (Royal Moonwort), and the *Lycopodium* (Club Moss), are all indigenous in our peat soils. Again, we can scarcely doubt, that at this remote epoch our atmosphere had a very different composition from what it now has, and that its difference exerted a powerful influence upon the formation of those bodies of vegetable combustion. The comparison of the successive development of vegetables and animals is not one of the least remarkable parts of the study of these fossil organized bodies. This is beautifully expressed by M. A. Brongniart. He displays by a philosophical reasoning the effects produced by a supposed cause. He states with great perspicuity, why land animals did not exist at one period; why cold-blooded animals became more numerous at another period; and lastly, he gives cogent reasons for the appearance of animals of a more complicated structure, the mammiferæ and birds, in the fourth period.

M. A. Brongniart's reasonings upon this subject are so well epitomized by Professor Jameson, in the Philosophical Journal for March 1829, that I should think it improper at present to enter into more minute details.

The study of this occult science truly opens a hidden field of animated beings and things, whose early call into existence proves

proves the omnipotence of the design. It brings into view a world little looked into or thought of, owing to the obscurity with which it was surrounded. It develops the early, the sublime, the successive works of the great Creator, which before were all supposed to be drowned and scattered about by the mighty burstings of a universal deluge. In other words, the contemplation of these stupendous operations is the true philosophy of the science of geology. If therefore the attention lately paid to the study of fossil conchology has been so highly instrumental in clearing up the many doubts respecting the different sedimentary formations; if the works of Baron Cuvier and others, founded on the early observations of Werner, have afforded us so many interesting proofs of successive creations, from those of the early inhabitants of the deep up to the more complicated structure of the quadruped;—may we not expect equal pleasure and instruction from an application to the study of these ancient vegetable remains, which when once properly examined, will facilitate our knowledge of the forms, characters and qualities peculiar to each epoch, and of the degree of temperature and humidity which must have existed during each successive period.

December 6, 1829.

VIII. *Compendious Tables for converting Time into Space, and Space into Time.* *By* A Correspondent.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE value of compendious and *correct* tables is well known to all practical men, and as among astronomers none are more frequently required than those for the conversion of time into space, and *vice versâ*, I send you two sets for the purpose, the arrangement of which seems preferable to any I have seen; the first may be fixed up in an Observatory; the latter consigned to a traveller's pocket.

I have the honour to be,

Yours, &c.

A Table for converting Space into Time.

Space. Deg. = Min. = Sec. =	Time. H. Min. Sec.	Time. Min. Sec. Thirds	Time. ·Sec.	Space. Deg. Min. Sec.	Time. H. Min. Sec.	Time. Min. Sec. Thirds	Time. ·Sec.	Space. Deg.	Time. H.	Time. Min.
1	0	4	.07	31	2	4	·07	70	4	40
2	0	8	·13	32	2	8	·13	80	5	20
3	0	12	·20	33	2	12	·20	90	6	0
4	0	16	·27	34	2	16	·27	100	6	40
5	0	20	·33	35	2	20	·33	110	7	20
6	0	24	·40	36	2	24	·40	120	8	0
7	0	28	·47	37	2	28	·47	130	8	40
8	0	32	·53	38	2	32	·53	140	9	20
9	0	36	·60	39	2	36	·60	150	10	0
10	0	40	·67	40	2	40	·67	160	10	40
11	0	44	·73	41	2	44	·73	170	11	20
12	0	48	·80	42	2	48	·80	180	12	0
13	0	52	·87	43	2	52	·87	190	12	40
14	0	56	·93	44	2	56	·93	200	13	20
15	1	0	·00	45	3	0	·00	210	14	0
16	1	4	·07	46	3	4	·07	220	14	40
17	1	8	·13	47	3	8	·13	230	15	20
18	1	12	·20	48	3	12	·20	240	16	0
19	1	16	·27	49	3	16	·27	250	16	40
20	1	20	·33	50	3	20	·33	260	17	20
21	1	24	·40	51	3	24	·40	270	18	0
22	1	28	·47	52	3	28	·47	280	18	40
23	1	32	·53	53	3	32	·53	290	19	20
24	1	36	·60	54	3	36	·60	300	20	0
25	1	40	·67	55	3	40	·67	310	20	40
26	1	44	·73	56	3	44	·73	320	21	20
27	1	48	·80	57	3	48	·80	330	22	0
28	1	52	·87	58	3	52	·87	340	22	40
29	1	56	·93	59	3	56	·93	350	23	20
30	2	0	·00	60	4	0	·00	360	24	0

A Table for converting Time into Space.

Time. H.	Space. Deg.	Time. Min. Sec. Thirds.	Space. Deg. Min. Sec.	Space. Min. Sec. Thirds.	Time. Min. Sec. Thirds.	Space. Deg. Min. Sec.	Space. Min. Sec. Thirds.	Time. for decimal parts of a Second.	Space. for decimal parts of a Second.
1	15	1	0	15	31	7	45		
2	30	2	0	30	32	8	0		
3	45	3	0	45	33	8	15		
4	60	4	1	0	34	8	30		
5	75	5	1	15	35	8	45		
6	90	6	1	30	36	9	0		
7	105	7	1	45	37	9	15		
8	120	8	2	0	38	9	30		
9	135	9	2	15	39	9	45		
10	150	10	2	30	40	10	0		
11	165	11	2	45	41	10	15	·1	1·5
12	180	12	3	0	42	10	30	·2	3·
13	195	13	3	15	43	10	45	·3	4·5
14	210	14	3	30	44	11	0	·4	6·
15	225	15	3	45	45	11	15	·5	7·5
16	240	16	4	0	46	11	30	·6	9·
17	255	17	4	15	47	11	45	·7	10·5
18	270	18	4	30	48	12	0	·8	12·
19	285	19	4	45	49	12	15	·9	13·5
20	300	20	5	0	50	12	30		
21	315	21	5	15	51	12	45		
22	330	22	5	30	52	13	0		
23	345	23	5	45	53	13	15		
24	360	24	6	0	54	13	30		
25	375	25	6	15	55	13	45		
26	390	26	6	30	56	14	0		
27	405	27	6	45	57	14	15		
28	420	28	7	0	58	14	30		
29	435	29	7	15	59	14	45		
30	450	30	7	30	60	15	0		

A Table for converting Space into Time—Time into Space.

Space.	Time.			Time.	Space.	Time.	Space.	
Deg. Min. Sec.	H. Min. Sec.	Min. Sec. Thirds	= ·Sec.	H.	Deg.	Min. Sec. Thirds.	Deg. Min. Sec.	Min. Sec. Thirds.
				1	15			
1	0	4	·07	2	30	1	0	15
2	0	8	·13	3	45	2	0	30
3	0	12	·20	4	60	3	0	45
4	0	16	·27	5	75	4	1	0
5	0	20	·33	6	90	5	1	15
6	0	24	·40	7	105	6	1	30
7	0	28	·47	8	120	7	1	45
8	0	32	·53	9	135	8	2	0
9	0	36	·60	10	150	9	2	15
10	0	40	·67	20	300	10	2	30
20	1	20	·33	Dec. of Sec.		20	5	0
30	2	0	·00	Time.	Space.	30	7	30
40	2	40	·67	·1	1·5	40	10	0
50	3	20	·33	·2	3·	50	12	30
60	4	0	·00	·3	4·5	60	15	0
70	4	40		·4	6·			
80	5	20		·5	7·5			
90	6	0		·6	9·			
100	6	40		·7	10·5			
200	13	20		·8	12·			
300	20	0		·9	13·5			

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 30, 1829.—*This day being the Anniversary, the following Address was delivered by the President, Davies Gilbert, Esq. M.P. &c.*

On a former occasion I had the honour to observe, that however gratifying, or even festive, might be the general character of these annual assemblages of men engaged in the active pursuit, or in the patronage of science; yet we could not hope entirely to escape from the usual admixture of pain with pleasure, and of good with evil, incident to all human affairs; that we must expect to hear, with feelings of deep regret, the names of various persons announced to us as departed from this life; of persons endeared to us by private friendships growing out of similar occupations and pursuits, or made objects of our regard and esteem by their splendid careers in the fields of science, by the credit they had acquired for themselves, for this Society, for their country, by their enlargement of human knowledge, and by the consequent benefits they had bestowed on mankind.

Such must undoubtedly be our feelings at such review of the preceding year: but in no previous interval of twelve months has the Society collectively, or have its individual members, experienced losses so severe, or so much in every respect to be deplored, as may be selected from the list this instant read.

The names of Wollaston and of Young and of Davy must occur to every one.

I should have shrunk, under any circumstances, from the unequal task of endeavouring to explain, to illustrate, or to appreciate the works of individuals so preeminent; fortunately for me, the duty is not required. Each of these distinguished persons has already obtained a biographer who will not fail to employ his utmost exertions in discussing topics which I could but slightly touch. A recital of the titles merely of their various works would occupy a large portion of the time usually employed on these occasions.

WOLLASTON.

Dr. William Hyde Wollaston, having passed through the regular gradations of English education, and obtained a Fellowship at Cambridge, devoted some few of his earliest years to the ordinary practice of medicine. But confident in his own abilities, buoyant on the hereditary talents of his family, and urged by that ardent desire for investigating physical truths, for interrogating Nature, and recording her responses, which those alone who have felt are duly qualified to appreciate, Dr. Wollaston withdrew himself from medical practice and from the country; repaired to London, and there employed the whole powers of his mind in those pursuits which have since raised him to the eminence he has so justly obtained.

His first communication to the Society arose out of the profession he had left—An Examination of Gouty Concretions and of Calculi. In this analysis, with the acumen that distinguished him through life, Dr. Wollaston detected essential discriminations of species which had been previously confounded, and the importance of the research may well be expressed in his own words:—

"If in any case a chemical knowledge of the effects of diseases will assist us in the cure of them, in none does it seem more likely to be of service than in the removal of the several concretions that are found in various parts of the body."

Medical investigations were, however, soon abandoned for the great departments of Metallurgy, Electro-Chemistry, Optics, Crystallography, Astronomy.

In the first, two new metals, Palladium and Rhodium, were discovered among the ores of platinum, about the time that platinum itself was rendered malleable in large masses, and thereby adapted to the uses and wants of the philosophical world. The two metals, with all the processes requisite for their extraction, and all their known properties of combination, solution, precipitation, &c., as matters of scientific discovery, were forthwith communicated to our Transactions; but the manipulations used in consolidating platinum, not involving any general principle, he deemed, and rightly deemed, a species of private property, imposing no other duty on the possessor than to furnish an ample and steady supply adequate to the demand. This he most satisfactorily afforded; and finally he made known to the public, through our Society, the art of manufacturing this metal; thereby giving to science that which might have been fairly retained as a family inheritance.

The

The science of Crystallography dates a new æra from his invention of the Reflective Goniometer. That delicate instrument, by discriminating angles previously compounded one with another, has proved that a vast variety of substances have primitive forms or cleavages peculiar to themselves, and consequently exhibit external angles descriptive of the individual or of the class.

Besides inventing the goniometer, Dr. Wollaston has bestowed on the science of Optics, various improvements of telescopes and microscopes, and a deep investigation into the arrangement and into the functions of the optic nerves.

We have also the Camera Lucida, founded on the ingenious principle of allowing two pencils of rays, distinct and independent from each other, to enter the pupil of an eye at the same time. By this contrivance the most complicated forms may be delineated with perfect accuracy; and we owe to the same ingenuity the application of concavo-convex lenses to the most extensively beneficial of all optical contrivances, thereby enlarging the field of view and rendering vision more distinct.

We are indebted, moreover, to the same powerful mind for interesting astronomical speculations, extending beyond the limits of the solar system; for several mechanical inventions; and for assistance afforded to our manufacturers in their chemical processes.

These talents and these exertions have enrolled the name of Wollaston on the lists of all the most learned Societies of Europe.

Much more remains to be noticed; but I must here close. The editor of a volume can alone do justice to this extraordinary man.

YOUNG.

Dr. Thomas Young came into the world with all the advantages of early ability cultivated by academical education and improved by foreign travel, and with a confidence in his own talents growing out of an expectation of excellence entertained in common by all his friends; an expectation more than realized in the progress of his future life.

Mathematics in the most abstruse recesses of modern improvements; astronomy, theoretical and practical; experimental and mechanical philosophy; chemistry; natural history; ancient and modern languages; philology; in addition to the regular practice of medicine; were carried to such an extent that each might have been supposed to have exclusively occupied the full powers of his mind.

One thus highly endowed by the gifts of Nature and stored with the multifarious fruits of labour and of assiduous application, might well be imagined to have satisfied himself with the possession of abstract or general knowledge, disposed rather to speculate on systems than to descend into the region of individual facts. On the contrary, Dr. Young, as if time could be extended at his will, has peculiarly distinguished himself by labour in detail.

We have from him A Course of Lectures on Natural Philosophy, and

and the Mechanical Arts, in two volumes quarto; a work replete with the most minute and multifarious details, and with references to all known writings on the different subjects.

We have from him Elementary Illustrations of the Celestial Mechanics of Laplace, displaying such powers of rendering simple and familiar the obscurities of a work in all other respects equal to the highest expectations of the present age, that one cannot but deeply regret the sudden discontinuance of what promised so much utility to the rising generation, by smoothing difficulties, and thus leading on young minds to the attainment of what the Greeks κατ' εξοχην denominated Learning.

Dr. Young did not neglect to illustrate various subjects connected with his more immediate profession. Among several others, A Treatise on Consumption has obtained a considerable degree of reputation. But the most difficult investigations gave him, in all probability, the greatest delight. The corpuscular and vibratory theories of light; the motions and oscillations of fluids, with the theory of the tides; the nature and powers of capillary attraction; were objects of his peculiar and successful attention. Magnetism as connected with electricity; the magnetism and figure of the earth; the whole theory of chances, with the probable duration of human life; the difficult task of determining, with an accuracy sufficient for scientific purposes, the exact interval between the line of suspension and the centre of oscillation, of bodies not assumed to possess any strict geometrical forms or unvarying densities; the different temperatures of the Diatonic scale, are among the various subjects illustrated by his care: while the duties of Secretary to the Board of Longitude, involving a minute and constant superintendence of the Nautical Almanac, throughout all the stages of its construction, and final publication, were sufficient, during many years, to have absorbed a large portion of the time of any ordinary man. But, at the very moment when these duties had become, from different causes, most burdensome on his mind, a new object for pursuit was found and eagerly followed through fields heretofore unexplored. The military occupation of Egypt by an European power in the concluding years of the last century, together with the investigations made during that time into the stupendous and interesting remains of antiquity still preserved in that far-famed country, did not fail of exciting an ardent curiosity throughout the civilized world, respecting the figures and characters engraved on the most durable materials, but of which nothing had been known since the revival of letters, beyond a traditional account, derived from ancient writers, of their being Hieroglyphics. The discovery, however, of some Polyglot inscriptions having been supposed likely to afford a key, several men of great learning and in different countries joined eagerly in the career of decyphering them; among whom Dr. Young is supposed to have maintained the precedence which he first gained. One very curious and important fact has been established beyond the reach of doubt or controversy. When foreign nations, the Persians, the Greeks and the Romans,

Romans, gained possession of the country and learnt the use of these symbolical characters, they endeavoured in succession to express the particular sounds of their own languages in proper names, by using the hieroglyphic as an acrostic of the word with which it had been previously associated in the original designation of things. The same process is said to be now actually in progress with the symbolic characters of China*, making a certain limited number of characters Acrostic and Phonic: and thus has been developed the only rational manner in which the greatest of all human inventions, the formation of an alphabet, could have been achieved. As a precious yet melancholy gift, we may shortly expect a posthumous work on the Egyptian or Coptic language, in part dictated by the dying breath of this most distinguished person, of whom it may therefore be truly said, that from the tomb he illuminated mankind.

I must here conclude my inadequate and superficial sketch, drawing, however, if one may be permitted to do so, an inference from one so preeminent; that, although expatiating through the

* "Though it is likely that all hieroglyphical languages were originally founded on the principles of imitation, yet in the gradual progress towards arbitrary forms and sounds, it is probable that every society deviated from the originals in a different manner from the others; and thus for every independent society, there arose a separate hieroglyphic language. As soon as a communication took place between any two of them, each would hear names and sounds not common to both. Each reciprocally would mark down such names, in the sounds of its own characters, bearing, as hieroglyphics, a different sense. In that instance, consequently, those characters cease to be hieroglyphics, and were merely marks of sound. If the foreign sounds could not be expressed but by the use of a part of two hieroglyphics, in the manner mentioned to be used sometimes in Chinese dictionaries, the two marks joined together, became in fact a syllable. If a frequent intercourse should take place between communities speaking different languages, the necessity of using hieroglyphics merely as marks of sound, would frequently recur. The practice would lead imperceptibly to the discovery that, with a few hieroglyphics, every sound of the foreign language might be expressed; and the hieroglyphics, which answered best this purpose, either as to exactness of sound or simplicity of form, would be selected for this particular use; and, serving as so many letters, would form, in fact, together what is called an alphabet. This natural progression has actually taken place in Canton, where, on account of the vast concourse of persons, using the English language, who resort to it, a vocabulary has been published of English words in Chinese characters, expressive merely of sound, for the use of the native merchants concerned in foreign trade; and who, by such means, learn the sound of English words. To each character is annexed a mark, to denote that it is not intended to convey the idea, but merely the foreign sound attached to it. The habit of applying the sound, instead of the meaning of hieroglyphics, to foreign words, led to the application of them likewise as sounds, to assist the memory in the pronunciation of other hieroglyphics in the same language, but not in common use; and the repeated application of them for those purposes may be at length supposed to have effaced their original use."—*Stanton's Embassy*, vol. ii. p. 576-8.

fields

fields of science and of literature, he has successfully collected flowers from all, appropriating the well known passage of Lucretius,

> Floriferis ut Apes in saltibus omnia libant,
> Omnia nos.

Yet, referring to an equally well known apology for his condensed mode of writing, prefixed to the Aphorisms of Hippocrates,

> Ὁ Βιος βραχυς· ἡ δε Τεχνη μακρη,

we may be allowed to hope that others of less powerful abilities or of less persevering energy of mind, may concentrate the objects of their research within the limits of some defined portion of science, rather than make inadequate endeavours to embrace the whole.

Davy.

I am now led by the succession of melancholy events to that portion of my duty on the present occasion, which is by far the most painful to myself.

With Wollaston and with Young I have been intimately acquainted for many years; but Davy I have known from his childhood. I knew his parents, his family, and his relations. I witnessed his commencement in science, and by recommending him, at that decisive moment, to the patronage of Dr. Beddoes, I may have had the good fortune to fix and to smooth the splendid course which has carried him to that pinnacle of Fame, which his abilities, his energies, and inventive faculties, entitled him to attain.

It may not be uninteresting here to notice the first experiment that gave me a strong feeling of his merit, and which I believe has never been laid before the public.

Davy, then about seventeen, had formed an opinion adverse to caloric, or to the materiality of heat, and he attempted an *experimentum crucis* in the following manner :—Having procured a piece of mechanism set in motion by a spring, he added two horizontal plates of brass, the upper one carrying a small metallic cup, to be filled with ice, revolved in contact with the lower. The whole machine, resting on a plate of ice, was covered by a glass receiver, and the air exhausted. It was then allowed to move, when the ice in the small cup was soon observed to melt; and the conclusion was drawn that this effect could proceed from vibratory motion alone, since the whole apparatus was insulated from all accession of material heat by the frozen mass below, and by the vacuum around it.

This experiment does not, unquestionably, decide the important matter in dispute with respect to our ethereal or transcendental fluid; but few young men remote from the society of persons conversant with science, will I believe any where present themselves, who are capable of devising any thing so ingenious.

Davy continued his researches on the nature of heat after his removal to Dr. Beddoes at Clifton in the autumn of 1798, and published them in a provincial collection of tracts. This paper caught

caught the attention of Count Rumford, and became the medium of his invitation to the Royal Institution.

At Clifton, Davy's thoughts were directed to a multiplicity of subjects, many having reference to the main object of Dr. Beddoes's pursuit at that time,—the application of factitious airs or gases to the purposes of medicine. In his system of therapeutics, as in the subsequent theories of electro-chemistry, oxygen, the supporter of combustion, held one extremity of the scale as a stimulus, while inflammable gases occupied the other extremity, as sedatives; various combinations were tried. Carbonated hydrogen was thought to be narcotic. Azote or nitrogen, in its simple state appeared to be noxious only from the absence of oxygen; combined with that active principle, in what has since been named a Deutoxide, it produced instantaneous suffocation. The protoxide had indeed been made, and to a certain degree examined; but it was reserved for Davy to ascertain its exact proportions, previously to the establishment of the atomic theory, and to multiply experiments on the medical qualities of an air supposed to increase present action without inducing subsequent debility, and to act rather by augmenting the power of receiving excitement, the excitability, than in the usual mode of stimulus. The ingenuity of the chemist who investigated Gaseous Oxide remains upon record, but the panacea has long since vanished into empty space.

Here Davy exercised himself, moreover, in one of the most beautiful departments of analytical chemistry, and to which the destructive operations of our predecessors were directly opposed—the ascertaining proximate elements of organic substances. He mainly in these researches separated and distinguished the principle forming an insoluble compound with gelatine, from the gallic acid, to which it is nearly allied. He ascertained its identity in various vegetable bodies, and improved its application to the purposes of manufacture. But the discoveries that will enroll his name among those few destined to go down to the latest posterity, were made in London and at the Royal Institution :—

The metallizing the alkalies and the earths, which has opened entirely new views into the material world, with reference to the construction of the earth, of volcanic actions, and of the most curious meteorological phænomena.

The ascertaining that galvanic action augments, reduces, or inverts all chemical affinities, so as to carry alkalies through acids, and with a power of magic to transfer the constituent parts of a compound body to the opposite poles of a galvanic pile. This union of electricity with chemistry must for ever rank among the most splendid of theoretical discoveries. It has disclosed views entirely new of the most important energies of nature; and when the action of electro-chemistry shall have become, like extended quantity, subject to rigorous calculation, and when the powers of corpuscular attraction shall be thus placed within our reach, the whole must be referred to the acumen of that mind which first detected

tected the principle, leaving to others the comparatively easy task of subsequent addition.

The true nature of that substance previously named oxymuriatic acid, with the discovery of a new supporter of combustion negative in comparison with all other substances except oxygen on the galvanic scale. This discovery, like those already noticed, has impressed a new form on chemical science, and especially it has corrected some too hasty generalizations in the theories and in the nomenclature of that most distinguished chemist who enlightened the world through the medium of a new language.

To these may be added, on account of its practical utility and the ingenuity of its construction, although on a scale not in any degree commensurate to those of discoveries affecting general principles or extensive science,—the invention of what has been termed the safety lamp, for guarding against the explosion, in coal mines, of inflammable air. The fact had previously been observed, that flame would not pass through narrow tubes. I have letters from Davy which explain the progress of his thoughts. His first idea was to admit air at the lower surface of the lamp through tubes sufficiently long, and slender in diameter to exclude flame; and to allow the escape of air through similar tubes at the top; surrounding the sides with glass, talc, or horn, in the usual way. But since internal combustion and explosion cannot be avoided, it is obvious that such coatings must be liable at least to the danger of being scorched or burst. I have a second or a third letter with the following query :—Since the length of a tube adequate for resisting flame appears to diminish in some higher ratio than its section, may not the interstices of wire gauze be considered as tubes, and the thickness of the gauze itself as their lengths?—And if so, will not wire gauze, made sufficiently fine, stop the progress of flame and be free from the dangers attached to other transparent substances, from internal explosion? I need not add that this plan has succeeded.

The protection of copper sheathing on vessels, I have had occasion to notice in a former year; and I shall only repeat, that it forms a most legitimate deduction from the laws of electro-chemistry; and that whenever practical difficulties shall be overcome, a doubt can scarcely be entertained of its ultimate success.

Such is the imperfect sketch that I have presumed to give of Sir Humphry Davy, omitting much of importance in his high character of an inventive philosopher, by which he has added to the credit, to the honour, and to the fame, of this most distinguished Society, by which he has diffused a lustre on the province which gave him birth and on the entire nation to which he belonged.

Some short account of so extraordinary a man in his early years, may however be expected from the only member of the Society who has witnessed them.

In infancy his mind ran upon romance. He had probably read or heard some tales of chivalry. His ardent wish was to issue forth armed cap-à-pie and to clear the world of giants and monsters. At school,

school, in Penzance, he advanced to eastern tales and legends, so that many contemporaries remember standing round him with delight, whilst he repeated, varied or invented fiction for their amusement. The gradation from this habit to measured poetry was natural and easy. The grand objects of nature laid hold on his imagination; and we have from his pen, at an age not much further advanced, a poem on St. Michael's Guarded Mount, equal, if not superior, to any of the numerous tributes paid to that magnificent promontory, equally interesting to the antiquary, to the geologist, and to the admirers of scenery at once rugged and sublime. Painting, about the same time, became also a favourite amusement; and specimens, indicative of no common genius, may now be seen at the Royal Institution, where they illustrated his admirable, and admired Lectures.

These attempts at painting become doubly and trebly interesting on another account. Some of his contemporaries have expressed to me their belief, that experiments instituted for the purpose of preparing colours, first directed Davy to the pursuit of chemistry. Notwithstanding these various avocations, his advancement in school learning at Truro, under Dr. Cardew, in whose praise as a master too much cannot be expressed, was equal to that of his most able companions; and Davy returned to the house of his benefactor at Penzance, with sufficient acquirements, and with sufficient means derived from his father, for executing his favourite plan of studying at Edinburgh, and there procuring a Medical degree. The execution was, however, delayed by the advice and authority of Mr. Tonkin, who recommended a preparatory medical education at Penzance. It was finally abandoned, I believe, at the Royal Institution.

In a work composed on the bed of sickness, and when all rational hope of a permanent recovery had ceased, the original genius of his youth again burst forth; a spirit which had bestowed on his Lectures an eloquence and an interest to be derived from no other sources. No one can read *Salmonia* without having feelings excited similar to those of the writer; no one, reading it, could form any other opinion than that of its author having devoted the whole of his time to the exclusive cultivation of natural history and of elegant literature.

The poetic bent of Davy's mind seems never to have left him. To that circumstance I would ascribe the distinguishing features in his character, and in his discoveries:—a vivid imagination sketching out new tracts in regions unexplored, for the judgement to select those leading to the recesses of abstract truth.

Having characterized Davy by poetic genius, I would venture to ascribe minute accuracy, even in the merest trifles, as the distinction of Wollaston, and almost universal acquirements as the characteristic of Young. While in soundness of judgement combined with general ability of the highest class, no discriminations can be found.

On recurring to the list of deaths in the preceding year, many other

other names will unfortunately be found of individuals highly entitled to our respect and regard.

Among these I would mention Dr. Edward Ash.

Dr. Ash has not, I believe, honoured our Transactions with any communication; but his acquirements in science are well known, and every contemporary at Oxford will attest the elevated reputation he obtained in the University for the classic literature cultivated in that seat of learning. In after life he adorned the profession from which this Society has derived a large portion of its splendour and renown.

Lord Buchan, a man of genius and of letters, to whom the world is indebted for a Life of Napier, with an account of his most splendid invention, the canon of logarithms, without which neither the Tables, nor the reductions of modern Astronomy, could be executed, nor many of the formulæ in the higher geometry be brought into practical use. For nearly ten years Lord Buchan has remained the senior Fellow of this Society, ever since the decease of the late king, who for some years bore in a double sense, the relation of father to the Royal Society.

Lord Oriel, late Mr. Foster, Speaker of the Irish House of Commons, was distinguished for his attention to the agriculture, to the trade, and to the manufactures of the nation, more especially of that part with which he was most intimately connected; and during a life so long continued, that he alone, of all the gentlemen of the United Kingdom who were members of the last parliament of George the Second, lived to take a seat in the parliament of the present king.

Mr. Smithson has added eight communications to our Transactions. He was distinguished by the intimate friendship of Mr. Cavendish, and rivalled our most expert chemists in elegant analyses: but the latter part of his life has been passed abroad.

Sir Christopher Hawkins was not merely the patron, but the active promoter of science and knowledge in Cornwall, his native county, where so ample a field is laid open for investigation; and he has given to the world an ingenious treatise On the Trade of the Ancients with Britain for Tin, its then only export.

Dr. Wavell has been distinguished by the discovery of a very peculiar mineral,—a sub-phosphate of alumina,—to which his name has been affixed.

The Rev. Robert Mares, a gentleman eminent for general proficiency in literature, and whom we have all known and esteemed.

And lastly, I would mention Mr. Tavel, highly distinguished as a tutor in the University, and the particular college, most eminent for physical sciences; and we have to lament his loss at a period of life when the attainments of early youth are most likely to be rendered beneficial to the world.

On the Foreign List,

M. VAUQUELIN.

I have a letter from his brother, announcing the death of M. Vauquelin on the 14th of this month.

M. Vauquelin

M. Vauquelin was born, I apprehend, about the year 1763: thirty years afterwards he became associated with M. Fourcroy, and he has since ascended through all the honours of science bestowed in France, and in 1823 his name was inscribed on the Foreign List of the Royal Society. His communications to different Societies have been numerous, and we have a separate work on the difficult art of assaying metals with reference to their commercial importance. M. Vauquelin undoubtedly ranked high among the band of natural philosophers who have given in the last fifty years an impulse to science never before experienced, and never expected to occur.

On delivering the Medals:—

MR. CHARLES BELL.

To no department of science has the Royal Society been indebted in nearly so great a degree for eminently distinguished members or for important communications, as to that of Medicine.

Ουδεις ἀγεωμετρητος εισιτω was the inscription affixed to the academy of Plato.—"Let no one without ability, acquirements and industry, enter here" might be inscribed on a School of Medicine. But the man of ability, of acquirement and of industry, once within the walls, finds himself surrounded by so many objects for inquiry, important in themselves and calculated to raise the feelings, on every side, that his exertions are excited and his energies called into action, till all the powers of his mind acquire the habit of applying themselves with full and undivided efforts to whatever subject is presented to their grasp.

The object to which Mr. Bell has successfully applied the energies of his powerful mind is one preeminently conspicuous in his own profession and in its utility to mankind. Of all the branches of human knowledge, anatomy has experienced the greatest difficulties in struggling against passions, prejudices, and superstition. Throughout the Mahometan world I believe the science is unknown. Astronomy, chemistry, general knowledge have been persecuted in dark ages; but here, and in these times, in the country which gave birth to Harvey three hundred years ago, difficulties are still opposed against the acquirement of this most practical and useful science; so that, unless some remedy is applied, experience must be hereafter acquired by operations on the living subject, instead of on those in which vitality and the sense of pain are no longer to be found.

From these and from other causes the progress of anatomy has been peculiarly slow. The skeleton, indeed, obtruded itself into notice by so many ways, that osteology grew into a science by efforts of its own. To this, after a considerable interval, was added a knowledge of the muscles, of the digestive and secretory organs, with that branch of the absorbents then named the lacteals. The circulation of the blood, as we all know, was discovered by the great Harvey, whose portrait adorns this room. The nature of respiration could not be developed till modern chemistry had ascertained the nature and the composition of elastic fluids; and the general system of lymphatics, diffused over the whole body and ending in the subclavian vein, has been reserved almost for our own time. There still remained a system the most important of all, the medium connecting the sentient, the self-moving principle, with the external frame; the principle that imparts the vital energies to common matter destined to lose them all when that communication is withdrawn. The mere existence of nerves has indeed been long known, but, till latter times, in a manner so imperfect that tendons were blended with them, and the error has not yet entirely disappeared from the phrases of common speech. At last the nerves were delineated on plates and described in words; their *origins* found in the brain, the medulla oblongata, and the spinal cord; their courses traced through conglobate glands, and, after blending with each other in a thousand different ways, they were found to constitute a net-work surrounding and penetrating the whole animated frame. Conjectures were formed as to different functions being imputable to different nerves, as to the use of ganglions, and as to the probable cause of assimilated actions and of distant sympathies residing in those nervous intersections.

We are indebted to Mr. Bell for much more than realizing all that could previously be considered but as mere hypothesis. We learn from his most profound researches, That nerves subservient to the mere functions of life may be traced, even in the human frame exhibiting a simplicity of construction little different from those of the lowest animals. That, as the scale of being advances, nerves are superadded differing in their origin, in the purposes for which they serve, and of a complexity proportionate to the variety of functions they have to perform, associating the actions and feelings of parts otherwise unconnected, as is peculiarly observable in the nerves affecting respiration and in those of the face; thus adapting the whole frame to the performance of those complicated duties for which it is designed. We further learn—That all the nerves connected with voluntary muscles, and not these alone, have a double origin arising from distinct roots, and from distinct columns of the spinal marrow; that each part continues separate from the other throughout the whole of their course, and of their ramifications, one part conveying the influence of the will, the other part transmitting sensation in an opposite direction; thus establishing a circle of actions.—But I am conscious of my inability to convey any adequate idea of those most important researches, the subject of five communications printed in our Transactions for 1821, 1822, 1823, 1826, and for the present year.

To these I refer not medical gentlemen, for they, I am sure, have already made them the subject of their most serious attention; but I refer every man of a liberal mind to whom science and knowledge are delightful in themselves *.

On delivering the Medal in the name of the Royal Society, I need only pronounce the simple word, *Procede.*

* Cognitio ipsa rerum consideratioque delectat.—*Cicero.*

Your

Your second Medal has been adjudged to M. Mitscherlich.

No attempts of naturalists have been more unremitting or more ardently pursued than those for connecting the external characters of all bodies with their internal qualities, which really constitute their natural families, their true classes, genera, and species.

The labours of Linnæus, so successful in the organized kingdoms, and so much the contrary in respect to minerals, may be cited as an example. Since the great discovery by Mr. Dalton of Definite Proportions, by far the most important, I have always thought, since the laws were demonstrated which bind together the universe, the hope of obtaining such characters has been raised higher than before; while crystals measured by the goniometer of Dr. Wollaston promised to rival, in the quality of scientific indices, the fructification of plants, which they had always emulated by their beauty. But these hopes were not at once realized; anomalies appeared, and no lengthened clue could be found till M. Mitscherlich ascertained,—That the chemical elements of bodies may be arranged into classes or groups giving isomorphite crystallization; so that when compounds are formed by the unions of these substances with equal atomic proportions, crystals are produced identical in their primitive and in their secondary or modified forms. Hence it follows, that the mixture, in any proportions, of compound bodies having this quality, will present crystals of the form common to each in its unmixed state; as is said to take place in all the varieties of magnesian limestone, however the relative proportion between the two carbonates may change. The investigation has been extended to other bodies found dimorphous under various circumstances, explaining further apparent anomalies in the atomic theory: but the subject is at once too novel and too intricate for further discussion here.

M. Mitscherlich has had the additional good fortune to detect a very irregular and unexpected property of crystallized bodies, in reference to heat. Their expansions in different directions, are found to vary after different laws in respect to temperature; thereby modifying their action, in giving polarity to the rays of light. So extraordinary have the laws of expansion been found in Iceland spar, that while in common with all other substances it expands in bulk by the accession of heat, and its linear dimension is extended in one direction, the sign of expansion is actually changed in the direction perpendicular to the former, and it contracts.

Perhaps I may be permitted to read the Report of a Committee appointed by your Council of last year.

November 26, 1828.

"Your Committee have taken into consideration the subject referred to them; and, bearing in mind the important object for which it has been so referred, they desire that they may not be understood as pronouncing sentence on any disputed point, or as assuming any weight to be due to their judgement beyond what the circumstances under which they meet may authorize, when they state it as their opinion that the doctrine of the equivalent forms of chemical

chemical elements, as announced by Professor Mitscherlich, in his Memoirs communicated to the Royal Academy of Berlin in the year 1819, and to the Royal Academy of Stockholm in 1821, and modified and extended in his subsequent writings, is one of high importance in crystallography, and has received from M. Mitscherlich himself, and from a variety of other crystallographic and chemical researches and analyses by others up to the present time, such subsequent elucidation and confirmation as now to form a very considerable branch of science, and one in which further progress is confidently to be expected, and indeed is continually making; and that, however opinions may differ as to the extent to which his original views are borne out, and whatever degree of obscurity may yet be regarded as hanging about some parts of the doctrine itself, or its general applicability, it has already furnished the means of grouping together a vast number of facts under general laws, and afforded reasonable solutions of a great many difficulties both in chemistry and mineralogy.

"Your Committee are further of opinion, that the facts discovered by Professor Mitscherlich, of the difference of expansibility, by heat, of crystallized bodies in different directions, is a highly important step in pyrometry, and is sufficiently established by himself, by decisive experiments, to leave no doubt of its reality; while the corresponding changes in the optical properties of diaphanous crystals, produced by the same cause, appear to them important accessions to our knowledge of the properties of light and the relation between it and crystallized matter.

"Finally, your Committee have no hesitation in stating it as their opinion, that, should the Council of the Royal Society think proper, now or at any subsequent period, to mark their estimation of these services rendered to science by a Medal, such reward would be fully justified by their intrinsic merit and value."

For these discoveries your Medal has been awarded to M. Mitscherlich, with the full confidence that the approbation of the Royal Society of England thus expressed, cannot fail of animating a man of science in a distant country to renewed and vigorous exertion in the prosecution of a work that has procured for him such a high distinction.

LINNÆAN SOCIETY.

Nov. 3rd and 17th.—A. B. Lambert, Esq. V. P. in the Chair. Read A Description of *Filaria Forficulæ,* by Mr. Benj. Maund, F.L.S., accompanied with a specimen. This is an intestinal worm found in the Earwig; sometimes 2 or 3 in one individual, each not less than three inches long; the whole cavity of the abdomen, and in some instances part of the trunk being filled with it. When removed, they live two or three hours in water, but die immediately in the atmosphere.

A communication from J. E. Bowman, Esq. F.L.S. was also read: On the Parasitical Connection of *Lathræa squamaria,* and the peculiar structure of its subterranean leaves.

After

After some remarks on the interest which attaches to the examination of remarkable exceptions to the general laws of structure in vegetables, and especially as regards parasitic plants, whether cryptogamous or phænogamous, Mr. Bowman relates some curious peculiarities which in the last and present year he had detected in the *Lathræa squamaria.* Having caused the flowering stems of a young plant to be trenched round, detaching from the ash-tree under which it grew the contiguous portions of root, the whole mass was insulated and separated; and then, by cautious agitation in a neighbouring stream, the soil was washed away, and the roots and fibres of the ash, together with the subterranean stems of the *Lathræa,* completely matted together, were left exposed; and showed, that though its base does not penetrate the stock as in *Orobanche,* yet it is truly parasitical, all its forked fibres having minute tubercles by which they attach themselves to the roots of the tree.

It is remarkable, that instead of rising towards the surface, the embryo stem takes a downward direction, till it comes to the roots of the tree, when it spreads among them and fixes its tubers on them, the flowering branches being the only parts that appear above the surface.

To show the nature of the parasitical connection, Mr. Bowman then gives a full and minute description of the tubers and their connecting fibres, which had hitherto escaped notice, illustrated with highly magnified figures. The tubers seem to be succulent homogeneous substances, sending down from their surface a tap into the alburnum of the roots of the tree, to which the parasite has become attached, but never into the woody fibre.

In treating of the tooth-shaped scales, from which the plant has been by some called *Dentaria squamaria,* and Toothwort, Mr. Bowman mentions that, their real character being mistaken, they had been considered as roots, till Sir J. E. Smith latterly referred them to their true character, of a subterranean herbage. The absence of green colour in most plants that are parasitic on roots, Mr. Bowman ascribes not merely to their borrowed nutriment, but more especially to their having no true leaves; all plants destitute of leaves furnished with pores not being subject to the action of the atmosphere and of light. The *Lathræa,* however, though furnished with true leaves, has the pale hue of the other parasites; and this Mr. Bowman ascribes to the functions of the leaves being performed in the total absence of light, and to their being destitute of pores.—The paper concludes with a minute description of the curious structure of the subterranean leaves, which appear to be furnished with cells lined with innumerable minute glandular papillæ, which Mr. Bowman supposes to perform the office of cuticular absorbents. So that whereas ordinary leaves with a porous cuticle, when acted on by air and light, receive carbonic acid gas, and throwing off the oxygen, retain the hydrogen and carbon; in the case of the *Lathræa,* whose leaves are buried, the air-valves, instead of being in the cuticle, where they would have been obstructed by the surrounding earth, are placed within the convoluted cells, which he has discovered within the substance of the leaf.

Dec. 1st.

Dec. 1st.—Read, Part of a narrative of a journey in Mexico, with a description of the natural history of the tract passed through; in a letter from W. Schiede, M.D. and Ferd. Deppe, to A. B. Lambert, Esq. V.P.L.S. translated from the original German.

15th.—Read, Observations on the *Vicia angustifolia* of the English Flora, by Edward Forster, Esq. V.P. and Treas. L.S.

GEOLOGICAL SOCIETY.

Nov. 6th, 1829.—The Society assembled this evening for the Session.

George Biddell Airy, Esq. M.A. Fellow of Trinity College, Cambridge, and Professor of Astronomy in that University; John Macpherson Grant, Esq. of Ballindalloch, N. B. and attached to His Majesty's Legation at Turin; John Heywood Hawkins, Esq. of Bignor Park, Sussex; Philip Duncan, Esq., Fellow of New College, Oxford; and William Cavendish, Esq., M.P. M.A. Trinity College, Cambridge, and Belgrave-square London, were elected Fellows of this Society.

A Paper was read, "On the Tertiary Deposits of the Vale of Gosau in the Salzburg Alps; by the Rev. Adam Sedgwick, Pres. G.S. F.R.S. &c., and Roderick Impey Murchison, Esq. Sec. G.S. F.R.S. &c."

The authors present this as the first of a series of memoirs in which they hope to throw some light on the structure of the tertiary formations in Salzburg and Bavaria, and their varied relations to the secondary rocks of the Austrian Alps.

These deposits, the highest members of which descend into the flat regions near the banks of the Danube, become, in their lower groups, more elevated and more highly inclined; and, as they approach their southern or Alpine barrier, are sometimes vertical: whilst in the valley of Gosau and far within that barrier, formations with the same organic remains are found at much higher elevations, inclosed in Alpine limestone, on which they rest unconformably, and in a nearly horizontal position. This deposit of Gosau the authors conceive to have been formed in one of the arms of an ancient sea which, like the present salt-water lochs of Scotland, must have penetrated deeply into the then existing valleys of the Alps; whilst its actual position incontestably proves that it must have been prodigiously upheaved at some time posterior to the epoch of its formation.

In ascending the drainage of the Traun to the district under review, patches of these tertiary formations are described as occurring in various small transverse valleys between Gmunden and Ischel; but these are comparatively at low elevations, and all traces of them are lost in the higher regions between Ischel and the Lake of Hallstadt, which is about 1700 feet above the level of the sea. The valley of Gosau is described as situated more than five miles to the west of that lake and about 900 feet above its level. The formations which the authors consider Tertiary, occupy the flanks of this valley, and are chiefly exhibited in two hilly ranges, the Horn on the west, and the Ressenberg on the east. The beds of these hills are nearly horizontal,

horizontal, have an estimated thickness of not less than 2600 feet, and are shut in on all sides by Alpine limestone, forming on the south a great serrated barrier, the highest pinnacles of which are more than 10,000 feet above the level of the sea.

The following abstract of detailed sections derived from the Horn and the Ressenberg, exhibits the strata in descending order.

1st. Red and green slaty, micaceous sandstone several hundred feet thick (cap of the Horn).

2nd. Green, micaceous, gritty sandstone extensively quarried as whetstone, succeeded by yellowish, sandy marls (Ressenberg).

3rd. A vast, shelly series consisting of blue marls alternating with strong beds of compact limestone and calcareous grit, the upper beds of which are marked by obscure traces of vegetables; and the middle and inferior strata, by a prodigious quantity of well preserved organic remains, out of which the authors collected upwards of eighty species of bivalve and univalve shells, and fifteen species of corals. (Localities :—beds of torrents descending into Gosau-Thal.)

4th. The above shelly series graduates downwards into beds of a more conglomerate form which pass into a red sandstone and marl containing gypsum; and a coarse conglomerate, forming the base of the whole system, rests upon, and abuts against, the alpine or saliferous limestone. (Locality :—Russbach.)

Amongst the shells occurring in the group No. 3, are

Bivalves:—Crassatella 2 species, Corbula 1, Pectunculus 3, Cardium 3, Plicatula 2, Gryphæa 2, Trigonia 2, Pecten 1, Solen 1, Anatina 1, Lucina 1, Astarte 1, Venus 2, Cypricardia 1, Isocardia 1, Ostrea 2, Hippurites 2 *, &c. &c.

Univalves:—Melania 2, Melanopsis ? 1, Ampullaria 1, Neretina 1, Natica 3, Trochus 1, Turbo 1, Turritella 2, Cerithium 6, Nerita 2, Turbinella 1, Fusus 2, Rostellaria 1, Buccinum 3, Mitra 2, Volvaria 2, Conus ? 1, &c. &c.

Corals:—Turbinolia 1, Caryophyllia 3, Fungia 2, Cyclolites ? 2, Astrea 5, Madrepora 2.

The above organic remains have been examined by M. Deshayes and Mr. J. Sowerby, neither of whom detected a single species identical with any known fossil of the secondary rocks, whilst they consider the greater number of the genera to be eminently characteristic of the tertiary period.—The authors have further remarked a strong resemblance between these fossils and certain unpublished species of the Vicentino, and Mr. Sowerby has identified a few species with well-known tertiary shells. It is, therefore, concluded both from negative and positive zoological evidence, as well as from the unconformable position of the beds, that the whole deposit of Gosau must be considered tertiary, or, in other words, younger than the chalk. At the same time, the great proportion of new species contained therein, and the absence of those identifications with recent shells which

* The genus Hippurites is placed among the bivalves on the authority of M. Deshayes.

which mark the fossils of the younger tertiary groups, prove that it must be ranked with the most ancient deposits of that series.

In the basins which have been best examined, there is an entire break between the secondary and tertiary groups. But the great mechanical agents which in these localities have elevated and ground down the secondary rocks, before the commencement of the tertiary, may not have acted universally. There is therefore reason to expect in distant localities new groups of rocks by which this break may be filled up; and by help of which it will perhaps be found that the newest secondary rocks and the oldest tertiary, graduate finally into each other.

Nov. 20.—J. R. Gowen, Esq. of Highclere, near Newbury, and William Holbech, Esq. of Farnborough, Warwickshire, were elected Fellows of this Society.

The reading of a paper, "On the Tertiary Formations which range along the Flanks of the Salzburg and Bavarian Alps," being in continuation of the memoir On the Valley of Gosau, by the Rev. Adam Sedgwick, Pres. G.S. F.R.S. &c., and Roderick Impey Murchison, Esq. Sec. G.S. F R.S. &c. was begun.

Dec. 4.—Nicholas Dennys, Esq. of Cambridge Terrace, Regent's Park; John Willimott, Esq. of Jermyn-street, St. James's; William Higgins, Esq. of Coggeshall, Essex; and Edward Spencer, Esq., of Highgate, were elected Fellows of this Society. His Imperial Highness the Arch-duke John of Austria; Professor Hausmann, of Göttingen; M. Hoffmann, of Berlin; M. Voltz, of Strasbourg; M. Dufrenoy, of Paris; and Dr. Ami Boué, were elected Foreign Members of the Society.

The reading of the paper by the Rev. Adam Sedgwick, Pres. G.S. F.R.S. &c., and Roderick Impey Murchison, Esq. Sec. G.S. F.R.S., &c. begun at the last meeting, was concluded.

The authors, having in a former communication described the great relations of the tertiary formations on the north flank of the Alps to the older part of the chain, proceed in this paper to confirm their conclusions by a series of detailed transverse sections, commencing with the hills near the foot of the Traunsee, and ending with the lofty hills of molasse and conglomerate near the Lake of Bregenz.

1. *Section at the foot of the Traunsee.*—The tertiary formations here commence on the north side of the Traunstein; and the lower beds are described as being chiefly argillaceous, of a great thickness, and in a highly inclined position. They contain some of the Gosau fossils, and in their prolongation form the base of a hill 1800 feet high, composed of alternating beds of sandstone and of sandy marl. This whole system is surmounted by great alternating masses of conglomerate, sandstone, and marl, forming a succession of parallel ridges in the country north of Gmunden; and still further towards the north, and in a higher part of the series, are beds of lignite.

2. *Section of Salzburg.*—Great parallel ridges of conglomerate and sandstone extend at the foot of the higher Alps, from the denudation of the Traun to that of the Salza. The conglomerates resting immediately on the older limestone, re-appear on the left bank of the

the river, and form a mural precipice on the S.W. side of the city of Salzburg. They are described in detail, and are shown to have originated in the mechanical degradation of the neighbouring chain; and having a high inclination which carries them under the micaceous sandstones of the northern plains, are, on that account, referred to the lower part of the tertiary system.

3. *Section from Untersberg to the plains N.E. of Reichenhall.*—The authors here give a short account of the great secondary system of Alpine limestone; and the Untersberg beds, which contain innumerable Hippurites, are shown to belong to the highest part of that series. Over the Untersberg beds, the section exhibits the following succession.

a. A great deposit of marl and marlstone, generally of a gray, but in some places of a red colour; containing a few fossils resembling those of the chalk formation.

b. Sandy, micaceous marls alternating with conglomerates and micaceous, calc-grit, with Nummulites. Subordinate to this system are red and variegated marls, with gypsum.

c. A system of beds composed of blue, micaceous slate-clay and greenish, micaceous sandstone.

d. A great succession of alternating masses of blueish, micaceous marl, slate-clay, sandstone, and conglomerate. Some of these upper marls contain beds of gypsum and fossils, resembling the suite of Gosau. The whole of the preceding series is succeeded towards the north by the tertiary, slaty, green sandstone of the plains.

As all the deposits above described are conformable to each other, there is a difficulty in drawing the precise line of demarcation between the secondary and tertiary formations: the authors (though not without some hesitation) place the nummulite-rock, which is associated with the lower gypseous marls, at the base of the tertiary group.

4. *Section from the Stauffenberg, through the Kachelstein and the Kressenberg, towards the plains of Bavaria.*—In this section the Stauffenberg and the Kachelstein belong to the outer zone of secondary Alpine limestone, which in this region is enormously dislocated, so that the subordinate beds are not only contorted and pitched up at high angles, but generally plunge in towards the axis of the chain. The Kressenberg rises to the height of 500 or 600 feet on the north side of the Kachelstein, and forms a gradual slope towards the northern plains. Its subordinate beds dip at high angles of elevation towards the south, those which are nearest the secondary ridges being inclined at 80°. This position gives the system of the Kressenberg the appearance of dipping under the secondary rocks, an appearance which the authors consider entirely deceptive, and for which they account by the intervention of a great fault. They consider the beds of the Kressenberg hills as tertiary; because, though inclined in the same general direction with the secondary mountains, they are not conformable to them; because they contain no Ammonites, Belemnites, or other secondary fossils; and, lastly, because they contain very many organic remains which characterise tertiary formations. The authors here

 refer

refer to the list of fossils derived by Count Munster from this locality, and they entirely coincide with the opinions which he has published respecting them. This tertiary system is almost entirely composed of sand and sandstone which, here and there, contain many particles of green earth, in some places resembling tertiary molasse, and in others not to be distinguished from secondary, green sand. Subordinate to this system are eleven beds of granular hydrate of iron (varying from five to seven feet in thickness), which are extensively worked.

After the details of the preceding section, the great derangements of the neighbouring Alpine chain are briefly noticed. It is shown that there is a double axis of elevation, one of which passes through the great calcareous zone; and along the line of the Inn produces a southern dip in the saliferous series, which seems to carry them under the older rocks but which on the contrary form the true central axis.

5. *Tertiary deposits in the Valley of the Inn.*—These were probably once of considerable extent, occupying a basin about twenty miles in length, but not more than three or four miles in its greatest breadth. They are now chiefly seen near Häring, where a bed of coal thirty-four feet in thickness is extensively worked by means of long horizontal levels, which traverse a great succession of strata. These beds are described in great detail, and are principally composed of fetid marls in various states of induration. The coal and overlying beds contain many land and fluviatile shells, and have at first sight the appearance of a great lacustrine formation. Some of the beds above the coal, contain innumerable impressions of well-preserved dicotyledonous and other plants, many of which are in the course of examination by M. Adolphe Brongniart. There are, however, several marine shells in the strata, which show that the sea ascended up this part of the valley of the Inn during the period of the Häring deposit. From the general character of these marine shells, some of which have been identified with those of the London clay, the authors are disposed to refer the whole deposit to an early part of the tertiary period.

6. *Sections of the tertiary formations of Bavaria.*—The authors first remark, that the line of demarcation between the secondary and tertiary groups, is generally well defined; but they also derive from this region several proofs that the tertiary seas ascended up the old valleys of the Alps a long way to the south of the average direction of this line. In proof of this they refer to some deposits in the valley of the Isar. They then describe in detail the sections between Füssen and Schöngau, in which an enormous succession of beds is laid bare on the banks of the Lech.

They afterwards describe the section of Nesselwang, in which the lowest strata of the tertiary series are of great thickness, and are raised against the side of the Alps in a vertical position. They remark that the tertiary system has here a coarser structure than in most parts of the range; that beds of conglomerate abound in the lower part of it; and that the beds of molasse and marl are entirely subordinate to them. Lastly, the authors remark no less than

than three or four distinct zones of coal or lignite, separated from each other by sedimentary deposits of enormous thickness; as some of these zones occur in the lower, and some in the higher parts of the tertiary group, they infer that the existence of lignite is, of itself, no general test of the age of a tertiary deposit.

7. *Section through the hills at the east end of the Lake of Constance.*—After making some remarks upon the great elevation of the tertiary formations in the south-western extremity of Bavaria, the authors proceed to describe the transverse section exhibited by the hills above Bregenz. They commence with a description of the unmmulite-rocks of Haslach, which are associated with, and form a prolongation of, the secondary system of the Stauffen and the Salzburg chain. They also refer the nummulite-rocks and marl-slate above Oberdorf to the same system, and compare them with the unmmulite-ironstone of Sonthofen. In consequence of the derangement of the strata, and the accumulations of transported materials, the first commencement of the tertiary beds is obscure; but they rise into hills of the elevation of about 2500 feet above the Lake of Constance, and mark the prolongation of the secondary series, on the northern extremity of a ridge called Rexberg, ten or twelve miles S. E. of Bregenz. The lower part of the tertiary system, is composed of green, micaceous sandstone, to which certain beds of conglomerate are subordinate, and it is described as perfectly identical with the great deposit of adjoining molasse which forms the base of the tertiary formations of Switzerland. This sandstone occupies the successive ridges which extend from the neighbourhood of Oberdorf to Bregenz. And, as in the greater part of this long range the beds are highly inclined and have an undeviating dip towards the north, their united thickness must be enormously great.—The authors afterwards describe, with many details, the great complex deposit of conglomerates alternating with greenish sandstone and variously coloured marls which constitute the upper tertiary group, and compose the whole mass of the mountain ridge extending northwards from Bregenz. This whole section is considered of importance, partly from the great scale upon which the formation is developed, and still more from its forming a connecting link between the tertiary deposits or molasse of Switzerland and those which are exhibited in the several sections described in this paper.

Finally, the authors give a short summary of the conclusions which seem to follow from the facts stated in the memoir.

1. The tertiary formations of Austria and Bavaria appear to have been formed in an ancient mediterranean sea, the limits of which may be in a considerable measure ascertained; and the great mechanical deposits above described seem to have originated in the gradual degradation of the Alpine chain, partly by the action of the sea on the flanks, and partly by the erosion of the torrents descending from the mountains, and carrying great masses of transported materials below the level of the waters.

2. In some instances the tertiary beds are unconformable to the Alpine

Alpine limestone; in other instances they are conformable. And there are beds which, both from their fossils and from their structure, seem to exhibit a connecting link between the secondary and tertiary formations.

3. The system above described contains three or four distinct zones of coal or lignite, with many thousand feet of conglomerate, sandstone and marl between each; beginning in the lower, and ending in the upper parts of the series.

4. These younger deposits have the same general relations to the older chain, as the subalpine tertiary formations of the north of Italy; from which it seems to follow that the northern and western basins of the Danube, and the tertiary basin of the subalpine and subapennine regions, must have been left dry at the same period. The conclusion is further confirmed by the suite of fossils in the adjoining molasse of Switzerland.

5. All the transverse sections prove the recent longitudinal elevation of the neighbouring chain. The tertiary beds form an inclined plane, down which the Alpine waters stream into the Danube in nearly undeviating lines, greatly contrasted with the sinuous channels through which the waters escape into the plains from the older rocks.

6. The authors endeavour to confirm the preceding conclusion by the facts exhibited in the drainage of the south of Bavaria. They state that the whole system of drainage, is in a state of continual change and of progress, and that the rivers have not yet worked for themselves any thing like permanent channels.

7. The authors lastly account for some of the greater denudations, by debacles which must have taken place during the elevation of the Alps, and by the bursting of a succession of lakes since that period. In confirmation of which, they state that there is not a single valley among the newer formations of southern Bavaria, in which may not be seen many parallel terraces (like the parallel roads of Scotland) indicating the residence of nearly stagnant water at several successive levels.

A paper On the discovery of the bones of the Iguanodon, and other large reptiles, in the Isle of Wight and Isle of Purbeck; by the Rev. William Buckland, D.D. V.P.G.S. F.R.S. &c. &c., was then read.

Hitherto the Iguanodon has been found only within the limits of the Weald of Sussex, where it was first discovered by Mr. Mantell, in the iron sandstone formation of Tilgate Forest. Dr. Buckland has recently ascertained the existence of this animal in two other localities of the same formation: one near Sandown Fort on the south coast of the Isle of Wight; the other in Swanwich Bay, at the eastern extremity of the Isle of Purbeck. In all these places its matrix is the same, ferruginous sandstone, to which the name of Wealden or Hastings sandstone, has been applied by recent observers in geology, being intermediate between the lowest beds of the green sand formation and the upper beds of the Purbeck limestone, and its fossil shells exhibiting such an admixture of marine

remains

remains with those of freshwater, as seems to indicate the former existence of a great estuary in the district wherein they have been deposited.

From the size of the bones of the Iguanodon, described by Mr. Mantell and Mr. Murchison*, it has been ascertained that this herbivorous reptile was of extraordinary magnitude; but a single bone of its foot has been lately found near Sandown Fort, which shows that its proportions probably exceeded those of the most gigantic quadruped yet discovered. The bone alluded to seems to be the external metacarpal bone of the right foot; it is twice as large as the corresponding bone of a large elephant; its length is six inches, its breadth at the upper extremity five inches, and its weight six pounds. A gigantic pelvis was also found in the same iron-sand at Sandown Fort. Among the bones discovered in the Isle of Purbeck by the Rev. J. C. Bartlett, the most remarkable are large vertebræ, and toe bones of the Iguanodon, in size and form resembling those engraved by Mr. Mantell from Tilgate Forest; there are also various bones of other species of reptiles; a fragment of a femur, resembling that of the Megalosaurus; bones of large and small Crocodiles, and of more than one species of Plesiosaurus. All these animals have been found by Mr. Mantell, similarly associated in the Hastings sandstone of Tilgate Forest. Dr. Fitton has ascertained the shells in this iron-sand at Swanwich and Sandown Fort to be identical with those of the same formation in the Weald†; and the addition of so many reptiles to the list of their common organic remains, affords still further evidence of the identity of the strata in which they occur.

ROYAL ACADEMY OF SCIENCES OF PARIS.

May 4th.—*Manuscripts received at this Sitting*:—A letter from M. Roulin On some earthquakes felt in America;—A memoir by M. Lamé On the motion of heat in polyhedrons;—A sealed packet from Dr. Gaillon;—A memoir by M. Girou de Buzareingues On the number of marriages, and of the births of the sexes in the several months;—A geological notice On several substances found in some trenches at Marseilles;—A practical and historical treatise On mental alienation in animals inferior to man, by M. Pierquin;—A memoir On the equilibrium and motion of fluids, by M. Cauchy.

M. Frederic Cuvier, in the name of a Commission, gave an account of the work presented by M. Villermé On distribution of conceptions and births of man in the several months. It appears, among other circumstances, from the observations of M. Villermé, that the months in which there are most births are in the following order;—February, March, January, April, November, and September, which gives the conceptions in the order of May, June, April, July, February, and March. The autumnal equinox is the period of the fewest conceptions. Few women conceive during the first week of their marriage. Years of scarcity and Lent produce similar effects upon conceptions.

* See Mantell, Tilgate Forest; and Geol. Trans. vol. ii. 2nd Series.

† See Annals of Philosophy, Nov. 1824.

M. Cordier

M. Cordier gave a favourable account of the efforts of the officers of La Chevrette, to contribute towards the progress of Geology;—M. Daussy read a memoir On the geographical situations of Cairo, of Alexandria, and some other points of the Mediterranean.

The Section of Rural Economy presented to a secret committee the following list of candidates for the vacant place of Correspondent: MM. John Sinclair, Gasparin, Bonafous, Baron de Voght, Crud, Bigot de Morogues, and Hartig.

May 11.—*Manuscripts*:—New memoirs On indelible inks, presented by the Keeper of the Seals;—A sealed packet by M. Vellot; —A letter On several physical subjects, by M. Dos Rios y Souza;—A letter from M. Baudelocque, On the methods of stopping uterine hemorrhages;—A memoir by M. Destrem, On the fossil bones of the caverns of Bize in Languedoc;—An anonymous sealed packet (presented by M. Dumeril) containing experiments which prove the radical cure of a certain disease.

M. D'Urville read an abridged account of his Voyage.

A Correspondent was elected by ballot. M. Gasparin had 26 votes, M. Sinclair 17, and M. Bonafous 5.

May 18.—*Manuscripts*:—Memoir On the integration of equations of partial difference;—A letter to M. Ternaux On the *Silos* which he had constructed at Saint Ouen;—A letter from M. Deleau On the place at which woods are formed;—A letter from Heurteloup On an elastic gum instrument for preventing the infiltration of the urine after the operation for the stone;—M. Dulong read a memoir On the specific heats of elastic fluids.

May 25.—*Manuscripts*:—Two memoirs by M. Suremain Misseri, On the mathematical theory of music;—A work by M. Coriolis On the theory of machines;—A note by M. Cottereau On a case of phthisis cured by chlorine;—A memoir by M. Sturm On the solution of numerical equations;—Algebraical researches by M. Evariste Galois.

M. Geoffroy Saint-Hilaire presented the portrait of a monster now existing at Turin; it consists of two girls, separate to the waist, and having only two legs.—M. Cuvier gave a very favourable report respecting the fourth and fifth Geological Memoirs addressed to the Academy during the voyage of the Astrolabe by MM. Quoy and Gamard.—M. Dumeril gave a favourable account of a memoir On monstrosities, presented by M. Le Sauvage, Professor at Caën.—M. Puissant gave a verbal account of the first number of the Topographical Atlas of the Department of Puy-de-Dôme, by M. Busset.—A memoir was read by M. Savart On the structure of metals; and one by M. Cauchy On the equilibrium and motion of fluids.

June 1.—*Manuscripts*:—A letter from M. Olbers, who returned thanks to the Academy for having elected him an Associate;—Description of the *cystotome suspenseur*, a new surgical instrument, by M. Tronchon;—Theory of vibrations, and its application to various physical phænomena, by M. Le Baron Blein;—A sealed packet by M. Amusset;—A memoir On the theory of equations, by M. Sturm; —A Theorem on the motion of heat in spheres, by the same;—Researches on algebraic equations of the first degree, by M. Gallois;—A letter

A letter from M. Virey On the spontaneous rise of the spider's web in the air;—M Navier read a memoir On the flow of elastic fluids in vessels and pipes;—M. Strauss read an extract from his work On the Anatomy of the Spider.

June 8.—*Manuscripts:*—A sealed packet from M. Ségalas;—A treatise On the employment of atmospheric air in diseases of the ear, by M. Deleau;—A memoir On a new method of finding the longitude, by M. Devoulx;—Two new demonstrations of the reality of the roots of transcendental equations, led to by several mathematico-physical questions;—A sealed packet by M. Cauchy;—A memoir On refractory crucibles, by M. Boyer, manufacturer of porcelain;—A memoir by M. Marcel de Serres, On a cavern of fossil bones in the Department of the Eastern Pyrenees.—M. Latreille, in the name of a Commission, made a very favourable report respecting the *Monographe des crustacés amphipodes,* by Mr. Milne Edwards.

M. Arago announced the irreparable loss which the Academy had sustained in the person of Sir H. Davy.

June 22.—*Manuscripts.*—A letter to Ariste On the theory of celestial crosses in general, and on the cross of Migné in particular, by M. Clos;—Designs executed by M. Sainson during the voyage of M. d'Urville;—Description of the apparatus belonging to moveable lighthouses, by M. Castéra;—A letter in which M. Dutrochet announces that he has discovered that a capillary column of water conducts electricity very badly.—The Academy learnt with grief the death of Dr. Thomas Young, one of the most illustrious of its members; and also that of M. Abel, a young geometer of Christiania of the greatest hopes.

M. Magendie announced that M. Leroux, an apothecary, had discovered in the willow two substances analogous to quina and cinchonia.—M. Navier, in the name of a Commission, gave a very favourable account of the new work of M. Coriolis, On the calculus of machines.—M. Cauchy, in the name of a Commission, reported respecting a manuscript treatise On the differential calculus, presented by M. Finck.—M. Elie de Beaumont read a memoir entitled, Researches on some of the revolutions of the surface of the globe; &c. &c.

June 29.—A memoir by M. Lionville On the analytical theory of heat;—Observations on the *Molluscæ,* by M. Audouin;—Memoir by M. Cristol, On the caverns of fossil bones lately discovered in the South;—A new method of destroying the stone in the bladder, by M. Rigaud de Jemigné;—A memoir on the same subject, by M. Dudon. The Academy afterwards heard a notice by M. Héricart de Thury, On a new well bored at Saint Ouen, by MM. Flachat;—Some remarks by M. Gay Lussac, in answer to the doubts expressed by the mayor of Carantau, On the placing of a conductor on the steeple of that city;—Some considerations by M. Du Petit-Thouars On the *Orchideæ* of the Isle of France;—A report by M. Cauchy, On a memoir by M Abel, of Christiania, respecting a general property of a very extensive class of transcendental functions;—And, lastly, A memoir by M. Lugol, On the employment of iodine in scrofula.

IX. *Intelligence and Miscellaneous Articles.*

DISTINCTIVE CHARACTERS OF TANNIN AND GALLIC ACID.

TO determine the different properties of these substances, M. Pfaff employed them of the greatest purity, and he obtained the following results. In a dilute solution of gold, gallic acid gives a blue greenish colour, which appears brown by reflected light, and the gold is perfectly reduced. Tannin merely reduces the gold to a lower state of oxidation, and the liquor becomes purple. Gallic acid a faint yellowish tint in the solutions of titanium. Tannin precipitates orange-red flocculi. Tannin precipitates tartarized antimony white, but gallic acid occasions only slight turbidness after a considerable time. Gallic acid renders the caustic alkalies brown; the colour which it produces with the carbonated alkalies is at first yellow, with a brownish tint, but it becomes soon of a deep green. Tannin is precipitated by the pure and carbonated alkalies, and the liquor becomes brown, without changing to green. The salts of morphia, strychnia, quina and cinchonia, are not precipitated by gallic acid, but they are by tannin. In its combination with the alkalies, tannin seems to undergo a change, which approximates it to gallic acid. The scum of coffee owes its property of turning the white of egg green, with the influence of the air, to the gallic acid which it contains; and the white of egg appears to produce this effect by the carbonate of soda which enters into its composition. M. Pfaff did not find any gallic acid in the plants which contain emetin and veratria.—*Schweigger's Annals, Journal de Pharmacie,* Aug. 1829.

PRECIPITATE OF SILVER RESEMBLING THE PURPLE POWDER OF CASSIUS.

M. Frick states that if a dilute solution of tin in nitric acid, prepared without heat, be mixed with a dilute solution of nitrate of silver, the solution after some minutes becomes yellow, afterwards brown, and eventually of a deep purple colour; if dilute sulphuric acid be added to it, a deep purple precipitate will be obtained, but which does not possess the property of colouring glass.—*Ibid.*

ON SOME PROPERTIES OF SILVER: BY M. WESLAR.

This author states, that chloride of silver which is coloured by light, is not, as has been generally admitted, a mixture of reduced silver and undecomposed chloride, but a new compound containing less chlorine than the common chloride of silver. M. Weslar calls it subchloride: one circumstance, among others, favourable to this opinion is, that the chloride of silver coloured by light is not acted upon by nitric acid. Ammonia and a solution of muriate of soda convert this subchloride into metallic silver and chloride. Subchloride of silver cannot be obtained free from chloride by exposing the latter to light; it may be procured by allowing silver to remain in a solution of muriate of copper or iron. It is owing to the production

tion of this subchloride that silver which contains copper is blackened by solution of muriate of ammonia.

If silver be put into a strong solution of common salt, the metal is attacked: after long contact the solution appears to be weakly alkaline, and by evaporation yields crystals formed of chloride of sodium and chloride of silver.

It is well known that a hot solution of sulphate of iron dissolves silver, and that the metal is precipitated as the solution cools. M. Weslar has found that the silver is not totally precipitated; and consequently that it is soluble at common temperatures in a solution of sulphate of iron, and more so as the sulphate is more acid. Dilute sulphuric acid does not act upon silver at common temperatures. In order that action may ensue, it is only necessary to add a drop of solution of sulphate of iron. To explain these facts, it must be supposed that the oxygen of the air is conveyed to the silver by the medium of the solution of iron; the iron of which at the moment that it yields oxygen to the silver, retakes it from the surrounding air.

The solution of chloride of silver in common salt is not decomposed by potash. This fact may be explained by the great affinity of the chlorine for the silver and the potassium for oxygen: it is undoubtedly owing to the same affinities, that the complete decomposition of chloride of sodium by oxide of silver is effected.—*Ibid.*

PREPARATION OF HYDRIODIC ÆTHER.

M. Serullas proposes the following process for obtaining this compound:

Put into a tubulated retort,

Iodine..........	40 parts.
Alcohol	100
Phosphorus	$2\frac{1}{2}$

The latter is to be added in small fragments, and the retort is to be shaken; the distillation is to be continued nearly to dryness; then stopping it add 25 to 30 parts of alcohol, continue the distillation, and cease at the same point as before.

Water being added to the product, the æther is immediately separated and sinks; it is to be washed in the usual way, and redistilled from some fragments of chloride of calcium.—*Ibid.*

RUSSIAN GOLD AND PLATINA.

The *Bibliotheque Universelle* for July last contains an extract from a Prussian Journal, from which it appears that the richest beds of platiniferous sand occur in the district of Tahil in the Uralian mountains. Last summer fresh beds were discovered in the western branch of the Uralian mountains. Banks of platiniferous sand, from $2\frac{1}{2}$ to 5 feet thick occur especially in the hollows, and they are covered with a bed of turf varying from a few inches to five feet in thickness; these banks are composed of common sand, and a grayish-green argillaceous sand. The last banks discovered near Tahil contained from 1 to 3 pounds of metal in about 3700 pounds of sand.

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This territory is also very rich in gold. According to the documents supplied by Professor Fuchs, from the commencement of the summer of 1823 to the month of August in the same year, 7792 workmen were employed in the gold washings of the Uralian mountains, and they procured 1460 pounds of pure gold from 225,000 times its weight of auriferous sand; the number of workmen was then increased to 11,500, and these in August and September obtained 1274 pounds of gold from about 320,000 times its weight of sand. To sum up, from the 1st of May to the 1st of October, 2824 pounds and some ounces of gold were procured from about 270,000 times its weight of sand, to which are to be added 27 pounds and some ounces, obtained before the 1st of June 1824.

Le Globe of the 21st of October last, states that MM Humboldt, Rose and Ehremberg, under the sanction of the Russian Government, had set out to inspect these mines, and also the gold mines of Borosowk, the malachite mines of Gumeselesski and Tagilsk. It is stated that the washings of the gold and platina mines yield annually 6000 kilogrammes, or about 12,000 English pounds of gold; and M. Humboldt was astonished to find masses of gold in these auriferous sands, weighing from 2 to 3 and even from 18 to 20 pounds each: they occur a few inches under the grass. From the 59th to the 60th degree of latitude, fossil elephants' teeth occur enveloped in this alluvial soil; this circumstance is regarded as a proof that the formation of these auriferous sands is very recent.

M. Humboldt observes that it is very remarkable, that in the middle and northern parts of the Uralian mountains, the platina is found in abundance only on the western and European slope, whilst the rich gold washings are on the Asiatic sides of the Bartiraya.

NEW PROCESS FOR PREPARING FORMIC ACID.

To procure formic acid, M. Wœhler recommends that a mixture of starch and peroxide of manganese should be put into a retort, sulphuric poured upon it and subjected to distillation: much carbonic acid is evolved, and an acid liquor passes over at the same time, the smell of which is very penetrating, and it irritates the eyes strongly; this is formic acid, rendered impure by a volatile matter, to which its strong smell is owing. When this liquor is saturated with an oxide, the smell ceases and the solution has a yellow tint. It is only necessary to distil the formiates thus obtained, with sulphuric acid, to obtain pure formic acid, and without any smell but that which it naturally has. The salts which even the impure acid forms with barytes, lime and lead, have precisely the same crystalline form as the same salts prepared with the native acid: the salt of lead was analysed; its acid was found to contain the same constituents as common formic acid.—*Hensman's Repertoire,* May 1829.

CHROMATE OF ZINC AS A PIGMENT.

M. Lampadius directs that chromate of zinc should be prepared by adding a solution of chromate of potash to one of sulphate or muriate of zinc. There is not at first any precipitation, but on adding more chromate,

chromate, a deep yellow coloured precipitate is formed. A yellow liquid remains, which being decomposed by solution of potash, also gives an abundant precipitate, but of a paler colour: these colours may be employed either in oil or with varnish.—*Ibid.*

GREEN LAKE PREPARED FROM RED CABBAGE.

M. Lampadius states, when a hot infusion of red cabbage is treated with a solution of acetate, or still better, subacetate of lead, a very fine deep grass green lake is obtained. Muriate of antimony with a similar infusion gives a rose red precipitate. Infusion of cabbage may be employed to dye cotton and linen of these colours, after having used acetate of lead and muriate of antimony as a mordant.—*Ibid.*

DECOMPOSITION OF SULPHURET OF CARBON BY ELECTRICITY.

M. Becquerel puts some sulphuret of carbon into a tube, under a solution of nitrate of copper of less specific gravity; a plate of copper is then placed in both liquids: these substances form a pile.

The carburet of sulphur is decomposed, as well as a portion of the nitrate of copper; many crystals of protoxide of copper are formed on the copper plate, and carbon in very small brilliant laminæ, and of a metallic appearance, is deposited on the sides of the tube.—*Annales de Chimie*, Sept. 1829.

DETECTION OF ADULTERATION IN CHROMATE OF POTASH.

M. Zuber states that this salt is frequently adulterated with muriate or sulphate of potash; and he proposes the following method of detecting them: Add a large excess of tartaric acid to the chromate of potash to be tried, the chromate will be decomposed, and acquire in about ten minutes a deep amethyst colour. It will now, if pure, form no precipitate with nitrate of barytes or silver, by which means the presence of muriate or sulphate of potash may be ascertained.—*Ibid.*

REMARKS ON ONE OF THE EXPERIMENTS FROM WHICH MR. RITCHIE HAS INFERRED THE INADEQUACY OF THE CHEMICAL THEORY OF GALVANISM.

In Mr. W. Ritchie's "Experimental Examination of the electric and chemical Theories of Galvanism," lately published in the second part of the Philosophical Transactions for the present year, it is stated towards the conclusion of the paper, (Phil. Trans. 1829, p. 365,) that "the following experiment is not only at variance with the theory of Dr. Wollaston, [which "assumes that positive electricity is set at liberty by the combination of oxygen with one of the metals" in a galvanic arrangement,] but seems also hostile to some of the generally received notions of chemists."

For this experiment a cylinder of copper is employed, (of which a figure is given in the Transactions,) about an inch in diameter, and two inches long, having a small copper tube soldered in one end, whilst

whilst the other end is left open. Within this is placed a small cylinder of zinc, having a copper wire soldered to the lower end. "The wire, being covered with a thread and passed through the tube, is firmly cemented with electric cement, metallic contact being carefully avoided. Another end having a strong brass tube with an internal screw is now soldered in the top of the copper cylinder. The interior surface of the cylinder of zinc is covered with electric cement to prevent the acid acting upon it. The whole is now nearly filled with water, and a little sulphuric acid is introduced into the zinc cylinder by means of a very slender glass funnel. The whole is now completely filled with water; and a solid screw dipped in electric cement, and screwed into the top of the brass tube, whilst it is heated, renders the whole completely air-tight. The acid is now to be mixed with the water by frequently inverting and shaking the cylinder. If the copper and zinc cylinders be connected with the galvanometer, the battery will continue to act for a day or two with the same energy as if the whole had been left exposed to the air. As there is no room for the disengagement of hydrogen, the oxygen of the water cannot combine with the zinc to convert it into an oxide; nevertheless chemical action goes on, and the zinc is dissolved in the acid. From this experiment it is obvious that the oxidation of the zinc and the combination of nascent hydrogen with the electric fluid, as Dr. Bostock supposes, has nothing to do with the production or transfer of the electricity which appears at the surface of the zinc. The metal is still, however, dissolved or reduced from a solid to a fluid state; and as its capacity for caloric has undergone a change, may not its capacity for the electric fluid have also undergone a certain change? Hence it is possible that the true theory of galvanism may be more intimately connected with that of latent heat than has yet been supposed. Since the zinc is dissolved without the assistance of oxygen from the water, it appears that the atoms of the acid have combined with the pure brilliant atoms of the metal, without the necessity of the metal being first converted to an oxide."

It will probably be considered by philosophers, however, that before conclusions of such importance as those which Mr. Ritchie has arrived at, can be drawn from any experiment, that experiment must be performed in an unexceptionable manner; with every attention to the condition and quantities of the substances made use of, to the phænomena manifested during its progress, and to the results in the production of which the action terminates. Whether such attention was bestowed upon the experiment in question, does not appear from the preceding account, as given in the Philosophical Transactions; but we are bound to believe that the author would not have impugned the theories on galvanism of Dr. Wollaston and Dr. Bostock, nor any of "the generally received notions of chemists," except upon what he deemed satisfactory grounds.

Mr. Ritchie's first inference is, that "as there is no room for the disengagement of hydrogen, the oxygen of the water cannot combine with the zinc to convert it into an oxide," while "nevertheless chemical action goes on, and the zinc is dissolved in the acid."

Now,

Now, with respect to there being no room for the disengagement of hydrogen, it remains for Mr. Ritchie to prove either that, under such circumstances, hydrogen requires "any room" for its evolution, or that it does not make room for itself during the experiment. It does not appear that he employed any means for ascertaining whether hydrogen was in reality evolved, or not; but that having filled the cylinder with water and sulphuric acid, he concluded that it could not possibly be given out. But the only obstacles to its disengagement would be the difficult compressibility of the water, and the comparative inaptitude of that fluid to dissolve the hydrogen. From the lately discovered facts, however, respecting the compressibility, even to liquefaction, of many of the gases, there are strong grounds for concluding, that, under such great pressure as the disengagement of hydrogen by the mutual action of the zinc, water, and sulphuric acid, in a close vessel, must itself produce, aided by some degree of solvent power in the water, the gas would become condensed in the water, while its oxygen combined with the zinc.

Supposing that muriatic acid and sulphuret of iron were to be inclosed in an air-tight vessel so as to fill it, it is certain, from Mr. Faraday's experiments on the liquefaction of the gases*, that on opening the vessel, if the requisite proportions had been employed, we should find that the sulphuret had been dissolved, and a solution of iron formed. And supposing that an opake vessel had been employed, through which the operation of the agents could not be observed, supposing also that no further information had been communicated respecting the experiment than Mr. Ritchie has given upon his, we might reason, that since there was "no room for the disengagement of hydrogen," therefore the sulphur and the iron must have dissolved in the muriatic acid, without oxidation or other previous change of condition; whereas the truth would be, that the hydrogen equivalent to the oxygen combined with the iron, would have become united with the sulphur, and remained in the solution in the form of liquid sulphuretted hydrogen until the pressure was removed by the opening of the vessel, when it would assume the gaseous state.

It seems expedient, therefore, that Mr. Ritchie should ascertain, in repeating the experiment, whether the hydrogen is not in reality dissolved in the solution of zinc found in the cylinder after the experiment. But possibly the hydrogen, or a portion of it, under pressure and electric influence, may have entered into combination with the copper, or with some of the constituents of the brass or of the cement. These bodies, therefore, it also seems proper to examine with the view of determining this question.

The statement that the dissolved zinc cannot be combined with oxygen in the solution, rests entirely on the assumption that the hydrogen of the water cannot be disengaged. But no account is given of the properties possessed by, or of the effects of reagents upon, this remarkable solution of zinc; for aught that appears in Mr. Ritchie's statement, the metal may exist in it in the form of oxide; and if so, the equivalent of hydrogen must have been liberated, and would be found

* See Phil. Trans. 1823; or Phil. Mag. vol. lxii. p. 418.

in one of the states just mentioned; and in this case the experiment as forming a ground of opposition to any of the theories mentioned, becomes altogether nugatory.

Mr R.'s representation, "that the atoms of the acid have combined with the pure brilliant atoms of the metal, without the necessity of the metal being first converted to an oxide," is at variance with all our knowledge respecting the action upon the metals of solvents containing oxygen.

It is true that some instances are on record of the simple solution of bodies in sulphuric acid, without their oxidation, and that one of these bodies, tellurium, is a metal for the oxide of which the acid has considerable affinity, combining with it to form a crystallizable salt. But in all those cases in which simple bodies are concerned, the acid must be employed in a concentrated state, in order that the effect may be produced; while water, very gradually added, as when the solutions attract it from the atmosphere, causes the oxidation of the substances dissolved; or if added quickly, it precipitates them in an uncombined state *. It would appear therefore that no result of this nature can take place in Mr. Ritchie's experiment, since the acid he employs becomes diluted as soon as the experiment commences. But if by possibility the case should be as he represents, a proper examination of the solution of zinc obtained will enable him to determine the fact.

If it should ultimately appear that the water is not decomposed in Mr. Ritchie's experiment while yet the zinc is dissolved, the solution can take place only, in all probability, by the sulphuric acid giving up to the zinc a portion of its oxygen; and becoming reduced to one of the other combinations of sulphur and oxygen; while the oxide of zinc so produced will exist in the solution in the form either of hyposulphate, sulphite, or hyposulphite of zinc. And whether such is the case or not, can readily be determined by examining the solution.

It would appear, therefore, that before this experiment can be received as conclusive evidence on the point for the determination of which it was instituted by the author, it must be shown that no hydrogen is extricated during the experiment, or enters into combination with the water or any other substance present; that the zinc is not in the state of oxide in the solution; and consequently, that no portion of the water, either of that introduced into the cylinder or of that chemically united with the acid employed, undergoes decomposition. To render it perfectly satisfactory, ascertained quantities of zinc, water, and acid, should be employed; it should be ascertained whether any, and, if any, how much, of the zinc remains unacted upon, and also whether the entire quantity of water as well as that of acid is reobtained, either as such, or as combined in any manner with the zinc, after the experiment; and further, whether hydrogen gas does not escape either during the experiment or on opening the copper cylinder in order to ascertain the solution of the zinc.

The above minute examination of Mr. Ritchie's experiment has been entered into, and every possible view taken of the results stated

* See Phil. Mag. and Annals, N. S. vol. iv. p. 68.

to

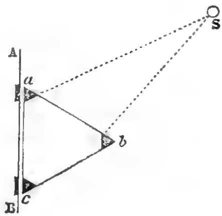
to have been obtained, including several very *improbable* alternatives, for the purpose of showing the necessity that exists for the repetition of the experiment, with the precautions suggested in these remarks, before any important conclusions can be founded upon it respecting the theory of galvanism.

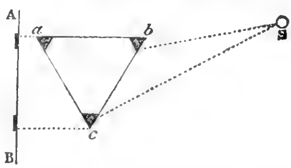
Dec. 28, 1829. E. W. B.

EXPERIMENTS ON LIGHT AND SHADOW MADE BY MEANS OF THE PRISM. BY DR. J. READE.

Exp. 1.—Having placed a piece of white paper close to one of the planes of an equilateral prism, on looking through it at the clouds the black angles of the prism reflecting their shadows were very apparent, as represented in the following figure:

A B, a sheet of white paper held close to and touching the plane of the prism *a c*. *a b c* three opake and dark angles forming black shadows, which passing through the plane fall on the paper A B. If we now hold a lighted candle behind the prism, these black shadows are immediately changed to blue at top, orange at the bottom, demonstrating in the most conclusive manner, that it is the black shadows of the prismatic angles which give the colours of the spectrum, and not any decomposition of the solar ray. If when the sun is shining we bring the prism with its attached paper into the rays, and then turn the instrument on its axis so as to bring the plane *a c* to an angle of 45°, with the paper A B, as thus represented, a reflected and not a refracted spectrum is formed. The black shadows from the angles *a* and *c* are passed through the prism and rarefied by the reflected light from the plane *b c* into orange at the bottom, blue at the top. On looking at the inside of the plane *b c*, we see the window reflected as soon as the coloured spectrum is formed; and, as I believe Mr. Brougham first remarked, if a pin or any other slender body be held in the spectrum, light-coloured shadows are seen. Now as every separate light forms its own shadow, we need no stronger proof that light is reflected from the plane *b c*; and as the light is reflected upwards, the spectrum ascends on the opposite wall.





J. Reade, M. D.

P.S. Since writing the above, I have made the following experiment: The sun shining on the slates of an opposite roof to my bed-chamber, I pulled down the blinds, leaving a triangular corner open. I now held a pencil before a sheet of white paper and saw three coloured

coloured shadows,—blue, yellow, and brown: on bringing a lighted candle near the blue, it was changed to a perfect lake; on bringing the blue to overlap the yellow, a green was formed.

Cork, August 26, 1829.

OCCULTATION OF ALDEBARAN, ON OCTOBER 15TH, 1829, OBSERVED BY DR. BURNEY.

In the evening of October 15th, about $9^h\ 6^m$ mean time, an occultation of Aldebaran by the moon was observed here; as the moon approached, Aldebaran became less ruddy, and when in contact it showed very little colour. After a perfect contact of the star with the moon's northern limb, at an angle of about 69 degrees from her vertex, it was *six seconds of time* clinging, to appearance, to her enlightened limb before it disappeared.

ON THE COPPER-COLOURED LIGHT REFLECTED FROM THE DARK PART OF THE MOON'S DISC. BY DR. BURNEY.

In the evenings of October 30th and 31st, 1829, the non-illuminated part of the moon's disc, when near the horizon, reflected a *dull copper colour;* a circumstance that often happens while the sun, or rather the earth, is passing through the southern signs of the ecliptic, but seldom if ever while passing through the northern signs. By considering the relative positions of the sun and moon with that of the earth, and the small angle subtended by the latter during the first four days of the moon's age, when the phænomenon is exhibited to the best advantage, the dull copper colour seen on the moon's opaque body in clear weather, particularly when near the horizon after sunset, appears to be effected by means of the solar rays reflected to the regions of the moon, from the extensive water in the Ethiopic Ocean and the Great South Sea, according as the earth advances in its annual motion round the sun: as it is well known the reflected solar rays from water are extremely bright, and as they proceed through a clear atmosphere in the direction of the moon, it is probable that they produce a faint light upon the dark part of her disc. In the opposite season of the year, when the sun's declination is north, there is more heat in this latitude, and consequently more vapours in the atmosphere, which intercept the incident and reflected solar rays, and do away their effect; nor is the moon's angular position in the heavens so convenient to receive them as in autumn and winter.

Cold in southern latitudes as far as 56 or 57 degrees, is said by late voyagers to be a mere chimera, and that snow is scarcely ever seen on the ground in these parallels, although reverse to nature in comparison of the low temperature and rigour of the winter in the same parallels in the northern hemisphere; therefore, snow on the ground in South America, or the Cape of Good Hope, or New Holland in any part of the year, cannot be the means of conveying the sun's reflected rays to the regions of the moon; nor is the snow in North America so situated during our winter, as to cause light from the

the earth to be reflected from the dark part of the moon's disc, as has been supposed by some modern philosophers.

The reflected light from the unilluminated part of the moon in her first quarter, as before mentioned, is sufficient to establish the fact of the existence of an attenuated lunar atmosphere, which, according to the ingenious calculations of the celebrated M. Schroeter of Lilienthal, cannot be much less than a mile and a half in height If the moon were a mere cinder, as some would have her to be, and without any atmosphere, is it possible that the light she receives from the earth could be reflected to it from the dark part of her disc?

LAW OF PATENT INVENTIONS.

Extracts from the Minutes of Evidence taken before a Select Committee of the House of Commons, appointed to inquire into the present State of the Law and Practice relative to the granting of Patents and Inventions. Session of 1829.

The Committee state in a report of a few lines (dated 12th June 1829), that they found the subject so intricate and important, that it occasioned the necessity of examining witnesses at great length; wherefore they were only prepared to report the minutes of the evidence taken before them, together with several documents: but they recommended to the house, that the inquiry should be resumed early in the next session.

These minutes of evidence occupy 160 folio pages, and the documents mentioned by the committee are contained in an Appendix of nearly 100 pages more. We have found it impracticable to condense into the few pages we can devote to this subject sufficiently copious extracts to convey to our readers the particular sentiments of each witness; hence we have preferred the evidence of those gentlemen who appear to have given the most important information to the committee. Amongst these are Davies Gilbert, Esq. M.P. President of the Royal Society, Arthur Howe Holdsworth, Esq. M. P., both members of the committee; Mr. John Taylor, Mine Agent, Mr. Brunel, Mr. Farey, and Mr. Clegg, Engineers; Arthur Aikin, Esq. Secretary to the Society of Arts, and others.

The evidence given by Mr. Farey is the most extensive; and as he furnished nearly all the documents contained in the Appendix, we shall for the present make extracts from his evidence and papers, proposing to continue the subject in a future Number.

Mr. John Farey, Engineer, called in and examined, 11th May.

Have you had considerable experience in the practice of taking out letters patent?—Yes, in assisting inventors professionally, to enable them to bring forward new inventions, and make them practically useful; in advising them how to secure their inventions by patent; in preparing titles and specifications for patents for new inventions; and assisting inventors to support their patent rights at law when disputed.

Can you state the expense of taking out a patent?—I can state the sum totals; for England, I think it will be found to amount to 120*l.*; for Scotland, perhaps 100*l.*; and for Ireland, I think more

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than 125*l.*; and there is a small increase if the patent for England includes the Colonies. Those sums include an average of ingrossing and stamps for the specifications.

Does the charge vary with the length of the specification?—With the length and with the difficulty; but the charges that depend most upon its difficulty are not included in the above sums, because professional charges for advice and assistance in bringing the invention to bear, and specifying it, vary in every degree.

What time does it take to obtain a patent?—It is said to be six weeks; but they are certainly now two months upon the average, and that is frequently extended to a much longer period.

During the time between making the application, and sealing the patent, has the applicant any security for his invention?—No security whatever; there is even an increased necessity for secrecy beyond that which existed before his application; because his application has called attention to his procedure, and declared what is the object of his pursuit. It is a common practice with manufacturers, who begin an invention solely for their own use, without any thought of a patent, when they have obtained such a prospect of advantageous results as to see that a patent would be desirable, they destroy all the models, and every vestige of them, and even send away the workmen who made them on some distant embassy, to avoid any chance of the secret being called forth by the competition that exists among rival traders as soon as one makes an application for a patent. It frequently happens that patents are delayed very long in their progress through the offices, so as to occasion a very great grievance.

What constitutes such a publication during the interval between the application for the patent, and the sealing, as to vitiate the patent?—That has never been decided with precision. It is supposed that an invention being communicated by the patentee to any person whose assistance is necessary to carrying on the invention towards perfecting it, would not be considered as a publication. If those persons were to communicate it at second hand, I apprehend that such breaches of trust would be considered as publications; but it has never come before a court in such a way as to give the positive opinions of the court on the subject. Whenever an invention, or a new article produced by an invention, has passed from one person to another by sale, before the date of a patent, the patent cannot be maintained.

Then the objects of keeping an invention secret after it is determined to take a patent are, lest the invention should be pirated; and lest there should be such an act of publication as to vitiate the patent?—And also the risk of calling up an opposition to the grant of the patent; because the instant that a man, by any means, announces to his competitors in trade that he is engaged upon a new invention, they are all upon the watch to find out what it is.

Do you not word the title obscurely, in order to avoid directing public attention to the subject?—Yes, but there is a danger in being too obscure, because then a court of justice may afterwards hold

hold that it is an invalid patent, for want of coincidence between its title and the specification. To prepare a title to a patent, it must not be made so clear as to call the attention of rivals, and enable them to discover the subject, nor so obscure as to incur the danger that a court of justice may afterwards rule, that it is an imperfect definition or title of the invention described in the specification.

Do you see any remedy for that inconvenience?—The remedy is obvious: to make the right of the patentee secure from the time he makes his application; on condition of his then lodging a paper of the heads of his invention—a statement of the principle on which he founds his invention; also the final specification and description of the means of executing the invention, should be engrossed in the patent itself, so that the title of the patent, instead of being the only means of reference between the two documents, should become a mere indorsement, and a matter of no importance: the latter is done in France.

If a person having made an invention of the same nature as that for which a patent is sought, and has carried it on secretly, is it supposed that the previous secret exercise of the invention will vitiate the patent?—It is decided that the secret exercise of an invention would not vitiate the patent; but it is assumed that the person so carrying it on secretly, would inevitably reveal the secret the moment that he knew that a patent was applied for, and a disclosure of the secret by him, in the interim between the date of the application and the date of the patent, would vitiate the patent; that is the reason for leaving the patent open to destruction by a publication of the invention, between the date of application and the date of the patent; viz. that those persons who have been previously secretly practising the same invention, may have an opportunity of publishing it, so as to destroy the patent. But publications from the fear of an expected patent rarely take place; because, when a person applies for a patent, he does not declare himself so clearly by the title, that the person could be certain that it is identical with the previous secret practice.

Is it supposed that the exclusive right given to the patentee will be valid against another party previously exercising the same invention secretly?—It is generally assumed to be so, but I am not aware of any decision upon that point, and it could scarcely come to the test, because the same secrecy that would enable him to have hitherto concealed his process, would also prevent any legal proofs of his having infringed the patent; it is exceedingly difficult to obtain sufficient proof of infringement. Many patents are infringed for years together, without it being possible to obtain any redress, from the difficulty of proving the exercise of the identical invention.

Will you explain what protection it is to a person applying for a patent to lodge a caveat?—In general, it is no protection whatever. If a patentee, after having disclosed his invention to some agent, or associate, or patron, before applying for a patent, has a suspicion that such person might be applying for a patent himself, at an earlier period

period than he (the inventor) was ready with his application, then by lodging a caveat, he would have notice and detect the treachery, before it was too late. A patent cannot pass through the office where a caveat has been lodged, without notice being first given to the person who lodges the caveat.

Supposing he opposes the patent, what steps does he then take?—He may cause the Attorney General to summon the parties, and after examining the respective inventions, he will decide whether they are so dissimilar that the patent is to pass, or not.

Suppose there are two applications for a patent, and that the Attorney General informs the parties that the inventions are the same, and awards the patent to the first applicant, is it not in the power of the rejected applicant, on receiving that information, to effect such a publication as will destroy the patent right?—It is in his power; but I never knew an instance where the Attorney General did declare that there was such a similarity and equality of rights to a secret invention, as to induce him to refuse granting a patent; he has sometimes advised the parties to join their interests in one patent, when he felt difficulty in deciding, and has informed them that by contending at law, their patents, if he allowed them both to pass, would probably destroy each other.

Is the Attorney General the sole judge between two patent applicants?—He is; he summons the parties before him; they bring their drawings and models, engineers, witnesses, attorneys or agents, to explain; and sometimes, if it is requested, he will go and visit the machinery. Each party has a separate private hearing.

Does not the sort of decision he is called upon to pronounce, require considerable knowledge of mechanics?—It requires a very deep knowledge, to form a decision between the merits of the respective inventions, particularly as the inventions at the period when they must necessarily be exhibited to the Attorney General, are seldom organized in that state of perfection which will enable him to judge by results only.

Is it not an inconvenience that a question between two concurrent applicants should be decided by a person, perhaps, not very competent to decide such a question?—It is not felt an inconvenience, from the circumstance that the Attorney General almost always sees sufficient ground of new invention, to grant the patent, or both, if two are applied for; therefore the oppositions become of no effect. Patents are at all times subject to be annulled by a court of justice, if not found to be good in every respect, and the Attorney General recommends the King to grant the patent almost uniformly; if there are two applications for patents for similar inventions, he allows both patents to pass. When there would be a chance on very close inquiry, by competent judges, of finding collision, it is usual to advise the parties to join in a patent, but nothing more.

When the patentee comes before the Attorney General, he has to give a general description of the nature of the invention?—In case of opposition, but not without; it is a great evil that the

the applicant is no way fixed to what he does intend to specify. There have been some speculators who the instant that they find out that a person of talent is occupied with an invention, apply for a patent, with a title sufficiently general to cover the invention; and having thus got the start of the inventor, if they can get at the invention before the time of specifying, they have a good chance of making it their own; for even if the real inventor makes opposition, the Attorney General can rarely be convinced of the identity of the same invention, when differently explained by two parties, with different models or drawings, and when one party has an interest in disguising the similarity; or that party may purposely describe quite a different invention to the Attorney General, who keeps no record; hence, after having either from ignorance of the real invention, or from design, made a fictitious explanation to the Attorney General, he may get the real invention by treachery, and put it into his specification.

What remedy would you propose for that?—To fix every applicant, as is done in France and America, by some specification in the first instance, containing a definition of the principle and object of the invention, leaving the patentee afterwards to prepare a more complete specification when he had organized, and proved the means by which the invention is to be carried into effect; that final specification should be examined, and ought only to include such matters as are, in the opinion of competent examiners, a fair extension of his original idea, as expressed in the first deposit. As it is now, the patentee is not obliged in any way to declare what his invention is, till he puts his specification to the patent previously granted to him, and in the meantime he may change his plan within all the latitude that the generality of the title will permit.

Would you advise that a person applying for a patent should have his specification ready at the time of his first application?—Not his complete specification; that is done in other countries, but would be impracticable here; because the spirit of rivalry and competition is so strong, that an invention cannot be put to the test of experiment, or brought to bear in secrecy, so as to enable an inventor to defer his application until he is prepared to specify properly: that may be done in foreign countries, where secrecy can be more readily preserved, but not here. The specification, with all the details of description necessary for instructing the public how to practise the invention, cannot be made till the invention has been actually practised. I recommend that the inventor should state all the particulars necessary to define the principle, and explain the outline of his invention; so that when the complete or real specification is afterwards made out, it can be decided by competent judges, whether it is a fair development of the ideas originally recorded in the provisional definition.

You would compel him, on applying for his patent, to lodge a statement of the principles to be afterwards developed in the specification, and you would require that when the time has arrived for the inrolment of the specification, and consequently granting the patent,

patent, he should give the details, to be transcribed into the patent itself?—Yes; the knowledge of those details does not always exist at the time most proper for applying for the patent, and therefore it is impossible that they should be described, nor will they come into existence, until the thing has been put into practice; as it is now, the two months which elapse between the time of applying for the patent, and the time when the patent is granted, are lost, as to any such creation of the means of putting the invention into practice.

Would there not be a difficulty in securing to any one, a property in what was not distinctly defined?—For a permanency it would be an excessive difficulty; but for the short term of two or three months, it would not be any inconvenience to give a right, that is completely defined by the outline of a specification. Every patent now granted, is in force for two, four, six, and some for eighteen months, before there is any definition whatever, of the right it confers; for no record at all is now given in the first instance, nor is any writing usually preserved, in case it is explained to the Attorney General, upon hearing of opposition.

You would require this greater precision in the meaning of the first application, not for the benefit of the applicant, but for the benefit of other inventors?—For the benefit of all inventors who are occupied at the same time, on the same subject; it is an intolerable nuisance to persons who are engaged in speculations, that they are perpetually in danger of having their inventions or improvements stolen from them, and put into the specifications of some existing patents, not yet specified, but which have titles that will cover their inventions.

Are the inventors whose ideas have been so appropriated by previous patentees, called upon to prove the negative, that the patentee did not invent what he specifies?—Yes; or they must prove that they had the invention in use before the date of the patent; because the invention became the property of the patentee from that date, although he is not called upon to declare what it was for a long time after.

Is there any limiting law to the time which is allowed for the specification after the issue of the patent?—None by law; by custom it is two months for a patent for England; for England and Scotland, four months; and for England, Scotland and Ireland, six months; of late years, on declaring in the first instance that they mean to proceed with patents for England, Scotland, and Ireland, they obtain six months at once in the English patent. It is quite discretionary in the Crown how long time should be allowed, and on special showing, longer terms are occasionally granted.

Do you see any inconvenience in allowing a long time for the specification?—It is essential to getting sufficient specifications made of important inventions, to allow a long time; and on the other hand, it is an excessive grievance to persons engaged in like pursuits, unless some provisionary definition of the legal right that is conferred by the patent, were to be made public.

Does

Does not it often happen, that after a person has taken out a patent for an invention, he makes some material improvement in the invention which at present requires another patent?—It is a very common case for the same invention to require two, three, and four successive patents; and it is a very great hardship, it operates prejudicially to the public as well as to the inventor. In France, as improvements arise, successive specifications of those improvements can be added (at very little expense) to the original specification.

Would not that be very advantageous?—Very advantageous indeed; here there is no remedy for a defective specification. A patentee is compelled to specify his invention within the given period; and if he is not then prepared to specify his invention, with all its details of execution, in a perfect manner, his patent right must take the chance of his imperfect specification. If he afterwards practises the invention in a better manner than that which he specified, instead of its being held that he is deserving of public approbation, for having pursued his course of invention further than at first, the courts of law assume that he has committed a fraud, by concealing something which he ought to have put into the specification. He can only refute that imputation by proving in evidence, that the specification does contain sufficient instructions to enable the public to exercise the invention with real advantage. It is impossible for him to prove the negative, that he did not know the improvement at the time of specifying. When a man invents and takes out a patent for a machine, he must use his utmost exertion to get it made, and put to work, before the time when the specification is due, in order to make a trial of it, and regulate his specification thereby: perhaps some part fails, or requires to be re-made, and prevents his making any trial; but the time being come, he must make his specification as well as he and his advisers can guess, though he has gone through nearly all the trouble and expense of a trial. A few days after having inrolled, he may find out, upon experiment, some important improvement in the means of carrying his invention into effect, which either had not occurred to him before, or if he had thought of it, he could not have safely put it into the specification, because it was a mere speculative idea. If he had put in that speculative idea, and it had turned out on subsequent trial to be wrong, it would be said in a court of law that it was done to mislead the public from the real invention, which he reserved for his own private practice.

In that case the inconvenience arises from want of time?—Yes; and the expense of those hurried proceedings, to get a sufficient trial of new machines to enable us to specify properly is excessive, being frequently obliged to keep people working night and day. I have sat up all night many times myself, for such work, and have undergone such fatigue, that I could not be any way sure of what I was doing. Even when a successful trial has been accomplished, there remains so little time afterwards, that the specification must be composed in such haste as to run the greatest risk of some inaccuracy or error.

Supposing a specification to be so accurately drawn as to be sufficient to inform the public how to use the invention, and subsequently to that, the inventor discovered some improvement in his invention, his patent is not forfeited by his using that improvement?—No, not by law; but he must prove that his original invention would answer the purpose proposed, and be a useful and beneficial practice, and the fact of his departing from it, is presumptive against him; therefore, when the right comes to be tried in a court of law, the inquiry does not turn upon the real patent machinery that is in actual use, doing business and public good, but it is often necessary to make old-fashioned and obsolete machines that were described in the specification when no better known, but which have been superseded by better ones, and are of no use except to satisfy a court that what was specified will really do; and if by such evidence the court can be persuaded that they will do, then, however inferior they may be to the more recent editions of the invention (which are never examined), the patent escapes from being set aside for want of sufficient description. If the opposite parties can persuade the court that the machines described will not answer, then the patent is set aside, without any inquiry into the real merits of the invention in its modern form, which is in daily use, and is the real subject of the action.

Is it not quite fair that the patentee should be bound to give such a specification?—Unquestionably; but when an inventor's patent is set aside because he has not fully described his invention, it ought to be on the ground that the secret has been withheld, so that the public are really not in possession of it, and have consequently not derived the benefit of such possession. Instead of making ridiculous inquiries whether an obsolete specification is so defective as to destroy the patent, it should be amended by a new one, corresponding with the improved state of the practice. On the other hand, if the public are really in possession of the invention, and deriving benefit from its exercise, the patent ought not to be set aside, because they did not become possessed of it by means of the old specification. That remedy for a bad specification is merely penal, and the public have no advantage from it whatever; they do not get any more complete specification by annulling the patent. If the patentee were to be compelled to bring a better specification, there would be a real advantage; and if he refused to do so, then the present penalty of forfeiture would be very properly applied.

Do you think that the public would have any security against those imperfect specifications, by the appointment of a commission to examine the specification before it was inrolled?—It would be very easy to have specifications examined and verified either by a competent commission, or by suitable referees. The courts of justice now trust to the examination and opinion of others, but they do so in an improper manner, because it is by parties, brought by interested individuals, and when it is too late to amend any defects. It is quite a branch of my business, where there are any disputes upon patents, to examine and speak to the precision or defects of the

the specification; where there is any doubt about it, I always have machines made in exact conformity with the drawings, and put them to the actual test.

What should you think of the appointment of a commission authorized to examine specifications, and to certify whether they were sufficient to enable the invention to be used by the public?—I think that a specification ought not to be inrolled at all, till it is made sufficient; and that there should be no further inquiry about the sufficiency of the description, except by way of appeal against the examiners; one-tenth of the trouble and expense that is now incurred, to find out whether it is sufficient or not, when it is too late to make any remedy, would have made it sure at first.

Do you think a commission would be a proper mode of determining that sufficiency?—I think a commission well constituted would determine that and other points very well; but I think it would be very objectionable that any previous examination should take place, as to the merit of inventions; because it is impossible to foresee which will, by future cultivation, grow up to maturity, and which will not be worth such cultivation; hence every one should be allowed a fair chance. Any competent person can say whether a specification is intelligible or not; and if it is not sufficient, the inventor should be called upon to make it sufficient: if he proves that having used reasonable diligence he has not had time to do so, more time should be allowed than was at first granted. If he makes improvements afterwards, he should be called upon to inrol them, so as to keep the records of the Patent Office a correct transcript of his operations.

You would propose to secure to the applicant, from the time of his application, his right to the principle of his invention?—Yes, to the invention of which he details the heads; and all such fair development of those heads as he is prepared to specify completely how to practise them, at the time when his specification is due, or with such extension of that time as is reasonable.

So that before his patent is issued, he should be required to inrol his specification?—Yes, in order that the specification may be transcribed in the patent itself, having been first approved to be sufficient, and if found deficient, or not confined within the limits of the heads first lodged, it should be amended.

By a record of the heads of an invention, could you so secure to the applicant his invention, as to leave him at sufficient liberty to pursue his invention, for the purpose of making a complete specification?—I think he ought to be bound to be able to define his principle very accurately at his first application, because he ought not to be allowed even to apply for a patent, when his invention is a mere vision; he ought not to come until he has done all that can be done mentally by himself, and by projection on paper, so as to be fully prepared to state all the principles or heads of his invention, leaving nothing remaining to be done but what requires to be decided by experiment.

In the experiments which it would be necessary to make, in order to perfect the specification, would he not run great risk of divulging his invention, although he had entered the principal heads?—Those heads should be kept secret, but public notice of the application should be given; one object of my proposition is, that if an accidental disclosure of the whole secret did take place, the inventor should not suffer any material injury from it.

Will you instance, in the case of Mr. Watt's improvement in the steam engine, the proposals which you suggest for the improvement of the process in obtaining a patent?—I think that the deed which Mr. Watt inrolled for his specification, ought to have been lodged at the time of making the first application for his patent; and he should have been allowed at least two years for making engines and the experiments necessary for specifying the means by which those principles should be carried into execution; within those two years he should have made such a specification as would have really instructed competent workmen how to practise the invention.

Could Mr. Watt have made that statement of heads of invention, without previous experiments?—Yes, at the time he drew up those heads (which I say are not specific enough to be a specification,) he really had made no engine, and only a private experiment by himself, with a very incomplete model.

Would those heads have secured him against any rivalry?—They did so in fact most completely; his paper was most admirably well drawn, and very definite; but those who wanted to practise the invention could not do it upon that specification. It told them plain enough what they were forbidden to do, during the term of his patent, but did not explain how they might do it, after the expiration of that term.

Then he gave in for a complete specification, that which, according to your ideas, ought to have been given in in the first instance?—Exactly so; and he never did make any complete specification such as I think ought to be given in the second instance; the consequences of that omission have been important in his case, for long after the expiration of his patent, which was prolonged and kept in force in the whole for more than thirty years, those who wanted to make steam engines, had to go and steal a knowledge of his invention from his factory, or from examining engines made by him, with as much difficulty as if he had never had a patent. Reading the specification did not answer the purpose at all.

[To be continued.]

LIST OF NEW PATENTS.

To J. Aitchison, Clyde-buildings, Glasgow, merchant, for his improvements in the concentrating and evaporating of cane juice, solutions of sugar, and other fluids.—Dated the 15th of September 1829.—6 months allowed to enrol specification.

To T. Cobb, Calthorpe-house, Bradbury, Oxford, esquire, for his improvements in the manufacture of paper, intended to be applied to the covering of walls, or the hanging of rooms, and in the apparatus for effecting the same.—15th of September.—6 months.

To

To T. Westwood, of Princes-street, Leicester-square, Middlesex, watchmaker, for his improvements in watches and time-keepers.—23rd of September.—6 months.

To I. Brown, Gloucester-street, Clerkenwell, watchmaker, for his improvements applicable to watches and other horological machines. 23rd of September.—2 months.

To H. Tyler, Warwick-lane, brass-founder, for his improvements in the construction of water-closet.—23rd of September.—2 months.

OBITUARY.—M. NIELS HENRIK ABEL.

The mathematical sciences have sustained a great loss in the premature death of M. Abel, whose brilliant discoveries, when quite young, raised the highest expectations of the fruits of his maturer years. Although his labours are but partially known in this country, we hope that a short account of his life will not be unacceptable to our readers. Niels Henrik Abel was born on the 25th of August 1802, at Frindöe, in the province of Christiansand, on the western coast of Norway, where his father was a clergyman. He showed at first no marks of genius; but at the age of 16, being then at the public school of Christiania, his extraordinary talent for mathematics at once began to develop itself, and he rapidly studied Euler's Introduction to Analysis, his Differential and Integral Calculus, the works of Lacroix, Francœur, Poisson, Gauss, and especially those of La Grange. He next entered the University of the same city. Having lost his father, and being without fortune, he availed himself of the assistance usually granted there to the poorer students; and, besides, had afterwards an allowance conferred on him by the Government. In 1820 he published his first paper, intitled " A general method of finding functions of a variable quantity, a property of these functions being expressed by an equation between two variable quantities." Some time after he imagined he had succeeded in finding the general solution of equations of the fifth degree. Having perceived his error, he resolved not to desist until he had either accomplished that solution, or demonstrated the impossibility of the general solution of equations of a higher degree than the fourth. In the latter task he succeeded: his paper was printed in 1824, at Christiania, in the French language. At the recommenda-ion of some Professors of Christiania, he now obtained from the Government an allowance for two years, in order to prosecute his studies abroad. Having spent the allotted time principally at Berlin and Paris, he returned to Christiania. During his absence from his country he published some excellent papers, among which those on Elliptic Functions, which have been honoured with the highest praise by the distinguished veteran Le Gendre, the discoverer of this branch of analysis. It is well known that at the same time, and unknown to him, another young mathematician, Professor Jacobi of Königsberg, who has just published an elaborate work, intitled " Fundamenta Nova Theoriæ Functionum Ellipticarum: Regiomonti, 1829," began to cultivate with the greatest success the same abstruse part of mathematical analysis. After his return to Christiania) M. Abel had at first no regular appointment; and only a short time before his

his death he began to receive a fixed salary. Unfortunately, his assiduous labours, and the anxiety of mind caused by the uncertainty of his prospects, had undermined his delicate health; and his short career was suddenly terminated on the 6th of April 1829, in a village near Arendahl, where he was on a visit to some of his relations. A very acceptable offer, made to him by the Prussian Government, of a Professorship in the University of Berlin, reached Christiania a few days after his death.

M. VAUQUELIN.

M. Vauquelin, the celebrated chemist, died lately at the house of M. Duhamel, in the same commune as that in which he was born, after a severe illness of a few weeks. This learned, scientific, and good man, carries universal regret with him to the tomb.—*Journal des Debats.*

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1829.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·44. Nov. 18. Wind N.—Min. 29·55 Nov. 24. Wind N.E.
Range of the mercury 0·89.
Mean barometrical pressure for the month .... 30·021
Spaces described by the rising and falling of the mercury............ 4·840
Greatest variation in 24 hours 0·660.—Number of changes 21.
Therm. Max. 58° Nov. 12. Wind W.—Min. 30° Nov. 16. Wind N.E.
Range 28°.—Mean temp. of exter. air 43°·70. For 30 days with ☉ in ♏ 45·53
Max. var. in 24 hours 20°·00—Mean temp. of spring-water at 8 A.M. 53·24

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere in the evening of the 4th ... 98°
Greatest dryness of the atmosphere in the afternoon of the 1st ... 52
Range of the index 46
Mean at 2 P.M. 71°·7.—Mean at 8 A.M. 78°·1.—Mean at 8 P.M. 78·5
—— of three observations each day at 8, 2, and 8 o'clock .... 76·1
Evaporation for the month 1·00 inch.
Rain in the pluviameter near the ground 1·67 inch.
Prevailing wind, N.E.

Summary of the Weather.

A clear sky, 4½; fine, with various modifications of clouds, 6; an overcast sky without rain, 15; foggy, ½; rain and snow, 4.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
11	3	27	1	9	8	11

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	9½	2½	1	1½	3½	3	6	30

General Observations.—The first part of this month was generally mild and fine; the latter part was cloudy and humid, with some light rain, frequent strong gales of wind, and comparatively cold for the season.

In the morning of the 1st the ice was one-sixth of an inch thick on the ground, and icy efflorescences appeared on the inside of the windows, being

ing the first time this autumn. There was a difference of 17 degrees between the maximum temperature of the 12th and 25th, and a difference of 22 degrees between the minimum temperature of the 12th and 16th!

A great depression of the mercury in the barometer took place on the 21st and 22nd, with a fresh wind from the S.E., succeeded by a S.W. wind. On the 23rd a very heavy gale set in from the N.E., and occasioned much damage among the ships and vessels in the Downs, and along the eastern shore of the British Channel on the 24th: it brought on snow here, which continued to fall throughout the night and most of the following day, when there was no variation of temperature at the ground from the freezing point, and the depth was three inches. This was unusually early for the appearance of snow in the immediate vicinity of the sea, as we seldom see any lie on the ground here before Christmas in the coldest winters. The bending aspect of the trees and shrubs, which were heavily clad with a snowy garment, contrasted with the long transparent icicles that were pendent from the roofs of the houses, had a magnificent, yet novel appearance in November: a rising temperature, with wind and rain in the night, dissolved nearly all the snow on the level ground by the morning of the 26th.

More favourable weather for getting the seed into the ground last month and the early part of this, could not have been desired; and even that depth of snow, from its containing and absorbing so much oxygen, has served as the best compost to the neighbouring lands, and softened the ground surprisingly.

The mean temperature of the external air this month is three degrees and three quarters lower than the mean of November for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are, two meteors, one rainbow, and nine gales of wind, or days on which they have prevailed; namely, two from the North, three from the North-east, two from the East, one from the South-west, and one from the West.

REMARKS.

London.—November 1, 2. Fine. 3. Cloudy. 4. Stormy and wet. 5—9. Very fine. 10. Stormy and wet. 11. Foggy morning: wet. 12, 13. Drizzly. 14. Cloudy. 15. Slight rain in morning: fine. 16, 17. Clear and frosty. 18. Cloudy. 19, 20. Dense fog; so much so in the evenings, that the coaches to and from London were obliged to be guided by torch-light. Some of the mails were, in consequence, an hour behind their usual time of passing Turnham-Green. 21. Dense fog in morning: cloudy. 22. Stormy and wet: 23. Stormy, but fair. 24. Cold and stormy: heavy gale at night accompanied with snow. 25. Snow on the ground to the depth of three inches: cloudy. 26. Drizzly: snow gone. 27, 28. Foggy. 29. Drizzly. 30. Cloudy.

Penzance.—November 1, 2. Clear. 3. Fair: rain. 4. Rain. 5. Fair. 6, 7. Misty. 8. Fair. 9. Clear: fair. 10. Rain: fair. 11. Fair: misty rain. 12, 13. Misty. 14. Fair. 15. Showers. 16—18. Clear. 19. Showers. 20, 21. Clear. 22. Showers. 23, 24. Fair. 25. Rain: sleet hail. 26. Rain. fair. 27, 28. Misty. 29. 30. Fair.

Boston.—November 1—3. Fine. 4. Cloudy: rain at night. 5. Fine. 6, 7. Fine: rain at night. 8. Fine: rain at night. 9. Fine. 10. Cloudy: rain P.M. 11. Fine. 12. Rain. 13, 14. Cloudy. 15. Fine: rain A.M. and P.M. 16. Fine. 17. Cloudy: rain P.M. 18—21. Fine. 22. Cloudy: snow A.M. and rain P.M. 23. Cloudy. 24. Rain and stormy: showers of sleet, hail, and rain during the day. 25. Snow and stormy: snow and rain during the day. 26. Cloudy: Rain A.M. 27, 28. Cloudy. 29. Foggy. 30. Cloudy.

Meteoro-

Meteorological Observations made by Mr. BOOTH at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1829.		Barometer.							Thermometer.							Wind.				Evap.	Rain.			
		London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
		Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Nov. 1		30·217	30·207	30·25	30·25	30·23	30·22	29·72	46	28	50	38	44	31	36	N.	NE.	NW.	N.	...	...	...	...	...
2		30·273	30·233	30·20	30·18	30·29	30·25	29·75	54	35	52	43	47	39	42	SW.	N.	NW.	NW.	...	...	...	...	...
3		30·227	30·102	30·15	30·10	30·27	30·18	29·70	49	42	54	45	54	46	41·5	W.	SW.	SW.	calm	0·10	...	...	0·070	...
4	☽	29·843	29·677	29·75	29·74	29·87	29·70	29·34	51	35	54	47	53	38	45	S.	SW.	S.	NW.	...	0·34	0·720	·560	...
5		30·063	29·777	30·15	30·15	30·10	30·00	29·42	56	42	55	45	50	47	43	SW.	W.	NW.	NW.	...	...	...	...	0·02
6		29.986	29·898	30·05	30·00	30·05	29·98	29·43	56	37	53	46	54	45	46	SW.	W.	SW.	NW.	·10	...	...	...	·03
7		30·025	29·930	29·95	29·95	30·00	29·97	29·41	54	29	52	48	52	35	41·5	NW.	NW.	NW.	NW.	...	...	...	...	...
8		30·092	30·046	30·15	30·15	30·10	30·09	29·54	51	40	50	44	48	40	40	W.	W.	NW.	calm	...	...	...	...	·02
9		30·140	30·056	30·15	30·10	30·16	30·11	29·56	52	35	52	43	53	45	37	W.	NW.	W.	NW.	·10	...	...	...	·05
10		30·036	29·811	30·01	30·00	30·08	29·88	29·50	53	34	55	48	53	46	43·5	SW.	SW.	SW.	calm	...	·51	·080	·230	...
11	○	30·038	29.983	30·06	30·04	30·05	30·00	29·50	54	42	55	50	54	51	40	S.	SW.	E.	calm	...	·28	...	·075	·52
12		29·910	29·827	30·04	30·00	29·99	29·97	29·34	59	44	57	51	58	52	51·5	NW.	W.	W.	calm	·10	·04	·090	·020	·63
13		30·176	30·031	30·10	30·08	30·20	30·07	29·66	45	40	58	50	57	41	44·5	NE.	W.	NE.	calm	...	·02	...	...	·30
14		30·206	30·106	30·12	30·10	30·18	30·15	29·74	50	41	56	52	51	48	42	N.	NW.	E.	calm	...	...	...	...	...
15		29·945	29·847	30·15	30·08	29·98	29·92	29·36	55	33	52	49	55	35	50	NE.	NE.	W.	NW.	·10	·06	·235	·035	·10
16		30·364	30·225	30·22	30·20	30·35	30·17	29·80	44	21	45	40	44	30	35	NE.	E.	N.	calm	...	...	...	...	·04
17		30·395	30·343	30·30	30·30	30·38	30·35	29·96	43	31	42	34	41	35	35	N.	E.	N.	calm	...	...	...	...	...
18	☾	30·471	30·416	30·30	30·28	30·44	30·38	29·97	43	22	44	32	43	34	36·5	NW.	E.	NE.	NW.	·15	...	...	...	·03
19		30·461	30·423	30·28	30·25	30·44	30·41	30·03	43	23	45	36	42	36	29	NE.	SE.	NE.	NW.	...	...	.060	...	...
20		30·412	30·380	30·20	30·15	30·38	30·35	30·00	38	24	47	40	48	35	28	S.	SE.	NE.	calm	...	...	...	...	...
21		30·354	30·159	30·10	30·00	30·30	30·10	29·94	39	26	50	45	46	37	30	SE.	SE.	SE.	calm	·10	...	...	...	...
22		29·707	29·555	29·76	29·75	29·74	29·58	29·33	44	35	49	43	48	37	33	S.	N.	N.	calm	...	·08	·070	·110	...
23		29·850	29·648	29·76	29·74	29·69	29·63	29·36	44	36	46	40	44	38	42·5	NE.	E.	NE.	E.	...	·03	...	...	·09
24		29·706	29·571	29·60	29·60	29·64	29·55	29·46	39	30	42	36	42	32	38	NE.	E.	NE.	E.	·10	·41	...	·300	·04
25		29·843	29·577	29·65	29·65	29·66	29·55	29·33	34	30	45	35	41	32	33	E.	NE.	NE.	F.	...	·05	·300	·220	·33
26	●	29·900	29·890	29·60	29·60	29·84	29·78	29·67	40	35	48	33	42	37	38	E.	SE.	NE.	E.	...	...	.090	...	·20
27		29·880	29·783	29·65	29·60	29·78	29·74	29·47	41	35	52	42	43	39	40·5	SE.	SE.	NE.	calm	·10	·01	...	...	·13
28		29·842	29·787	29·75	29.70	29·84	29·78	29·40	42	39	52	45	44	40	40·5	N.	N.	NE.	calm	...	...	...	...	...
29		29·898	29·874	29·80	29·70	29·87	29·84	29·51	44	38	50	42	43	41	39·5	NE.	SW.	NW.	calm	...	·03	...	·050	...
30		29·878	29·850	29·65	29·60	29·84	29·80	29·54	42	38	48	45	44	42	41	SE.	SN.	E.	SE.	·05	...	...	...	...
Aver.		30·471	29·555	30·30	29·60	30·44	29·55	29·59	59	21	58	32	58	30	39·4					1·00	1·86	1·645	1·670	2·53

THE

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[NEW SERIES.]

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XI. *Notes on the Geographical Distribution of Organic Remains contained in the Oolitic Series of the Great London and Paris Basin, and in the same Series of the South of France. By* HENRY T. DE LA BECHE, *F.R.S. &c.**

AT the present time, when rocks, so dissimilar in their mineralogical character and thickness, are referred to the same geological epochs, from the nature of their organic remains, it becomes important to ascertain, as far as our information will permit, to what extent the opinions usually entertained on this head are well founded; and if they should appear to be only partially correct, to determine to what distances, fossils, supposed characteristic, may be available. Many serious difficulties attend an examination of this nature. 1st, Very able observers of rocks may not be good zoologists, or may not have had the fossils obtained by them from particular strata, determined by those that are. 2ndly, We want synonyms for the same shells to which different names have been assigned by different authors. 3rdly, We do not possess complete lists, even of the fossils found at various known places. And 4thly, We cannot state that any given fossil discovered in one rock may not occur in another because not yet found in it. We are however in general furnished with lists of those fossils which occur most abundantly at any given place; and the organic remains, supposed characteristic, are most frequently detailed, from the very general desire, now existing among geologists, to adopt this mode of identifying strata, so that, notwithstanding the difficulties above enumerated, we may, by comparing the lists of those on whose accuracy we can depend, arrive at some useful conclusions respecting the geographical distribution of organic remains in a given group of rocks.

* Communicated by the Author.

M. Elie de Beaumont has already remarked on the constancy of the geological facts observable in the oolitic belt of the great geological basin which contains London and Paris *;—and M. Dufrénoy † has shown that in the South of France appearances are not very materially different. Therefore we may conclude that, though there may be many subordinate differences, there is a general resemblance in the mass of the oolite series in England and a large part of France, leading to the supposition that the rocks of which it is composed were formed under similar general circumstances.

It would appear that the three systems into which Mr. Conybeare ‡ has divided the oolite series of England, will be available in the parts of France comprehended within this sketch; therefore, in treating of the organic remains, we shall first consider the lower oolitic system, then the middle, and afterwards the upper.

As data whence to draw conclusions respecting the geographical distribution of the organic remains in the rocks under consideration, I shall avail myself, for the North of England, of the very excellent work of Mr. Phillips §. For the South of England I shall be compelled to use scattered information, as we have not any very detailed and published description of the organic remains contained in the oolite of this part of our island ||. For Normandy I shall have recourse to M. de Caumont's work on Calvados ¶. For the North of France, to the memoir of M. Boblaye **. And for the South of the same country, to the observations of M. Dufrénoy ††.

In order that the reader may judge of the general character of the oolite series of which the fossils will be noticed in the sequel, I have brought together the views of the geologists above named respecting the different portions of which they have particularly treated.

* Note sur l'uniformité qui regne dans la constitution de la ceinture Jurassique du grand basin géologique qui comprend Londres et Paris.—*Annales des Sciences Naturelles.* Juillet 1829.

† *Annales des Mines.* 1829.

‡ Outlines of the Geology of England and Wales.

§ Illustrations of the Geology of Yorkshire.

|| The lists of organic remains of the oolite contained in the well known "Outlines," &c. of Conybeare and Phillips, are much too general to be of service in inquiries of the present kind; but when local information could not be obtained, I have employed them for the midland and South of England, which principally afforded the materials for their construction.

¶ Essai sur la Topographie Géognostique du departement du Calvados. *Caen* 1828.

** Mémoire sur la formation Jurassique dans le Nord de la France.—*Ann. des Sci. Naturelles.* Mai 1829.

†† Des formations secondaires qui s'appuient sur les pentes méridionales des montagnes anciennes du centre de la France.—Part entitled "Formations Jurassiques du Sud-ouest de la France".—*Annales des Mines,* tom. v. 1829.

General

*General View of the Oolite Formation of Yorkshire (according to Mr. Phillips).**

a.	Kimmeridge clay	depth uncertain.
b.	Upper calcareous grit	60 feet.
c.	Coralline oolite	60
d.	Lower calcareous grit	80
e.	Oxford clay	150
f.	Kelloways rock	40
g.	Cornbrash limestone	5
h.	Upper sandstone, shale, and coal	200
i.	Impure limestone (Bath oolite)	30
k.	Lower sandstone, shale, and coal	500
l.	Ferruginous beds (inferior oolite)	60
m.	Upper lias shale	200
n.	Marlstone series	100
o.	Lower lias shale	500

Under the heads of different districts Mr. Phillips presents us with more detailed tabular views, from whence the following are selected:

In the Tabular Hills.

1. Upper calcareous grit, containing fossils resembling those in No. 3.
2. Coralline oolite, marked by corals, echini, plagiostomæ, melaniæ, &c.
3. Lower calcareous grit, pinnæ, gryphææ, ammonites, &c.
4. Gray argillaceous earth, containing many fossils at the bottom.
5. Ferruginous or argillaceous sandstone, with remarkable gryphææ, ammonites, &c. (Kelloways rock of the S.)†

Moorland District.

6. Impure, sometimes oolitic limestone, full of shells, (the cornbrash of geologists).
7. Sandstone, shale, ironstone, and coal, with carbonized wood, ferns, and other fossil plants.
8. Impure, often oolitic limestone and ironstone, with many fossil shells (oolite of Bath).
9. Sandstone, shale and coal, with carbonized fossil plants.
10. Subcalcareous, irony sandstone, often containing shells, called *dogger* (inferior oolite).
11. Upper lias shale, or alum shale, with nodules of argillaceous limestone, ammonites, belemnites, &c.
12. Ironstone and sandstone strata, with terebratulæ, pectines, cardia, aviculæ, &c.
13. Lower lias shale, with gryphææ, pinnæ, plagiostomæ, &c.‡

* Illustrations of the Geology of Yorkshire, pp. 32, 33. † Ibid. p. 43.
‡ Ibid. p. 35.

General View of the Oolitic Series in Somersetshire and Wiltshire.

It would be quite out of place to present a general view of this part of our island, as the labours of Smith and Conybeare are well known, and as the rocks in question constitute the type of the oolitic series generally, if we except the Portland beds.

It may be remarked that the Bath oolite does not occur on the coast of Dorsetshire. A series of clays with limestone, resembling forest marble and cornbrash largely developed, there separate the inferior oolite from the Oxford clay.

The coral rag, as is often the case, is also wanting on the same coast; that is to say, the beds containing such an abundance of corals, whence the name, do not occur, though an equivalent to the Oxford oolite does.

General View of the Oolite Rocks of Normandy (according to De Caumont).*

1. Argile de Honfleur (Kimmeridge clay). Glos sandstones, considered subordinate.
2. Calcaire de Blangy.
3. Coral Rag, consisting of a series of beds containing many oolites

* In my paper "On the coasts of France, and of the inland country adjoining; between Fecamp to St. Vaast:" inserted in the 1st volume of the Geological Transactions, New Series, and written from observations made in the spring of 1821, deceived by the mineralogical resemblance of this limestone, as developed at Hennequeville cliff, to some of the Portland stone, I attributed the Calcaire de Blangy to that rock: but I now perfectly agree with the French geologists, that the Honfleur and Havre clay is equivalent to the Kimmeridge. Subsequent examinations have shown me that this is very probable; consequently it was an error to refer the limestones in question to the Portland stone.

The following section, by M. de Caumont, of Hennequeville cliff, shows the superposition of the Kimmeridge (Honfleur) clay, the Calcaire de Blangy, and the coral rag.

1. Chalk with gray flints, and numerous alcyonia ...... about	100 feet.
2. Green earth........	40
3. Argile de Honfleur........	60
4. The same, alternating with many beds of ferruginous sandstone full of quartz grains and globules of oolitic iron.	20
5. The same, alternating with many beds of more compact sandstone, and full of broken shells forming a kind of lumachella........	10
6. Sandstone more siliceous than the preceding, containing fewer shells........	6
7. Siliceous limestone, very hard, containing globules of oolitic iron........	1
8. Whitish and slaty siliceous limestone........	3
9. Limestone resembling No. 7........	$2\frac{1}{2}$
10. The same, full of the interior casts of trigoniæ........	$1\frac{1}{2}$
11. Many beds more or less hard, resembling the preceding ...	3

12. Bed

oolites and corals, passing, in the lower part, into a ferruginous and calcareous sandstone (calcareous grit of the English).

4. Argile de Dives (Oxford clay). Black blue clay, rarely yellowish: in the upper part, subordinate beds of oolitic limestone; in the lower, marly limestone of a gray, yellowish or blue colour, apparently represents the Kelloway rock.
5. Cornbrash?
6. Forest Marble, consists of a series of beds more or less oolitic, and more or less sublamellar, is very often fissile, and contains fragments of shells and corals.
7. Great Oolite. Upper beds sometimes resemble forest marble; middle and lower beds of a finer grain, rarely oolitic, not sublamellar, often as soft as chalk. Caen stone belongs to this division. M. de Caumont considers the clay of Port-en-Bessin as equivalent to the Caen stone.
8. Inferior Oolite. Upper part resembles the Caen stone; connection between the great and inferior oolite. In the lower part, two or three beds of yellowish or gray calcareous sandstone, containing ferruginous oolites; full of shells.
9. Lias. Upper part contains belemnites; and the lower, the Gryphæa incurva.—There seems an equivalent of the sand and marlstone of Smith upon the top of the lias.

The above is condensed from the general descriptions contained in the *Essai sur la Topographie Géognostique du Departement du Calvados.* It will be found not to differ materially from the general view I presented in my paper on Normandy, if the Portland beds be withdrawn and the marl and marlstone there noticed be considered equivalent to the Kimmeridge clay, except indeed that M. de Caumont considers the Port-en-Bessin marls as representing the Caen stone.

General View of the Oolite of the North of France (according to M. Boblaye).

The rocks of this class which M. Boblaye had occasion to observe,

12. Bed of black flint, passing into a gray sandstone, and finally into the limestone	½ feet.
13. Many limestone beds passing into sandstone	4
14. Yellowish limestone without shells, many beds	5
15. White marl	1
16. Yellowish white limestone containing casts of spiral shells	6
17. Whitish marl	1
18. Limestone resembling No. 16	2
19. Limestone full of corals (coral rag)	6
20. The same, more compact	5
21. Oolite of the coral rag	

"constitute

"constitute a nearly equal thickness, about 500 metres (1640 English feet) from the lias to the coral rag inclusive.

"The maximum of absolute height is 350 metres (1148 English feet) in England, and 400 metres (1312 English feet) in the Ardennes.

"1. The marls of Florenville, Houldizy, &c. are characterized by the Gryphæa incurva, plagiostomæ, &c. as the lias.

"2. The sandy limestones, the micaceous marls, the ferruginous limestones of Florenville, Orval, Carignan, Dreux, &c. perfectly represent that great sandy and ferruginous zone so well described by the English geologists, under the names of sand, marly sandstone, and inferior oolite. The Gryphæa cymbium and Plicatula spinosa are its most characteristic fossils.

"3. The fullers' earth is easily recognised in the blue marls of Lamouilly, Vaux, &c. It is also in their lower part that the fullers' earth of the Ardennes is found.

"4. The great oolite appears with all its characteristics in the extensive quarries of Chauvaney, Brouesnes, Luzy, Ballon, &c.

"5. The white marls of La Jardinette near Stenay, of Luzy, &c. offer the most perfect analogy with the Bradford clay of the English geologists; the fossils are the same; we more particularly mention the following: Turritella, Ostrea acuminata, Terebratula digona, T. coarctata, Cydarites ornatus, &c.

"6. Above this well-defined geological horizon there reigns some uncertainty. If the forest marble is not recognisable with certainty in the coarse lumachella which covers the marls, in the compact madrepore limestones, and sandy limestones of Stenay; and if, moreover, the beds which, from their fossils and mineralogical composition, have appeared to me equivalent to the Stonesfield slate, do occupy a somewhat different position, and will not permit us to sustain the analogy;—it is not the same with cornbrash. Its fossils (Avicula echinata, Terebratula subrotunda), and its mineralogical characters, are found in the coarse limestones of Stenay, Beaumont, &c.

"7. The marls of Stonne, Belval, Dun, &c. present us in their composition, their thickness and their fossils (Gryphæa dilatata, Pinna lanceolata, &c.), with the most perfect resemblance to the Oxford clay.

"8. The sandy and ferruginous oolite corresponds with the calcareous and ferruginous sandstone which the English place at the base of the coral rag (calcareous grit).

"Lastly, the coral rag appears with its distinctive characters near Belval, Dun, &c. We have there found numerous univalves, Melania, Turritella, &c. Ostrea gregaria, Lima rudis, and the numerous echinites, mentioned by the English*."

* *Annales des Sciences Naturelles*, tom. xvii. pp. 79, 80.

General

General View of the Oolitic Series of Burgundy (*according to M. Elie de Beaumont*).

"If we proceed from Flogny to Ancy-le-Franc, we observe the following rocks rise successively from beneath the green-sand and chalk.

"1. Compact limestone which corresponds in its position with the Portland stone of English geologists.

"2. A system of marly limestone and gray marl characterized by the Gryphæa virgula (Kimmeridge clay).

"3. A very thick series of compact limestones with a conchoidal fracture, of limestones with an earthy and cretaceous fracture, and oolite (Oxford oolite, coral rag).

"4. Beds of a gray marly limestone with an earthy fracture, (calcareous grit, Oxford clay).

"From beneath these last rise the limestones, often oolitic, which form the plains and plateaux on the South of Ancy-le-Franc, limestones which are precisely the same with those of the Chammes d'Avenay, and the Vallée de l'Ouche, near the Pont d'Ouche.

"If the facts above mentioned are correctly stated, and if the long cliff or escarpment (the course of which through the north-eastern provinces and centre of France was noticed fifty years since by Guettard,) really contain the Oxford clay and coral rag among the beds of which it is composed, we may see the Bath oolite and *Calcaire à polypiers* in the oolite limestone, the beds of which rise on all sides from beneath those of the cliff. The yellowish-white and marly limestone of Burgundy would then represent the Fullers' earth of the English, and the Banc bleu of Caen; the entrochite limestone would be the inferior oolite; and the second marly stage, which rests immediately on the gryphite limestone, would correspond with the thick marls which in England cover the lias. We should then see that the constancy of the geological facts, noticed in Great Britain, Normandy, in the Bas Boulonnais, and in the Ardennes, is preserved in Burgundy, as might be expected *à priori* *."

General View of the Oolite of the South of France (*according to M. Dufrénoy*).

"In the secondary basin of the S.W. of France, separated from that of Paris by the mountains of Auvergne, Limousin, and La Vendée, the oolite series may be divided into three distinct groups, corresponding with the three systems of the same formations in England. The separation of these groups

* *Annales des Sciences Naturelles.* Juillet 1829.

is,

is, however, not nearly so well pronounced; the beds corresponding with the Oxford and Kimmeridge clays are but rarely observed in this part of France, and appear to be replaced by marly limestone: this is most commonly the case between the middle and superior systems.

"The numerous subdivisions noticed by the English geologists are but very imperfectly seen in the secondary basin under consideration; there are nevertheless some sufficiently constant.

"The lower is the only part of the oolite which appears on the eastern extremity of the basin; it forms a considerable mass on the N. of the department of the Hérault, which advances into the sea near Montpellier and Cette.

"The oolite formations are greatly developed on the W. of the basin; from Cahors to the ocean they form a chain with a mean breadth of twelve leagues: they are more than twenty-five between the mountains of Limousin and La Vendée.

"Between the two extreme points we have named, the three systems of the oolite may nearly always be seen; the inferior frequently resting on the marls and limestone which we have referred to the lias.

"In some localities (Milhau, near Villefranche, &c.) the inferior system presents micaceous marls containing Gryphæa cymbium, belemnites, &c. which may be compared to the sand of the inferior oolite: we also find sublamellar limestones, compact limestones containing beds of oolitic iron, and beds of a white oolite, furnishing excellent building-stone; the latter beds, well developed only at Mauriac, in the Aveyron, represent the great or Bath oolite. They are associated with polypifers as at Caen. In other parts of the basin, principally in the east, this system is composed of compact limestones of a yellowish-gray colour, containing an abundance of silex, (environs of Nontron, Poitiers, &c.) and beds of slightly oolitic limestone. The beds of compact earthy limestone containing many ammonites and terebratulæ appear to form the upper part of this system: by comparing these fossils with those found in the cornbrash in England, we are led to assimilate the rocks. The lower system is the thickest; it forms by itself more than three quarters of the whole depth; it occupies more than twelve leagues of the fifteen or sixteen which this formation covers between the Sables d'Olonne and Rochefort.

"The middle oolite system is in a great measure composed of marly limestone beds: yet in many places (Marthon, forest of La Braconne, Pointe de Duché, Pointe d'Angoulin, &c.) considerable masses of polypifers are associated in it with thick beds of irregular and earthy oolite. The prodigious abundance of polypifers, the nature of the oolite, and the occurrence of many

many fossils, lead us to assimilate these beds with those named Coral Rag by the English, and the oolite which accompanies them to the Oxford oolite. These are the only two subdivisions which can be made in the middle group, and even these so pass into each other that, at the Pointe d'Angoulin and Marthon, numerous polypifers are found in the midst of the oolite beds. Beds of very marly limestone cover this system. We already find some of the Gryphæa virgula, the presence of which with us (France), characterises the clay separating this system from the upper. This group of the oolite covers a space about two leagues and a half broad between La Rochelle and Rochefort; it is more considerable between Poitiers and Angoulême, and between the last town and Confolens."

"The upper system is the most uniform in this basin; it is often reduced to a few marly beds, containing a prodigious quantity of the Gryphæa virgula, attached to one another, and nearly forming by themselves a bed of lumachella. In some localities this is covered by compact marly limestone forming very thick beds (Cahors), in which this little gryphite, so characteristic in France, is found here and there disseminated. From the environs of Angoulême to the ocean, beds of oolite are observed nearly constantly to cover these marls containing the Gryphæa virgula; they reappear in other places (Pointe du Rocher), and are immediately in contact with the greensand. By comparing this system with that which exists in England, we may assimilate the oolite of which we have spoken with the Portland stone, while the lower beds of marl, containing the Gryphæa virgula, correspond with the Kimmeridge clay*."

The reader being now in possession of what may be termed the mineralogical character of the oolitic series at the various places above noticed, I shall proceed to examine the organic character of the same series derived from the same authorities, commencing with the inferior system.

* Formations Jurassiques du Sud-ouest de la France.—*Annales des Mines*, tom. v. p. 430—434.

Organic Remains of the Inferior System of the Oolitic Series.—Subdivision. Lias.

[The Species marked in Italics occur in more than one locality.]

North of England. Yorkshire.—Phillips.	South of England. Lyme Regis.—De la Beche.	Normandy. Calvados.—De Caumont.	South of France. Bruniquel, Villefranche } Dufrénoy.
Dicotyledonous Wood	Plants, Dicotyl. Wood, & Lignite	Lignite	Coal.
	Pterodactylus macronyx (Buckl.).		
Crocodile	Crocodile?		
	Plesiosaurus dolichodeirus	Plesiosaurus.	
Ichthyosaurus	Ichthyosaurus communis	Ichthyosaurus.	
	———— platyodon.		
	———— tenuirostris.		
	———— intermedius.		
Crustacea	Crustacea.		
Fish	Fish	Fish.	
	Ichthyodorulites (Buckl. & De la B.)		
	Dapedium politum	*Dapedium politum.*	
	(*Many other fish.*)		
	Coprolites (Buckl.).		
Polypifers (*rare*)	Polypifers (*rare*)	Polypifers (*rare*).	
Echinites	Echinites	Echinites.	
Cidaris (smooth spine).			
Ophiura Milleri (Phil.).			
Pentacrinites *Caput Medusæ*	Pentacrinites *subangularis*	Pentacrinites *Caput Medusæ?* ...	Pentacrinites *Caput Medusæ.*
———— *Briareus*	———— *Briareus*	———— *subangularis?*	
	———— *basaltiformis.*		
Belemnites tubularis (Y. & B.) ...	Belemnites *pistilliformis*	Belemnites (2 or 3 species)	Belemnites *pistilliformis.*
———— compressus (Y. & B.)	———— *elongatus*	....	———— sulcatus.
———— *elongatus* (Miller)	———— acutus	....	———— apicicurvatus.
Nautilus astacoïdes (Y. & B.)	Nautilus striatus	Nautilus truncatus.	
———— lineatus	———— intermedius.		
———— annularis (Phil.).			
Ammonites *Walcotii*	Ammonites *Walcotii*	Ammonites *Walcotii*	Ammonites *Walcotii.*

Ammonites *fimbriatus* . …………	Ammonites *fimbriatus* …………	Ammonites *fimbriatus*.	
——— heterophyllus.			
——— *Henleii*………………	——— *Henleii*.		
——— subcarinatus(Y.& B.).			
——— heterogeneus(Y.&B.).			
——— crassus (Y. & B.).			
——— *communis* ……………	——— *communis*.		
——— angulatus.			
——— annulatus.			
——— fibulatus.	——— lævigatus.	——— Strangewaysii.	
——— subarmatus.			
——— maculatus (Y. & B.).			
——— gagateus (Y. & B.).			
——— *planicostatus* . ………	——— *planicostatus*.		
——— balteatus (Phil.).			
——— arcigerens (Phil.)	——— armatus.	——— *falcifer* ……………	——— *falcifer*.
——— brevispina.			
——— Jamesoni.			
——— erugatus (Bean.)	——— ellipticus.	——— decipiens.	
——— nitidus (Y. & B.).			
——— anguliferus (Phil.).		(A large ammonite sometimes	——— Stokesii.
——— lenticularis (Phil.) .	——— discus.	2 or 3 feet in diameter.)	
——— crenularis (Phil.).			
——— Clevelandicus(Y.&B.)	——— Loscombi.		
——— *Turneri*. …………	…………………………	…………………………	——— *Turneri*.
——— geometricus (Phil.) …	——— *stellaris* . ………	——— *stellaris*.	
——— vittatus (Y. & B.)			
——— sigmifer (Phil.) . ……	——— Greenovii.		
——— Hawskerensis(Y.&B.)	——— Brookii.		——— Humphresianus.
——— Conybeari.			
——— *Bucklandi* . …………	——— *Bucklandi* . …………	——— *Bucklandi*.	
——— *obtusus* ……………	——— *obtusus*.		
——— striatulus.			
——— ovatus (Y. & B.)			
——— Mulgravius (Y. & B).	——— Davæi.		

Table *continued.*

North of England. Yorkshire.—Phillips.	South of England. Lyme Regis.—De la Beche.	Normandy. Calvados.—De Caumont.	South of France. Bruniquel, Villefranche } Dufrénoy.
Ammonites exavatus (Y. & B.)	Ammonites Birchii.	Ammonites *concavus*.	
——— *concavus?*	——— Bechei.		
——— *elegans?*		——— *elegans*.	
Gryphæa *incurva* (Sow.)	Gryphæa *incurva* (Sow.)	Gryphæa *incurva* (Sow.)	Gryphæa { *arcuata*, Lam. *incurva*, Sow. }
——— depressa (Phil.)	——— (another species).	——— dilatata.	——— obliquata.
——— *Maccullochii*	....	....	——— *Maccullochii*.
			——— cymbium.
			——— gigantea.
Spirifer *Walcotii*	Spirifer *Walcotii*	Spirifer *Walcotii*	Spirifer *Walcotii*.
Terebratula punctata	Terebratula *ornithocephala*	Terebratula *ornithocephala*	Terebratula *tetraëdra*.
——— resupinata	——— crumena	——— *acuta*	——— obsoleta.
——— trilineata (Y. & B.)	——— serrata	——— quadrifida.	
——— *acuta*.			
——— bidens.			
——— triplicata (Phil.).			
——— *tetraëdra*.			
Plicatula *spinosa*	....	Plicatula *spinosa*.	
Pecten sublævis	Pecten	Pecten barbatus	Pecten *equivalvis*.
——— *equivalvis*	....	——— *equivalvis*	——— (other species).
——— lens.			
Plagiostoma *gigantea*	Plagiostoma *gigantea*	Plagiostoma *gigantea*	Plagiostoma sulcata.
——— pectenoïdes			——— *punctata*.
——— rustica?			
Avicula *inæquivalvis*	Avicula *inæquivalvis*	....	Avicula *inæquivalvis*.
——— cygnipes	——— lanceolata.		
Crenatula ventricosa	Crenatula?		
Cucullæa (smooth)	Cucullæa.		
Nucula ovum	Nucula	....	Nucula clariformis.
Pinna folium (Y. & B.)	Pinna	Pinna lanceolata	Pinna.

Modiola *scalprum*	Modiola *scalprum*	Modiola cuneata..........	Modiola *scalprum.*
——— *Hillana*	——— *Hillana.*		
Trigonia literata (Y. & B.)..........	Trigonia	....	Trigonia striata.
	Cytherea.		——— (another species).
Unio concinnus	Unio	Unio crassissimus.	
—— crassiusculus.			
—— Listeri.			
—— abductus (Phil.).			
Pholadomya obliquata	....	Pholadomya gibbosa	Pholadomya.
		——— lyrata.	
Trochus *anglicus*..........	Trochus *anglicus*..........	....	Trochus *imbricatus.*
	——— *imbricatus.*		
Turbo undulatus (Phil.)	Turbo.		
Inoceramus dubius.	Helicina expansa.		Pleurotomaria anglica.
Hippopodium ponderosum.			
Cardium truncatum.			
——— multicostatum (Bean.)	Melanea.		
Corbula? cardioïdeum (Phil.).			
Corbis?			
Crassina minima (Phil.).			
Venus.			Lima antiqua.
Pullastra..........			—— (another species).
Amphidesma donaciforme (Phil.).			
——— rotundatum (Phil.).			
Sanguinolaria elegans (Phil.).			
Mya literata.			
Orbicula reflexa.			
Rostellaria?			
Actæon.			
Natica.			
Serpula capitata (Phil.)..........	Serpula (*very rare*).		
Dentalia giganteum.			

List of Organic Remains in the Lias of the North of France, according to M. Boblaye.

Shells:—Belemnites (abundant); Ammonites (species not enumerated); Gryphæa *arcuata* Lam., (*incurva* Sow.); Ostrea or Gryphæa nana; Pecten (large); Plagiostoma *gigantea;* Plagiostoma *punctata*; Cytherea (abundant); Pleurotomaria ornata; Cirrus.

From the foregoing Lists the following Table of Lias Organic Remains which occur in more than one situation may be constructed:

Name.	North of England.	South of England.	Normandy.	North of France.	South of France.
Plants, Lignite or Coal	*	*	*	...	*
Plesiosaurus	...	*	*	...	...
Ichthyosaurus	*	*	*	...	...
Crustacea	*	*	...	...	...
Fish	*	*	*	...	...
Polypifers	*	*	*	...	...
Echinites	*	*	*	...	...
Pentacrinites Caput Medusæ	*	...	*	...	*
——— Briareus	*	*	...	...	...
——— subangularis	...	*	*	...	...
Belemnites (abundant)	*	*	*	*	*
Ammonites Walcotii	*	*	*	...	*
——— fimbriatus	*	*	*	...	...
——— Henleii	*	*	...	...	...
——— communis	*	*	...	...	...
——— planicostatus	*	*	...	...	...
——— falcifer	...	...	*	...	*
——— Turneri	*	...	...	...	*
——— stellaris	...	*	*	...	...
——— Bucklandi	*	*	*	...	...
——— obtusus	*	*	...	...	...
Gryphæa incurva *Sow.* / ——— *arcuata* Lam.	*	*	*	*	*
——— Maccullochii	*	...	...	...	*
Spirifer Walcotii	*	*	*	...	*
Terebratula ornithocephala	...	*	*	...	...
——— acuta	*	...	*	...	...
——— tetraëdra	*	...	...	...	*
Plicatula spinosa	*	...	*	...	...
Pecten equivalvis	*	...	*	...	*
Plagiostoma gigantea	*	*	*	*	...
——— punctata	...	...	...	*	*
Avicula inæquivalvis	*	*	...	...	*
Modiola scalprum	*	*	...	...	*
——— Hillana	*	*	...	...	...
Trochus anglicus	*	*	...	...	...
——— imbricatus	...	*	...	...	*

Unfortunately

Unfortunately M. Boblaye has not furnished us with a list of the ammonites in the district noticed by him, otherwise we might probably have seen that some of those discovered elsewhere, were also found in the North of France. Ammonites concavus and A. elegans are marked as questionable in the North of England, but they are stated to occur in the lias of Normandy. The Dapedium politum is found in the South of England and in Normandy.

Of the organic remains above enumerated, the Gryphæa incurva *Sow.* (G. arcuata *Lam.*) appears the most characteristic fossil.—Belemnites are also abundant. Unfortunately the synonyms of the fossils are not well understood; so that the species cannot be determined with precision. Ammonites Walcotii, Spirifer Walcotii, Pecten equivalvis, Plagiostoma gigantea, Avicula inæquivalvis, Modiola scalprum, have also a wide range. Ammonites Turneri and Terebratula tetraëdra occur in localities widely separated from each other.

Ammonites Bucklandi is extensively found, though, by the accompanying lists, it is only noticed in the North and South of England and in Normandy.

Of the 90 species of fossil shells enumerated by Mr. Phillips in the lias of Yorkshire, 26 occur in the same rock in the other localities; of these 15 are discovered at Lyme Regis, 9 in Normandy (belemnites not being specified), 2 in the North of France (neither ammonites nor belemnites being specified), and 8 in the South of France.

Of the 36 species mentioned as found at Lyme Regis, 19 have been observed in the other places; of these 15 occur in Yorkshire, 8 in Normandy, 2 in the North of France, and 7 in the South of France.

Of the 25 species enumerated in Calvados, 12 are discovered in the other localities; of these 9 occur in Yorkshire, 8 at Lyme Regis, 2 in the North of France, and 5 in the South of France.

Of the 5 species noticed by M. Boblaye in the North of France, who unfortunately does not mention the species of either ammonites or belemnites, 3 are found in the other localities; and of these 2 occur in Yorkshire, Lyme Regis, and Normandy, and 3 in the South of France.

Of the 26 species noticed in the South of France, 13 are found in the other localities; and of these 8 occur in Yorkshire, 7 at Lyme Regis, 5 in Normandy, and 3 in the North of France.

[To be continued.]

XII. *On*

XII. *Account of a new Paddle-Wheel, in which the Motion is obtained by means of an eccentric fixed Crank and Levers. Invented by* King Williams*.

With an Engraving.

A REPRESENTATION of this paddle-wheel and some of its parts in detail is given in the accompanying drawing. (Plate I.) Fig. 1. is the end-view of the frame and paddles. A is a cast-iron centre-piece or bosh, which receives the wrought-iron arms 1, 2, 3, 4. B is the fixed crank; C the collar revolving upon it, bearing the connecting rods 9, &c. as also shown at T in fig. 3. H shows the angle made by any one of the paddles when the wheel is in that position; and when it arrives at the dotted lines F, just entering the water, it makes an obtuse angle with its axis (see G), so that the paddle acts with a propelling power the instant it enters the water, without any loss of power whatever; and when F arrives at E, it has the greatest power, and its lever and rod 9 make an angle best adapted to resist the pressure of the water at E. When E arrives at the dotted lines D, it leaves the water in a direction nearly perpendicular to its surface.

Fig. 2. 8 is a view of one of the paddles, showing part of the double frame *m m*: I the horizontal spindle which connects the two frames together; the fulcrum of the paddle being between the shoulders of the spindle I and the frames *m m*, but move freely. J is a screw on the paddle-spindles; the nut K serves to screw the four angles all fast together.

Fig. 3. *n* is the revolving collar working on crank C, fig. 1, with its major-rod. T is the same turned down, to show how the other rods are connected. U is a brass bush to fit the crank bearing *z*, fig. 4. P one of the bolts fitting into L, fig. 2.

Fig. 4. The fixed crank, and part of frame *m m*. R the engine-shaft. W the engine-crank. X and Y bearing the part 6, go into 6 of frame *m*, and are there keyed fast. The pivot 5 goes into hole 5 of the fixed crank, and there revolves freely, carrying with it the whole of the frame. The part of the other frame represented at 7 bearing on O, the crank is there set fast by a strong screw, but can be moved so as to give a different angle to the paddles.

This plan for a paddle-wheel was first invented by me for a ship of war, to work in midships, as it does away with the back-water. But the principle is well adapted to any steam-vessel. It has been tried on a large scale in the Mediterranean,

* Communicated by the Author.

in

in voyages from Trieste to Venice, and it gave the greatest satisfaction to the parties making the trial, shortening the voyage by two hours, and causing a great saving of coal. The plan is simple, and a wheel constructed upon it would have very little friction. The paddles may be renewed in a few minutes, as they are screwed and dowelled on, so that a broken paddle may be removed, or one of a different size fixed on, with the greatest ease; and the inclination, or dip and rise of the paddles can be changed in five minutes, by a *set-screw* on the fixed or eccentric crank.

A working model of this paddle-wheel, on the scale of an inch to a foot, was completed by me in January last (1829); it was shown to various gentlemen engaged in steam-navigation and the manufacture of machinery; and in the beginning of August last, it was placed in the hands of Mr. — Morris, parchment-manufacturer, of Long-Lane, Bermondsey, who kindly endeavoured to introduce it to the notice of some parties high in office. It remained with him for about three months, and was deposited for exhibition in the National Repository of Arts, King's Mews, on the 12th of November last.

On the 14th instant, I removed it from the gallery, for the purpose of showing it to Mr. Richard Taylor, to whom a drawing and description of it had been previously submitted for publication in the Philosophical Magazine and Annals of Philosophy; and it now remains in my possession, and may be inspected by any person who may wish to examine it.

Should any gentleman engaged in the construction or application of machinery to steam-navigation be desirous of trying paddle-wheels on this principle, I shall be happy to construct a model, in which the principle will be simplified, and the expense of construction of the wheel materially reduced.

5, Old George-Street, Suffolk-Street, Southwark, Dec. 21, 1829. KING WILLIAMS.

XIII. *On Artificial and Natural Arrangements of Plants: and particularly on the Systems of Linnæus and Jussieu.* By WILLIAM ROSCOE, *Esq. F.L.S.*

[Continued from page 23.]

INSTEAD of dwelling further on the endeavours of the French botanists to invalidate the labours of Linnæus by resorting, as Ventenat has done, to the well-known censures of Haller and others, I shall in the sequel of this paper endeavour to ascertain the relative merits of the two systems which now principally offer themselves to our acceptance; in which I shall attempt to show,

I. That the method of Jussieu is not in fact a natural, but an artificial one.

II. That, as an artificial method, the system of Jussieu is inferior to that of Linnæus.

III. That the artificial and natural methods of arrangement are, and must always remain, essentially different from each other, as well in the means employed as in the objects to be attained.

I. Could we suppose it possible for a person to be born with some superior instinct, which enabled him to decide at first sight on the character of a plant, and the genus and order to which it belonged, we might perhaps be induced to assent to his decisions, and allow him arbitrarily to establish his system. But, even with this conviction on our minds, circumstances might arise to shake our belief in his infallibility; and if, like Bernard de Jussieu, he should, in one short order of only eight genera, unite together the *Bromelia* and the *Hydrocharis*, the *Musa* and the *Galanthus*, we should perhaps feel inclined to ask upon what similarity in the flower, root, or seed, he had founded his opinion.—Nor would it be sufficient for the ends of science, if the decisions of this superior being were always free from error. For this purpose, we must not only know, but must be enabled to communicate our knowledge to others; and how this could be done, without our giving some specific reasons for our convictions, and for the assent to them which we claim, it is not easy to conceive.

These difficulties were perceived by the younger Jussieu; who, instead of giving us a mere list of genera, arbitrarily arranged in orders, characterized from some one of the principal genera in each order, has condescended to explain the grounds of his opinions by an arrangement or system, founded on the visible and tangible parts of the plants themselves. From this moment it was evident that no supernatural intelligence had dictated the arrangement; which, notwithstanding its more imposing title, was to be judged of, like all other arrangements, only by its superior ingenuity, accuracy, and utility. It might indeed be more skilfully executed than the system of Linnæus; but still it appealed to the same organs of sense, and submitted to be judged by the same rules.

In one view of the subject, all modern systems may indeed be denominated natural, as they are all deduced from some part, property, or peculiarity, of the plants themselves: those of Morison, Ray, Herman, and Gærtner, from the fruit; of Tournefort, Knaut, and Rivinus, from the corolla; of Magnol;

nol, from the calyx; that of Linnæus, chiefly from the number, proportion, and situation of the stamina; and that of Jussieu, from the mode of germination, and situation of the stamina; but principally, like that of Tournefort, from the number and disposition of the petals. It is true, that some of these methods may be greatly preferable to others; but it is equally true, that there is scarcely one of them that does not possess some advantages which the others do not afford, and which have induced their respective authors to give them the preference. Some of them may even approach nearer to a natural system than the rest; or, in other words, may occasion less separation among plants which have a real affinity: others may pay less regard to this object, and may in some degree sacrifice it for the purpose of giving a more correct, extensive, and intelligible nomenclature; but the distinctions on which they are founded are equally natural; although it may not be possible for any method that is confessedly founded upon the sensible phænomena of the vegetable kingdom, whatever its pretensions may be, to unite together the families of plants in the strict natural orders and relative situations, or occasionally to avoid separating those which the general convictions of our senses assure us ought to be united.

If however it be still asserted that the system of Jussieu is to be preferred, as exhibiting a more exact conformity to the affinities of nature than that of Linnæus, may we be allowed to ask upon what this superiority is founded, and in what particular part of the system it consists? Are the affinities of plants more likely primarily to result from the petals, or from the stamina? from the part which shelters the immediate organs of reproduction, or from those organs themselves, connected as they are with the very nature and fructification of the plant? Supposing a doubt to arise whether a plant ought to be arranged with such as agreed with it in the corolla, or in the stamina, how would a skilful naturalist be inclined to decide? or which would he consider as the most powerful affinity? In whatever manner the orders of the two Jussieus may have been formed, they exhibit, at least, as many incongruities to the general observer, as the classes and orders of Linnæus. What would such an observer, unacquainted with the secret chain employed by these authors, say to the union in the same class of the *Palmæ* with the *Junci?* the *Musæ* with the *Hydrocharides?* the *Proteæ* with the *Atriplices?* the *Jasmineæ* with the *Scrophulariæ?* the *Rhododendra* with the *Campanulaceæ?* or, in short, to the many tribes apparently wholly discordant from each other, in conformation, in habit, in qualities, which occur in almost every class? Can the system of

Linnæus exhibit any associations more revolting to his conceptions, or which would tend more decisively to convince him that, whatever may be their pretensions, these systems are in fact equally artificial, and that their assumed natural affinities are nothing more than a partial resemblance, founded on some peculiarity of habit or conformation, which may serve to decide its situation in a nomenclature, but has often little or no relation to the real and essential nature of the plant?

II. If such be the fact, our inquiry will now take a different shape. It is no longer a question as to the superiority of one system over another, but a question of degrees as to the superior execution of a similar method. Let us then, whether we choose to denominate them both natural or both artificial, briefly compare the rival arrangements of Linnæus and Jussieu.

The most important difference between these two methods consists in a preliminary distinction made by Jussieu, by which he divides the vegetable kingdom into three departments, to each of which he applies a separate mode of arrangement, whereas Linnæus applies his method indiscriminately to the whole. By the plan of Jussieu we are in the first place to ascertain whether the plant which we examine rises from the seed without a cotyledon, with one cotyledon, or with two cotyledons *; and having determined this point, we then proceed by other rules to distinguish the individuals in each department. By that of Linnæus we take the plant without any regard to its mode of germination, and from the parts of fructification immediately determine its character, and assign it to its proper genus. That the mode in which plants arise from the seed †, or, more strictly speaking, that the seed itself, of which the cotyledons are formed, affords a true natural distinction, cannot be doubted; but in estimating the advantages of this distinction, we must also estimate its disadvantages, and form our decision upon the whole result. The object attained by

* This distinction it may be observed was made by Linnæus himself, as the foundation of his *Regnum Vegetabile;* with the necessary and indeed indispensable addition of the *Polycotyledones.*

"Tribus vegetabilium tres vulgo numerantur.

Monocotyledones.	Fruges 1. 2. 3.
Dicotyledones.	Plantæ 4. 5.
Polycotyledones.	Rhizophora.
Acotyledones.	Cryptogamæ 6. 7. 8. 9."

Linn. Reg. Veg. 3.

† In his *Philosophia Botanica,* Linnæus has carried this method much further than Jussieu has done; having divided the Monocotyledones into

perforatæ.	Gramina.
unilaterales.	Palmæ.
reductæ.	Cepa.

And

by Jussieu is the separating from the great mass of vegetables, two portions; one of which, the acotyledones, comprehends the cryptogamous plants of Linnæus, and forms the first class of Jussieu: the other, the monocotyledones, includes the gramineous and liliaceous plants, and forms the second, third, and fourth of his classes. These distinctions may be admitted to be well founded*; but what are the advantages they afford over those of Linnæus? who has also referred the Cryptogamous Plants to a distinct class by a peculiarity equally natural, the inconspicuity of their flowers, and with a few exceptions, not perhaps difficult to have been avoided, has arranged the gramineous and liliaceous plants in orders as natural as those of Jussieu.

In this respect, then, the two systems are nearly upon an equality; and to say the truth, it was almost impossible for any naturalist, upon a subject where the grounds of distinction were so numerous and so manifest, to adopt a different conclusion. But if nothing be gained in this instance by Jussieu, can we also say that nothing is lost? Is it no disadvantage, on discovering an unknown plant, to be under the necessity, before we proceed to its further investigation, of ascertaining in what manner it commenced its growth, and whether it rose from the seed with one or with two cotyledons, or without any cotyledon whatever? To whom are we to apply for this information? Or are we to be turned round to ascertain the primary distinction by the sensible appearance, and instead of saying that the plant rose from one cotyledon and is therefore a grass, that it is a grass and therefore rose from one cotyledon? At all events, it imposes a difficulty on the student

And his Dicotyledones into

immutatæ.	Legumina &c.
plicatæ.	Gossypium.
duplicatæ.	Tetradynamia &c.
obvolutæ.	Helxine.
spirales.	Salsola &c.
reductæ.	Umbellatæ.

And in his Polycotyledones he enumerates Pinus, Cupressus, and Linum, p. 102.

* Yet it must be observed that in the numerous tribe of the *Orchideæ*, which Jussieu has arranged among his Monocotyledonous Plants, others have not been able to discover the slightest trace of a cotyledon. For instance, "ORCHIS MORIO. Acotyledoneus, ne vel minimo placentæ rudimento unquam exserto."—"LIMODORUM VERECUNDUM. Embryo minutus, acotyledoneus." V. *Salisbury in Linn. Trans.* vol. vii. pp. 31, 32.—Again, some plants have been discovered to have more than two cotyledons, as in *Pinus*, and *Dombeya;* the cotyledons of the latter of which "are distinctly four." *Smith's Introd. to Bot.* pp. 98. 289. And even the Mosses are said to have numerous seed-lobes, "so that these plants are very improperly placed by authors among such as have no cotyledons." *Ib.* p. 190.

without

without affording an adequate advantage, and throws a doubt over the great mass of individuals of the vegetable kingdom, to be removed only by inquiring into the mode of their early growth, in order to separate from the rest some detached plants which are equally as well separated by other distinctions quite as natural and more permanent, and which it is indeed impossible should be confounded with them.

This peculiarity in the method of Jussieu being considered, the two systems, as far as they regard the great mass of the vegetable kingdom, may now be placed in more direct comparison. Linnæus has founded his primitive distinctions on the number and proportions of the stamina; not omitting the diversities arising from their situation. Jussieu, disregarding in his primary distinctions the number of the stamina, has recourse merely to their situation, which he distinguishes into three different manners, as being placed upon, around, or below the germen, under the appellations of *Epigyna*, *Perigyna*, and *Upogyna* *. This distinction is applied however only to his apetalous and polypetalous plants, the monopetalous plants being distinguished not immediately by the stamina, but by the situation of the corolla. This necessarily compels him to commence his definitions by the corolla, and accordingly he first divides his dicotyledonous plants into *apetalous*, *monopetalous*, and *polypetalous*. Of these the apetalous are to be again subdivided by the stamina, which are considered with respect, not to the number, but the situation; and as in the absence of the corolla the stamina are inserted *directly* into the style or germen, this is denominated the *absolutely immediate insertion* of the stamina, constituting the fifth, sixth, and seventh, of his classes. The *monopetalæ*, distinguished into separate tribes by the corolla, which is for the most part staminiferous, and is therefore said to exhibit the *mediate insertion* of the stamina, form the eighth, ninth, tenth, and eleventh, classes; and the *polypetalæ*, characterized again by the situation of the stamina, the insertion of which is here called *simply immediate*, as it *accidentally* varies at times into the mediate insertion, or in other words is found sometimes on the germen and at others on the corolla †, form the twelfth, thirteenth and fourteenth

* With respect to these distinctions, the most important in the arrangement of Jussieu, the reader (*μόνον* "Αρρην ἔστω) may consult Mr. Salisbury's "Observations on the Perigynous Insertion of the Stamina of Plants;" where he has undertaken to show that such perigynous insertion is entirely factitious, and that there is no instance whatever, in the whole vegetable kingdom, of stamina being inserted in the calyx.—V. *Trans. Linn. Soc.* vol. viii. p. 1.

† "Insertio immediata vel est *absoluta* in mediatam mutari nescia, dum corolla

fourteenth of his classes; his fifteenth and last being composed of diclinous or irregular plants, not properly reducible to any other head.

Independent, therefore, of the distinctions arising from the cotyledons, which, however well founded, have been shown to be of little practical utility, the system of Jussieu is the system of Tournefort; in which Jussieu has, it seems, discovered advantages resulting from the incidental connection between the stamina and the corolla, of which Tournefort himself was not aware*. It must also be observed that the primary distinctions of Linnæus extend at once through the twenty-four classes, whilst those of Jussieu, arising from the cotyledons, extend only to three; the secondary, founded on the corolla, form only three more; and the subdivisions of these by the *stamina* and *antheræ*, including the anomalous class of *Diclines irregulares*, form in the whole only fifteen classes, thus obtaining much less in point of distinction by four separate processes than Linnæus has obtained by one.

The consequence of this is, that there are on an average a much greater number of plants in each of the classes of Jussieu than in those of Linnæus. In order to designate these classes, Linnæus has recourse solely to the stamina, from the number, proportion, and situation of which he has formed all his distinctions, which he has comprised in one single expressive word, fully indicative of the grounds upon which the class is founded. Jussieu, on the contrary, in order to arrive at the distinctions of his classes, has taken a more circuitous path, and instead of referring to a single part, and defining it by a single word, has recourse to various peculiarities, as well in the mode of germination as in the fructification. Thus the compound flowers, forming a natural order, are designated by Linnæus by the term *Syngenesia;* whilst Jussieu denominates them

corolla supprimitur, ut in apetalis; vel est *simplex*, in mediatam *fortuitò* mutabilis, dum corolla existens non gerit stamina, et tamen ferre interdum potest, ut in plerisque polypetalis," &c.—*Juss. Gen. Pl.* p. 79.

* "Tria inde eruuntur signa primaria, ferè essentialia ac cæteris spectabiliora, jam in Tournefortianâ methodo feliciter adhibita, singula ter dividenda a situ staminum in apetalis et polypetalis, corollæ in monopetalis."—*Juss. Gen. Pl.* p. 80.

"On retrouve donc ici une des grandes divisions de Tournefort prise de la corolle, organe très secondaire en lui-même, mais qui, par son union avec un organe principal et essentiel *dont Tournefort n'avoit pas connoissance*, se trouve passer au premier rang."—*Extrait des Registres de la Soc. Roy. de Med. à Paris.*

But had Jussieu preserved a strictly natural method, he would have adopted the distinctions on the cotyledons, as suggested by Linnæus. In deserting these he has evidently fallen into an artificial one, having no connection whatever with the foundation on which his system is built.

Plantæ

Plantæ dicotyledones, monopetalæ, corolla epigyna, antheræ connatæ. To say nothing of the inconveniencies introduced into the science by the substitution of a long definition for an appropriate appellation, the consequence of this diversity in the two systems is in other respects important. The separation of the vegetable kingdom into classes is only one step towards an arrangement. The subordinate divisions of orders and genera require other distinctions. It becomes necessary, therefore, not to expend, as it were, in the formation of the classes those peculiarities which may be applied with so much effect, and which are indeed indispensable in the subordinate arrangements. Of this Linnæus was fully aware; and he has accordingly reserved for this purpose, not only certain particularities in the situation of the stamina, but the whole advantages arising from the corolla, calyx, and nectarium; and, what is of still greater moment, the distinctions dependent on the number and form of the style and stigma. Jussieu, on the contrary, has prematurely deprived himself of many of these distinctive characters, although from the greater magnitude of his classes he has greater occasion for them. Those which arise from the number of the petals, as well as the situation of the stamina, he has applied to the formation of his classes, and in some instances, as in his tenth and eleventh classes, has even resorted to the antheræ for these leading distinctions. The consequences of this will more fully appear by a brief comparison of these arrangements in their subordinate divisions.

[To be continued.]

XIV. *Reply to* Caleb Mainspring's *Observations on Prize Chronometers. By* A Correspondent.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN your Magazine for December last, there is a paper, signed "Caleb Mainspring," on the subject of Prize Chronometers.

As that paper contains some allusions to the Admiralty and the late Board of Longitude, I must beg leave explicitly to state, before proceeding further, that I am totally unconnected with either. It is not necessary to suppose the Admiralty to know any thing about science, but it is certain that the late Board of Longitude contained a number of the most eminently scientific men in the British dominions: but with neither of these points have I any thing to do.

My object is to expose the absurdity of the person's arguments

ments who has assumed the name of Caleb Mainspring. I am afraid this writer is either some disappointed chronometer-maker, who has failed in his attempts to gain a Greenwich prize, or else he is in "good sober truth" the mighty oracle of such a club as that which he himself describes, held at the "Tippling Philosopher in Liquorpond-street." If this latter is the case, as is most probable, he should be content to confine his nonsense within the walls of his club-room, where alone it will be received as gospel: in the present case I am afraid he has got himself into as bad a scrape as Paddy O'Rourke the Irish schoolmaster, who, not content with bearing away the palm from the King's excise-officer in a disputation on the cubic contents of a whiskey-still (which he had computed to the third decimal place), actually engaged in a contest with the parish priest of Kildrogan, which ended in poor Paddy being excommunicated.

But to come to the point: I hereby aver that the watch No. 2, in the paper referred to, is a much more perfect instrument for the purposes of navigation than was ever yet constructed by any maker; and that No. 1, or the chronometer as it is termed, only possesses that common degree of merit which is to be expected from such works at the present day.

With respect to the general merits of the rule given by the late Board of Longitude I have nothing to say, as our tippling philosopher has deferred giving us the original document till some future time; but it is certain that in the case of these two watches, that rule would award the prize to an instrument which has arrived nearer perfection than any one which has yet been made, in preference to one of ordinary merit only.

To prove this, let us examine the rates of the two watches, as given for sixty days: No. 2 has a constant mean rate, but is liable to a variation, the maximum of which is $3''{\cdot}8$; No. 1 has an accelerating rate, and is also liable to a variation in that acceleration of $1''{\cdot}2$. Now the gentleman from Liquorpond-street informs us, that these sixty days are fair specimens of what will take place, and that the two watches may be depended upon to go in this manner for twelve months. Now let us suppose the two watches sent to sea on the first of the sixty days for a voyage of six months, it is evident that at the end of that time, or at the end of any indefinite time, the watch No. 2 will never be in error more than the daily variation $3''{\cdot}8$, or less than one mile of longitude. It is to be hoped that by whatever rule the merits of watches are to be tried, our tippling philosopher will never wish for a better one than this for its proper purpose.

On the contrary No. 1, which has a constantly accelerating rate,

rate, must have been sent on the first of the sixty days to the vessel which it was to direct, with a less rate than 5″, at which it was then going. We will, however, take it at 5″; then with precisely the same daily increase, &c. in its rate, which is given it for the sixty days, it will at the end of six months be 2′ 50″·4 in error, or upwards of forty miles of longitude. What can our tippling philosopher say to this? If he has no better time-piece than this to direct his motions, he will in about six years be going to Liquorpond-street half an hour too late, at the risk of being fined a gallon of treble X.

In the rates for the sixty days as given, No. 1 would be in error at the sixtieth day 56″·8, or more than fourteen miles in longitude, without taking any notice of the variation in the rate; whereas No. 2, as before stated, would not have one mile error.

Should there be any opportunity of getting a new rate for these watches, the result will not be much more satisfactory. Let us take the chances from their going for the sixty days, and if we take new rates for every 15, 20, and 30 days, the watch No. 2 still maintains the superiority.

Rate for each 15 days.		Rate for each 20 days.		Rate for each 30 days.	
No. 1.	No 2.	No. 1.	No. 2.	No. 1.	No. 2.
+5·00	+6·84	+5·015	+6·795	5·26	6·80
5·52	6·76	6·080	6·905	6·70	6.80
6·547	6·44	6.845	6·700		
6·853	6.76				

In fact, No. 2 is invaluable, and No. 1 of no use except for short voyages. I have only to add that our Liquorpond-street friend ought to give us the "original document;" the subject is an interesting one: and although he says this document is "much too sublime for our vulgar ears," yet as it appears to be perfectly correct in its application in the present instance (the only one I believe ever brought against it), it is certainly to be presumed innocent until proved guilty.

January 3, 1830. F. K.

XV. Decas duodecima Novarum Plantarum Succulentarum; *Autore* A. H. HAWORTH, *Soc. Linn. Lond.—Soc. Horticult. Lond.—Soc. Cæs. Nat. Curios. Mosc.—necnon Soc. Reg. Horticult. Belgic. Socius: &c. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

HEREWITH I transmit you my twelfth Decade of New Succulent Plants, which consists of ten new species of DeCandolle's new and remarkable Natural Order of CACTEÆ, whose botanical characters are given in the third volume of his excellent *Prodromus Regni Vegetabilis.*

The

The native countries of these new plants are stated below throughout, and also their technical characters and definitions; with detailed descriptions and with due acknowledgements to those collections in which I have been obligingly permitted to make them from the living plants. These, it is hoped, will be found welcome to your botanical readers both at home and abroad; for I am happy to add, that every plant you have published from me, with due reference to your valuable Miscellany, (as quickly as time allows it,) enters and takes its appropriate place in those great records of botanical science, (now publishing on the Continent,) the vast works of Decandolle and Schultes;—the former in Natural Orders, and above mentioned; and the latter in Linnæan Classes, and called *Caroli a Linné, Systema Vegetabilium, Editio Nova.*

Perhaps no Linnæan genus of plants of similar magnitude has received so many additions to it, through the unceasing labours of modern research, (and which, in every direction, is extensively proceeding,) as his famous Genus Cactus. In his own last edition of the celebrated *Species Plantarum*, vol. i. p. 166, published in 1764, he enumerates, from every then existing source, only twenty-three species of *Cactus*, divided into four sections; and many even of those species he had never seen, having taken them up from the representations or accounts of others. And even the last edition of our classic *Hortus Kewensis* has but twenty-four.

But we may now behold, even in a state of successful cultivation, not fewer than *ten times as many.*

It is in consequence of this exuberant harvest, that the writer of this paper is now able to complete his twelfth Decade of *new* Succulent Plants, and all of the natural order *Cacteæ;* notwithstanding the new *Systema Vegetabilium* of Sprengel, in five very large octavo volumes. The *Melocacti* and *Echinocacti* of Link and Otto, with many plates, (which they have very kindly presented to the writer,) and the excellent *Prodromus Systematis Naturalis Regni Vegetabilis* of the great botanist DeCandolle; and on the present subject his still more recent and interesting *Revue de la Famille de Cactées*, with coloured figures, (which he has also kindly given to the writer,) and in which above forty new species of *Cacteæ* (all sent alive from Mexico to DeCandolle, by the writer's friend Dr. Coulter*,) are for the first time arranged, named, and described. In the last-mentioned publication too, every thing, whether old or new, pertaining to the Cactean natural order of plants, is extensively discussed, with the ability of an historian,

* Dr. Coulter went out as physician in the service of the Real del Monte Company, having been recommended to the Directors by some of the Officers of the Linnæan Society.

torian, the judicious patience of a philosopher, and the hand of a master: and all this with a degree of success that renders every innovation dangerous, every improvement hazardous, and every useful addition of very difficult accomplishment. I remain, Gentlemen,

Yours, &c.

Chelsea, Oct. 1829. A. H. Haworth.

Ordo Nat. CACTEÆ, *DeCandolle Prod. Syst. Veg.* 3. 457.

Cactoidea *Vent.* Cacti *Juss.* exclus. Grossul.
Opuntiaceæ *Juss. Dict.* exclus. Grossul.
Cactus *Linn. &c. &c.*

Tribus Opuntiaceæ, *Decand.—Semina* parietalia.

Genus Mammillaria *Nob. Synops. Succ. DeCand. &c.*—Cactus *Linn. &c.*

Cotyledones 2, parvæ. *Flores* parvi, diurni: fructus nudus; et (in paucis) pullulantes propagines ex mammillarum basibus; nunquam è spinarum fasciculis*, excepto fortassè in *M. viviparâ* Nutt. quæ exindè monstrosa appareat.

Suffrutices carnosi, absque axe ligneo, &c. ut in *Synops. Succ.* l. c.—*Decand. Prod.* &c. *Flores* albi, rosei, rubri.

fulvispina. M. (Globular tawny-spined) subrotunda: mam-
1. mis sub-13-spinosis: spinis sub-quatuor, cæteris plùs duplò longioribus, extimis horizontalibus.

Obs. Planta nunc viget in Regio Horto Kewensi, pomi mediocris magnitudine, sub-trientalis, floribus rubris majusculis, ferè ut in *M. rhadanthâ* Otto, at forsan majoribus, et spinis duplò longioribus; inferioribus albis trilinearibus totam plantam ferè tegentibus: sub-4-superioribus patulis, cæteris 2-3-plò longioribus, fulvis. *Flores* in hoc genere majusculi rubri apicales.

Habitat in Braziliâ. *Floret* Sept. St. ♄.

Genus Cereus, *Mill. Dict.* 1768. Cereus *Nob. Synops. Succ.* 1812. et *DeCand. Cat. Hort. Monspel.* 1813.

Cotyledones nullæ. *Corolla* magna, tubo longo squamato, è spinarum fasciculis oriens: fugax, plerumque nocturna.

Frutices aphylli carnosi, axe centrali ligneo durissimo, medullam includente, floribus albis rubris luteisve.

* Instead of these two words, I propose in future to employ the word *Spinarium* for the peculiar fascicles of spines in Cacteæ, for which some writers use the term *Area.*

* *Sectio*

* *Sectio* GRANDANGULARES, erecti. *Nob.*

magnus. C. (The great 12-angled) validissimus, simplex,
2. profundè sub-duodecim porcatus, spinis inæqualibus brevibus rigidissimis atris.

Habitat in Insulâ Dominicâ. ♄.

Obs. I have only seen the summit of this fine and very distinct species of *Cereus*, which is now growing in the nursery of Mr. Rolls, in the King's Road Chelsea, amongst a great number of other well-managed Cactaceous plants. Mr. R. procured it from the captain of a French vessel, who brought it from St. Domingo. It was then about a yard high, and has had large white flowers open night and day, about half a foot long. Its lower part decayed, and the summit only, which is now in fine health and in flower-bud, survived its arduous journey.

Descriptio. Exemplarium unicum solum vidi; durum læve perviride diametro 4-5-unciali, porcis ferè biuncialibus subacuatis; apice depresso; spinis variantibus usque ad 12 in singulo subdistanti fasciculo, validis sesquilineam longis; lanâ ordinariâ densissimâ inconspicuâ, spinis breviore.

ferox. C. (Fierce upright) oblongo-teretiusculus: costis sub-
3. octodecim: spinis divaricantibus fulvis densissimè tectis.

Ex Braziliâ introduxit Dom. Loddiges, in cujus eximio horto in caldario viget.

Caudex firmus nunc dodrantalis diametro sub-biunciali, atro-viridis, spinis extantibus intricatis horridis ferè tectus: harum sub-sex exteriores in singulo fasciculo; 4-5 mediocres, unaque centralis duplò longior seu uncialis et sexies crassior plùsve.

Prope *Cereum multangularem* locarem, sed ab eo longè nihilominùs distat.

** *Sectio,* PARVANGULARES, erecti. *Nob.*

Æthiops. C. (The black-spined) erectus obtusè sub-octangu-
4. laris: angulis brevibus: spinarum fasciculis lanâque brevissimâ centrali, omninò nigerrimis.

Habitat in Braziliâ. Sed in Regio Horto Kewensi, etiamque in Horto Chelseiano, cum ferè innumeris aliis Cactaceis facilè viget.

Cereo repando similis, sed adhuc longè humilior et simplex: perviridis. *Spinæ* sub-duodecim in singulo fasciculo, mediocres variè erectæ sive patentes, seu expansæ.

expansæ. Variat ab exemplo a Chili in Regio Horto Kewensi, adhuc gracilior, spinis perpaucis mox ad lucem nigris, sed per medium latissimè pallescentibus: nihilominùs mera varietas. Spinarum nigredine optimè ab omnibus distinguitur.

Obs. Amongst our gardeners, this singular plant, with its black spines and black beard-like terminal brush, will be called a Grim-the-Collier of Cerei; as has long since been our *Hieracium aurantiacum,* from its having a grime-coloured calyx.

*** *Sectio* REPENTES. *Linn.*

*Ramis ramulis*que articulatis, articulis alatim plùs minùs 3-4-angularibus parvispinosis; sæpiùs variè longeque scandenter radicantibus, inter rupes seu scopulos, vel insuper muros arboresve.

+ ERECTIORES, ramis sæpiùs 4-angularibus, et tunc minùs alato-angulatis.

setiger. C. (Small quadrangular) suberectus ramis paucis
5. 4-angularibus: setarum fasciculis sub-viginti setatis; setis 3-4-linearibus subæqualibus radiantibus pallidis.

Habitat in Braziliâ. In caldario, apud Dom. Loddiges, sine floribus viget.

Plantæ facies paululùm refert ad *Stapeliam asterias* (dictam), at fortè demùm longè elatior, angulis concaviusculis. Ad setarum basin sive inter eas lana brevis ordinaria extat, in singulo fasciculo. Prope *Cereum bifrontem* Nob. accedit.

+ PROSTRATIORES, floribus magis reptantibus sæpè speciosissimis.

undatus. C. (Great China triangled) magnus: validissimè scan-
6. dens triangulatim sulcatus: articulis majoribus sublobulato-crenatis, quandoque torquatis.

Habitat in Sinâ. Exindè Hort. Soc. Londini nuper introduxit; et in ejus nobili horto nunc facilè sine floribus viget. Simillimus *Cereo triangulari,* sed duplò major plùsve; ramis magis viridibus, magisque radicanter scandentibus, radiculis longioribus numerosioribus, et muros maximè adhærentibus, spinarum fasciculis ordinariis parvis. G. H. ♄.

Genus vel *Subgenus* EPIPHYLLUM. *Herm. Nob. &c.*—Cereus *Aliorum.*

Suffrutices ramis alato-compressissimis crenatis, carnosis, sed tenuibus. Cætera *Cereorum* vel *Opuntiæ.*

crispatum.

crispatum. E. (Curly-edged) ramis cuneato-oblongis undatis: 7. marginibus crispatim magno-crenatis.

Habitat in Braziliâ; nunc sine floribus viget in nobili horto Hort. Soc. Londini. St. ♄.

Suffrutex adhuc sesquipedalis, ramis dichotomis, ramulis glabris lætè viridibus, costis, venisque semi-erectis utrinque convexiusculis: *spinulis* ordinariis in crenarum axillis perpaucis vel incipientibus, quandoque ferè nullis omninò.

Genus OPUNTIA. *Tourn. Miller. Nob. &c.*—Cactus. *Linn. &c.*

Cotyledones duæ, multoties majores quàm in affinibus. *Flores* e spinarum fasciculis rosulares, absque tubo: per dies aperti: fructu fasciculatim setoso.

Frutices succulenti, ramis ferè semper compressis articulatis, junioribus crassis carnosis absque ligneo axe; plùs minùs minutim foliolosis; *spinis* (foliolis caducis sediformibus) *setis*ve fasciculatis axillaribus, in fasciculis distinctis regularibus, sæpiùsque in quincuncibus, in variis speciebus plùs minùs distantibus remotisve.

Sectio ANGUSTILOBATÆ, articulis sublanceolato-teretibus crassis, mox compresso-teretiusculis.

longispina. O. (Slender long-spined) articulis compresso-te-8. retiusculis: spinis purpurascentibus, aliisque minoribus fulvis, unâque tenui tereti antiquissimâ triunciali.

Habitat in Braziliâ? In horto amici Dom. Hitchin apud Nordovicum viget, absque floribus. Hoc nomen in *Cat. Hort. Berl.* sed omninò absque descriptione, et fortè alia est.

glomerata. O. (Long flat-spined) ramis cæspitosè confertis: spi-9. nis centralibus solitariis linearibus acuminatis utraque planis longissimis.

Habitat in Braziliâ, et in nobili horto Hort. Soc. Londini nunc sine floribus viget. St. ♄.

Obs. Planta tota ferè glomeratim hemisphærica est. Ramuli sublanceolato-teretes carne farctim crassi, subvirides, vix semunciam lati. Areolæ ordinariæ setis brevissimis densissimis uniformibus, unâque spinâ plùs minùs centrali corneâ, corneoque colore biunciali, vix flaccidâ, neque rigidâ, sed in arcum flexibili.

Tribus RHIPSALIDEÆ. *DeCand. Pr.* 3. 475. Semina axi baccæ centrali affixa. *DeCand.* l. c.

Genus RHIPSALIS. *Gærtn. Nob. DeCand. &c.*—Hariota *Adanson,* secund. *DeCand. in Rev. Cact.* 77.

Coty-

Cotyledones duæ breves obtusæ. *Calyx* 3-6-partitus brevis. *Petala* sex oblonga patula. *DeCand.* l. c.

Frutices americani parvi parasitici succulenti aphylli insuper præcipuè arborum truncos ramosve. *Rami* et *ramuli* articulati plerumque penduli, teretes, vel sæpiùs obsoletè striati, seu sulcatuli, sive incipienter 4-5-angulati, semper graciles et plùs minùs subfasciculatim dichotomi, dichotomiis semiexpansis, *setarum* exiguarum mox deciduarum fasciculi, ut in *Cereo*, incipientes, seu ferè obliterati, et quasi, in ætate præcipuè, evanescentes, earum areâ punctiformi sive cicatrice solùm relictâ; ut in *Bot. Mag.* tab. bona 2740, pro punctos veros delineata. *Flores* sessiles laterales minimi albi, et in *R. grandiflorâ* Nob. opuntiacei: *petalis* recurvis lutescentibus, et in hoc genere giganteis.

Cereuscula. R. (Small quadrangular) subflexuosè scandenter
10. radicans, articulata: ramis exiguis subfasciculatis quadrangularibus: setarum radiis interradiis longioribus criniformibus expansis.

Habitat in Braziliâ, et nunc viget in nobili horto Hort. Soc. Londini, St. ♄, adhuc sine floribus.

Suffrutex nunc pusillus debilis, *ramis* antiquioribus flexuosis, subfiliformibus teretiusculis, *Cereorum* modo valdè scandenter radiculantibus: *ramulis* subfasciculatim distinctis sive distantibus 4-5-angularibus lætè viridibus, lentisque ope albo *verè* punctulatis, setisque affinium parvis, fasciculatim gerentibus. *Setarum* radii sub-sex in singulo fasciculo, variè patentim vel horizontaliter, sive recurvo-aperientes, subsesquilineares, radiorum umbone fusco.

R. mesembryanthoidi Nob. similis; sed duplò altior, minùs aggregata magisque angularis; sed ante eam locarem.

Obs. Adanson's ancient generic name of *Hariota*, must however be restored to this group; or at least to the cylindric, smooth, unangled, pendulous part of it; as soon as it can be determined whether the species allied to my *Rhipsalis mesembryanthoides* do not form of themselves a distinct subgenus from Adanson's genus *Hariota;* which they probably do. I have never seen Adanson's book, or should, quite as a matter of course, have adopted his earlier genus *Hariota.* It is probable that both these names may be retained.

Obs. In *Burman's* edition of the excellent and faithful outline figures of *Plumier's Pl. Americ.* two gigantic *Cerei* are represented

presented on tab. 195. These two fine plants (as appears by a very careful examination of all the letter-press), DeCandolle, through oversight, somewhat confuses by the name of *C. fimbriatus* in the third volume of his excellent *Prodromus*, p. 464, (and cites *Lam. Dict.* i. 539, which I do not possess,) but makes no proper mention of the gigantic thorns of one of these remarkable plants. Wherefore it will be a great service to our science to re-describe *both* from Plumier's figures, as follows, after observing that some of the synonyms appear to be errŏneous.

CEREUS. *Miller, &c.*

grandispinus. C. (Tubeless, great-spined) octangularis: acu-
1. leis subtrientalibus, flore campanulato absque tubo; fructu globoso spinoso.

Cactus 8-angularis, spinis subulatis. *Plum. Pl. Am. t.* 195. *f.* 2.

Habitat in Dominicâ. ♄.

Maximus; obtusissimè angulatus. *Spinæ* fasciculatæ subulatæ, 12-13 in singulo fasciculo, radianter patentes. *Flos* subtriuncialis, *petalis* obtusè lanceolatis serratis, *fructu* fasciculatim spinuloso, diametro biunciali, nidulantibus numerosis seminibus.

serruliflorus. C. (Great slender-spined octangular) setis se-
2. muncialibus; petalis tubo quintuplò brevioribus; fructu subconico magno squamoso inermi.

Habitat in Dominicâ. ♄.

Maximus; obtusè angulatus. *Setæ* fasciculatæ subviginti, æquales, in fasciculis singulis remotis, radianter stellato-patentes. *Petala* angustè lanceolata serrulata vix uncialia, tubo 4½ unciali squamato, squamis lineari-lanceolatis integris erectis.

Cereus erectus octangularis. *Plum. Pl. Am. t.* 195. *f.* 1.

Obs. Cereo hexagono simillimus, sed in spinis longè numerosioribus, longioribus et tenuioribus, quoque in caudicis sulcis minùs profundis proculdubio differt. A priore anomalo *Cereo* in omnibus ferè recedit.

Obs. The progression of time and experience, and the present opportunity, enable me to amend materially, the definitions of the following *Cacteæ;* and thereby to identify them more completely than has heretofore been done.

MAMMILLARIA. *Nob.*

discolor. M. (Depressed two-coloured) simplex, bicolor; sub-
1. rotundo-depressa, apice (seu apicem versus) valdè discolorata.

Habitat fortassè in Americâ calidiore. St. ♄.—In caldario vigebat ante 1810 apud Dom. Loddiges. Posteà apud Dom. Vere, juxta Kensington; et periit ibi ante 1812.

M. simplex, plus duplò major, spinis nec rubris neque fuscis, sed "ni malè memini" fulvis flavescentibusque, uno colore toto apicem versus, altero inferiore. Species tam distincta quàm singularis. Non est M. discolor, *Decand. Rev. de Cact.* tab. 2, sed fortè minor. Plantam mortuam possideo. Cætera ignota vel oblita.

geminispina. M. (Long twin-spined). *Nob. in Phil. Mag.*
2. Jan. 1824.

Ab M. geminispina *De Cand. Rev. de Cact. pl.* 3, differt in multis; sed præcipuè in spinis intertextis, recurvo-radiantibus niveis, totam plantam eleganter tegentibus; harum duabus in singulo fasciculo, cæteris validioribus, subarcuantibus, apice nigris, et multoties (nec merè duplò) longioribus.

stellata. M. (Clustered hoary) irregulariter cæspitosa; spi-
3. nis radiantibus fasciculatis niveis pubescentibus tecta: inferioribus piliformibus: paucis supremis multo validioribus horizontalibus, apice stramineis.

Cactus stellaris *Willd. Enum.* 30. Mammillaria stellaris *Nob. in Suppl. Pl. Succ. p.* 72. Sed non est Mam. pusilla *De Cand. Rev. de Cact. pl.* 2. *f.* 1. Est Mam. stellaris *Sw. Hort. Suburb. p.* 171; sed figuram ibi citatam, *Lod. Bot. Cab.* 79, non investigavi.

lanifera. M. (The woolly) *Nob. in Phil. Mag.* Jan. 1824:
4. sed non est M. lanifera *DeCand. Rev. de Cact. pl.* 4. Planta nostra spinas longiores et difformes gerit, ut in descriptione nostrâ, l. c.

Melocactus.

communis. M. (Common Turk's-cap) subrotundus 14-angu-
5. laris: spinis corneis et cornicoloribus mediocribus brevibusve validissimis rectis.

Cactus Melocactus *Linn. Sp. Pl.* 1666.—*Ait. Hort. Kew. ed.* 1 & 2.—*Willd. Sp. Pl. t.* 2. 938.—*DeCand. Pl. Gr. t.* 112—et *Rev. de Cact. pl.* 6. synonymis paucis erroneis.—*Nob. in Synops. Succ. &c.*—Echino-Melocactus *Clus. Exot. t.* 92.—*Lob. Ic.* 2. *t.* 24.—*Adv.* 2. 377. *t.* 27.—*Brad. Succ.* 4. *p.* 9. *t.* 32.—Sed non est Melocactus communis, *Link et Otto Melocact. p.* 8. *t.* 11, qui spinis irregularibus, longioribus, tenuioribus, &c. gaudet.

ECHINOCACTUS, *Link et Otto. De Cand.* &c.—Cacti? 346. *Nob. Synops. Succ.* 173-4 et Cacti 1. 2. 3. *Nob. Suppl. Pl. Succ.* 73-4. 75.

Suffrutices succulentissimi subrotundi obovales vel oblongi; sæpè simplices multicostati; costis, crebrè et horridè regulariter fasciculato-spinosis. *Flores* diurni rosacei, sæpè sessiles, parvi, tubo brevi seu nullo: vel majores, tubo declarato, ut in *Bot. Reg. t.* 137. Cætera *Cerei.*

Subgenus naturale, inter *Melocactum* et *Cereum:* absque spadice, et (ut dicitur) dicotyledonatum.

recurvus. E. (Miller's broad-spined) subrotundus 15-angula-
6. ris, spinis latis recurvis creberrimis.

Echinocact. recurvus *DeCand. Prod.* 3. 462.—Cactus recurvus *Mill. Dict. ed.* 8. *No.* 3.—*Nob. Synops. Succ.* 173.—Cactus nobilis *Linn. Mant. Pl.* 243.—*Willd. Sp. Pl.* 2. 939. *Spreng. Syst. Veg.* 2. 494.—Non *Nob.*

Obs. Hæc species, à Cacto latispino *Nob. in Phil. Mag.* 1823, præcipuè differt fortè numero angulorum: et vix ab Echinocacto cornigero *DeCand. Rev. de Cact. p.* 36. *pl.* 7, nisi in majoris spinæ situ.

Obs. Linnæus should not have wilfully and knowingly changed Miller's prior and faultless name of *recurvus* for this plant: and to prevent the confusion it created, I restored it in *Synops. Pl. Succ.* in 1812, and gave the name of *nobilis* to the following plant, because it was so called in the celebrated Royal Garden of Kew, where it yet exists, but has not flowered.

nobilis. E. (Dense-spined noble) erectus, oblongus, multi-
7. costatus, spinis creberrimis validis rectissimis nigris horridè tectus.

Cactus nobilis. *Nob. Synops. Pl. Succ.* 174, et sic dictus in Regio Horto Kewensi: sed non *Aliorum.* Cactus reductus *Link Enum.* 2. 21. Cereus reductus *DeCand. Prod.* 3. 463.

Obs. Suffrutex ferè tripedalis diametro subquadriunciali, ("ni malè memini,") firmus. *Spinarum* fasciculi, inter se intricatim et valdè intertexti, radiis sub-duodecim nigris mox atris in singulo fasciculo, æqualioribus, et magìs erectis seu minùs expansis quàm in plurimis. Harum spinarum perpaucæ ferè unciales, et cæteris ferè duplò longiores validioresque. In juventute à Cacto gibboso *Nob. in Synops. Succ.* 173. vix distinguendus, at in ætate oblonga sub-tripedalis seu plusquam evadit; sed gibberum notabilem sub

 singulo

singulo spinarum fasciculo, ut in C.? gibboso *Nob. in l. c.* semper gerit. Hi duo fortassè sectionem novam constituunt, et *gibbiferum* nomen propono.

Obs. This plant is doubtless an *Echinocactus*, and to prevent confusion (as above-mentioned) ought to occupy the name of *nobilis*, although by some modern authors it has been changed. Such worse than useless alterations ever breed confusion. They blur the beauty of our flowery science, and clog its progress to perfection, in a needless way. No name however great, no authority however bold, nor any argument however ingenious, ought at this enlightened period to be encouraged, by adopting such wilful, useless, and unnecessary change.

parvispinus. E. (small-spined oblong) Cactus parvispinus
8. *Nob. Suppl. Pl.* 73.

Simillimus E. meonocantho, *Link et Otto Melocact.* 19-20, *t.* 15. Sed hic est suboblongus, spinâ unâ alterâve in singulo spinarum fasciculo subadunco sive recurvante.

Erinaceus. E. (The Hedgehog) *Nob. Suppl. Pl. Succ.* 74,
9. sub Cacto.

Simillimus est Melocacto polyacantho, *Link et Otto Melocact. p.* 12, 13. *t.* 16. *fig.* 4. Præcipuè differt angulorum numero, absentiâque plantis lanuginis apicalis. *Spinarum* fasciculi ferè similes.

Hystrix. E. (The Porcupine) Cactus Hystrix *Nob. Suppl.*
10. *Pl. Succ.* 73.

Obs. Hic est simillimus Melocacto Salmiano *Link et Otto Melocact. t.* 13. sed magìs oblongus, spinis longè paucioribus.

Genus CEREUS. *Mill. Nob.* in 1812. *DeCand.* &c.—Cacti pars. *Linn.—Willd.—Sprengel,* &c.

* *Sectio,* GRANDANGULARES, erecti, *Nob.*

hexagonus. C. (Great six-angled Surinam) simplex erectus,
11. maximus sæpiùs altè 6-costatus: spinarum fasciculis mediocribus.

Cactus hexagonus *Linn. Sp. Pl.* 1. 667.—*Willd. Sp. Pl.* 2. 940.—*Mill. Dict. ed.* 8. *No.* 1.—*Ait. Hort. Kew. ed.* 1 & 2.—*Bot. Repos. t.* 513.—*Nob. Synops. Succ.* 179. &c.

Cactus Peruvianus. *DeCand. Pl. Gr.* 58. et *Prod.* 3, 464.

Cereus surinamensis. *Eph. N. C.* 3. *p.* 402-3. *t.* 7. 8.

Cereus rectus maximus, &c. *Brad. Succ.* 1. *p.* 1. *t.* 1.

Obs.

Obs. Omnium maximus 40-pedalis; plerumque simplex; 5-6-7 variantibus angulis.

Cereus Peruvianus. *DeCand. Rev. de Cact.* pl. 17, est monstrosa varietas: etiamque Cactus abnormis *Willd. Enum. Supp. p.* 31.

peruvianus. C. (Great 8-angled Peruvian) erectus magnus,
12. suboctangularis: angulis obtusis lævioribus glaucis: spinis albicantibus.

Cactus peruvianus *Linn. Sp. Pl.* 667.—*Willd. Sp. Pl.* 2. 941. *Enum. Supp. p.* 32.—*Hort. Kew. ed.* 1. & 2.—*Nob. in Synops. Succ.* 171, sub Cereo.

Cereus eburneus *Pr. Salm. in Cat. Hort. Dyck.*—*DeCand. Prod.* 3. 464.—Cereus Americ. 8-ang. spinis albicantibus. *Brad. Succ. t.* 12.—Euphorbiæ arbor Cerei effigie. *Lob. Ic.* 2. *p.* 25. et *Morris. Ox. sect.* 17. *t.* 37.

** *Sectio* REPENTES. *Linn.*

quadrangularis. C. (Repent quadrangular) *Nob. in Synops.*
13. *Succ.* 181.

Cactus repens sub-quinque angularis. *Plum. Americ. ed. Burm. t.* 199. *f.* 1. Sed absque verbis ultimis descriptionis, quæ ad *Cereum grandiflorem* evidenter pertinent.

Flos, secundum *Burm.* l. c. nocturnus micans candidissimus, omniumque amœnissimus et suavissimus. Odorem præbet spectaculum.

bifrons. C. (Rooting quadrangular) *Nob. Suppl. Pl. Succ.*
14. excluso synonymo ibi dubitato *Willdenoviano.*

Obs. Adhuc apud Dom. Loddiges viget, et certissimè distinctus est à *Cereo speciosissimo,* ut non voluit *DeCand. Prod.* 3. 468. Longè humilior est et magis decumbens, magìsque scandenter radicans.

tenuispinus. C. (Long wool-spined) debilis ramosus subarticu-
15. losus facilè scandenter radicans: ramis gracillimis triangularibus: setis tenuissimis flexis sublaniformibus.—*Nob. Phil. Mag.* 1827.

Obs. Apud Dom. Tate, adhuc viget, scandenter repens, subpedalis, absque floribus. Omnium mihi notorum gracillimus. *Setæ* laniformes secus plantæ angulos in fasciculos confertos ordinarios inter se, fluxuosè intermixtos, angulos tegentes. In ramorum apice lana longè copiosior videtur. Fortè idem cum C. Myosuro. *DeCand. in Prod.* 3. 469. A.D. 1828.

Genus OPUNTIA.

Sectio CYLINDRICÆ. *DeCand.*

imbricata. O. (Imbricate stemmed) *Nob. Rev. Pl. Succ.* 70.
16. sub Cereo.

Est

Est *Cereus undulatus* hortulanorum. Simillima *Cereo cylindrico* (dubitato). *Nob. Synops. Succ.* 183: sed ultimus est robustior, elatior, et minùs ramosus.

In totâ sectione, foliola sediformia in junioribus ramulis majora et magìs conspicua quàm in plurimis Opuntiis; sed ut in illis citiùs decidua.

Obs. Opuntia moniliformis, *Plum. ed. Burm. t.* 198, novum *subgenus* constituit naturale. Globoso-articulata. *Flores* tubo Cerei; Opuntiarum limbo expanso rosaceo; unde bifrontem faciem gerit. SPHÆRARIAM nomen propono.

XVI. *Sketch of the* SYSTEMA GLOSSATORUM *of* Fabricius, *Vol.* 1. *From* Illiger's *Magazin für Insektenkunde*, vi. p. 277.

" La Mort l'a surpris" (Fabricius) " lorsqu' il allait publier le *Systema Glossatorum. Illiger* en a donné un extrait dans son *Magazin Entomologique.*" Cuv. Reg. Anim. iv. p. 117. 1^re^ edit.

WE believe that the *Extract* alluded to by Cuvier has never been published in our language, and that very few British lepidopterists know any thing of Fabricius's *Systema Glossatorum*, except by name. The original work has never appeared, and we are not aware that any other account of it has been given to the world than that by M. Illiger, at least Latreille, in 1811, knew of no other; for he says, " n'ayant connu cette nouvelle classification," (viz. *glossatorum*, " le dernier travail" de Fabricius) que par l'extrait qu'en a publié M. Illiger," &c. (Recueil d'Obs. de Zool. *Humb.* et *Bonpl.* i. p. 237.) We hope, therefore, that the following abridged translation of the article above-mentioned may be acceptable to our readers. Illiger has preserved the specific names of the *Entomologia Systematica*, which he says, in the later work (the *Systema Glossatorum*,) are frequently changed for others, chiefly derived from those of the plants on which the caterpillars feed; and he also expresses a doubt as to the stability of some of the following genera, many of which are founded on very minute distinctions, principally the hairiness of the palpi.

Genus 1. URANIA.—*Palpi* two, very long, triarticulate; second joint hairy. *Antennæ* setaceous, multiarticulate.
Type. PAPILIO *Leilus.* NOCTUA *Patroclus.* 7 species.

2. AMATHUSIA.—*Palpi* two, long, villose, triarticulate; second joint

joint longer, curved; third short, compressed. *Antennæ* filiform.

Type. Papilio *Phidippus.*

3. Papilio.—*Palpi* two, short, triarticulate; first joint hairy; second longer, incrassate towards the extremity, subcylindrical. *Antennæ* with an elongated club.

Type. * Trojans.

Papilio *Hector—Pammon.*

** Grecians.

P. *Brutus—Podalinus—Machaon.* 125 Species.

4. Zelima.—*Palpi* short, biarticulate; second joint rounded at the apex. *Antennæ* long, clavate.

Type. Pap. *Pylades.* 3 Species.

5. Morpho.—*Palpi* long, fimbriate, quadriarticulate; second joint very long, compressed, ciliated on both sides. *Antennæ* filiform.

Type. Pap. *Achilles—Menelaus—Hecuba.* 19 Species.

6. Cethosia.—*Palpi* two, long, triarticulate; second joint very long, deeply ciliated externally. *Antennæ* with an elongated club,—somewhat pointed at the apex.

Type. Pap. *Cydippe—Biblis*‖—*Penthesilia.* 7 Species.

7. Castina.—*Palpi* two, short, triarticulate; third joint very short, cylindrical, naked. *Antennæ* clavate; club attenuated towards the apex, and terminating in a somewhat curved point.

Type. Pap. *Icarus,* Cram.—*Cyparissias—Orontes.* 13 Species.

8. Euplœa.—*Palpi* two, short, triarticulate; second joint longer, third short, hairy, obtuse. *Antennæ* clavate, multiarticulate; last joint obtuse.

Type. Pap. *Plexippus—Similis—Corus.* 32 Species.

9. Apatura.—*Palpi* two, moderate, villose, triarticulate; second joint very long, pilose at the end; third conical, depressed. *Antennæ* clavate; club elongate-cylindrical.

Type. Pap. *Iris—Bolina—Alimena.* 14 Species.

10. Limenitis.—*Palpi* two, projecting, triarticulate; second joint longer, hairy, pilose at the end; third rather long, acuminated. *Antennæ* sub-clavate.

Type. Pap. *Populi—Niavius—Camilla.* 14 Species.

11. Cynthia.—*Palpi* two, long, triarticulate; second joint very long, posterior half pilose. *Antennæ* clavate; club compressed.

‖ Misplaced—probably an error of Illiger's. See Genus No. 13.

* Wings

* Wings caudate.

Type. Pap. *Arsinoë—Interrogationis.*

** Wings indented.

Type. Pap. *Œnone—Iatrophe—Cardui—Allionia.*
25 Species.

12. Vanessa.—*Palpi* long, very pilose, triarticulate. *Antennæ* clavate.
Type. Pap. *Io.—Atalanta—Urticæ—Levana.* 30 Species.

13. Biblis.—*Palpi* long, twice as long as the head, triarticulate; third joint nearly as long as the rest, nutant. *Antennæ* clavate.
Type. Pap. *Biblis—Leucothoë—Nauplia—Neærea.* 37 Species.

14. Hipparchia.—*Palpi* two, long, slender, compressed, fringed with long hairs towards the extremity, triarticulate; third joint short, incurved. *Antennæ* clavate, apex somewhat acuminated.
Type. Pap. *Hermione—Fauna—Mæra—Ligea—Epiphron—Galathea—Pilosellæ—Hyperanthus—Rumina.* 119 Species.

15. Neptis.—*Palpi* slender, triarticulate; third joint conical, very pointed. *Antennæ* short, clavate.
Type. Pap. *Melicerta—Aceris.* 21 Species.

16. Brassolis.—*Palpi* long, triarticulate; second joint longer, posterior half pilose; third joint obtuse. *Antennæ* clavate.
Type. Pap. *Sophoræ—Cassiæ—Obrinus.* 30 Species.

17. Paphia.—*Palpi* two, villose, triarticulate; second joint longer, curved, dilated on the inner side, towards the anterior extremity, ciliated; third joint short, roundish. *Antennæ* clavate.

* Wings bicaudate.

Type. Pap. *Jasius—Pollux.*

** Wings caudate.

Type. Pap. *Varanes.—Laërtes—Chorinæus.*

*** Wings dentated.

Type. Pap. *Medon.—Ursula.*

**** Wings entire.

Type. Pap. *Orion.—Itys—Antiochus.* 79 Species.

18. Melanitis.—*Palpi* two, long, villose, triarticulate; third joint compressed, roundish. *Antennæ* filiform.
Type. Pap. *Leda—Undularis.* 9 Species.

19. Argynnis.—*Palpi* two, triarticulate; second joint dilated on

on the inner side towards the anterior extremity. *Antennæ* clavate, club compressed, round.

* Wings dentate.

Type. Pap. *Paphia—Cynara—Cethosia—Aglaia.*

** Wings entire.

Type. P. *Liriope—Morpheus—Hermes.* 41 Species.

20. Thais.—*Palpi* two, slender, separate, recurved, quadriarticulate; fourth joint cylindrical, pilose. *Antennæ* short, clavate.

Type. Pap. *Hipsypile.* 1 Species.

21. Idea.—*Palpi* two, compressed, short, triarticulate; third joint very short, conical, acute. *Antennæ* filiform.

Type. Pap. *Idea.* 2 Species.

22. Doritis. *Palpi* two, short, slender, ciliated, pilose at the base, quadriarticulate; last joint small. *Antennæ* short, rather stout, clavate.

Type. Pap. *Apollo—Mnemosyne.* 4 Species.

23. Pontia.—*Palpi* two, long, triarticulate; joints nearly equal; third rather slenderer, conical. *Antennæ* long, clavate.

Type. Pap. *Cratægi — Rapæ—Daplidicæ—Elathea—Belia.* 94 Species.

24. Colias.—*Palpi* two, short, triarticulate; first and second joints nearly equal; third small, slender, acute. *Antennæ* short, clavate.

* Wings rounded.

Type. Pap. *Palæno—Hyale—Glaucippe.*

** Wings angular.

Type. P. *Rhamni—Cleopatra.* 35 Species.

25. Hætera,—*Palpi* two, long, slender, nearly naked, triarticulate; second joint very long. *Antennæ* filiform.

Type. Pap. *Piera—Diaphanus.* 16 Species.

26. Acræa.—*Palpi* two, long, ciliated, triarticulate; third joint small, naked. *Antennæ* clavate.

Type. Pap. *Horta.*

27. Mechanitis.—*Palpi* two, long, triarticulate; second joint longer, nearly naked; third joint prominent, conical. *Antennæ* filiform.

Type. Pap. *Calliope—Polymnia—Doris—Psidii—Phyllis.* 49 Species.

28. Libythea.—*Palpi* two, very long, projecting, compressed, triarticulate. *Antennæ* short, rigid, fusiform.

Type. Pap. *Celtis—Carinenta.*

29. Melitæa.—*Palpi* two, long, quadriarticulate; two last joints short, equal. *Antennæ* clavate; club flattened, obtuse.
Type. Pap. *Lucina—Cinxia—Cynthia—Maturna.* 15 Sp.

30. Helicopis.—*Palpi* two, very slender, naked, triarticulate. *Antennæ* clavate; club elongate, cylindrical.
Type. Hesperia *Cupido—Gnidus.* 2 Species.

31. Hesperia.—*Palpi* two, compressed, triarticulate; second joint very long, pilose; third joint cylindrical, naked.—*Antennæ* clavate.

* Wings tricaudate.
Type. Hesp. *Amor—Helius—Faunus.*
** Wings bicaudate.
Type. H. *Vulcanus—Marsyas.*
*** Wings caudate.
Type. H. *Bœtica—Æmon.*
**** Wings entire.
Type. H. *Thisbe—Æsopus—Prelus.* 108 Species.

32. Lycæna.—*Palpi* biarticulate; first joint ciliated externally; second joint cylindrical, naked. *Antennæ* clavate.

* Wings bicaudate.
Type. Hesperia *Mars—Echion.*
** Wings caudate.
Type. H. *Amyntas—Rubi.*
*** Wings entire.
Type. H. *Meleager—Arion—Corydon—Adonis—Ledi—Virgaureæ—Phleas.* 150 Species.

33. Erycina. — *Palpi* two, recurved, compressed, nearly naked, triarticulate; last joint small. *Antennæ* clavate; club slender, subcylindrical.
Type. Pap. *Melibœus—Lysippus—Orsilochus.* 11 Spec.

34. Myrina.—*Palpi* very long, recurved, triarticulate; third joint rather shorter, compressed. *Antennæ* fusiform.
Type. Hesperia *Alcides—Heleus.* 8 Species.

35. Thecla.—*Palpi* long, triarticulate; second joint longer, ciliated; third joint cylindrical, naked. *Antennæ* clavate.
Type. Hesp. *Betulæ—Spini—Quercus.* 8 Species.

36. Nymphidium.—*Palpi* two, long, biarticulate; first joint very long, slightly pilose. *Antennæ* subclavate.
Type. Hesp. *Caricæ—Telephus—Athemon.* 28 Species.

37. Danis.—*Palpi* very short, incrassate in the middle, biarticulate. *Antennæ* clavate.
Four new Species.

38. Emesis.

38. Emesis.—*Palpi* very short, approximate, triarticulate; third joint small. *Antennæ* clavate.

Type. Hesp. *Ovidius—Absalon.* 11 Species.

39. Thymele.—*Palpi* short, thick, triarticulate; second joint rather longer, distorted, pilose; third joint short, cylindrical, naked. *Antennæ* clavate, uncinate.

* Wings caudate.

Type. Hesp. *Proteus—Mercatus—Acastus.*

** Wings entire.

Type. Hesp. *Thrax—Gnelus—Bixæ.*

*** Wings rounded.

Type. Hesp. *Aracinthus—Malvæ—Tages.* 131 Species.

40. Helias.—*Palpi* long, projecting, very pilose, triarticulate; second and third joints almost equal. *Antennæ* clavate.

Type. H. *Phalænoides.* 1 new Species.

41. Pamphila.—*Palpi* two, biarticulate; first joint longer, hairy throughout. *Antennæ* short, clavate; club with a small recurved hook.

Type. Hesp. *Comma—Paniscus—Fritillum—Lavateræ.* 34 Species.

42. Laothöe.—*Palpi* two, pilose, very obtuse, biarticulate. *Maxillæ* very short, squamose, indistinct. *Antennæ* filiform, with the joints squamose beneath.

Type. Sphinx *ocellata—Quercûs—Tiliæ—Populi.* 21 Sp.

43. Sphinx.—*Palpi* stout, pilose, very obtuse, biarticulate. *Antennæ* filiform, squamose beneath.

* Wings indented.

Type. Sphinx *Ello—Tetrix.*

** Wings entire.

Type. Sphinx *Nerii.—Atropos—Euphorbiæ—Ligustri.* 74 Species.

44. Sesia.—*Palpi* two, short, stout, pilose, very obtuse, biarticulate. *Antennæ* clavate; club terminated by a slender hook.

* Wings indented.

Type. Sphinx *Œnotheræ.*

** Wings entire.

Type. Sphinx *Stellatarum—Fuciformis.* 18 Species.

45. Ægeria.—*Palpi* two, projecting, triarticulate; second joint longer; third joint shorter, conical, acuminated. *Antennæ* cylindrical, quadriarticulate; last joint long, slender, acuminated.

 Type

Type. Sesia *Apiformis—Ichneumoniformis—Vespiformis.* 19 Species.

46. Amata.—*Mouth* with projecting laminæ, the labia covered by the base of the maxillæ. *Palpi* very short, with only a single joint. *Antennæ* setiform.

Type. Zygæna *Passalis—Cerbera.*

47. Zygæna.—*Palpi* biarticulate; second joint longer, very pilose externally. *Antennæ* incrassate in the middle.
Type. Zyg. *Filipendulæ—Scabiosæ—Quercus.* 17 Species.

48. Glaucopis.—*Palpi* long, recurved, triarticulate; second joint longer, externally pilose; third joint compressed, naked. *Antennæ* setiform, pectinated.
Type. Zyg. *Argynnis—Pugione—Halterata—Infausta.* 65 Species.

49. Procris.—*Palpi* two, slender, recurved, arched, triarticulate; joints nearly equal. *Antennæ* cylindrical.
Type. Zyg. *Statices—Pruni.* 9 Species.

Illiger adds that the following species, which are contained in an appendix, were not referred by Fabricius to any of his new genera:—Papilio *Cenæus—Chremes—Hesperus—Mirus—Pentheus—Darius—Dædalus—Polymenus—Nerina.*

[Communicated by J. G. Children, Esq. F.R.S. &c.]

XVII. *On the Tides in the Port of London. By* John William Lubbock, *Esq. F.R.S. & L.S.**

In the Companion to the British Almanac for 1830, I have published a disquisition on the tides, in which I have endeavoured to show the connection between the theory of the tides and the facts as they are observed in the Thames at London. M. Bouvard compared the observations made at Brest with the results of Laplace's theory; but no other comparison of the kind has ever been instituted; which is highly remarkable in the instance of a phænomenon which is of daily occurrence and striking regularity, and where a vast mechanical power is perpetually exerted before our eyes. The tide-tables which have hitherto been published in this country, have been calculated by methods which are studiously kept secret. The first attempt, however, which has been made to reconcile theory with observation has produced tide-tables which will, I believe, be found as accurate as any of those calculated by empirical rules long tried, and probably often corrected.

* Communicated by the Author.

I believe

I believe many persons suppose that no theory of the tides can be expected to apply to places separated from the main ocean by long and narrowing channels, as is the case in the port of London. This notion will, I think, be removed, if it be considered that the condition which justifies the application of the theory to a river, is not that the tides should there be the same in time or in height as they are in the ocean, but that they should be *as regular* in the former as in the latter case; and that we may here reckon upon this degree of regularity *at least*. Such a situation is in fact, what Laplace considers in the case of Brest as a remarkably happy circumstance for the application of theory. "La situation de ce port (Brest) est très favorable à ce genre d'observations: il communique avec la mer par un canal fort vaste, au fond duquel ce port a été construit. Les irregularités du mouvement de la mer parviennent ainsi dans ce port très affaiblies: à peu près comme les oscillations que le mouvement irrégulier d'un vaisseau produit dans le baromètre sont attenués par un étranglement fait au tube de cet instrument."—*Mémoires de l'Institut*, 1818: p. 1.

As long as the bed of the river continues the same, the tide will be regularly transmitted to any point of it, modified only by an alteration of the constant quantities in the formula which the theory gives. This will be the case, even if we suppose the tide which we have here, to be compounded of two tides which arrive by different courses and after different intervals, as might very easily be shown to follow from the nature of the expression by which the height of the water at a given time is represented. The theory of the tides of Laplace in the second volume of the *Mécanique Céleste*, is one of the most splendid instances of his unrivalled skill in the application of mathematics to questions in physical science; and there can be no doubt that his theory will give the circumstances of the tides at the port of London. Laplace no where hints at any restriction which would exclude such a case, and accordingly it is included with others in the *Annuaire du Bureau des Longitudes.* It is however somewhat remarkable that the table which is given in that work has not undergone any alteration since the time of Bernouilli, who first gave it, or has ever, as far as I am aware, been compared with observation.

This being so, it must, I think, be considered a work of some interest to institute upon a sufficient number of observations a comparison of the theory with the facts; and this is what in the course of last year I executed with regard to the times

times of high water, employing for the purpose no less than nine thousand observations of those made at the London Docks in the course of twenty-five years.

It is found that the times of high-water calculated from the table given in the *Annuaire*, would differ widely from the observed time; but by applying the formula from which this table was deduced with alterations in the constant quantities, according to Laplace's theory, the mean of the observed times agrees with the time calculated, with an exactness which amounts almost to identity, as will be seen by the following table, which results from the expression,

$$\tan 2(\theta' - \lambda') = \frac{\frac{m\,P^3}{m'\,P'^3}\sin 2(\theta - \theta' - \lambda + \lambda')}{1 + \frac{m\,P^3}{m'\,P'^3}\cos 2(\theta - \theta' - \lambda + \lambda')}$$

in which equation P is the parallax of the sun.

θ ...... hour-angle of the sun, at the time of high water.

m ...... mass of the sun.

The same quantities dashed refer to the moon.

λ and λ' are constants depending upon the local circumstances of the port, and their values for the London Docks deduced from all the tides observed by day during twenty-five years are, $\lambda' = 1$ hour 32 minutes, $\lambda - \lambda' = 2$ hours.

The logarithm of $\frac{m\,P^3}{m'\,P'^3}$ deduced from the same observations is 9·52452.

If, according to M. Damoiseau (*Mémoires des Savans Etrangers: Théorie de la Lune*), vol. i. p. 502, the mass of the moon be equal to the mass of the earth divided by $\frac{1}{75}$, and the moon's mean horizontal equatorial parallax be 3431″·73, page 569; and if, according to Laplace, *Exposition du Système du Monde*, page 12, the sun's mean parallax be 26″·54, or 8″·59; and if, according to the same work, page 209, the mass of the earth be equal to that of the sun divided by 354936 log. $\frac{m\,P^3}{m'\,P'^3} = 9{\cdot}62170$. The value of the same quantity deduced by Laplace from the observations of the tides at Brest, is 9·62833. The column headed "Observed," is the mean of about nine thousand observations; in which, as they were made at all seasons of the year, the variations in the declinations of the sun and moon and the equation of time can have no perceptible influence. The inequality of *variation* in the moon's parallax

parallax might influence the result; its effect is difficult to determine exactly, but it must be nearly insensible.

Time of the Moon's Southing.	Time that the Moon's Southing precedes the Time of High-Water.			
	Observed.		Calculated.	
0	2^h	2^m	2^h	0^m
1	1	47	1	47
2	*1	32	*1	32
3	1	18	1	17
4	1	5	1	4
5	*0	55	*0	55
6	0	52	0	54
7	1	3	1	6
8	1	32	1	32
9	1	59	1	58
10	2	9	2	10
11	2	10	2	9

As this coincidence has been misunderstood, I may remark that the calculated times were made to correspond with the mean of the observed times in *two places only;* namely, in those which correspond to the moon's southing at two and five hours: their near agreement *throughout*, proves that the form of the expression from which the calculated times were deduced, is correct. Their coincidence in the first of these points amounts to inferring from the observations the values of the constants λ and λ'; their coincidence in the second amounts to inferring the value of the fraction $\frac{m\ P^3}{m'\ P^3}$ from the observations, the difference would be altogether inconsiderable if the received values of the quantities m, m', P, and P$'$, had been adopted. This mean result is not affected by the neglect of the equation of time which is perhaps perceptible in the table of page 66, in the Companion to the British Almanac.

The observations seem to indicate a mass of the moon greater than that which has been deduced from the phænomena of precession and nutation; but to determine so delicate a point, the observations must be discussed and examined with greater care, and the effect of the inequality of *variation* in the moon's parallax considered.

I regret that I was misinformed with respect to the manner in which the *time* was obtained. It was not taken from Rotherhithe

therhithe church, as I stated in the Companion to the British Almanac, but frequently from the clock of Wapping church, which is close to the pier-head; but this point is of no consequence whatever.

It may perhaps interest some of your readers whose notice has been drawn to this subject, to see a comparison between the calculation and the fact, as far as the time has yet gone. M. Dessiou's calculations begin with the 1st of January: thirteen days of that month only have now elapsed; and the shortness of the time is of itself a serious obstacle to any exact agreement of theory and observation; for such an agreement is to be looked for only in mean results deduced from many comparisons. The accidental causes which influence particular tides are so many and so obscure, and the mode of observation so inexact, that I do not conceive that either these tide-tables, or any other, can be free from deviations from the noted time, which may often amount to a quarter of an hour. It appears by the following register and calculations, that, as far as the observations have yet been made, the mean time of high-water at the London Docks has differed by less than four minutes from the tables in the British Almanac.

I am well aware that the practical application of this subject requires considerable caution, and that the observations which it would be desirable to make, are by no means so simple as might at first be imagined. The difficulty arising from the insensible change of the height of the tide just at the maximum, which produces what is called the hanging of the tide, is of an obvious nature. It may not perhaps be so obvious that the rise and fall before and after the maximum are by no means symmetrical; but this I understand from Captain Eastfield is the case, under whose superintendence a most valuable series of observations has been undertaken at the East India Docks, but which are not likely to be continued for any great length of time. In a country like ours, where both from scientific and commercial considerations the subject of the tides is so well worth attention, it is not too much to hope for the assistance of Government in procuring at least one series of accurate observations continued for many years.

When the mode of observing the time of high-water has been rendered as accurate as it ought to be and might easily be made, and when a series of observations have been made on such an improved plan, we may endeavour to introduce greater exactness into our tables; but at present the agreement is more than sufficient for practical purposes, and the mean error is within the limits of the errors of the observations.

Com-

Comparison of Tides observed January 1 *to* 13, 1830, *with the Tide Tables in the British Almanac.*

1830.	A.M.			P.M.		
	Observed.		Calcul.	Observed.		Calcul.
	London Docks.	St. Katherine Docks.	British Almanac.	London Docks.	St. Katherine Docks.	British Almanac.
Jan. 1	6^h 15^m	6^h 8^m	6^h 1^m	6^h 45^m	6^h 32^m	6^h 26^m
2	7 10	7 8	6 52	7 30	7 47	7 22
3	7 50	8 10	7 54	8 40	8 50	8 30
4	9 0	9 8	9 8	10 0	10 5	9 49
5	10 20	10 30	10 29	11 5	11 5	11 6
6	11 35	11 40	11 41	0 0	0 0	0 0
7	0 15	0 15	0 11	0 30	0 25	0 39
8	1 10	1 8	1 6	1 20	1 20	1 29
9	2 0	1 53	1 51	2 5	2 15	2 11
10	2 35	2 39	2 31	2 45	2 38	2 49
11	3 15	3 16	3 7	3 35	3 40	3 25
12	3 45	3 48	3 41	4 0	4 13	3 58
13	4 20	4 25	4 14	4 45	4 50	4 29
	69 30	70 8	68 46	63 0	63 40	62 13
		Equation of time	1 24		Equation of time.	1 24
			70 10			63 37

The tide-tables in the British Almanac are calculated to apparent time, as are all the tables in the same publication, as the observations are in the mean time, the mean error

$$\text{A.M.} = \frac{70{\cdot}10 - 69{\cdot}30}{13} = 4 \text{ minutes nearly}$$

$$\text{P.M.} = \frac{63{\cdot}37 - 63{\cdot}0}{13} = 3 \text{ minutes nearly.}$$

XVIII. *On a new Salt obtained by the partial Decomposition of Perchloride of Mercury.* By R. Phillips, *F.R.S. L. & E., &c.*

THE action of chlorides and water, as a theoretical subject, has perhaps occupied more than a requisite degree of attention: it has however again lately excited my notice, and the experiments suggested by it have led to the formation of a mercurial salt which has not, I believe, hitherto been described.

Although when some chlorides,—those, for example, of antimony and bismuth,—are mixed with water, we have demonstration of the production of muriatic acid and a metallic oxide by their mutual decomposition; yet the supposition of similar action is sometimes attended with considerable difficulty, and especially when the base of the chloride has but slight affinity for oxygen, as in the instances of gold and mercury.

With respect to perchloride of mercury, if it does not become bipermuriate by solution in and decomposition of water, it appears to me that the excess of chlorine should destroy vegetable colours: but this is so far from being the case, that litmus paper is reddened by the solution precisely as it is by any acidulous salt. It occurred to me also, that if the salt remains a perchloride in solution, we might probably even in that state, by abstracting half the chlorine, reduce it to protochloride or calomel, as in the dry way, by the addition of mercury.

It is however well known that potash, soda, and lime-water precipitate peroxide of mercury from a solution of corrosive sublimate; but as the action of the carbonated is in many cases different from that of the caustic alkalies, and as they decompose some bisalts without converting their bases into carbonates, I added carbonate of lime to a solution of corrosive sublimate, and heated the mixture: to my surprise a dark-coloured precipitate was soon formed, and I supposed, at first, not only that an atom of chlorine had been separated, but that the carbonate of lime had occasioned the deposition of protoxide of mercury, by extending its action to the protochloride formed,—an effect which it is not perhaps generally known that it is capable of producing to a certain extent. On examination I found that the action which had occurred was very different from what I had imagined; the precipitate was crystalline, extremely heavy, and of so dark a colour that the larger crystals appeared nearly black. It was very sparingly soluble even in boiling water, and scarcely at all in cold, so that the hot solution deposited crystals on cooling. It was readily dissolved by acids, and the solution in acetic acid yielded peroxide of mercury by potash, and chloride of silver by the nitrate.

To ascertain whether the whole of a given portion of corrosive sublimate could be decomposed by carbonate of lime, I boiled equal weights of these substances in water for a considerable time: on adding potash to the solution, an abundant precipitate of peroxide of mercury was obtained, showing that a portion of the mercurial salt remained undecomposed, although the carbonate contained lime enough to saturate nearly three times as much muriatic acid as the chlorine of the sublimate could yield. It appears therefore that counteraction takes place as long as the muriate of lime formed remains in solution, and complete action is prevented, just as the presence of the resulting sulphate of potash hinders the total decomposition of sulphate of barytes when it is boiled in a solution of carbonate of potash. I found also that the solution by

evaporation

evaporation yielded a crystalline deliquescent salt, composed of the muriates of lime and mercury, but in what proportions I have not yet determined by experiment, though from the nature of the dark-coloured salt formed, it would be easy to deduce them.

In order to examine the nature of the salt precipitated by carbonate of lime, and first to ascertain whether it contained any water, a portion which had been moderately dried was put into a glass tube and heated in a salt-water bath. No moisture was observed; the salt suffered no change of appearance or loss of weight. It therefore contains no water of crystallization. At higher temperatures it yields water and corrosive sublimate, and afterwards mercury and calomel.

The quantity of chloride of silver which the salt would yield, was determined by dissolving 100 grains in acetic acid; solution of nitrate of silver being added, gave a precipitate which weighed 27·5 grains; and on repeating the experiment 28 grains were obtained, giving a mean of 27·75 of chloride of silver, equivalent to 7 grains of muriatic acid.

One hundred grains of the salt were boiled in a solution of potash; the peroxide of mercury precipitated when dried weighed 93·5 grains. Allowing for the errors of experiment, the salt then appears to be composed of

Muriatic acid........................	7·
Peroxide of mercury..............	93·5
	100·5

If we consider this salt as a dipermuriate of mercury, it will consist of

One atom of muriatic acid	37	or	7·8
Two atoms of peroxide of mercury...	432	or	92·2
	469		100·0

It will be observed that the oxygen in the peroxide of mercury amounts to 4 atoms, while the muriatic acid contains only 1 atom of hydrogen; and as these are not in the proportions required to form water, it is I think quite evident that the salt in question is an anhydrous muriate, and not a chloride. I am confirmed in this opinion by a remark of Dr. Thomson's on muriate of gold, which I have seen only since I had arrived at a similar conclusion with respect to the salt in question. In the Transactions of the Royal Society of Edinburgh, vol. xi. p. 28, Dr. Thomson mentions a muriate of gold composed of

Two atoms of muriatic acid	9·25
One atom of peroxide of gold	28·
Five atoms of water	5·625
	42·875

He

He observes also that Berzelius has lately maintained that muriatic acid is incapable of combining with metallic oxides; consequently, that no muriates exist, but merely chlorides, or compounds of chlorine and the metal, united to a certain quantity of water.

"But considerable difficulty," Dr. Thomson observes, "will be experienced in applying this reasoning to the muriate of gold. If this salt be a chloride, it is obvious from the experiments stated that it is composed of

Two atoms of chlorine	9
One atom of gold	25
	34"

With respect to the permuriate of tin noticed in the same paper, Dr. Thomson remarks, not only that the oxygen and hydrogen which it contains, could not form water, but he adds, "Nor supposing the salt to be a chloride, could any reason be assigned why the tin is thrown down by an alkali in the state of peroxide rather than protoxide." So also with respect to the dipermuriate of mercury now described; we cannot imagine it to be a chloride, for it contains three more atoms of oxygen than are required to constitute water with the hydrogen: and on the same account we cannot explain why potash, supposing even that it yields oxygen and becomes a chloride, should precipitate peroxide instead of protoxide of mercury. As then the dipermuriate of mercury could not be formed either by the decomposition of water, occasioned by the intermediate action of the alkali, or by the transfer of its oxygen, as when protochloride of mercury is decomposed by potash,—I conclude that corrosive sublimate in solution exists as a bipermuriate of mercury, becoming so immediately by the decomposition of two atoms of water. From a solution of bipermuriate thus ready existing, we may conceive any subsalts to be formed by the action of an alkali; but, for the reasons already given, the alkali cannot, it appears to me, occasion either the decomposition of water, or supply the oxygen requisite to the formation of the peroxide.

In concluding, I shall mention a few circumstances respecting the preparation of the new compound. It is well known, that when only a moderate quantity of lime-water, or of solution of potash, is added to one of corrosive sublimate, that a reddish-brown precipitate is at first occasioned, which subsequently assumes a yellow colour by an excess of the precipitant employed.

I attempted to form the dipermuriate by decomposing a solution of corrosive sublimate both with carbonate of soda and with lime-water, employing such proportions of each as would detach

detach the required quantity of muriatic acid: the precipitates obtained in both cases were reddish-brown, not at all crystalline, much less dense than the dipermuriate, and contained scarcely 6·5 per cent of muriatic acid. From all these circumstances I have no doubt that both products were mixtures of dipermuriate and peroxide of mercury.

The colour, density and crystalline appearance of the dipermuriate of mercury depend on the mode in which it is prepared. When hard carbonate of lime, such as Iceland spar, is put into a solution of corrosive sublimate, months are required to obtain even a few crystals of the salt; these are nearly black, and adhere strongly to the carbonate: powdered marble acts more readily, and without the application of heat. By this method I have produced the most regular crystals, and they may be separated by elutriation from all admixture of the undissolved carbonate: some which I procured were sufficiently large to exhibit distinct rhombic planes. Precipitated carbonate of lime acts more rapidly than powdered marble, but still the effect is slowly produced unless the mixture be heated: it is difficult, however, to procure the dipermuriate without some admixture of carbonate of lime; and its colour is much lighter, and the crystals are smaller than those obtained by slower action.

By the following process I obtained the dipermuriate perfectly free from all admixture:—Dissolve 1 atom = 272 grains of perchloride of mercury in water, heat the solution; and add to it 648 grains = 3 atoms of peroxide of mercury, obtained either by decomposing the pernitrate with heat, or corrosive sublimate with potash; I prefer the latter on account of its state of minute division. When the mixture is boiled, a dark precipitate soon begins to form, and eventually nearly the whole is converted into the dipermuriate; a small portion of corrosive sublimate, yielding about 6 grains of peroxide of mercury, remains undecomposed, and of course an equivalent quantity of peroxide is diffused through the dipermuriate formed; this is easily separated by boiling the precipitate with about 100 grains of muriatic acid diluted with water.

The dipermuriate thus procured is perfectly black, dense, crystalline, and frequently very brilliant. The salt may also be procured possessing similar appearance, by heating peroxide of mercury in a quantity of muriatic acid, less than that required for converting it into corrosive sublimate.

While the foregoing paper was printing, I observed that in M. Gay-Lussac's Lectures (vol. i. lecture 17), he mentions the formation of subchloride of mercury, by mixing the perchloride

chloride with peroxide, and by some other processes. He adds, however, that it has never been analysed:—from the last-mentioned circumstance, as well as some general views offered in my communication, I have not thought proper to suppress it, although it contains less novelty than I had imagined.

XIX. *Suggestion of a New Method of Trial for the Chronometers at Greenwich. By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

HAVING perused in your valuable Journal the remarks of Caleb Mainspring on the System of Prize Chronometers at Greenwich, and having also noticed the Proposal of another trial number suggested by J. L. T. as published in the last Number of your Journal, I am induced as a chronometer-maker to address this letter to you upon the subject; hoping that my observations may prove useful to those who have any interest in the matter. The Admiralty, by awarding premiums for chronometers, has proved how much might be effected by the encouragement of science, as will be seen by comparing the rates of the chronometers for the first year's trial with those of the subsequent ones. But when the Admiralty, in their wisdom, offered premiums for the best chronometers after one year's trial at the Royal Observatory, they of course did not intend that the inferior chronometer should hold the place of its superior; but such, however, is the case by the present system of deciding the prizes, as the remarks of Caleb Mainspring clearly show. The perfection of a chronometer, in my opinion, consists in maintaining its rate with uniformity, and not irregularity; and the more steadily a chronometer goes, the greater is the proof of its being perfect in the different parts of its construction: and I consider that a chronometer which varies but little from its rate, but which goes at an irregular pace, is not equal with one that varies a little more, but which goes more regularly. By the present method of deciding the prizes, the chronometer may be the reverse of what I have stated, as will be seen by the example given by Caleb Mainspring, published in your Journal for December last. But now, whilst I find fault with the present system, I consider myself called upon to suggest another, such as would remedy the evil complained of; and therefore I propose that the variation from the mean monthly rate be not doubled, as is now the case, but that it may be left single, and added to the mean extreme variation, which would give the true error of the chronometer;

nometer: and in adopting this method, the limits might be reduced to 4 seconds for the first premium, $4\frac{1}{2}$ for the second, and $5\frac{1}{2}$ for the third. Why the error from the rate has always been doubled, I am at a loss to conceive. And now I ask, what would have been the result of the trial as exemplified by Caleb Mainspring, in which he makes his own common watch to beat his master's chronometer? According to my proposal it would have been as follows:

My Master's Chronometer.

		″
Greatest mean monthly rate		6·70
Least ditto..........................		5·26
	Difference......	1·44
Greatest extreme variation in April = 1″·20, in May = 1·20	Mean=	1·20
	Trial number	2·64

My own Common Watch.

		″
Greatest mean monthly rate		6·80
Least ditto		6·80
	Difference......	0·00
Greatest extreme variation in April = 3″·80, in May = 3·80	Mean =	3·80
	Trial number	3·80

And so would have been the reverse of what he made it by the present method of ascertaining the trial number. I have now suggested what I consider to be the best mode of deciding the quality of a chronometer. With respect to the proposal of J. L. T., it appeared to me that it would not be so practicable, nor perhaps so good a test, as the one I have made; as, on examining his system, an idea entered my mind, that if, instead of taking the mean rate of the whole time of the trial as a standard rate for deducting each day's difference, the mean rate of the first month was taken as that given rate, the result would not be a bad test. But after trying the result upon two chronometers that had equally varied from their rates, and had also gone with the same regularity, I found that the result of the trial would be in favour of that chronometer which went nearest to the first month's rate during the first part of the trial, though it varied at the latter end: but by taking the mean of the whole time as the given rate, the result was in favour of that which varied from its rate at the commencement of the trial, but which made no greater error afterwards. As I consider those two chronometers equal in quality,

quality, I do not approve of the system; and moreover, I think that a chronometer which makes a great extreme variation in any month's rate, would not be so much affected by it. I do not see the necessity of squaring the differences as proposed by J. L. T., but I think it would be very laborious; as for exact calculations the mean rate must be given to the hundredth part of a second.

Having now suggested my system for deciding the quality of a chronometer, I leave it to the investigation of those who are willing and competent to decide upon its merits. Trusting my observations may prove useful, I remain

Your most obedient servant,

January 20, 1830. R. J. M.

XX. *Notices respecting New Books.*

On the Theory of the Small Vibratory Motions of Elastic Fluids, by J. CHALLIS, *M.A., Fellow of Trinity College, and of the Cambridge Philosophical Society**.

THE following is the introduction to Mr. Challis's memoir, from which the reader will obtain a correct view of the importance of the investigation which it contains.—" Any one that has given much attention to the mathematical theory of sound, will be aware that notwithstanding the labours of the most eminent mathematicians, great obscurity is still attached to it. Much of this obscurity, I have been led to think, is owing to the manner in which *discontinuous unctions* have been introduced into the subject; and as geometers of late have been more engaged in the use of them than in scrutinizing the evidence on which they rest, I will endeavour to state, as briefly as possible, the nature of this evidence. It depends, I believe, almost entirely on the authority of Lagrange, and on his two dissertations contained in the first and second volumes of the Miscellanea Taurinensia. His first Researches, however admirable in other respects, cannot be adduced in reference to the point before us, because that part of them which bears upon it, contains a step in the proof which can by no means be admitted. In fact, it mainly depends on the sum of the series $\cos\theta + \cos 2\theta + \cos 3\theta +$ &c. *ad infinitum*, which he determines to be always equal to $-\frac{1}{2}$. And in truth, if the exponential expressions be put for the cosines, and the series be summed to infinity, this result is obtained. But the objection is, that a mode of summing a converging series is applied to one which is not convergent. The only legitimate method is to sum the series to m terms, and to find what the sum becomes when m is infinite. Lagrange does this; he finds the sum to be $\frac{\cos m\theta - \cos(m+1)\theta}{2(1-\cos\theta)} - \frac{1}{2}$, and says, that the first term disappears when m is infinite, because

* From the Transactions of the Cambridge Philosophical Society.

the

the 1 may be neglected in comparison of m. But it cannot be admitted that two arcs, however great, which differ by a quantity θ, have the same cosines independently of the value of θ. The fallacy of the reason assigned for neglecting 1, will be apparent, by putting the sum of the series under this other form,

$$\frac{\cos \frac{m+1}{2}\,\theta \sin \frac{m\,\theta}{2}}{\cos \frac{\theta}{2}},$$

which does not give the same result as before, when 1 is neglected in comparison of m. I have adverted to this error, because in consequence of it, Lagrange exhibits to view a discontinuous function, the possibility of doing which, may well be called in question. It is not necessary to inquire how the reasoning may be conducted, if this step be corrected, because the second *Researches* are in principle the same as the first, and are not liable to a similar objection. In these he has elaborately, yet strictly shown, as far as I have been able to follow the reasoning, that the motions he is in search of, are not subject to any law of continuity;—that the motions, for instance, at a given instant, in a column of fluid stretching between two given points, cannot be given generally by any known line or function. He supposes, therefore, that they will be given, by a *new* set of functions, neither algebraical, transcendental, nor mechanical, but discontinuous *per se,* and by this property of discontinuity distinguished from every other. This definition has been admitted by all subsequent writers. But it deserves to be considered in what sense, and to what extent an investigation of this nature can demonstrate any property of functions. The science of quantity is a perfect science; it needs not the aid of any other, and exists prior to its applications to questions of nature, and independently of them. When in the applications, any form or property of functions is arrived at by the operations that are performed, it will always be possible to arrive at the same, by abstracting from the physical question, and performing the same operations by pure analytical reasoning. For in the applications, we are, in general, concerned about time, space, force, and matter,—ideas of a totally dissimilar kind, but possessing this in common, that we can conceive of them as consisting of parts, and in virtue of this common quality, after establishing a unit for each, we are able to express their observed relations numerically, or by lines or letters the representatives of numbers. All subsequent reasoning is then conducted according to the rules of analysis, and cannot possess a greater generality in regard to the modes of expressing quantity, than the operations conducted by those rules admit of. If an attempt be made to prove the existence of discontinuous functions by pure analysis, it will be impossible to succeed, because, as Lagrange says, "the principles of the Differential and Integral Calculus, depend on the consideration of variable algebraical functions, and it does not appear, that we can give more extent to the conclusions drawn from these principles, than the nature of of these functions allows of. But no person doubts that in algebraic functions, all the different values are connected together by the law of continuity." (*Misc. Taur.* tom. i. p. 21.) Accordingly, no discontinuous function can be *exhibited to view.* The inference to be drawn with certainty from Lagrange's reasoning is, that if a number of particles, constituting a line of fluid, are in motion, the line which bounds the ordinates erected at every point, proportional to the velocities at a given instant, is not necessarily regular. It may consist of portions of continuous curves, connected together at their extremities, and be expressed analytically by a function, which possesses no distinctive property of discontinuity, but changes form abruptly and in a manner always given by the data of the problem to be solved. But if he had limited himself to this inference, and not supposed the existence of a new order of functions, he could not have determined the velocity of sound, and must have confessed that the analytical theory had not succeeded in solving that problem. For the demonstration he gives of it, rests altogether on the existence of discontinuous functions, such as they are above defined: and herein it differs entirely from Newton's solution of the same problem, which requires no new property of curves or functions, but deduces the velocity directly from the constitution of the medium:—a method, which certainly at first sight appears the more natural. As, however, we are sure that the velocity of the propagation of sound, must be a deduction from the principles on which the analytical investigation is founded, if no other mode of making the deduction can be thought of, we must be content to take up with discontinuous functions. No person can object to them who does not supply an equivalent, provided always they be considered in the present state of analytic science, not as demonstrated to exist, but as hypothetical, and like all hypotheses, established only by the extent and success of their applications. It was necessary to premise so much as this about discontinuous functions, in order to give a reason why any one, who treats of the vibrations of an elastic medium, has a right, if he can, to leave these functions out of consideration; and that the best possible argument for their non-existence is, to show how to do without them.

In the dissertation that follows, I have reasoned as if all functions were *per se* continuous; and setting out with this principle, have discussed the integrals containing arbitrary functions, prior to any supposition about the mode in which the fluid was put in motion; considering that as the investigation which led to these integrals was conducted without reference to any such supposition, and as they are consequently applicable to every point in motion, all inferences drawn from such discussion, must also apply to every point in motion. This method of treating the subject, dispenses with that of D'Alembert and Lagrange, who consider the differential equation of the motion, to be equivalent to an infinite number of equations of the same kind as itself, each of which applies to a single point. The first inference drawn from this manner of reasoning on the motions in space of one dimension is, that every point

point is moving in such a manner, as results either from a motion of propagation in a single direction, or two simultaneous motions of propagation in opposite directions. The velocity of the propagation is determined, and is, for air, the quantity commonly obtained by theory for the velocity of sound. Again, it is shown that the forms of the functions are not entirely arbitrary, but limited by the nature of the question to a certain species, the primary form of which corresponds to the curve that occurs in Newton's reasoning, and by writers on the theory of vibrating chords called the *Taylorean Curve.* As any number of these curves will simultaneously satisfy the partial differential equations, it is inferred that the vibrations they indicate, may *co-exist.* If any portions of these curves, or of the curves resulting from the combination of any number of them, be joined together at their extremities, and so form an irregular line, every two consecutive ordinates of which differ by an insensible quantity, as this line will satisfy the same differential equations, it indicates a possible motion, which is consequently of that bizarre and irregular kind, which Lagrange first demonstrated to be the general character of the vibrations. The particular form, however, of this line is given, when the particular mode of the disturbance which caused the motion is given. I have endeavoured to exhibit as clearly as possible, the mechanical reasons of this kind of motion.

In the next place, the bearing of the theory on the musical sounds produced in tubes, is briefly considered, and particular attention is paid to the mode in which the air vibrates in a tube open at both ends, because on this point, the view I have taken, leads to an inference which is at variance with the received theory.

The equation which gives the motion in space of two dimensions is integrated approximately, and the approximation is shown to be such, that the integral will apply with accuracy to almost all cases that can occur. Euler's integral of the equation that applies to the motion in space of three dimensions, which has ever since his time been considered to be particular, is here shown to be the proper general solution, and adequate to solve all the cases of small motions. This view of it is justified by its application to some problems of interest, particularly to oblique reflections, and the problem of resonances. In conclusion, I have stated as a result of the whole preceding investigation, the manner in which analysis points out the laws of any phænomena, the theoretical inquiry into which conducts to the solution of a partial differential equation.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 28.—THE following papers were read:—Experiments on the Influence of the Aurora Borealis on the Magnetic Needle; extracted from Letters from the Rev. James Farquharson to Captain Sabine.—On the Production of Regular Double Refraction in the Molecules of Bodies by simple Pressure; with Observations on the Origin of the doubly refracting Structure. By David Brewster, L.L.D. F.R.S. Lond. & Ed.

GEOLOGICAL SOCIETY.

Dec. 18.—Benjamin Blake, Esq. Captain in the Bengal Army; Matthias Attwood, Esq., M.P., of Gracechurch-street, London, and Muswell-hill, Middlesex; James Hall, Esq. of Southampton-street, Russell-square; and Thomas Clement Sneyd Kinnersley, Esq. of Essex-court, Temple,—were elected Fellows of this Society.

M. J. J. D'Omalius D'Halloy, &c. &c. Governor of the Province of Namur, in the kingdom of the Netherlands, was elected a Foreign Member of this Society.

A paper was read entitled "Observations on part of the low countries and the north of France, principally near Maestricht and Aix-la-Chapelle;" by William Henry Fitton, M.D. F.G.S. &c.—The general structure of the country on the confines of the Netherlands and France has been described, several years ago, by M. D'Omalius D'Halloy; and various memoirs, since published by other persons, confirm his statements. The basis of the whole tract consists of the coal-measures, with subjacent shale, grit, mountain-limestone, reddish sandstone and conglomerate, and finally transition-slate. Above this series of highly inclined beds, other strata, unconformable and nearly horizontal, repose; which, in the Boullonois, include the upper part of the oolitic groups; but, in advancing eastward, descend no lower than the green-sands. The country therefore is analogous to the vicinity of Bristol and Bath; but the overlying formations there go down to the lower oolite, lias, and new red sandstone.

The object of the author's inquiries was, to determine what beds are found, in the tract which he examined, above the coal; and how far they agree with their equivalents in England. He describes in succession the several strata: the list including, in a descending order,—1. Beds above the chalk;—to which are referred,—2. The stone and calcareous sands of Maestricht.—3. White chalk, passing into the Green-sand formation,—which comprehends.—4. Fire-stone, with—5. Green and ferruginous sands.—6. Obscure traces of clays beneath the sands. The whole being unconformable and superior to—7. The coal-measures, &c. &c.—The paper is accompanied by lists of the fossils, examined and named by Mr. Sowerby; and by a sketch of a general map, with sections on a larger scale.

1. *Beds above the chalk.*—The Crag, of Suffolk, &c. is stated, on the authority of Mr. Warburton, to have been observed on the French coast between Calais and Cape Blanc-Nez; near Antwerp; in the neighbourhood of Tongres; and at other places in the Netherlands. The fossils also of Klein-Spawen between Tongres and Maestricht, include, along with several shells of the calcaire-grossier, some of those found in our crag.

The sands which immediately precede the chalk, along the road from London to Dover, precisely resemble those in the same situation, on the line from Calais through St. Omer, Cassel, and Lille, &c.: the prominent hill of Cassel, however, is not topped with clay, but seems to consist entirely of sand, including very numerous fossils, contained principally in loose concretional beds of stone. These fossils,

fossils, many of which are the same with those of similar sands near Brussels, agree, in general, with those of the London-clay; and thence it would appear, that the separation of that stratum from the sands immediately incumbent on the chalk is not well founded. Beds of the sands here referred to occur, in the same geological place, in Kent; near St. Omer; at Cassel; at Mount-Panisel, and Ciply, south of Mons; at Brussels; between Charleroi and Fleurus; and at Kleyn-Spauwen, between Tongres and Maestricht.

2. *Maestricht stratum.*—Between the deposition of the sands last mentioned and of the chalk, a considerable interval must have elapsed; during which various beds may have been deposited, of which no trace, or but obscure remains exist, at present, in England. The well known stratum of St. Peter's-Mount near Maestricht is one of these: it is throughout superior to the white chalk, into which it passes gradually below, but the top bears marks of devastation, and there is no passage from it to the sands above. The siliceous masses which it includes are much more rare than those of the chalk, of greatei bulk, and not composed of black flint, but of a stone approaching to chert, and, in some cases, to calcedony:—and of about fifty species of its fossils in the author's collection, about forty are not found in Mr. Mantell's catalogue of the chalk fossils of Sussex*. The author therefore, with Mr. Hony†, and Mr. Conybeare‡, regards this bed as differing from, though intimately connected with the chalk.

A very fine section of the Maestricht bed is visible on the sides of the valleys of the Meuse and of the Jaar; and in the heights opposite to Visé the bed gradually rising from Maestricht disappears, and is succeeded by white chalk with flints. The section of this stratum, and all the accompanying circumstances, at Ciply, south of Mons, accord remarkably with those of Maestricht; and from M. Desnoyer's statements, a bed of the same description seems to exist also in the Cotentin.

3. *Chalk.*—The thickness of this stratum in the Netherlands is much less than on the coasts of the Channel; especially of the part containing flints, which is succeeded, in descending, by chalk without flints, passing into marl, and thence into fire-stone and green-sand. The white chalk is well seen at Wonck and Heur le Romain opposite to Visé; and, on the north of Aix-la-Chapelle, a remarkable group, which the author refers to the lower part of the chalk, consists of hard beds of grey and cream-coloured limestone, alternating with calcareous sand. This stratum, which abounds in fossils, many of them belonging to the lower chalk of England, has been found at a considerable depth at Cawenberg on the north-west of Maestricht; it is prominent in the well known quarries of Cunroot, on the east of Fauquemont, and caps the heights on the north-west of Aix-la-Chapelle, from Schneeberg to the west of Laurensberg; a small outlying portion remaining also on the top of the Louisberg, near Aix. A stratum like this is mentioned by Mr. Forschammer as

* Geol. Trans. 2d Series, III. 201.

† Geol. Trans. II. 310. ‡ Outlines, p. 63.

occur-

occurring in a similar place below the chalk of Denmark, on the shores of the Baltic; and seems also to exist in the Cotentin.

4. *Green-sand formation.*—The marly chalk is succeeded by the equivalent of our upper green-sand, or fire-stone (the *Pläner-kalk* of Germany,) in some places identical with that of Surrey, Kent, and Wilts; and like that stone is employed exclusively in constructing the interior of furnaces and buildings under water: extensive quarries for these purposes being worked at Königsberg opposite to Vaels, on the confines of the Prussian and Dutch territories. In this country however, there is not, beneath the fire-stone, (or at least does not distinctly appear,) a stratum of clay, like our Gault; but the chalk, becoming gradually charged with green particles, passes, in general without an intermediate valley, into green and ferruginous sands, obviously analogous to the lower green (or Shanklin) sands of England.

5. These sands are well exhibited in the hills on the south-west of Aix-la-Chapelle, and, extending beyond the chalk, occupy a large portion of the surface above the coal and mountain-limestone country. Distinct sections of the stratum are seen on the sides of Louisberg close to Aix, and along the road from that city to Liege,—the scenery of which resembles that of the Woburn sand-hills; and on the descent of this road to the Calamine Works, near Moresnet, beds are found in the sand, analogous to the fuller's-earth of Woburn and of Nutfield, in Surrey. The fossils which abound in this formation include (along with many species common to them and the superior beds, and hitherto not found in England) some species almost characteristic of our lower green-sand; among which may be mentioned the Trigonia aliformis, and Rostellaria Parkinsoni. The sands, at the Louisberg, include a thin bed of lignite; and near the bottom of the formation at Gemenich, and thence along the foot of the hills to Eynatten, a remarkable stratum of grit from 6 to 10 feet in thickness, of great firmness and uniformity, occurs,—resembling in its characters the grey-wether stone of England, &c. and possibly the equivalent of some of those beds of conglomerate which occur in our green-sand, (the Bargate stone of Surrey, &c.) though differing from them in external character. The ferruginous sands of Grafenberg and other hills on the east of Dusseldorf, belong also to this formation, containing the same fossils as at Aix-la-Chapelle, and occupying a similar unconformable position above beds of limestone; a striking section of which is visible on the banks of the Dussel, at Neanders-Höhle. The sands extending from thence to the north and eastward into Germany, are there well known under the denomination of Quader-sandstein.

6. In some places, the more ancient strata come in beneath the green-sands without any intervention; in others, there are indications of intermediate beds of clay, but too indistinct to admit of ascertaining their relations.

7. The coal-formation and other subjacent beds are not considered in the present paper; the author referring for an account of them to the works of local geology already published or in progress; and to the paper on the Environs of Bristol, by Messrs. Buckland and

and Conybeare, (Geol. Trans. 2nd series, vol. i.) for a description of the analogous portions of England, which may perhaps remove some of the difficulties connected with the corresponding formations in the low countries.

The chief points of difference then, between the formations above referred to and their equivalents in England, are—1. The apparent identity of the fossils in the sands above the chalk, with those which appear in the clay of London.—2. The Maestricht stratum, distinctly superior to the chalk, and differing from that bed in its fossils and other characters, is without any equivalent yet ascertained in this country; but some facts are mentioned, which show that the former existence of such a stratum above our chalk is not improbable, and that further traces of it may still be discovered upon sufficient search.—3. The hard beds (of Cunroot, &c.) form a remarkable feature of the lower chalk in the country above described.—4. The absence or indistinctness of the Gault, is one of the principal circumstances distinguishing the green-sand formation from ours; and the want of a valley, like that which commonly exists in this country along the foot of our chalk-hills, is an important difference of external feature.—5. The entire absence of the formations which, in the south-east of England, succeed the green-sand (the Weald clay, Hastings-sands, and Purbeck strata), deserves also to be mentioned; for, of these beds, though so fully developed on our coast, none have yet been distinctly recognised upon the continent, and traces only detected in the interior of England and in the lower Boulonnois.

In conclusion, the author remarks upon the great diversity of the upper and unconformable formations which, in different places, are in immediate contact with the older and inclined strata beneath. In some cases (as near Bristol) the red marl, lias, and lower oolite;—in others (lower Boullonois) the upper oolite; in others still, the green-sands, the gault, and even the chalk itself,—are in contact with the coal strata. It may be difficult to explain the cause of this variation, and to account for the absence of the beds which are wanting; for the upper formations bear no obvious marks of disturbance, and are generally horizontal or very little inclined.

Jan. 1, 1830.—The Rev. J. Henry Coddington, of Trinity College, Cambridge, was elected a Fellow.

A paper was read, "On the Geology of the shores of the Gulf of La Spezia; by Henry Thomas De la Beche, Esq. F.G.S. F.R.S. &c."

The chief objects of this memoir are to show,

1st. That the marbles of Porto Venere, although possessing some of the characters of transition rocks, may be the equivalents of part of the oolitic series.

2nd. That the diallage rock and serpentine of Southern Liguria have been protruded through the former at a period later than their formation.

Previous to his description of the geological structure of the district, the author gives a short sketch of its physical outline and superficial covering. The Alpi Appuani, or mountains of Massa and Carrara, form a distinct group, being separated from the main range of the Apennines, by a considerable depression, and from the hills of

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La Spezia by a plain through which the Magra flows. The plain is covered by gravel rising to some height above the Mediterranean. Of this gravel the banks of the Frigido afford a good section. Near Ponzo, between La Spezia and Borghetto, a torrent cuts through a hill composed of large rounded boulders and gravel, the coherence of which is trifling. These boulders could not have been produced by any causes at present existing in the district. The boulders are carried down the bed of the torrent but a short distance beyond the places where they occur as component parts of the hills. In the bed of the Vara, into which this torrent flows, there is gravel of the usual size, which may have been formed, and afterwards cut through, by the river.

Stratified Rocks.—1. *a.* Lignite, clay, sandstone and conglomerate, are described as being seen in vertical strata at Caniparola, near Sarzana, the shaly beds containing the Fucoides intricatus (Ad. Brongn.) and the conglomerate being made up of compact limestone, macigno sandstone, and jasper, cemented by clay. These tertiary beds are supposed to have been thrown into their present vertical position by the forces which elevated the adjoining Alpi Appuani.

1. *b.* Breccia, with a porous limestone cement, is one of the youngest rocks in the gulf of La Spezia, where it occurs in promontories, and caps some of the cliffs :—from its resemblance to the rauchwacke of the zechstein, it has been erroneously referred to that formation.

1. *c.* Siliceous sandstone is connected with the breccia above-mentioned, with which it is associated in contorted beds at St. Terenzo. The author does not pronounce positively upon the relative ages of the rocks of this group, although he asserts that they are all younger than the macigno.

2. *Macigno.*—Two sandstones of somewhat the same mineralogical structure, but of very different age, are comprehended under this name by the Italians ; but the author here restricts the term to that which is highest in the order. The macigno is a brown and gray sandstone, both calcareous and siliceous, generally micaceous, with black specks, and is occasionally mixed with shale. It occurs near the Bagni di Lucca overlying gray compact limestone, which ranges from thence into the district under consideration, and has similar relations near Massa and Carrara, details and diagrams of which are given ; it is also much developed north of La Spezia, and on the right bank of the Magra. In the absence of organic remains, the author has not been enabled to decide upon the equivalent of this rock.

3. *Gray compact limestone or Porto Venere marble.*—At La Spezia this group consists of, 1. Dark gray, black and yellow limestones, interstratified with schists and argillaceous slates ; 2. Dolomite ; 3. Dark gray compact limestone in thin beds ; 4. Ditto with brown shale, and containing Orthoceras, Ammonites, Belemnites, and round balls of iron pyrites ; 5 and 6. Shale, with compact thin-bedded limestone, resembling that of the Jura. The islands of Tino and Palmaria are composed of this system, whence it rises into the high land of La Castellana, and extends to Pignone, forming the mountains of Coregna, Santa Croce, Parodi, and Bergamo. The dolomite occupying the

the centre of this range presents the appearance either of an included bed, or of a great dyke which throws off the strata on either side. The fossils of Coregna collected by the author (first noticed by Guidoni) are,

Orthoceras :—A species resembling O. elongatum of the lias, and also O. Steinhaeri of the coal measures.

Belemnites (many alveoli of).

Ammonites :—15 species, one of which is the A. erugatus of the Yorkshire lias (Phillips's Geol. of Yorkshire); and another resembles A. Bucklandi; whilst two are fossils of the coal-measures, viz. A. Listeri and A. biformis. The remainder are undescribed, but have been drawn by Mr. J. Sowerby to illustrate this memoir. From the nature of these organic remains, and principally from the presence of belemnites, the author, whilst admitting the conflicting nature of the evidence, similar to that observed in parts of the Alps described by M. Elie de Beaumont, inclines to the belief that this range of limestone, &c. is equivalent to the lias or some member of the oolitic series.

4. Brown shale and variegated beds are seen beneath the gray limestone; and again, below the variegated strata, there is a considerable developement of brown sandstone and gray schist, which constitutes a high range extending from La Castellana to beyond Vernazza, wherein a large Fucus is found. This gray schist at Monte Rosso seems to have been penetrated by diallage and serpentine rocks.

Saccharine limestone, &c. of Capo Corvo.—The coast section of Capo Corvo exhibits thick and thin beds of gray limestone alternating with schists; a thick-bedded fine conglomerate which passes into chlorite and micaceous schists; and saccharine limestone of various colours with mica schist; the whole in highly inclined and contorted positions. Similar rocks occur between the mouth of the Magra and Ameglia, where they are covered by the gray limestone, and contain a subordinate conglomerate very much resembling that of the Valorsine. The author is disposed to refer this group to the same age as the older conglomerates which occur between the high Alps and their calcareous zones on the side of Italy.

Carrara Marbles.—These seem to form part of the system of gneiss and mica schist of the adjoining Alpi Appuani, being distinctly stratified and underlying the gray limestone, resembling that of Porto Venere.

Gneiss and mica schist are well exposed in the valley of the Frigido near Massa.

Unstratified Rocks: Diallage Rock and Serpentine.—The author observed no traces of stratification in these rocks throughout Southern Liguria, and he coincides with the views of those who consider them to have had an igneous origin. In the Valley of Cravignola serpentine and diallage rock traverse gray limestone and schist, and in one part are in contact with jasper rock, which, as is noticed by M. Brongniart, rests upon contorted limestone and schist. Between Monte Rosso and Vernazza the schists are much disturbed, and near Capo Mesco, and again at Levanto, diallage rock and serpentine passing into each other are protruded from beneath highly inclined beds of sandstone, in which are also many faults. These serpentine rocks seem to be prolongations of the great developement of the same system in Southern Liguria; and, to illustrate more fully their nature, the author gives a section of their relations in a contiguous district at Monte Ferrato, where, as has already been noticed by M. Brongniart, gray compact limestone and slaty shale and jasper are covered by serpentine and diallage rocks, which, in one place, seem to traverse and cut through the strata.

In conclusion, the author observes, that if the Porto Venere marbles be considered equivalent to any part of the oolite formation, they afford a striking example of the little value of mineralogical structure as a character taken by itself, and show the extreme caution that should be used in assigning names to rocks from hand specimens, brought home by distant expeditions, without the accompaniment of organic remains. He considers that the diallage rock and serpentine of Southern Liguria, have been intruded among these rocks subsequent to the epoch of the oolite formation; and regards the diallage rock and serpentine as of igneous origin, concurring in opinion with those geologists who attribute to these rocks in common with granite and trap, and the forces that ejected them, the contortion and fracture of the stratified rocks, and their consequent elevation into ridges and mountains.

LINNÆAN SOCIETY.

Jan. 19th—An Account was read of the Mode of Growth of Corals of the genus Fungia. By Mr. Samuel Stutchbury, A.L.S.

The writer observed, when collecting specimens near Tahiti, that though the larger ones were unattached, in the young state they are fixed to rocks or dead fragments of coral, growing on a foot-stalk, and remaining attached till about an inch in diameter, when they separate.

A communication was also read: On a new species of Wild Swan, taken in England, and hitherto confounded with the Hooper. By William Yarrell, Esq. F.L S. &c.

To this new species, which Mr. Yarrell names *Cygnus Bewickii*, after the celebrated artist who so greatly promoted the study of natural history by his works, he gives the following character: " C. rostro semicylindrico atro, cerâ flavâ, corpore albo, caudâ rectricibus 18, pedibus nigris." Mr. Yarrell's previous suspicions that a species distinct from the Hooper existed, occasioned by an inspection of several preparations, were confirmed by an examination of three specimens of different ages which he obtained in the markets of the metropolis, where they were brought during the late severe weather. The new species is one-third smaller than the Hooper at the same age. It differs much more in anatomical structure than in external character, and principally in the trachea, a minute description of which in all the specimens is given.

ROYAL

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Sixteenth Annual Report of the Council.

In presenting the Sixteenth Annual Report, the Council cannot but congratulate the Society on its increasing interest and improvement: they have great satisfaction in stating, that since the last anniversary meeting, a considerable addition has been made to the Museum, by the erection of a new Cabinet, and that the metallic and earthy minerals are now completely arranged.

The Cabinet, which was removed from the Museum, has been placed in the apartment lately occupied by the Penzance Library, and a portion of it already appropriated to Cornish geology, consisting of rock specimens illustrative of the geology of several districts of the county.

The Council have much pleasure in informing the Society, that several sections of the map of Cornwall, presented last year by George S. Borlase, esq. have been transmitted to the following gentlemen, who have kindly undertaken to lay down the geology, &c. of the different districts to which they refer: W. M. Tweedy, Esq.; Mr. Henwood; Mr. Petherick, Lanescot Mine; G. B. Kingdon, Esq. of Stratton; and Mr. Mitchell, of Breage. Any other gentlemen who are inclined to assist in this most interesting labour, may be supplied with sections of other districts of the county by applying to the secretary, who will also furnish them with the instructions sent with the sections already circulated, that a uniform plan may be observed.

Some further communications on the Stream Works of Cornwall have been received this year; but as it is highly desirable that all possible information should be obtained on a subject of such importance, both in a philosophical and geological point of view, the Council again take the liberty of impressing it strongly on the attention of those members who reside in the neighbourhood of these deposits.

The donations of minerals this year have not been so numerous as on many former occasions; but those which have been received are interesting, and a considerable acquisition to the Cabinet; particularly a series of specimens from the silver lode in Dolcoath mine, presented by Captain Petherick; a specimen of gold imbedded in micaceous iron stone, from Coçaes mine, Brazil, by G. C. Fox, Esq.; and several specimens of Elba iron, by Mr. Thomas Morgan. In addition to the above minerals, a very interesting series of organic remains, both animal and vegetable, with Wood-tin, &c. from Happy Union Stream Work, at Pentuan, near St. Austle, has been presented by Mr. John W. Colenso; in illustration of a paper which will be read before the Society at this meeting.

The Council have to lament the absence of one of the greatest ornaments and most efficient members of the Society, who is prevented from attending the meeting by extreme illness; but they sincerely hope that he will ere long be again enabled to fill the high situation he has held in the Society from its establishment. Every person present must anticipate the individual alluded to, and regret his absence:

sence: the individual is Mr. Joseph Carne, a gentleman to whom the Society is not only indebted for many of its most valuable communications, but for the uniform and strenuous support he has always given it.

The Council cannot close their Report, without calling the attention of the meeting to the severe loss which they and the whole scientific world have sustained during the last year, by the deaths of two of the most eminent honorary members of the Society, Dr. Wollaston, and their renowned and illustrious countryman, Sir Humphry Davy, Bart. (By Order) E. C. Giddy,

October 16th, 1829. Secretary.

The following papers have been read since the last meeting:—1. Some observations made in Cornwall, in the summer of 1829. By John Hawkins, Esq. F.R.S. Honorary Member of the Society.—2. Description of Happy Union Tin Stream Work at Pentuan, in the parish of St. Austle, Cornwall, with some observations thereon. By Mr. J. W. Colenso.—3. On the use of iron among the early nations of Europe. By Thomas Hingston, M.D. Librarian of the Society.—4. Geological notices in Italy. By the Rev. Samuel J. Trist.—5. Observations made at St. Michael's Mount. By John Hawkins, Esq.—6. Appendix to a paper, on the Tin Stream Works of Cornwall. By Wm. J. Henwood, F.G.S. Member of the Society.—7. Notice of pseudo-morphous crystals of tin, found in Wheal Coates, St. Agnes. By Stephen Davey, Esq. Member of the Society.

At the anniversary meeting, held on the 16th Oct. 1829, Davies Gilbert, Esq. M.P. P.R.S. &c. &c. *President*, in the chair;—the Report of the Council being read, it was resolved, That it be printed and circulated.

Officers and Council for the present year:—*President:* Davies Gilbert, Esq. M.P. P.R.S., &c. &c.—*Vice-Presidents:* John Hawkins, Esq. F.R.S.; Lieut. General Tench; John Samuel Enys, Esq.; Robert Were Fox, Esq.—*Secretary:* E. C. Giddy, Esq.—*Librarian:* Thomas Hingston, M.D.—*Treasurer:* Joseph Carne, Esq.—*Assistant Secretary:* R. Moyle, Esq.—*Council:* Thomas Peel, Esq.; Thos. Bolitho, Esq.; Samuel Borlase, Esq.; George Harvey, Esq.; G. C. Fox, Esq.; Wm. Millett Boase, M.D.; Richard Fox, M.D.; Stephen Davey, Esq.; Rev. C.V. Le Grice; John Armstrong, Esq.

David Brewster, L.L.D. F.R.S. &c., and the Rev. W. V. Vernon, M A. F.R.S. President of the Yorkshire Philosophical Society, were elected Honorary Members of the Society.

XXII. *Intelligence and Miscellaneous Articles.*

M. ALDINI'S INCOMBUSTIBLE DRESS FOR FIREMEN.

At the request of M. Aldini, a commission was named by the Academy of Sciences, at Paris, composed of MM. Fourier, Dulong, Chevreul, Flourens, and Gay Lussac, to examine the dress constructed by him for the protection of firemen from the action of the flames, and to report upon it. M. Aldini, in consequence, explained to these gentlemen the principle of his invention, and invited them to witness some experiments at the station of the firemen

men in the Rue de la Paix, in the presence of the civil authorities and several other individuals.—The following account is principally taken from the Report upon those experiments, made by M. Gay Lussac to the Academy.

The dress of M. Aldini consists of two garments;—one, a thick tissue of amianthus, or of wool rendered incombustible by being steeped in a saline solution: the other, an iron wire-gauze dress covering the former. The idea of the latter, he says (in a pamphlet lately published), was suggested to him by the chain-armour of the ancients, which he found to be impervious to flame; upon the principle first discovered by Sir H. Davy, and employed by him in the construction of his safety-lamp. This wire-gauze dress, however, would not alone be sufficient to protect the body from the action of heat, though it might ward off the flames: but the dress of amianthus, or wool, by its thickness and non-conducting power accomplishes this, and forms with the wire-gauze an efficient defence, for the time at least that the exertions of the firemen require.

The head-piece consists of a stout cap of amianthus-cloth, fitting close to the skull, and covering the throat, having holes made in it for the nose and mouth: spectacles are also provided for the eyes, lined with fine brass wire-gauze.

The metallic dress consists of five different pieces:—the helmet, between which and the amianthus cap there is a considerable space, and which is moreover furnished with a mask in front, to afford additional security to the face; a cuirass; arm- and thigh-pieces, the latter joining the cuirass over the hips; a pair of boots; and a shield of an oval form, five feet and a half long, and two and a half wide. This shield is useful in stopping or turning any strong jet of flame, and thus enabling the fireman to see his way; and it is proposed to construct frames on a similar principle, to intercept the flames issuing through a door or other aperture.

The whole of this dress is composed of iron wire-gauze, the meshes of which are about one twenty-fifth of an inch in diameter: the weight of it altogether is about 15lbs. The fireman has likewise a basket, covered with wire-gauze, strapped to his back, for the purpose of transporting a child through the flames; ropes and double gloves also of amianthus have been made, with the latter of which red-hot bars of iron may with safety be carried in the hand.

We shall proceed briefly to describe the experiments, which appear at least satisfactorily to establish the efficacy of the principle of M. Aldini's invention.

A fireman, with the double protection of the incombustible cloth and wire-gauze, subjected his face to the flame of a straw-fire held in a chafing-dish, for the space of 1 minute and 30 seconds. Another, armed as the former, with the addition of a sheet of amianthus in front, supported the heat during 2 minutes and 37 seconds without any symptoms of suffering. The pulse of the first rose during the experiment, in the space of a minute, from 80° to 120°, and that of the second from 72° to 100°.

The next experiment was still more satisfactory. Two parallel hedges, about 3 feet distant from each other, were formed of straw and

and brushwood, piled upon bars of iron. When these were set on fire, the flames from the two rose in a body to the height of at least 9 feet, and filled the entire space between; while the heat was too great to approach nearer than 8 or 10 paces. At this instant, six firemen, accoutred with the dress of M. Aldini, and following each other at a slow pace, traversed the flames between the hedges many times in succession. One of them carried an osier basket covered with wire-gauze, containing a child eight years old, protected only with a mask of incombustible cloth. This experiment, which the bystanders witnessed with apprehension, had a most satisfactory result; and had the smoke been more dense, it would have been entirely decisive. The firemen were unhurt: the one with the child retreated from the fire in the space of a minute, on account of the cries of the child, who was frightened at a sudden movement of the man in shifting it on his shoulder. The child was also uninjured, and when taken from the basket its pulse had risen only from 84° to 98°. The other firemen sustained the experiment 2 minutes and 22 seconds; and on coming out, were in a profuse perspiration.

The pulse of the fireman who carried the child rose	from 92° to 116°
That of the second	.... 88° to 152°
That of the third	.... 84° to 138°
That of the fourth	.... 78° to 124°

The main question was, the possibility of supporting respiration in the midst of the flames: and if by this we suppose the men to be completely enveloped in them for 2 or 3 minutes, their situation certainly appears most perilous. M. Gay Lussac observes, that when a furnace is heated so as to flame and smoke, the air within is entirely deprived of oxygen; and therefore it is certain, that if the immediate action of the flames were guarded off by the wire-gauze, still it would be impossible to sustain respiration in the midst of them. We must therefore conclude that if the firemen did not experience the difficulty of breathing which we should naturally expect, they must have been supplied in some way with pure air. There are several ways of accounting for this; and one, which M. Gay Lussac suggests, appears the most probable: viz. that the men were supplied by a current of fresh air from the space between the two garments. Besides this, we cannot suppose that their heads were constantly enveloped in the flames, and they would of course find favourable moments for breathing; but the power of suspending the breath is also an excellent resource, which every fireman ought by practice to acquire. The fireman has another difficulty to contend with, in the dense volumes of smoke, which prevent his breathing, blind his sight, and consequently retard his exertions. To obviate this, it has been proposed to furnish a supply of air from a portable reservoir; or by means of a flexible tube, rising from the feet to the mouth, through which the fresh air would naturally rise, as the heated air escaped above.

There is little doubt that amianthus may easily be manufactured: M. Aldini has succeeded in weaving a stout cloth of it, 9 feet 5 inches long, and 5 feet 3 inches wide, being nearly equal to the celebrated one preserved in the Vatican. But the cost of this material

terial cannot admit of very general use, and on this account M. Aldini is endeavouring to substitute for it a manufacture of wool.

Wool is naturally but little inflammable, and when steeped in a solution of sal ammoniac and borax, or alum, burns to a cinder without inflaming; it is also slowly penetrated by heat. It appears from an experiment of M. Flourens even to have an advantage over amianthus. That gentleman presented a finger covered with amianthus cloth to the flame of a wax candle, and afterwards repeated the experiment, substituting a covering of the prepared wool of the same thickness. In the first case he experienced the effect of the heat sooner than in the latter. In point of œconomy, facility of preparation, and convenience, from its greater lightness and weaker conducting power, the preparation of wool has the preference over amianthus: and though its resistance to fire is less than the latter, it is still sufficient in all ordinary cases, and may form a very sufficient substitute.

The experiments which we have related above, have been made under the superintendence of M. Aldini at Milan, Turin, Geneva, and Paris. M. Aldini has not only the merit of ingenuity in this invention, but of indefatigable zeal in pursuing it. He has displayed the greatest industry in perfecting and bringing it to the test of experiment; and he has the satisfaction in return of receiving the unanimous approbation of those most competent to judge of the merits of his invention. M. Gay Lussac has subsequently moved that the Academy, when it proceeds to the distribution of the Montyon Prizes, should offer a reward to M. Aldini, worthy of his honourable exertions for the preservation and protection of human life.

M. Aldini having lately arrived in England, has published a short account of his experiments, and announces a more extensive work with plates entitled, "The art of preserving firemen and workmen from the action of flame, and of saving human life in cases of fire: with a series of experiments performed in Italy, Geneva, and in Paris."

METHOD OF OBTAINING SKELETONS OF SMALL FISHES.

Some time since I was employed in making observations on the produce of some of the ponds in the neighbourhood of London; and I discovered that the Tadpole was a very serviceable animal in anatomizing the very small fishes, as well as some of the larger sorts, generally found in such places; the Tadpole acting in the same manner as the Ant. I have tried the experiment several times, and on various sorts of fishes, and was always successful, particularly with that very little one called by children Stickleback: even in these the skeleton was at all times perfect. My method is this: I suspend the fish by threads attached to the head and tail, in a horizontal position, in a jar of water such as is found in the pond, and change it often till the tadpoles have finished their work, which if two or three tadpoles are allowed to work on so small a fish as the species just mentioned, they will complete in twenty-four hours. I always select the smallest sort of tadpoles, as they can insinuate themselves between the smallest bones, without destroying their articulation.—T. Bluett.

LAW

LAW OF PATENT INVENTIONS.

Extracts from the Minutes of Evidence taken before a Select Committee of the House of Commons, appointed to inquire into the present State of the Law and Practice relative to the granting of Patents and Inventions. Session of 1829.

[Mr. Farey's evidence, continued from p. 76.]

Do you consider that the term of fourteen years issufficient in all cases?—By no means. In my opinion fourteen years of profitable ex-ercise of an invention is always sufficient, if it has not been preceded by loss that is to be repaid. The question is, when that profitable exercise will begin, and how much previous loss and outlay is to be made up: in some instances it begins from the first; in many instances it does not take place at all, during the term of fourteen years. In the case of Mr. Woolf's invention of working steam engines by high pressure steam acting expansively, (either in one or in two cylinders,) there was no profitable exercise of the invention for at least ten years out of the fourteen, and there was so much loss incurred at the first, that the profit made during the last four years never repaid it.

Will you explain what means it is necessary to resort to, in order to obtain an extension of a patent in this country?—That can only be done by a specific Act of Parliament, which is very difficult to obtain and very expensive to solicit.

Are there many instances in which patents have been extended by that means?—Several; but they have rarely been extended, unless the inventors had the foresight to get them extended at an early period of the invention, when there was no opposition to them; Mr. Watt would have found it difficult to have obtained his extension at a late period, or without the influence of Mr. Boulton.

Does not the law, as to the duration of patents, operate very unequally upon different patentees?—Excessively so, almost in the inverse ratio of the merit and importance of the invention. An important invention is only a source of expense and labour to the inventor during several years, until it is brought to bear very completely; and frequently the greater part of the term expires before it is brought to bear at all. It often happens that the profit arising from the first exercise of it, after it is brought to bear, will not repay the loss and expenses which have been occasioned by its first establishment.

Can you give any instance of this oppressive operation of the law?—Many: Mr. Woolf's is a striking instance; he carried on business to a loss for at least ten years of his patent, and though he made profit in the last four years, it did not pay the loss during the first period. The extension since given to that invention is so important, that the existence of deep mining in Cornwall at this moment depends upon it. The difference in cost between the quantity of coals consumed by the engines now in use (which are all on Mr. Woolf's system), and by an equal force of engines, such as were in use before he went into Cornwall in 1813, would absorb the profit of all the deep mining that is now carried on in Cornwall. I think Mr. Woolf is more entitled to a public reward, for the services he has rendered, without any recompense, than any inventor who has ever

ever been rewarded by Parliament. The establishment, and consequent profitable exercise of many inventions when made complete, is often retarded and prevented by public prejudice, and ignorance of their true value; also by the opposition of workpeople, and the fear that they may mutiny, in establishments where new inventions are first practised. I could give many instances of such cases: Mr. Eaton's, who invented the self-acting mule to put up by power, is a strong one; the few machines that he made, when I prepared his specification ten years ago, have continued in profitable use ever since, but no more can be got introduced. Also Mr. Morton, for whom I made a specification in 1818, for a new slip to draw up ships, in order to repair, instead of a dry dock.

Will you state to the Committee what is the mode of proceeding by a patentee when his patent is infringed?—He must wait till he gets unquestionable evidence of the fact of infringement, which is often a very great difficulty. It facilitates the obtaining evidence to apply to the Court of Chancery; but if he has got good evidence, he had much better come to a court of common law at once.

What is the course of defence usually made by the infringer of a patent?—The usual course (if unquestionable evidence is given of the infringement) is to contend that the patent is bad in law. Such defences almost always prevail; it is so exceedingly difficult to maintain a patent, the grounds upon which a patent may be vitiated being so numerous. Several different defendants may act in concert, by infringing the patent in every quarter, and making a common purse to carry on the war; that is the best course for them, because if the patentee succeeds in one action, he must then try another and another, till his money is all gone, and he can scarcely ever keep his patent right alive to overcome them all. The few patents that have been supported, have been commonly sustained by collusion with the infringers themselves; after one trial has decided that the patent is not absolutely bad, they combine with the patentee to allow them free use of the patent on moderate terms, and then, by making a common purse, they prosecute and suppress all new infringements: to effect that, they must keep up the appearance of law proceedings, but defend themselves so as to let the patentee get a verdict, which is only sham; but, added to the common purse, it serves to terrify new infringers, who are not allowed to have licenses or practice at all, whereby the patent right becomes a close monopoly, instead of a general practice paying a small rent to the patentee. If patent rights were made more secure in law, and by less expensive proceedings, it would not suit the interests of patentees to enter into such combinations, but, on the contrary, to promote the most extensive and open use of their inventions, under licenses, at a moderate tax.

Supposing an incorrect specification has been made, what remedy have the public besides that of using the invention; have they any means of setting the patent aside?—A *scire facias* may be brought in the King's name against a patentee, calling on him to show why his patent should not be repealed.

Is it not the law with regard to patents, that you cannot take advantage of the invention of a principle, without describing how that principle is to be exercised?—It is. The patent contains a provision, that no other person shall counterfeit, imitate or resemble the invention; that is construed in the courts to mean, that they shall not take the same principle, and use other means of carrying it into effect. But if the new means used by infringers are so superior as to supersede the original altogether, then they are commonly allowed to be used, on the ground that they must necessarily be new inventions, and not the patent one, which from its non-success must be defective.

So that if in a specification you describe one mode of carrying the principle into practice, that patent would not cover other modes, by which the same principle might be carried into effect?—It would, if those other modes do not produce any superior effect to the original mode; but if they are so very superior, as to supersede the original, they could not be stopped by the patent. That is the manner in which I have found the courts to proceed; but it may be easily imagined, in such matters the courts will be continually deceived in the facts; and as there is no declaratory law, all inferences from precedents of this nature are very deceptive, because we cannot know upon what facts (real or assumed) the courts founded their decisions.

Does not this prohibition to take out a patent for a principle, lead to a great multiplication of counts in the specification of a patent?—It does; and it is a great trouble and difficulty in preparing a specification, because it is necessary to foresee all the varieties of modes of execution that may be given to the invention or principle, which is the essence of the patent right. It is necessary, for the security of the inventor, to describe them all, to preclude other people using them, and that the inventor may have his choice of that way which future experience will decide to be the best.

Would not the allowing the patent to cover the principle, provided there were one good method described of carrying the principle into effect, lead to a more precise and clear specification?—Decidedly it would; but if the patent were given for the principle exclusively, it would be pernicious to the public, that other inventors should not be permitted to work upon that principle by other methods of execution, so as to produce a better result.

You think that for a limited period, between the taking out the patent and the inrolling the specification, the right ought to cover the principle of the invention?—Unquestionably, during that period the inventor should have an entire right to all those principles of which the heads are detailed in the first paper that is lodged, and all possible applications of them, in order that he may have his choice, which application he will pursue in practice. Having specified, his right should be contracted to what he has so specified, but leaving him full liberty to add supplements, and thereby keep his right always enlarged up to the fullest extent that he can maintain, by continuing his labours.

Supposing a man falls upon the discovery of an entirely new principle

ciple of science, such as the condensation of steam, and its application to mechanism, how would you propose to secure the advantage to the discoverer of that principle, without precluding the attempt at improvement by other persons?—I do not conceive that patents are or ought to be a recompense to the inventor for the merit he has displayed, in what he has done previous to the grant being made; it is a sort of bargain, or a lease granted of some small portion of the public employment that has not hitherto been cultivated; that if the lessee will go to work to bring the new invention to bear, he shall have the benefit of working it for some certain time, which it is supposed will leave him a fair term, after it is brought into profitable exercise; but if the time, when properly employed, does not allow that fair term of profitable exercise, then some extension or recompense should be allowed, to cover the deficiency.

Supposing a person had discovered the principle of the condensation of steam, as applicable to first movers, and had merely given, as the mode of carrying that into effect, the form of the atmospheric engine; do you think that his patent ought to have precluded Mr. Watt, during the continuance of fourteen years, from applying for a patent for his steam-engine?—Certainly not; but at the same time it is very unjust to an inventor, that because he is superseded by some successor, he should lose all benefit from his patent; they ought to be allowed to go on together, and the profit ought to be fairly divided between them according to their previous labours, and expenses not yet recompensed, and the share each one has had in obtaining the improved result; neither ought to be stopped, the public ought to be served in all cases, and a recompense ought to be found for all those that have served the public. Such cases are not likely to occur very frequently in great inventions, unless very long patent rights were established; a term even of twenty years is barely sufficient to establish one such invention, much more to see it superseded by another.

Supposing a person has discovered a principle, without inventing any method of carrying it into effect, and subsequently some method of using that principle is invented by another person; do you conceive that the person who has invented the method, should make some compensation to the person who has discovered the principle?—I think that he who has invented the method should be made to divide the advantage with him who had before discovered the principle, because both parties have contributed to the public benefit; they are in the relative situation of a land-owner, and the farmer who cultivates his estate,—both should participate.

How could you arrange the compensation?—It could only be arranged by arbitration.

Do you know any instance of a secret patent being granted?—Some specifications have been kept secret by specific Acts of Parliament. In the interval between granting the patent and inrolling the specification, application is made to Parliament to suspend the operation of that clause in the patent, whereby the specification is to be inrolled; and to enact that the specification shall not be made public, commissioners are appointed by the Act to take charge of the specification, to examine its sufficiency, and to answer all legal questions concerning it, without disclosing the secret.

Do you know upon what grounds the specification is kept secret?—It is commonly upon the ground that the invention shall not get away to foreigners.

What is your opinion of the policy of such a provision?—I think it is always improper, that there should be any thing like a secret specification, under any circumstances whatever.

May it not operate unjustly against individuals, who, without knowing any thing of it, may be expending large sums of money in endeavouring to carry the same thing into effect?—Decidedly so; and as a means of keeping our inventions from foreigners, it is the highest premium that can be offered to an inventor, to go, or send it, abroad himself, because he will have no competition in the foreign country, and can obtain a patent there; it also offers a high premium to all the workmen to go abroad, who first get a knowledge of the invention (and who in that character, are for a time very important to both nations which shall get their services), whereas if foreigners can get intelligibly written specifications from the offices, for a small sum of money, they prefer setting their own men to work on such instructions, to the expense of taking our workmen away; or they do not begin at first, to seduce our workmen; and after a time when they do begin, we have got several instructed, so that the loss of a few is not felt. I know practically, the evils which have been experienced from useful workmen being enticed abroad, in order by their means, to steal secret inventions, of which they possessed an exclusive knowledge: the best remedy was to take out a patent in France, and I was sent there to solicit the patent, and thus prevent their exercising the invention there, after they had stolen it. That French patent has one of the most complete specifications I ever made; it is in the French language, and is now deposited in Paris; but no specification of the same invention exists in England, or in English. After that French patent was so obtained, a very considerable manufactory was established under it there by Englishmen; but the origin of the measure was to prevent seduction of workmen, which would not have happened until a later period when it would have been of no consequence, if foreigners could have got a specification from our patent offices; for they would have set their own men to work on it by preference.

Does not the claim of originality, with respect to a single minor point which is not original, vitiate the whole patent?—It does.

Do you not think that that is very impolitic?—It is a most excessive hardship and injustice, and every way impolitic; the reason for it is, that so long as patents are granted merely upon the request of the inventors, and whilst they are left at liberty to specify what they please, if there were not some limiting penalty of that sort, they would put inventions *ad infinitum* into their specifications, for the chance, that if one hook did not catch, another might.

Do not you think that the proper course would be to set aside that part which is not original?—Decidedly, that is justice; and yet it should not be allowed to the inventors to put an unlimited number

ber of inventions into their specifications; but the practice of leaving them to their own discretion, under the penalty of setting aside their whole patent, because they have exceeded just bounds (which bounds are not defined) is both unjust and impolitic.

Since a patent for the three kingdoms, with the attorney's bill, costs from four to five hundred pounds, does not that operate as a tax upon different inventions with great inequality?—The expense of the patents for the three kingdoms is decidedly too high; I think that the present expense of a patent for England, would be about the proper expense for a patent for the whole of the British dominions. I mean, that in the absence of any other check to an unlimited multiplication of patents, they should not be granted too cheaply. At present, while patents are to be had merely for paying the fees, there must be some limitation applied by means of the expense, and I think the present expense for England is a limitation which is sufficient; but when to the expense of a patent for England, the expense of one for Scotland, and another for Ireland, is added, and also an additional variable expense depending upon the difficulty of the subject, the sum total is in my opinion too much for mere limitation, and becomes a tax upon particular inventors, who are commonly the most deserving of encouragement. The expenses relating to the specification vary according to the nature of the subject. I think the sum total to the patentee should be made invariable, or nearly so.

[To be continued.]

LIST OF NEW PATENTS.

To J. Moore, Broad Wier, Bristol, gentleman, for his new or improved machinery for propelling carriages, also for propelling ships, vessels, or other floating bodies, and for guiding propelled carriages; and apparatus for condensing the steam of the steam-engine, after it has propelled the steam-engine piston.—Dated the 30th of September, 1829.—6 months allowed to enrol specification.

To W. Rodger, Norfolk-street, Strand, lieutenant in the royal navy, for his improvements in the construction of cat-head stoppers. —30th of September.—6 months.

To T. Banks, Patricroft, within Barton-upon-Irwell, Lancaster, civil engineer, for his improvements in steam-engines.—30th of September.—6 months.

To P. Descroizilles, Fenchurch-street, London, chemist, for his improvements in apparatus for removing the down from cotton and certain other fabrics by singeing.—7th of October.—6 months.

To W. Church, esquire, Heywood House, near Birmingham, for his improvements in machinery for propelling vessels and other machines capable of being propelled by steam, and in boilers applicable to the same, and also to other purposes.—15th of October.—6 months.

To W. Church, esquire, Heywood House, near Birmingham, for his improvements in, on, or upon instruments for sharpening knives and other edge-tools, and the machinery or apparatus for manufacturing the same.—15th of October.—6 months.

AURORA BOREALIS.

At six o'clock P.M. Dec. 14th, a bright light appeared about the magnetic north, and gradually formed into a small segment of a circle, from the base of which in the horizon fourteen perpendicular columns of light emanated to altitudes of from ten to twenty degrees; their breadths varied from half to one and a half degree, and they showed a faint red colour. At a quarter past six a well-defined flame-coloured arc arose from the aurora, and was three degrees in width; its vertex, when at its greatest height, was about sixteen degrees in altitude, and its extremities terminated in the N. by E. and N.W. by W. points of the horizon; but it suddenly disappeared, or was apparently extinguished by a dense passing mist. When the mist had cleared away, a distant mild light, which often varied in height, remained about the magnetic north till nine o'clock, or twenty minutes after the moon rose. A very heavy dew fell during this meteoric appearance, and several small meteors occasionally appeared over the aurora.

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1829.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·58. Dec. 31. Wind N.E.—Min. 29·60 Dec. 18. Wind E.
Range of the mercury 0·98.
Mean barometrical pressure for the month .... 30·117
Spaces described by the rising and falling of the mercury............ 4·380
Greatest variation in 24 hours 0·530.—Number of changes 21.
Therm. Max. 53° Dec. 5. Wind S.—Min. 18° Dec. 27. Wind N.E.
Range 35°.—Mean temp. of exter. air 36°·13. For 30 days with ☉ in ♐ 39·72
Max. var. in 24 hours 18°·00—Mean temp. of spring-water at 8 A.M. 51·12

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere in the evening of the 21st ... 96°
Greatest dryness of the atmosphere in the afternoon of the 26th... 65
Range of the index 31
Mean at 2 P.M. 79°·3.—Mean at 8 A.M. 84°·1.—Mean at 8 P.M. 82·4
—— of three observations each day at 8, 2, and 8 o'clock .... 81·9
Evaporation for the month 0·80 inch.
Rain in the pluviameter near the ground 1·20 inch.
Prevailing wind, N.E.

Summary of the Weather.

A clear sky, 2; fine, with various modifications of clouds, 9; an overcast sky without rain, 15; foggy, 1; rain, 4.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
10	5	30	2	2	13	13

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
5	14½	4½	1½	2½	1	0	2	31

General

General Observations.—The first part of this month was dry, and alternately mild and frosty: the latter part was unprecedentedly cold for December, with a prevailing N.E. wind, which brought on a high pressure the last six days.

The 5th instant was foggy, with the exception of three hours in the afternoon, and was the mildest day in the month. The morning of the 15th was also foggy, and the maximum temperature for the twenty-four hours occurred in the night. On the 20th a sufficient quantity of snow fell to cover Portsdown Hill. It again snowed on the 22nd, and on the 24th and 25th nearly three inches in depth fell here, the greater part of which disappeared by evaporation, the other part remained on the ground till the 3rd of January, in places shaded from the sun's rays. On the 26th the moats, marshes and ponds in this neighbourhood were firmly frozen, and thronged with skaiters and sliders. Early in the morning of the 28th the thermometer receded to 18 degrees, and one that had been exposed on a bed of snow the preceding night, stood at the same time at 14 degrees. A large steel magnet (which had also been exposed to the frost) held in the hand a quarter of a minute, to bring down the metallic float of a thermometer at the time of this low temperature, so benumbed the fingers, as to render the touch insensible for several minutes. On the 29th the maximum temperature did not exceed 29 degrees.

The mean temperature of the external air this month is 12·27 degrees colder than the mean of December 1828! it is also upwards of six degrees colder than the mean of December for the last fourteen years, and the coldest December by one degree and a quarter we have ever registered.

The atmospheric and meteoric phænomena that have come within our observations this month, are, one lunar halo, four meteors, an aurora borealis, and five gales of wind, or days on which they have prevailed; namely, four from the North-east, and one from the East.

REMARKS.

London.—December 1. Fine. 2. Cloudy. 3. Foggy morning: cloudy. 4—6. cloudy. 7. Cloudy: slight fog at night. 8—10. Fine. 11, 12. Cloudy. 13. Very fine. 14, 15. Dense fog. About 10 o'clock A.M. on the 14th the fog was so dense that coachmen and others were obliged to lead their horses. On both nights the mails were detained by it, and had to be guided by torch-light. 16. Fine. 17. Cloudy. 18. Stormy with snow. 19, 20. Clear and cold. 21. Cloudy, with snow in the afternoon to the depth of two inches. 22, 23. Cloudy. 24—28. Clear and frosty. 29. Cloudy and cold, with some sleet at night. 30. Cloudy. 31. Clear and frosty. Ice on the ponds upwards of four inches in thickness.

Penzance.—December 1. Fair: rain. 2. Fair: showers. 3. Clear: rain. 4. Fair. 5. Fair: misty. 6. Misty. 7. Clear. 8. Fair: misty. 9. Rain: fair. 10. Rain: fair. 11. Fair. 12. Rain: fair. 13. Misty. 14. Fair: clear. 15. Clear. 16. Clear: showers. 17. Clear: rain. 18. Hail: showers, &c. 19, 20. Clear. 21. Showers. 22. Hail and snow. 23. Clear: fair. 24. Snow. 25, 26. Clear. 27, Fair. 28, 29. Clear. 30, 31. Fair.

Boston.—December 1—6. Cloudy. 7. Fine. 8—10. Cloudy. 11. Fine. 12. Cloudy: rain A.M. and P.M. 13—16. Fine. 17. Foggy. 18. Fine. 19. Cloudy: snow early A.M. and rain P.M. 20. Cloudy. 21. Fine. 22, 23. Cloudy. 24. Stormy. 25. Fine: snow showers. 26, 27. Cloudy. 28. Cloudy: snow showers. 29—31. Cloudy.

Meteoro-

Meteorological Observations made by Mr. Booth at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1829.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Dec. 1	29·856	29·814	29·55	29·50	29·76	29·76	29·51	45	37	50	45	46	41	39	E.	SE.	E.	calm	...	...	...	0·030	...
2	29·783	29·741	29·55	29·50	29·74	29·72	29·42	45	40	53	46	49	45	39·5	E.	W.	SE.	calm	...	...	0·200	...	...
☽ 3	29·840	29·822	29·60	29·58	29·82	29·79	29·45	44	40	51	44	48	43	40·5	SE.	SW.	E.	calm	0·10	...	...	·005	...
4	30·038	29·820	29·60	29·56	30·03	29·78	29·46	47	42	53	45	50	42	39	SE.	W.	SE.	E.	...	...	·25	·020	...
5	30·419	30·297	30·15	30·10	30·41	30·26	29·84	51	40	53	43	53	44	44	SW.	NW.	NE.	calm	...	...	...	...	...
6	30·550	30·516	30·30	30·28	30·50	30·47	30·07	41	21	52	44	47	29	41	W.	SW.	E.	calm	·07	...	...	...	...
7	30·420	30·332	30·25	30·20	30·38	30·32	30·05	34	32	45	44	38	34	26	W.	SE.	NE.	calm	...	...	...	...	...
8	30·441	30·280	30·20	30·18	30·38	30·32	29·90	37	30	48	43	40	29	34	NE.	SW.	NE.	calm	...	...	...	...	...
9	30·435	30·303	30·25	30·23	30·37	30·27	30·07	31	30	46	44	37	34	33·5	NE.	W.	NE.	calm	·08	...	·130	...	...
○ 10	30·186	30·160	30·10	30·05	30·16	30·13	29·80	39	25	46	40	41	34	35	E.	SE.	NE.	calm	...	...	·130	·090	...
11	30·231	30·208	30·00	30·00	30·20	30·18	29·72	40	36	48	40	43	39	36	SE.	SW.	S.	SW.	...	...	...	...	...
12	30·259	30·218	29·98	29·98	30·26	30·22	29·71	44	37	52	43	46	41	37	SW.	W.	SW.	S.	·05	...	...	...	...
13	30·261	30·253	30·00	30·00	30·26	30·24	29·80	50	28	52	50	45	36	40	SW.	W.	S.	S.	...	...	.060	...	0·06
14	30·390	30·340	30·15	30·12	30·34	30·30	29·91	33	27	48	46	46	31	35·5	SE.	SE.	E.	SW.	...	...	...	...	...
15	30·396	30·346	30·25	30·20	30·34	30·34	29·95	41	34	48	40	42	39	33	SW.	E.	N.	NW.	·10	...	...	...	...
16	30·329	30·178	30·10	30·10	30·28	30·17	29·94	40	32	45	39	42	35	31	E.	E.	NE.	NW.	...	...	...	...	...
17	30·136	29·916	30·02	30·00	30·04	29·65	29·64	38	25	47	32	39	33	33	NW.	NE.	N.	calm	...	...	...	·420	...
☾ 18	29·849	29·683	29·80	29·80	29·76	29·60	29·40	39	32	43	34	38	35	31·5	NE.	NE.	NE.	calm	·06	·13	·400	·050	...
19	29·915	29·849	29·85	29·85	29·88	29·84	29·60	34	30	43	32	39	33	33	E.	NE.	N.	calm	...	...	·280	...	...
20	29·980	29·929	29·88	29·84	29·94	29·87	29·60	37	21	42	30	35	24	33	NE.	NW.	N.	calm	...	...	...	...	·14
21	29·982	29·849	29·88	29·85	29·94	29·85	29·53	33	29	45	32	39	30	27	E.	N.	NW.	calm	·08	...	.385	·300	...
22	29·818	29·778	29·70	29·70	29·74	29·68	29·47	36	28	40	34	39	31	31	E.	NE.	E.	calm	...	...	...	·085	...
23	29·820	29·748	29·83	29·82	29·75	29·70	29·60	30	24	38	28	33	25	32	NE.	NE.	NE.	E.	...	...	...	...	...
24	29·963	29·781	29·84	29·80	29·88	29·72	29·50	31	26	37	32	32	28	33·5	N.	NE	NE.	E.	·07	...	·465	·120	...
25	30·214	30·101	30·04	30·00	30·10	29·98	29·82	32	25	35	30	34	27	31	NE.	NE.	NE.	calm	...	...	...	·080	...
● 26	30·468	30·415	30·25	30·20	30·36	30·30	30·06	29	24	32	27	30	27	29·5	NE.	NE.	NE.	E.	...	...	...	...	...
27	30·484	30·462	30·30	30·30	30·40	30·40	30·14	34	15	32	26	30	18	26·5	N.	NE.	NE.	calm	·09	...	...	...	...
28	30·425	30·349	30·30	30·30	30·36	30·28	30·04	30	22	33	26	32	22	16·5	NW.	NE.	NW.	calm	...	...	...	...	...
29	30·430	30·395	30·25	30·20	30·36	30·34	30·10	30	24	34	26	29	26	25	NE.	NE.	NE.	calm	...	·02	...	...	...
30	30·537	30·509	30·35	30·20	30·42	30·40	30·21	32	27	34	28	33	29	29	NE.	NE.	NE.	calm	...	...	...	...	...
31	30·656	30·585	30·35	30·35	30·58	30·47	30·26	32	27	34	30	34	27	32	NE.	NE.	NE.	calm	·10	...	...	...	...
Aver.	30·656	29·683	30·35	29·50	30·58	29·60	29·79	51	15	53	26	53	18	33·1					0·80	0·15	2·275	1·200	0·20

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XXIII. *Notes on the Formation of extensive Conglomerate and Gravel Deposits. By* Henry T. De la Beche, *F.R.S. &c.**

[With a Plate.]

At the present time, when actual causes are by some geologists considered adequate to the production of nearly all the phænomena which we observe in the structure of the earth's crust, it becomes important to ascertain, as far as our knowledge will permit, the value of such causes, and thence judging how far the whole, or any part of them, may have been capable of forming the rocky masses of which the surface of our planet is composed.

It has been imagined that extensive conglomerate and gravel deposits are owing to causes similar to those now existing; in some cases to the joint action of rivers on their beds and seas on their shores, and in others solely to the action of the former. To ascertain if such causes could have produced such effects, we should examine the present action of seas on their coasts and rivers on their beds, so far as regards the production of rounded gravels.

I. *Action of Tidal Seas on their Coasts.*

It has long been known, and often remarked, that seas gain on some coasts and lose on others; in other words, that seas cut away and destroy rocks, even the hardest, in some places, and pile up the detritus, acquired either from this destruction, or from rivers, in others. Playfair has well observed, that rounded gravel "can only be found in the beds of rivers, or on the shores of the sea; for in the depths of the ocean, though currents are known to exist, yet there can be no motion of the water

* Communicated by the Author.

water sufficiently rapid to produce the attrition required to give a round figure and smooth surface to hard and irregular pieces of stone".* Although it is acknowledged that no trituration of rock fragments into rounded gravel is now effected in the bottom of the ocean, it has been supposed that gravels formed or collected on the shores of continents or islands are conveyed there, to be consolidated, and converted into beds of conglomerate. An attention to the effects of seas on their coasts will, however, show us that these gravels do not travel outwards into great depths, but that the ocean exerts its power to throw them back upon the dry land whence they were derived. Attention to a sea coast with a shingle beach during a gale of wind will show this. Every breaker is more or less charged with shingles, which are forced forward as far as the broken wave can reach, and in their shock against the beach drive others before them, that were not held in momentary mechanical suspension by the breaker. By these means, and particularly at the top of high-water, the shingles are projected on the land beyond the reach of retiring waves. Heavy gales and high tides combined seem to produce the highest beaches; they do indeed sometimes cause breaches in the ramparts they have raised against themselves, but they quickly repair it. The great accumulation of beach upon the land being effected at the height of the tide, when the tide ebbs, it is quite clear the sea cannot deprive the land of what it has thrown upon it. In moderate weather and during neap tides various little lines of beach are formed, which are swept away by a heavy gale; and when these little beaches are so obliterated, it might be supposed by a casual observer that the sea was diminishing the beach; but attention will show that the shingles of the lines, so apparently swept away, are but accumulated elsewhere. These remarks of course only apply to such situations where the sea, during gales, has no access to cliffs or piers, from whence there might be a back-wave carrying all before it; but to such situations, and they are abundant, where the breakers meet with no resistance, and strike nothing but the more or less inclined plane of a shingle beach. Even in cases where the waves in heavy gales and high tides do reach cliffs, and for the time remove shingle beaches, it is curious to see how soon these latter are restored when the weather moderates, and when the breakers, in consequence of a diminished projecting force, cease to recoil from the cliff behind.

Shingle beaches travel in the direction of the prevalent winds, or those which produce the greatest breakers; of this, excel-

* Illustrations of the Huttonian Theory, p. 7.

lent

lent examples are seen on our southern coast, where the prevalent winds being W. or S.W. the beaches travel eastwards. If rocky projections or points of land occur on the east of any shingle beach so travelling, the sea soon forms a considerable barrier against itself, more particularly when the mouths of valleys or flat lands back the shingles; such flat lands or mouths of valleys thus obtaining protection from the ravages of the sea (Plate II. fig. 2). If the streams which discharge themselves into the sea from such valleys or flat lands are small, their mouths are barred by the beach, and the water percolates through the shingles. Such streams, in cases of flood, cut through the shingle a passage again to be dammed up by the effects of a gale of wind.

It would appear that though shingle or pebble beaches travel coastways, in consequence of the general direction of the breakers, there is no evidence of their being transported outwards or into the depths of the ocean. The seaward front of most shingle beaches, particularly when they defend tracts of flat country, is bounded by a line along the edge of the beach; above this line the beach generally makes a considerable angle with the sands, in cases of sandy flats.

In cases where shingle beaches are not entirely quitted by the tide, sandy, shelly, or very fine gravel soundings are commonly obtained a short distance from the shore, unless the bottom be rocky, in which latter case it is generally a mixture of sand, rock, or fine gravel* and shells. In fact, if the present continents or islands were elevated above, or the sea depressed beneath, the present ocean level, shingle beaches would be found to fringe the land, but not to extend far seaward.

It is but rarely that the pebbles on shingle beaches are found to have travelled considerable distances, even along shore; in the Chesil Bank indeed,—that extraordinary ridge of pebbles about sixteen miles long, which connects the Isle of Portland with the main land,—the shingles seem to have travelled twenty or thirty miles from the westward. This bank is remarkable on many accounts, and among others for the power the sea has exhibited of heaping up a barrier against itself, even when not backed by land, provided it has two solid resting places

* These gravels are generally fine, very different in size from the common shingles of beaches. It might be supposed by persons unaccustomed to take soundings, that the gravels marked on charts were coarse, resembling shingles; but in general such gravels do not exceed the size of a nut, and are most commonly smaller. Such fine gravels are very frequently mixed with shells; and no soundings are more common on coasts, particularly our own coasts, than gravel with shells, sand and gravel, and sand and shells.

for each end of the bank (See fig. 1.). It also appears that the shingles do not travel from the bank (see fig. 1.); for Portland Roads have a bottom of clay, the continuation of the Kimmeridge clay of the base of Portland and the Ferry Point, affording one of the best holding grounds for vessels in the Channel; and the bottom to the S.W. of the bank is sand, fine gravel with shells, or rock*.

Shingle beaches are generally formed on the sea shores under consideration, from the harder parts of the neighbouring coasts, destroyed by the joint action of atmospheric agency, land springs, and the sea. The softer portions are soon washed away, and even the harder, first forming the shingles, are eventually ground down into sand. It is, however, by no means uncommon to find, in coasts composed of both hard and soft materials, taluses of blocks or large indurated concretions, detached from the cliffs, and defending them from that quick destruction that would otherwise ensue.

The effect of the joint action of the sea and air upon hard rocks is well seen in the Scilly Islands. There the granite decomposes into its usual blocky forms, the angles gradually disappear, and eventually the masses fall on the beach, where the tremendous breakers of that coast grind them against each other into balls, and often hurl them high up on the shore.

I know not how Playfair could have imagined that following waves were merely confined to the shore†, for the destruction of coasts of equal hardness almost always bears a proportion to the extent of open sea to which they are exposed, allowance being always made for the force and duration of prevalent winds.

The power of the sea to erect barriers against itself, under other circumstances than those previously noticed, is very ably

* This bank also possesses considerable interest in another point of view. The hills behind the bank are composed of clays and loose rubbly or slaty limestones (Forest marble, Cornbrash, Oxford clay, Oxford oolite, and Kimmeridge clay), which, if not protected by this mass of shingles, would soon be swept away before the heavy seas rolling in from the Atlantic, and breaking with so much fury on this coast. That they have not been thus attacked is evident, for the large rounded forms of the hills and dales are only here and there marked by little cliffs, cut by the water intervening between the bank and main-land; it therefore seems fair to conclude, that since the existing order of things the Chesil Bank has existed, and that the main-land behind it has not, since it acquired its gentle undulatory form, been attacked by the furious waves from the Atlantic.

† Illustrations of the Huttonian Theory, p. 432.—Had it been the Professor's fate to have lain in the trough of a heavy following sea in the middle of the Atlantic, or to have rejoiced in the dexterity of the helmsman in avoiding the shock of a far seen heavy wave, he would hardly have supposed that following waves were confined to the shore.

illustrated

illustrated by Mr. R. C. Taylor, in his Geology of East Norfolk. After observing that the land encroachment at Lowestoft Ness had been effected at distinct and distant intervals; that its form had been influenced by the direction of the currents in the channel, and the position of the adjacent shoals, and that the lines of growth are indicated by a series of concentric ridges or embankments, inclosing certain areas, he observes, " a rampart of heavy materials is first thrown up to an unusual altitude, by some extraordinary tide, attended with a violent gale. Subsequent tides extend the base, and heap up lighter substances on its summit. Sand is blown from the beach and fills the interstices. The *Arundo arenaria* and other plants, by degrees obtain a footing; creep along the ridge, give solidity to the mass, and in some cases form a matted covering of turf: meanwhile another mound is forming externally, and by the like process rises and gives protection to the first. Occasionally the sea forces its way through one of the external and incomplete mounds, but it is singular to observe how soon the breach is repaired*."

In tropical countries the advance of the Mangrove trees outwards from sheltered situations at the bottoms of creeks, bays, or the mouths of rivers, has a great tendency to increase land at the expense of the sea. So long as the sea continues sheltered, it throws up no barrier against the Mangroves; but when their strange stilt-like roots have advanced the mass of these trees to places exposed to the waves, the sea accumulates a beach against them, forming lagoons or lakes †.

I have observed off Jamaica, that the coral reefs and islands are protected on the side of the prevalent winds and breakers by shingle beaches composed of rounded pieces, and, occasionally, large fragments of coral ‡.

* On the Geology of East Norfolk, p. 52, and the highly instructive plate 6.

† Lakes of this description at Albion, south side of Jamaica, contain numerous alligators (Crocodilus acutus, *Cuv.*) and marine fish. The zoology of some of these lakes would be interesting to geologists, as they become brackish from the heavy rains that drain into them from the mountain sides, and may thus contain marine and fresh-water animals.

‡ I remarked one or two curious instances of the growth of land behind such beaches at Old Harbour. There are many islands, some covered by Mangrove trees; one in particular struck me: on its windward side there was a beach of coral shingles, evidently on the increase, the older or back part bound together by tropical sea-coast creepers; behind these were the Mangroves, mixed, if my recollection does not fail me, with some other tropical sea-shore trees near the beach, but alone and advancing into the sea on the leeward side of the island, where, not being exposed to the breakers, they accumulated silt and mud about their roots, and thus extended the island in that direction.

From

From what has been above stated, it will be seen that the sea endeavours to throw back upon the land the detritus it has received from it, and even, as in the case of corals and shells, of hard substances that have been formed in it, and that the pebbles or shingles are not likely to quit the coast under ordinary circumstances *; indeed the common velocity of tides seems inadequate to transport them in moderate depths, where the power of the waves on the surface of the sea ceases.

II. *Action of Tideless Seas on their Coasts.*

The principal difference between these and those above noticed, consists in the phænomena attendant on the discharge of rivers into them, which will be noticed under the head of rivers. Shingle beaches are accumulated, and protect lands behind them, but from the want of tide we do not see their bases, and they appear of inferior dimensions to those on tidal seas. From the want of tide, which should successively present different portions of a cliff to the greatest action of the breakers, the destruction of coasts is not so great, and the spaces of open sea being more or less limited, the battering power of the breakers is greatly inferior to that of the great ocean swell, discharged on a tidal coast. Still the same rejection of detritus derived from the land will be observed when it does not fall into deep water, beyond the reach of the moving power of such seas; and we know of no current sufficiently strong in tideless seas to distribute the gravel that has been thrown into their deep waters.

Large lakes present nearly the same phænomena as to shingle beaches as tideless seas; and as most of them are lower at one time than another, we may observe the shingle beaches better; and it is by no means uncommon to see a skirting of shingle round them when their waters are low.

III. *Action of Rivers on their Beds.*

Rivers most frequently, though not always, take their rise among hills and mountains, and are supplied either by the melting of snows or glaciers, the draining of rain waters, or by springs. The two former particularly bring down fragments formed by decomposition from the neighbouring rocks, into the bed of the river. In mountainous regions fragments of rocks of greater or less dimensions fall into the river from the mountain sides. The river also undermines its banks, and the loose decomposed surface of the rocks tumbles into it.

* Even in the case of sands, which do not enter within the scope of this memoir, there is a tendency in the sea to throw them upon the land. Witness the sandy Dunes, so common on various coasts.

From

From these sources the river obtains the materials for its gravel. The greater the velocity of the water, the sooner will the angular fragments be ground by attrition into pebbles. Rivers are most rapid in high mountain ranges, having to find their way from a high to a much lower level in comparatively short distances. Now as the decomposition and the fall of rocks is greatest amid high mountains, and as the rivers are most rapid in the same situations, the greatest quantity of river gravel is there produced.

In low situations, where rivers lose their rapidity, gravels are rarely formed, but sands or mud are common. In times of flood, gravels formed in their beds, in the high lands, are brought down into their beds in the plains; but even these do not appear to travel far. It is asserted, but has not been proved, that rivers carry their gravels to considerable distances; but I cannot avoid suspecting that pebbles derived from great gravel plains, or from cliffs of loosely aggregated conglomerate, such as the Nagelfluhe of Switzerland, cut away by the rivers, and thus carried into their beds, have been sometimes mistaken for gravels transported from great distances by the rivers. There is no want of gravel, composed of pebbles from the high Alps, in the bed of the Rhone, where that river quits the lake of Geneva, or in the bed of the Ticino, where it quits the Lago Maggiore; and I presume no person would imagine that the gravels have been brought down by either river from the Alps, as all such pebbles must have been quietly deposited in the bottoms of the respective lakes. In both instances the gravels have been derived from conglomerates formed by more general causes, cut through by the rivers after they have quitted the lakes. Innumerable other instances might be produced. The same observation applies to rivers cutting great gravel plains, where they obtain pebbles, derived originally from distant rocks, from their banks, but to the transport of which, by the rivers, physical obstacles oppose themselves. Such obstacles commonly present themselves in the shape of lakes, the beds of which it is impossible the rivers could have cut. Into these the rapid and detritus-bearing rivers deposit their gravels and sand, so that such rivers constantly tend to fill up lakes so situated. The detritus, thus driven into a lake, will always be deposited in a peculiar form, variously modified according to the depth of the lake, and the pebbly or sandy nature of the detritus.

In cases where rivers discharge pebbles into lakes, that of the Drance torrent for instance, which deposits its pebbles in the lake of Geneva, the advance is gradual and local. It is obvious that the stratification resulting from these causes must have

have a peculiar figure; and supposing a lake, nearly filled by these means, to be examined after drainage, the beds of gravel, sand or clay, would be very irregular, and not be disposed horizontally.

To take examples from the Alps; the present transport of river-formed pebbles from a large part of these mountains is prevented by numerous lakes on their north and south sides. On the north the Rhine deposits its mountain detritus in the lake of Constance, and the Rhone its transported pebbles and sands in the lake of Geneva. Between these the lakes of Zurich, Lucerne, &c. receive the gravels of other alpine rivers. On the south the Lago Maggiore receives the alpine detritus of the Ticino, the lake of Como that of the Adda, and the lakes of Garda, &c. perform the same office to other rivers. From these circumstances it will be evident that the river-formed pebbles of a large portion of the Alps cannot travel by the rivers into either the ocean or the Mediterranean: it might at first sight be supposed that the Po could transport the river-formed pebbles of a large portion of the Alps into the Adriatic; but the Po becomes a sandy-bedded river before it receives the Ticino.

It may also be supposed that though the Rhone can transport no alpine detritus beyond the spot where it enters the lake of Geneva, yet that, after it has quitted that lake, it can carry all the pebbles borne down by the Arve from the district of Mont Blanc. I have often stood at the junction of the two rivers, and could not perceive that there were marks of any great transport of pebbles by the Arve, though it held, as is common to most alpine glacier waters, a considerable quantity of sand in mechanical suspension. The banks of both the Arve and the Rhone afford abundance of rounded alpine pebbles, and it would be no easy matter to say, how much of the pebble bed of the Arve was derived directly from the Alps, and how much from its banks near Geneva. But supposing the Arve did bring down abundantly pebbles from the Alps, their progress would seem to be checked at the gulf known as the Perte du Rhone.

As a general fact, it may be fairly stated that rivers, where their courses are short and rapid, bear down pebbles into the seas near them, as is the case with the torrents in the Maritime Alps; but that when their courses are long, and changed from rapid to slow, they deposit the pebbles where the force of the stream diminishes, and finally transport mere sand or mud to their mouths, as is the case with the Rhine, Rhone, Po, Danube, &c.

IV. *Dis-*

IV. *Discharge of Rivers into Tidal Seas.*

Tidal rivers, when large, most frequently keep their mouths open, though there is always a tendency to form bars and sand-banks; as for example, the Thames, the Severn, the Seine, the Loire, the Tagus, the St. Lawrence. In such situations, the detritus, if any, is small enough to be held in mechanical suspension.

Some tidal rivers, or rivers which discharge themselves into tidal seas, form deltas when the force of the current is considerable, the tides small, or the seas not much subject to storms setting in shore, but merely to land and sea breezes. Of the mouths of such rivers, the Missisippi, the Oronoco, the Ganges, and the Yellow River of China, are examples. The detritus brought down by such rivers to their mouths is either sand or mud; therefore they do not contribute towards the formation of gravels at the bottom of the ocean.

Tidal rivers, when small, have a tendency to be blocked up by the sea, which often increases the bars into long banks of pebbles or sand, and it is with difficulty that the rivers deliver their waters into the sea: if the line of coast keeps the direction of the prevalent winds, the difficulty is increased, and the river generally gains a cliff or some hard ground, for one of its banks, before it can effect its escape into the sea. Good examples of these embouchures are seen on our southern coast. The Teign has a tendency to be blocked up by the bank upon which part of Teignmouth is built, named the Den, which the easterly winds, producing the greatest breakers on this beach, drive across the mouth of the river from E. to W., and the Teign escapes by the side of the Ness Point, which affords it support. The Axe is also deflected from its course by the pebble bank thrown up from W. to E. by the prevalent W. and S.W. winds, which here afford the heaviest breakers, and it escapes into the sea by supporting itself against Axmouth cliff; the sea, however, is constantly endeavouring to bar up its passage (fig. 2.).

The harbour at Shoreham is a good example of a river deflected from its straight course by banks thrown up by the sea (fig. 3). The river in this case escapes through a gap which it has formed in the bank itself.

It is obvious that in these cases the sea rejects the detritus it receives from the rivers, and forces it back, with the cliff detritus, upon the land.

The great flats on the western coast of South America are excellent examples of mud and sandy detritus forced back upon the land.

V. *Discharge of Rivers into Tideless Seas.*

These discharges are more or less modified, according to the open waters and prevalent winds to which they are exposed; and in general they tend to push forward deltas before them (fig. 4.), which more or less protrude according to the depth of water into which the rivers deliver themselves, the greater or less shelter of the coasts, the quantity and nature of the detritus held in mechanical suspension, and the force of the current. Those rivers which push forward great deltas, such as the Nile, Rhone, Po, Danube, and Volga, bear mud and silt before them, and of these materials the deltas are almost wholly composed. The rivers which bear down pebbles into tideless seas are short, rapid, and of the torrent kind. Most frequently, from the high and mountainous nature of the coasts, the gravel is deposited in deep water, and therefore, being out of the influence of breakers and waves, remains quietly at the bottom, unless carried by currents sufficiently strong to remove it: of currents so strong we have not any known examples in a tideless sea. Nice will afford a good example of such deposits. The Var and the Paglion bring down pebbles into the Mediterranean, which are almost immediately conveyed into deep water and remain undisturbed, extending but a short distance seaward; for the gravel soundings obtained further from the coast must not be confounded with the river detritus, such soundings being upon the prolongation of the tertiary conglomerates beneath the level of the sea*.

If tideless seas, such as the Mediterranean, Black and Caspian Seas, were to become dry, these deltas and gravel deposits would be very apparent, both more or less presenting the advance noticed in the case of lakes, and we should not have beds of detritus parallel to the coasts, but a series of projections with a stratification peculiar to each, but not common to the whole.

Upon a review of the phænomena productive of gravels on sea beaches and in river beds, it will, I think, appear probable that in neither case could pebbles be furnished in such a way as to afford materials for those great deposits of gravel and conglomerate, which we observe in rocks that must have been formed at various epochs; the coasts present lines of shingle or sand, more advanced in cases of the embouchures of rivers into tideless, generally calm, or nearly tideless seas, and the rivers afford mere lines of pebbles. To make these

* It should always be recollected that in gravel soundings the probabilities are as great of finding rounded pebbles beneath the sea as on the surface of the land.

materials

materials available in the formation of extensively deposited gravels and conglomerates, some greater and more general force than the action of seas on their coasts, or rivers on their beds, must collect them together. This force it seems natural to seek in masses of water more or less voluminous according to circumstances. To produce these at various times and in greater or less abundance, the various dislocations of strata everywhere so observable, seem adequate. It is now known that mountains have been raised at different epochs, and that horizontal strata, even those deposited at comparatively recent geological epochs, have been shattered and broken into faults, a large proportion of which are only covered by the gravels that have been termed diluvium. Can we imagine that such great convulsions and disruptions of our planet's crust could have been unaccompanied by violent movements in the mass of waters, and that debacles, as they are called, have not been frequent and great? It seems but rational to infer that such debacles or deluges must have more or less resulted from every great convulsion, and have been more or less extensive according to the power of the disrupting force. Such causes could easily form the extensive gravel and conglomerate deposits; we now observe, not only by their own destructive power, but also by amassing all the river and sea-shore gravels within their influence.

According to this theory, the extent of gravels would correspond with the extent of the disturbing forces, and would be general where these forces were applied generally, and partial where these forces were applied partially.

Explanation of Plate II.

Fig. 1. Represents the Chesil Bank, and the soundings on either side of the bank; and a small shingle beach, near Weymouth, defending Lodmoor Level from the sea. A small beach, called the Drift, is thrown up by the sea, on the N.W. end of Portland, and nearly incloses a triangular space, into which the sea enters at high water. Scale 2½ miles to 1 inch.

Fig. 2. The mouth of the Axe river, Devon, affords an example of a shingle beach defending a low country from the sea, and of a river turned from its direct course by a shingle bank, and forced to seek support from a cliff in order to escape into the sea. Scale 2 inches to 1 mile.

Fig. 3. Example of a river deflected at a right angle from its course by a bank thrown up by the sea. Scale 1 inch to 1 mile.

Fig. 4. Delta of the Nile. Scale 70 miles to 1 inch.

 XXIV. *On*

XXIV. *On the Construction and Arrangement of the Berlin Astronomical Ephemeris for* 1831. *By* Professor Encke*.

THE construction of the Ephemeris for this year is the same as that for the preceding one, with the exception that some columns have been added to those relating to the positions of the planets, and to the occultations of stars. With a view to prevent all misunderstanding in regard to the times of rising and setting of the sun and moon, and of the changes of the moon, I distinctly remark that, with the exception of solar eclipses, the times given never refer to *apparent time*, but are always meant for *mean solar time.*

The comparison of the end of the last with the beginning of the present Ephemeris, has led to the discovery of differences, fortunately of no moment, which had arisen from error, and from neglecting small quantities.

In the calculation of the Ephemeris of the sun both for the last and for the present year, the tables of Professor Bessel, published, subsequently to the calculation, in Professor Schumacher's *Nachrichten*, could not be applied. Having however derived, from the corrections which had been published, the elements on which they were founded, I constructed from the same the necessary tables, and thus the difference is of any consequence in one column only. In order to obtain a rigorous agreement with Professor Bessel's tables, it will be necessary to increase the mean right ascension of the sun, or the sidereal time at the mean noon, given in the Ephemerides for the two years, throughout, by $+0''\cdot06$. This correction is constant through the whole year, because the smaller corrections dependent on the two nutations are exactly the same in Bessel's and in my tables.

In the calculations for the positions of the moon, my aim has again been to be accurate to $\pm 0''\cdot5$; yet there are places where the differences appear to indicate the necessity of corrections greater than this quantity. A revisal of the calculations having, however, not shown any error, the data have been given without alteration. The columns of the place of the moon at the two culminations have in the present Ephemeris been calculated more accurately, and each datum has been found directly without interpolation. Although the excellent method of Professor Bessel of predicting occultations of stars, which, with his permission, I have reprinted in this Ephemeris†, will perhaps supersede the use of the lower culminations here given; yet I did not think it proper to leave them out, as they were necessary to me for the calculation of occultations of

* Translated from the original German.

† See Phil. Mag. and Annals, Nov. and Dec. 1829.

stars,

stars, and as they may, perhaps, still be used for observations of the moon in general.

I have now availed myself, in the calculations for Mercury, of the corrections of Lindenau's tables, which Prof. Schumacher had already published some time since. I deem it not superfluous again to observe, that the examination of the places calculated for every second day by their differences, is not sufficient to discover all possible errors of the calculation.

There is an error of 10″ in the heliocentric place of Venus on the 31st of December in the preceding Ephemeris, caused by an error in the calculation, which extends likewise to the geocentric place; this error has however no influence on the data for other days, as the place on that day had been calculated directly and could not be examined by differences.

Mr. Hansen has kindly informed me that the longitude of the node of Venus, used in the calculations for this year and the preceding one, differs from the value assigned to it in Lindenau's tables. I have thought proper to make an alteration in this element, because the value derived for 1808, in the preface of the tables, from the latest epoch, is smaller by 1′ 15″ than the value afterwards adopted. The calculation is founded on the epoch of 1750, and from that date forward an annual motion of 31″·2 has been applied, contrary to what the author himself declares to have been formerly adopted. As the calculations of the transits of Venus likewise give a smaller longitude of the node, and a motion of the node smaller than 31″·2, I have thought that I might assume the longitude of the node of Venus $\Omega = 74° \; 33' \; 48'' + 30{\cdot}66 \, (t - 1765)$.

Hence we have for 1808, $\Omega = 74° \; 55' \; 46''$; while, according to the preface, the observations have given $\Omega = 74° \; 56' \; 37''$; and the tables have $\Omega = 74° \; 57' \; 52''$. The values adopted by me give therefore a result more nearly approximating to the latest observations, than that of the tables, and agree at the same time with those transits which must give the longitude of the node with greater accuracy than any other observation. If, however, later observations should prove the longitude of the tables to be more accurate, I shall adopt their values in future.

Of all heavenly bodies whose places were given in the last Ephemeris, Ceres was the one whose places were likely to deviate most from the truth. For the present year, I have therefore derived new elements from the last oppositions, taking into calculation the perturbations of Jupiter only, respecting which a more detailed explanation will be found below*. Although it cannot be expected that these preliminary determinations will very accurately represent the places of Ceres, yet they will give

* To be given in a future Number of the Phil. Mag. & Annals.

them

them with sufficient accuracy to enable observers to find Ceres with certainty, even among small stars, as the error will probably always fall short of a minute in arc. For the Ephemeris of the preceding year, published in the former volume, I beg therefore to substitute the following, calculated for 1830, by the latest elements.

Ceres 1830.									
Ephemeris for the Opposition.									
12h Mean Time.		Geoc. Rt. Ascen. ⚳			Geoc. Declin. ⚳			Log. Distance. ⚳ from ♁	⚳ from ☉
			′	″	°	′	″		
April	14	14h	57	40·25	−4	39	9·0	0·23566	0·42772
	15		56	54·41		36	51·0	0·23473	
	16		56	7·60		34	35·3	0·23387	
	17		55	19·88		32	22·3	0·23307	
	18		54	31·28		30	12·3	0·23234	0·42820
	19		53	41·86		28	5·5	0·23167	
	20		52	51·67		26	2·2	0·23106	
	21		52	0·79		24	2·6	0·23052	
	22		51	9·26		22	7·0	0·23005	0·42869
	23		50	17·14		20	15·7	0·22965	
	24	14	49	24·49	−4	18	28·8	0·22931	
	25		48	31·38		16	46·7	0·22905	
	26		47	37·88		15	9·5	0·22885	0·42919
	27		46	44·05		13	37·7	0·22873	
	28		45	49·94		12	11·3	0·22867	
	29		44	55·62		10	50·5	0·22868	
☍	30		44	1·16		9	35·6	0·22876	0·42968
May	1		43	6·62		8	26·8	0·22891	
	2		42	12·05		7	24·3	0·22913	
	3		41	17·52		6	28·2	0·22942	
	4	14	40	23·09	−4	5	38·7	0·22978	0·43018
	5		39	28·82		4	56·0	0·23021	
	6		38	34·76		4	20·3	0·23071	
	7		37	40·98		3	51·6	0·23127	
	8		36	47·52		3	30·1	0·23190	0·43067
	9		35	54·46		3	16·0	0·23260	
	10		35	1·83		3	9·3	0·23335	
	11		34	9·70		3	10·2	0·23418	
	12		33	18·11		3	18·7	0·23507	0·43117
	13		32	27·13		3	35·1	0·23602	
	14	14	31	36·80	−4	3	59·4	0·23704	
	15		30	47·17		4	31·6	0·23812	
	16		29	58·30		5	11·7	0·23926	0·43167

The manner in which the perturbations of the small planets are calculated,—viz. by applying the corrections to the elements themselves, and not to the places calculated by the mean elements,—renders a generally true exhibition of their orbits impossible, and the data given in astronomical books neither refer to mean elements generally, nor even to variable elements taken for a certain moment of time. As however for all the four new planets, the effect of Jupiter at least, although perhaps not on the same hypothesis of its mass, has been applied, it may perhaps be interesting to exhibit the form of the four orbits for the same moment of time. The following elements of Pallas, Juno, and Vesta, refer to the moment of the opposition of Pallas; those of Ceres properly for the moment of her opposition. The distance being however small, the change of the latter, in order to reduce them accurately to the moment to which the others belong, would be very small.

Elements of the small Planets.
Epoch of the mean longitude 1831. July 23.
0^h mean Time of Berlin.

Vesta.

Mean longitude	84° 47′ 3″·2
Mean anomaly	195 35 26 ·2
Longitude of the perihelion	249 11 37 ·0
Longitude of the node	103 20 28 ·0
Inclination	7 7 57 ·3
Angle of eccentricity	5 4 50 ·8
Mean daily sidereal motion	977·75540
Log. of the semi-axis major	0·373185

Juno.

Mean longitude	74° 39′ 43″·6
Mean anomaly	20 22 30 ·9
Longitude of the perihelion	54 17 12 ·7
Longitude of the node	170 52 34 ·5
Inclination	13 2 10 ·0
Angle of eccentricity	14 48 24 ·2
Mean daily sidereal motion	813·52533
Log. of the semi-axis major	0·426424

Pallas.

Mean longitude	290° 38′ 11″·8
Mean anomaly	169 33 11 ·3
Longitude of the perihelion	121 5 0 ·5
Longitude of the node	172 38 29 ·8
Inclination	34 35 49 ·1
Angle of eccentricity	14 0 16 ·3
Mean daily sidereal motion	768·54421
Log. of the semi-axis major	0·442892

Ceres.

Ceres.

Mean longitude	307° 3′ 25″·6
Mean anomaly	159 22 2 ·1
Longitude of the perihelion	147 41 23 ·5
Longitude of the node..................	80 53 49 ·7
Inclination.................................	10 36 55 ·7
Angle of eccentricity....................	4 24 3 ·9
Mean daily sidereal motion...........	769·26059
Log. of the semi-axis major	0·442622

With these elements the places of the planets may be determined almost the whole year to a few minutes. If the planets were to be arranged by the length of the great axis, Pallas and Ceres ought properly to exchange places. As, however, by this manner of applying the perturbations, Ceres may and will have at times, in consequence of the periodical changes of the great axis, a greater mean distance, it will not be necessary to deviate from the arrangement usually followed.

With regard to Jupiter and Saturn, it had been overlooked, when preparing the preceding volume, that the data of the tables of epochs were to be corrected, on account of the inequality of the precession. Without regarding the changes of the longitudes of the perihelion and of the node, as well as the greatest equation of the centre, all which will have but an exceedingly small influence, the heliocentric longitudes of Jupiter and Saturn, as given for the year 1830, must, for the reason above assigned, be augmented throughout by 2″·2 decimal seconds, or 0″·7 sexagesimal seconds. The influence of this correction on the geocentric places will not be of any consequence for the declinations of the two planets, as it may be assumed with sufficient accuracy $= 0''\cdot3 \,.\, \frac{r \cos \delta}{\Delta} \cos \lambda$, where r and λ designate the heliocentric distance and longitude, Δ and δ the geocentric distance and declination. In like manner, the principal part of the influence of the geocentric right ascension may be applied by increasing the right ascensions in time by 0″·05.

The ratio of the axes of the orbits of all the satellites of Jupiter, given in the preceding volume, deviates considerably from the truth. This ratio was obtained by the reduction of the positions of the orbits of the satellites to the ecliptic, for which purpose Gauss's formulæ were applied. In calculating them, however, it was overlooked, that these formulæ do not give the inclination itself, but only one half of it; so that the ratio of the axes given in the preceding year's Ephemeris refers to an inclination of the orbits, which is only one half of what it ought to be. This error may for the greatest part be remedied by substituting for the given

given divisor $\frac{a}{b}$ throughout one half of it. The sign remains unchanged. These columns being of less importance, and as, although incorrect, they truly indicate the position of the satellite with respect to the great axis, I have not deemed it necessary to give here the corrected values.

The calculations of the path of the moon have again been divided between Messrs. Herter, Wolfers, and Dannemann. Science has lost the services of the latter gentleman by a sudden and premature death, shortly after he had finished his share of these calculations, when on the point of undertaking a situation at the public school at Lingen.

Mr. Herter has, besides, had the kindness to undertake the calculations for Mercury; while Mr. Wolfers has completely calculated the paths of Venus, of Jupiter, and of Saturn, and the occultations of Jupiter's satellites.

The apparent places of Maskelyne's thirty-six principal stars will have, in this year's Ephemeris, an unequalled degree of accuracy, as Prof. Bessel, at my request, has had the kindness to have them calculated by one of his pupils, agreeably to his latest investigations. The comparison of the calculations of 1830, which has been added, proved that, with the exception of α Aquilæ, no alterations would have been required in the right ascensions. For this star the correction is for Jan. 0... $-0''\cdot012$.

The differences in the declinations do not exceed $\pm\ 0''\cdot02$, with the exception of α Orionis, whose declinations are to be thus corrected.

+ 7° 21′

52·76	81	49·61	26	57·12	104	60·59	73
51·95	70	49·87	37	58·16	97	59·86	85
51·25	59	50·24	50	59·13	87	59·01	95
50·66	48	50·74	62	60·00	71	58·06	97
50·18	35	51·36	73	60·71	53	57·09	97
49·83	25	52·09	84	61·24	31	56·12	92
49·58	14	52·93	94	61·55	10	55·20	83
49·44	4	53·87	112	61·65	14	54·37	
49·40	4	54·99	106	61·51	35		
49·44	17	56·05	107	61·16	57		

These differences, which never exceed $0''\cdot2$, were caused by an error in the reduction to 1830.

There is besides a difference of $0''\cdot12$ for the declination of γ Aquilæ for Jan. 0, by which quantity the value given is too small.

The other nine stars will, indeed, not have the same accuracy, either as to the original mean position, or as to the manner

manner of calculating the reductions. Neither can it be denied that for well-furnished observatories too great a number of principal stars is unnecessary and a waste of time. I have, however, believed, that I ought not to swerve from the example of Professor Schumacher. For observatories which have not the advantage of a perfectly firm position of their instruments, a greater number of northern stars, which may be observed both above and below the pole, may be of advantage in many cases, even if the rigorous determination may be better derived from the thirty-six principal stars. Some differences, imperceptible in practice in the mean places, compared with the data of the preceding year, arise from a new derivation of the same.

New tables of δ Ursæ Minoris, which we are to expect from Professor Bessel, could not yet be made available for the present year. A comparison of these tables with the data for 1830 and 1831, has proved that the following corrections are to be applied to the date of the Ephemeris with respect to this star.

δ *Ursæ Minoris.*				
Date.	1830.		1831.	
	Right Ascen^n.	Declination.	Right Ascen^n.	Declination.
	″	″	″	″
January 0	+0·48	+0·26	+0·57	0·20
April 10	+0·56	+0·20	+0·61	0·25
July 19	+0·64	+0·23	+0·58	0·24
October 27	+0·60	+0·23	+0·64	0·24

Applying therefore, in both Ephemerides throughout, these corrections,—

Right ascension............ +0″·60 (time)
Declination .... +0″·24 (arc),

we shall have nearly such an approximation to the latest determinations, as a rigorous calculation by them would have admitted.

Agreeably to the wish of some of the astronomers who use this Ephemeris, I have given the conjunction of the planets Mercury, Venus, Mars, Jupiter and Saturn, with the moon and with each other, in every case in right ascension, for every month, even when no occultation will take place. For the four new planets the intensity of light in their opposition has been given agreeably to Professor Bessel's idea; viz. that the intensity of light which the planet would have when equidistant

distant from the sun and the earth, and at a distance equal to its mean one, or to its semi-axis major, should be taken as unity. For the purpose of determining the longitude, the stars which are on the parallel of the moon at the time of the moon's transit over the meridian, have again been selected. Agreeably to the wish of Professor Argelander, the horary motion of the moon in right ascension has always been added, in order to facilitate to observers in other places the exact calculation of the transit. The given horary motion of the moon in right ascension, being multiplied by the difference of longitude from Berlin, expressed in parts of an hour, taken negatively if east, and the product being applied to the given right ascension, the time of transit will be obtained with perfect exactness, as the given horary motion is that belonging to a lunar hour. The declination which is added has already for the greater part been divested of the influence of parallax, at least for Berlin, and may serve in our northern countries, without any further correction, for pointing the instruments. It may perhaps be doubtful whether, for observations of the moon's second limb in the early hours of the morning, the addition of stars would be of essential service, as they must often, for having the necessary light, be at a considerable distance from the parallel of the moon. Considering the accuracy of the principal stars, it appears that the derivation of the right ascension of the moon's second limb from all principal stars, and the position of the instrument, would lead to results quite as accurate as would be obtained by the observed difference in right ascension, of stars which are at considerable distances from the parallel of the moon.

The arrangement with regard to occultations of stars is sufficiently explained by the paper on that subject (Phil. Mag. for Nov. and Dec. 1829). The size of the page did not permit me to add at once the declination of the stars, which is to be taken from the list of occulted stars immediately following. The calculation was made twice, in order to ensure exactness: First, by the method explained in the preceding year's volume, which, especially for several occultations on the same day, facilitates the decision as to their taking place, or not. Next, a moment of time was chosen, which was as near the time of the smallest distance, and as convenient for interpolation, as possible; and for this moment the values of p, q, p', q' were calculated, and thence the immersions and emersions were deduced. The year 1831 is distinguished by many considerable occultations. Aldebaran will be occulted six times, Regulus twice, and, besides, Jupiter and Saturn once.

Although the tables next following will no more serve the purpose for which they were intended in the preceding volume, as the quantities p, q, p', q' give now the relative position of the moon to the stars and the motion of the former, they have, notwithstanding, been added, in order to facilitate the calculation of the occultations of such stars as are not given in our list.

Both my own comparison and the communications of others have brought to my knowledge a greater number of misprints than ought to exist in such a volume. Most of them, or almost all, do not arise from a want of attention in the printing-office, which has perfectly fulfilled every expectation, but from the unavoidable transferring and copying of the columns. It would hardly be worth while to enumerate them, as by the regularity of the differences every considerable error will be easily detected at first sight. I beg, however, to add this one remark—that, as in the case of logarithmic tables one acquires the habit, when taking out a number, always to cast one's eye on the preceding and following number, so, in the use of this book, the small trouble of slightly looking at the numbers close to the one wanted, ought not be dispensed with. For accurate calculations it is besides always necessary to form several orders of differences.

XXV. *On Artificial and Natural Arrangements of Plants: and particularly on the Systems of Linnæus and Jussieu.* By William Roscoe, *Esq. F.L.S.*

[Concluded from page 104.]

ACCORDING to each of these systems, the classes are divided into orders. Linnæus, still aiming at simplicity, but founding his decisions on strong natural distinctions, has for this purpose recourse to the pistillum, or style, the immediate organ of impregnation, and essential to the formation of the fruit. As a single word has expressed the class, so another word now gives us the order; and to a practical botanist the expression *Pentandria monogynia* suggests the idea of a division of plants including, among many others, the natural order of asperifoliæ; as that of *Pentandria digynia* does of the umbelliferæ. The difficulties under which Jussieu labours now become apparent. He has indeed formed the vegetable kingdom into fifteen classes; under which heads he has arranged one hundred tribes or orders, each consisting of various families of plants supposed to be allied to each other; but when we ask for the distinctions of these orders, or, in other words, by what peculiarities they are to be recognised, and

and in what terms they are to be described, we find only a series of appellations, mostly derived from some particular genus of plants which is supposed sufficiently predominant to give a name to the order, and which order includes certain other genera which appear to be related to it*. If, however, we are dissatisfied with this mode of distinction, as affording us no determinate idea, nor giving us any clue to discover how such order is formed, we can only have recourse to a comparison of the descriptions placed at the head of each of the orders of which each class is composed. That the Jasmineæ may form a part of the same natural *class* as the Gentianæ, although their relation be not very apparent, may be admitted; because they equally germinate from two cotyledons, and have each a monopetalous corolla situated beneath the germen; but when we ask why these genera are not also of the same order, we must seek for an answer in the description prefixed to each order in the body of the work; until by a careful perusal and comparison of these descriptions, which in many respects agree, we are at length enabled to determine in what the difference between a Jasmine and a Gentian, a Laurus or an Atriplex, really consists. In this secondary part of the system it will therefore scarcely be denied that the advantages of perspicuity and precision are wholly on the part of Linnæus, whatever may be the case as to the natural order of the plants; in which respect, however, it is by no means clear that Jussieu possesses any superiority over his predecessor.

From classes and orders we descend to genera, in the determination of which the chief difficulties of the science consist; but as in some of the orders the number of genera is very great, it has been found indispensably necessary to divide such orders into sections, so as to place each genus in its proper relative situation, and break in as little as possible upon their natural or apparent affinities. This Linnæus and his subsequent editors have endeavoured to do by a kind of collateral arrangement placed at the head of each class, though not strictly conformable to the rest of the system. For the discrimination of these sections there remained ample materials. The stamina and pistils had indeed already been employed in characterizing the classes and orders; but the corolla, as well with respect to the number of its petals as its form and situation, the calyx, the receptacle, the germen, the stigma and the fruit, all offered important marks of discrimination, which have been made use of so as greatly to assist the student, although

* Thus the 4th order of the 8th class is denominated *Jasmineæ*, and includes the genera of Maytenus—Nyctanthes—Lilac—Hebe—Fraxinus—Chionanthus—Olea—Phillyrea—Mogorium—Jasminum and Ligustrum.

though not with all the beneficial effect that might have been expected, or so as to define with accuracy the relative situation of each genus. The same mode of dividing the orders into sections has also been resorted to by Jussieu; but as he had already employed the corolla and the situation of the stamina in order to characterize his classes, he has been obliged to have recourse in his subordinate divisions to other distinctions. He therefore chiefly employs for this purpose the number of the stamina, and the style, with the addition of the receptacle, and particularly of the fruit. Thus it appears that the two systems of Linnæus and Jussieu are in this respect nearly a transposition of each other; and that whilst Linnæus begins his great divisions with the essential organs of fructification, and proceeds to characterize his inferior divisions by parts of less natural importance, Jussieu has formed his leading distinctions upon the corolla, and the situation of the stamina; and has employed the number of the stamina and style to divide his orders into sections. Which of these methods is to be preferred the reader will decide; but as they are in fact equally natural, or equally artificial, that which most clearly defines the plant in question, which supplies a concise and intelligible nomenclature, and most effectually assists the student in his researches, is undoubtedly to be preferred: and in these respects it will scarcely be contended that the system of Jussieu is superior to that of Linnæus.

In forming their genera, both Linnæus and Jussieu have exerted all their talents. They were both of them equally convinced that these combinations were founded in nature, and ought equally to be adhered to under every mode of arrangement. Here then there can be no comparison, except as to the superior skill exhibited in the composition and description of such genera. Which of them has excelled in this respect I shall not take upon myself to decide; but if the preference is to be given to Jussieu in any instance, it is perhaps in the full and scientific manner in which his genera are defined.

But whatever may be the merits of these rival systems in other respects, there is one objection still remaining against that of Jussieu, which strikingly reminds us of the prediction of Linnæus, and renders it as a nomenclature entirely useless. Unable to comprehend in any of his divisions all known genera, he is compelled to annex to the close of several of his orders many plants, which he denominates *genera affinia;* besides which, he is obliged to add at the end of his work a long appendix of plants whose proper stations he has not been able to ascertain; not from the want of opportunity for investigation, for many of the plants were obvious; but because they either fall under

under different classes with equal claims, or are not reducible to any class whatever. As a nomenclature this defect is fatal; for, unless the inquirer can be confidently assured that some part of the system will afford him the information he requires, he is disheartened in his efforts, and relinquishes his search in disgust.

Here, then, the comparison between these rival systems necessarily terminates; and whatever may be the merits of Jussieu as a botanist, it is sufficiently clear that they are not exemplified in the superiority of his arrangement as a nomenclature of the vegetable kingdom. In fact, the inconveniencies arising in such arrangement from its primary distinctions being founded on the mode of germination, from the want of a succinct and explicit division of the classes into orders and sections, and particularly from the unfortunate circumstance of a considerable portion of vegetables not being included in any part of the system, compel us to conclude that, as a nomenclature and series of plants, it is greatly inferior to that of Linnæus; and that, however excellent it may be in some respects, it will never supplant in general use that long established work.

III. That the work of Jussieu, considered as an illustration of the natural affinities of plants, possesses great and intrinsic merit, we may however readily admit; but that the study of plants in their natural orders can supply the want of an artificial system, may safely be denied. In fact, these two methods are as distinct in their objects as they are in their means, and should never be confounded with each other. The one commences its observations with the obvious and exterior appearances of plants; and, seizing upon the most striking characters, immediately arranges them into their different classes and families. No distinctions are employed but such as are visible, and present; and wherever the plant is seen in its perfect state, it bears upon it its own name and character. As the means thus employed are confined to the exterior of the plant, so the object in view is limited to the mere knowledge of its proper appellation; and as soon as that is attained, the purpose of an artificial system is complete.—A *real* natural system, on the other hand, if such a one should ever be practicable, must be founded on a long and intimate acquaintance with the nature of plants, their habits and places of growth, the form and qualities of their seed, the manner of their evolution, increase, and reproduction, the peculiarities of their radication, their interior substance, whether medullary or concentric, the infinitely varied formation of their vascular system, by which the plant is not only enabled to circulate the juices necessary

necessary to its support, but to elicit those peculiar qualities of acids, salts, gums, resins and aroma, by which they are distinguished, and on which their natural combinations so ultimately depend. When these facts are sufficiently developed, the system then proceeds to arrange the individuals of the vegetable kingdom, not by their exterior phænomena, but by those primitive and secret alliances by which nature has bound them together; uniting such as are most nearly allied, and separating such as have no inherent affinity to each other. In an artificial system, some plain and obvious distinction, such for instance as the number of the stamina, is decisive of the character. In a natural system this must depend on some more remote circumstance, such as the mode of germination of the plant, and which, though deeply founded in nature, cannot at the instant be demonstrated, but must for the present be admitted on the credit of the founder. Even to determine the primary distinctions on which such a system should rest, is a matter of no small difficulty: and notwithstanding the concurrent authority of both Linnæus and Jussieu, it is by no means certain that the number of cotyledons with which a plant germinates is the most secure foundation; or whether, for instance, the classification by Gærtner from the seeds themselves is not to be preferred. Hence there arises between the two modes of arrangement this important distinction, that an artificial method, devised and completed by one person, may readily be communicated to another, and is as intelligible to the student as to the preceptor; whilst, on the contrary, the knowledge of a natural system is chiefly confined to the author, and cannot be fully attained by any other person without entering into the same investigations, and ascertaining the same facts; many of which might perhaps afford different results, or lead to different conclusions. Whenever a pretended natural system relinquishes these primary distinctions, and attempts to arrange the genera and species of plants by their exterior phænomena, it is no longer natural but artificial; and the superstructure being wholly different from the basis, it becomes incongruous and absurd; neither furnishing the recondite information which is obtained from the study of the natural relations of plants, nor affording us those advantages of a ready discrimination which we derive from an artificial arrangement. As long as these truths are acknowledged and acted upon, a real progress will be made in the science; and to no country has the world been of late more indebted than to France, for that knowledge and information which a deep inquiry into the recesses of the vegetable kingdom can alone supply; although this country may also boast of many distinguished

guished followers. It is however to be regretted, that these eminent men have either not been aware of the true limits of the science which they cultivate, or have not been satisfied to confine their efforts within the bounds which it prescribes; but have endeavoured to establish their system as capable of exhibiting a complete arrangement of the vegetable kingdom, which would render unnecessary all the labours of their predecessors; and still more is it to be regretted, that they should have endeavoured to establish such an opinion on the authority of Linnæus himself, and should have represented him as speaking a language the most foreign from his thoughts, and as having condemned a system which he laboured with incessant assiduity to establish, on which his hopes of fame were in some measure founded, and which will certainly not defraud him of those honours which are so justly his due.

XXVI. *On the Fossil Trees found in Jefferies Rake Vein at Derwent Lead Mine in the County of Durham.* *By Mr.* H. L. Pattinson, *Assay-Master to the Commissioners of Greenwich Hospital in the Manor of Alston Moor, Cumberland.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I OBSERVE in Mr. Witham's interesting paper "On the Vegetation of the First Period of an ancient World," published in your Magazine for the present month, [Jan. 1830.] a notice of the fossil trees discovered about twelve months ago in Jefferies Rake Vein at Derwent Lead Mine in the county of Durham, in which he states that it has been the opinion of some gentlemen who have visited these ancient relics, that they have been washed into and deposited in their present situation by some aqueous revolution. Soon after the discovery of this very singular natural phænomenon, I visited the place, and must confess I was one of those who drew the conclusion mentioned by Mr. Witham; at least I conceived, that the trees had been deposited in the situation in which they were found after the formation of the vein, but by what means I did not conjecture. The following detail of the particulars connected with this extraordinary occurrence will enable others to judge how far this opinion is borne out by the facts of the case.

The lead mine of Jefferies Rake seems to form nearly the eastern extremity of a series of very rich lead ore veins, extending from Garrigill Gate through Neuthead, Coulcleugh, Allenheads, and so on to the Derwent veins; in the counties of Cumberland, Northumberland, and Durham respectively.

The continuity of any one vein in this series cannot, indeed, be traced for the entire distance; but it is probable that the whole have been formed by some general power operating in a direction nearly east and west.

Jefferies Rake vein has been worked for a great number of years, and has yielded considerable quantities of lead ore, which has been principally raised in the upper series of lead measures above the great limestone of this district, numbered 153 in Forster's Section of the Strata, and marked "Great limestone" in a copy of this section, vol. xlv. plate 2. of the Philosophical Magazine. Here the strata of sandstone, called provincially "Hazels," are much thicker than in many other places not far distant, where they have been sunk through, which circumstance has rendered the mining ground at Jefferies Rake much more valuable. The vein was formerly worked by shafts from the surface; but the adit mentioned by Mr. Witham called Deborah's Level, was driven up to the vein some years ago, and by means of it the workings have been carried considerably deeper towards the eastern extremity. Here an engine-shaft was sunk to the adit level thirty-eight fathoms, and below the adit fifty fathoms, and out of this shaft galleries or drifts are still driving in the vein at various heights, to explore the ground in an easterly direction; in one of these galleries or drifts the fossil trees forming the subject of this communication were discovered. The following is a section of the strata sunk through in the engine-shaft just mentioned, communicated to me by Mr. Dolphin:—

	Fath.	Ft.
Part of a sandstone called the Routen-well Sill ...	4	0
Slate clay or plate	4	0
Sandstone called the Slate Sill	8	0
Plate	4	0
Sandstone called Hipple's Sill	8	0
Plate	10	0
To floor of adit level	38	0
Below the adit to the floor of the drift or gallery in which the trees were discovered—sandstone called the Grit Sill	15	0
Immediately upon the sole of this drift is a plate or *fump* bed from six to twelve inches thick, called very properly by Mr. Witham a "thin stratum of bituminous shale much carbonized."		
	53	0
Below this drift—Grit sill	25	0
Craig sill	8	0
Plate	2	0
	88	0

The

The grit sill here made forty fathoms thick is usually divided into two near the middle by a plate bed, and in that case the separate parts are called the High and Low Grit Sills; but in this situation, as far as I know, the only bed between them is the thin stratum of carbonaceous shale mentioned above.

The grit sill here is a soft sandstone not difficult to work, and in point of situation among the strata of lead measures, there seems to be some correspondence between it and the firestone sill, No. 137 of Forster's Section.

The drift alluded to, in which the trees were discovered, was driven east from the engine-shaft about fifty fathoms, the whole distance in the vein, having the north cheek or wall of the vein on the left side, and the south or sun cheek (as it is called by the miners) on the right side respectively. The vein stood almost perpendicular, and was from three to four feet wide; but instead of being filled between the cheeks with a matrix of quartz and fluor spar, as in situations further west where it had been productive of lead ore, it contained a compact stone very much harder than the stratum of sandstone in which it occurred.

At the distance of fifty fathoms from the engine-shaft the first tree was discovered, standing directly in the vein in close contact with the north cheek, upon which an impression was left corresponding to the indentations on the surface of the tree. The diameter of the tree was twenty-two inches, and the space between it and the sun cheek of the vein was filled up with the hard veinstone mentioned above, by which it was closely enveloped all around; but between the veinstone and the tree was a thin black carbonaceous skin very smooth and slippery to the touch. The substance of the fossil was a hard stone apparently the same as the veinstone by which it was surrounded. The workmen broke this tree to pieces, and found it extend to and rest upon the thin stratum of carbonaceous shale, below which it did not appear to penetrate, but it extended upwards into the roof of the drift to an unknown distance: the length taken down and broken to pieces was about four feet.

The second tree, twenty inches in diameter, was met with at the distance of five feet from the first, and, like it, was found standing upright in close contact with the north cheek of the vein; the rest of the space between the cheeks being filled up with veinstone as before. The same coaly matter occurred between the substance of the fossil and its envelope, and in this case the space contained also some particles of lead ore. This tree, like the first, rested upon the bed of carbonaceous shale, and penetrated upwards into the roof to an unknown distance.

At eight feet from the second, the third tree was discovered, standing upon the bed of carbonaceous shale, and penetrating upwards into the roof of the drift as before, but differing from the two last instances by being near the sun cheek of the vein. Towards the base of the tree it was in contact with the cheek, where an impression of bark was made; a little further up it bent off the cheek to the distance of six or eight inches, and further up still it again approached the cheek, but did not touch it. The space all around was completely filled up with the veinstone, as in the instances already mentioned. This is the tree in Mr. Witham's possession.

The fourth tree was a very large one, two feet in diameter, and occurred at the distance of three feet from the last, standing like it against the sun cheek of the vein, but differing from all the others in the circumstance of its penetrating through the carbonaceous bed downwards, below the drift sole, to an indefinite distance, and being cut off entirely by a bed, as the miners called it, at two feet above the floor of the drift, no vestige whatever appearing further up in the vein.

The fifth tree, four feet from the last, three feet in diameter at the bottom, and twenty inches at top, was standing in the exact position in which it was found, extending five feet in height from the carbonaceous bed at the drift sole upon which it rested, to near the drift roof where it terminated, and could no longer be traced; the cause of its discontinuance did not appear. The annexed is a rough drawing of this tree as it stood in the mine at the time I saw it, and I believe still stands, for the inspection of the curious. The marks across the surface were occasioned by its having been broken by the removal of the stone from around it.

This tree was the last discovered at that time, and I did not perceive any traces of other trunks in each cheek of the vein; but Mr. Witham was at the spot some time afterwards, and it is probable that further discoveries had been then made.

Now, to account satisfactorily for the occurrence of these trees in this situation is a very difficult matter; but if veins have been at one time open fissures, and have been rendered what they now are by subsequent filling up by deposition or infiltration, it is impossible to resist the conclusion that the trees in question were deposited in the fissures forming Jefferies Rake

vein

vein while empty, and afterwards surrounded by the present contents of the vein.

The fact mentioned by Mr. Witham, of other trunks being discoverable in each cheek of the vein, and that of large fossil trees of the same species being frequently found standing upright in sandstone strata connected with seams of coal, certainly favour his view of the subject; and it is further strengthened by the circumstance that about that part of the series of lead measures to which the grit sill belongs, there are frequently found thin coal seams accompanied by numerous vegetable impressions.

Hoping these observations will promote further inquiry into this curious matter, I am, Gentlemen, yours, &c.

Lowbyer-Alston, Jan. 15, 1830. H. L. Pattinson.

XXVII. *Observations on some Parts of* Mr. De la Beche's *Paper on the Classification of the European Rocks. By* A Correspondent.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

FOR some time since Mr. De la Beche's paper On the Classification of Rocks appeared in your Journal, I have wished to make a few inquiries concerning it; but my time having been much taken up during the last three months, I have not had an opportunity of doing so before. With regard to Mr. D.'s preliminary observations, it must be confessed by every one, that there is no matter in geology of greater importance than the being able to offer a satisfactory answer to the question,—Can we in distant countries recognise deposits as similar to our own by their organic contents, and consider them as established portions of beds in our own quarter? which involves the question put by the Baron Cuvier in his *DiscoursPréliminaire*,—"Are there certain animals and plants peculiar to certain strata and not found in others?" (Jameson, p. 47). Mr. D. considers the subject very forcibly, and not only states with truth how little has been yet done towards that answer by observations of the earth's surface, but in great measure shows that we must answer it in the negative, if we take into consideration the present distribution of organized beings on our globe. But can he in accordance with these statements agree in the many instances which we are continually having presented to us, of persons first assuming this question as answered in the affirmative from no wider induction than this country

country affords, and then, by its means, naming and classifying strata in accordance with our own, and in fact, absolutely identifying them with certain terms of our series?

A good example of this may perhaps be found in Dr. Buckland's paper On the Irawadi River. We are told that shells there found have been identified by Mr. Sowerby as those of our London clay; while we are left to conclude that others were likewise found which that gentleman could not identify. We may also, I think, justly infer, that on a more extended examination of the country, many would be found in the same "dark slaty limestone," widely differing from any of our London clay fossils. Now every geologist will, perhaps, confess that "the London clay" presents as narrow a field for observation as any of our strata, being principally confined to two small basins in our island; while the neighbouring one at Paris affords an example of the great variance in appearance and mineralogical details, of an equivalent formation, even when its organic contents are partly the same. Does it appear, then, scientific, or even reasonable, from two patches of clay in a small island, as the type, together with a few doubtful auxiliaries on the Continent, to affirm positively,—that it may be considered as now established on the authority of Mr. Crawfurd's notes and specimens, that the Burmese country contains London clay? In the same way Mr. Colebrooke is represented as having established the occurrence of this bed in the N.E. corner of Bengal. Even if other beds of great extent in Europe could be thought recognizable from certain fossils in distant parts, of which we may be allowed to doubt, surely "theLondon clay," which we cannot show the existence of even in the very next basin to that from which it derives its name, cannot be discovered with any degree of certainty. I would therefore ask Mr. De la Beche, as a member of the Geological Society, present very likely when the above-mentioned paper was read, whether, consistently with his statements about temperature and the distribution of animal and vegetable life, he can agree in thus assigning "the London clay" a locality on nearly the opposite side of our globe?—in a part where, unless we uphold the Wernerian theory of beds encircling the earth like the coats of an onion, we must allow the probability of depositions having been made perfectly independent of those on which we live, if we may not actually conclude that they are so;—in a latitude also, where every objection made with regard to difference of temperature will hold good to a great degree.

In confirmation of the above, the following passage of Cuvier's work (Jameson, page 11) may perhaps be brought forward:

forward:—"When we institute a more detailed comparison between the various strata, and those remains of animals which they contain, we presently perceive that this ancient sea has not always deposited mineral substances of the same kind, nor remains of animals of the same species; and that each deposit has not extended over the whole surface which it covered. There has existed a succession of variations; the former of which alone have been more or less general, while the others appear to have been much less so. The older the strata are, the more uniform is each of them over a great extent; the newer they are, the more limited are they, and the more subject to vary at small distances. Thus the displacements of the strata were accompanied and followed by changes in the nature of the fluid, and of the matters which it held in solution; and when certain strata, by making their appearance above the waters, had divided the surface of the sea by islands and projecting ridges, different changes might take place in particular basins." Might not Cuvier have had in his mind, at the writing of this last sentence, the differences observed between the London and Paris basins? And ought we then to profess to detect "the London clay" in the Burman empire? Far, therefore, from expecting to find the same series of beds in a distant country, which exists in our own, should we not rather look for another bearing no analogy to it?—Each bed differing from the one of the same probable age in our own series, both from its mineralogical character being altered more or less by accidental circumstances, and its organic contents being those of another latitude, which have existed in a temperature and under circumstances probably highly different from those of our own climate?

I would fain take this opportunity of considering the subject more fully, were it not that I feel conscious of my inability to do justice to it, even in the present state of our observations of the earth's surface. I will therefore, for the present, do no more than offer a few observations on the two first groups of Mr. De la Beche's classification of our rocks. In the outset, as he has professed to lay down a new classification, I cannot help regretting he should have kept up the terms "alluvial" and "diluvial," which in my opinion, and I think in that of many of your readers, are as hurtful to true geological science, as any that have yet been invented, and conducive to more mistakes and confusion than even the theoretical division of rocks into primary, transition, &c., which he himself has seen the necessity of doing away with. Furthermore, I must confess I do not see the accuracy of the division of these two groups, speaking relatively to their order in the geological series. By the "alluvial group," I presume he means a group of of beds of the newest formation, deposited by causes now in operation, continually going on, such as the tranquil action of rivers, &c.; by "the diluvial," a group formed earlier than the last, ending where it began, the effects of violent and sudden inundations. But will not the latter naturally be somewhat intermingled with, and even preceded by, some of the former? for the first of the alluvial deposits will manifestly be coincident with the commencement of the present state of our globe (meaning thereby, the effects of the last of the geological revolutions, the one immediately preceding the creation of the cavern hyænas and bears), and will therefore precede, be covered, and perhaps partly destroyed by, the earliest diluvial catastrophe. Where, then, are the limits of this group to be found? Or, how are we to know the one from the other? Not surely by the presence of sand, and rolled portions of the older rocks; for these we may find among the silty deposits of any considerable river. We must in this case confine the diluvial group to the gravel and boulders on hill-tops and other situations, not likely to have ever formed part of the beds of water-courses. But can these be classed as a distinct group, formed in all probability at various periods since the commencement of the present state of our globe? Mr. Vernon (Phil. Mag. and Annals, Jan. 1830) has clearly shown the occurrence of alluvial deposits previous to any signs of diluvial action having taken place: if therefore we retain the terms, and distinguish, as the latter gentleman does, the diluvial beds by large stones being present, must we not divide the groups into three,—antediluvial alluvia, diluvia, postdiluvial alluvia? Or perhaps a fourth would be necessary, designating a silty deposit lying between two gravel beds, if we should ever meet with such a section as diluvial alluvia. What is to prevent us from removing all this difficulty by classing the groups together, after the manner of Dr. Fleming in his excellent "Tabular View of the Geological Epochs" (Preface to "British Animals"), as a modern group, or by any other name adapted for the purpose; signifying in detail, the causes of the very different effects portrayed in the two parts? Again, supposing that the alluvia constitute a distinct group formed since the diluvial period, as Mr. De la Beche seems to contend, I think, with Mr. Vernon, that Mr. D. has very insufficient data before him, for concluding that man and monkeys first appeared during this epoch. Can a few caverns and gravel-beds, perhaps inadequately searched, in one quarter of the globe, and that not the most likely one to be the burial-place of the human race, render the fact even presumable? As Mr. Vernon remarks, the only consequence that can justly be inferred is, that these regions of the earth were

were not peopled previous to the diluvian epoch. As yet I have supposed Mr. De la Beche to be reasoning purely in a geological sense, without any reference to the Mosaic account of our globe. But taking this into consideration (and I should hope that Mr. De la Beche is willing to consider it as an inspired writing), is it proper, I would ask, in a country like this, merely to admit, and that with considerable difficulty, that there is no reason why man should not have lived previous to the diluvial period, and conclude by saying that the mass of evidence is at present against it? evidence purely negative, and scarcely able to bear sifting.—On the Continent things are in such a state, that before long I have no doubt man will be admitted among the number of animals found fossil; and even in our own island, appearances have shown the probability of the occurrence of human bones; for in the neighbourhood of our most extensive bone-cavern, that at Torquay, many creditable persons attest the finding of pieces of pottery, flint axes, and other works of art, deep in the osseous loam, as I doubt not Mr. D. well knows. If such are found, why should human remains be absent? Have we not every reason to conclude that they are there, until the cavern has been thoroughly examined? On this subject, however, we shall probably know more when the work proposed to be published by a gentleman there resident, on the contents of this cavern, appears.

I am by no means an advocate for bringing geology into contact with the Bible: on the contrary, I think that for the present at least they must essentially be kept separate; and I therefore dislike the occurrence of the term "diluvial" in any system of the science, as continually bringing into view the Noachian Deluge, as if geologists assigned it as the cause of all the gravel and rubbish on the earth's surface. Yet, as it must be the wish of every Christian to see the two accounts of the occurrences on our globe agree (and they no doubt ultimately will agree), I think it would be better if opinions opposed to the Divine Record, in parts where it is distinctly expressed, were omitted, unless supported by a very wide induction of facts collected from every quarter; and then only mentioned as true in very qualified terms. The treatises of Penn, Faber, and others, show how impossible it is at present to offer the two in connection: while the "Scriptural Geology" forms a miserable instance of what ignorance and prejudice can effect when wandering from the subjects really in debate; it stretches every expression of the inspired historian further than even common sense can warrant, and treats the Bible as a full record of science.—I have the utmost respect, no man more so, for the industry and research which Mr. De la Beche displays in the walks of this science, and am inclined to pay great deference to his experience and judgement in all matters of observation; and accordingly offer these remarks more in the spirit of inquiry than of controversy, wishing to know more fully his views on the subject of his first two groups of the geological series, and leaving all due room for misunderstandings of the subject, from the mere sketch he has professed to give of it. But I should imagine it possible for him to devote all his time and attention to the subject, and complete a system tolerably perfect, without entering into any statements of opinion hostile to Sacred Writ.

I am afraid I have already trespassed too much on your time; and will therefore subscribe myelf, Your's &c. Z.

[That it is wrong to endeavour to represent divine revelation as dependent on any views of natural philosophy, was the opinion of a distinguished teacher of theology long before the present controversies had arisen.—Edit.

"The *Natural Philosophy* of the Pentateuch ought not to induce us to reject it. It is not at all likely that God, in order to enable a man to be a *lawgiver* of the Jews, should reveal to him all the causes of the phænomena of nature. But why, you will say, did Moses give this as an *authentic* account of the *creation?* Suppose I answer, *I do not know?* It seems to me as if that would be no sufficient reason for *rejecting* our whole *system of religious dispensations.*—Suppose I answer, Moses might be an inspired writer as a *religious* minister, and be left to his own notions, or to notions *established* in his time, as a *natural philosopher:* and yet he always might write and speak in those different characters, in one and the same tone and style? Even that would be sufficient to hinder our *rejecting* the Pentateuch."—*Lectures in Divinity by Dr. John Hey, Norrisian Professor of Divinity in the University of Cambridge, from* 1780 *to* 1795, vol. i. p. 196.]

XXVIII. *Specific Characters of Cygnus Bewickii and C. Ferus.* *By* W. Yarrell, *Esq. F.L.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

THE great similarity in the external appearance of our two wild swans, will render a slight alteration necessary in the specific characters hitherto attached to the *Anas Cygnus (ferus)* of Linnæus.

In addition, therefore, to your obliging notice of Bewick's swan, in the Philosophical Magazine of last month, may I request insertion for the following short distinguishing characters of both species:

Ordo, Natatores, Illiger.—*Fam.* Anatidæ, Leach.—*Gen.* Cygnus, Meyer.

C. *Bewickii,* rostro semicylindrico atro, basi aurantiacâ, corpore albo, caudâ rectricibus 18, pedibus nigris.

C. *ferus,* rostro semicylindrico atro, basi lateribusque (his ultra nares) flavis, corpore albo, caudâ rectricibus 20, pedibus nigris. I am, &c.

Ryder Street, St. James's, Feb. 6, 1830. William Yarrell.

XXIX. *On*

XXIX. *On the Geology of Havre**. *By* John Phillips, *Esq. F.G.S. &c. &c.*

THE general accordance in the nature and order of succession of the strata of the opposite coasts of England and France, has been demonstrated by several eminent geologists. Dr. Fitton has shown that the "denudation" of Sussex is continued across the Channel into the Boulonnais; and Mr. De la Beche has traced the oolitic system of the South of England into the interior of Normandy. The object of the present communication is to make known the appearances observable at a remarkable point of the French coast near Havre, where the chalk and the beds beneath it are exhibited in a very instructive section; and to compare the order of stratification which there prevails, with the analogous series of English rocks.

The complete series of the formations beneath the chalk has been only lately ascertained, and they can be seen together in very few situations. In consequence of unconformity of stratification, the chalk and green-sand are found to rest, in different situations, on very different members of the inferior formations. One remarkable example of this kind on the South coast is explained by Mr. De la Beche, and another is represented in one of the sections which accompany my "Illustrations of the Geology of Yorkshire."

But besides the discordance introduced by this want of conformity in their planes and areas of deposition, considerable deviations happen in the nature and thickness of the strata beneath the chalk. The total disappearance of the green-sand from the escarpments of the wolds of Lincolnshire and Yorkshire is probably not due to an unconformity between it and the chalk, but to a real deficiency of the deposit.

To illustrate so variable a series of strata, it is desirable to bring together sections taken in distant places, and to confirm the order of succession by a careful comparison of the organic remains.

The chalk in the neighbourhood of Havre is the lowest part of that thick stratum, and is remarkably stored with large black nodular flints, arranged in parallel rows, at unequal distances, in some parts closely crowded, while above and below they are scattered far asunder.

Organic remains do not appear to be plentiful. In proceeding from Havre along the shore towards the N.W., we observe the strata beneath the chalk. At a small village about

* Read before the Yorkshire Philosophical Society, December 2, 1829; and communicated by the Author.

a mile from the town, clay, full of Gryphæa nana, M. C., is dug beneath the sand of the beach, for the fabrication of bricks and hexagonal tiles, which are white within, and only partially reddened on the surface by the flame passing over them.

Half a mile beyond, the chalk hills are separated from the sea by a buttress of chalk rubble resting on green and ferruginous sands, in which I found a shark's tooth, small Terebratula, and fragments of Inoceramus. These materials, in a state of confusion, lie in the cliff against the sea, upon some of the lower pale beds of the Kimmeridge clay, alternating with thin layers of ochrey stone.

The clay contains abundance of Gryphæa nana, and of another small species with striated valves. Some specimens of a quadrate Serpula, Rostellaria composita?, smooth Terebratula spines and plates of Cidaris, Cucullæa, Amphidesma recurvum (*Phil.*), Trigonia costata, Pholadomya, Isocardia? Teredo, Inoceramus.

In the alternating layers of stone we find Rostellaria composita, Amphidesma recurvum, and A. securiforme (*Phil.*), Ostrea gregarea (a coarse variety exactly like one in the coralline oolite at Sinnington, Yorkshire), Melania Heddingtonensis, M. striata.

A similar series of stony layers, containing some fossils of the coralline oolite formation, occurs near the bottom of the Kimmeridge clay, in North Wilts, and in the Vale of Pickering.

Further on, these beds rise a few feet into the cliff, and still lower portions of the blue clay appear, inclosing one continuous layer of Ostrea deltoidea (*Sow.*), and beneath this more compact clay, with abundance of casts of the shell which he calls Mya depressa. Above all these layers of clay and stone lies a mass of much darker clay, apparently more sulphureous, with nodules of argillaceous ironstone, as at Speeton in Yorkshire, but without any marks of the Astacidæ, which are common there. Upon this lies a confused heap of brown sand and blocks of white and greenish sandstone, which have fallen from the neighbouring steeps, and constitute a sort of undercliff.

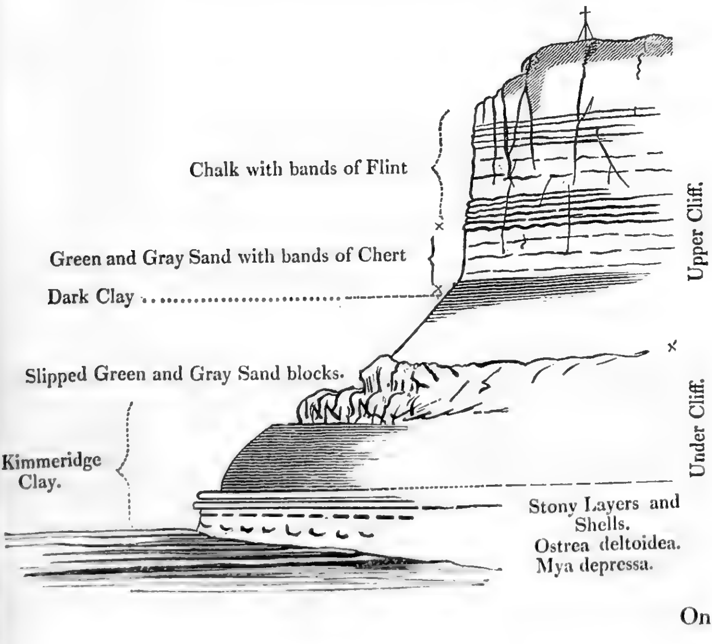
In the steep cliff which rises above this mass of fallen sandstones we see the chalk, resting on about thirty feet of green and gray sand, and this reposing on the uppermost part of the argillaceous series before noticed. The line of division between the clay and the sandstone is pretty clearly marked, but that between the green-sand and the chalk is almost imaginary. On close inspection the mouldering surface of the sand, and the pale hue of its layers of *chert*, serve to point out its extent, and to distinguish it from the firmer chalk with its more decided bands of *flint;* but at the junction these characters

racters are less distinct, and the whole mass appears naturally to rank as one formation.

The profusion of fossils which these sand beds contain, reminded me of the rich repository of Chute farm in Wilts, and the species are mostly similar. I collected specimens of

Wood, fibrous, hard, heavy, brown.—Spongiæ or Siphoniæ of different forms.—Millepora—Lunulites (*Lam.*) Retepora?—Galerites subuculus (*Lam.*)—Cidaris papillata, plate and spines.—Crinoidal Column.—Pecten asper, P. quinquecostatus (*Sow.*) P. with large rough ribs.—Dianchora striata (*Sow.*)—Plagiostoma like P. duplicatum.—Crassatella?—Ostrea.—Inoceramus.—Teredo?—Chama or Gryphæa?—Terebratula pectinata, and two plaited species described by Smith from the greensand of Wilts; also a smooth species.—Magas pumilus.—Balanus, large valve of the operculum.—Ammonites complanatus (*Mantell*).—Nautilus elegans (*Mantell*), a very large and fine specimen.—Sharks teeth.

Some of these blocks contain nodules of pale gray chert.



On

On comparing what is above said with other well-known sections of the strata immediately below the chalk, the following conclusions appear to be warranted.

1st, That the green and gray sand of Havre is perfectly identical with the green-sand of Warminster, contains similar subordinate masses of chert, incloses the same characteristic fossils (Siphoniæ, Galerites, Pectinidæ, and Terebratulæ), and lies in the same geological position, immediately in contact with the lower chalk.

2ndly, That the series of strata observable at Havre below the green-sand is very similar to the great clay formation of the Vale of Pickering in Yorkshire, of which the upper part appears analogous to the gault, while the lower part is decidedly Kimmeridge clay, no trace of the Portland oolite or wealden formation being visible in either district. Whether any portion of the ferruginous sands observed in great confusion at Havre belongs to the lower green-sand, I cannot undertake to decide.

3rdly, That the manner in which the fossils are disposed in the Kimmeridge clay at Havre, is extremely like what obtains in North Wilts and in Yorkshire; the shells being most plentiful towards the bottom of the deposit, and accompanied by partial layers of stone, which indicate the proximity of the coralline oolite formation beneath. As near Wotton Basset in Wilts, at Heddington near Oxford, at Brickhill in Buckinghamshire, at Welton, Elloughton, Malton, Helmsley, and Kirby Moorside in Yorkshire, the flat oysters (Ostrea deltoidea, *Sow.*) lie in a continuous layer parallel to the stratification, enveloped in pale blue clay, and unmixed with other shells.

XXX. *On the Position of Rocks in the Geological Series, whence the Salt Brine is derived, in the State of New York, North America.* *By* A Correspondent.

A CORRESPONDENT, adopting the signature E. W. B., in the Phil. Mag. and Annals of Philosophy for July 1829, page 75, decides, without personal inspection, and at a distance of more than three thousand miles from the beds, that a particular saliferous rock, because it contains brine springs and cavities of old crystals of salt, "is in reality the equivalent of the new red sandstone." And he imputes to Mr. Featherstonhaugh the error of supposing it to be the old red, on account of the contiguity to each other of the new and old red sandstone

sandstone in the county of Monmouth. He then infers that this (supposed) error casts "a doubt on Mr. F.'s opinion, that none of the beds which are in England higher in the series of formations than the coal measures, are to be found in North America, north of 40° N. lat."

The details by which Mr. Featherstonhaugh supported his views of the succession of rocks in North America, in his letter to Mr. Murchison, read before the Geological Society, January 2, 1829, were necessarily excluded from the abstract of that paper, published in No. 9 of the Proceedings of the Geological Society. But it does not appear in any part of that abstract, that reliance was placed upon the old red sandstone, more than upon any other rock, in the table over which his name is placed, to assert the agreement in the order of rocks in England and North America, which was the main geological truth of the letter to Mr. Murchison.

If Mr. F. has fallen into so great an error, then that agreement does not exist: and it will be true, according to Mr. Eaton's Synopsis, that the productive bituminous coal-measures of this continent are found much higher up in the series than the lias. If E. W. B., as a philosophical geologist, is prepared to admit this, Mr. F. is not. He perceives in such a proposition an utter abandonment of a well-adjusted and continuous operation of causes, which constitutes one of the highest and most attractive branches of geological knowledge. And from this consequence there can be no escape: for the author of the Synopsis already named, has placed his 3rd grauwacke above his cornitiferous limestone; has described the thin beds of coal lying in it in the State of New York*, and has advised the boring for thicker beds. This has not yet been done in the State of New York, though even the Government has been strongly solicited to encourage the undertaking. But in Pennsylvania the coal-beds of this formation are thicker, and at Pittsburgh the coal lies immediately upon the rock called Cornitiferous in the Synopsis.

If Mr. F. has fallen into this error, he must then contend, according to the Synopsis, that the lias in North America lies under the carboniferous limestone; for if any limestone corresponding in its geological associations, in its extent, in its mineral and cavernous structure, *in its fossils*, deserves to be thought the equivalent of the carboniferous limestone of the British islands, then the cornitiferous lime-rock, and the geodiferous lime-rock of the Synopsis, constitute its equivalent.

* Canal Rocks, Part i. page 141.

But

But this must rest upon assertion; the identity is not so susceptible of proof as that of coal. Coal makes no mistakes.

But where are the mineral or fossil characters of the lias of Europe to be found in the lias of this Synopsis? It would be a vain attempt to look for them: nothing can be more dissimilar. Instead of the calcareo-argillaceous beds with their marly partings, we find imperfect beds of limestone, and isolated beds of gypsum, laid in a slaty shale; and instead of the Ichthyosaurus, the Gryphæa incurva, the Ammonites Bucklandi, &c. &c. we have the Trilobite, and never-ending congeries of Producta and Spirifers. What is the reason that the Saurians and other characteristic fossils have not been found in the State of New York? We have the true answer to the question, in supposing the formation not to exist there.

But this saliferous rock whence the brine is derived, and which is acknowledged to be subjacent to the pseudo lias; this must, it would appear, be new red sandstone, because it contains brine, and cavities of former crystals. There is nothing very extraordinary in crystals of salt being found in muriatiferous earths; any desiccated salt lake might be expected to contain them, and such lakes do not affect particular formations. But is it because gypsum and rock-salt are found in new red sandstone in England, that the rock which contains brine in America must be its equivalent? For the gypsum of the State of New York is found in the lower beds of the carboniferous limestone or in the lias, according to the Synopsis, and not in the saliferous rock. The saliferous rock then, to vindicate its claims to promotion in the series, has to place its reliance on the brine. Now suppose that brine were drawn from a red sandstone on this continent, the equivalent of MacCulloch's primary red sandstone (and there is such a rock in North America), which even alternates with the gneiss, and at Suil Veinn is *horizontally* laid upon the highly elevated gneiss. Would that constitute the rock a new red sandstone? How could we account for the brine? And how do we account for the brine drawn from the transition slates on this continent? At the city of Albany borings have been made more than four hundred feet into those slates, and a perennial source of mineral water has been disengaged, which upon analysis gives 64 parts of muriate of soda. The celebrated mineral waters too of Ballton rise through the transition slates. It must also be remembered that the analogy entirely fails with the new red sandstone in Cheshire, since rock-salt has not yet been found in this part of North America.

To illustrate still further the true position of this saliferous rock as it is called in the Synopsis, E. W. B. is referred to the

the following details of a very accurate section, to the depth of seven hundred feet, of the strata at the Salt Works of Kiskeminitas in Pensylvania.

Feet.	Diluvium.
20.	Sandstone.
3.	Coal.
3.	Limestone.
120.	Sandstone.
4 to 5.	Coal.
3.	Limestone.
88.	Sandstone.
2.	Coal.
50.	Sandstone.
5 to 6.	Coal.
100.	Carboniferous limestone.
300.	Saliferous rock resting upon transition limestone.

In this saliferous rock thirty-six wells have been sunk at these works and in the vicinity for brine.—That this order of succession is not peculiar to Kiskeminitas will appear from the following passage: "In many parts of the western country boring for salt water is frequently continued some hundreds of feet (sometimes as much as four hundred feet) below the surface, through calcareous and sandstone rocks, and *occasionally through beds of coal**."

The writer of this paper is by no means so anxious to vindicate Mr. Featherstonhaugh's opinions, as to give a proper influence to facts. In Mr. F.'s table of North American rocks, the three great calcareous formations, primitive limestone, transition limestone, and carboniferous limestone, are in their proper places in the general series, and with their becoming associations. The old red sandstone is found supporting the carboniferous series, and the coal-measures being the terminating formation of the series, (which the Synopsis admits †,) put the seal of more than probability (in the writer's opinion) to the correctness of Mr. Featherstonhaugh's observations.

New York, North America,
Nov. 7, 1829.

G. W. F.

* Journal of Travels into the Arkansas Territory, p. 31.—*Nuttal.*

† As to the basalt of the Synopsis, which overlies the 3rd grauwacke, that is a pure piece of fancy: *it has never yet been found in North America overlying that part of the series by any one.* And as to the rest of the upper beds of the Synopsis, with the exception of the calcareous clay made by Mr. F., they are nothing but ordinary diluvial and alluvial matter.

XXXI. *Notes on the Geographical Distribution of Organic Remains contained in the Oolitic Series of the Great London and Paris Basin, and in the same Series of the South of France.* By HENRY T. DE LA BECHE, *F.R.S. &c.*

[Continued from p. 93.]

OOLITE SERIES.—*Lowest System.*—Subdivision, INFERIOR OOLITE.

North of England. Yorkshire.—Phillips.	South of England. Dundry Hill.—Conybeare. Sowerby.	Normandy. Calvados.—De Caumont.
Dicotyledonous Wood.		Vertebræ of Ichthyosaurus.
	Crustacea.	Lignite.
Caryophyllia convexa (Phil.).		
Meandrina.	Echinites.	Echinites.
Cidaris.		
SHELLS.	SHELLS.	SHELLS.
Mya calceiformis (Phil.).	Mya V scripta.	
—— dilata (Phil.).	Panopæa gibbosa.	
—— literata.	—— intermedia.	
—— æquata (Phil.).		
Pholadomya obliquata (Phil.).	Pholadomya obtusa.......	Pholadomya Murchisoni.
	—— lirata.	—— æqualis.
—— fidicula.	—— ambigua.	
Cardita *similis*	Cardita *similis*	Cardita *lunulata*.
	—— *lunulata*.	
Cardium acutangulum (Phil.).	Cardium.	
—— incertum (Phil.).		
—— striatulum.		
—— gibberulum (Phil.).		
Trigonia *striata*.................	Trigonia *striata*...........	Trigonia *striata*.
—— angulata.	—— *costata*.	—— *costata*.
—— *costata*.................	—— clavellata.	
Nucula lacryma...............	Nucula.	
—— variabilis.		
—— axiniformis (Phil.).....	Cucullæa oblonga.........	Cucullæa decussata.
Cucullæa reticulata (Bean.).		
—— cancellata (Phil.).		
Modiola *plicata*...............	Modiola cuneata.	
—— aspera?	—— *plicata*.	
—— ungulata.	—— gibbosa.	
Mytilus cuneatus (Phil.).......	Mytilus.	
Gervillia lata (Phil.)...........	....	Gervillia pernoides.
Lima *proboscidea*?	Lima *proboscidea*	Lima *proboscidea*.
		—— gibbosa.
Plagiostoma *giganteum*	Plagiostoma *giganteum*.	Plagiostoma duplicatum.
	—— *punctatum*.	—— *punctatum*.
	—— rigidum.	
Pecten *lens*	Pecten *lens*	Pecten corneus.
—— abjectus (Phil.).	—— barbatus.	—— vimineus.
—— virguliferus (Phil.).		
Avicula *inæquivalvis*	Avicula costata...........	Avicula *inæquivalvis*.
—— Braamburensis.		
Ostrea solitaria..................	Ostrea gregaria...........	Ostrea Marshii.
	—— palmata.	
Terebratula trilineata (Y. & B.)	Terebratula intermedia..	Terebratula concinna.
—— *obsoleta*.	—— carnea.	—— biplicata.
—— bidens (Phil.).	—— semigloba.	—— emarginata.
	—— digona.	—— bullata.
	—— *ornithocephala*.	—— ovoïdes.

North of England. Yorkshire.—Phillips.	South of England. Dundry Hill.—Conybeare. Sowerby.	Normandy. Calvados.—De Caumont.
	Terebratula media.	Terebratula lata.
	——— *sphæroidalis.*	——— *sphæroïdalis.*
	——— perovalis.	——— dimidiata.
Turbo muricatus	Turbo *ornatus.*	Turbo *ornatus.*
—— unicarinatus (Bean.).		—— rotundatus.
—— lævigatus.		
Trochus bisertus (Phil.).	Trochus *fasciatus*	Trochus *fasciatus.*
—— *granulatus.*	—— *granulatus.*	—— *granulatus.*
—— pyramidatus (Bean.)	—— *sulcatus.*	—— *sulcatus.*
	—— *ornatus.*	—— *ornatus**.
	—— *punctatus.*	—— *punctatus.*
	—— *elongatus.*	—— *elongatus.*
	—— *abbreviatus.*	—— *abbreviatus.*
	—— similis.	—— concavus.
	—— *bicarinatus.*	—— *bicarinatus.*
		—— imbricatus.
		—— reticulatus.
Melania *Heddingtonensis* ?....	Melania *Heddingtonensis.*	Melania *Heddingtonensis.*
—— *lineata.*	—— *lineata.*	—— lineata.
Nerita costata	Nerita lævigata..........	....
Rostellaria composita	Rostellaria—.............	Rostellaria Parkinsonii.
Nautilus *lineatus*	Nautilus *lineatus*	Nautilus obesus.
Belemnites	*Belemnites*	*Belemnites.*
Ammonites striatulus, (*and two others not named.*)	Ammonites *læviusculus*...	Ammonites *læviusculus.*
	——— *discus.*	——— acutus.
Gastrochæna tortuosa.	——— corrugatus.	——— *contractus.*
Psammobia lævigata (Phil.).	——— *contractus.*	——— *discus.*
Amphidesma securiforme(Ph.)	——— Banksii.	——— quadratus.
Unio abductus (Phil.).	——— Brocchii.	——— rotundus.
Pullustra oblita (Phil.).	——— *Blagdeni.*	——— biplex.
Crassina elegans.	——— *Braikenridgii.*	——— *Braikenridgii.*
—— minima (Phil.).	——— *annulatus.*	——— Gervillii.
Isocardia concentrica.	——— falcifer.	——— *Blagdeni.*
—— rostrata.	——— Sowerbii.	——— triplicatus.
Lucina.	——— Brownii.	——— Brongniartii.
Nucula lacryma.	——— *elegans.*	——— *annulatus.*
—— variabilis.		——— complanatus.
—— axiniformis (Phil.).	Myoconcha *crassa*........	Myoconcha *crassa.*
Gryphæa.	Astarte trigonalis.	Astarte planata.
Lingula Beanii (Phil.).	—— *excavata.*	—— *excavata.*
Orbicula.	—— elegans.	—— rugata.
Solarium calix (Bean.).	Cirrus nodosus.	—— imbricata.
Turritella cingenda.	—— Leachii.	Pinna pinnigena.
—— muricata.	Pinna lanceolata.	
—— quadrivittata(Phil.).	Fistulana.	
Natica tumidula (Bean.).	Donax.	
—— adducta (Phil.).	Perna aviculoïdes.	
Actæon glaber (Phil.).		
—— humeralis (Phil.).		
Auricula Sedgevici (Phil.).		
Terebra vetusta (Phil.).		
Serpula deplexa (Bean.).		
Vermicularia compressa (Y.& B.).		

* The Trochus ornatus of Sowerby is the Pleurotomaria ornata of Defrance, and the T. granulatus of Sowerby is the P. granulata of Defrance.

M. Boblay

M. Boblaye furnishes us with the following list of the Fossils of the North of France: he considers the Gryphæa Cymbium as most characteristic of the marls and calcareous sands, and the Plicatula echinata of the ferruginous limestones:

Ammonites Delonchampi, and many others.
Belemnites trisulcatus (Blainville), and many others.

Gryphæa arcuata.	Lythodomus.
——— *Cymbium.*	Modiola.
Plicatula spinosa (Sow).	Pinna.
Plagiostoma pectinoïdes.	Encrinites.
Pecten (new species).	Caryophyllia.
Ostrea.	Turbinolia.

M. Dufrénoy mentions that near Aubenas a lamellar limestone rests on lias, and contains an abundance of Entrochi. He also states, that near Uzer the Ammonites are so abundant as almost to compose the rock: the principal are,

Ammonites *elegans.*
——— *annulatus.*

With these are found Terebratula *ornithocephala*, T. *obsoleta*, Modiola, and Unio crassissimus. Near Villefranche, micaceous argillaceous beds seem to afford a passage of lias into inferior oolite; they contain Gryphæa obliqua, G. *Cymbium*, and *Belemnites.*

From the above lists we may construct the following table of organic remains found in more than one of the localities enumerated; omitting Gryphæa Cymbium, the only fossil of the North of France found in either of the other localities.

Name.	York-shire.	Dundry Hill.	Cal-vados.	South of France.
Wood or Lignite	*	...	*	...
Echinites	*	*	*	...
Cardita similis	*	*	...	...
——— lunulata.	...	*	*	...
Trigonia striata	*	*	*	...
——— costata	*	*	*	...
Modiola plicata.	*	*	...	...
Lima proboscidea	* ?	*	*	...
Plagiostoma giganteum	*	*	...	...
——— punctatum.	...	*	*	...
Pecten lens	*	*	...	...
Avicula inæquivalvis	*	...	*	...
Terebratula sphæroïdalis	...	*	*	...
——— ornithocephala	...	*	...	*
——— obsoleta	*	...	...	*
Turbo ornatus	...	*	*	...
Trochus granulatus	*	*	*	...

TABLE

TABLE *continued.*

Name.	York-shire.	Dundry Hill.	Cal-vados.	South of France.
Trochus fasciatus	...	*	*	...
——— sulcatus	...	*	*	...
——— ornatus	...	*	*	...
——— punctatus	...	*	*	...
——— elongatus	...	*	*	...
——— abbreviatus	...	*	*	...
——— bicarinatus	...	*	*	...
Melania Heddingtonensis	*	*	*	...
——— lineata	*	*	*	...
Belemnites	*	*	*	*
Ammonites læviusculus	...	*	*	...
——— discus	...	*	*	...
——— contractus	...	*	*	...
——— Blagdeni	...	*	*	...
——— Braikenridgii	...	*	*	...
Myoconcha crassa	...	*	*	...
Astarte excavata	...	*	*	...
Ammonites elegans	...	*	...	*
——— annulatus	...	*	...	*
Nautilus lineatus	*	*	...	...

Of the seventy-two species of fossil shells enumerated in the inferior oolite of Yorkshire, thirteen have been met with in the other localities; and of these, eleven occur at Dundry Hill, seven in Calvados, and one in the South of France.

Of the sixty-seven species mentioned as found at Dundry Hill, thirty-two occur in the other localities; and of these, eleven are found in Yorkshire, twenty-four in Calvados, and three in the South of France.

Of the sixty species enumerated in Calvados, twenty-five are found in the other localities; and of these, twenty-four occur at Dundry Hill, and seven in Yorkshire.

Of the seven species of fossil shells noticed in the South of France, five are found in the other localities; and of these, three occur at Dundry Hill, one in Yorkshire, and one in the North of France.

From this it would appear, that there is a much greater resemblance between the zoological character of the inferior oolite of Dundry Hill and Calvados, than there is between the same rocks of the former place and Yorkshire.

[To be continued.]

[It is necessary to remark respecting the portion of this paper printed in the last Number of this Journal, that the Trochus anglicus of Sowerby is the Pleurotomaria anglica of Defrance; therefore the South of France must be added to the localities already given of T. anglicus. Orthoceras elongatum and Ammonites latæcosta should be added to the list of organic remains found in the lias of Lyme Regis; and Ammonites Stokesii to that of the same rock in Calvados.]

XXXII. *Pro-*

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 18. A PAPER was read, entitled "Observations made with the Invariable Pendulum (Jones's), No. 4, at the Royal Observatory, Cape of Good Hope, for the purpose of determining the Compression of the Earth." By the Rev. Fearon Fellows, astronomer of the Cape Observatory: communicated by the Lords of the Admiralty; who also communicated a Notice of a Meteor observed at the Cape of Good Hope, Oct. 19, 1829; in a letter from Captain Ronald, assistant-astronomer at the Cape Observatory.

Read also, A Memoir on the Developement of Functions, by J. Walsh, Esq.

LINNÆAN SOCIETY.

Feb. 2.—A paper was read, entitled The Natural History of *Petrophila*, a Lepidopterous genus, in its larva state inhabiting rivers, and furnished with branchiæ. By the Rev. Lansdown Guilding, B.A. F.L.S. &c.

Mr. Guilding relates that the very singular little moth upon which he establishes this Genus occurs in myriads, in its larva state, on the blocks of basaltic trap that occupy the bed of the river of St. Vincent. Much as it differs in its habits from the majority of Lepidoptera, one European species he considers to agree with it in its œconomy, and to be perhaps referable to the sub-genus which he would separate from *Botys*, which, from the variety of forms in which it abounds, appears to him to call for division. The types in Mr. Guilding's cabinet which are most remarkable, and which he incidentally enumerates and describes, are Gen. 1. *Chloephila*, Spec. *lineolata*, found in St. Vincent's: Gen. 2. *Kamptoptera*, Spec. *fuscescens*, rare, in St. Vincent's: Gen. 3. *Phakellura*, Spec. *hyalinata* (Fabr. Ent. Syst. ii. 2. 213 ?) abundant in the Antilles.

It is the *Botys stratiotalis* (Kirby and Spence, iv. 56, 74.) in which Mr. Guilding finds so close a resemblance to his *Petrophila* in many respects, that he is persuaded of their near affinity, although there is a trifling difference in the pupal spiracula and in the shape of the branchiæ.

The larva obtaining its food on the rocks in the stream, forms silken tunnels, under which it moves in safety, without danger of being carried off by the current. When at maturity the larva builds a more compact habitation, which, together with the metamorphosis of the insect, the author minutely describes; as well as a small Trichopterous insect found in great abundance in its society, and resembling it in œconomy.

Mr. Guilding thinks it probable that many of the European *Botydæ* found in fenny places, as *B. Lemnata*, *sambucata*, &c. approach to the Petrophilæ, while those in hedges and gardens should remain in a separate genus. His arrangement is as follows:—Insecta LEPIDOPTERA: Sect. NOCTURNA: Fam. *Botydæ*: Gen. *Petrophila*: Spec. *fluviatilis*. *P.* ar-

P. argenteo-nivea, fuscescente adumbrata, alarum superiorum strigis apicalibus angulatis, punctulis duabus intermediis, lineisque baseos tribus subcommunibus fuscescentibus: alarum inferiorum plagâ posticâ argenteo-iridescente, atro-maculatâ: abdomine fusco fasciato.

Subjoined to the paper is an Addition to the natural history of Xylocopa Teredo, and several insects which had been the subjects of former communications, accompanied by additional drawings to complete the description and figures given in Linn. Trans. vol. xv.

A paper was also read On the Functions of the Leaves of Plants. By J. H. Dallas, M.D. F.L.S.

The author states that it is his object to point out the physiological relations of leaves, from which the conclusion is drawn, that they are the digestive organs of plants.—From the air plant of China and some others, he infers that the leaves are not merely respiratory organs, but organs of nutrition.

GEOLOGICAL SOCIETY.

Jan. 15.—William Parker, Esq. of Albany-street, Regent's Park; and the Rev. H. P. Hamilton, of Trinity College, Cambridge, were elected Fellows of this Society.

A paper was read, entitled "On the Fossil Fox of Œningen, with an account of the Lacustrine Deposit in which it was found," by R. I. Murchison, Esq. Sec. G.S. F.R.S. &c.

The author visiting Œningen in 1828, acquired among other organic remains a perfect skeleton of a carnivorous quadruped, imbedded in a layer of slaty limestone, and the specific character of which has since been ascertained through the scientific labours of Mr. Mantell.

A short account is given of the works of the various authors who have described the fossils of Œningen, from the time of Scheuchzer to that of Karg. Cuvier, however, is mentioned as the first who gave true specific characters to the vertebrated animals of this formation, and who ascertained that all the mammalia hitherto discovered in it were "Rodentia."

The author differing in opinion from an eminent French geologist, who has described this deposit as subordinate to the molasse, proceeds to show that the formation is exclusively lacustrine; and in proof of this, he offers, 1st, a description of the deposit, and its relations to the surrounding country; and 2ndly, a sketch of the organic remains.

Œningen is situated about midway between Constance and Schafhausen, on the right bank of the Rhine, where that river traverses the tertiary marine formation of molasse. This formation is here covered by patches of marl and limestone, which extend over the space of two or three miles, and are now well exposed in several quarries, the lowest of which is two hundred, and the highest six hundred feet above the Rhine, and in all of them are found organic remains, exclusively freshwater and terrestrial. The lower, or Wangen quarries, consist of light-coloured, sandy marlstones, divided

vided from each other by thin layers of brown marl, and white slaty limestone, in which leaves of dicotyledonous plants, fishes, &c. are not unfrequent. The upper quarries offer a section nearly thirty feet deep, and are worked for the extraction of building-stone and limestone. A detail of the beds is given, which shows a passage downwards, from brown clay into cream-coloured, indurated marl, and afterwards into a fissile, fetid, marlstone, containing flattened shells of Planorbes, small Lymnei, &c., and Cypris: to these succeed light-coloured, fetid, calcareous, building-stone; beneath which is a finely laminated bed containing insects, Cypris, shells of Anodon, and many plants: then follow two thin bands of fetid limestone, in the uppermost of which a large tortoise has been found, and in the lower was discovered the carnivorous quadruped. Both these animals were in positions which show that their remains had not been disturbed since they first sank down into the silt of the lake. The succeeding strata consist of slaty marl, several bands of slaty marlstone, limestone, and strong-bedded building-stone, with a repetition of finely laminated layers of marl, including plants and fishes, after which the incoherent sandstone of the molasse is reached, and forms the base of the quarry.

A description of the fossil quadruped is then given by Mr. Mantell, who has ascertained its specific character, by first clearing away the surrounding matrix, and afterwards comparing the skeleton with those of many varieties of the fox. He has no hesitation in referring the animal to the genus Vulpes; but a difficulty occurs in positively assigning to it a specific character, owing to the compressed state of the head, which prevents the true form of the frontal bone and post-orbital apophyses from being determined. After noticing this and some slight variations of structure, which he is of opinion are insufficient to establish a variety, much less a species, he concludes that the animal bears a closer analogy to *Vulpes communis* than to any other species with which it has been compared.

The author proceeds to remark upon the existence beneath the lumbar vertebræ of the fossil fæces of the quadruped, which on being analysed by Dr. Prout, afforded the same proportion of phosphate of lime as the Coprolites described by Dr. Buckland. In this case, however, the whole of the adjoining rock is impregnated, though in a less degree, with phosphate of lime; thus affording a strong presumption that the bituminization of the marlstone is due to the decomposition of the vast quantity of animal matter contained in it. All the other quadrupeds occurring at Œningen have proved to be Rodentia: amongst which, the Anoema Œningensis has lately been figured by Mr. König; and a Lagomys was this year found by Professor Sedgwick and the author in a second visit to the quarries.

A synopsis follows of many of the birds, fishes, reptiles, insects, &c. In the insects there is a strong accordance in generic characters to those now inhabiting the district. Mr. Curtis recognizes Formicidæ and Hymenopteræ. Mr. Samouelle has noticed larva of Libellulæ similar to our common English species Libellula depressa, also the genera Anthrax, Cimex, Coccinella, Cerambyx, Blatta, and Nepa, some

some of which are known to feed upon such plants as we here find them associated with in their fossil state, and others are well known inhabitants of stagnant pools.

Of the numerous plants, the few the author collected have been examined by Mr. Lindley, who considers one to be undistinguishable from the recent Fraxinus rotundifolia, others strongly to resemble Acer opulifolium and A. pseudoplatanus; and a specimen of the leaf of an extinct poplar, remarkable for its form, has been named by him Populus cordifolia.

In conclusion the author infers,

1st, That the deposit of Œningen is of purely lacustrine origin, and that its formation must have occupied a protracted period.

2ndly, That the tertiary marine formation of the molasse, was deeply excavated before the lacustrine accumulation commenced.

3rdly, That, from the intermixture of species undistinguishable from those now existing, with others which are decidedly extinct, this deposit must be considered one of those instructive examples which exhibit a gradual passage from an ancient state of nature to that which now prevails.

4thly, That, as it differs in most of its organic remains from all the fresh-water formations hitherto described, either near to, or remote from it, it must have been an independent deposit; and judging from its fossils and superposition to the molasse, it must have been of recent origin.

5thly, That recent as its origin may have been, the lacustrine basin has since been re-excavated to a great depth through horizontal strata of limestone, the highest of which are still seen six hundred feet above the present bed of the Rhine.

6thly, That although the deposit must have been formed long before the Rhine occupied its present level, the organic remains indicate, that even in those days there were insects, fishes, and plants almost identical with our own; and that among the quadrupeds there existed one, undistinguishable from the common fox now inhabiting our latitudes.

Feb. 5.—James Calder, Esq. of Calcutta, and Edward Johnstone, Esq., of Trinity College, Cambridge, were elected Fellows of this Society.

A letter addressed to the Secretary, R. I. Murchison, Esq. F.R.S. was read, entitled, "On the animal remains found in the Transition Limestone of Plymouth," by the Rev. Richard Hennah, F.G.S.

This is the last of a series of communications by the author on the same subject; and in this he endeavours to classify all the organic remains found by him in the Plymouth limestone. In this arrangement there are enumerated several genera of Polyparia, including Spongia?, Stylina, Caryophyllia, Turbinolia, &c.; several species of Crinoidea, and genera of Conchifera and Mollusca.

After a detailed description of many species in each of the above classes, the author concludes, that as the number of Zoophytes bears a very large proportion to that of the Bivalves and Univalves, the Plymouth limestone must be considered to be one of the earliest deposits. But he states that great obscurity still involves the relative distribution of these animals in their order of superposition.

A paper was afterwards read, "On the gradual Excavation of the Valleys in which the Meuse, the Moselle, and some other Rivers flow; by G. Poulett Scrope, Esq., F.G.S. F.R.S."

The paper commences by a remark on the value which would attach to a test by which any one valley could be ascertained to be the result either of a rapid and violent, or of a slow and gradual excavatory process; since the forces of aqueous erosion are of a general nature, and while in activity in one river channel, were probably not idle in others. Such a test has been pointed out by the author in central France, where lava-currents which have flowed into valleys at intervals of time, appear now at different heights above the actual river-bed, marking the successive steps of the progress of excavation.

The author finds another equally valuable test in the extreme sinuosities of some valleys. Any sudden, violent, and transient rush of water of a diluvial character, could only produce straight trough-shaped channels in the direction of the current, but could never wear out a series of tortuous flexures, through which some rivers now twist about, and often flow for a time in an exactly opposite direction to the general straight line of descent, which a deluge or debacle would naturally have taken. Curvatures of this extreme kind are frequent in the channels of rivers flowing lazily through flat alluvial plains; and the author shows the mode in which the curves are gradually deepened and extended, till the extreme of aberration is corrected at once, and the direct line of descent restored, by the river cutting through the isthmus, which separates two neighbouring curves.

But examples must be infinitely rarer of whole valleys characterized by extreme sinuosity; because, in the author's opinion, the frequent shiftings of the channels of streams tend to obliterate their windings, and reduce the sum of the several successive excavations or valley to a more or less straight form. Still there are occasional instances where the bias of the river, or direction of its lateral force of excavation, has remained so constant as to give to the valley itself the utmost degree of sinuosity.

The author quotes the valley of the Moselle between Berncastle and Roarn, excavated to a depth of from 600 to 800 feet through an elevated platform of transition rocks. The windings are often so extreme, that the river returns after a course of seventeen miles in one instance, and nearly as much in two others, to within a distance of a few hundred yards of the spot it passed before; wearing away on either side the base of the ridge-shaped isthmus that separates the curves, and inclosing a peninsula of elevated land five or six hundred feet high; but sloping *towards* the bottom of the curves, where it is strewed with boulders, left there, the author presumes, by by the river as it gradually deepened its channel and extended its lateral curvature.

The valley of the Meuse near Givet, offers, through a great distance, a number of similar windings, and the same thing is seen at intervals in many of the other rivers of that country. Parts of the Seine below Paris, and the valley of the Wye between Hereford and Chepstow, are examples nearer home.

Valleys which like these twist about in the same regular curves as the channel of a brook meandering through a meadow, can, according to the author, only be accounted for by the slow and long-continued erosion of the streams that still flow in them, increased at intervals by wintry floods. To attribute them to a transient and tremendous rush of water in the main direction of the valley, is in his opinion impossible. He contends that whilst these valleys were slowly excavated, other rivers could not have been idle during the same protracted period; but will have produced likewise an amount of excavation proportioned to their volume and velocity, and the nature of the rocks they flowed over. In the examples quoted, the rocks are mostly hard transition strata, yet the valleys are wide and deep. Where softer strata, as sands, clays, and marls, were the materials worked upon, the valleys excavated may be expected, as they are found to be, far wider in proportion to the volume of water flowing through them. The comparative softness of the materials also, by accelerating the lateral erosion of the stream, will have multiplied the shiftings of its channel, and reduced their sum with greater certainty to one average direction. Hence the deeply sinuous valleys are only found penetrating the more solid rock formations. The author thinks that a certain subdued velocity in the stream is also necessary to produce this result; and, therefore, in mountainous districts, where the torrents and rivers are most rapid, their course is nearly straight; thus confirming the author's opinion, that extreme curvature of channel can only be produced by a slow and comparatively tranquil process of excavation.

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

Jan. 22.—On this evening Mr. Faraday gave an illustrated account of the endeavours lately made by the Chevalier Aldini to guard firemen from the effects of flames. He first demonstrated the general principles of flames, then adverted to the peculiar circumstances and conditions of the flames which arise in the combustion of dwellings, their lateral and undulatory action, &c. and finally illustrated M. Aldini's propositions, by exhibiting the apparatus of that philosopher (who is at this moment in London), and showed the defence it afforded by exposing a man, guarded by its means, to the powerful flame of portable gas. The defensive armour consists of a double clothing of asbestos cloth and wire-gauze, and its defensive power appeared to be very great. Some magnificent specimens of asbestus cloth, of great strength and very close in texture, were extended upon the tables.

An unique collection of specimens of crystallized glass was laid out in the library.

Jan. 29.—Mr. Fordham developed and illustrated a plan which he has of transferring the power of fixed and cheap first movers to locomotive carriages, &c. travelling on common turnpike roads. He considers the power of a steam-engine, moving with the locomotive carriage, as very expensive when compared to an equal power obtained by a large ordinary fixed engine, a wind or water-mill, or other common first mover; and conceives, that if the latter could be transferred to the locomotive carriages, the saving in price of power might be far greater than the expense of transference. His plan is to condense air into cylinders, and then to use this condensed air as the motive force. He pointed out the numerous circumstances which seemed to be in favour of this plan; as the diminished weight of the locomotive carriage; the continual diminution of pressure as the carriage proceeded; the cheapness of fixed steam-engine power, &c.; not forgetting to estimate the actual draught required for a carriage, and the sufficiency of the force he proposed applying in giving that draught.

A very beautiful portable astronomical arch, by Captain Kater, was explained in the library by Captain Groves, and also some other curious and novel nautical instruments.

Feb. 5.—Mr. Burnet gave an illustrated account of the oak, and especially of the naval oak of Great Britain; referring principally to those circumstances which were connected with its durability or decay. Numerous specimens of oak of all ages, and from many ancient buildings and ships, were put together for comparison; and also of oak cut at different times and prepared in different ways. The results of this mass of experience were drawn forth, and, as far as possible, embodied in a few general conclusions.

Feb. 12.—This evening Mr. Ritchie briefly developed the first principles of electro-magnetism, with the view of setting forth, in a distinct and practical manner, M. Ampere's proposal of carrying on telegraphic communication by means of this extraordinary power. Of course the principle consists in laying down wires, which at their extremities shall have coats of wire and magnetic needles so arranged, that when voltaic connections are made at one end of the system, magnetic needles shall move at the other. This was done by a small telegraph constructed for the purpose, where, however, the communication was made only through a small distance, the principle being all that could be shown in a lecture-room.

Some magnificent specimens of native platina from the Ural mountains were upon the library table.

Feb. 19.—A series of remarks on the comparative value of the water in different kinds of steam-engines, was made by Mr. Ainger. The use of water, alcohol, æther, condensed gases, &c. were first considered, with the promise of advantage held forth by their proposers;

posers; and the conclusion arrived at, that water is by far the most favourable. After this, the effect of increased pressure on the vapour was considered, and the probable advantages which might be philosophically deduced, independent of practice, were set forth.

YORKSHIRE PHILOSOPHICAL SOCIETY AND NEW MUSEUM.

Annual Meeting of the Yorkshire Philosophical Society; and Opening of the New Museum.

The anniversary meeting of the Yorkshire Philosophical Society was held February 2, for the first time, in the Theatre of the New Museum.

The Yorkshire Museum stands in an enclosure of about three acres, part of the site of the once rich and powerful Abbey of St. Mary *, which, since the Dissolution, has been the property of the Crown, and was munificently granted by His present Majesty in 1827, to the Yorkshire Philosophical Society. The venerable ruins of the Abbey occupy the north-western side of the enclosure; the Roman multangular tower and ancient city walls separate it from the city to the south-east. On an eminence in the centre, the Museum rears its noble front, looking down upon the river, and to the extensive landscape beyond. The entrance to the grounds from the city is by a Doric gateway, or propylæum. On either side of the walk leading thence to the Museum, the ground is appropriated to a Botanic Garden. The remainder of the enclosure is laid out and planted with a view to picturesque embellishment;—and with particular reference to the favourable display of the venerable remains of antiquity which adorn and consecrate the ground.

The front of the Museum extends 102 feet, and was designed by W. Wilkins, Esq., R.A. On the right of the hall is the library. A door on the left of the library leads to the staircase and council-room. Directly opposite the front door, corresponding folding doors lead into the theatre or lecture-room, a beautiful room, ornamented by six Corinthian columns, and four pilasters supporting beams enriched by guilloche ornaments. On the right and left of the lecture-room are spacious apartments, for the collections in geology and mineralogy; the former containing a suite of nearly ten thousand specimens of British rocks and fossils, arranged in the order of their position in the earth; the latter exhibiting above two thousand minerals, classed according to their chemical relations. At the back of the lecture-room, and connecting the two lateral rooms, is the museum for zoology.

The chair was taken by the President, the Rev. W. V. Vernon, who said, he regretted that he had to apologize for the absence of the Archbishop, who would not have been prevented from attending by any thing but a severe, though he trusted temporary indisposition.

He then read the Report of the Council, of which the following is an abstract.

* A highly interesting history and description of this abbey, the remains of which have lately been excavated, are published by the Rev. C. Wellbeloved, Curator of Antiquities to the Society, in the "Vetusta Monumenta," vol. v. of the Society of Antiquaries of London.

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The Council commence their Report by congratulating the Society on taking possession of its new premises, and having thus entered on a new æra of its existence. "Hitherto it could only be considered as preparing the plan, not as acting the part, of a scientific institution for the county of York. It has now that of which all who have attended to the history of such institutions, know the importance,—not a local only but a visible habitation, and stands forth to open view, challenging the public eye, and satisfying, as it is hoped, the public taste,—it calls with confidence on whatever public spirit, whatever respect or zeal for science is to be found, within the most extensive, the wealthiest, and the most patriotic district in England." After noticing the great obligations of the Society to the architects who had given it their valuable and gratuitous assistance, and stating that the Council had endeavoured to do justice to Mr. Wilkins's designs, by adhering to them with scrupulous fidelity, the Report gives an account of the plan contemplated for laying out the garden in such a manner as to combine a botanical distribution of the plants with a pleasing effect to the eye, mentions the intention of the curator * to adopt Decandolle's method of arrangement; and states, that by the activity of the sub-curator, lately appointed, 500 plants have been obtained; and that from the liberality to be anticipated in these donations, the Society may soon expect to possess a very large collection of hardy herbaceous plants.—The beautiful specimens of architectural sculpture displayed in the Museum are then mentioned, and a hope expressed, that large accessions will be made of other antiquarian relics. "A depositary for the numerous remains of Yorkshire antiquities has long been desired, and where can such a depository be formed with greater propriety than in the capital of this county, amidst so many existing monuments of ancient grandeur, and the yet distinct vestiges of successive generations; and under the auspices of an institution which will afford, in the increasing stores of its Museum, the readiest and most invaluable aid to the labours of the future topographer, as well as to those of the student and historian of nature. And though there is no part of real knowledge, whether of art or nature, to which the Society does not wish to apply itself, yet local information, and whatever relates to Yorkshire, are its principal objects:—of those objects, in whatever degree its establishment may be enlarged, it ought by no means ever to lose sight. Other collections within these walls will have their use for the instruction of the student; but as far as the progress of learning or science are concerned, intelligence of the district to which we belong, is our proper study, and our most useful pursuit."—Several instances are then noticed of the attention of the Society to subjects of local research, as in Mr. Rankin's account of the discovery of a Roman road on the Wolds; in Mr. Phillips's excellent illustrations of the geology of our coast, which have been the means of communicating to distant countries the information to be drawn from the Society's collections; and in the investigation of a very remarkable deposit of fossil bones on Mr. Worsley's estate, near North Cliff: it is added, that at the

* The Rev. William Hincks, M.A. F.L.S.

same

same time more general inquiries have not been neglected, and that the Society's cabinets have been enriched from every quarter, particularly by the contributions of Mr. Marshall, Mr. Stapylton, and Mr. Strickland.—The Report next announces that Mr. Phillips will deliver, during the present year, a course of lectures on the Œconomy and Organization of Animals; and the Curator of Botany (the Rev. Mr. Hincks) has offered to favour the Society, in the months of May and June, with eight lectures on that science, the profits of which will be appropriated to the garden fund.

The accounts of the Committee are arranged under six heads: the first of these includes the total expenditure of the main building (6860*l*). Under the second head is placed the expense of the lodge and entrance gates, the boundary walls, and outbuildings. The third comprises the repairs of the ruined Abbey, the excavations carried on to trace the old monastic foundations, the making of the roads and walks, and the formation of the garden. The furniture of the Museum, and the new cases prepared for the reception of specimens, form the fourth head: and under the fifth and sixth are classed the claims of the former occupiers, printing and other miscellaneous expenses. The sum total of the expenditure on all these heads is nearly 9800*l*.,—a sum greater indeed than had been anticipated, but not greater than was necessary to do justice to the plan of a County Museum, and to fulfill the wishes of the subscribers. The sum which is in hand to answer that demand, is about 8300*l*., thus leaving a debt of 1500*l*.; a debt which, the Report adds, will undoubtedly prove a heavy incumbrance, if means cannot be devised of paying it off. "We may indeed go on; but let the meeting consider in what manner we shall go on as respects the character and utility of the institution. We may go on, giving a salary of 60*l*. a year to the keeper of the Museum—and would any member of the Society wish to retain his services on such inadequate terms? We may go on—but with an unfurnished laboratory, and with a library to which the naturalist and the antiquary might refer in vain. We may go on—but we must leave it to others to explore, even in our own county, the mysteries of Nature, and to collect the monuments of Art."

From the Treasurer's Annual Report, which was laid before the meeting, it appeared, that during the last year the receipts amounted to 528*l*. 4*s*.; and the expenditure to 499*l*. 9*s*. 1*d*.; leaving a balance in the hands of the Treasurer of 28*l*. 14*s*. 11*d*.; which, added to 73*l*. arrears due from members, leaves 101*l*. 14*s*. 11*d*. in favour of the Society.

It was then moved by Sir George Cayley, and seconded by W. Marshall, Esq. that the warmest thanks of this Society are eminently due to the Rev. W. Vernon, for his uniform zeal in the execution of his arduous office as President of this Institution, but more especially for his indefatigable exertions in founding and completing this Museum.

The Resolution was carried with great applause.

The President, in returning thanks to the Meeting, said, that the Institution had risen to its present flourishing state by the combined exertions

exertions of many persons whom he had the pleasure of seeing assembled in that room; and of none more than of those friends whose kindness had so much amplified the services which he had rendered to the Institution. For himself, "he had only," in the words of Bacon, "taken upon him to ring a bell to awaken better spirits than his own." Whatever exertions he had made for the establishment of the Society, were founded upon fixed principles; upon the opinion, that such societies were eminently serviceable to the advancement of knowledge; and upon the conviction, that the advancement of knowledge is of the most real and practical importance to the best interests of mankind. He considered the promotion of science as the highest kind of charity, because science is the parent of all those arts which minister to the supply of human wants; and if a man has any wish to do good, how could he do it so extensively as by promoting the sciences on which those arts depend? Every one, he said, saw the utility of supporting a hospital, and it was surprising to him that every one should not also see the utility of supporting those sciences from which hospitals derived the power of doing good. The most powerful remedies for the alleviation of human suffering were the inventions of experimental philosophy,—chemistry. The great discovery of the circulation of the blood, on which so much of the treatment of diseases depended, was drawn by the philosophical mind of Harvey, from the analogy of the valves of the veins with the mechanism of a hydraulic engine. He considered the promotion of science also as one of the most powerful methods of promoting religion; and he had formed this opinion, not only from the obvious tendency of the contemplation of nature to fill the mind with religious impressions, but also from historical facts. That there was sometimes such an anomaly as an irreligious philosopher was true; but any one who examined the history of human opinions accurately, would find, that the times in which there was least science, had been those in which both superstition and atheism flourished most. In proof of this assertion, he cited some remarkable statements of Petrarch in the fourteenth century, and of Mersenner upon the rise of philosophy, in the sixteenth century.

The President then read a paper On the History and Influence of Literary and Scientific Institutions, tracing them from an early period to the present time, and remarking on their merits and defects. The following were some of his concluding observations:—"I wish the national Society in the metropolis of Great Britain for the important work of the advancement of human knowledge, were such in its establishments and resources as to be worthy of such a country as this; and I wish that all such Societies were founded on the plan of the New Atlantis in respect to one of its most essential principles, a principle which the Royal Society at its first institution was unhappily obliged to relinquish. One of the greatest defects in the state of learning remarked by Bacon was this;—that the sciences had never possessed a whole and entire man. He thought it 'necessary to their progression, that those who are to generate and propagate them should be placed in such a condition as may content the ablest man to to appropriate his whole labour, and continue his whole age in that function and attendance; otherwise,' says he, 'if the fathers of sciences be of the weakest sort, or ill maintained, *et patrum invalidi referent jejunia nati:*' to which he adds, 'There will hardly be any main proficiency in the disclosing of Nature, except there be some allowances for expenses about experiments, whether they be experiments relating to Vulcan or Dædalus, furnace or engine, or any other kind; and therefore as secretaries and spials of princes and states bring in bills for intelligence, so you must allow the spials and intelligencers of Nature to bring in their bills, or else you shall be ill advertised.' Now this defect has not been supplied in the constitution of the Royal Society, or of any of the Societies which have been since formed in England; in them the sciences have still, for the most part, no more than fractions of men. In saying this, I do not mean to undervalue the labours of those who unite scientific pursuits with the active business of life. I do not forget, that the very author of the remarks I have quoted, lived himself in the full tumult of human affairs, and was at once the first philosopher and the most consummate speaker and comprehensive lawyer of his day. I wish, indeed, it were enough considered, that men may contribute very usefully to philosophy without taking the name of philosophers. I wish it were fully understood, that philosophy itself is after all no more than an ignorant child capable of gaining instruction from the commonest observations, and being improved by the slightest hints: but how are these scattered lights to be concentrated, if there be no one whose business it is to collect them? How can we expect any great or uniform progress in the most arduous of all business from gratuitous services and broken time? In thus noticing, Gentlemen, the defects as well as the merits of other societies, it has been my object to deduce from this experience the path which we ought ourselves to pursue. To a great metropolitan institution the point of which I speak is only a question of a greater or less degree of vigour; to us I am persuaded it is a question of life or death. The edifice, indeed, in which we are now for the first time assembled, seems to promise us some kind of perpetuity; all the fortunate circumstances with which we are surrounded are so many pledges of permanence; but whether it shall be a perpetuity of honour, and a perseverance of utility, or whether we shall be obliged in the issue to own that we have abused the favour of the Crown, and wasted the money of the county, depends in my opinion principally on one thing, on our allowing or not allowing liberal salaries to the scientific servants of the Society, such salaries, that is, as will enable them to devote their whole time to the objects of the Institution. It is not the mere arrangement of a Museum, or the care of a Garden, which is to be provided for by such a Society as this,—the materials for a natural history of Yorkshire should be collected, its antiquities investigated, its arts and manufactures examined, and the principles of science applied to their improvement. There is also a philosophical correspondence to be maintained; there are scientific foreigners to be received; there are inquiries of students to be answered; lectures are to be delivered, original observations are to be made, and experiments devised. Not, indeed, that the whole of the labours should be devolved on the salaried officers, but that all which must be necessarily left defective by their more occupied coadjutors may be supplied, and the Institution kept uniformly moving towards its intended objects."

Mr. Phillips, the Keeper of the Museum, then enumerated the donations which had been received since the last meeting. One of the most attractive parts of the large display of donations accumulated in one month, was the collection of bones of antediluvian quadrupeds discovered on the property of W. Worsley, Esq. at Bielbecks, near Northcliff. Of these important reliquiæ, Mr. Worsley has determined to present a selection to the cabinet of the Society, already rich in such evidence of the ancient condition of the earth. Placed beside the similar series from the caves of Kirkdale, Banwell, Kent's Hole, and Plymouth, they will afford to every visitor of the Yorkshire Museum, the means of studying the structure and habits of the lion, hyæna, elephant, rhinoceros, and other gigantic animals, which formerly inhabited our island.

The DINNER was attended by upwards of sixty gentlemen. After which many animating addresses were delivered, from which we select the following:

The Chairman.—The toast I am going to propose to the present company, is one to which I hope we shall all contribute as much as is in our power. The prosperity of the Yorkshire Philosophical Society you heard much about in the morning; and no doubt we all feel great satisfaction at having built the Museum. It will be recollected, that when Gresham College was pulled down to build an excise office, nobody cared either about the lecturer or the lectures; they were transferred to a little room, over the Royal Exchange, and hurried over, to an audience consisting of very few persons: and thus the Institution, which was founded by that liberal citizen Sir Thomas Gresham, in the reign of Henry VIII., and which had produced many of the most renowned scientific professors, such as Briggs, Wren, and others,—has dwindled into insignificance. I hope that the reverse will be the case with this Society; and that the building of the Museum will be the means of giving to it increased efficacy and energy. Gentlemen, I beg to give, "Success to the Yorkshire Museum."

The Hon. E. Petre rose, and was warmly greeted.—In proposing the health of my reverend friend, (and proud am I to call him so,) I am sure it will be drunk with the respect it deserves. If we look at him in the various situations he fills; if we look at him as the Christian minister; if we look at him as the patron of science,—we shall find him in all actuated by the same well regulated zeal, the same spirit of true benevolence. I like to see the clergy come forward, and support every thing having for its object the extension of knowledge; for it shows to me, as it must to every sensible man, that they are influenced by a desire to promote the welfare of their species. You all know the rapid progress which knowledge is

making

making through this kingdom; you all know how the people are making great strides in the acquisition of knowledge; and it behoves every man to promote the diffusion of knowledge in every way which his humble abilities will permit. Gentlemen, I will not detain you any longer. I will only express the great gratification with which I appear before you this day, as the humble instrument for making a proposition I know will be so acceptable. It is the first opportunity I have had of showing my respect for my reverend friend; and I hope he will not think that my feelings are the less sincere, because I now so imperfectly express them.

The Chairman rose, and as soon as he could obtain a hearing from the cessation of the applause with which he was received, he said—I had undoubtedly the greatest reason, in the morning, to blush at hearing the very flattering manner in which I was taken notice of by my friend on my left; and I have reason now to be overwhelmed by feelings it is difficult to express. I am well aware that the compliments expressed are far greater than any exertions of mine can demand; but I am not the less grateful for the feeling, kind, and affectionate manner in which my health has been proposed, and in which the meeting received what my friend has been pleased to say of me; I am not the less delighted to find, that my exertions, whatever they may have been, are satisfactory to this meeting. But it is a maxim of common prudence to remember, "quid valeant humeri, quid ferre recusent." I am not so much elated by applause, of which I have reason to be proud, as not to know that, although in the infancy of the Institution I may have had it in my power to be of use to it, the case is different when it has arrived to maturity. Very different powers are required to nurse the infant and to govern the man; and I feel that the time is come, when a person of higher station in the county, and of greater abilities, should be called to occupy the office I now fill. Whilst the Society is in difficulties, however, it will not become me to relinquish it; whilst that is the case, you may always command my best exertions. And when I relinquish my post (which I shall be happy to do, because I shall leave the Society no longer in want of the assistance I have been able to give it), I shall still retain a vivid recollection of the uniform kindness of its members, and of those with whom I have been more immediately in the habit of acting; and to whose exertions I feel the Society is more indebted than it is to myself.

The Chairman proposed the health of the future Chief Magistrate of this city, The Hon. Edward Petre,—which was received with great applause.

Mr. Petre in returning thanks, expressed his earnest desire to be a supporter of every scientific institution in the city.

The Rev. D. R. Currer proposed the health of Sir George Cayley, the President of the York Mechanics' Institute.

Sir G. Cayley said,—Mr. President and Gentlemen, I thank you in the name of the Mechanics' Institute, for the honour you have conferred upon us. The Mechanics' Institute is a humble sister of the same family as the Philosophical Society. Science and Art stand, with respect to each other, as cause and effect, and each mutually aids the progress of the other. I am sorry to say, that though nominally the President of that Society, I am, in fact, from my distant residence, the most inefficient member it has. I am glad, however, to know that our virtual and real President (the Rev. C. Wellbeloved) is here. I shall call upon him to give us the particulars of the present state of the Institution. I may observe, generally, that I am aware that there is a divided opinion as to the utility of such societies; and that many hold a conscientious opinion, that is adverse to them. There seems to be a fear that they may induce the lower orders of society to become too cunning for their rulers;—surely society cannot be so constituted by Him who called us into existence, as to become too wise for itself. Even our Philosophical Society has not escaped without similar aspersions, and those from very high quarters; but I do not perceive that we are getting too wise at all. I cannot but believe that the best basis on which to build up the security of the British Government, is on the solid information and good sense of the people, and at the same time it forms the best security for the safe administration of the Government. Man is a social animal; when alone he is weak and helpless, but in combination becomes invincible. Almost every man has some mental perfection in conjunction with many deficiencies, but when called together in such a society as the Mechanics' Institute, for mutual instruction and assistance, the best powers of each are brought to bear upon the concerns of all, and many centuries of experience in their united ages are made available towards the common stock. To the powers of combined effort we owe the valuable Institution we are now met to celebrate; without it, all the materials for science would have been scattered unclassed and useless; so beautifully and so surprisingly however are interwoven the laws of creation, that out of these individual deficiencies arise most of the endearing ties of life. Had we all been perfect in our powers, and each as capable as the other in every particular, we should not have perceived the necessity of mutual assistance, and would never have experienced all that series of friendly exertion, gratitude, and affection, which flow from this source. But I have already said too much on this subject; I will only occupy your attention for a few moments longer, in mentioning that we have lately discovered, at the depth of four feet of alluvial deposit, within the basin of the extensive antediluvian lake which Dr. Buckland conceived to have flowed nearly up to the cave at Kirkdale, a Roman sword of brass in the most perfect preservation, a human jaw with the teeth perfect, and a copper coin of Hadrian, all within a few feet of each other, at the same depth, and resting upon the bed of gravel which seems to have formed the real bottom of this lake. I mention the circumstance, because as it is the fashion now to find human bones, and works of human hands, in company with antediluvian quadrupeds, (two instances in France and one in Germany,) I wish to guard against any false impression injurious to Dr. Buckland's very satisfactory theory —a morass with occasional floods will account for the case.

Sir

Sir J. V. B. Johnstone the President, and Mr. Dunn the Secretary, of the Scarbro' Society, returned thanks, on their healths being given.

Dr. Belcombe.—Mr. Chairman, I beg to give you the health of a gentleman who has been already alluded to, and whose name has only to be mentioned to be received with enthusiasm—the Reverend C. Wellbeloved.

Mr. Wellbeloved.—If my name is connected with the York Mechanics' Institute, it is my duty to acknowledge the honour: for I do feel honoured by my connection with that Institution, under our admirable President, who is always present with us in spirit, though we regret that he is frequently absent in person. We have a very admirable bust of him in the room where we hold our meetings, which always reminds us of him; at the same time we are sorry that we have only his bust. When the York Mechanics' Institute was set on foot, I took an active part in promoting it. I am proud that I did so; for it is a valuable Institution, and I am convinced that its tendency is good. I was told in the outset, that I should do a great deal of harm; that society would be disorganized; that we should have no subordination, no inferiors; that apprentices would be no longer bound by their indentures, but would rise into journeymen; that journeymen would leave their masters, and set up as masters for themselves; that we should have no servants; and in short, that we should have nothing but confusion. I have seen nothing of this—I have heard nothing of it—of nothing approaching to it. But I will tell you what I have seen. I have seen a number of young people assemble night after night, for the purpose of completing their necessarily deficient education. I have seen others meet to improve themselves in architecture or drawing; others attend lectures, and express the greatest anxiety to procure information. I have seen them devote hours, too frequently devoted to debauchery and dissipation, to the investigation of subjects interesting to all human beings: and seeing this, I rejoice that I have been enabled to do any thing to forward their views. I have seen too, a number of young persons come, night after night, to the library,—which, I regret to say, is not so well furnished as it ought to be,—for books, which they carry home with them, to read by their firesides; instead of being induced to seek for recreation in the haunts of idleness, and amongst idle companions. Since the commencement of this benevolent Institution, I have seen nothing else arising from it; and as long as this is the case, it shall have my services, and all the time I can bestow on it. I think we are all bound to support such Institutions. We have Infant Schools, Sunday Schools, National Schools, and Lancasterian Schools. What are they for, but to enable the population to read? When they are taught to read, will they not read? And what will they read? Are they to be left without guides to direct them what course to pursue? Mechanics' Institutes come in aid of these schools, to give the people proper books; and to keep them out of improper courses, which they would otherwise fall into. I believe that the members of Mechanics' Institutes will

will not fall into those false and erroneous opinions to which allusion was made in the morning. By instructing them, and cultivating their minds, we shall preserve them from the arts of Carlile and others, who go about the country to induce the ignorant and uninformed to embrace their erroneous and dangerous doctrines. I can see no objection to such Institutions; and I am happy to say, that not only Sir George Cayley, but our worthy President himself, is enrolled amongst the members of the York Mechanics' Institute; and I wish such examples were more generally followed. If, Sir, (addressing the Chairman) all in a similar rank to yourself would act as you do, and those who support you,—if they would employ their advantages in the same manner, there would be no danger of their ever being overtaken by their humbler brethren; they will always keep their station; and however long the spur may be in the toe of the mechanic, it will never reach the heel of the aristocracy.

The original founders of the Museum—A. Thorpe, W. Salmon, and J. Atkinson, Esqrs.—the gentlemen who laid the foundation of the Museum by the valuable donation of those splendid remains which were found at Kirkdale.

The Chairman gave the health of Mr. Smith, the Father of English Geology.

Mr. Smith returned thanks; and said, he could truly say with Mr. Atkinson, that nothing gave him greater pleasure than the promotion of science, and particularly in assisting this Society; for he had been very handsomely received in this county, where he had now been a resident many years. He had always endeavoured not to be behind in the *March of Intellect;* and had early habituated himself to habits of reflection and of combination; and this was the best way to acquire useful knowledge.—He then gave, at some length, an account of the progress of his discoveries; the origin of which he dated about forty years ago, when he was employed in superintending canals and coal mines, and in surveying land, in the west of England; and concluded by returning thanks for the honour the company had done him.

Mr. Phillips, This toast was drunk with very great applause.

Mr. Phillips rose to acknowledge the honour done him; and after returning thanks to the President and company, he said—I cannot but think of the day on which I first arrived in York. On entering the rooms, I found a small collection, but great zeal among the members; and though I did not then expect to be in any peculiar manner connected with the Society, I was sure its exertions must be successful: and when I was afterwards appointed to the situation I now hold, I expected to derive great pleasure and advantage: that it would enable me to follow pursuits in which I have always delighted; and bring me in contact with individuals much my superiors in talents and in opportunities;—these hopes have been fulfilled, and more than fulfilled. The years I have spent in York have been years of great pleasure and of great instruction; and I am gratified that my lot has fallen amongst those whose object is to cultivate knowledge, and to diffuse it wide as the winds.

winds. If health is spared me, I hope to fill the office in which I am placed to the satisfaction of the Society. It has placed in my hands great means of instruction; and from the extraordinary kindness and vast assistance I have received from all persons with whom I have been connected, I feel that much is demanded of me; and I declare that I will, as much as in me lies, discharge my duties with a view solely to the interests of the Society. I do feel, however inadequate may be the expression of those feelings, great gratitude for the manner in which I have been received, by the members generally, and by the officers of the Society particularly and individually; and I trust that my actions will not disappoint their good opinion of me.

On the health of the Curators being given, thanks were severally returned by E. Strickland, Esq., W. Marshall, Esq. Curator of Mineralogy, James Atkinson, Esq. Curator of Comparative Anatomy, and the Rev. C. Wellbeloved, Curator of Antiquities.

ASTRONOMICAL SOCIETY.

Jan. 8.—The following communications were read :—

1. Extract of a letter from Professor Schumacher to Francis Baily, Esq.

Occultation of Aldebaran, Altona, Dec. 9, 1829 :

	h	m	s	
Im.	23	41	12,27	Sidereal time, very exactly observed.
Em.	0	40	51,40	

"There were five observers, so placed that no one could be disturbed by the others. Each had his chronometer, compared before and after with the transit clock. *We saw nothing uncommon at this observation.* The immersion and emersion appeared instantaneous to all, except to Mr. Peterson, who fancied that the star adhered to the bright limb of the moon for a second, at the emersion. But the telescope used by this gentleman was the least perfect of the whole, and shows the stars not as small round discs, but with emerging rays."

2. Extract of a letter from George Dollond, Esq. to W. S. Stratford, Esq.

"The immersion of Aldebaran was observed (Dec. 9, 1829) at Ormskirk, near Liverpool, by the Rev. W. R. Dawes, at $5^h\ 36^m\ 20^s,25$ mean solar time (N. latitude 53° 34′ 15″).

	m	s	
Resulting longitude	11	36,5	West.
Longitude previously determined .	11	36,0	

"I have just received the monthly notice for November, containing a summary of the observations of Aldebaran. The appearances are so variable and uncertain, that I am convinced there is an optical deception depending upon the eye; and I believe the phænomena will vary as the observer directs his particular attention to the moon or to the star.

"On the 21st of August I observed the occultation, and directed my my attention particularly to the limb of the moon, allowing the star to approach, when it gradually disappeared behind the moon, until one-half of its apparent disc was hidden, the last half disappearing instantaneously. I also had an observation of the emersion on the 9th of December, when I allowed the star to pass from the moon for about two seconds, and then directed my attention particularly to the star. *It appeared to stop, and then return towards the moon's limb.*"

3. Observations made with a sextant of Dollond and a reflecting circle of Troughton, for ascertaining within what degree of accuracy these instruments would give the latitude of a *known* station, by Capt. Basil Hall, R. N.

At the request of Mr. Henderson, who was desirous of ascertaining the degree of accuracy to be expected from nautical instruments, Captain Hall undertook a series of observations to determine the latitude of the Calton Hill Observatory. The instruments used were, a sextant by Dollond, of 11 inches radius divided to 10″, the telescope magnifying eighteen times; a reflecting circle by Troughton, 10 inches diameter, divided to 20″, with three verniers; and an artificial horizon by Dollond. The time was taken from a half seconds' chronometer by Molyneux, compared before and after the observations with the transit clock.

The latitude of the Calton Hill Observatory, as determined by Mr. Henderson from the data of the Trigonometrical Survey, is 55° 57′ 19″,5.

The computations were made by Mr. Henderson from Schumacher's Hülfstafeln, and *no observation rejected.*

DOLLOND'S SEXTANT.

The index always moved the same way in making the observation and determining the index error.

1829,	Latitude.
Sept. 9, by a mean of 24 circum-meridional observations of the sun	55° 57′ 20″,0
16, by a mean of 16 (roof of horizon reversed)	55 57 18,1
by a mean of 40	55 57 19,0

TROUGHTON'S REFLECTING CIRCLE.

Captain Hall conceives there is a slight degree of flexure in the index bar; and that, if the index be pushed from the zero, face left, and also from the zero, face right, the angles given by the vernier will in both cases be too great, and *versâ vice**. To obviate this error, care was taken to move the index *in the same direction,* face right and face left.

	Latitude.
Sept. 12, 13, 14, 15, 16, by a mean of 58 circum-meridional observations of the sun, face east, face west, the roof reversed, &c.	55° 57′ 16″,9

* [If these precautions be neglected, it would seem, from Capt. Hall's observations, that an extreme difference of 13″ might arise in his circle from this cause alone. Query: Does not this difference arise from a twist in the centre work?—Sec.]

CAPTAIN

CAPTAIN KATER'S LITTLE CIRCLE (THREE INCHES DIAMETER.)

Sept. 11, by a mean of 10 observations of Polaris .... 55° 57′ 15″,0

The observations were made in the passage of the Observatory, leading to the centre of the building; and Capt. Hall is enabled to contradict most satisfactorily an opinion that the Edinburgh Observatory is unstable or liable to tremors. Though the hill was covered with people, the streets below traversed by carriages and waggons, and workmen were carrying on their operations within the enclosure of the Observatory, Capt. Hall declares that he never placed an artificial horizon on ground where there were so few tremors.

4. On the longitude of the Armagh Observatory, as determined by transits of the moon and moon-culminating stars, by Dr. Robinson.

This method of determining the differences of longitude between two distant places is superior to all other methods in ease and simplicity, and would seem to be scarcely inferior in accuracy when a considerable number of observations is employed. Ever since the annual lists of moon-culminating stars have been published, Dr. Robinson has regularly observed them, partly to furnish corresponding observations for other astronomers, and partly to confirm and improve the longitude of Armagh, determined by his predecessors. He was thus led to notice a source of error perhaps not sufficiently attended to, and to adopt a correction which he conceives will add to the accuracy of the results to be obtained.

It is well known that telescopes give different diameters of the heavenly bodies, which appear too large in small and inferior instruments. When such are used, the transit of the moon's first limb will appear too early, and that of the second too late; whence, as the stars are wholly unaffected, the longitudes deduced from observations of a single limb will be erroneous to between twenty and thirty times this spurious increase of disc, compared to the disc shown by the standard telescope of comparison. This error may be easily eliminated when an equal number of transits of each limb can be obtained, but this is obviously impossible in most cases, and inconvenient in all.

The quantity of this optical defect, as it respects the sun, is easily ascertained for any telescope, by comparing the diameter as given by his transit with that given in the Ephemeris, or by the standard transit telescope. Sixty observations of both limbs gave the solar irradiation of the old Armagh transit compared directly with the Greenwich transit $+0^s,14$, and compared with that of Dublin $+0^s,32$.

Using these comparative solar irradiations respectively as an approximate correction for the lunar irradiation, Armagh was found to be west of Greenwich,—

	m	s
by 16 corresponding observations ☽ 1 *L* at Armagh and Dublin*	26	23,52
by 10 ——————————— at Armagh and Greenwich	26	26,71

The differences of these from the true longitude (26^m 30^s W. nearly) previously determined, being so nearly proportional to the

* Longitude of Dublin, determined by the Bishop of Cloyne, 25^m 22^s W. assumed

assumed or solar irradiation, Dr. R. was led to conjecture that the lunar irradiation may be obtained, generally, by multiplying the solar irradiation by 1·7 *.

In 1827 the new transit was put up at Armagh, and the solar irradiation was found, by seventy-four observations, to be $+0^s,01$ compared with Greenwich, and $+0^s,19$ with Dublin, through the data of the *Nautical Almanac;* whence the comparative lunar irradiations would be respectively $+0^s,02$ and $+0^s,32$, on the preceding hypothesis. With this correction, the longitude of Armagh was found to be

		m	s
by 10 corresponding observations ☽ 1 *L*	Armagh and Dublin	26	30,49 W.
5 ———— ☽ 2 *L*	————	26	30,34 W.
	Mean...	26	30,40

which must be very near the truth.

by 6 corresponding observations at Armagh and Greenwich 26 29,89

but with a much greater probable error.

It would appear, therefore, that this mode of obtaining the comparative lunar irradiation applies to the new transit as well as to the old†.

All the results have been obtained by the method of minimum squares, and the probable errors are also given

The formula is stated, by which the longitude is deduced from the observations, with a type of the calculation.

XXXIII. *Intelligence and Miscellaneous Articles.*

ELECTRICAL FORMATION OF CRYSTALLIZED SULPHURET.

MANY chemical compounds which occur in the bowels of the earth, may also be produced in the laboratory; and there are some others which may also be obtained artificially, but not crystallized as they occur in nature.

M. Becquerel has attempted to supply the deficiency which existed in this branch of science, and to furnish geology with the requisite facts to explain their formation. It results from his labours, that what was required in the apparatus of chemists to enable them to emulate the products of nature, was the influence of electric forces acting with slight tension and in a continued manner.

* This greater irradiation of the moon may be owing to her greater brilliance, and to the sympathy excited in the adjacent portions of the retina. Dr. R. finds that the lunar irradiation is much less in the day-time. During the winter months, the dark limb of the moon might probably be observed with powerful telescopes, and the irradiation determined.

† Old transit of Armagh 3 ft. focal length, 2 in. aperture, partly covered by an illuminator.

New ————	5 ————	$3\frac{3}{4}$ ————	limited for sun and moon to $2\frac{3}{4}$.
Greenwich transit...	10 ————	5 ————	
Dublin transit	6 ————	4 ————	of which $2\frac{3}{4}$ only are used.

In

In a memoir which M. Becquerel has read to the Academy, he has given an account of experiments upon the crystallized metallic sulphurets, beginning with that of silver. The apparatus employed consisted of two glass tubes, the lower part of which was filled with very fine clay slightly moistened, with a fluid conductor of electricity. In the upper part were poured the fluids, the reaction of which upon each other and upon the plate of metal, one end of which was placed in each of them, occasioned the electrical effects requisite to produce the compounds. The two tubes were placed in another which contained a fluid to establish a communication between them. In order to obtain the sulphuret of silver, a saturated solution of nitrate of silver is put into one of the tubes, and a saturated solution of hydrosulphuret of potash in the other, and the ends of a plate of silver is placed in both of them. That which is in contact with the nitrate, is soon covered with metallic silver; whilst at the other, which is the positive pole, water and sulphuret of silver are formed, and the latter combines with the sulphuret of potassium. This double sulphuret is gradually decomposed by the action of the nitric acid, which takes place slowly, because in chemical decompositions effected by electrical forces of slight tension, the oxygen goes first alone to the positive pole, and the acid follows afterwards. Sulphuret of potash is formed; the sulphuret of silver is developed and crystallized in beautiful small octahedral crystals, the appearance of which resembles that of the crystals of the same substance found in silver mines.

The crystallization of the sulphuret of silver is owing to the very slow decomposition of the double sulphuret, which gives the molecules time to effect the oscillatory motion necessary for similar faces to react upon each other, according to the laws of crystallization.

Sulphuret of copper perfectly resembling the natural compound may be obtained by the same process. Oxysulphuret of antimony or kermes is procured in small octahedral crystals of a deep brownish red colour, and in laminæ. As to the sulphurets of iron and zinc, which decompose readily by the simultaneous action of air and water, it is necessary to employ certain precautions to prevent this decomposition. M. Becquerel has however succeeded in obtaining very small cubic crystals of sulphuret of iron, of a yellow colour, resembling that of pyrites. The iodides, the bromides, and crystallized metallic seleniurets may be obtained by the same process. The iodide of lead is in brilliant yellow octahedral crystals; that of copper has a similar form, but a different colour.

M. Becquerel concludes from these results, that it is probable that nature has adopted a similar process in forming natural sulphurets; and he explains how the phænomena may operate at the instant of the consolidation of the masses.—*Le Globe*, Nov. 4, 1829.

Without in the slightest degree questioning the accuracy of the above detailed experiments, I may be allowed perhaps to doubt whether the inferences deduced from them are perfectly correct; for there are facts on record which prove that sulphuret of iron, in crystals, may be formed *viâ humidâ*. In the Phil. Trans. for 1798, there is is an account by Mr. Wiseman of the formation of pyrites in the Mere of Diss, a piece of water containing a vast quantity of mud derived from the streets of the town, where it has been accumulating for ages. Mr. Hatchett, who examined both the pyrites and the water in which it was produced, remarks that "to Mr. Wiseman's observations we are much indebted, as they make known the recent and daily formation of martial pyrites, and other ores, under certain circumstances. It is not to be supposed," he continues," that such effects are local, or peculiar to Diss Mere; on the contrary, there is reason to believe that similar effects on a large scale have been and are now daily produced in many places. The pyrites in coal mines have, probably, in great measure thus originated." Mr. Hatchett makes other and similar observations, which it is not requisite that I should copy.

Another instance of the recent formation of sulphuret of iron from solution is detailed by Mr. Pepys in the Transactions of the Geological Society, vol. i. p. 399. Observing some oily matter and hairs on the surface of a solution of sulphate of iron which had remained undisturbed for about twelve months, he poured off the solution, and found "at the bottom of the vessel a sediment consisting of the bones of several mice, of small grains of pyrites, of sulphur, of crystallized green sulphate of iron, and of black muddy oxide of iron." Instead then of supposing that electricity had any share in the production of this effect, I agree entirely with the observation of Mr. Pepys, that "these appearances may with much probability be attributed to the mutual action of the animal matter, and the sulphate of iron, by which a portion of the metallic salt seems to have been entirely deoxygenated."—R. P.

SIR HUMPHRY DAVY AND DR. WOLLASTON.

The following eloquent and accurate estimate of the powers of the above-named illustrious and lamented philosophers, is given by Dr. Henry in the Preface to the eleventh edition (just published) of his excellent work, The Elements of Experimental Chemistry:—a work which we again strenuously recommend to every cultivator of the science of which it treats.

"It is impossible to direct our views to the future improvement of this wide field of science, without deeply lamenting the privation, which we have lately sustained, of two of its most successful cultivators, Sir Humphry Davy and Dr. Wollaston,—at a period of life, too, when it seemed reasonable to have expected, from each of them, a much longer continuance of his invaluable labours. To those high gifts of nature, which are the characteristics of genius, and which constitute its very essence, both those eminent men united an unwearied industry and zeal in research, and habits of accurate reasoning, without which even the energies of genius are inadequate to the achievement of great scientific designs. With these excellencies, common to both, they were nevertheless distinguishable by marked intellectual peculiarities. Bold, ardent, and enthusiastic, Davy soared to greater heights; he commanded a wider horizon; and his keen vision penetrated to its utmost boundaries. His imagination, in the highest degree

gree fertile and inventive, took a rapid and extensive range in pursuit of conjectural analogies, which he submitted to close and patient comparison with known facts, and tried by an appeal to ingenious and conclusive experiments. He was imbued with the spirit, and was a master in the practice, of the inductive logic; and he has left us some of the noblest examples of the efficacy of that great instrument of human reason in the discovery of truth. He applied it, not only to connect classes of facts of more limited extent and importance, but to develop great and comprehensive laws, which embrace phænomena, that are almost universal to the natural world. In explaining those laws, he cast upon them the illumination of his own clear and vivid conceptions;—he felt an intense admiration of the beauty, order, and harmony, which are conspicuous in the perfect *Chemistry of Nature;*—and he expressed those feelings with a force of eloquence, which could issue only from a mind of the highest powers, and of the finest sensibilities.—With much less enthusiasm from temperament, Dr. Wollaston was endowed with bodily senses of extraordinary acuteness and accuracy, and with great general vigour of understanding. Trained in the discipline of the exact sciences, he had acquired a powerful command over his attention, and had habituated himself to the most rigid correctness, both of thought and of language. He was sufficiently provided with the resources of the mathematics, to be enabled to pursue, with success, profound inquiries in mechanical and optical philosophy, the results of which enabled him to unfold the causes of phænomena, not before understood, and to enrich the arts, connected with those sciences, by the invention of ingenious and valuable instruments. In *Chemistry,* he was distinguished by the extreme nicety and delicacy of his observations; by the quickness and precision with which he marked resemblances and discriminated differences; the sagacity with which he devised experiments, and anticipated their results; and the skill with which he executed the analysis of fragments of new substances, often so minute as to be scarcely perceptible by ordinary eyes. He was remarkable, too, for the caution with which he advanced from facts to general conclusions; a caution which, if it sometimes prevented him from reaching at once to the most sublime truths, yet rendered every step of his ascent a secure station, from which it was easy to rise to higher and more enlarged inductions.—Thus these illustrious men, though differing essentially in their natural powers and acquired habits, and moving, independently of each other, in different paths, contributed to accomplish the same great ends—the evolving new elements; the combining matter into new forms; the increase of human happiness by the improvement of the arts of civilized life; and the establishment of general laws, that will serve to guide other philosophers onwards, through vast and unexplored regions of scientific discovery."

From the same work we also copy the annexed view, which contains the latest facts known respecting the

Compounds of Phosphorus and Hydrogen.

"The section on these compounds (vol. i. p. 459) having been recomposed

composed under circumstances of interrupted attention, a few errors have crept into the text, towards which I must request the reader's indulgence. In that section I have endeavoured to extract, from a mass of conflicting testimony, the most probable evidence of the two generally admitted species of phosphureted hydrogen gas, considering the third variety as of too doubtful existence to be reasoned upon as a definite compound. The experiments of Dumas, of which I have given an account, have been recently repeated by M. Buff, in the laboratory of M. Gay Lussac. So far as respects proto-phosphureted hydrogen, the results of Dumas were fully confirmed, especially that 100 volumes require 200 of oxygen gas for saturation. But in the combustion of per-phosphureted hydrogen with oxygen, some differences were observed. This gas (containing from 13·5 to 14·5 per cent of impurity), when heated in a graduated glass vessel, deposited phosphorus without any change of volume, and, though it lost its spontaneous inflammability, continued to be absorbable to the same amount by solution of sulphate of copper. Besides being decomposed by keeping, its composition appeared to be varied by the degree of heat used in its production, so that we can never be sure of having it twice alike. Heated in contact with copper, each volume expanded to 1·5; mixed with three volumes of carbonic acid it burned completely both with oxygen gas and with air, without leaving any trace of phosphorus. In the quantities of oxygen consumed by the combustion of a given volume, great and unaccountable variations were observed, the lowest being 204, and the highest 270 volumes of oxygen, to 100 of the per-phosphureted gas.

"Both gases agree in giving 1·5 volume of hydrogen from each volume, when decomposed by antimony, zinc, potassium, or bi-chloride of mercury; as well as in being absorbable by sulphuric acid, sulphate of copper, and chloride of lime.—The following Table, which I have compiled from various authorities, exhibits the two species in contrast with each other.

"*Table of the Gaseous Compounds of Hydrogen and Phosphorus.*

NAMES.	Specific gravity. By expt.	Specific gravity. Calculated.	Each vol. takes oxygen.	Product.	Each vol. contains vols. of hyd. phos.	By weight consists of hyd. phos.
1. Proto-phosph[d] hydr[n] gas (*not spontaneously inflammable*)	1·214	1·2067	2· or 1·5	Phosph[c] acid... Phosph[s] acid.	1·5+0·5	1+10·5917
2. Per-phosphur[d] hydr[n] gas (*spontaneously inflammable*)	1·761	1·7580	2·675 or 1·875	Phosph[c] acid... Phosph[s] acid.	1·5+0·75	1+15·8875

"The following Tables are added for the better comparison of the composition of the phosphoric and phosphorous acids, as deducible from the two proportions of oxygen, with which each variety of phosphureted hydrogen unites.

"*Results*

"*Results of the Combustion of the two Species of Phosphureted Hydrogen Gas with Oxygen Gas.*

	= Vols. of hydr. phosp.		For phosphoric acid.	For phosphorous acid.
8 vols. of proto-phosphureted gas	12 + 6	take oxygen	16	12
	Deduct oxygen due to the hydrogen..		6	6
	Oxygen due to the phosphorus......		10	6
8 vols. of per-phosphureted gas	12 + 9	take oxygen	21	15
	Deduct oxygen due to the hydrogen..		6	6
	Oxygen due to the phosphorus......		15	9

"The results of the combustion of both gases conspire, therefore, if correct, to prove that the proportion of oxygen in the phosphoric acid is to that in the phosphorous as 10 to 6, or 15 to 9, which are the same proportions as 5 to 3.

"From a review of the compounds of hydrogen and phosphorus, I am disposed, however, to abide by the opinion expressed, vol. i. p. 465, that more facts are wanted to decide the constitution of these gases. The facility of obtaining the proto-phosphureted hydrogen in a state of purity, the uniformity of its composition when first evolved, its permanency when long kept, and its uniting constantly with the same proportion of oxygen, point out that gas as a true chemical compound. Whereas, the variable purity of the per-phosphureted species, its change of constitution by mere standing, and the uncertainty as to the proportion of oxygen required for its combustion, show an instability of composition, which is inconsistent with the characters of a true chemical compound.

"The equivalent number for phosphorus, however, as derived from the composition of either species of phosphureted hydrogen, does not accord with the relative weight of that substance (viz. 12), deducible from its compounds with oxygen. The nearest approximation to the number attained by the latter method is 10·5917. Multiplied and more correct experiments will, it is to be hoped, reconcile these incongruities."

ANALYSIS OF SILICEOUS MINERALS BY ALKALINE CARBONATES.

The ready fusion, observed by M. Berthier, of many atomic mixtures of salts, may be applied to the analysis of siliceous minerals by alkaline carbonates, aided by a spirit-lamp. A mixture of five parts of carbonate of potash and four parts of carbonate of soda is so fusible, that between 200 and 300 grains may be rendered perfectly liquid by a spirit-lamp flame. If sand be added to the mixture, there is an effervescence as lively as if an acid had been added. This effervescence occasions the expulsion of part of the substance; and by the addition of too much sand, the mass would become too difficult of fusion,

fusion, unless the sand or mineral had been previously pulverized and mixed with the carbonates. Hence the operation should commence with the mixture of the carbonates and the mineral. In this manner considerable quantities of felspar may be readily decomposed by the heat of a spirit-of-wine lamp.—*Annalen der Physik*, 1828. *Royal Institution Journal.*

EXAMINATION OF SOME MINERALS. BY M. VICTOR HARTWALL.

Fergusonite.—This mineral, named in honour of Robert Ferguson, Esq. of Raith, occurs near to Kikertauvak, not far from Cape Farewell, in Old Greenland. On account of its near resemblance to Ytestantalite, it was referred to that species, until Haidinger, by a careful survey of its crystals, proved it to be a new species. Being analysed, it afforded the following constituent parts:

Colombic acid	47·75
Yttria	41·91
Oxide of cerium	4·68
Zirconia	3·02
Oxide of tin	1·00
——— uranium	0·95
——— iron	0·34
	99·65

Manganesian Epidote or Pistacite.—The mineral found at St. Marcet, in Piedmont, and known to mineralogists under the name of Manganesian Epidote, was referred to the epidote genus, on account of its series of crystallizations.

This mineralogical determination it was desirable to have confirmed by chemical analysis; and further, chemists were curious to know the particular state of oxidation of the manganese and iron which it contains. The following is the analysis of M. Hartwall:

Silica	38·47
Alumina	17·65
Lime	21·65
Peroxide of manganese	14·08
——— iron	6·60
Magnesia	1·82
	100·27

M. Hartwall infers by the calculation of the result of the analysis, that the manganese and iron occur in the mineral in the state of peroxide. This is proved, not only by the diminished quantity of the isomorphous alumina along with them, but also by the reddish-brown colour of the mineral.

Pyrophillite, a new mineral, by M. R. Hermann of Moscow.—This mineral occurs in the Uralian Mountains, and is known to mineralogists under the name of Radiated Talc. But its relations before the blowpipe are different from those of indurated talc. Heated before the blowpipe, without any re-agent, it divides in a fan-shaped manner into a swollen mass, which occupies twenty times the

the space of the original specimen. The pounded mass is quite infusible. If heated in a glass retort, there condenses in the upper part of it a water which does not attack the glass, and which on evaporation leaves no silica. Soda dissolves the mineral, with effervescence, into a clear yellow glass. Phosphoric salt dissolves it into a colourless glass, leaving a siliceous skeleton. It acquires a blue colour with solution of cobalt. By these characters the mineral is well marked, and is distinguished from talc, particularly by its relations with solution of cobalt, its aqueous contents, and its fan-splitting by heating. Subjected to analysis it yielded :

Silica	59·79
Alumina	29·46
Magnesia	4·00
Oxide of iron	1·80
Water	5·62
(A trace of oxide of silver.)	
	100·67

The name of Pyrophillite is given to it on account of its exfoliation on exposure to heat.—*Jameson's Journal*, Jan. 1830.

IMPROVEMENT IN THE SMELTING OF IRON.

Heated air for blast furnaces has been used for some time at the Clyde Iron Works, and with great success. Experiments have proved that iron is smelted by heated air, with three-fourths of the quantity of coal required, when cold air, that is air not artificially heated, is employed for that purpose, while the produce of the furnace in iron is at the same time greatly increased. All the furnaces at Clyde Iron Works are now blown with it. At these works the air, before it is thrown into the blast furnaces, is heated 220° of Fahr. in cast iron vessels placed on furnaces, similar to those of steam-engine boilers. It is expected that a higher temperature than 220° will be productive of a proportionally increased effect. But this is a subject of experiment. It is supposed that this improvement will accomplish a saving in the cost of the iron in Great Britain, to the amount of at least £200,000 a year.—*Ibid.*

NEW VEGETO-ALKALIES OBTAINED FROM CINCHONA.

Dr. Serturner, in re examining the products obtained by chemical means from the cinchonas, finds that the precipitates produced by alkalies from the acidulated infusion of these barks contains, besides cinchonia and quina, other vegeto-alkalies, which are to be considered as modifications of the former. The new bodies recall the case of opium to the mind, in which narcotine exists simultaneously with morphia. The new substances, and especially that named by M. Serturner *chinioïdia*, exist in the alkaline precipitate in intimate combination with a resinous subacid substance, which is not injurious, but is of no advantage. It is very difficult to separate these two substances, and M Serturner succeeded only when he used the charcoal obtained when croconic acid is prepared by Liebig's proce s. This substance, combined with animal charcoal, completely decolours the solution of the alkaline matter in sulphuric acid (diluted with three or four pints of water); but it is necessary afterwards to act on the thick solution with alcohol, to separate earthy salts.

The new vegeto-alkalies exist in the red and yellow cinchona with the quina and cinchonia. The chinioïdia has more alkaline power and capacity of saturation, and also more medical power, than any other vegeto-alkali in the cinchona; but it resembles them by its insolubility in water, its colour, and taste. Its alkaline reaction on known vegetable colours, and its intimate state of combination with the brown extractive matter, are remarkable. Its salts are very fusible by heat, and become viscid like some balsams.

According to M. Serturner, in febrifuge power, chinioïdia is as superior to quina and cinchonia as these are to ordinary bark. It is to this alkali that many cinchonas are indebted for their medical powers. M. Serturner has, in many cases, given his new medicine in doses of 2 grains three times per day. The patients take a little vinegar after each dose, for the purpose of saturating the gastric juice, which by its alkaline nature would else decompose the salt. From 12 to 24 grains have, in all cases, sufficed to prevent the return of the fever; whilst patients in the same neighbourhood, treated with the sulphate of quina, had frequent returns of the disease.—*Hufeland's Journal. Royal Institution Journal.*

ANALYSIS OF INDIGO.

The following are the results of an analysis of a specimen of indigo, denominated *fine blue* in the Calcutta market,—made in the year 1820:

Oxide of iron	5·75
Alumina	0·75
Lime	0·90
Green vegetable matter	8·80
Red or brown ditto	2·00
Pure indigo	79·50
Loss	2·30
	100.00

Royal Institution Journal, Jan. 1830.

PROCESS FOR PROCURING BROMINE.

According to M. Lowig, the following is an advantageous method of preparing bromine:—He evaporates the mother water of the salt springs of Kreutznach, in large iron vessels, to one-third of its original quantity, and allows crystallization to take place for some days. He decants the supernatant liquor, dilutes it with water, and adds sulphuric acid until no further deposition occurs: he then strains the liquor, presses the residue, and evaporates it to dryness. The remaining matter is dissolved in an equal weight of water: by this a great quantity of insoluble sulphate of lime is separated; it is then distilled

distilled with oxide of manganese and muriatic acid. Excess may be added, as there is no danger of forming chloride of bromine.

M. Lowig does not perfectly agree with M. Balard as to the specific gravity and boiling point of bromine. Thus, according to M. Balard, the sp. gr. is 2·966; while M. Lowig finds it to be 2·98 to 2·99 at 59° of Fahr. and its boiling point he has ascertained to be 113° instead of between 116° and 117° Fahr.—*Journal de Pharmacie*, Dec. 1829.

ELASTIC POWER OF STEAM *.

MM. Arago, De Prony, Ampère, Girard and Dulong, constituting a committee appointed by the French Academy to determine the elastic power of steam at high temperatures, have given the following Table as the results of their investigation:

Elasticity of Steam atmospheric pressure taken as unity.	Corresponding temperature on Fahrenheit's scale.	Elasticity of Steam atmospheric pressure taken as unity.	Corresponding temperature on Fahrenheit's scale.
1	212°·00	13	380°·66
1½	233·96	14	386·94
2	250·52	15	392·86
2½	263·84	16	398·48
3	275·18	17	403·82
3½	285·08	18	408·92
4	293·72	19	413·78
4½	301·28	20	418·46
5	308·84	21	422·96
5½	314·24	22	427·28
6	320·36	23	431·42
6½	326·26	24	435·56
7	331·7	25	439·34
7½	336·86	30	457·16
8	341·78	35	472·73
9	350·78	40	486·59
10	358·88	45	499·13
11	366·85	50	500·60
12	374·00		

Le Globe, Dec. 9, 1829.

COMPOUNDS OF IODINE.

M. Serullas has lately read a memoir to the Academy of Sciences "On the action of different acids upon the neutral Iodate of Potash," &c. &c.

The following are the principal results:—That there are two iodates of potash; the first is produced by the imperfect saturation

* See Phil. Mag. Dec. 1822, and Jan. 1827.

 of

of chloride of iodine with potash; it is a crystalline double compound, which being separated and crystallized, gives the bi-iodate of potash.

The other iodate results from the action of one of the following acids on the neutral iodate of potash:—the sulphuric, nitric, phosphoric, muriatic, and silicated fluoric; or it may be obtained directly by treating a great excess of iodic acid with potash.

During the incomplete saturation of chloride of iodine by potash, a double compound is formed of chloride of potassium and acidulous iodate of potash, in definite proportions.

No such compounds as acidulous iodate or chloro-iodate of soda exist. Davy's process of obtaining iodic acid by the oxide of chlorine and iodine may be advantageously replaced, by that of precipitating the soda from the iodate by means of silicated fluoric acid.

M. Serullas has also discovered a process by which iodic acid is obtained abundantly and well crystallized. It consists in treating a solution of iodate of soda with an excess of sulphuric acid. The mixture left to spontaneous evaporation gives in a short time pure crystals of iodic acid. The sulphate of potash formed and the excess of sulphuric acid remain in solution. M. Serullas concludes from his researches, that the double acids designated by Davy iodo-sulphuric, iodo-nitric and iodo-phosphoric, do not exist; and he attributes Davy's mistake to the small quantity of materials which he employed in his experiments, and their being made only once.—*Ibid.*

MANUFACTURE OF PAPER FROM ULVA MARINA.

Specification of a Patent for manufacturing Paper from a material not heretofore used for that purpose. Granted to Elisha Hayden Collier, *of London, but late of Plymouth County, Massachusetts, April* 15, 1828.

The following is the description of my mode of manufacturing paper from a marine production, or sea grass, designated by botanists, as "Ulva marina."

First: all rock, roots, and shells, to be carefully separated from it.

Secondly: the dust to be cleared from it, by beating it.

Thirdly: to be steeped in lime-water, in order to discharge the salt from it, and thus prevent decomposition.

Fourthly: to be partially pulverized. (It can be bleached perfectly white by the use of oxymuriate of lime, otherwise called chalonic acid) [chloride of lime].

Fifthly: to be made into pulp in the usual manner, either by beating, or in a paper engine.

Sixthly: to be dipped, pressed, sized, and dried in the usual way.

As the sea-grass, or Ulva marina, is capable of being manufactured into paper by other modes than that above described, I claim as my invention, the manufacture of paper from the said sea-grass, or Ulva marina, not by any particular mode, but by any process whatever which it may undergo; and whether such paper is composed entirely of the said sea-grass, or Ulva marina, or mixed in any proportion with other materials heretofore known, or used for the manufacture of paper.—E. H. Collier.—*Journ. of Franklin Institute,* vol. iv.

LIST OF NEW PATENTS.

To T. J. Fuller, Commercial-road, Limehouse, Middlesex, civil engineer, for his improved mechanical power, applicable to machinery of different descriptions.—Dated the 28th of October 1829.—6 months allowed to enrol specification.

To G. Danre, Birmingham, manufacturer, for his self-acting air or gas regulator, or stop-cock, for governing the flow of air or gas, which may be applied to other purposes.—2nd of November.—6 months.

To J. M^cCurdy, Great James-street, Bedford-row, gentleman, for his improvements in the method of constructing mills and mill-stones for grinding.—2nd of November.—2 months.

To J. Viney, Piccadilly, colonel in the royal artillery, for his improvements in steam-boilers, and in carriages or apparatus connected therewith.—2nd of November.—6 months.

To J. Soames, junior, Wheeler-street, Spitalfields, soap-maker, for his preparation or manufacture of a certain material produced from a vegetable substance, and the application thereof to the purposes of affording light and other uses. — 2nd of November. — 6 months.

To J. Tucker, Hammersmith, brewer, for his exploding shot or projectile.—2nd of November.—6 months.

To J. Stewart, George-street, Euston-square, pianoforte-maker, for his improvements on pianofortes.—2nd of November.—2 months.

To J. Cowderoy, Britannia-street, City-road, gentleman, for his improvements in machinery for making bricks.—2nd of November. —6 months.

To F. Naish Stoneason, Wells, Somerset, gentleman, for his improvements in the manufacture or application of silks mixed or combined with other articles.—2nd of November.—2 months.

To W. Gooch, Mount-street, Berkeley-square, for his improvements on baths of different descriptions, which improvements are applicable to other purposes.—7th of November.—6 months.

To D. Macdougal, Edinburgh, horticulturist, for his improvements on syringes, applicable to garden and other purposes.—10th of November.—6 months.

To T. Osler, Birmingham, chandelier-furniture-manufacturer, for his improvements in the construction of glass and metal chandeliers, and other articles for ornamental lighting.—10th of November.— 6 months.

To J. Gibbs, Crayford-mills, Kent, timber-merchant, for his improvements in machinery for cutting marble, wood, and other substances.—12th of November.—6 months.

To J. W. Dodgson, Lower Shadwell, Middlesex, pump and engine maker, for his improvements in ships' scuppers, and which may be applied to other purposes.—17th of November.—6 months.

To T. Gethen, Furnival's-inn, London, gentleman, for his improvements in dressing woollen cloths.—21st of Nov.—6 months.

To W. Clutterbuck, Oglebrook, Stroud, Gloucester, for his improvements in the shears used for cutting or cropping of woollen cloth, and other fabrics requiring shearing.—21st of November.— 2 months.

METEORO-

METEOROLOGICAL OBSERVATIONS FOR JANUARY 1830.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·59. Jan. 1. Wind N.E.—Min. 28·60 Jan. 20. Wind E.
Range of the mercury 1·99.
Mean barometrical pressure for the month .... 29·969
Spaces described by the rising and falling of the mercury............ 7·060
Greatest variation in 24 hours 1·050.—Number of changes 22.
Therm. Max. 44° Jan. 7. Wind N.W.—Min. 17° Jan. 31. Wind N.E.
Range 27°.—Mean temp. of exter. air 32°·98. For 30 days with ☉ in ♑ 30·30
Max. var. in 24 hours 21°.—Mean temp. of spring-water at 8 A.M. 48·15

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere in the evening of the 24th ... 96°
Greatest dryness of the atmosphere in the afternoon of the 10th... 56
Range of the index 40
Mean at 2 P.M. 79°·5.—Mean at 8 A.M. 82°·8.—Mean at 8 P.M. 82·3
—— of three observations each day at 8, 2, and 8 o'clock .... 81·5
Evaporation for the month 0·78 inch.
Rain in the pluviameter near the ground 2·81 inches.
Prevailing wind, N.E.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 6; an overcast sky without rain, 14; foggy, 1½; rain and snow, 6½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
4	2	30	0	1	9	19

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
7	12½	2	3	0	½	2	4	31

General Observations.—The weather this month has been very cold, generally cloudy and humid, with frequent heavy falls of snow, which between the 11th and 20th appear to have been unprecedented. Here the moats and marshes were ice-bound till the 23rd, and the houses and ground most of the time covered with snow; but the ice was rugged, from the snow-water having frozen upon it. It snowed more or less on fifteen days, and the whole depth upon a plane surface was sixteen inches; but forty or fifty miles inland the depth exceeded four feet, and in several dales in Gloucestershire and Somersetshire it was found the square of that number in depth, in consequence of the great drifts from off the hills, which rendered the roads impassable for nearly two days, till passages for the coaches, &c. were cut through it. In the night of the 18th snow fell here seven inches in depth; but assuming its specific gravity at one-tenth the weight of water, the quantity received in the rain-gauge on being dissolved, gave eight inches in depth; the snow, however, was very moist and adhesive, and weighed down the branches of short trees and shrubs to the ground. A remarkable circumstance accompanied this heavy fall of snow, namely, a depression of 1·05 inch of mercury in the barometer in twenty-four hours, notwithstanding the wind blew very strong from the N.E. most of the time.
The

The maximum temperature of the external air on the 17th was five degrees below the freezing point, and the minimum temperature in the night of the 18th was fourteen degrees below that point.

There was a recurrence of hard frosts and snow on the last three days, and the 31st was the coldest day since January 14th, 1820: the mean temperature of the twenty-four hours for each of these days was only 20·5 degrees.

It appears from various published reports, that the cold in the South of France and in Spain has long prevailed, and been more severe than has been experienced there for a great number of years; but that in Scotland and places in a higher north latitude, although they have had very deep snow, yet it has been found comparatively mild for a winter like this; therefore the rigour of the frosty air seems to have been confined within the parallels of fifty-five and thirty-eight degrees of north latitude, with prevailing North and North-east winds from over the continent of Europe. Ireland being within these parallels, it is curious that its inhabitants should at the same time also have enjoyed a mild atmosphere.

The mean temperature of the air at this period is 1°·44 lower than that of any month during the last fourteen years; nor has there been so long a continuance of severe frosts since the winter of 1813 and 1814.

The atmospheric phænomena that have come within our observations this month, are, one lunar halo, and four gales of wind, or days on which they have prevailed; namely, one from the North, two from the North-east, and one from the South-east.

REMARKS.

London.—January 1. Hazy: clear and frosty at night (a slight covering of snow on the ground). 2. Hazy and cold. 3. Snow nearly gone: foggy; rain at night. 4—6. Foggy. 7. Fine: clear and frosty. 8. Stormy. 9. Very fine: stormy at night. 10. Clear and stormy. 11, 12. Stormy, with heavy snow-showers. 13. Stormy: the snow four inches deep. 14. Stormy. 15. Foggy. 16. Slight fog in the morning: clear and cold. 17. Hard frost. 18. Strong hoar-frost: cloudy. 19. Cold and cloudy: strong gale at night, with snow. 20. Heavy rain. 21. Clear and cold: boisterous gale at night. 22, 23. Stormy, with showers of sleet. 24, 25. Cloudy. 26. Drizzly morning: foggy. 27. Rainy. 28. Cloudy: sleet. 29. Cloudy. 30. Stormy: rain. 31. Cold and cloudy. Snow almost gone.

Penzance.—January 1—5. Fair. 6. Clear. 7. Fair: showers. 8. Fair. 9, 10. Fair: showers. 11. Rain: showers. 12. Fair. 13, 14. Snow. 15—18. Fair. 19. Fair: rain. 20. Snow. 21—23. Fair: rain. 24. Fair: rain at night. 25. Fair. 26. Clear: rain at night. 27, Clear: hail-showers. 28, 29. Clear. 30. Rain. 31. Fair.

Boston.—January 1—4. Cloudy. 5—9. Fine. 10—12. Stormy. 13, 14. Cloudy. 15. Snow. 16. Rain. 17. Cloudy. 18, 19. Fine. 20. Snow, and stormy: rain A.M. and P.M. 21—26. Cloudy. 27. Snow. 28, 29. Cloudy. 30. Rain. 31. Cloudy.

Average heat of January	1824	36·3
Do.	1825	37·4
Do.	1826	30·8
Do.	1827	32·9
Do.	1828	39·3
Do.	1829	32·5
Do.	1830	31·6

Meteorological Observations made by Mr. BOOTH at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Jan. 1	30·787	30·716	30·50	30·50	30·59	30·58	30·31	35	28	34	30	36	31	33	NE.	NE.	NE.	calm	...	...	...	...	...
☽ 2	30·589	30·555	30·50	30·45	30·51	30·48	30·19	36	30	38	27	36	34	29	NE.	NE.	N.	calm	...	...	...	...	...
3	30·531	30·443	30·40	30·38	30·46	30·39	30·19	37	32	37	30	39	33	34	E.	NE.	NE.	calm	0·08	0·25	...	0·190	...
4	30·385	30·299	30·30	30·28	30·33	30·26	30·00	37	33	40	32	39	36	35	E.	NE.	NE.	calm	...	...	...	·030	...
5	30·372	30·297	30·25	30·24	30·32	30·28	29·93	43	25	45	35	42	26	34·5	N.	N.	W.	calm	...	·04	...	·005	...
6	30·412	30·124	30·20	30·18	30·37	30·20	29·97	38	32	47	32	39	36	32	SW.	NW.	NW.	calm	·07	...	...	...	...
7	30·019	29·868	30·02	30·00	29·97	29·88	29·39	45	32	46	35	44	30	38	NW.	NW.	NW.	calm	...	...	0·195	...	...
8	30·208	30·108	30·15	30·10	30·17	30·10	29·76	38	28	41	34	40	28	32·5	N.	NE.	N	NW.	...	...	...	...	...
○ 9	30·246	30·039	30·20	30·15	30·20	30·03	29·85	37	32	47	32	40	33	28	N.	NW.	W.	calm	·10	...	...	·010	...
10	29·826	29·801	30·05	30·03	29·89	29·84	29·47	41	30	47	36	39	33	32	NE.	N.	N.	NW.	...	...	·100	...	...
11	29·778	29·697	30·00	29·98	29·81	29·68	29·40	37	29	47	37	40	31	34	NE.	N.	NW.	NW.	...	...	·090	·020	...
12	30·004	29·859	30·00	29·90	29·94	29·82	29·57	35	28	42	37	36	30	33	N.	NE.	NE.	NW.	·06	...	...	·030	...
13	29·958	29·845	30·02	29·80	29·90	29·76	29·67	34	23	36	30	33	22	32·5	N.	NE.	N.	NW.	...	...	...	·010	...
14	29·970	29·944	29·90	29·76	29·91	29·86	29·65	32	29	32	28	30	27	32·5	NE.	NE.	NE.	E.	...	...	...	...	...
15	29·946	29·938	29·90	29·90	29·91	29·88	29·70	35	27	34	23	35	26	34	E.	NE.	NE.	calm	·08	...	...	·070	...
16	29·997	29·962	29·90	29·88	29·90	29·88	29·66	31	15	36	30	34	22	34	E.	NE.	N.	calm	...	...	...	·040	...
☾ 17	30·017	29·965	29·85	29·85	29·89	29·85	29·62	26	20	32	31	27	23	31	NE.	NE.	NE.	calm	...	...	...	...	0·50
18	30·054	29·922	29·85	29·83	29·93	29·89	29·75	25	10	40	25	28	18	16	SW.	NE.	NE.	calm	·10	...	...	...	...
19	29·771	29·520	29·50	29·10	29·65	29·36	29·45	32	25	40	33	30	26	16·5	N.	SE.	SE.	calm	...	...	·440	·845	...
20	29·122	28·720	29·30	29·00	29·04	28.60	28·76	35	28	43	30	38	30	30·5	NE.	N.	E.	E.	...	·65	...	·030	...
21	29·537	29·407	29·40	29·40	29·42	29·40	29·00	38	32	48	31	38	36	34	NE.	NW.	SE.	calm	·06	...	...	·700	·95
22	29·813	29·640	29·50	29·30	29·66	29·52	29·37	37	30	46	35	38	32	35	NE.	S.	SE.	E.	...	·16	·750	·490	...
23	30·088	29·930	29·80	29·70	29·83	29·82	29·65	36	31	40	40	35	33	32	SE.	E.	NE.	calm	...	·03	·080	·010	...
● 24	30·352	30·097	30·00	29·85	30·07	29·87	29·63	42	30	44	37	38	31	34	E.	NE.	N.	calm	·05	...	...	...	...
25	30·427	30·352	30·30	30·20	30·33	30·25	29·91	41	31	44	38	39	34	31	NE.	NE.	N.	calm	...	...	·140	...	...
26	30·372	30·188	30·24	30·18	30·28	30·11	29·95	39	32	42	32	39	35	32	N.	NE.	NE.	calm	...	...	...	·040	...
27	29·939	29·840	29·95	29·90	29·86	29·79	29·57	39	32	43	34	40	34	33·5	NE.	N.	SW.	calm	·08	·21	·210	·120	...
28	30·229	30·112	30·10	30·00	30·16	30·06	29·76	37	34	41	35	39	34	35	N.	NE.	NE.	calm	...	·09	·300	·050	...
29	30·214	30·188	30·12	30·10	30·18	30·17	29·90	37	28	42	30	38	30	33·5	W.	NE.	NE.	calm	...	...	...	·100	...
30	30·174	29·995	30·05	30·00	29·97	29·89	29·60	33	20	45	32	41	20	35	N.	N.	NW.	NW.	...	·11	·175	·020	·20
☽ 31	30·171	30·026	29·95	29·95	30·06	30·06	28·88	27	18	40	34	24	17	23	NE.	SE.	E.	S.	·10	...	...	...	...
Aver.	30·787	28·720	30·50	29·00	30·59	28·60	29·66	45	10	23	48	44	17	31·6					0·78	1·54	2·480	2·810	1.65

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

APRIL 1830.

XXXIV. *Letter relating to the Figure of the Earth.* By JAMES IVORY, *Esq. M.A. F.R.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I HAVE to request you will insert, in your next publication, the few lines which follow, relating to the figure of the earth, on which I have formerly written in your Journal. It is not my intention to add any thing new on this subject, but merely to state briefly what I have contributed to the theory, and to assert my claim to my own proper notions.

1st, I have shown the insufficiency of Clairaut's theory, as it is universally taught and applied, for finding the figure of equilibrium of a homogeneous planet supposed fluid.

The reason of the insufficiency is, that the theory inadvertently neglects the attraction between certain portions of the fluid, and by this means omits to take into account pressures prevailing in the interior of the mass and vanishing at the surface, which cannot but have an influence on the figure of equilibrium*. This remark alone overturns all the arguments that have been urged against me in the foreign journals. But, if the objections brought forward in these works fall to

* Clairaut demonstrates the equilibrium from the single consideration that the whole force at every point of the outer surface is perpendicular to the surface. Besides the perpendicularity of the whole force to the surface, Maclaurin's demonstration essentially requires certain properties peculiar to the ellipsoid. How are the two demonstrations to be reconciled? The answer is, that Clairaut omits the internal pressures I have alluded to; and when the inadvertence is corrected, the two modes of reasoning will agree, or rather will be identical. Yet the property of the ellipsoid, which is essential to Maclaurin's reasoning, has been said to be accidental, and not necessary.

the ground, the use that has been made of them in this country, and persisted in for so great a length of time, by men who certainly advanced nothing of their own to apologize for their conduct, can never be repaired, whether it be considered in point of real injury, or as harassing the feelings of one with whom they had no pretence to interfere in the inexcusable manner they have done.

2ndly, The true conditions for the equilibrium of a homogeneous planet in a fluid state, deduced *à priori* from the principles of hydrostatics, without neglecting any cause tending to change the figure of the fluid, are given, for the first time, in the Phil. Trans. for 1824.

I do not pretend that no faults can be found with the investigation I have there given, although the faults are not of great moment. I know well that, in following my original ideas, I have not succeeded in reducing the reasoning to the utmost degree of simplicity, as will appear from the work I have in hand, which will speedily be published. It is very probable too, that, in writing hastily and under great irritation, I may have fallen into some inadvertencies in answering objections not easily disentangled without going back to the first principles of the equilibrium of fluids, the theory of which is incorrectly laid down, as far as this problem is concerned. But I apprehend there is nothing in all this that will appear very extraordinary to those who judge candidly, and are acquainted with the progress of scientific discovery. Are all former writings on this problem perfectly correct? And what are all the objections, without exception, that have been urged against me? The solution of the problem I have published is perfectly exact: and, although no part of the celestial mechanics has been more copiously treated, the only way in which the difficulties that occur can be overcome, has escaped the penetration of every geometer from the time of Clairaut, in 1743, to the latest publication on the subject in 1829.

One objection has been brought forward, which, as it may operate against my intended publication, ought now to be rectified and removed. I allude to a note published in the Phil. Trans. for 1826. I am persuaded that the author of the note is now convinced that it is founded on misapprehension. And as the Royal Society has given publicity to the note, it is not too much to expect from its justice that it will be induced to reconsider the matter. The subject I press upon its attention has always remained the least satisfactory part of the system of Newton, and the part in which the least improvement has been made; and, if we may judge from the interest which other scientific societies have taken in the research, we

may

may expect that it merits some regard from that learned body, by which no important point of the philosophy of Newton can ever be deemed indifferent or unfashionable. If new information be wanted, I engage to produce demonstrations that will bring the question to a perfect decision, as soon as I know that a proper use will be made of them. This will only put me to the trouble of transcribing part of what I have written.

3rdly, Maclaurin demonstrated that a homogeneous planet supposed fluid, is in equilibrium when it has the figure of an oblate elliptical spheroid of revolution; and he investigated the equation of the surface of the spheroid. D'Alembert, in examining this equation, discovered that it admits of more than one solution; and it is now well known that the number of solutions is two, and no more. It is therefore a mathematical deduction from the equation of Maclaurin, that the same mass of fluid, revolving with the same angular velocity, will be in equilibrium in two different figures. But there must be a physical reason that determines the number of figures of equilibrium: and this reason must be a part of a solution of the problem *à priori*. Accordingly, in examining the forces in action in the interior of the mass, which forces are entirely omitted in the usual manner of solving the problem, I found that two different sets of surfaces may be traced within the fluid, each of which is possessed of the property of the level surfaces in Clairaut's theory, that is, the intensity of pressure is the same at all their points. The two sets of interior surfaces have different relations to the outer surface, and one set only can properly be called level surfaces. The definition of the level surfaces given by Clairaut is exact only in one particular case of the equilibrium of a homogeneous fluid entirely at liberty; and no other definition has ever been thought of by any geometer.

4thly, When the difficulties respecting the equilibrium of a homogeneous fluid are overcome, the same principles are easily applied to a heterogeneous fluid. In this latter case the equation of the surface cannot be found in a finite expression, but it may be determined to any required degree of approximation. Clairaut solved the problem long ago, retaining only the first power of the ellipticity. I have already published in this Journal*, a solution which takes in the second power of the oblateness, by a method which leaves no doubt respecting the equilibrium of the fluid, and which requires no more than the labour of calculation to extend it to any power of the oblateness.

* Phil. Mag. for July 1826, pp. 5 & 6. The formula only is given, as the analysis would have taken up too much space. But I apprehend that a formula of this nature is sufficient to secure to its author the possession of the method of investigation, when it shall be in his power to publish it.

 The

The several points I have enumerated place the theory of the figure of the planets upon grounds entirely new. I conceive that the views I have developed are important, both because they overcome a difficulty insuperable in any other way, and because they will facilitate the progress of knowledge by superseding much of the abstrusest and least satisfactory writings on the celestial mechanics. My only object in what I have written is to assert and prove my claim to what I have discovered, by my own labour, in spite of much unprecedented opposition. I put myself in comparison with no one; I will enter into no altercation on the subject; I merely claim what I have the justest title to claim; nor do I think it necessary at present to explain particularly the reasons that have induced me to write this letter.

It may be proper to add that the subject of this letter has no connection with another contestation, entirely mathematical, relating to the series for computing the attraction of spheroids. I remain, Gentlemen, yours, &c.

March 8, 1830. JAMES IVORY.

XXXV. *On the Blue Colouring-matter of Lapis Lazuli, and on artificial Ultramarine. By* DR. FR. W. SCHWEIGGER-SEIDEL.*

THE mineral colour known by the name of Ultramarine, esteemed for its beauty and durability, especially in oil-painting, has long been an object of chemical inquiry. The lapis lazuli, from which the colour is obtained by careful washings, is procured from Asia (partly through the East Indies, partly by way of Orenburg), where it is found in Little Bucharia, Thibet, several provinces of China, and Siberia†. It seems to have been known to the Romans under the name of Sapphire, as appears from some passages of Pliny‡. But the production of ultramarine seems not to have been invented till the end of the fifteenth century; the name of *Azurrum ultramarinum* (the origin of which is very evident) is said to have been first used in the year 1502 by Camillus Leonarius §. It once formed a considerable article of trade in Italy, where this colour was probably first produced, and even now the greatest quantity, and that of the best quality, comes from there.

* From the *Jahrbuch der Chemie*, &c. N.R. Band xxii. p. 206.

† This is different from the lazulite or *copper lazure* (Armenian stone), which owing to the similarity of their colour used formerly to be mistaken for it; hauyn seems to be more nearly related to lapis lazuli.

‡ *Hist. Nat.* lib. xxxvii. 38, 39.

§ Leuchs's *Farben-und Färbekunde*, ii. 198.

Whether

Whether it be in consequence of a lessened demand, and consequent diminished manufacture since the discovery of prussian blue, and other cheaper blues, or in consequence of a diminished importation of the lazure-stone, that this colour has become so very scarce, this much is certain, that its high price (an ounce of the best quality being said to sell now at from one hundred to two hundred francs*) has greatly limited its use; whilst formerly, especially in the sixteenth century, it was almost wasted by painters, as is proved by many pictures of that period.

The value of the colour naturally led to a desire of producing it artificially. Some assert, that the art was known in the sixteenth century, but kept secret. But this probably implied only the art of obtaining ultramarine of the best quality from the lazure-stone. What are called artificial lazure-stones, for the production of which there are many formulæ†, are in fact artificial pieces of glasses coloured with some metallic oxide (mostly oxide of cobalt), which will of course yield no ultramarine. Indeed the colour of lapis lazuli was generally ascribed, until lately, from the results of chemical analyses, and according to analogy, from a metallic oxide (oxide of cobalt, copper, iron, &c. supposed to be contained in it). Wallerius derives it from silver‡, which, however, has not been found by any modern chemist, and which was probably only believed to be it through a well-known mistake usual in former times. The common opinion, however, was, that the blue colour of the mineral was produced by oxide of copper, until it was shown by Marggraf, that the lazure-stone contained oxide of iron only, and no oxide of copper §. It was his analyis which gave the first explanation of the component parts of this stone; for the accounts of Rinmann and Cronstedt are not sufficiently defined. Klaproth's subsequent analysis || generally confirms the results of that of Marggraf, except that he points out a portion of alumina which the latter overlooked; for the rest, he also inclined to the opinion that the blue colour was produced by the oxide of iron. It was Guyton de Morveau who first drew public attention to a portion of potash contained in the lazure-stone, and which he thought accidental, but considered that it

* Leuchs's *Farben–und Färbekunde*, p. 205. Thénard *Traité de Chimie*, tom. ii. (618) p. 210.

† Compare some of them in Leuchs, p. 487.

‡ *System. Mineral.* i. 312.

§ See his Chemical works, vol. i. p. 121–134, and Hochheimer's *Chem. Minéralogie*, vol. i. p. 239–244.

|| See *Beiträge*, &c. vol. i. p. 180–196, and Schweigger's *Journal*, vol. xiii. p. 488. xiv. p. 531. and xli. p. 234. He found silica and alumina, carbonate of lime, sulphate of lime, and oxide of iron.

was

was chiefly the sulphur it contained which, combined with the iron, produced the colouring matter of the stone*. This view, however, was refuted by Clement and Desormes, who proved that the ultramarine contained sulphur, but no iron†; which conclusion was confirmed by the experiments of R. Phillips, on the methods of ascertaining the degree of purity of the ultramarine‡. Clement and Desormes at the same time mentioned a considerable proportion of soda in the ultramarine, which also seemed to contain some potash §. These two chemists, however, express no opinion as to the cause of the blue colour. Thénard, indeed, does not deny the possibility of a coloured body being produced by the combination of colourless bodies, but adds that the loss of 0·8 per cent, experienced by MM. Clement and Desormes in their analysis, might lead to the supposition that it was just the colouring substance which had escaped them‖. Phillips expresses the opinion that the lazure-stone perhaps owes its colour to a peculiar substance *not metallic*, and recommends this part of the subject to the attention of chemists ¶.

With this difference of views on the nature of the colouring-matter in the lazure-stone, scarcely any result could be expected from the experiments instituted for producing ultramarine artificially; indeed they were all unavailing. An interesting accident, however, had led to a probable hope of the result ultimately turning out advantageously. M. Tassaërt,

* Compare Scherer's *Journal* (1800), vol. iv. p. 659, and more at large vol. v. p. 709; also *Ann. de Chimie*, xxxiv. p. 54, and Von Crell's *Chem. Ann.* 1801, p. 467: he notices the following substances as appearing accidentally in various quantities in the lazure stone,—carbonate and sulphate of lime, and at times even barytes.

† Gehlen's *Journ. für Chem. u. Phys.* vol. i. p. 214—221, and *Ann. de Chim.* March 1806, tom. lvii. p. 317—364. Compare also *Journ. des Mines*, xvii. (No. 100) p. 322; and this (Schweigger's) *Journal*, vol. xiii. p. 489; vol. xiv. p. 331, and vol. xli. p. 235.

‡ Vol. xli. of this (Schweigger's) *Journal*, p. 233—241. Comp. also Annals of Philosophy, No. 51, July 1823, p. 31. The methods of examination are given here with mountain blue, prussian blue, indigo, smalt, and oxide of cobalt, although we may venture (as Phillips says at p. 239) to declare an ultramarine as genuine, which in a few minutes "(developing sulphurous acid gas, especially on being heated)" loses its colour when an acid is poured on it, leaves an insoluble dirty white residue, and forms a colourless solution.

§ They at least saw crystals of alum, like Guyton de Morveau. They found no sulphurous acid gas, and even carbonate of lime does not always appear; but always sulphur in connection with soda, alumina and silica, which therefore must be considered as the essential components of the ultramarine.

‖ See his *Traité de Chimie*, 1e A. tom. ii. p. 208; and Schweigger's *Journal*, vol. xli. p. 236.

¶ In this (Schweigger's) *Journal*, vol. xli. p. 239.

super-

superintendent of a manufactory of sulphuric acid and soda, found, on breaking up the hearth of one of his smelting furnaces for soda, in the foundation of it, a blue substance which as long as the hearth had been built of brick, and not of sandstone as it was then, he had never noticed*. Vauquelin on examining this substance found it greatly to resemble the lazure-stone, and the analysis also indicated alumina and silica united with soda and sulphite of lime, but at the same time with iron and sulphuretted hydrogen, from which latter components in connection with alkali Vauquelin felt inclined to deduce the blue colour of this substance as well as of the lapis lazuli †. Soon after L. Gmelin examined a volcanic product thrown out by Vesuvius, which Breislak (in his *Voyages dans la Campanie*) mentions as a seventh kind of lazulite, and which was afterwards classed by Bruun Neergard with the hauyn ‡. Nevertheless this mineral seemed to agree in its external characters more with the lapis lazuli than with the hauyn, which induced L. Gmelin to repeat the analysis of lapis lazuli at the same time, and to compare the results of these analyses with those he had recently obtained from the chemical investigation of the hauyn §. The result was, that the blue volcanic product above mentioned had in reality a great similarity with the lazure-stone even in its chemical composition. But the same observation was also applicable to the hauyn, which seemed to differ from the lazure-stone, essentially, only by a proportionately great quantity of sulphuric acid, and by its containing potash instead of the soda found in the lazure-stone. The latter, however, was also the case in the blue volcanic mineral, by which the latter seemed again more closely related to the hauyn than to the lapis lazuli, or at least to form an intermediate link between the two minerals. This induced L. Gmelin to arrange the lazuli, containing soda, with the hauyn, containing potash, as species or subspecies nearly allied, but to consider the blue mineral, under the name of *earthy hauyn*, as a mere variety of the common, called *granular hauyn*. In other respects the volcanic product differs from the two other

* According to a verbal communication of Dr. W. Weissner, the administrator Herrman at Schönebeck had made a similar discovery some years ago, and declared the substance to be an ultramarine produced by a chemical process. Perhaps we ought also to add to this the blue colouring-matter which at times dyes the calcined potash a beautiful lazure blue, and which has been usually attributed to metallic oxides or finely divided carbon.

† Compare this (Schweigger's) *Journal*, vol. xiii. Old Series, p. 486, &c. and vol. xiv. p. 333. *Ann. de Chim.* tom. lxxxix. p. 88. Thénard, tom. ii. p. 748. Fechner, ii. p. 418.

‡ *Journ. des Mines*, No. 125.

§ *Observationes Geognosticæ et Chemicæ de Hauynâ*, &c.

substances

substances by containing a considerable proportion of iron: L. Gmelin, however, also found iron in the lazuli, and he would not have been disinclined to take the colouring principle for protosulphuret of iron, had not Clement and Desormes shown that there is no iron in the ultramarine.

Almost at the time when Vauquelin's and Gmelin's investigations of substances resembling lazulite* (which evidently were indebted for their existence to chemical processes nearly related,) raised the possibility of an artificial production of ultramarine almost to a certainty, without, however, giving any clear explanations respecting it, another German chemist (who has not only enriched the science in so distinguished a manner, but also the arts by a number of ingenious investigations) found in quite a different way an indication of the colouring-matter in the lazuli, and he would have required but little further investigation to become perfect master of the artificial production of ultramarine.

By the communication of some experiments on the fuming sulphuric acid, which were published in the year 1815 in this (Schweigger's) Journal†, Dœbereiner developed his views on the composition of sulphur, as consisting of hydrogen and a probably metallic body (*schwefelstoff*), whence he felt inclined to deduce the blue colour of Vogel's blue sulphuric acid. "And if," concluded this able chemist, "the colour of the pure sulphureous substance is really blue, the colour of the ultramarine seems to be solely produced by this substance; and that from potash or soda, sulphur, silica and alumina, under certain conditions, a blue similar to the ultramarine, only less brilliant and beautifully clear, may be produced, I have shown a year ago to Professors Gehlen and Schweigger. I have been withdrawn from this investigation by other occupations, but shall soon again devote myself to it, and communicate the results." He, then, was the chemist who for the first time pronounced the colouring principle of ultramarine to be sulphur.

Unfortunately Dœbereiner has not again pursued his beautiful discovery: it is therefore the more satisfactory that the fact is now confirmed in many journals, with the intelligence which, no doubt, will please the practical chemists, that another of our most distinguished German chemists, Professor C. G. Gmelin of Tubingen, has succeeded in the discovery of a proper chemical process for the production of ultramarine.

* See this (Schweigger's) *Journal*, vol. xiv. Old Series, p. 325—335, where at p. 331 a tabular view is given of the analyses here alluded to. Let it also be observed that Gmelin found traces of potash besides the soda in the lazuli, and 2 per cent of magnesia.

† Vol. xiii. Old Series, p. 476—484.

We

We cannot conclude this review more suitably than by a verbal transcript of the following account from the *Berliner Hand und Spener'sche Zeitung*, (10th April 1828,) No. 84, and which in substance seems to be from the distinguished inventor himself.

" *Tübingen.*—Prof. C. G. Gmelin, who for some time past has been employed in the investigation of ultramarine, has arrived at the conviction that sulphur is its colouring principle, and particularly that there is no metal, properly so called, entering into its composition. Gmelin had received some ultramarine from Paris eighteen months ago, but which, according to the opinion of M. Seybold, the artist at Stuttgard, was not of the best quality. In order, therefore, to obtain ultramarine of all kinds, and to determine by strict analysis what proportions of its component parts are most favourable to the production of its fiery colour, he addressed himself months ago to Prof. Carpi at Rome. During a short residence he made in Paris, in the spring of 1827, he expressed it as his opinion to the chemists of that metropolis, especially to M. Gay-Lussac, that ultramarine, with the investigation of which he told them he was then engaged, might be produced artificially. It is perhaps, therefore, his own fault if another (M. Tunel of Paris, who wishes to keep his discovery a secret) has anticipated him in this respect. The process by which, according to M. G.'s inquiries, the production of ultramarine is always successful, is the following:—Procure silica containing water and alumina; calculate how much a given weight of these earths will leave after being calcined. (By Gmelin's investigations 100 parts of hydrous silica contained only 56, and 100 parts of hydrous alumina only 32·4 parts of pure earth.) Next, dissolve as much of the hydrous silica as can be dissolved in caustic soda, and calculate the quantity of earth used. Add now to 72 parts of this silica, (calculated as free from water,) 70 parts of alumina (also calculated in a state free from water); add the latter to the silicate of soda, and let it evaporate, stirring it all the time till the residue presents a damp powder. (One may also take at once 60 parts of dry caustic soda to 72 parts of alumina obtained from alum, the latter being reduced to the dry state.) This colourless mixture of silica, soda, and alumina, is the foundation of the ultramarine, which is to receive its blue colour. For this purpose, melt in an earthen crucible, well closed, a mixture of two parts of sulphur and one part of anhydrous carbonate of soda, and when the mass is properly melted, throw very small portions of the first mixture at once into the middle of the crucible: as soon as the effervescence produced by the rising of the aqueous vapours has ceased, throw in another portion, and so on; and keep the crucible, when the whole mixture has been introduced, for about one hour in a moderate red glowing heat (if the heat is too great, it destroys the colour); when cold, pour water into the crucible, and separate by means of it the brown residue of sulphur mixed with the ultramarine. A superabundance of sulphur may be expelled by a moderate heating. If the colouring is not of an equal intensity, the most fiery ultramarine (and this is a very important circumstance) may be obtained by washing, and separating it from those parts which are less coloured. From the component parts of the ultramarine as given by the analysis, it cannot be formed, without a medium. Thus this colour is nothing else than a silicate of soda dyed with sulphuret of sodium.

"The natural ultramarine contains a not inconsiderable portion of potash and sulphuric acid; and it is very probable that the artificial production here mentioned may be usefully varied, but this can only be discovered by experiment."

XXXVI. *Notes on the Geographical Distribution of Organic Remains contained in the Oolitic Series of the Great London and Paris Basin, and in the same Series of the South of France. By* HENRY T. DE LA BECHE, *F.R.S. &c.*

[Continued from page 205.]

Lowest System.—Subdivisions: CORNBRASH, FOREST MARBLE, and GREAT OOLITE.

IT has been considered that the former of these is a proper subdivision, and may be traced to considerable distances, but that the two latter may pass into or represent each other. Great difficulty must always attend these minute divisions. Mr. Phillips gives only five feet as the thickness of the cornbrash in Yorkshire; it is represented as from eight to sixteen feet in the neighbourhood of Tellisford and Farley Castle, near Bath; M. de Caumont doubts its existence in Calvados, though there is a rock which M. Desnoyers and myself consider may be referred to it; and M. Boblaye mentions it in the North of France, but it does not so clearly appear to exist in the South of the same country. The forest marble and great oolite seem to occur extensively. In Calvados the latter, according to M. de Caumont, is intimately connected with the inferior oolite.

Mr. Phillips observes (Illustrations, &c. p. 158), "In the midland counties, the fuller's earth rock of Mr. Smith does by no no means furnish a constant or well-marked line of distinction between the middle, great, or Bath oolite, and the inferior oolite; and I am decidedly of opinion that in the northern part of Northamptonshire, and throughout Rutland and Lincolnshire, there is but one thick oolite rock beneath the cornbrash, resting upon brown sandstone which immediately covers the upper lias shale."

GREAT OOLITE *and* FOREST MARBLE.

In Yorkshire, above and beneath the rocks considered by Mr. Phillips equivalent to the Bath oolite, there are two large deposits of coal, sandstone and shale, containing many fossil plants. In the inferior the following genera are found: Equisetum, 2 species.—Lycopodites, 1.—Thuytes, 1.—Scolopendrium, 1.—Sphænopteris, 4.—Neuropteris, 1.—Pecopteris, 2. —Cycadites, 4.—Flabellaria? 1. In the superior: Equisetum, 1.—Lycopodites, 1.—Thuytes, 1.—Scolopendrium, 1.—Aspleniopteris, 1.—Sphænopteris, 4.—Neuropteris, 1.—Pecopteris, 8.—Cycadites, 4.—Flabellaria? 1.—Phyllites, 1.—and Dicotyledonous Wood.

*Organic Remains of the Great Oolite,—Yorkshire.**

Plesiosaurus?
Plants.
Millepora straminea (Phil.).
Retepora?
Tubipora or Eunomia.
Cidaris vagans.
Echinus germinans.
Mya calceiformis (Phil.).
Panopæa gibbosa?
Psammobia lævigata (Phil.).
Amphidesma decurtatum.
Pholadomya acuticostata.
——— nana (Phil.).
——— producta?
——— obliquata (Phil.).
Corbula depressa (Phil.).
Isocardia nitida?
——— *concentrica.*
——— angulata?
Cardium cognatum (Phil.).
——— acutangulum (Phil.).
——— semiglabrum (Phil.).
Cardita similis.
Cytherea dolabra (Phil.).
Pullastra recondita (Phil.).
Crassina minima (Phil.).
Lucina despecta (Phil.).
Trigonia *costata.*
Trigonia conjungens.
Modiola imbricata.
——— ungulata (Y. & B.).
Pinna cuneata (Phil.).
Cucullæa imperialis (Bean.).
——— cylindrica (Phil.).
——— cancellata (Phil.).
——— elongata.
Nucula *variabilis.*
——— lacryma.
Perna *quadrata.*
Gervillia acuta.
Avicula Braamburiensis.
Plagiostoma *cardiiforme.*
——— interstinctum.
Pecten lens.
——— demissus (Phil.).
——— abjectus (Phil.).
Lima rudis.
Ostrea *Marshii.*
——— gregarea?
——— sulcifera (Phil.).
Gryphæa bullata? or gigantea.
Terebratula spinosa (Smith).
——— *globata.*
——— intermedia.
Natica adducta (Phil.).
Turbo muricatus?

* Illustrations of the Geology of Yorkshire, p. 149—152.

Trochus monilitectus (Phil.).
Delphinula?
Phasianella cincta (Phil.).
Turritella cingenda.
Melania Heddingtonensis.
——— *striata?*
Terebra vetusta (Phil.).
Actæon glaber (Bean.).
Rostellaria composita?
Nautilus.
Belemnites abbreviatus (Miller).
Ammonites Blagdeni.
Vermicularia nodus (Phil.).
Serpula lacerata (Phil.).

*Organic Remains of the Forest Marble.—Calvados.**

Terebellaria ramosissima (Lam^x.)
——— antelope (Lam^x.)
Berenicea diluviana (Lam^x.)
Alecto dichotoma (Lam^x.)
Idmonea triquetra (Lam^x.)
Theonoa chlatratra (Lam^x.)
Chrysaora damæcornis (Lam^x.)
——— spinosa (Lam^x.)
Eunomia radiata (Lam^x.)
Spiropora tetragona (Lam^x.)
——— cespitosa (Lam^x.)
——— elegans (Lam^x.)
——— intricata (Lam^x.)
Fungia orbulites (Lam^x.)
Millepora dumetosa (Lam^x.)
——— corymbosa (Lam^x.)
——— conifera (Lam^x.)
——— pyriformis (Lam^x.)
——— macrocaule (Lam^x.)
Caryophyllia truncata (Lam^x.)
——— Brebissonii (Lam^x.)
Limnorea mamillaris (Lam^x.)
Entalophora cellarioides (Lam^x.)
Turbinolopsis ochracea (Lam^x.)
Eschara.
Alcyonium.
Clypeus sinuatus.
——— *clunicularis.*
Encrinites pyriformis.
Pentacrinites.
Apiocrinites.
Asteria.
Crustacea.
Modiola elegans.
Trigonia *costata.*
Trigonia gibbosa.
——— duplicata.
Avicula *echinata.*
——— costata.
Gervillia pernoïdes (Deslong.).
——— siliqua.
——— monotis (Desl.).
——— costellata (Desl.).
Lima proboscidea.
Plagiostoma punctatum.
Pecten corneus.
——— vimineus.
——— vagans.
Ostrea *Marshii.*
——— palmetta.
Pinna pinnigena.
Mactra gibbosa.
Terebratula tetraëdra.
——— biplicata.
——— *digona.*
——— *coarctata.*
——— reticulata.
——— *globata.*
——— plicatella.
——— serrata.
——— truncata.
Patella rugosa.
Trochus elongatus.
Belemnites.
Nautilus truncatus.
Ammonites *annulatus.*
Nerinea.
Isocardia.
Lucina.
Plants.

Organic Remains of the Great Oolite,—Calvados.†

Crocodile.
Megalosaurus.
Teeth and palates of Fish.
Ichthyodorulites (Buckl. & De la B.).
Ammonites.
Belemnites.
Pinna.
Mytilus amplus.
Avicula inæquivalvis.
Lima gibbosa.
Ostrea Crista Galli.
Pecten corneus.
Terebratula biplicata.
——— obsoleta.
Plants.

* De Caumont, *Essai* &c. pp. 147, 148.

† *Ibid.* 153, 154.

Organic

Organic Remains of the Fuller's Earth (Terre à Foulon),—*North of France.**

Ammonites.
Nautilus.
Belemnites compressus.
——— dilatatus.
Terebratula *approaching* vulgaris.
Terebratula media.
Lutraria? } Casts.
Donacites? } Casts.
Donacites Alduini?

Organic Remains of the Great Oolite,—North of France.

M. Boblaye states† that the fossils which appeared to him the most proper to characterize, by their abundance, the lowest beds, are Ostrea acuminata, Terebratula media, and a Madrepore composed of small cylindrical tubes united in bundles.

Organic Remains of the White Marls (Bradford Clay) in the North of France.‡

Ammonites vulgaris.
Nerinea.
Turritella.
Ampullaria or Turbo?
Serpula.
Pecten.
Spondylus imbricatus (or Podopsis).
Pinna (species analogous to that of the Ile d'Aix).
Avicula *echinata.*
Ostrea *costata.*
——— acuminata.
Gryphæa lituola (Lam.).
Astarte planata.
Isocardium.
Hemicardium?
Terebratula *digona.*
——— *coarctata.*
——— media, and others.
Cydarites ornatus.
Pentacrinites (numerous).
Madrepora (various and abundant).
Crustacea.

The most characteristic fossils are considered to be Gryphæa lituola, Terebratula digona, and T. coarctata.

Cornbrash.

Organic Remains of the Cornbrash in Yorkshire.§

Cellaria Smithii.
Millepora straminea (Phil.).
Cidaris vagans.
Clypeus *clunicularis.*
——— orbicularis.
Galerites depressus.
Pentacrinus Caput Medusæ.
Mya literata.
Sanguinolaria undulata.
Pholadomya Murchisoni.
——— ovalis.
Amphidesma decurtatum (Phil.).
——— securiforme (Phil.).
Unio peregrinus.
Isocardia minima.
Cardium citrinoïdeum (Phil.).
Trigonia clavellata.
Modiola *cuneata.*
Pinna cuneata (Bean.).
Plagiostoma rigidulum (Phil.).
——— interstinctum.
Pecten *fibrosus.*
——— demissus (Phil.).
Ostrea *Marshii.*
Terebratula ovoides.
Trochus granulatus.
Terebra? granulata (Phil.).
Melania Heddingtonensis.
——— vittata (Phil.).
Bulla? or Actæon.
Ammonites Herveyi.
——— terebratus.
Vermicularia nodus (Phil.).
Serpula intestinalis (Phil.).
Belemnites are not found in the cornbrash of Yorkshire.

* Boblaye, *Ann. des Sci. Nat.* vol. xvii. p. 57. † *Ibid.* pp. 58, 59.
‡ *Ibid.* pp. 60, 61. § Phillips's Illustrations, &c. p. 143—145.

Organic

*Organic Remains of the Cornbrash and Forest Marble,—North of France.**

Avicula *echinata.*
Plagiostoma *cardiiforme.*
Pecten *fibrosus.*
——— lens, and two others.
Gryphæa lituola.
Ostrea, large and flat, hinge very broad.
Terebratula *subrotunda.*
Spatangus.
Nucleolites columbaria.
Millepora.
Fish teeth.

It will at once be observed how very little the lists of organic remains enumerated at the different localities agree with each other. Unfortunately we have no good detailed and published lists of fossils in the rocks under consideration, either in our midland or southern counties; but as Mr. Conybeare's lists† are principally compiled from these parts of England, I shall employ them, though by their general nature they do not exactly enter into the object of these notes, adding those fossils that have appeared in Sowerby's Min. Conch. since the publication of the "Outlines."

The additions to Mr. Conybeare's lists are, for the cornbrash, Mytilus sublævis, Isocardia *concentrica*, Pecten annulatus, and Perna *quadrata*;—for the Stonesfield slate, Patella lata;—and for the Bradford clay, Terebratula *coarctata*, and Serpula triangulata. The additions to the list of great oolite fossils are considerable; the following are all from Ancliff in the environs of Bath.

Fossils of the Great Oolite,—Ancliff.

Astarte orbicularis.
——— pumila.
Cucullæa minuta.
——— rudis.
Pectunculus minimus.
——— oblongus.
Arca pulchra.
Nucula *variabilis.*
——— mucronata.
Ostrea obscura.
——— *costata.*
Gryphæa minuta.
Trigonia imbricata.
——— cuspidata.
——— Pullus.
Terebratula flabellula.
Terebratula furcata.
——— hemisphærica.
Orbicula granulata.
Patella ancyloides.
——— nana.
Emarginula scalaris.
Actæon cuspidatus.
——— acutus.
Nerita minuta.
——— costata.
Turbo obtusus.
Rissoa lævis.
——— acuta.
——— obliquata.
——— duplicata.
Buccinum unilineatum.

* Boblaye, *Ann. des Sci. Nat.* vol. xvii. pp. 62, 63.
† Outlines of the Geology of England and Wales, p. 206—212.

Comparative View of the Organic Remains of the Great Oolite (including Fuller's Earth and Bradford Clay), the Forest Marble, and the Cornbrash.

F. E. Fuller's Earth.—G. O. Great Oolite.—B. C. Bradford Clay.—F. M. Forest Marble.—C. Cornbrash.

Name.	England. Conyb. and Sow.	Yorkshire. Phillips.	Calvados. De Caumont.	North of France. Boblaye.
Crocodile	* C.	....	* G.O.	
Megalosaurus	* F.M.	....	* G.O.	
Fish teeth and palates	* F.M.	....	* G.O.	* F.M. & C.
Crustacea	* F.M.	....	* F.M.	
Plants	* F.M.	* G.O.	* G.O.	
Belemnites	* F.M. & B.C.	* G.O.	* F.M. & G.O.	* F.E.
Patella rugosa	* F.M.	....	* F.M.	
Modiola imbricata	* C.	* G.O.		
Trigonia clavellata	* C.	* C.		
——— costata	* C., F.M. & B.C.	* G.O.	* F.M.	
Ostrea Marshii	* C.	* G.O. & C.	* F.M.	
——— Crista Galli	* F.M., B.C. & G.O.	....	* G.O.	
——— costata	* G.O.	....	....	* B.C.
——— acuminata	* B.C.	....	....	* G.O. & B.C.
Pecten fibrosus	* C., F.M. & G.O.	* C.	....	* F.M. & C.
Plagiostoma cardiiforme	....	* G.O.	....	* C. & F.M.
Avicula echinata	* C.	....	* F.M.	* B.C., C. & F.M.
——— costata	* C. & B.C.	....	* F.M.	
Lima gibbosa	* C.	....	* G.O.	
Terebratula subrotunda	* C.	....	....	* C. & F.M.
——— intermedia	* C.	* G.O.		
——— digona	* C. & B.C.	....	* F.M.	* B.C.
——— obsoleta	* C., B.C. & G.O.	....	* G.O.	
——— reticulata	* B.C.	....	* F.M.	
——— globata	....	* G.O.	* F.M.	
——— coarctata	* B.C.	....	* F.M.	* B.C.
——— media	* F.E.	....	....	* F.E.
Isocardia concentrica	* C.	* G.O.		
Perna quadrata	* C.	* G.O.		
Nucula variabilis	* G.O.	* G.O.		
Clypeus sinuatus	* (all beds)	....	* F.M.	
——— clunicularis	* (all beds)	* C.	* F.M.	

There are abundant remains of corals or polypifers in the great oolite or in the forest marble in the South of England, Normandy, and North of France.

From the above comparative list it will be perceived that there is a more general agreement between Mr. Conybeare's lists (obtained from the midland and southern counties of England) and those of Normandy, than there is between the same lists and those formed in Yorkshire;—possibly the circumstances that produced the coal and abundance of plants which

which accompany it in the latter locality, may have had great influence on the animal life of the vicinity.

M. Dufrénoy mentions that in the oolite beds of Mauriac, the upper contain many polypifers, and appeared to him analogous with the calcaire à polypiers of Caen (cornbrash and forest marble). He enumerates the following fossils in these beds:

Pecten obscurus.	Modiola *cuneata?*
——— *fibrosus.*	Unio crassissimus.
Terebratula *subrotunda.*	Melania *striata?*
——— perovalis.	Ammonites *annulatus.*
——— tetraëdra.	Patella.
——— concinna.	Echinites (*very small*).
Plagiostoma punctatum?	Encrinites.
——— ovale.	Madrepores.

At Maisons Blanches, Ruffec, and Négres near Couhé, M. Dufrénoy remarks, "In the first of these localities the rock contains an abundance of smooth Terebratulæ; and strikingly resembles the limestone observed between Oxford and Blenheim, referred to the cornbrash." The fossils are:

Pecten obscurus.	Terebratula bullata.
——— laminatus.	——— ornithocephala.
——— barbatus.	Ammonites elegans.
Plagiostoma punctatum.	——— *annulatus.*
Terebratula perovalis.	Rostellaria.

Of the 26 species mentioned in the Yorkshire cornbrash, 3 occur in France, one (*Ostrea Marshii*) being the same with one of the 3 of the great oolite: of these, 1 is found in the forest marble of Calvados; 1 in the forest marble and cornbrash of the North of France; and 2 in the Mauriac beds, South of France. Of the two Echinites of the Yorkshire cornbrash, 1 is found in the forest marble of Calvados.

Of the 32 species of fossil shells enumerated in the forest marble of Calvados, 7 are found in the other localities: of these, 3 occur in the great oolite, and 1 in the cornbrash of Yorkshire; 3 in the white marls or Bradford clay of the North of France; and 1 in the Mauriac beds, South of France. Of these 32 species, 7 are noticed in the lists of Mr. Conybeare and Mr. Sowerby; 2 in the forest marble, 5 in the cornbrash, and 5 in the Bradford clay.

Of the 6 species noticed in the forest marble and cornbrash of the North of France, 3 are found in the lists of Mr. Conybeare and Mr. Sowerby: of these, 3 occur in the cornbrash, 1 in the forest marble, and 1 in the great oolite: Of the same 3 species, 1 occurs in the great oolite, and 1 in the cornbrash of Yorkshire; 1 in the forest marble of Calvados; and 2 in the Mauriac beds, South of France.

Of the 61 species of fossil shells enumerated in the Yorkshire great oolite, 7 occur in the lists of Mr. Conybeare and Mr. Sowerby: and of these, 6 are mentioned in the cornbrash, 1 in the forest marble, 1 in the Bradford clay, and 1 in the great oolite. Of the 61 species, 3 are stated to occur in the forest marble of Calvados, and 1 is found in the cornbrash and forest marble of the North of France. Melania striata is marked questionable in the great oolite of Yorkshire and in the Mauriac beds.

Of the 7 species noticed in the great oolite of Calvados, 3 occur in Mr. Conybeare's lists, 2 in the cornbrash, 1 in the forest marble, 2 in the Bradford clay, and 2 in the great oolite. None are mentioned either in Yorkshire or the North of France.

Of the 9 species contained in the white marls of the North of France, supposed equivalent to the Bradford clay, 4 occur in the lists of Mr. Conybeare and Mr. Sowerby, 2 in the cornbrash, and 3 in the Bradford clay. Of the 9 species, 3 are found in the forest marble of Calvados. None are mentioned in Yorkshire.

Of the 2 species noticed as characterizing the great oolite of the North of France, 1, Ostrea acuminata, is mentioned as found in the Bradford clay of Mr. Conybeare's lists, but is not noticed either in Calvados or Yorkshire.

Of the 3 species of shells, exclusive of Belemnites, in the fuller's earth of the North of France, 1, Terebratula media, is found in the same rock in England (Conybeare).

Of the 12 species enumerated in the Mauriac beds, 5 are found in the other localities, 2 being questionable: of these, 2 occur in the Yorkshire cornbrash, 1 being questionable at Mauriac; 1 is marked doubtful both in the Yorkshire great oolite and the Mauriac beds; 2 occur in the cornbrash and forest marble of the North of France, and 1 in the forest marble of Calvados. Of these 12 species, 2 are found in Mr. Conybeare's lists; 1 in the cornbrash; and 1 in the cornbrash, forest marble, and Bradford clay.

By considering the cornbrash, forest marble, Bradford clay, great oolite, and fuller's earth, as a mass in which sometimes divisions can be made, while at others none can be observed, the most common fossils would appear to be fish teeth and palates, Clypeus clunicularis, Belemnites, Pecten fibrosus, Avicula echinata, Terebratula subrotunda, T. digona, T. coarctata, numerous polypifers, and the Bradford encrinite.

The following is a list of the fossils from the celebrated Stonesfield slate, which belongs to the division under consideration,

ration, formed from the writings of Professor Buckland*, Dr. Fitton†, and M. Adolphe Brongniart‡.

Didelphis.
Pterodactylus.
Megalosaurus Bucklandi.
Tortoise scales.
Crocodile scales, teeth, and bones.
Fish teeth and palates.
Ichthyodorulites.
Insects.

VEGETABLE REMAINS.

Fucoides furcatus.
Sphenopteris hymenophylloides.
——— ? macrophylla.
Tæniopteris latifolia.
Zamia pectinata.
—— patens.
Thuytes divaricata.
——— expansa.
——— acutifolia.
——— cupressiformis.
Taxites podocarpoides.

SHELLS.

Nerita. 2 species; one banded; another banded and ribbed; both preserving their colour.
Turritella?
Another spiral univalve.
Astarte.
Avicula ovata.
Gryphæa. 2 species; one of small size, another large.
Lima *rudis*.
Modiola *imbricata*.
——— aliformis.
———, another species.
Mytilus.
Ostrea; a plicated species, and probably another.
Pecten *fibrosus*.
——— *obscurus*.
Pholadomya *acuticostata*.
Pinna.
Plagiostoma; nearest to *cardiiforme*.
Terebratula *obsoleta*.
——— maxillata.
Trigonia impressa.

Of the 10 known species of fossil shells here enumerated, 3 are found in the Yorkshire great oolite, and 1 in the cornbrash of the same county; 1 occurs in the great oolite of Calvados; 1 in the cornbrash and forest marble of the North of France; and 2 in the Mauriac beds, South of France. The Plagiostoma cardiiforme, which one of the above shells is stated to approach, is found in the great oolite of Yorkshire, and in the cornbrash and forest marble of the North of France.

M. Dufrénoy presents us with the following list of organic remains found in the beds between the Pointe Duche, and the Pointe d'Angoulin. M. Dufrénoy seems inclined to refer the beds to the middle oolitic system; but their organic contents will do equally well, if not better, for that now under consideration.

Isocardia concentrica (Sow.).
Cardita obtusa (Sow.).
Terebratula triquetra (Sow.).
——— ornithocephala.
——— acuta (Sow.).
Lima antiqua (Sow.).
Actæon cuspidatus (Sow.).
Encrinites pyriformis.
Isocardia transversa (d'Obigny.).
——— brevis (d'Obigny.).
Plagiostoma læviusculum (Sow.).
Lima rudis (Sow.).
Ostrea gregaria (Sow.).
——— expansa ?? (Sow.).
Mya gibbosa (Sow.).
Modiola.

* Geological Transactions, New Series, vol. i. p. 394.
† Zoological Journal, vol. iii. p. 417.
‡ See list of vegetable remains, inserted in the *Tableau des Terrains qui composent l'écorce du Globe, par Alex. Brongniart*, p. 413.

Nerinea.

Nerinea. 2 species.
Gervillia.
Nerita.
Pteroceras.
Cidarites.
Spines of Cidarites.
Polypifers. Many species.
Serpula.
Gryphæa ? dilatata ? ? ?

The same author presents us with the following list of organic remains from beds in the vicinity of La Rochelle, which may probably also belong to the system under consideration.

Astarte elegans (Sow.).
——— pumila (Sow.).
Arca pulchra (Sow.).
Cucullæa elongata (Sow.).
Terebratula perovalis.
Modiola.
Lutraria ovalis (Sow.).
Lucina.
Pholadomya Proteii.
Ammonites annulatus.
Mytilus pectinatus.
Turritella muricata.
Trigonia.
Dicerata.
Cardium.
Ammonites Lamberti.
Caryophyllia.

Middle Oolitic System.—Subdivision: OXFORD CLAY and KELLOWAY ROCK.

Yorkshire.—Phillips.	Midland and S. England. —Conybeare.	Calvados.—DeCaumont; De la Beche.
Mya literata, O. C. & K.		Crocodile.
—— calceiformis, K. (Phil.)	*Ichthyosaurus*..............	*Ichthyosaurus.*
Sanguinolaria undulata, O.C.		*Plesiosaurus.*
Crassina lurida, O. C.		Fish.
——— carinata, O. C. (Phil.)		
Pholadomya obsoleta, O.C. & K. (Phil.)....................	Pholadomya deltoidea, K.	Pholadomya ovalis.
Corbis ? ovalis, K. (Phil.)		——— ambigua.
——— lævis ? K.		
Corbula curtansata, K. (Phil.)		
Amphidesma recurvum, K. (Phil.)		Isocardia concentrica.
Lucina lirata, K. (Phil.)		
Crassina carinata, K. (Phil.)		
Cardium dissimile, K. (Murch)	Exogyra digitata, K.	Trigonia elongata.
Trigonia *clavellata*, K.		——— *clavellata.*
Modiola cuneata, O. C. & K. ?		——— *costata.*
——— pulchra, K. (Phil.)		Modiola subcarinata.
Nucula elliptica, O. C. (Phil.)		Nucula pectinata.
——— nuda, O. C. (Y. & B.)		
Cucullæa concinna, O. C. & K. ? (Phil.)		
Pinna mitis (Phil.) O.C. & K. ?		Pinna tetragona.
Trigonellites politus (Phil.) O. C.		
Plagiostoma duplicatum, O.C. & K.	Plagiostoma obscurum, K.	
——— finely striated, O.C.		
Avicula expansa (Phil.) O.C. & K.	Avicula inæquivalvis, K.	

Yorkshire.—Phillips.	Midland and S. England. —Conybeare.	Calvados.—De Caumont; De la Beche.
Avicula Braamburiensis, K.		
Lima rudis, K.		Lima proboscidea.
Pecten *fibrosus*, K.	Pecten *fibrosus*, K.........	Pecten *lens*.
——— *lens*, K.		—— vimineus.
——— demissus (Phil.), K.		Ostrea *gregarea*.
		—— plicatilis.
Ostrea inæqualis (Phil.),O.C.	Ostrea *palmetta*, O. C.	—— *palmetta*.
——— undosa (Bean), K.		—— minima (Deslong.).
——— archetypa (Phil.), K.		—— carinata.
——— *Marshii*, K............	Gryphæa incurva, K.......	—— *Marshii*.
		Gryphæa Maccullochii.
Gryphæa *dilatata*, K.	——— *dilatata*, O. C. ...	——— *dilatata*.
Perna quadrata, K............	Perna *aviculoides*, O. C....	Perna *aviculoides*.
Terebratula *ornithocephala*,K.	Terebrat. *ornithocephala*,K	Terebrat. *ornithocephala*.
——— socialis (Phil.), K.		——— biplicata.
Turbo sulcostomus (Phil.),K.		
Cirrus depressus (Phil.), K.		
Trochus guttatus (Phil.), K.		Trochus Gibsii.
Turritella muricata, K.		
Rostellaria trifida (Bean),O.C	Rostellaria, O. C. & K....	Rostellaria.
——— bispinosa (Phil.),K.		
Patella *latissima*, O. C.	Patella *latissima*, O. C.	
Belemnites sulcatus (Miller), O. C. & K.	Belemnites, O. C. & K....	Belemnites.
——— gracilis (Phil.),O.C		
Nautilus hexagonus?, K. ...	Nautilus, O. C. & K.	Nautilus.
Ammonites plicatilis, K.		
—— vertebralis? O. C.......	Ammonites *armatus*, O.C.	Ammonites *armatus*.
—— oculatus (Phil.), O.C.		——— communis.
—— Vernoni (Bean), O.C.		
—— athleta (Phil.) O.C. & K.		
—— *Kœnigi*, K...............	——— *Kœnigi*, K.........	——— omphaloides.
—— bifrons (Phil.), K.		
—— Gowerianus, K.		
—— perarmatus, K.		——— plicomphalus.
—— *Calloviensis*, K.	——— *Calloviensis*, K....	——— acutus.
—— *Duncani*, K.	——— *Duncani*, O. C....	——— *Duncani*.
—— gemnatus (Phil.), K.		
—— *sublævis*, K...............	——— *sublævis*, K.	——— *sublævis*.
—— flexicostatus, (Phil.)		
—— funiferus (Phil.), K. (nearly resembles A. excavatus)		——— excavatus.
		——— annulatus.
—— lenticularis, (Phil.)		
Serpula intestinalis (Phil) O.C.	Serpula, O. C..............	*Serpula† quadrangularis.
Astacus rostratus (Phil.), K.		*Ananchites bicordata.
Astacus, O. C.		*Galerites depressa.
Tooth of Squalus, O. C.		——— patella.
Saurian bone, K.		*Nucleolites scutata.
		*Cidaris.
Fossil Wood	Dicotyledonous Wood ...	Veget. remains (abundant.)
Spatangus ovalis, K.		

† The organic remains marked with an asterisk are from the observations of M. Desnoyers.

M. Boblaye

M. Boblaye furnishes us with the following lists of organic remains in the Stenay blue marls and the Oxford clay; these he has kept distinct, because he is not certain whether the former should be united to the cornbrash or to the Oxford clay.

Fossils of the Stenay Blue Marls,—North of France.

Plesiosaurus.
Ammonites coronatus?
Serpula.
Ostrea nana? or Gryphæa.
Ostrea.
Trigonia *costata.*
——— *clavellata.*
Pecten (*small*).
Nucleolites.

Organic Remains, Oxford Clay,—North of France.

Ammonites.
Belemnites.
Ostrea pectinata.
—— pennaria.
—— *gregarea.*
—— flabelloides (Lam.).
—— deltoidea (Sow.).
Gryphæa *dilatata.*
Anomia.
Pinna lanceolata (Sow.).
Pholadomya.
———— (approaching P. Protei).
Modiola tulipea (Lamarck).
Mytilus.
Terebratula (approaching T. subrotunda).
———— media (Schlot).
Pecten.

From the above materials, the following list may be constructed, of organic remains which occur in more than one of the localities mentioned: the Stenay marls being considered as part of the Oxford clay; they may indeed represent the Kelloway rock.

Name.	Yorkshire.	Mid. and S. Engl.	Calvados.	North of France.
Plesiosaurus	..	..	*	*
Ichthyosaurus	..	*	*	..
Trigonia clavellata	*	..	*	*
——— costata	..	..	*	*
Pecten fibrosus	*	*	..	..
——— lens	*	..	*	..
Ostrea palmetta	..	*	*	..
——— Marshii	*	..	*	..
——— gregarea	..	..	*	*
Gryphæa dilatata	*	*	*	*
Perna aviculoides	..	*	*	..
Terebratula ornithocephala	*	*	*	..
Patella latissima	*	*	..	..
Belemnites	*	*	*	*
Ammonites armatus	..	*	*	..
——— Kœnigi	*	*	..	..
——— Calloviensis	*	*	..	..
——— Duncani	*	*	*	..
——— sublævis	*	*	*	..
Vegetable Remains	*	*	*	..

Of

Of the 63 species of fossils enumerated in the Oxford clay of Yorkshire, more than one half are new: but of the remainder, 12 occur in the other localities; of these, 8 are mentioned in Mr. Conybeare's lists, 7 are discovered in Normandy, and 2 in the North of France.

Of the 16 species noticed in Mr. Conybeare's lists of Midland and Southern England, 11 are found in the other localities, and of these, 8 occur in Yorkshire, 7 in Calvados, and 1 in the North of France.

Of the 34 species enumerated in Calvados, 12 are found in the other localities; and of these, 7 are discovered in Yorkshire, 7 in Midland and Southern England, and 4 in the North of France.

Of the 11 species, not questionable, mentioned as found in the North of France, 4 are discovered in the other localities; and of these, 2 occur in Yorkshire.

It will be observed that the Gryphæa dilatata is found in all the localities; and that Ammonites Duncani, A. sublævis, and Terebratula ornithocephala, are common to England and Normandy.

Subdivision: Coral Rag, Oxford Oolite, and Calcareous Grit.

*Organic Remains of the lower Calcareous Grit of Yorkshire.**

Dicotyledonous Wood.
Spongia.
Crinoidal columns.
Echinus germinans.
Cidaris vagans.
Spatangus ovalis.
Clypeaster pentagonalis (Phil.).
Galerites depressus.
Pholadomya simplex (Phil.).
———— deltoidea ?
Sanguinolaria undulata.
Mya literata.
Isocardia tumida (Phil.).
Crassina carinata ?
Venus.
Lucina crassa.
Modiola bipartita.
Avicula ovalis (Phil.).
Lima *rudis*.
Gryphæa bullata ?
———— chamæformis.
———— inhærens.
Ostrea *gregarea*.
Terebratula socialis.
Cirrus cingulatus (Phil.).
Actæon retusus (Phil.).
Turritella muricata.
Rostellaria bispinosa ? (Phil.).
Trochus granulatus.
———— *bicarinatus*.
Belemnites sulcatus.
Ammonites Sutherlandiæ ?
———— perarmatus.
———— instabilis.
———— solaris (Phil.).
———— *vertebralis*.
Dentalium.
Serpula lacerata (Phil.).

Organic Remains of the Coralline Oolite of Yorkshire.†

Dicotyledonous Wood.
Crocodile.
Ichthyosaurus.
Palatal teeth of Fish.
Astacus rostratus (Phil.).
Spongia floriceps (Phil.).
Turbinolia dispar (Phil.).
Caryophyllia cylindrica (Phil.).

* Phillips's Geology of Yorkshire, p. 134—136. † *Ibid.* p. 126—132.

Caryophyllia,

Caryophyllia, like C. flexuosa (Sol. & El.).
——— like C. *cespitosa* (S.&El.)
Astræa *favosioïdes* (Smith).
——— inæqualis.
——— micastron.
——— arachnoides.
——— tubulifera (Phil.).
Meandrina.
Pentacrinus Caput Medusæ.
Cidaris florigemma (Phil.).
——— *intermedia* (Flem.).
——— monilipora (Y. & B.).
Echinus germinans (Phil.).
Clypeus *sinuatus*.
——— emarginatus (Phil.).
——— *clunicularis*.
——— dimidiatus (Phil.).
——— semisulcatus (Phil.).
Spatangus ovalis (Park.).
Galerites depressus.
Pholas recondita (Phil.).
Modiola? inclusa (Phil.).
Mya literata.
Pholadomya (like P. Murchisoni).
Amphidesma? recurva.
Psammobia lævigata (Phil.).
Tellina ampliata (Phil.).
Corbis lævis?
Crassina ovata (Smith).
——— elegans.
——— aliena (Phil.).
——— extensa (Phil.).
Venus.
Cytherea.
Corbula curtansata (Phil.).
Cardium lobatum (Phil.).
Isocardia rhomboidalis (Phil.).
Cardita similis.
Trigonia *costata*.
——— *clavellata*.
Hippopodium ponderosum.
Nucula.
Cucullæa oblonga.
——— contracta (Phil.).
——— triangularis (Phil.).
——— pectinata (Phil.).
——— elongata?
Arca quadrisulcata.
——— æmula (Phil.)
Modiola imbricata?
——— ungulata (Y. & B.).
Trigonellites antiquatus (Phil.).
Pinna *lanceolata*.
Perna quadrata.
Gervillia aviculoides.

Avicula expansa (Phil.).
——— ovalis (Phil.).
——— elegantissima (Bean).
——— tonsipluma (Y. & B.).
Plagiostoma læviusculum.
——— rigidum.
——— rusticum.
——— duplicatum.
Pecten abjectus.
——— inæquicostatus (Phil.).
——— cancellatus.
——— demissus.
——— *lens*.
——— viminalis.
——— vagans.
Lima *rudis*.
Ostrea *gregarea* (Smith).
——— solitaria.
——— duriuscula (Bean).
Chama or Gryphæa? mima (Phil.).
Gryphæa bullata?
Terebratula intermedia.
——— globata.
——— ornithocephala.
——— ovata?
——— obsoleta?
Orbicula? radiata (Phil.).
Delphinula.
Natica arguta (Smith).
——— nodulata (Y. & B.).
——— cincta (Phil.).
Turbo *muricatus*.
——— funiculatus (Phil.).
Trochus granulatus.
———? tornatilis (Phil.).
Turritella muricata.
——— cingenda?
Terebra melanoïdes (Phil.)
———? granulata.
Melania *Heddingtonensis*.
——— *striata*.
Bulla elongata (Phil.).
Murex Haccanensis (Phil.).
Ammonites perarmatus.
——— triplicatus.
——— *plicatilis*.
——— Williamsoni (Phil.).
——— Lamberti.
——— Sutherlandiæ.
——— sublævis.
——— lenticularis.
——— *vertebralis* et cordatus.
Belemnites sulcatus? (Miller).
——— fusiformis? (Miller).
Vermicularia compressa (Y. & B.).
Serpula squamosa (Bean).

Organic

*Organic Remains in the Coral Rag and Calcareous Grit of Midland and Southern England.**

Wood.
Ichthyosaurus.
Cidaris papillata (Park.).
——— *intermedia* (Park.).
——— diadema (Park.).
Clypeus *clunicularis.*
——— *sinuatus.*
Caryophyllia approaching C. carduus.
——— *cespitosa?*
Astrea *favosioïdes* (Smith).
——— approaching A. annularis.
Ammonites excavatus.
——— giganteus.
——— *plicatilis.*
——— *vertebralis.*
——— splendens.
Nautilus.
Belemnites.
Melania *Heddingtonensis.*
Melania striata.
Turbo *muricatus.*
Helix?
Trochus *bicarinatus.*
Ampullaria.
Turritella?
Ostrea *gregarea.*
——— Crista Galli (Smith).
Pecten *fibrosus.*
——— *lens.*
——— arcuatus.
——— *similis.*
Chama.
Trigonia.
Lima *rudis.*
Lithophaga.
Mytilus.
Modiola.
Serpula.

To these may be added, Pinna *lanceolata*, Trigonia *costata*, and T. *clavellata*, as found at Weymouth. Ostrea deltoidea enters into the upper calcareous grit, and Gryphæa dilatata into the lower calcareous grit of the same place.

Fossils of the Coral Rag of Normandy.†

Clypeaster.
Cidaris. } Numerous.
Clypeus. }
Caryophyllia. }
Astrea. } Numerous.
Madrepora. }
Ammonites.
Nautilus.
Melania *Heddingtonensis.*
Trochus.
Nerinea.
Ostrea *gregarea.*
——— minima.
——— a large species.
Lucina or Tellina.
Pecten *fibrosus?*
——— *lens?*
——— *similis?*
Modiola.
Lima.
Ampullaria.
Venus.
Lucina.
Chama.
Trigonia.
Gervillia.
Mytilus.
Pinna pinnigena.
Dicerata (very abundant).

Fossils of the Coral Rag in the North of France.‡

Polypifers (numerous).
Crinoidal remains (numerous).
Turrilites, appr^g T. Babeli (Brong.).
Melania *striata* (numerous).
———? approaching M. lactea.
Turritella?
Terebra appr^g T. sulcata (numerous).
Cidarites globatus (Schl.).
Echinus.
Echinital spines, (numerous).
Plagiostoma rigidum.
Pecten.
Ostrea *gregarea.*
Lima *rudis.*
Terebratula, appr^g T. digona.

* Conybeare, Outlines of England and Wales, pp. 187, 188.
† Desnoyers, *Annales des Sciences Naturelles*, tom. iv. p. 371; and De Caumont, *Topographie Géognostique du Calvados*, pp. 128, 129.
‡ Boblaye, *Annales des Sciences Naturelles*, tom. xvii. p. 72.

M. Elie

M. Elie de Beaumont notices numerous polypifers and echinital spines in the coral rag and Oxford oolite of Burgundy. The same rocks also contain large Nerineæ, Deciceratæ, large fibrous shells, dentated oysters, striated Terebratulæ, vegetable impressions, &c.*

M. Dufrénoy describes rocks, which he considers equivalent to coral rag, near Marthon and Rochefoucault, and states that they contain an abundance of polypifers of the same kinds as those of the coral rag, and numerous crinoidal remains.†

The reader will perceive that, though the coral rag and Oxford oolite occur in numerous parts of England and France, their organic remains have not been well determined, excepting by Mr. Phillips in Yorkshire. The other published lists are exceedingly defective, more particularly in the catalogue of the polypifers from which the rock has received one of its names. The following list, therefore, of organic remains which have been noticed in more than one of the localities, will be exceedingly meagre; whereas if the polypifers had been better known, it would have been quite the contrary.

Name.	Yorkshire.	Midl. and S. Engl.	Normandy.	North of France.
Vegetable remains	*	*	...	...
Ichthyosaurus	*	*	...	...
Crinoidea	*	*	...	*
Astræa favosioïdes	*	*	...	...
Clypeus clunicularis	*	*	...	...
——— sinuatus	*	*	...	...
Cidaris intermedia	*	*	...	...
Trigonia costata	*	*	...	...
——— clavellata	*	*	...	...
Pinna lanceolata	*	*	...	...
Lima rudis	*	*	...	*
Ostrea gregarea	*	*	*	*
Pecten lens	*	*	* ?	...
Turbo muricatus	*	*	...	...
Melania Heddingtonensis	*	*	*	...
——— striata	*	*	...	*
Trochus bicarinatus	*	*	...	...
Belemnites	*	*	...	...
Ammonites plicatilis	*	*	...	...
——— vertebralis	*	*	...	...

Astræa favosioïdes is the only polypifer among the numerous species contained in the coral rag, which has been probably determined in more than one locality, unless, indeed, we except the Caryophyllia stated to resemble C. cæspitosa of Ellis and Solander.

* *Annales des Sciences Naturelles*, Juillet 1829.

† *Annales des Mines*, 3e liv. 1829, pp. 404 & 406.

Of the 13 species of Echinites enumerated in the coralline oolite and calcareous grit of Yorkshire, 6 were first named by Mr. Phillips; of the remainder, 3 are found in Midland and Southern England. The species of Echinites are not enumerated in the Continental lists.

Of the very numerous fossil shells of the Yorkshire lists, the greater proportion are new; 12 of the remainder are found in the other localities; 12 in Midland and Southern England; 3 in Normandy (one being questionable); and 3 in the N. of France.

Of the 21 species contained in Mr. Conybeare's lists, or noticed in the Weymouth beds, 12 are found in the other localities; and of these, 12 are found in Yorkshire, 3 in Normandy (1 being questionable*), and 3 in the North of France.

Of the 7 species mentioned in Normandy, 3 are questionable; of the remainder, 3 are found in Northern, Midland, and Southern England, and 1 in the North of France.

Of the 5 species determined with certainty in the coral rag of the North of France, 3 are found in Northern, Midland, and Southern England, and 1 in Normandy.

Ostrea gregarea occurs in all the localities; and Lima rudis, Melania Heddingtonensis, and M. striata, are extensively distributed. It is probable, that when the Echinites and Polypifers shall have been determined with care, many species will be found widely dispersed.

Upper Oolitic System.—Subdivision: KIMMERIDGE CLAY.

We unfortunately at present possess very little information respecting the distribution of the organic remains of either this subdivision or the following. Mr. Phillips gives the following list of the Kimmeridge clay fossils in Yorkshire.†

Dicotyledonous Wood.
Ostrea *deltoidea.*
Belemnites.
Ammonites plicomphalus?
Fragments of Ammonites.

Organic Remains in Midland and Southern England.‡

Plesiosaurus *recentior.*
Ichthyosaurus.
Belemnites.
Nautilus
Ammonites.
Trochus.
Turbo.
Melania Heddingtonensis.
Ostrea *deltoidea.*
——— *Crista Galli.*
Pecten.
Avicula.
Astarte lineata.
Astarte ovata.
Trigonia *costata.*
——— *clavellata.*
Venus.
Modiola.
Cardita (Pholadomya).
Cardium.
Mactra.
Tellina.
Chama.
Terebratula.
Serpula.

* Two other questionable species might be added.
† Geology of Yorkshire, p. 125.
‡ Conybeare, Outlines, &c. pp. 178, 179.

Fossils

Fossils of the Kimmeridge Clay (Argile de Honfleur) *of Normandy.**

Crocodilus longirostris.
——— brevirostris.
Plesiosaurus *recentior.*
Ichthyosaurus.
Pholadomya Protei (Alex. Brong.).
Amphidesma securiforme (Phil.).
——— recurvum (Phil.).
Trigonia *costata.*
Mya *depressa.*
Isocardia?
Gervillia siliqua (Deslong.).
——— pernoides (Deslong.).
Donacites Alduini.
Inoceramus.
Lucina.
Cucullæa.
Terebratula.
Gryphæa *virgula.*
——— nana.
Ostrea *deltoidea.*
——— *Crista Galli.*
——— gregarea.
Pteroceras Oceani (Alex. Brong.).
——— Ponti (Alex. Brong.).
——— Pelagi (Alex. Brong.).
Melania Heddingtonensis.
——— striata.
Rostellaria composita.
Trochus.
Belemnites.
Teredo.
Serpula.
Cidaris.
Lignite.

Of the organic remains contained in the Kimmeridge clay of other parts of France little is known, except that the Gryphæa virgula is very abundant, and as characteristic of this clay in that country as the Ostrea deltoidea is in England.

M. Dufrénoy, describing some marls near Angoulême, gives a list of the following fossils, but seems to refer them to the upper part of the middle oolitic system, though they pass (descent to the Pont de la Trouve) into marls full of the Gryphæa *virgula.*†

Mya mandibulata.
——— *depressa.*
Pholadomya ovalis.
Pholadomya acuticosta.
Trigonia *clavellata.*
Natica sinuosa??

Besides the Gryphæa virgula contained abundantly in the Kimmeridge clay of the environs of Cahors (South of France), M. Dufrénoy mentions Terebratula perovalis. Lignite occurs in this clay or marl near the Pont de Rodes.

From the above the following small Table may be formed:

Name.	Yorkshire.	Midl. and S. Engl.	Normandy.	Angoulême.
Plesiosaurus recentior ...	...	*	*	...
Ostrea deltoidea	*	*	*	...
——— Crista Galli	...	*	*	...
Gryphæa virgula	...	...	*	*
Trigonia clavellata.........	...	*	...	*
——— costata..........	...	*	*	...
Mya depressa..............	...	...	*	*

* Alex. Brongniart, *Tableau des Terrains, &c.* pp. 410, 411; De Caumont, *Topographie Géognostique du Calvados*, pp. 117, 118; and Phillips, *Phil. Mag. and Annals*, March 1830.

† *Annales des Mines*, deuxième série, tom. v. p. 414.

 Subdi-

Subdivision: PORTLAND STONE.

Although this rock, as has been before stated, is observed in points crowning the oolitic series in England and France, its organic remains have not been well described except in Midland and Southern England: I shall therefore content myself by referring to Mr. Conybeare's lists.*

[To be continued.]

XXXVII. *On the spontaneous Purification of Thames Water. By* JOHN BOSTOCK, *M.D. F.R.S. &c.* †

IN the Report respecting the analysis of the water of the Thames, which I presented, in April 1828, to the Commissioners appointed by His Majesty to inquire into the supply of water in the metropolis, I have stated that when the experiments were nearly brought to a close, a quantity of water was sent to me, purporting to have been "taken in the river, in the current of, and immediately at the mouth of the King's Scholars' Pond sewer." I described it as "in a state of extreme impurity, opaque with filth, and exhaling a highly fœtid odour." When it had been about a week in my possession, a considerable quantity of black water subsided from it, but the fluid was still dark-coloured and opaque, and nearly as offensive as at first, while the odour and colour were only in part removed by being passed through a layer of sand and charcoal six inches in thickness.

The water remained for some time in my laboratory without being attended to; when, after an interval of some weeks, I observed that a great change had taken place in its appearance. It was become much clearer, whilst nearly the whole of the sediment had risen to the surface, where it formed a pretty regular stratum of about half an inch in thickness; the odour, however, still continued extremely offensive, perhaps even more so than at first. From this time the process of depuration, which had thus spontaneously commenced, was continued for about eight weeks, when the water became perfectly transparent, without any unpleasant odour, although still retaining somewhat of its original dingy colour.

After the formation of the scum mentioned above, the next change that I observed was its separation into large masses or flakes; to these, as well as to the scum itself, a number of minute air bubbles were attached, to which, no doubt, they owed their buoyancy: after some time the masses again subsided, leaving the fluid almost totally free from any visible extraneous matter. The quantity of gas discharged was inconsiderable,

* Outlines of the Geology of England and Wales, p. 176.

† From the Philosophical Transactions for 1829, part ii.

so that it was difficult to obtain any of it for examination. It seemed to be principally composed of carbonic acid, containing a little sulphuretted, and perhaps carburetted, hydrogen gas.

When the process of depuration appeared to be complete, the water was filtered through paper, and was then subjected to the same mode of analysis which was employed on the former occasion.* It was now perfectly transparent, and without taste or odour, but still retaining a slight brown tinge. It sparkled when agitated or poured from one vessel to another, and by boiling, a quantity of gas was disengaged from it: at the same time a thin film of carbonate of lime formed on the surface, which gradually subsided: 10,000 grains left by evaporation a saline crust of a light brown colour, which, after being thoroughly dried, weighed 7.6 grains. By the appropriate tests, the water was found to contain lime, sulphuric acid, muriatic acid, and magnesia. There was a trace of alumine and an indication of potash; but no ammonia, sulphur, or iron could be detected. The lime, the magnesia, and the sulphuric and muriatic acids were all of them obviously in much greater quantity than in the specimens of the Thames water previously examined. If we suppose the sulphuric acid to be combined with a part of the lime, and the remainder of the lime to be in the state of carbonate, and that a part of the muriatic acid is combined with the magnesia and the remainder with soda, as was conceived to be the case in the Thames water generally, the respective quantities of these salts in 10,000 grains will be as follows:

	grs.	grs.	
Carbonate of lime ...	4.20 ...	1.55	Salts contained in the Lambeth water, which was considered as the most impure of the specimens formerly examined.
Sulphate of ditto	.66 ...	.12	
Muriate of soda ... / Muriate of magnesia	2.74 ...	.23	
	7.60	1.90	

The result of this analysis shows, that although the water has, by this depurating process, freed itself from the great quantity of organic matter which it contained, and acquired a state of apparent purity, which might render it sufficiently proper for many purposes, yet that the quantity of saline matter is increased as much as fourfold. The greatest proportionate increase is in the muriates, which are very nearly twelve times more in the purified water than in the Thames water in its ordinary state. The carbonate of lime is between two and three times as abundant as before, and the sulphate of lime between five and six times. I may remark, that this water, when examined in its foul state, gave very obvious indications of both

* Report, p. 80—81.

sulphur

sulphur and ammonia, neither of which could be detected after depuration.

This depurating process may be denominated a species of fermentation; *i. e.* an operation, where a substance, without any addition, undergoes a change in the arrangement of its component parts, and a new compound or compounds are produced. The newly formed compounds were, in this case, entirely gaseous, and, except a part of the carbonic acid, were discharged. The saline bodies, being not affected by this process, remained in solution, leaving the fluid free indeed from what are considered as impurities, yet so much loaded with earthy and neutral salts, as to be converted from a soft into a hard water*. The source of the saline bodies may be supposed to be the organic substances, principally of an animal origin, which are so copiously deposited in the Thames; of these the most abundant are the excrementitious matters, as well as the parts of various undecomposed animal bodies. The different species of the softer and more soluble animal compounds act as the ferment, and are themselves destroyed, while the salts which were attached to them are left behind. It may be conceived therefore, that the more foul is the water, the more complete will be the subsequent process of depuration; and we have hence an explanation of the popular opinion, that the Thames water is peculiarly valuable for sea stores, its extreme impurity inducing the fermentative process, and thus removing from it all those substances which can cause it to undergo any further alteration.

The brown colour which the water exhibited after its depuration appeared to depend on the solution of a minute quantity of what is generally termed extractive matter, and which is observed in water that contains decayed vegetable substances; it is almost always present in the beginning of winter in the water of ponds, or of slow streams that have received the falling leaves. After the heavy rains that occurred in December 1827, the New River water, with which my cistern is supplied, was observed to be very turbid and dark-coloured. By remaining some hours at rest, a quantity of earthy matter subsided, and left the water nearly transparent, but the dark colour still continued†.

* The terms hard and soft, as applied to water, are obviously relative; but water which contains as much as 5 grains in the pint of saline matter, is generally regarded as too hard for many œconómical and manufacturing processes. The water in question contained 4.36 grains per pint.

† It is not easy to institute any exact comparative scale of the shades of brown. An infusion formed by digesting, for 10 days, powdered galls in twenty times their weight of water, and afterwards diluting the infusion with an equal bulk of water, will exhibit a colour nearly similar to that of the New River water in the state in which I examined it.

I found

I found that this colouring matter was not removed by boiling, nor by filtration through sand and charcoal, but that alum and certain metallic salts, especially when heated with it, threw down a precipitate, and left the water without colour. Of the metallic salts the most effectual appeared to be the sulphate of iron; a drop of the solution of this salt, boiled with 500 times its bulk of the water, threw down a flocculent, orange-coloured precipitate, and left the water perfectly colourless. I obtained the same results, only much less in degree, when these reagents were added to the Thames water after its depuration.

The sediment which was removed from the water by filtration, as mentioned above, appeared to be a heterogeneous mass of various substances, about $\frac{9}{10}$ths of which was siliceous sand; it also contained a black matter, which gave the whole a dark gray colour, and which was removed by a red heat; a number of fine fibres that looked like animal down; and some large fibres probably of vegetable origin: there were also bits of wood, fragments of coal, and small shining particles of a metallic nature, which seemed to be sulphuret of iron. The mass indeed consisted of all those substances which were casually introduced into the Thames, and which had not been decomposed by the fermentative process. They must of course differ, both in quantity and in quality, in every different portion of the water, so as to render it unnecessary to attempt a more minute examination of them: in the present instance, the sediment, when completely dried at a temperature of 200°, was in the proportion of about 9 grains in 10,000 grains of the water.

XXXVIII. *On the Elements of the Planet Ceres. By Professor* Encke *.

SINCE the completion of the first calculations for newly determining the orbit of Ceres, one of my respected astronomical friends has given me the hope that the investigations on this subject will be more completely and more accurately performed by another hand. It will therefore be sufficient in this place to explain the ground-work of my determination, in order the better to form an estimate of the confidence to which the places derived from it are entitled.

The perturbations were developed in the same manner as for the other small planets, in regard to the elements themselves, and not to the places of the planets in space. A review of the last determination of Professor Gauss (Zach's Monthly Correspond. 1809, May,) on which all places of the planet hitherto given were founded, and some trials made at the latest

* From the *Astronomisches Jahrbuch* für 1831, p. 275.

opposi-

oppositions, seem to prove sufficiently that the equations for the perturbations, if developed as is usual for the old planets, would require to be extended considerably beyond the first power of the eccentricity, if great accordance is intended. In the same proportion, however, the calculation of a single place would have become irksome, even taking into consideration the facility afforded by the excellent construction of the tables of perturbations (Zach's Corresp. 1803, March); and therefore, even if every part had already been perfectly developed, still this method would hardly have deserved the preference on the score of brevity of computation.

As an interval of time, the number of one hundred days was selected for this first approximation, and only the attraction of Jupiter was taken into account. The mass of this planet was taken, according to Nicolai, at $\frac{1}{1053 \cdot 924}$. This value, which is one-eightieth part more than the old determination by Laplace, appears in the cases of Pallas, Juno, and Vesta, to agree better with the observations, and therefore seems likewise for Ceres to deserve the preference.

The four oppositions necessary for deducing the elements were found to be, from the observations published, as follows:

☍ ⚳	Mean Time at Göttingen.	Heliocentric Longitude.	Geocentric Latitude.
	h ′ ″	° ′ ″	° ′ ″
1820 Jan. 25	3 43 10	124 38 29·6	+11 58 35·2
1821 May 22	5 43 47	241 12 36·4	+ 5 41 46·0
1822 Aug. 22	8 28 23	329 5 15·6	−14 53 14·6
1825 Mar. 14	11 5 56	174 4 50·4	+17 10 32·9

And proceeding from the elements at the moment of the first opposition, the computations of the perturbations for the following ones gave the following corrections of the elements, in which however the precession is still to be added to all the longitudes.

Correction.	1821. May 21·5.	1822. Aug. 22.	1825. March 14.
Mean Longitude ... = L =	− 1′ 30″·3	+ 2′ 30″·0	+ 1′ 36″·8
Long. of Perihelion = π =	− 16 17·2	−32 47·9	−107 55·7
Node.................. = ☊ =	− 50·6	− 41·9	− 2 20·3
Inclination .... = i =	+ 2·4	+ 8·3	− 19·1
Angle of Eccentricity = φ =	+ 12·5	+ 1 12·9	− 1 9·5
Daily Sider. Motion = μ =	+ 0·19072	+ 0·70887	+ 0·16263

These

These determinations require, perhaps, a repetition, being calculated with elements which give for the single oppositions places erroneous by fifteen minutes. For this very reason I did not deem it necessary to produce a perfect accordance of the elements with the oppositions, but was satisfied with such as gave errors in longitude less than 3″. The elements thus deduced, and true for the moment of the epoch, the longitude being referred, for the sake of agreement with the other small planets, to the mean equinox of 1810, are as follows:

Elements of Ceres.

Mass of Jupiter $\frac{1}{1053 \cdot 924}$.

Epoch 1822. Jan. 22. 0^h mean time at Göttingen.

$$\left.\begin{array}{l} L = 127^\circ\,36'\,51''\cdot 6 \\ \pi = 147\;\,36\;\,57\cdot 6 \\ \Omega = \;\;80\;\,41\;\,55\cdot 0 \end{array}\right\}\text{Mean equinox 1810.}$$

$$\begin{array}{l} i = 10\;\,38\;\;\,7\cdot 7 \\ \phi = \;\;4\;\,31\;\,18\cdot 0 \\ \mu = \;\;770\cdot 72468\;\text{(sidereal).} \end{array}$$

A rigorous comparison with the geocentric observations at the times of the above four oppositions, has presented the following differences.

Date.	Right Ascension.	Declin.	Place of Observation.
1820. Jan. 31	″ −5·8	″ +1·1	Mannheim.
Febr. 2	−6·2	−1·7	——
1821. May 16	+5·1	−0·6	Königsberg.
23	+0·1	+0·5	——
28	+1·9	−0·9	——
1822. Aug. 18	−4·7	+7·0	——
19	−5·6	+2·0	——
22	−3·0	+8·4	——
23	−4·8	+6·0	——
1825. March 9	−4·3	−0·3	Göttingen.
10	−6·0	+1·2	——
18	−1·7	+0·1	——
19	−1·9	+0·4	——
20	−3·0	+2·1	——

The two subsequent oppositions of 1827 and 1829 served as a test of the accuracy of the elements. For the former one

Prof. Gauss had the kindness to communicate to me the following observations:

1827.		Mean Time at Göttingen.	Rt. Ascension ♀	Declination ♀
Sept.	27	12h 11′ 28″·7	8° 50′ 36″·1	−13° 17′ 6″·1
Oct.	3	11 42 50·2	7 34 37·7	41 34·8
	4	38 3·7	7 21 56·4	45 6·0
	5	33 17·7	7 9 22·8	48 24·9
	6	28 31·9	6 56 53·0	51 37·1

For the opposition of 1829, I received the following excellent observations from Professors Schwerdt at Speyer and Argelander at Åbo, which are the more creditable as at the time of that opposition it was difficult to find this planet:

1828–1829.	Mean Time.	Right Ascension ♀	Declination ♀	Place of Observation.
Dec. 23	12h 49′ 20″	104° 47′ 50″·3	+28° 6′ 2″·0	Åbo.
28	24 42	103 32 53·6	34 37·0	—
29	19 44	103 17 25·6	40 7·2	—
Jan. 13	11 5 16	99 25 0·0	29 51 35·0	Speyer.
15	10 55 29	98 56 13·5	59 12·0	—

The computations of the perturbations, taken in the same sense as above, gave the following corrections of the elements:

Correction.	1827. Sept. 26.	1829. Jan. 17.
Δ L	+ 5′ 8″·4	+ 2′ 51″·1
$\Delta\ \pi$	−99 33·5	−94 28·6
Δ ☊	− 3 16·9	− 3 35·7
$\Delta\ i$	− 24·8	− 20·4
$\Delta\ \phi$	− 3 30·4	− 4 27·4
$\Delta\ \mu$	−0·11458	+0·03944

From which we obtain the following comparison:

1822.

Date.	Right Ascen. ♀	Declination ♀	Place of Observation.
	″	″	
1827. Sept. 27	− 2·1	+ 1·4	Göttingen.
Oct. 3	− 3·5	+ 0·8	——
4	0·0	+ 0·6	——
5	− 1·9	− 1·6	——
6	− 4·9	− 0·4	——
1828. Dec. 23	−25·6	−11·5	Åbo.
28	−28·3	−11·7	——
29	−27·0	−10·8	——
1829. Jan. 13	−26·4	− 9·3	Speyer.
15	−26·4	−10·6	——

Hence the oppositions of Ceres will be deduced as follows:

☍ ⚳ 1827. Sept. 26. $9^h\ 30'\ 45''$. Mean time at Göttingen.
Heliocentric longitude... $2°\ 58'\ 19''·5$
Geocentric latitude... ... −15 41 56 ·4

☍ ⚳ 1829. Jan. 1. $4^h\ 8'\ 47''$. Mean time at Göttingen.
Heliocentric longitude... $101°\ 3'\ 13''·5$
Geocentric latitude... ... +5 56 5·5

The mean geocentric errors in all six oppositions are consequently:

	Right Ascen. ♀	Declination ♀
	″	″
1820	− 6·0	− 0·3
1821	+ 2·4	− 0·3
1822	− 4·5	+ 5·9
1825	− 3·4	+ 0·9
1827	− 2·5	+ 0·2
1829	−26·7	−10·8

where the last somewhat more considerable difference answers to heliocentric errors of 13″ in longitude, and 7″ in latitude; so that it is to be hoped, even if these errors are chargeable to the elements only, and not, perhaps, also partly to the perturbation caused by Saturn and Mars, which have been neglected, that these errors will not render the finding of Ceres difficult for the approaching years, until the orbit shall have been more accurately determined.

The early development of the perturbations of Ceres, which was almost contemporary with the determination of the orbit itself, and the certainty, thereby obtained, of always being,

 for

for the future, sure of its position within ten or fifteen minutes of a degree, would appear to have been the cause that this planet, the first-discovered of the small ones, has been least observed in recent years;— at any rate I have not succeeded in obtaining accurate observations of Ceres at the times of its opposition, even since the period that the oppositions of the other small planets have been regularly observed by meridian instruments in German observatories. It is possible that some oppositions have really been entirely neglected. It is the more desirable that the future oppositions should not pass by unnoticed, as Ceres might likewise afford additional means of determining the mass of Jupiter, or might assist in answering the question which has lately been agitated, Whether for all planets, the attraction is rigorously proportional to their mass?

XXXIX. *On the Composition of Chloride of Barium, Nitrate of Lead, and Phosphoric Acid; and on the Atomic Weights of Iodine and Bromine.* *By Mr.* John Prideaux.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN a letter published in the Phil. Mag. and Annals, vol. vi. p. 166, I mentioned a scale of equivalents which, having found convenient in my own practice, I was induced to prepare for publication. This was deferred in consequence of Dr. Turner's experiments on chloride of barium; most of the equivalent numbers having been taken from Dr. Thomson's "First Principles," which those experiments tended to call in question. And it seemed desirable to wait the result of the further investigations promised in Dr. Turner's paper; in the mean while, having seldom found reason to be dissatisfied with the scale as it stood, employing my occasional leisure in testing it by more accurate experiments.

Not having seen any further progress reported by the London Professor, I send you the following account of some of these experiments.

So far as had occurred to me, chloride of barium and the sulphates precipitated each other completely in Dr. Thomson's proportions; as did nitrate of lead, and oxalate of ammonia: and on these equivalents depended the accuracy of many of the numbers in the "First Principles." If the experiment of Thomson, Berzelius, and Turner, with sulphate of potass, had been repeated by me, I should not presume to report it publicly: beside that, it is objectionable, without certain

tain precautions, from part of the potassa precipitating with the barytes.

(A.) Sulphate of soda of commerce being purified by repeated crystallization, some of the clearest and most regular crystals were melted, dried, and heated to redness for half an hour.

(B.) Oxalic acid was saturated with carbonate of ammonia, both of commerce.

(C.) Nitrate of lead was similarly prepared with nitric acid and ceruse; and both these salts purified also, by repeated crystallization, and selection of the crystals, were dried between folds of tissue paper in a current of warm air.

(D.) Chloride of barium was in the same manner prepared by neutralizing muriatic acid with common carbonate of barytes. Sulphuret of barium (sulphate ignited with charcoal) was added, and afterwards caustic barytes (carbonate similarly treated), till the liquid affected turmeric paper strongly. The salt was then crystallized, powdered, boiled in pure alcohol and digested therein for a night; redissolved in distilled water and subjected to a current of carbonic acid; recrystallized and heated to redness for half an hour, avoiding fusion, by which the salt had on a former occasion become alkalescent (perhaps from impurity, as it was differently prepared).

(1.) $13\frac{1}{4}$ grains of this chloride were precipitated by 9 grs. of sulphate of soda: the former weighed with every precaution against absorption of moisture; the solutions mixed hot; and the phial immediately placed in the sand-bath, where it continued for three hours. The clear supernatant liquid when cold, tested with its original ingredients, remained at first perfectly clear with each; but after a few minutes a slight opalescence appeared with the sulphate of soda; and in time, a very small quantity of precipitate subsided. I could not collect this satisfactorily, and the estimate of ·05 gr. for the quantity which might have been obtained from the whole solution, does not pretend to accuracy. The first precipitate weighed, after heating red, 14·68 grs.; and there was an opacity about the sides of the phial in which the solutions had been mixed.

(2.) $20\frac{3}{4}$ grs. of nitrate of lead were precipitated by $8\frac{7}{8}$ grs. of oxalate of ammonia; the solutions being mixed warm. The clear liquor remained perfectly transparent when tested with its original constituents in every way.

In checking these experiments by alternation of the ingredients, *i.e.* chloride of barium with oxalate of ammonia, and nitrate of lead with sulphate of soda, the result was not the same.

Oxalate of ammonia is a delicate substance to dry. Whether it it would be perfectly freed from hygrometric water, by remaining in a cold vacuum with sulphuric acid, I do not know; but conceiving there must be a vapour of that acid, which, however slight, must in the course of time settle more or less on the crystals, and still more on the powder, a current of warm dry air seemed a less objectionable mode. This too required caution; for if the temperature reached any thing approaching 212°, opacity appeared on some of the crystals, and the salt used in this state was always in excess. About 140° seemed to answer very well, and in this the salt was kept three or four hours; the nitrate of lead being kept beside it, that they might be equally dried.

(3.) $13\frac{1}{4}$ grs. of chloride of barium were precipitated by $8\frac{7}{8}$ grs. of oxalate of ammonia; and

(4.) $20\frac{3}{4}$ grs. of nitrate of lead by 9 grs. of sulphate of soda.

In both cases the anhydrous salt was in excess, and the precipitation manifest and immediate on applying the test.

All these experiments have been repeated as often as leisure would allow, with materials prepared at different times, and with identical results (except when, as above stated, there were overdried crystals of the oxalate of ammonia). The beam employed answers to the $\frac{1}{300}$dth of a grain; the weights are adjusted, by division and addition, from a standard eight-grain, used for that purpose only; and the salts were weighed by the French method, first balancing the weight and then putting the salt in its place. So far as confidence can be placed in the results, Thomson's numbers seem nearer approximations than those of Berzelius; according to which there should have been in Experiment 1. an excess of $\frac{13}{100}$ gr. of chloride of barium = 0·145 of sulphate; and in Experiment 2. an excess of $\frac{15}{100}$ gr. nitrate of lead = 0·13 nearly of oxalate.

The excesses in the 3rd and 4th Experiments correspond so far with the tables of Berzelius; they were not collected and weighed, because the absence of hygrometric water could not be depended on, though I can hardly believe the quantity could be such as to account for so much precipitate. That oxalic acid is almost exactly 4·5, I have no doubt; but it may be questioned whether azote is not under-rated in the "First Principles;" and as this is common to the two salts mutually equivalent, and deficient in relation to the others, the difference would be well accounted for. This would interfere with Dr. Prout's law; but whilst the ablest chemists are at variance on such simple experiments, we must be content with approximations.

Phosphoric acid is stated by Berzelius to consist of 5 atoms oxygen and 2 phosphorus: phosphorous acid, of 2 phosphorus and

and 3 oxygen. Thomson on the contrary makes, in the "First Principles," phosphorous acid an atom of each; and phosphoric, 1 phosphorus and 2 oxygen; to which he has since added a third acid (Phil. Mag. and Annals, vol. v.), of 1 phosphorus and 3 oxygen. An extract from Dr. Henry's "Chemistry," in your Number for the present month (March) is favourable to the views of the Swedish Professor, but with some expressions of indecision (regarding the uncertain composition of the phosphuretted hydrogen gases). The acid determined by Davy to consist of .... phosphorus 100, oxygen 134·5
and by Berzelius of .... —— 100 —— 133
is regarded by Thomson as 1 phosphorus and 2 oxygen; which opinion has some analogical corroboration from the constitution of the acid obtained from sulphur by Davy's process, *i. e.* combustion in oxygen. But the process of Berzelius, solution and cohobation (if the term may be so applied) with nitric acid, giving with sulphur an acid of 3 oxygen, the analogy fails, unless in the latter process phosphuretted azotic gas was formed, and $\frac{1}{3}$rd of the phosphorus thus wasted;—a case so improbable, that nothing less than the decisive experiments in the "First Principles," on acid of atomic weight 3·5, would have allowed the supposition to be entertained.

Combustion in chlorine and subsequent solution in water would be subject to the same uncertainty as combustion in oxygen, unless obviated by a current of chlorine through the solution. The following was however preferred:

Phosphorus melted in warm water was sucked up into a long glass tube, and there kept fluid until foreign matters, sinking or floating according to their specific gravity, had left the greater portion of it transparent and almost colourless. How to purify that substance more effectually, I do not know.

12·05 grains of the transparent part was put into a Woulfe's apparatus under water, and a current of chlorine gas, issuing close by the phosphorus, slowly passed. The phosphorus being little diminished after a week's continuance in this, the apparatus was opened, and a small test tube introduced, into which were inserted the end of the tube from the generator, and the phosphorus, so that the bubble of chlorine should rise along it. The action thus expedited, the bubbles rose cloudy, settling as a dense vapour on the surface of the water, and in two days the phosphorus was completely dissolved. On opening the apparatus the solution of phosphorus became turbid, and the sides of the bottle opaque and greasy, as though it were charged with powdered spermaceti. Although tempted by this new appearance, I preferred continuing the experiment as begun, and after leaving the tubulure open three days, the

the whole became transparent, the liquor (about 12 ounces) still continuing saturated with chlorine; but bleaching suddenly, when, on commencing the evaporation, it reached about 150°. Towards the end, about a drachm of nitric acid was added to insure complete oxidation; but no red fumes appeared. The acid heated red for a quarter of an hour weighed 29·25 grains. Yielding to the nail when cold, it still appeared to contain water; but as a small portion of black scum lay on a part of the surface, the ignition was not repeated for fear of decomposition. This black substance being separated on dissolving the acid, was too small to weigh. The acid dissolved in distilled water, and cautiously neutralized with bicarbonate of potassa, keeping it warm to dispel the carbonic acid, was precipitated with nitrate of lead in excess. The precipitate was boiled in weak acetic acid, and continued digesting in it twenty-four hours, to get rid of the excess of base so constantly tantalizing in precipitated phosphates, and to show the exact quantity of acid produced. In this I failed, the precipitated still weighing, after heating red, 130·7 grains. The experiment is, however, sufficient to prove that 12·05 grains of phosphorus require less than 17·2 of oxygen to form the phosphoric acid of 4·5, (for if the acid thus formed had been that of 3·5, the precipitate must have weighed at least 141·25 grains,) and 16·2 of oxygen is exactly the quantity to correspond with Sir H. Davy's experiment, by which 12·05 grains of phosphorus would produce 28·25 of phosphoric acid. The views of Berzelius on the phosphates appear to me correct, and the acid of 3·5 still a problem.

The new experiments of Berzelius quoted in the Quarterly Journal for September, seem to prove that the atomic weights of iodine and bromine have been deduced from materials not free from chlorine; and he makes them

	iodine 789·145	bromine 489·15
or × 2 by the English system ...	1578·290	——— 978·30

But as he makes chlorine 221·325, which × 2 = 442·650, and as the above numbers were obtained by comparison with chlorine, if the true number for that substance is nearer 4·5, iodine will turn out 16·04, and bromine nearly 9.95; numbers as near to 16· and 10· as the nature of the experiments would admit, and approaching, in the case of iodine, to the result of former experiments by Gay-Lussac and Prout.

I am, Gentlemen, yours, &c.

Plymouth, March 8, 1830. JOHN PRIDEAUX.

XL. *Account*

XL. *Account of some Optical Experiments.* By SAMUEL SHARPE, *Esq. F.G.S.**

Exper. 1.—I HELD a card between my eye and the candle, and moved it gradually till it very nearly hid all the flame, when the edge of the card was tinged with red, and the yellow and blue, &c. appeared in succession further from the card. The order of the colours proves that the ray of light is (at least principally) refracted round or towards, and not from, the edge†.

Fig. 1. A is a section of the flame,
B of the eye,
C D of the card;

and R Y V (using three, instead of the seven, for shortness) are the prismatic rays.

The red rays are in excess because of the colour of the flame; and when the card is moved still more towards V, the red is no longer seen, the blue and violet are so pale as to be hardly seen.

By measurement which was not very accurate, the red rays appeared to be refracted about 28′; and my measurement was not accurate enough to determine whether or not different substances refracted light differently. The brightest rays appeared *on* the card rather than by the side of it, as in fig. 8, making the fringe described in Experiment 5, and from the cause there mentioned. Part of the inaccuracy of the measurement mentioned above arose from this fringe.

If I applied to the ray a second card B (fig. 9), between the first card A and the candle, and in the same direction, the ray disappeared, being refracted to C.

If I applied it in the contrary direction, as D, no apparent change took place, because, though one ray might be refracted round D, there were others to fall on A; but when the second card was applied exactly opposite to A, as E, then the coloured rays were gradually drawn towards E, as it approached.

Exper. 2.— I held before my eye the eye-glass of a telescope having parallel cobwebs in the focus, and moved it before the candle till the light fell obliquely on the cobwebs: the cobweb at R (fig. 1.) was completely red; and as I moved it towards V, the cobwebs were prettily coloured with each of the colours in its turn.

* Communicated by the Author.

† Newton appears to contradict himself in his Optics and Principia; in one stating that light is refracted *round or towards*, and in the other *from*, an intervening edge.

 This

This experiment proved the refraction caused by any edge brought near to a ray of light, more clearly and satisfactorily than the former, but did not show whether the ray was refracted from or towards the intercepting edge, as it was not evident on which side of the cobweb the ray passed.

Exper. 3.—I then looked at the candle through a slit between two parallel cards, sometimes $\frac{1}{40}$th of an inch apart and sometimes less, when the candle appeared, as in fig. 2, very broad, with its edges red; nearer the middle it was yellow, and the middle slightly blue or violet; the red and yellow much in excess, because of the colour of the candle: there was also a row of smaller candles on each side, each of which had its outer edge red, and its inner one violet.

When the cards were further apart, the flame was less distorted, as in fig. 3, with more but narrower traces of side flames: when less apart, more distorted, as in fig. 4, with fewer and wider side flames.

In each case the width of the whole luminous appearance was the same, because the refraction was the same; but the greater the number of rays which passed through the opening, the greater the number of candles into which the coloured rays re-arranged themselves.

Exper. 4.—When I looked at the candle through a round hole in the card of about the same diameter ($\frac{1}{40}$th of an inch), the effect was much the same, but less regular in appearance, from its being circular: but when I lessened the hole and came within two feet of the candle (for in the former experiments I had been about ten feet distant), the distinctions of colour were lost, and the hole appeared as in fig. 5, with a distinct luminous centre, but the outer part was marked with dark rays diverging from that centre.

When I closed the eye-lids a little, the centre remained the same, but parts of the top and bottom of the outer circle were darkened as in fig. 6: when the eyelids were more closed, still more of the top and bottom was darkened, as in fig. 7, plainly proving that light enters the eye through the iris as well as through the pupil; the luminous centre being made by those rays which passed uninterruptedly through the pupil, and the radiated outer circle being the shadow of the iris as thrown on the back of the eye, unless lessened by the intervention of the eyelid, as in figs. 6 and 7.

The bright reflection of a window or candle on the bulb of a thermometer is surrounded by the same radiations, from the same cause.

Exper. 5.—If a card be held so as *just* to conceal an object, the card will appear fringed with the object, or the object may appear

appear as seen through the edge of the card (see Exper. 1,

Fig. 2.

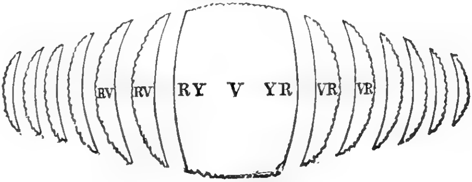


Fig. 1.

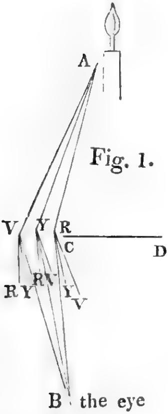


Fig. 3.

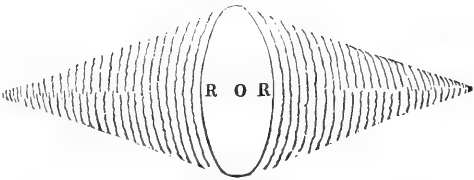


Fig. 4.

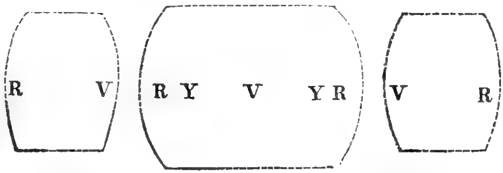


Fig. 9.

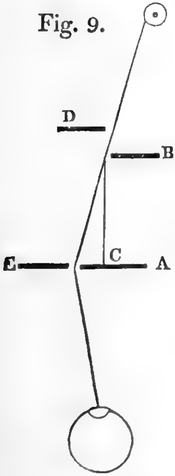


Fig. 5.

Fig. 6.

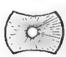


Fig. 7.

Fig. 8.

Fig. 11.

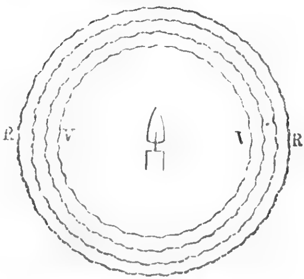
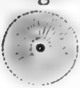


Fig. 10.



and fig. 8.); and this arises from the pupil not being a point,

but

but a space of sensible size, and the appearance is the same as when, with two eyes, we hide the candle by a card from one eye only, we may fancy that we see the candle through the card. And from this fringe or indecision of outline arises the slight pain which the eye sometimes feels, on looking at one piece of gauze of fine network through another.

This fringe appears and affects the eye like an object not exactly in the focus of a magnifying glass.

Exper. 6.—If the eye be directed in a piercing manner to an object at six feet distance, with a view to distinguish the minute parts, the lens of the eye is flattened, that is, accommodated to the purpose by having the focus made more distant; if it be then quickly turned to a point of light nine inches distant, there appears a dark spot in the middle, as in fig. 10.

Exper. 7.—If a light be seen through a coach-glass made dull by breath condensed on it, the light appears surrounded by a ring of the prismatic colours, the red being at the outside and the violet at the inner ring. Fig. 11.

Exper. 8.—If the bright spot of light on a thermometer-bulb be observed, the outer edge of the spot which enters the eye *through the iris*, (see fig. 5, 6, 7, 12,) will be seen tinged with red, showing that the eye is not quite achromatic, but is what opticians term *under*-corrected; but the pupil appears to be perfectly achromatic; though it may perhaps be unphilosophical to apply the term to the pupil; as it is itself our only standard we have no other by which we can compare it.

Canonbury, March 2, 1830. SAMUEL SHARPE.

XLI. *On a Specimen of Varvicite from Ihlefeld.* By Dr. EDWARD TURNER, *F.R.S. L. & E. &c. Prof. of Chemistry in the University of London.* (*In a Letter to* Richard Phillips, *F.R.S. L. & E.*)

My dear Sir,

I HAVE lately received a curious specimen of manganese from Prof. Stromeyer, in the history of which you will feel considerable interest, as my examination of it leaves little or no doubt of its being a specimen of varvicite. It was found about a year ago at Ihlefeld in the Hartz, and occurs in after-crystals having the form of the six-sided pyramid of calcareous spar. From this circumstance it is probable that the crystals at one time consisted of carbonate of manganese, which has been converted into its present state by subsequent decomposition.

Of the nature of the original crystals two views seem plausible. They may originally have been composed of carbonate of lime,

lime, which has been afterwards removed and carbonate of manganese substituted, by the action of carbonated springs holding the latter salt in solution; or, since carbonate of manganese is isomorphous with carbonate of lime, the former may possibly have assumed the pyramidal form in which calcareous spar sometimes occurs, by direct crystallization. However this may be, the crystals in my specimen are quite free from carbonate of lime, and contain, besides oxide of manganese, nothing but traces of baryta and oxide of iron.

The mineral is made up of small lamellated prismatic crystals, too confusedly intermixed to admit of an accurate determination of cleavage, but in lustre and general outline resembling varvicite and manganite. The colour of its streak and powder is like that of varvicite, and in hardness also it corresponds with that mineral. Its specific gravity is 4.623, while that of varvicite is 4·531. When converted into red oxide by a white heat, it loses 13·13 per cent, of which 4·98 are water and 8·15 oxygen; whereas your varvicite under the same circumstances loses 13·11 per cent, of which 5·725 are water, and 7·385 oxygen.

It appears to me from these characters that the mineral under examination must either be varvicite with a small admixture of peroxide, or a mixture of peroxide with a considerable quantity of manganite. But the last supposition is untenable; since a mixture of crystalline particles of such unequal hardness as manganite and the peroxide, could not fail to be recognised as such; while this mineral, throughout, is of uniform hardness.

It may not be useless, while writing on the subject of manganese, to state a fact which I do not remember to have seen noticed by others. When chemists, in preparing pure salts of manganese, add oxalate of ammonia or potash to a solution of the chloride, and the liquid retains its transparency, it is usual to infer the total absence of lime. But the inference is not altogether correct; for the solution of chloride of manganese, though perfectly neutral, gives to oxalate of lime a degree of solubility which that salt does not possess in pure water. Hence I have known a solution of manganese to retain its transparency an hour or two after oxalate of ammonia was added, but to deposit a little oxalate of lime in the course of twenty-four hours; and when, after that period, the clear liquid was precipitated by an alkaline carbonate, the carbonate of manganese taken up in nitric acid, and the nitrate decomposed by a heat short of redness, the residue yielded nitrate of lime to water.

I am, dear Sir, yours very truly,

University of London, March 15th, 1830. Edw. Turner.

XLII. *Results*

XLII. *Results of some Experiments on the actual Force of Draught of Carriages upon common Roads.* *By* B. Bevan, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE actual *force of draught* of carriages upon common roads has become a subject of interest and importance, as it relates to the principles of conveyance by rail-roads, and more so as it demonstrates the importance of attention to the surface of the road. I therefore take the liberty of sending you the results of some experiments of mine on that subject, conducted with considerable care in August 1824.

These experiments were all made, or reduced to roads perfectly level or horizontal, to separate the mechanical force due to the inclination of the hill or plane from the force necessary to overcome the friction of the carriage in its ordinary state as affected by the condition of the road; and by way of rendering them comparable with other experiments which have been or may yet be made on this subject, I have considered the gross load of the waggon and burden to be divided into 1000 parts.

Description of Road.	*Force of Draught.*
Loose sandy road	222
	240
	165
	163
	166
	190
	240
	215
	240
Mean	204 ...or $\frac{1}{5}$th.
Turnpike road new gravelled	121
	130
	180
Mean	143...or $\frac{1}{7}$th.
Ordinary bye-road	91
	121
Mean	106 nearly 1-9½.

Description of Road.	*Force of Draught.*
Hard compact loam	61
	36
	61
Mean	53 nearly $\frac{1}{19}$th.
Dry hard turf	40
	40
Mean	40...or $\frac{1}{25}$th.
Turnpike road with a little dirt	30
——— more dirt	39
Mean	34½...or $\frac{1}{29}$th.
Turnpike road free from dirt	30
	30
	31
Mean	30⅓ ...or $\frac{1}{33}$d.

From which it appears that *five* horses will draw with equal ease the same load upon a good hard turnpike road, as *thirty-three* horses can do upon loose sand! Or, if we assume the value of draught, upon a well-formed road in good condition, at sixpence per ton per mile, the equivalent price of draught will

		s.	d.
will be,	Upon hard turf	0	$7\frac{1}{4}$
	hard loam	0	$9\frac{1}{2}$
	ordinary bye-road	1	7
	newly gravelled road	2	2
	loose sandy road	3	1

I am, gentlemen, yours truly,

Leighton Bussard, 13th March, 1830. B. Bevan.

XLIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On the anatomy and physiology of the internal ear, by T. W. Chevalier, Esq.; communicated by Charles Bell, Esq. F.R.S.

The author denies the correctness of the commonly-received opinion, that sounds are modulated in their passage through the tympanum of the ear; and believes that the vibrations are transmitted without modification to a medullary substance, which he thinks may be regarded as a process of the brain itself. He refers, for the proof of this proposition, to a paper of which he is the writer, published in the 13th volume of the Medico-Chirurgical Transactions; and in which he endeavours to show that the *malleus* and the *incus* are so closely united by ligaments, as to preclude the possibility of their moving as levers upon each other. The author lays it down as a fundamental proposition, that every sound is characterized by three properties, which are quite distinct from one another. The first of these is its degree of loudness; the second its tone; and the third its quality or kind. He conceives that the ear is capable of effecting a mechanical separation of these three properties; and of distributing them on different portions of the organ,—without, however, destroying their physical unity: and he claims to himself the originality of the discovery of the several portions of the nerve of hearing on which these different properties of sound are respectively impressed. That part of the organ which is adapted to distinguish the loudness of sound he terms the *biameter;* that which conveys the perception of differences in tone he calls the *tonometer*; and thirdly, to that portion of the internal ear which is impressed by differences in the quality of sound, he applies the denomination of *poiometer.* He regards the cochlea as performing the function of biameter, viewing it as being essentially a conoidal tube, which is coiled into a spiral form merely for the sake of greater compactness and strength; for he observes, that in the ears of singing birds, where compactness is no object, the cochlea is a straight tube. In order to explain his view of the office of this part of the ear, he assumes it as a principle, that where a liquid is propelled through a conoidal tube, its pressure against the sides is inversely as the square of the area of a transverse section of the tube. This pressure, in the case of the scalæ of the cochlea, will be greatest at their apices. Hence, the impression of sound will be greatest at this part, and will diminish in regular gradation according as we trace the tubes from this part to their wider extremities;—so that

that the louder the sound, the greater is the extent of the scala cochleæ throughout which it is felt; an effect which will be still further augmented by the greater vascularity of the membrane of the scalæ as it approaches the cupola. The author conceives that the internal ear is protected from the injurious impression of very loud sounds by the action of the stapedius muscle, which totally intercepts their transmission by the ossicula to the membrane of the *fenestra rotunda,* and which is impressed upon a particular branch of the auditory nerve distributed upon that membrane. This view of the subject, he thinks, is corroborated by comparative anatomy; the base of the scala tympani being particularly developed in animals easily awakened by noises, as the cat, hare, and stag. The author ascribes to the mastoid cells more particularly the power of transmitting sounds through the bones of the head; and denies that any sonorous vibrations can take place in close cavities filled with elastic fluid.

The function of the auditory *tonometer* he assigns to certain medullary expansions, which he conceives he has discovered at the ampullular extremities of each of the semicircular canals. He is led to the belief, that the fluid in these canals is capable of a species of circulation, in consequence of the impulses received from the vibrations of the membrane of the *fenestra ovalis,* which is itself set in motion by the chain of ossicula. This he infers from the circumstance, that the common orifices of superior and posterior canals, and that of the exterior canals, are immediately opposite to the *fenestra ovalis* in the cavity of the vestibule, while their remote extremities are at the greatest possible distance from the direction of the original impulses given by the stapes. The perceptions of tone conveyed by the three semicircular canals in each ear coalesce in the mind into one perception; nevertheless there is an advantage in this triple organ, inasmuch as it may possibly be the means of our receiving perfect impressions from different sounds, whether they be concords or discords; and hence enabling us to perceive these qualities, for the perception of which the author does not see how a single organ could suffice. The writer, considering that, besides loudness and tone, sounds are also capable of being distinguished by some other qualities, thinks that these differences of quality may arise from different laws of vibration. He imagines a monochord, for example, may, when vibrating so as to occasion sound, perform its vibrations in very different modes of acceleration or retardation of its velocity, and impress these different modes of vibration on the air and other media by which the sound is transmitted to the ear. The organ for the perception of these differences he conceives to be a part of the vestibulum, which he styles the *poiometer,* and where he has discovered a cushion of medullary matter, over which the lining membrane of the vestibule is loosely extended, so as to be unsupported and depressed at its centre.

LINNÆAN SOCIETY.

March 2.—A. B. Lambert, Esq. V. P. in the chair:

A paper was read On the Botany of the Netherlands. By Joseph Woods, Esq. F.L.S.

March 16.— On this evening, (being the eve of St. Patrick,) Mr.

Mr. Bicheno, the Secretary, read a paper On the Plant intended by the Shamrock of Ireland, in which he attempted to prove by botanical, historical, and etymological evidence, that the original plant was not the white clover, which is now employed as the national emblem. He stated that it would seem a condition at least suitable, if not necessary, to a national emblem, that it should be something familiar to the people, and familiar too, at that season when the national feast is celebrated. Thus the Welsh have given the *Leek* to St. David, being a favourite oleraceous herb, and the only green thing they could find on the 1st of March; the Scotch on the other hand, whose feast is in autumn, have adopted the *Thistle*. The white clover is not fully expanded on St. Patrick's day, and wild specimens of it could hardly be obtained at this season. Besides it was probably, nay almost certainly, a plant of uncommon occurrence in Ireland during its early history, having been introduced into that country in the middle of the seventeenth century, and made common by cultivation. He then referred to several old authors to prove that the *Shamrock* was eaten by the Irish, and to one who went over to Ireland in the sixteenth century, who says it was eaten and was a *sour* plant. The name also of Shamrock is common to several Trefoils, both in the Irish and Gaelic languages. Now clover could not have been eaten, and it is not sour. Taking therefore all the conditions requisite, they are only found in the Wood-sorrel, *Oxalis acetosella*. It is an early spring plant; it was, and is abundant in Ireland; it is a trefoil; it is called *Sham-rog* by the old herbalists; and it is sour: while its beauty might well entitle it to the distinction of being the national emblem. The substitution of one for the other has been occasioned by cultivation, which made the wood-sorrel less plentiful, and the Dutch clover abundant.

A paper was also read by Joshua Brookes, Esq. F.R.S. & L.S. On the remarkable formation of the Trachea of the Egyptian Tantalus. This communication was illustrated by specimens.

GEOLOGICAL SOCIETY.

Feb. 19.—At the Annual General Meeting of the Society, held this day, the President, Professor Sedgwick, delivered the following Address from the chair:—

GENTLEMEN,

You have heard the report of the Council on the general state of our Society, containing an account of its property and of its debts, of the several sums received and expended during the last year, and a careful estimate of all our resources for the current year. You have also heard a separate report, from a select Committee, respecting the various collections of our Museum, and the progress which has been made in their arrangement.

I cannot allow this opportunity to pass away without expressing my hearty concurrence in the sentiments recorded by the Committee, and my admiration of the talents exhibited by our Curator, in a task of no common difficulty and of almost incredible labour.

At the same time, I should ill express my own feelings and those of the Society, did I not on this occasion also acknowledge the great obligations we owe to several members of our Council, and especially to Mr. Greenough, who during many years has fully given to us the benefit of his time and talents, both in directing us to those sources from which our collection might be supplied, and in arranging systematically the various specimens accumulated from time to time in our cabinets.

One result has been obtained from the excellent stratigraphical arrangements of Mr. Lonsdale, which I had not myself anticipated; they not only place in an instructive point of view the excellencies, but also the defects of our collection: and it appears from the report of the Committee, that some of the suites of specimens intended to illustrate the secondary formations of England are eminently defective. It will be the endeavour of the Council by the exchange of duplicates, and by all other means within their reach, to fill up these chasms in the Museum: and in effecting this object they look forward to the friendly cooperation of provincial bodies, associated for purposes like our own, and, above all, to the zeal and generosity of our own Members.

You will perceive, Gentlemen, from the report of the Council, that in the general estimate of the receipts and expenditure of the current year, there is a balance of about ninety pounds against the Society. Even such a deficit as this would produce feelings of deep regret, were it an indication of any general falling off in our resources; but the fact admits of ready explanation without any such disheartening conclusion. Our annual income is decidedly on the increase; but our general funds have not yet entirely recovered from the drain upon them which took place when we came into the occupation of these apartments. And during this year, besides paying up heavy arrears, we have incurred an expense of more than six hundred pounds in the publication of our Memoirs. There is, however, now laid up in the cabinets of the Museum a literary stock amounting in value to not less than twelve hundred pounds; which, though but in a small degree available against the present claims upon the Society, must in the end be productive of a considerable return.

Of the merits of the several memoirs which compose our last publication I am not called upon to speak; but I may direct the attention of the Gentlemen present to the number and beauty of its embellishments: and I am happy to record the expression of my thanks to Mr. Broderip, for the care with which he has superintended every part of it during its passage through the press. That Gentleman now retires from the laborious duties of the office of Secretary, which, for four years, he has filled so greatly to our advantage: but I am well assured, that we may still look with confidence for a not less efficient, though perhaps less laborious, application of his talents and experience in the promotion of our best interests.

During the past year about fifty additional Fellows have been enrolled

rolled on the lists of the Society; and among them I rejoice to observe the names of some persons eminently distinguished in this country by their knowledge in the exact sciences; and of others to whom we shall hereafter look, not merely for general support, but for active cooperation in the field. We have also added seven to the number of our Foreign Members: and I need not tell the Gentlemen present, that our body is honoured by the addition of these persons to its list*: for they stand without exception in the foremost rank of those who, by a combination of great personal labour and great talents, have pushed beyond their former limits those branches of natural knowledge, for the advancement of which we are incorporated. At the head of this number I rejoice to see the name of an illustrious Personage who, amid the distracting duties attached to his exalted rank and commanding station, has found time for the successful cultivation of science, which he adorns by his high intellectual attainments, and urges on by the force of his example.

After placing before you these subjects of congratulation, it is my painful duty to record the loss of an old Member of this Society, who took a deep interest in its wellbeing and progress. By the death of Mr. Holme we have lost a man of rare simplicity of manners, who in a life of retirement pursued science for its own sake, without any alloy of selfish feeling, or any view to his emolument or fame. He was an admirable botanist; and after many years of application had acquired no ordinary skill in some difficult parts of mineral analysis. In one of the Papers in our last publication I have had repeated occasions of acknowledging my obligations to him.

France has lately been deprived, by the death of M. Vauquelin, of a man who for more than half a century devoted the efforts of his powerful mind to the promotion of physical truth; and we have to lament the loss of a name which has long decorated the list of our Foreign Members. A proper homage has been already paid, by the President of the Royal Society, to the memory of this illustrious person; whose labours, however great the light they shed on our department of natural history, were still more nearly connected with exact science.

Several of the Papers read at our meetings, between the last Anniversary and our separation for the summer, have through different channels already come before the public. It would have been well, that at least a part of them should have appeared in our Transactions. But our funds have not always admitted of a sufficiently rapid publication to meet the wishes of those authors especially who have most original matter to communicate. This is

* His Imperial Highness John Archduke of Austria; Dr. Ami Boué; Prof. Hausmann of Göttingen; Prof. Hoffmann of Berlin; Prof. Voltz of Strasbourg; M. Dufrénoy, Professor at the Ecole des Mines, Paris; and M. D'Omalius D'Halloy, Governor of the Province of Namur in the Kingdom of the Netherlands.

a subject of regret, and well deserves the consideration of the Council for the coming year. The Transactions of the Society form unquestionably the most honourable official record of our labours. It is through them that we are represented in the great republic of science; and without them, beyond our own immediate circle, we possess neither voice nor animation.

The progress of our body in geological inquiry since the former Anniversary, will be best understood by glancing over the various memoirs which have been the subject of discussion at our meetings. It will be useless to do this in the exact order in which they came before us; I shall therefore follow that order in which the subjects themselves appear to be naturally connected with each other.

Our attention has been several times called to the theory of the excavation of valleys, and to the effects produced by river currents in modifying the form of the solid parts of the earth. The subject was introduced during the former year by a memoir of Messrs. Lyell and Murchison, on certain portions of the volcanic regions of Central France; in which they show (in accordance with the views of Montlosier, Scrope, and some other writers) that the existing rivers have, by a long continued erosion, eaten out deep gorges, not only through currents of basaltic lava which have flowed through the existing valleys, but also through solid rocks of subjacent gneiss. They further prove, on evidence which to me seems not short of demonstration, that no great denuding wave or mass of water lifted by supernatural force above its ordinary level, could have assisted in forming such denudations: for the country is still studded with domes of incoherent matter, the remnants of former craters; from which may be traced, continuously, streams of lava, intersected in the courses of the rivers by these deep gorges—the gages and tests of the erosive power of running water during times comparatively recent.

The elaborate Paper of Mr. Conybeare on the valley of the Thames is still fresh in our recollection. He proves that the erosive power of the river has, within the records of history, produced no effect on the general features of the country through which it flows, and that the propelling force of its waters is not now, and never could have been, adequate to the transport of the boulders which lie scattered on the sides and summits of the chains of hills through which it has found a passage: that much of the waterworn gravel, which has been drifted through the breaches opened in the sinuous line of its channel, is composed of rocks not found within the limits of its basin; and that the form of the country is often the very reverse of that which would have been produced by mere fluviatile erosion, however long continued. Similar facts are supplied by nearly all the greater valleys of England; and on the whole they point to one conclusion, that fluviatile erosion, as a mere solitary agent, has produced but small effects in modifying the prominent features of our island: at the same time they leave untouched all the facts of an opposite kind, supported by direct evidence, whether derived from the volcanic districts of Central France,

France, or from any other physical region on the surface of the earth.

The power of mountain torrents in transporting heavy masses of stone is strikingly illustrated in a short paper by Mr. Culley. He states that a small rivulet, descending from the Cheviot Hills along a moderate declivity, carried down, during a single flood, many thousand tons of gravel into the plains below; and that several blocks, from one half to three-quarters of a ton weight each, were propelled two miles in the direction of the stream. Facts, similar in kind, but on a scale incomparably greater, must be in the recollection of every one who has seen the Alpine torrents descending into the plains of the north of Italy.

When mountain chains abut in the sea, the laws of degradation are not suspended. At each successive flood, fragments of rock are drifted in the direction of the descending torrents, and rolled beneath the waters. This kind of action is indeed casual and interrupted; but it is aided by another action which is liable to no intermission—the beating of the surf and the grinding of the tidal currents on all the projecting parts of a steep and rocky shore. Under such conditions, I doubt not that there are now forming at the bottom of the sea, and at depths perhaps inaccessible, alternating masses of silt, and sand, and gravel, which, if ever lifted above the waters, may rival in magnitude some of the conglomerates of our older formations.

Our last Paper, on the excavating power of rivers, was from the pen of Mr. Scrope. He contends that diluvial torrents would only form trough-shaped channels, extending in the direction of the principal rush of water; but would never produce curves in which the excavating force worked in a direction opposed to that of the general current. He describes part of the course of the Moselle and of the Meuse, where the rivers wind through hard transition rocks, in long sinuous channels, varying in depth from 500 to 1000 feet. In one of the great flexures of the Moselle, the river, after passing over no less than 17 miles, returns to within 500 yards of the point from which it started. These phænomena are regarded by the Author as sure indications of slow fluviatile erosion. For he considers the idea of a great debacle, or diluvial current, winding its way back in lazy flexures towards the point from which it started, as absolutely unintelligible.

If I might give my own opinion on this debated question, I should say, that the existing river drainage of our physical region, is a complex result, depending upon many conditions—the time when the region first became dry land—its external form at the time of its first elevation above the sea—and all the successive disturbing forces which have since acted upon its surface. But none of these elements are constant: no wonder, then, that results derived from distant parts of the earth should be so greatly in conflict with each other. In the formation of valleys there is therefore little wisdom in attributing every thing to the action of one modifying

fying cause. We know by direct geological evidence, that nearly all the solid portions of the earth were once under the sea, and were lifted to their present elevation, not at one time, but during many distinct periods. We know that elevating forces have not only acted in different places at different times, but with such variations of intensity, that the same formation is in one country horizontal, in another vertical; in one country occupies the plains, in another is only found at the tops of the highest mountains. Now every great irregular elevation of the land (independently of all other results) must have produced, not merely a rush of the retiring waters of the sea, but a destruction of equilibrium among the waters of inland drainage. Effects like these must have been followed by changes in the channels of rivers, by the bursting of lakes, by great debacles, and in short by all the great phænomena of denudation. In comparing distant parts of the earth, we may therefore affirm that the periods of denudation do not belong to one, but to many successive epochs. And by parity of reasoning we may conclude that the great masses of incoherent matter which lie scattered over so many parts of the surface of the earth, belong also to successive epochs, and partake of the same complexity of formation.

The excavation of valleys seems therefore to be a complex result, depending upon all the forces, which, acting on the surface of the earth, since it rose above the waters, have fashioned it into its present form. We have old oceanic valleys which were formed at the bottom of the sea in times anterior to the elevation of our continents. Such is the great valley of the Caledonian canal, which existed nearly in its present form at a period anterior to the conglomerates of the old red sandstone. We have longitudinal valleys formed along the line of junction of two contiguous formations, simply by the elevation of their beds. To this class belong some of the great longitudinal valleys of the Alps. We have other valleys of more complex origin; where the beds through which the waters now pass have been bent and fractured with an inverted dip at the period of their elevation. Such is the valley of Kingsclere, described in a former volume by Dr. Buckland. We have valleys of disruption, marking the direction of cracks and fissures, produced by great upheaving forces. Such are some of the great transverse valleys of the Alps. Of valleys of denudation our island offers a countless number. Some are of simple origin: for example, the dry combes and valleys of the chalk, which appear to have been swept out by one flood of retiring waters during some period of elevation. Others are of complex origin, and are referrible to many periods, and to several independent causes. Lastly, we have valleys of simple erosion: such are some of the deep gorges and river channels in the high regions of Auvergne, excavated solely by the long continued attrition of the rivers which still flow through them.

I should not have dwelt so long upon this subject, had it not occupied a large portion of our attention during the past year; and I may

may be pardoned for entering a record of my own views on a question of no small complexity, and on which there is still much contrariety of opinion.

During the past year we have been presented with several memoirs describing formations superior to the chalk: which I shall also notice in the order of the subjects, without any regard to the time when they came before us.—In a Paper by Dr. Fitton on the structure of a portion of the low countries in the north of France, among other interesting details, is a description of three of this great class of formations. He points out deposits in the neighbourhood of Calais, Antwerp, and Tongres, which resemble the Crag of Suffolk. He compares the sands of St. Omer, Cassel, and Lille, with the sands which overlie the chalk in the London basin: and he states that the arenaceous beds of the hill of Cassel (like similar beds at Brussels) contain large suites of fossils, generally agreeing with those of the London clay. Lastly, he describes in detail the structure of St. Peter's Mount near Maestricht, and shows that the inferior beds form a gradual passage into the white chalk on which they rest; while the upper beds bear marks of degradation and mechanical interruption, and offer no indication of a passage into the superior sands. And he adds that, out of more than fifty species of organic remains collected by himself from this deposit, not more than ten are found in our best catalogues of chalk fossils.

I may here remark, that the suite of fossils in the Cassel sands throws no difficulty in the way of their comparison with the lower tertiary sands and plastic clay of England. The terms London clay and Plastic clay may be preserved as convenient mineralogical designations. They mark, however, nothing more than the subdivisions of one great deposit between the lower and the higher members, in which there is no line of zoological separation. In the London and Paris basins, there is a great chasm in the order of succession between the tertiary and secondary systems, which the labours of Geologists may in time enable us to fill up.—The Maestricht beds are so nearly related to the formation on which they rest, that they may be regarded as the last term of a new series of deposits, which we hope hereafter to find interpolated between the *calcaire grossier* and the chalk.

A Paper by Mr. Murchison makes us acquainted with the structure of the tertiary formations on the southern flank of the Alps between the Brenta and the Piave. They are divided into two great natural groups exhibited in two zones:—an outer zone containing shells which seem to be nearly identical with the well known fossils of the newer tertiary Sub-Apennine formations;—an inner and inferior zone containing in its higher portions a few shells resembling those of a part of the Bourdeaux basin, while its lower beds are distinguished by innumerable organic remains, more than half of which seem to be specifically identical with those of the *calcaire grossier* or London clay. These lower beds on the banks of the Brenta are inclined at 70° or 80°, and are based upon a nummulite rock, which is absolutely vertical, and conformable to the *scaglia* (containing ammonites

and

and belemnites), and together with it rises into peaks of considerable height on the extreme border of the chain: and there is no conglomerate or other mechanical degradation of the older rocks, to mark the junction of the secondary and tertiary systems. Some notion may be formed of the enormous thickness of these deposits from the statement, that a transverse section (from Asolo to Possagno) through beds of only a part of this series, inclined at various angles from 25° to 40° and exhibiting no invertions of dip, is not less than five miles in length. One important consequence seems to follow inevitably from these details: the last epoch of elevation of the neighbouring mountains must have commmenced during a period posterior to the tertiary formations described in this memoir.

In three Papers, recently presented by Mr. Murchison and myself to this Society, we have endeavoured to establish a series of similar conclusions, by induction from the phænomena observed on the flanks of the Salzburg and Bavarian Alps. I will not give you any analysis of details, which have so lately been the subject of discussion in this room. I may, however, briefly recall your attention to the results, which we consider best established and of most importance. We have shown that several transverse sections from the central axis of the Alps to the basin of the Upper Danube would present a succession of phænomena in very near accordance with those of other transverse sections from the same axis to the tertiary formations at the other base of the chain in the north of Italy. On both sides of this chain, after passing over the great secondary calcareous zones, we meet with the lower tertiary strata,—always highly inclined, sometimes vertical, and occasionally conformable to the beds of the older system. We contend that this remarkable symmetry confirms the hypothesis of a recent elevation of the Eastern Alps; and makes it probable, independently of arguments derived from organic remains, that the tertiary deposits of the Sub-Apennine regions and of the basin of the Upper Danube belong to one period of formation.

Thick masses of strata full of organic remains, and often occurring at low levels near the northern foot of the chain, are sometimes also found (e. g. in the valley of Gosau) in unconformable positions, caught up among the serrated peaks of the Alps, four or five thousand feet above the level of the sea. Such a disjunction of corresponding strata (and I may observe that the argument bears not upon their exact age), is inexplicable on any hypothesis which rejects the theory of elevation. We have concluded, chiefly on zoological evidence, that the unconformable beds of Gosau are more recent than the chalk. We believe that they contain neither ammonites nor belemnites, nor any other known species of secondary fossils; and on the whole we regard them as a term of that unknown series of formations which may hereafter close up the chasm between the lowest beds of the Paris basin and the chalk.

We have pointed out the limits of the old chain of the Salzburg and Bavarian Alps, and traced the direction of its valleys anterior to the tertiary epoch: and we have described a great deposit of lignite far up the valley of the Inn, containing freshwater and marine shells,

shells, which seem to connect it with the period of the London clay. We have further shown, that there are within the basin of the Upper Danube two or three higher zones of lignite separated from each other by sedimentary deposits of enormous thickness.

The tertiary system of Bavaria is shown to pass into, and to be identical with, the *molasse* and *nagelflue* of Switzerland. The higher part of this series must therefore (on the system of M. Studer) be of the same age with some of the formations of the Sub-Apennines. We have proved that enormous masses of sandstone and conglomerate many thousand feet in thickness, stretching from the base of the Alps to the plains of the Danube, are chiefly derived from the degradation of the neighbouring chain—that many of these masses cannot be distinguished from the newest detritus which lies scattered on the surface of the earth—that in their prolongation into Switzerland they sometimes contain bones of mammalia—that they are regularly stratified, and alternate with beds containing marine shells—and that they cannot have been caused by any transient inundation.

Finally, we point out the probable effect of debacles which took place when the basin was deserted by the sea. We show that the excavations produced by the retiring waters have been augmented by the bursting of successive lakes, of which we found traces in all the upland valleys of Bavaria; and that these excavations have been since carried on by the erosive power of the streams which roll down from the sides of the Alps to the plains of the Danube.

The greatest number of tertiary formations hitherto described appear to have been produced either in estuaries or mediterranean seas; the depth of which, however considerable, was probably much less than that of the wider oceans, wherein some of our secondary rocks have had their origin. These circumstances tend to explain the frequent alternations of marine and freshwater beds in the tertiary seas; and they satisfactorily account for the appearance of land shells, lignite, and other terrestrial remains, drifted, at many different periods, into the regular marine deposits of the tertiary groups. By the help of these alternations are certain species of marine and freshwater shells demonstratively shown to have been contemporaneous. And when this conclusion is once established, it may be applied to determine the age of those lacustrine formations which have never communicated with the sea.

In this way it has been shown that the enormous lacustrine deposits of Aix in Provence, of the Cantal, of the Limagne d'Auvergne, and of other districts in the south of France, belong to the period of the great tertiary system of the Paris basin. I have no time even to allude to the important works connected with these subjects, which we owe to the naturalists of France: and the two Memoirs of Messrs. Lyell and Murchison, "On the tertiary deposit of the Cantal," and "On the freshwater formations of Aix in Provence," have been already published*. I am not, therefore, called upon to give any regular analysis of their contents. I may, however, be permitted to recall

* See Edinburgh New Phil. Journal, Oct. 1829; and *Annales des Sciences Naturelles*, vol. xviii. p. 172.

your attention to the enormous thickness of a regular succession of deposits described by these gentlemen in a section extending from the hills above Aix to the coal works of Fuveau. We have at the base of the section a great system of alternating beds of limestone and shale containing many seams of coal, some of which are worked by perpendicular shafts 500 feet in depth. Over this succession of beds, come vast groups of strata forming ranges of hills composed of limestone, shale and sandstone. These are surmounted by thick deposits of red marl and fibrous gypsum, and by vast masses of conglomerate. Finally, over the conglomerate comes a series of beds conforming to the more ordinary tertiary type; remarkable for the regularity of their deposition, and for the beautiful preservation of the shells, the fishes, and even the insects contained in them. Such are the mineralogical characters of the lower members of this great series, that they have been referred (even by expert naturalists, who had not sufficiently examined the organic remains) to the old coal formation and the new red sandstone; but from top to bottom their fossils are exclusively tertiary and lacustrine. At the same time we attempt in vain, by joining in imagination the prominent elevations of the older rocks in the neighbouring regions, to restore the former barriers once containing that great body of water within which these deposits had their origin.

The Paper on the Cantal brought before us a series of facts no less striking and impressive. In this high region are the escarpments of an old lacustrine formation, nearly 500 feet in thickness, full of freshwater shells, many specifically identical with fossils of the basins of Paris and of the Isle of Wight: but here, as in the former case, there are no barriers to mark the limits of the lake within which this deposit was once confined. The same region also bears the impress of another succession of phenomena; for within the area of this ancient lake, and after the solidification of the beds of marl formed in its waters, burst forth one of those great trachytic eruptions which mark all the neighbouring parts of France. So that we now find beds of basalt, trachytic breccia, and other old volcanic rocks, overtopping, on the side of one valley, by more than 800 feet, the highest lacustrine rocks through which they have breached a passage to the surface of the earth: and in the neighbouring region the same old volcanic rocks have risen to several times that elevation.

When we examine the upper rock marl of the Isle of Wight, we see a deposit separated from us and the things about us, only by a few feet of transported gravel. The outline of the country might have been remodified, and the gravel formed by some transient inundation. We have therefore no measure of the time which may have elapsed since the first existence of the phenomena before us. If, however, we examine the shells in the rock marl, we find that few, if any, belong to species existing in our lakes or rivers. We cannot believe that there is so great a violation of continuity in the forms of animated nature, except in subordination to nature's laws; and we feel almost forced to seek for a solution of our difficulties amidst the ideal revolutions of former ages.

But

But how differently is the history of the same great period told off among the volcanic mountains of the Cantal and Auvergne! Great lacustrine formations, of the same age with the rock-marl of the Isle of Wight, are there proved by their organic contents to have been formed and solidified at a time anterior to the trachytic eruptions which upheaved and desolated the whole surface of the country. How long these great eruptive forces were in action, it is useless to conjecture; but they were followed by ages of repose, during which the surface of the land was reformed, and deep valleys were excavated by the erosive power of water. A new period of volcanic agency succeeded, marked by domes of cinders and scoriæ, remaining to this day almost unchanged, and by streams of lava, which may be traced from them into the existing valleys. And even these last operations, however recent in the order of geological events, were anterior to the records of history; so that we can still only approximate to their date, by a careful comparison of the effects since produced upon these streams of lava by the destructive power of the elements.

A description by Mr. Murchison of the lacustrine strata and fossils of Œningen is the last communication connected with tertiary formations, which I am called upon to notice. He shows that this deposit consists of horizontal beds of a considerable aggregate thickness, laid bare in quarries on the side, and near the summit of a ridge of hills, the base of which is washed by the waters of the Rhine—that they do not alternate with the *molasse* but repose upon it unconformably—and that from top to bottom they are of freshwater origin. He enumerates in detail a great variety of fossils (such as insects, plants, shells, fishes, tortoises, and mammalia,) discovered at different times in these quarries; and he adds a description (from the pen of Mr. Mantell) of a fossil fox not to be distinguished from the *Vulpes communis*, found in the middle beds of this system. From all these geological details, as well as from the position of the strata, he concludes that they belong to a very recent tertiary period. At the same time, the waters of the Rhine descend from the lake of Constance at a level no less than 600 feet below that of the old lake, in which the Œningen beds originated; and there is not in the present outline of the country any indication of the surface over which they once extended.

Such, Gentlemen, have been the prominent subjects of discussion during our meetings of the past year. Before I proceed to other questions, let me express my thanks to Mr. Vernon, for the zeal with which he has investigated, and the fidelity with which he has described, a deep excavation at North Cliff in Yorkshire. Under the ancient gravel of the district are found regular deposits of river silt, containing bones of the mammoth, the horse, the urus, the rhinoceros, the wolf, the ox, and deer; mingled with thirteen species of land or lacustrine shells, absolutely identical with those now living in the neighbouring district. Phænomena like these have a tenfold interest, when regarded as the extreme link of a great chain, binding the present order of things to that of older periods,

periods, in which the existing forms of animated nature seem one after another to disappear.

Twenty years are not yet passed away since MM. Cuvier and Brongniart first published their researches on the geological structure of the Paris basin. The innumerable details exhibited in their various essays; the beautiful conclusions drawn from unexpected facts; the happy combination of mineralogical and zoological evidence; the proofs of successive revolutions, till then unheard of, in the physical history of the earth—all these things combined, not merely threw new light on a subject before involved in comparative darkness, but gave new powers and new means of induction to those who should in after times attempt any similar investigations.

Mankind are, however, dazzled and astonished by great discoveries, as well as guided and instructed: and for some years after the publication of these admirable works, the naturalists of various countries, whose attention had been so loudly called to the deposits above the chalk, saw in them only a repetition of what was already described, and of which the true type was in every case to be sought among the formations of the Paris basin. Investigations conducted in this spirit sometimes ended in disappointment. But this was not the spirit recommended in the incomparable Essay of Cuvier*; for after exhibiting the true method of geological induction, and describing the intense and almost tormenting interest with which he had followed out his own investigations, he points to the long series of deposits in the Sub-Apennine hills, and states his conviction that in them lies concealed the true secret of the last operations of the ocean.

Since that discourse was written, much has been done; but much more still remains to be done. It has been my pleasing task to place before you the labours of some of our own body in illustrating the recent geological periods in the history of the earth: by such details alone can we expect to comprehend the more intricate phænomena of still older periods, and to connect them with the great physical laws by which all matter is governed.

Considered in the most general point of view, without any regard to the lacustrine beds, which are perhaps local or accidental, the tertiary groups of the Paris basin may be described as a great complex system of deposits, belonging to one protracted zoological period; characterized by extinct genera of mammalia, and by innumerable marine shells; but affording very few species by which we can connect them either with the chalk, or with the formations of our neighbouring seas. Their position is therefore entirely insulated; and by what new links they may be connected with the physical events which went before them and followed after them, can only be determined by a long series of observations. I have already pointed out the source from which some of the older links may hereafter probably be supplied. Of the same palæotherian age, and in the same insulated position, are the tertiary deposits of

* See *Discours Préliminaire*, p. 112, 1st edition.

Hampshire,

Hampshire, and some of the great lignite formations in the north of Germany.

The next group of the tertiary system is ill defined, and still but imperfectly understood. Some members of it are seen on the banks of the Loire, and have formed the subject of a late important memoir by M. Desnoyers; and the same portion of the series is represented on the eastern coasts of England, by the beds of crag overlying the London clay. It contains, like the former division, the bones of many mammalia, some of extinct, and some probably of living species; but the remains of the extinct animals do not belong to the palæotheria of the older period, but to the mammoth, the rhinoceros, and other animals, of which the bones are found so constantly in the superficial gravel. To the fossil shells of this division the same observations may be applied: many belong to species which are unknown, and perhaps extinct; others cannot be distinguished from the living shells of the neighbouring seas.

A third division of the system may comprehend all the higher Sub-Apennine deposits; distinguished by the bones of mammalia in still greater abundance, and by the number and beauty of the fossil shells, many of which are of living species. It is of enormous thickness in some of the low regions at the base of the Apennines; and it probably extends over a considerable portion of the basin of the Danube, and over the plains beyond the eastern termination of the Alps. I have, however, no time, nor do I possess information, to give any detailed account of its distribution.

During the periods in which the two last tertiary groups were elaborated in the sea, there must have been deposited on the land, in caverns, in fissures, and in beds of superficial gravel, many bones of the same species of animals by which those groups are characterized: and during the same periods may have originated in inland lakes some of the deposits of which we now only see the traces in masses of lacustrine marl, found in various countries resting unconformably upon the older strata.

It is impossible with our present knowledge, to form even a conjecture respecting the subdivisions into which the whole tertiary series may finally be separated. I am only anxious, in the mere outline I am now attempting, to describe the successive groups above the chalk in terms the most general, and in divisions the most comprehensive; especially, as they appear in connexion with our labours of the past year.

I must, however, notice one more group in the succession of marine deposits, before I can complete the ascending series and reach the limits of history: the name *tertiary* cannot perhaps with propriety be applied to it, as the animal remains contained in it are almost exclusively of the species now living in the nearest seas. To this class we may refer certain shelly deposits in the West India Islands—on the shores of the Red Sea—and on various parts of the shores of Italy, Sicily, and Spain. Their position, as might be expected, is generally low. But near the focus of volcanic action they rise to more considerable elevations: in proof of which I need only

only state, that beds of shells are found on the mountains of Sicily three thousand feet above the level of the Mediterranean, and of the same species with those now living in its waters *.

Such are the steps by which we ascend through the divisions of the tertiary period. I need not, however, inform you that we can seldom determine their relations by the mere evidence of superposition. Most frequently they appear in detached masses, the age of which can only be known by their fossils. This kind of evidence is, however, sometimes brought before us in a manner at once the most complicated and the most conclusive. It is to the labours of MM. Deshayes, Basterot, and other expert naturalists, who are devoting their talents and time to the completion of great works on the organic forms of the several tertiary groups, that we must look for information, which in the end may give us the means of a safer and wider induction.

With the exception of an interesting notice by Dr. Buckland of the occurrence of agates in the dolomitic strata of the Mendip Hills, not a single memoir has been read before us during last year, on the mineralogical structure of any part of the British Isles. I do not mention this without regret; for while any part of the structure of this country is unexplored, we have left unfinished that task, to perform which was the first great object of our association. The work of Mr. Phillips on the strata and organic remains of the Yorkshire coast offers, however, a splendid contrast to this portion of our year's productions. The clearness of the descriptions, the accuracy of the sections, the figures of more than 400 fossils faithfully arranged according to their grouping in the formations between the new red sandstone and the chalk, combine to make it one of the most valuable and instructive Essays in our language.

Much, Gentlemen, remains to be done, before the structure of the various formations of the British Isles can safely be appealed to as one of those complete middle terms of comparison, by help of which the disjointed fragments of a former world may in imagination be reunited. Respecting the perplexing phænomena of the Crag beds on the coast of Suffolk, we are greatly deficient in information. The accounts of all our tertiary strata, however excellent at the time they were written, must be entirely remodelled. Even the history of the oolitic series (the boast of English geology, and the type to which foreign naturalists are attempting to conform some of their own secondary rocks) is defective. We know, in admirable detail, the formations near Bath. On the coast of Yorkshire Mr. Phillips has left us nothing to desire. But a promised Memoir on the beautiful phænomena near Weymouth, after many years of expectation, is still unwritten: and a detailed transverse section through the wide oolitic beds of Northamptonshire is among our most important desiderata.

* This important fact was communicated by Mr. Lyell; and is described by him in a work now in the press.

Something

Something is left to be done in illustrating the upper part of the new red sandstone. It is here that the poverty of our secondary rocks offers a striking contrast to the riches of the coeval rocks on the flanks of the Vosges and on the banks of the Neckar. And this very poverty makes every scrap of information, whether derived from mineralogical or organic characters, of importance in assisting us to complete this broken part of our secondary series.

Even the history of our coal formations is not yet perfect. The association of the coal and mountain limestone of Northumberland has not been well explained. The great corresponding deposits of Cumberland are undescribed: nor does it appear in our published works, that coal is found alternating in the North of England with all parts of the mountain limestone group; and that beds of coal are worked in several places, resting upon transition slate, and surmounted by the whole limestone series. More than half of Ireland is a blank on our geological maps: and on many of the transition districts of England our information is lamentably defective.

The study of our older deposits is indeed difficult and toilsome, and unenlivened with the frequent occurrence of organic bodies. But no country, hitherto described, shows a more splendid series of phænomena, to illustrate the intrusive agency of crystalline rocks; and to exhibit the great successive internal movements, by which our continents have been elevated, and brought under those laws of degradation which have fashioned them into their present forms. In these investigations there is still a rich spoil ready for any one who will have the courage to stretch out his hands to grasp it. A part of it I have myself gathered among the mountains of Cumberland, with no small labour; which I shall count for gain, if I may be permitted, hereafter, to lay it up in the storehouse of this Society.

Leaving, however, the subject of British geology, I must call your attention to those Papers which, during our sessions of the past year, have described the general phænomena of secondary rocks.—On the secondary formations of the Netherlands we have heard some interesting remarks in a recent Paper by Dr. Fitton, above quoted; in which he describes the structure and distribution of the chalk, the firestone, and the green and ferruginous sands; shows their discordant position over the coal-measures; and indicates the characters, both in which they differ and agree with the corresponding members of the English series.

In a Paper on the geology of the shores of the Gulf of Spezia, beautifully illustrated by sections and drawings, Mr. De la Beche describes a long series of stratified and unstratified rocks. Among the former may be enumerated, beds of clay sandstone and conglomerate, supposed to be tertiary; beds of *macigno;* the marble of Porto Venere; the crystalline limestone of Capo Corvo, &c. among the latter, diallage rock, serpentine, mica schist, &c. He endeavours to show, from the structure of the district, and the fossils of the neighbouring rocks, that the marble of Porto Venere may belong to the age of the oolitic series; and that the diallage rocks and

and serpentine are a prolongation of the system of southern Liguria, and have been protruded by igneous action among the depository rocks, after the period of the oolites.

Among the contributions to our knowledge of the structure of foreign secondary deposits, I must lastly notice the communication of Mr. Murchison on the bituminous schist and fossil fish of Seefeld. This singular rock rises to a great elevation among the bare calcareous peaks of the Tyrolian Alps, and contains such a quantity of bituminous matter, probably derived from the animals imbedded in it, that some of its strata are broken up and exposed to a process of distillation, by which a great quantity of what may be called mineral fish oil is extracted for economical use. Among the fossil fish M. Valenciennes of Paris discovered at least four species; one a *clupea*, and three distinguished by quadrangular scales, without articulating points, and resembling the *Esox osseus;* but differing from that genus, both in the form of the tail and the position of the fins.

There is a large family of fish, made up of many genera and species, and distributed from the old red sandstone to the magnesian limestone, which belong to the order *Malacopterygii abdominales*, and are particularly distinguished, like the *Esox osseus*, by a pointed tail, the lower side of which alone is supplied with rays. It is obvious from this description that the Seefeld fish are not comprehended in that family. And as they are not identified with the fossils of any known formation, we must consider their place as still undetermined. This is at least a safe conclusion: and mineralogical indications in the calcareous regions of the Alps are of very small value in determining the question.

During the past year, we have received from Dr. Buckland several additional notices, drawn up with his well known sagacity and singular felicity of illustration, on the characters and distribution of various specimens of coprolites. The results of his inquiries are published in the last Part of our Transactions; and on that account I am precluded from any further remarks upon them. They belong, indeed, to important discoveries of the former year, and have already been noticed in the Anniversary address of my predecessor in this chair.

From the same pen we have also a description of the bones of the Iguanodon and other large reptiles, discovered at Sandown Bay in the Isle of Wight, and near Swanwich in the Isle of Purbeck. In both localities the formation is the same with that of the sandstone of Tilgate Forest, in which Mr. Mantell first discovered the remains of the Iguanodon, an herbivorous reptile of extraordinary stature. Dr. Buckland describes an external metacarpal bone (six inches in length, five inches in its greatest breadth, and six pounds in weight) of the right foot of some reptile, supposed, from the stratum in which it is found in Sandown Bay, and from the bones with which it is associated, to be an Iguanodon. It is in linear dimensions twice as large as the corresponding bone of a large elephant:

elephant: and we must consider the small proportion which the legs of a reptile bear to the length of its body, in order to form any notion of the gigantic proportions of this quadruped.

Finally, I have to notice a communication from Mr. Hennah, containing a systematic and descriptive catalogue of the fossils of the transition limestone of Plymouth, read at our last meeting.

Such, Gentlemen, have been the memoirs presented to us since our former Anniversary. I have brought them before you in that order in which they seem to cast light upon each other; and I have indulged in no comments but such as sprang immediately from the subjects themselves.

I rejoice in the number and activity of our provincial institutions; and still more that the same spirit which has of late years induced so many Englishmen to combine for the furtherance of natural knowledge, is extending to our colonies in America and Asia. From the labours of so many ingenious men, united for the same end, and with opportunities for observation so widely different, the happiest results may be anticipated.

I should wish to say something on the general structure of the Alps; and to describe the speculations of one of our Foreign Members and best fellow-labourers on the different epochs of elevation. These are inviting topics, to which, on a future occasion, I may perhaps return: but had I even time for their discussion, it would not be well for me, at present, to trust myself in so wide a field.

Of the various works which, during the past year, have been poured out from the German and French press, on subjects connected with geology, it is impossible for me to offer an analysis, or even an enumeration. Most of them are the productions not only of great talent, but of great good sense; not only of great labour, but of labour happily directed. And it is no small matter of pride to this Society, that its researches have been highly valued by the naturalists of the Continent. They have not given their praises to us grudgingly; but have sometimes scattered them with a lavish hand; and have, I fear, awarded to us higher honours than we ourselves can be conscious of deserving. I think I could point out more than one Essay, in which, during the past year, the geologists of the Continent have injured their descriptions of secondary formations, and impeded their own inductive powers, by fixing their eyes too steadily on the types of the Enlgish series.

I congratulate you on the completion of the geological map of Germany, by an illustrious naturalist, who for many years has devoted, and continues still to devote, the best efforts of his life to the promotion of our science. He has not affixed his name to this great work, and he perhaps still regards some parts of it but as an approximation. The elaborate and accurate maps of north-western Germany by Professor Hoffmann, and of the Odenwald and the neighbouring districts by Dr. Klipstein, belong also to the productions of the past year *. Professor Hoffmann's map is to us of peculiar interest; not merely from the extent and intricacy of the country it delineates; but also from the number of secondary formations which it represents, in perfect conformity with the subdivisions adopted in our own geological maps. Works of this kind are of inestimable value: they are the embodied results of observations without number, directed to one object; and, when well performed, may be regarded as the last generalizations from facts exhibited in their clearest and simplest form. But more than this,—they guide us to the fountain-head of information, and lead us to still more general conclusions, by giving us at every step of our way the means of comparison with the structure of other regions †.

To some admirable works on natural history, now in progress, which bear more or less directly on our subject, I have no time to allude. But I may point, with peculiar satisfaction, to the advancement of the work of M. Adolphe Brongniart on fossil plants, and to the appearance of a new number of the work of Goldfuss on organic remains. By the continued labours of these excellent naturalists, we are supplied with new terms of geological comparison, and new means of legitimate induction. I am happy also to announce the approaching publication of a general index to the volumes of Mr. Sowerby's "Mineral Conchology," in which the errors incidental to such a work will be corrected, and all the fossils arranged according to their position in the successive groups of the British strata. Such an Index has long been wanted; and its execution will be an advantage above all price to the student of secondary geology.

Each succeeding year places in a stronger point of view the importance of organic remains, when we attempt to trace the various periods and revolutions in the history of the globe. Crystalline rocks are found associated with the strata of almost every age; and the constant laws of combination which have produced a certain mineral form in rocks of one era, may produce it again in another. Nearly all the modifications of structure in rocks called primary are also found in secondary formations: and among tertiary deposits we sometimes find millstone-grit, red marl with fibrous gypsum, red conglomerates, compact, subcrystalline, and oolitic limestone; in short, all the distinguishing characters of secondary formations. The great barriers, which the fancy or ingenuity of

* Dr. Klipstein has also executed a geological map (not, I believe, yet published) of the districts north of the Main; on the same scale, and of the same extent, with the Odenwald map.

† The geological maps of Germany are sold by Simon Schropp and Co. of Berlin. I take this opportunity of observing, that the difficulty of procuring copies of works like these has long been a matter of complaint. Of the excellent geological map, by MM. Oeynhausen, von Dechen, and De la Roche, though published in 1825, not a single copy has, I believe, yet found its way into the shops of any of our geographers. I only procured it myself at Berlin.

geologists

geologists has at different times set up between the mineral productions of successive periods, have been thrown down, one after the other. I do not deny the importance of mineralogical characters; I only mean to assert that, taken by themselves, they are no certain indications of the age of any deposit whatsoever.

In reasoning from organic remains, from the succession of large groups alone can we establish any safe induction. Positive rules founded on the presence of particular genera or species are of comparatively small value. But the mind becomes wearied and bewildered by the endless succession of individual forms, and delights to take refuge in some generalization: and generalizations would be excellent things if we could be persuaded to part with them as easily as we form them. They might then be used like the shifting hypotheses in certain operations of exact science, by help of which we gradually approximate nearer and nearer to the truth.

In England, and many other parts of the north of Europe, nummulites are only found in tertiary rocks, and orthoceratites only in those of the transition periods; but in the secondary limestone of the Alps we find, abundantly, both orthoceratites and nummulites. Ammonites and belemnites have not yet been found among the strata called tertiary. But should the chasm between the secondary and tertiary systems ever be filled up, it may be as difficult to draw any line between them, as it now is to draw the line between the transition and secondary series. Belemnites descend no lower than the lias. Ammonites descend among the transition rocks: and it has been remarked, that in all the deposits under the lias, the concamerations of this genus are of a simpler figure (being marked at their junction with the outer shell only by lines undulating or in zig-zag,) than those of the corresponding fossils in the higher formations. As far as regards the English carboniferous and transition series, this rule is true. But the only ammonite I ever found in the magnesian limestone had those suture-like markings which distinguish this genus in the upper secondary beds. The producta is not found above the magnesian limestone (zechstein): it occurs abundantly in the lower part of that formation, and it also abounds among the fossils of the transition periods. Certain plants are eminently characteristic of our coal formations; but in England they also occur in the sandstone beds, which alternate with the mountain limestone. Near Magdeburg they are found in grauwacké; and M. Elie de Beaumont has, on the south flank of the Alps, found the same vegetable forms in beds of the age of our lias. Positive and negative rules like these, when kept in subordination to new facts, are of the greatest value; for they record in a few words the result of many observations.

When we examine a series of formations which are in contact, we constantly find them passing into each other: and when we place the groups of fossils derived from the successive terms of the series in the order of superposition, their passage is still more striking. I do not mean by this to vindicate the transmutation of species; because that doctrine is opposed by all the facts of any value in determining such a question. Neither do I assume any positive law of continuity such as may be predicated of a formula in exact science. I only wish to state a fact of general observation. We sometimes, however, find that this order in the works of nature is interrupted; a leaf seems to be torn out from the volume of her history. At the same time all the connecting links, which bind the successive mineral masses to each other, are broken; and their separation is marked by contortions and disruptions, by heaps of conglomerate, and by all the other proofs of violent internal commotions. But these internal commotions have not been universal: and when we get beyond their operation, we recover the lost page in the history of the world, as it is told in the succession of animal forms, and every thing is again reduced to harmony and order. I do not intend to deny that there may have been certain great epochs of elevation, of such wide-spreading violence as to affect every living thing on the face of the earth. This is a mere question of fact, and to be resolved solely by observation. I only wish to vindicate a principle which we know from experience to be of very extensive application, and to which I have before alluded in this address. I may therefore again be permitted to enforce it by a specific illustration.

In many parts of the west of England, the lias is only separated from the coal measures by a few hundred feet of red sandstone and conglomerate, which do not contain the vestige of an organic fossil. It might be supposed (and such a supposition would not be new) —that the red sandstone and conglomerate were formed during some short period of confusion, produced by the dislocation of the older rocks—that after a time the sea again became tranquil—and that the fossils of the lias were called into being, upon the ruins of an older world, by a new fiat of creative power. Nor should I object much to such a hypothesis, if it were only regarded as a mere explanation of local phænomena. But the fossils of the coal measures bear no resemblance to the fossils of the lias. There is, therefore, such a break of continuity, that we are forced in imagination to supply many new groups of organic forms, before we can bring the order of succession into accordance with the known analogies of nature. If we continue our investigations to the north of England, we see the coal measures less disturbed, and the dolomitic conglomerates less developed. We find, at the same time, new divisions of the dolomites; some of which abound in organic remains, having a resemblance to the fossils of the carboniferous strata, and being in a few instances specifically the same with them. We also find among them many new species of organized beings. Still the sequence is incomplete; the fossils of the dolomitic beds make but little approach to the fossils of the lias: and no part of the British Isles has hitherto supplied us with the intervening terms of the series. But if we extend our inquiries to the secondary formations of Germany and France (particularly in the regions of the Vosges, or on the banks of the Neckar), we meet with a solution of our difficulties. In the place of our barren deposits, between the magnesian limestone

stone and the lias, we have three great formations, each characterized by its suite of fossils; and among them we find a series of zoophytes, and shells, and great reptiles, gradually leading us to the organic types of the lias and the oolites. In proof of what I am stating, I need only refer you to that part of our collection, which we owe to the liberality of M. Voltz, whose labours have thrown so great a light upon this interesting chapter of the physical history of the earth.

In this way, by successive but secure inductions, we resolve our first difficulty; and are no longer startled at the change of organic types, in the west of England, between the coal measures and the lias. For between the times of their deposition, there were completed at least five great geological periods; each distinguished by its own group of animals, and each, therefore, probably continued during a long succession of ages. I must, however, forbear: the subject is boundless; but our time allows not of further details.

It is, I think, a matter of regret that there have not appeared, from time to time, in our language, works placing clearly before the world the progress of geology, the laws of its induction, and the subjects of its speculations. Such works, however, demand more than common powers,—a grasp of details only acquired by practical experience; and habits of mind fitted for the exhibition of them, in their most simple and general form. But above all, they require a moral elevation, and a dignified forbearance, to free the mind from those attractive visions of ancient cosmogony, and those seductions of fanciful hypotheses, by which the history of geology has so often been degraded.

It is indeed true that an essay representing our science as it now is, must in a few years be left at a distance by the progress of new discoveries. At the same time, to no works in the history of physics do we revert with more pleasure and instruction, than to those which record the progress of discovery, and the early approximations to general truth. Their lessons of wisdom remain; and we look back to them with veneration, as to ancient monuments, which, however rude, or ill suited to the fashion of our day, still bear the stamp of the genius that produced them.

But, Gentlemen, if our science has not been adorned in this country so much as we might have wished by its monuments of wisdom, it has been disfigured by its monuments of folly. There have issued from the English press, within a few years, such dreams of cosmogony as I believe find no parallel in the recent literature of continental Europe. It would be in vain to point out to such authors the nature of our data, or the method of our inductions; for they have a safer and a readier road to their own conclusions. It would be in vain to tell them—that the records of mankind offer no single instance of any great physical truth anticipated by mere guesses and conjectures—that philosophic wisdom consists in comprehending the last generalizations derived from facts each of which is only known by experiment

and

and observation; and in advancing, by such means, to those general laws by which all things are bound together. They seem not to know that inventive power in physics, unlike inventive power in works of art or of imagination, finds no employment in ideal creations, and only means the faculty by which the mind clearly apprehends the relations and analogies of things already known; and is thereby directed and urged on to the discovery of new facts, by the help of new comparisons—that the history of all ages (and I might add, the written law of our being, where it is declared that by the sweat of our brow shall we gather up our harvest) has proved this way of slow and toilsome induction to be the only path which leads to physical truth.

Laws for the government of intellectual beings, and laws by which material things are held together, have not one common element to connect them. And to seek for an exposition of the phænomena of the natural world among the records of the moral destinies of mankind, would be as unwise, as to look for rules of moral government among the laws of chemical combination. From the unnatural union of things so utterly incongruous, there has from time to time sprung up in this country a deformed progeny of heretical and fantastical conclusions, by which sober philosophy has been put to open shame, and sometimes even the charities of life have been exposed to violation.

No opinion can be heretical but that which is not true. Conflicting falsehoods we can comprehend; but truths can never war against each other. I affirm, therefore, that we have nothing to fear from the results of our inquiries, provided they be followed in the laborious, but secure road of honest induction. In this way we may rest assured that we shall never arrive at conclusions opposed to any truth, either physical or moral, from whatsoever source that truth may be derived: nay rather (as in all truth there is a common essence), that new discoveries will ever lend support and illustration to things which are already known, by giving us a larger insight into the universal harmonies of nature.

Had the authors to whom I have alluded, contented themselves with pointing out the errors of our logic, and the fallacies of our induction, they might, perhaps, have done us some service. For it cannot be denied that we have sometimes lost ourselves amidst the strange forms of nature which have started up before us, during our wanderings among the monuments of an older world: and in the records of our labours, a critical eye may perhaps sometimes discover that the modesty of our facts is but ill assorted with the boldness of our conclusions.

I should have been well content to have ended with these general censures. But during the past year there has been sent forth, by one of our own body, "a New System of Geology, in which the great revolutions of the earth and of animated nature are reconciled at once to modern science and to sacred history:" and to this title I will venture to add,—in which the worst violations of philosophic rule, by the daring union of things incongruous, have been adopted by the author from others, and at the same time decorated by new fantasies of his own. I shall not stop to combat the bold and unauthorized

rized hypothesis, that all the successive formations of the old schistose rocks were called into being simultaneously by a fiat of creative power anterior to the existence of creatures possessing life: nor shall I urge, that among these primitive creations of the author, are mountain masses of rock formed by mechanical degradation from rocks which preceded them, and beds of organic remains,—placed there, if we may believe his system, in mere mockery of our senses;—neither shall I detain you by dwelling upon the errors and contradictions which are scattered through the early pages of his volume. On this part of the "New System" all criticism is uncalled for here; for it soars far above us and our lowly contemplations. Its character is written, and its very physiognomy appears in that dignified and oracular censure which he himself has quoted from the works of Bacon: "Tanto magis hæc vanitas inhibenda venit et coercenda, quia ex divinorum et humanorum male-sanâ admixtione, non solum educitur philosophia phantastica, sed etiam religio hæretica." "This vanity merits castigation and reproof the more, as from the mischievous admixture of divine and human things, there is compounded at once a fantastical philosophy and an heretical religion."

All these things, Gentlemen, I shall pass over: but the author has stood forward as the popular expositor of the present state of secondary geology; of that very portion of our science, which has for so many years employed the best efforts of our Society. This part of the work appears not to contain one original fact, or the result of one original investigation: and of this we do not complain. We have, however, a right to look to it for information, which shall not repeat exploded errors; but shall make a near approach to the level of recent observations. But is this the case in the work before us? Unquestionably not. All the old errors in the arrangement of the English strata, between the chalk and the oolites, are unaccountably repeated;—errors which have been corrected since 1824, in our Transactions, in English and Scotch philosophical journals, and in various independent works of natural history; and have excited, during the last five or six years, more discussions in this room than have arisen out of any other part of secondary geology. Other antiquated errors, of like kind, have found a place of refuge in the pages of this "New System."

But let us pass over what may be, perhaps, only regarded as errors of omission, and see how the author has employed the materials before him. The best part of his narrative is made up of successive extracts, often taken word for word, yet without the marks of quotation, from various well-known works on geology. Many of these extracts, although in themselves admirable, appear in the book before us but as disjointed fragments, in the arrangement of which the author has but ill performed the humble duties of a compiler. For in the chapter on secondary formations, we find enormous faults and dislocations, of which there is neither any written record, nor any archetype in the book of Nature. Thus we find the lias sometimes below the oolites, sometimes between the oolites and the green-

green-sand*. In one page the cornbrash and forest marble have shifted places; in another the whole lower oolitic system is absolutely inverted†. Again, at p. 247, we are told that the several beds are given "as usual, in the ascending order;" yet in this very page the inferior members of the lower oolites are copied, word for word, from another book, and are in the descending order. On the next leaf, the same error is repeated in a still worse form: and within four pages of this last *bouleversement* we find the Oxford clay, the cornbrash, and the forest marble, twice shuffled under the great oolite‡. The goodly pile, Gentlemen, which many of you have helped to rear, after years of labour, has been pulled down and reconstructed: but with such unskilful hands that its inscriptions are turned upside down; its sculptured figures have their heads to the ground, and their heels to the heavens; and the whole fabric, amid the fantastic ornaments by which it is degraded, has lost all the beauty and the harmony of its old proportions.

So much has been written in illustration of the zoological history of our several formations, that the labour of a compiler is now made comparatively easy. Yet in the distribution of organic remains, given in the "New System," there is such a complication of errors as nearly baffles all attempts at description. In one place we are told, that the lower secondary rocks are characterized by the simplest forms of the animal kingdom. In another, we find fish enumerated among the fossils of the transition (or submedial) strata§. In one place our magnesian limestone is properly identified with the first flötz limestone of Werner. In another, our mountain limestone is placed on the same parallel; and, by a double blunder, is described "as the lowest sepulchre of vertebral animals ||."

In one page orthoceratites are brought near the order of corals. In another, a coral is figured as an encrinite. In a third, the Steeple Ashton caryophyllia (the characteristic fossil of the middle oolite), is figured as a fossil of the inferior system. In a fourth, a caryophyllia of the mountain limestone is figured among the organic remains of the cornbrash. And lastly, the celebrated lily encrinite (a characteristic fossil of the muschel-kalk, a formation unknown in England) is introduced and figured among the fossils of the lower oolitic system ¶.

Errors like these are above every thing calculated to mislead men who are unpractised in geology; and they do not terminate here. But I have no right to detain you with a longer enumeration **. I have stated enough to prove, that in the conduct of this work, the author

* "New System of Geology." Compare pp. 133, 153 with pp. 137, 197.

† "New System," pp. 187, 195.

‡ *Ibid.* p. 253.

§ Compare Introduction, p. xlix. and p. 143.

|| "New System," pp. 175, 177, 187.

¶ See pp. 149, 176, 251, 256, 257.

** For the purpose of illustrating the organic remains "of the successive mineral strata," there are at the end of the "New System" five plates representing groups of fossils, with their generic and specific names. Had the author has shown neither the information nor the industry which might justify him in becoming an interpreter of the labours of others, or the framer of a system of his own.

Are we then for ever to wander among the mere perplexities of details, and never to hope for any system by which we may combine them? You must have seen, Gentlemen, that I am not the advocate of any such steril sentiment. It is indeed true that in the very classification of our facts and of our phænomena, there are difficulties connected with all parts of natural history, which, for ages yet to come, may continue to require for their solution a combination of the greatest industry with the greatest skill. But these difficulties do honour to our science: and the same great rule by which the father of physical astronomy was guided, applies, at every step, to us and to our conclusions. "Effectuum naturalium ejusdem generis eædem sunt causæ," was the grand rule on which he founded his induction. In the same way, we see the effects pro-

the figures been well selected, they might have been of great use: as it is, they can only be the means of disseminating error.

Plate I. professes to represent the "shells of the mountain limestone." Of its thirteen figures three or four are well chosen; none of the rest ought to have appeared. One of them is wrong named; and a recent nerita, with all its fresh markings, has unaccountably found its place among these old fossils.

Plate II. "Shells of the Lias." In this plate, of twelve species, we are astonished to find a transition orthoceratite, the productus scoticus of the mountain limestone, and a scaphite of the green-sand, placed, side by side, with the gryphæa incurva, plagiostoma gigas, and some other true lias fossils!

Plate III. "Shells of the under Oolite." Thirteen species; and a more uncharacteristic assemblage was, perhaps, never before brought together. A tertiary mya and a nummulite have here found their way, for the first time, among the shells of the under oolite. Two or three of the other species ought to have appeared, if at all, in the next plate.

Plate IV. "Shells of the Cornbrash and upper Oolites." Here the confusion is still greater; for of twelve species, seven are positively misplaced, the others are ill selected, and one of them is wrong named. The mineral conchologist is confounded at the sight of the well known turrilites and hamites of the green-sand group, of the turritellæ and superb rostellaria macroptera of the London clay, jostled in among the fossils of the oolites. Had the author drawn out by lot, from all the fossils in Mr. Sowerby's work, the species which were to decorate this plate, chance might have given him a more illustrative series.

Plate V. "Shells of the Chalk and Superior Strata." Among the nineteen figures of this plate, no attempt is made to separate the shells of the chalk from those of the overlying tertiary deposits; although the two groups have not perhaps one species in common. In Plate I. two freshwater shells were introduced which were not characteristic; here freshwater shells are characteristic, but are omitted altogether; and the pecten quinquecostatus is the characteristic fossil of the green-sand.

One who was even moderately acquainted with the characteristic forms of organic remains, could never have been led into such a complication of errors: and they are the more discreditable, as the greater part of them might have been avoided by the mere exercise of the humblest duty of a compiler.

duced by the action of material things upon each other: and we know that the laws by which these material things are governed, are liable neither to change nor intermission. There is, therefore, one safe rule in all our inquiries, whether they be simple or complicated. Effects similar in kind to those which are produced now, must in all former times have been produced by some corresponding power of nature.

As the historians of the natural world, we can describe the order of the events which are past; and we can trace a succession of revolutions through which we go back, till we arrive at periods where the characters of nature's work are all obliterated, and there our descriptions end. Like things we can compare with like; and this comparison teaches us the analogies of the forms which we examine: but we define not the length of time during which they were elaborated; and still less do we dare to speculate about the physical revolutions of the ages which are to come.

The very commencement of the task of speculative geology requires a wide and philosophic knowledge of the physical world as it now is, and of all the great phænomena exhibited by the fragments of its former history. A mind so prepared has already within its grasp the means of a large induction: and our science, though hardly yet come out of its cradle, has supplied materials of thought for intellects the most robust, and results to satisfy imaginations the most ardent. Let us, therefore, go on as we have begun; giving up our best efforts to the search of new facts and of new phænomena, and using them like men who have no higher passion than the love of truth.

The greatest problems of astronomy are simple in their conditions. A few physical points moving in free space, with given velocities, in given directions, and acting upon each other in subordination to a given law,—these constitute the chief data for the mathematical analysis of the system of the heavens. And the results are of a corresponding simplicity. The phænomena of the heavens are demonstratively proved to recur in a fixed order, after the lapse of fixed periods of time; and the apparent aberrations from the general law are also proved to be but modifications of that law, and to return into themselves after the completion of definite secular periods. But where are the secular periods of geology, and where are its cycles of phænomena recurring, again and again, in a certain order? I must confess that I cannot discover even the traces of them; and I think we do injustice to our subject, in bringing it too nearly into comparison with the exacter sciences.

The earth has been brought into its present form by countless causes, of which we know nothing—by corpuscular and chemical action, varied by changes of temperature, of pressure, and of all other external conditions—by the violence of volcanic forces, called into being by unknown powers of nature, and at unknown intervals of time—by all the combined effects of mechanical degradation—and by all the endless modifications of matter, resulting from beings possessing the organs of life. These conditions are infinitely too complex and ill defined to come within the grasp of any exact analysis.

I believe

I believe therefore that our subject will never be so far abstracted from the materials which weigh it down, as to rise to the rank of an exact science. But this, at least, I will dare to predict; that so long as we are of one mind, and animated by our present spirit, year after year, we shall find new fields for investigation, and new grounds for rational induction. That which is exact in science must be circumscribed and defined: but of our labours we have no power to foresee the limits; and there is an intense and poetic interest in the very uncertainty and boundlessness of our speculations.

It is no small advantage that our studies are so large and so various, that they not only carry us into all the kingdoms of nature, but have a direct bearing on the business of life. Of their economical importance, I have, however, now no time to speak; and I would rather conclude by reminding you of their importance in all questions of physical geography, to which they are as essential as anatomy to the sculptor, or the knowledge of ancient tongues to the decipherer of ancient monuments—of the light they have shed on every branch of natural history—and of the problems they have suggested to the investigations of exact science. Our field is indeed so large, and our physical problems of such complexity, that we find at every step, how much we stand in need of the support of our fellow-labourers; and this feeling has produced a strong social sympathy, not merely among us, but among the geologists of all the nations of Europe. And it is to this principle that I am willing to attribute a part of the great excitement which has hitherto carried us on, and of those youthful and lusty efforts, which are the best indications both of our physical and of our moral health.

And now, Gentlemen, after having detained you so long, allow me to express my gratitude for the kind assistance which I have received from you in discharging all the duties of my office during the past year. And if your lives and mine should be spared till another Anniversary, I hope to have the delightful task of recounting to you the still more extended labours of our body, and of rejoicing with you at the gathering in of a still richer harvest.

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

Feb. 26.—Mr. Watson gave an account of the means of preventing ships from foundering at sea. He illustrated his proposed method by the use of numerous models, and by experimental illustrations of the actual floatage or buoyant power of certain bulks of different kinds of wood used in ship building. The principle which he adopts, and upon which every thing rests, is the buoyant power of tight copper tubes filled with air and built in with the vessel in the various convenient situations afforded in the lower part, as between the timbers, under the decks, &c. He illustrated his calculations by a model of an 80-gun ship, and then drew the results as to expense, saving, &c. upon the same scale. His conclusion was, that the saving of property and lives would be very great, and the adaptation very practicable.

 A micro-

A microscope constructed for Capt. Grover, and the first containing the application of Dr. Wollaston's doublet, was exhibited in the library. It performed exceedingly well: it was made from the model left by Dr. Wollaston to Capt. Kater.

March 5.—The subject this evening was, On the transmission of musical sounds through linear conductors, and their ultimate reciprocation. It was delivered by Mr. Faraday, but he informed the members that the matter, experiments, and new facts which he should have to bring forward, were altogether Mr. Wheatstone's. The general nature of musical sound was first distinguished from noise, and then its transmission through different bodies illustrated. The views gradually developed by Bacon, Hooke, and others, down to the present time, were detailed, by which ultimately we had gained much accurate knowledge of the conduction of sound through solid conductors.

After this the nature of the sounding-boards of instruments was entered upon, and it was shown how the vibrations of a string, rod or other almost inaudible phonic could be rendered strong and powerful when transmitted to planes extended in the direction perpendicular to the course of the vibrations, and also how far the inclosed volumes of air between the tablets of an instrument still further exalted the sound by resonance, and rendered it evident.

The conduction of sound by linear bodies and its ultimate reciprocation was then taken up, and numerous experimental illustrations were adduced. A tuning-fork vibrating on the roof of the Lecture-room had its vibration communicated inaudibly through forty feet or more of deal rod to the floor below, and then the sound developed by reciprocation; so that those close even to the tuning-fork referred the origin of the sound to the lower end of the apparatus. Conducting rods and wires were passed through the floor of the Lecture-room into a repository below, and the sounds of a piano transmitted from below to a harp above, whilst the sounds of the harp in the Lecture-room were transmitted to the pianoforte below. The music of stringed instruments, wind instruments, and even of the orchestrina or symphonium was equally well transmitted, being rendered evident and audible only at the place where by pre-arrangement their reciprocation had been provided for. Mr. Wheatstone intends shortly to arrange the new matter which he has been able to add to this branch of acoustical science in a separate form. Parts of it are already before the public in the Annals of Philosophy, and Quarterly Journal of Science.

March 12.—This evening Mr. Brande gave an account of the composition of Urinary calculi, and particularly of Dr. Wollaston's discoveries in that branch of chemistry. He did this, he said, because he was fully convinced that, if timely attention were paid to the attacks of sand and gravel in their early stages, many persons would be saved from the dreadful attacks of stone, and the evil cured before it had become unmanageable.

After stating what had been done before Dr. Wollaston's time, and then what had been effected by him, Marcet, Prout, and others, he he referred at last to the cases of Siliceous calculi, and stated that some new matter had been added to the subject lately, on this point, by Dr. Yelloly; but as we understand Dr. Yelloly's paper will come before the Royal Society, we shall probably have occasion to notice it more minutely hereafter.

March 19.—The evening was occupied by Mr. Ritchie, who gave an account of the methods proposed for measuring the intensities of natural and artificial light. He referred to the methods of Bouguer, Leslie, Rumford, and others; and amongst the rest to that upon which his own photometer is founded. This and the other instruments are known to the public. Mr. Ritchie's appears to be a very accurate and convenient instrument in numerous cases. By means of it the light of an Argand oil lamp and a gas lamp were compared; also the light of the Argand lamp and wax candles. The light of phosphorus burning in oxygen gas was compared to the light of the candle and lamp; and also a comparison made of the light of a wax-candle with that of the ball of lime ignited by the oxyhydrogen flame, as in Lieut. Drummond's application. Mr. Ritchie proposes to ascertain these numbers very accurately, and give the results to the public.

XLIV. *Intelligence and Miscellaneous Articles.*

CORRECTION IN MR. PHILLIPS'S PAPER.

IN order to complete the quotation from Dr. Thomson, the following is to be inserted after line 32, p. 131, of the Phil. Mag. and Annals, for February last:—

"The salt contains, besides,	5 atoms of water....	= 5·625
	2 atoms of hydrogen	= 0·250
	3 atoms of oxygen..	= 3·000
		8·875

Making a total of 8·875, which added to 34 make 42·875, the atomic weight of the solid salt. But 2 atoms of hydrogen, and 3 atoms of oxygen, cannot unite together so as to constitute water."

ACTION OF METALS ON WATER AND CARBONIC ACID, &c.

M. Despretz has stated to the French Academy, that nickel, cobalt, zinc, and tin, possess, like iron, the property of decomposing water at a red heat, and that their oxides are reduced by hydrogen at the same temperature; he has also observed that carbonic acid is converted by zinc and tin into oxide of carbon, and that this gas completely reduces the oxides of these metals. Thus a fact, which was considered as anomalous, extends to several metals and binary compounds.—*Le Globe.*

GLACIAL ACETIC ACID.

M. Despretz also states that crystallizable acetic acid may be procured by heating a mixture of an atom of acetate of lead, well dried, with one of concentrated sulphuric acid; and he further mentions, that in a good forge, at a high temperature, a mixture of sulphur and oxide of zinc yielded a sulphuret, which experienced mineralogists mistook for blende or native sulphuret.—*Ibid.*

METEORO-

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·33. Feb. 15. Wind N.E.—Min. 29·43. Feb. 7. Wind S.W.
Range of the mercury 0·90.
Mean barometrical pressure for the month .... 29·910
Spaces described by the rising and falling of the mercury............ 5·000
Greatest variation in 24 hours 0·440.—Number of changes 23.
Therm. Max. 53° Feb. 26. Wind S.W.—Min. 13°. Feb. 2. Wind N.E.
Range 40°.—Mean temp. of exter. air 36°·07. For 30 days with ☉ in ♒ 32·87
Max. var. in 24 hours 26°·00.—Mean temp. of spring-water at 8 A.M. 46·95

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, several times.................... 98°
Greatest dryness of the atmosphere in the afternoon of the 18th... 58
Range of the index 40
Mean at 2 P.M. 76°·6.—Mean at 8 A.M. 82°·7.—Mean at 8 P.M. 87·5
—— of three observations each day at 8, 2, and 8 o'clock .... 82·3
Evaporation for the month 0·72 inch.
Rain in the pluviameter near the ground 1·245 inch.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 3½; fine, with various modifications of clouds, 9; an overcast sky without rain, 10; foggy, 2½; rain, 3.—Total 28 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
16	10	27	2	10	12	16

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2½	5	2½	3	1½	9	2	2½	28

General Observations.—The weather this month has been mostly frosty, with a humid and cloudy atmosphere, and occasional gales of wind. The first six days and nights were extremely cold. Early in the morning of the 2nd the ground was lightly covered with snow, and a thick crust of ice continued all day on the inside of the windows in apartments with fire. The maximum temperature of the air did not exceed 21 degrees, and the minimum was within 13 degrees of zero; consequently this was the coldest day and night here, during the last fifteen years at least; and we suspect that a lower temperature has not been fairly registered in the south of England and in the vicinity of the sea, since Six's thermometer has been in use. The day was fine, with light snow, and the night cloudless, with a dry gale from N.E. At the time of this low temperature at 11 o'clock P.M., a wet finger, after being exposed to the wind about two minutes, adhered to a bar of iron: spittle applied to the same bar became solid ice in four seconds of time, and in six seconds on a fir rail. An adherence of a wet finger to iron did not succeed with a temperature of 16 degrees and a moister wind; therefore, in this as well as in former experiments, it appeared that the drier the air is, whether in motion or quiescent, the more rapid is the congelation of water, &c. It also snowed on the 3rd, which was nearly as cold as the preceding day. On the 4th the ice had much increased in Portsmouth harbour, in some places to the distance of three hundred yards from the western shore. Light snow descended in the morning of the 5th from a mixture of black and white *cumuli:* the night was very cold, the minimum temperature being 14 degrees. Haslar, Weevil, Portchester, and

and Fareham lakes, branching from the harbour, were firmly frozen over this and the preceding day; and on the reflux of the tide, immense masses of salt-water ice presented a snowy aspect about the shores to the distance of two or three miles, as far as the eye could perceive.

In the morning of the 6th there was much rime on the ground, &c. with faint sunshine; opposite winds in the day, and a little snow by night. During the morning of the 7th the sky was overcast, the clouds lowering, and the wind from every quarter of the compass, with a rising temperature. In the afternoon a gale sprung up from the S.W., which caused a sudden breaking up of the frost and a very rapid thaw, when a great quantity of ice in a connected chain upwards of a mile in length was carried out of the harbour; and copious streams of water ran down the wainscots and walls in apartments without fire. The successive masses of ice which had broken away from the lakes and the shores of Southampton river, had also a novel appearance in going out with the tide this and the two preceding days.

From this time to the 21st, the nights were generally frosty; and the last seven days the temperature of the air increased with a moist S.W. wind, accompanied with fogs, mild light rains, and spring-like weather at intervals. The difference between the temperature of the 2nd and 26th was 32 degrees.

The large lunar halo that appeared in the evening of the 28th, was remarkable for the breadth of its edge, which was upwards of three degrees, and of a faint vapourish colour; within the halo a close yellow corona appeared round the moon, which, with Aldebaran and the Pleiades, formed an isosceles triangle, Aldebaran being to the southward.

The atmospheric and meteoric phænomena that have come within our observations this month, are, four solar and four lunar halos, lightning and thunder early in the morning of the 23rd, and six gales of wind, or days on which they have prevailed; namely, two from the North-east, three from the South-west, and one from the West.

REMARKS.

London.—February 1. Clear and frosty: stormy at night, with snow. 2—5. Clear and frosty. 6. Cloudy and cold. 7. Drizzly rain: snow and frost nearly gone. 8. Fine: stormy rain at night. 9. Rainy morning: fine. 10. Clear and fine. 11. Slight fog in the morning: fine. 12, 13. Fine. 14—16. Foggy and cold. 17. Strong hoar-frost in the morning: foggy. 18. Clear and cold. 19. Slight fall of snow, which soon vanished: clear. 20. Fine. 21. Very fine morning: cloudy and wet. 22. Overcast, with rain at night. 23. Drizzly rain. 24, 25. Very fine. 26. Cloudy: rain at night. 27, 28. Very fine.

Penzance.—February 1. Snow. 2. Snow: clear. 3, 4. Clear. 5. Snow. 6. Heavy fall of snow: rain. 7. Rain: fair. 8. Clear. 9. Rain: fair. 10. Clear. 11. Clear: rain. 12. Fair: rain. 13. Clear: fair. 14. Clear. 15, 16. Fair. 17. Clear. 18. Showers: clear. 19. Clear. 20. Fair: showers. 21. Fair: rain. 22, 23. Rain: fair. 24. Misty: rain. 25. Clear: misty: rain. 26. Rain. 27. Misty. 28. Clear: fair.

Boston.—February 1. Fine. 2. Snow. 3, 4. Cloudy. 5. Snow. 6. Cloudy. 7. Cloudy: rain P.M. 8. Fine. 9. Rain. 10. Fine. 11. Fine: hail-storm early A.M. 12, 13. Cloudy. 14. Misty. 15. Cloudy. 16. Misty. 17. Fine. 18. Cloudy. 19, 20. Fine. 21. Snow. 22. Fine: rain early A.M. 23. Cloudy. 24. Cloudy: rain early A.M. 25. Cloudy. 26, 27. Fine. 28. Fine: rain P.M.

Meteoro-

Meteorological Observations made by Mr. BOOTH at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Feb. 1	29·965	29·862	29·85	29·83	29·90	29·83	29·63	26	20	36	31	27	19	24	NE.	NE.	NE.	NW.	...	...	...	...	...
2	29·925	29·877	29·82	29·80	29·80	29·74	29·70	25	18	31	25	21	13	22·5	NE.	NE.	NE.	E.	...	...	...	...	...
3	29·963	29·932	29·90	29·90	29·84	29·80	29·71	28	19	24	19	24	20	26·5	NE.	NE.	NE.	E.	0·08	...	...	0·010	...
4	29·930	29·831	29·90	29·88	29·82	29·75	29·66	30	12	32	20	30	20	29	NE.	NE.	NE.	calm	...	...	...	...	...
5	29·843	29·773	29·78	29·75	29·77	29·70	29·51	27	10	34	28	26	14	20	N.	SE.	N.	calm	...	...	...	...	...
6	29·686	29·666	29·48	29·46	29·60	29·58	29·35	26	15	45	29	30	20	22	NE.	SE.	E.	calm	·07	...	...	·005	...
○ 7	29·508	29·445	29·50	29·45	29·45	29·43	29·22	49	37	51	32	46	40	26	W.	SW.	SW.	S.	...	0·06	1·080	·090	...
8	29·723	29·472	29·70	29·60	29·76	29·58	28·96	49	38	50	45	45	40	41	W.	W.	SW.	W.	...	·51	...	·175	0·35
9	29·672	29·381	29·60	29·50	29·68	29·44	29·04	47	32	47	42	46	34	36	SW.	W.	W.	calm	·05	·02	·440	...	·30
10	30·129	29·849	30·00	29·90	30·08	29·88	29·41	46	25	45	38	45	30	37	SW.	NW.	NW.	W.	...	...	...	...	·14
11	30·252	30·227	30·10	30·00	30·19	30·18	29·74	46	31	47	35	43	34	33·5	W.	W.	S.	calm	...	...	...	...	...
12	30·188	30·176	29·90	29·86	30·14	30·13	29·74	45	28	47	38	40	35	37	S.	S.	SE.	S.	·06	...	·145	·070	...
13	30·208	30·171	30·00	29·92	30·13	30·12	29·80	44	26	48	40	42	29	35	SE.	SW.	SE.	calm	...	...	...	...	...
14	30·211	30·175	30·05	30·00	30·14	30·10	29·80	37	31	44	36	37	32	36	SE.	SE.	N.	calm	...	...	...	...	...
☾ 15	30·393	30·368	30·15	30·12	30·33	30·28	30·00	40	29	40	36	39	34	32	NE.	SE.	NE.	calm	·06	...	...	...	...
16	30·401	30·127	30·20	30·20	30·31	30·22	30·00	39	28	40	36	39	30	30	NE.	NE.	E.	calm	...	...	...	...	...
17	30·010	29·792	30·00	29·96	30·00	29·79	29·63	37	30	43	33	36	30	31·5	W.	N.	NW.	calm	...	...	...	...	...
18	29·813	29·692	29·90	29·80	29·82	29·74	29·34	40	25	44	35	39	30	32·5	N.	N.	NW.	calm	·10	...	...	...	...
19	29·849	29·828	29·90	29·90	29·84	29·82	29·43	40	22	46	31	41	25	31	W.	NW.	W.	calm	...	...	...	·010	...
20	29·912	29·824	29·88	29·80	29·88	29·85	29·47	46	27	42	34	39	28	28	W.	SE.	SW.	calm	...	...	...	...	...
21	29·645	29·482	29·70	29·70	29·69	29·53	29·25	45	28	48	34	42	31	35	W.	W.	SW.	W.	·10	·12	·110	·040	...
22	29·889	29·668	29·70	29·70	29·76	29·70	29·33	48	36	50	37	43	39	30	W.	W.	SW.	W.	...	·18	...	·140	·51
● 23	29·791	29·717	29·80	29·68	29·96	29·68	29·30	52	48	53	45	50	45	35·5	SW.	W.	SW.	W.	...	·09	·260	·055	...
24	30·164	29·985	30·00	29·90	30·16	30·06	29·50	58	48	53	49	52	45	48	W.	W.	SW.	SW.	·10	...	.055	·010	·22
25	30·255	30·207	30·05	30·03	30·20	30·10	29·65	59	47	50	46	50	45	51	W.	SW.	SE.	calm	...	...	...	·350	...
26	30·221	30·162	30·00	30·00	30·11	30·06	29·48	56	45	52	45	53	45	51	W.	SW.	SW.	W.	...	·33	·400	·275	...
27	30·110	29·986	30·08	30·05	30·13	30·06	29·37	57	42	53	49	51	43	50·5	SW.	W.	SW.	W.	...	...	...	·015	...
28	30·109	30·056	30·12	30·10	30·22	30·10	29·47	58	45	52	45	52	42	48	NE.	W.	SW.	NW.	·10	...	·020	...	·14
Aver.	30·401	29·381	30·20	29·45	30·33	29·43	29·51	59	10	53	19	53	13	34·2					0·72	1·31	2·510	1·245	1.66

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

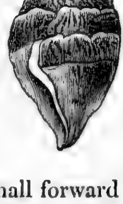
MAY 1830.

XLV. *Letter from the Rev.* Dr. W. Buckland, *F.R.S. &c. Professor of Mineralogy and Geology in the University of Oxford, on the Discovery of Coprolites in North America.*

Dear Sir,

I HASTEN to announce a discovery of the occurrence of Coprolites in the State of New Jersey, which has just been communicated to me by Mr. J. E. Dekay of New York; and as this is the first recognition of these substances in the transatlantic world, I think it due to Mr. Dekay to publish his discovery in his own words, extracted from his letter to me on this subject.

"Sir,—Among the various organic remains of our country which have attracted my attention, there is one specimen that has puzzled me exceedingly. Being aware of the strange and anomalous forms exhibited among the extinct saurians, I was disposed at one time to consider it as a tooth, and should most probably have published it as such; when a notice of your interesting coprological researches met my eye. The notice (Loudon's Magazine) was short, but sufficiently satisfactory; and I now am enabled to state that fæcal matter resembling your *Sauro-copros* has been found in the unconsolidated secondary strata of the United States. There is but a solitary specimen in our cabinet, or I would send it to you with much pleasure.



I have procured casts of it in wax, which I shall forward to you. The adjoining sketch will give you an idea of this coprolite. I shall ask permission of the Lyceum to allow it to be analysed. My imagination may possibly be too vivid, but in in the confused irregularly impressed lines on the surface of my coprolite, I feel assured that I detect the marks left by the membranous coats of the smaller intestines.

This substance was found some years since by Dr. Mitchill, and presented to the Lyceum of New York. It was found in the ferruginous sand formation of New Jersey. I have a clue to its locality; and if my health permits, will examine the country thoroughly in the spring. The district in which it occurs, is referred to your green-sand formation. Dr. Morton (American Journal, vol. xvii. p. 275) has recently described its fossil contents. As far as I am acquainted with the subject, it appears to be analogous to the chalk formations of your country (*craie inférieure* of the French geologists). This deposit furnishes also Mososaurus and Geosaurus, upon which I have written a short article for our "Annals."—But to return to the coprolite. Its length is nine-tenths of an English inch, and the thickness of the folds about one-tenth; in the lower end the folds may be traced some distance. Since reading the notice of your memoir, my attention has been called to some appearances which would in all probability have otherwise escaped my attention. In one part of the fossil, there is a quantity of foreign substances which have not yet been faithfully examined. It appears to be fibrous animal matter with small fragments of quartz. I am, Sir, with unfeigned respect,

Your humble servant,

New York, Jan. 26, 1830. J. E. Dekay."

"P.S.—I send by the same opportunity a cast of a bone of Megalonyx (metatarsal?) recently discovered at Big Bone Lick."

Together with this letter I have received the casts alluded to therein, and entertain not the slightest doubt of the accuracy of Mr. Dekay's conclusion respecting the coprolitic nature of the body from which these casts are taken. I also recognise on their surface the fine lines and impressions derived from the membranous coats of the small intestines.

Mr. Dekay mentions the occurrence of an extraneous substance, and of grains of quartz, within this coprolite: it is obvious that coprolites may contain extraneous substances of various kinds; viz. scales, teeth, and undigested fragments of bones of any animal they may have devoured; also shells, and fragments of shells, *e. g.* shells of small ammonites swallowed together with their molluscous inhabitants: since my paper on Coprolites was printed, I have seen a small ammonite entire, with its nacre beautifully iridescent, inclosed in a coprolite, in the collection of a lady at Lyme Regis. Miss Anning has also found

found fragments of pearly shell in other specimens of coprolite from the same place. A small fragment of quartz occurring within a coprolite may have existed in the stomach of some animal that was devoured by the saurian or fish, from which such coprolite has been derived; or it may have been accidentally swallowed by the animals in gorging their prey at the bottom of the sea.

The shape and size of the American coprolite accords sufficiently with those we find in the inferior chalk of Sussex, to add probability to Mr. Dekay's opinion, that it is from the inferior chalk or green-sand formation in New Jersey. And whatever may be its geological position, the discovery of this specimen in North America encourages me to anticipate the speedy verification of my conjecture, that coprolites will be found to occur universally wherever there are fossil remains of saurians, or of any other carnivorous animals that swallowed their prey entire, or which, like dogs and hyænas, devoured bones as well as flesh.

I remain, Dear Sir, yours, &c.

To R. Taylor, Esq. Wm. Buckland.

XLVI. *Observations on some late Statements by* Mr. Farey *respecting the Steam-Engines of Cornwall. By* Wm. J. Henwood, *F.G.S.*

To Richard Taylor, Esq. F.L.S. F.G.S. &c. &c.

Sir,

IN a late Number of the Phil. Mag. and Annals*, you have reprinted a part of Mr. Farey's evidence on the patent laws, in which are statements injurious to some of the improvers and builders of steam-engines. The substance of those to which I object, may be stated as follows:

1st, Ascribing to Mr. Woolf the "invention of working steam-engines by high-pressure steam acting expansively."

2ndly, "The difference between the cost of coal for a given force of engines in 1813 and at present, would absorb the profit of all the deep mining that is now carried on in Cornwall."

3rdly, Stating "that all the engines in the county are on Mr. Woolf's plan."

1st, I believe the invention of expansive working has been

* Feb. 1830. N.S. vol. vii. p. 152.

 with

with propriety ascribed to Mr. Watt by every person, excepting Mr. Farey: and in another place* even he has concurred in this opinion; variation in the elasticity of the steam employed by no means affecting the invention. Moreover, the evidence at present before the world, seems to preponderate against the use of steam of very high tension.

2ndly, With regard to the performance of steam-engines, it has been stated † that Mr. Watt's proposals were, that his engines should lift 23·44 millions of pounds one foot high, by the consumption of one bushel (= 84 pounds) of coal; and I am informed by Mr. Sims, an engineer well known in this county, that the average duty of Watt's engines here, whilst superintended by his own agents, was about 25· millions; but this will be seen more accurately stated in a paper recently read before the Royal Society, by its illustrious President. Subsequently to Mr. Watt's relinquishing the superintendence of the engines here, the average duty suffered a considerable diminution. The following may be regarded as an approximation to the duty for some years.

	Average of all Cornwall.	Average of Woolf's.	Highest of Watt's.	Highest of Woolf's.
1823	26·9 millions.	36· millions.	40·6	47· millions.
1824	28·	none at work.	42·1	
1825	28·97	34·7	42·3	41·9
1826	28·36	40·4	45·4	40·4
1827	31·9	none at work.	64·3	
1828	37·2		87·2	
1829 (to June)	40·72		79·	

3rdly, The construction of the Cornish steam-engines.

I presume Mr. Farey follows many other writers, in ascribing to Mr. Woolf the invention of an engine, all the mechanical contrivances of which were made by Mr. Hornblower. But the merit of invention apart,—*there is not one engine on this construction now at work in Cornwall; the last*, which was at Huel Alfred, *having been stopped in* 1826. Indeed, beside some five or six rotatory engines, neither of which exceeded twenty-eight inches cylinder, the only engines on Mr. Woolf's plan (or rather on Mr. Hornblower's, erected by Mr. Woolf) worked on the mines of this county, were at

Huel Vor...... 53 inches diameter of larger cylinder ‡.
Huel Unity ... 60 —— §

* Farey on the Steam-engine, p. 339. † *Ibid.* p. 337.

‡ The use of the smaller cylinder was discontinued soon after Mr. Woolf's superintendence ceased.

§ The small cylinder was not worked for some years, the engine at the time being under the supervision of Mr. Woolf.

Huel

Huel Abraham 45 inches diameter of larger cylinder*.
60 ——
Huel Alfred... 70 ——

I waited to see the remainder of Mr. Farey's evidence, but as you have not continued it in the March Number of your valuable Journal, I am induced to trouble you on this subject whilst that gentleman's statements are fresh in the minds of your readers. I have the honour to be, yours, &c.

Perran Wharf, near Truro,
March 16, 1830.

WM. J. HENWOOD.

XLVII. *Some general Considerations respecting the Propagation of Motion through Elastic Mediums; with Remarks on a former Communication.* *By* J. CHALLIS, *Esq. Fellow of Trinity College Cambridge, and of the Cambridge Philosophical Society*†.

THE following mode of considering the transmission of motion through elastic substances, which has not yet, I believe, occurred to any one, may serve to throw some light on a subject confessedly difficult, but which is nevertheless interesting; as it has direct reference to the manner in which our organs of hearing and sight are acted upon by external objects.

The elastic substance to be considered may be either solid or fluid. Suppose an indefinitely slender column of it, the transverse section of which is every where the same, and the parts of which are moving in any manner, to be divided into an indefinite number of very small *equal* masses. Call α, β, γ, three of these masses taken in order, of which α is nearest the origin of x the abscissa: the density of each may be considered the same throughout, but their densities to vary from one to another. Let z be the length of α, z' of β, z'' of γ. Then the distance between the centres of gravity of α and β is $\frac{z+z'}{2}$, and that between the centres of gravity of β and γ, $\frac{z'+z''}{2}$. Let v', v, be respectively the velocities of the centres of gravity of β and γ, at a time t reckoned from a given instant. Then $v'-v=$ their relative velocity. At the end of a small additional time τ, v' becomes $v'+\frac{dv'}{dx}v'\tau+\frac{dv'}{dt}\tau$,

* Subsequently removed to Huel Wentworth.

† Communicated by the Author.

and

and v becomes $v + \frac{dv}{dx} v\tau + \frac{dv}{dt}\tau$, and the relative velocity is,
$$v' - v + \frac{d.(v'^2 - v^2)}{2dx}\tau + \frac{d.(v'-v)}{dt}\tau.$$

But as $v' = v + \frac{dv}{dx}\delta x$, the two last terms of the preceding expression will involve small quantities of an order which may be neglected. Hence the relative velocity will be $v' - v$ during the small time τ. Now by propagated motion it is to be understood that the state of the masses γ, β, passes into that of β, α, supposing the propagation to be directed *from* the origin of x. Hence the distance between the centres of gravity of γ and β, will become that between the centres of β and α. If this take place at the end of the time τ, $(v' - v)\tau =$ the difference of these distances. Hence
$$(v'-v)\tau = \frac{z'+z''}{2} - \frac{z+z'}{2} = \frac{(z'-z)+(z''-z')}{2}.$$

But $z'' = z' + \frac{dz'}{dx}\delta x$, and $z = z' - \frac{dz'}{dx}\delta x$. Therefore, $z' - z = z'' - z'$, and consequently $(v' - v)\tau = z'' - z'$. If $\varrho' =$ the density of β, ϱ of γ, $\varrho' z' = \varrho z''$, and $z'' - z' = z' . \frac{\varrho' - \varrho}{\varrho}$.

Hence,
$$v' - v = \frac{z'}{\tau} . \frac{\varrho' - \varrho}{\varrho}.$$
Or, since $v' - v$ and $\varrho' - \varrho$ are the variations of v and ϱ at a given instant from one point to a contiguous point,
$$\frac{dv}{dx} = \frac{z'}{\tau} . \frac{d\varrho}{\varrho dx}.$$

Here $\frac{z'}{\tau}$ is the rate at which a given small mass passes into the state of the contiguous mass, abstraction being made of the velocity common to the two. Hence $\frac{z'}{\tau}$ is exactly the velocity of propagation. If it be constant and equal to b, $\frac{dv}{dx} = b\frac{d\varrho}{\varrho dx}$; and by integrating,
$$v = b\,h.l.\varrho + \phi(t) \qquad \text{(A)}$$
It is to be observed, that the proposition here proved is independent of all consideration of the constitution of the medium, and the influence of caloric and extraneous forces.

When the medium is a fluid, and the relation between the density and pressure is, $p = a^2\varrho$, the equations relating to the motion are,
$$\frac{d^2\phi}{dx^2} - \frac{2v}{a^2 - v^2} . \frac{d^2\phi}{dx\,dt} - \frac{1}{a^2 - v^2} . \frac{d^2\phi}{dt^2} + \frac{Pv}{a^2 - v^2} = 0,$$

$$a^2 h . l . \varrho = \int P \, d x - \frac{d \phi}{d t} - \frac{v^2}{2}, \text{ and } v = \frac{d \phi}{d x}.$$

(Lagrange, *Méc. Anal.* part ii. sect. xii. Art. 8.)

P is the accelerative force impressed on the fluid. In the case in which $P = 0$, M. Poisson has obtained a particular integral of the first of these equations, from which it may be inferred that $v = a h . l . \varrho = f(x - a t - v t)$. (1)

(*Journal de l'Ecole Polytechnique*, cah. xiv.)

Also when P is constant, by Monge's method of integration may be obtained,

$$v = a h l . \varrho + P t = f\left(x - a t - v t + \frac{P t^2}{2}\right) \qquad (2)$$

Each of the two systems of equations (1) and (2) refers to propagation in a single direction; and it will follow from the general equation (A) that in each of the two cases the velocity of propagation is exactly a. This result may also be obtained from the equations themselves; for if in (1) x and t be made to vary, so that ϱ does not alter, it will be found that $\frac{d x}{d t} = a + v$. But $\frac{d x}{d t}$ obtained on this hypothesis is the velocity of propagation *together with* the velocity of the fluid. Hence the velocity of propagation is a. If in (2) x and t be made to vary so that ϱ remains unaltered, and consequently so that $d v = P \, d t$, the resulting value of $\frac{d x}{d t}$ is again $a + v$. This subject I have considered more at length in a recent communication to the Cambridge Philosophical Society; at present I wish to make another kind of application of the equation (A).

Experience has shown that the velocity of propagation in air of the same temperature is constantly the same, and is independent of the magnitude of the condensations and motions. The precision with which slight modifications of the disturbing causes are appreciated by the ear, is very probably dependent on the absolute uniformity of propagation of every individual portion of the aërial undulations, and the constancy of their *type* during their progression. Assuming that they are of this nature, the equation (A) will be applicable to aërial propagation. Now, by the general consideration of fluid motion in space of one dimension, the two following equations are obtained:

$$\frac{d . h . l . \varrho}{d t} + v \frac{d . h . l . \varrho}{d x} + \frac{d v}{d x} = 0$$

$$\frac{d p}{\varrho d x} = P - \frac{d v}{d t} - v \frac{d v}{d x}.$$

(Lagrange, as above cited.)

Combining

Combining these equations with (A), we arrive at,

$$\frac{dp}{\varrho\, dx} = \mathrm{P} + b^2 . \frac{d\varrho}{\varrho\, dx} - \phi'(t) ; \qquad \text{(B)}$$

and $\phi(t)$, which is the value of v when $\varrho = 1$, may be supposed to be nothing or constant, so that $\phi'(t) = 0$. First, let $\mathrm{P} = 0$. Then $\frac{dp}{\varrho\, dx} = \frac{b^2 d\varrho}{\varrho\, dx}$, and p being a function of ϱ, must be equal $b^2 \varrho$. This is the only relation between p and ρ by which uniform propagation can obtain, when no force is impressed on the fluid.

As the velocity of aërial propagation is not found to be that which would be deduced theoretically from the relation between p and ρ, which experiments on air at rest make known, some additional force, which is latent in the quiescent state of the particles, must be called into play by their motion. In Laplace's theory $\mathrm{P} = - k^2 a^2 \frac{d\varrho}{\varrho\, dx}$, and $b = a\sqrt{1 + k^2}$. Hence from (B),

$$\frac{dp}{\varrho\, dx} = (b^2 - k^2 a^2) \frac{d\varrho}{\varrho\, dx} = \frac{a^2 d\varrho}{\varrho\, dx},$$

and consequently $p = a^2 \rho$, which ought to be the case. Hence this theory, which every thing tends to confirm, is not restricted to small motions.

If $p = a^2 \rho^{1+n}$, $\mathrm{P} = \left(a^2 \rho^n (1 + n) - b^2 \right) \frac{d\varrho}{\varrho\, dx}$, a quantity which, when ρ is nearly 1, and $b = a\sqrt{1 + n}$, is very small. Consequently, when the motions are small, the velocity of propagation may be very approximately deduced from the law, $p = a^2 . \rho^{1+n}$, without supposing any additional force to act. But if this law really obtained in nature, the type of a series of undulations would not be constant during their propagation.

To take another instance of propagated motion, let us consider the passage of light through transparent bodies according to the undulatory hypothesis. It has been usual to suppose the diminished velocity of the propagation of light in the interior of mediums, to be accounted for by the existence of a less elastic force of the æther in their interior than exterior to them. But this supposition leaves out of consideration the obstacle to the free motion of the particles of the æther, which must be presented by the material particles of the medium. Let us suppose that $p = a^2 \rho$ both out of the medium and in it; and as the velocity of propagation in the medium is uniform and less than a, let $\mathrm{P} = \frac{k^2 a^2 d\varrho}{\varrho\, dx}$. Then by reason of the equation (B), $b = a\sqrt{1 - k^2}$. If $\frac{b}{a} = \frac{1}{m}$, $k^2 = 1 - \frac{1}{m^2}$. The retarding

tarding force P, according to this view, will result from the *mean* effect of the reflections of the undulations from the material particles of the medium. Its ratio, k^2, to the accelerative force which under the same state of density would exist out of the medium, might be shown to vary as the number of the material particles in a given space, and the velocity of propagation conjointly; but the reasoning for this purpose is too long to be inserted here. Admitting this, which of itself is probable, to be the case, if δ = the density of the medium,

$$1 - \frac{1}{m^2} = \frac{H\,\delta}{m}, \text{ or } \frac{m^2 - 1}{m\,\delta} = H.$$

The quantity H will be the same in different states of the same medium, but different in different mediums. If the states of the medium be as different as the aëriform and fluid or solid, H will probably not retain the same value, because this quantity must be a function depending on the arrangement of the atoms. If the medium be a gas, $m = 1$ nearly, and $\frac{m^2 - 1}{\delta}$ is nearly constant for different states of compression. This M. Biot has found by experiment to be the case in atmospheric air. The constancy of the value of $\frac{m^2 - 1}{\delta}$ for different states of the same medium is a result which has been deduced from the corpuscular theory of light; and in this respect (perhaps, in this alone) the corpuscular seemed to have the advantage over the rival theory.

I take this opportunity of adverting to a communication I made to the Phil. Mag. and Annals of Philosophy for August 1829, in which it was stated that the integral of the equation, $\frac{d^2\varphi}{dx^2} + \frac{d^2\varphi}{dy^2} + \frac{d^2\varphi}{dz^2} = 0$, obtained on the supposition that φ is a function of $x^2 + y^2 + z^2$, or r^2 and t, is not necessarily restricted to a particular case, in its application to the motion of incompressible fluids, but points to a general law of the motion. The reason of this is, that the equation $d\varphi = u\,dx + v\,dy + w\,dz$, on which the integral is dependent, implies that when the parts of the fluid move *inter se*, and not in such a manner that it may be considered solid, the motion in every individual portion is directed to a fixed or moveable centre; in the same manner as the equation $dV = X\,dx + Y\,dy + Z\,dz$ implies, that the impressed forces are directed to fixed or moveable centres. This remark, which is important to the theory of fluid motion, was verified in the instance in which the motion is in space of two dimensions, by showing that the same result is arrived at whether φ be supposed a function of

r and

r and t, or the forms of the arbitrary functions in the complete integral, (which in this instance may be obtained,) be determined on the supposition that the origin and direction of coordinates are not fixed. I must, however, observe that the problem, selected in illustration of these principles; viz. to find the velocity at the vena contracta of a stream issuing from a small orifice in any vessel, is incorrectly solved. An accurate solution of this problem, and a fuller consideration of the whole subject, will be found in the memoir above alluded to.

Trin. Coll. Cambridge,
March 15, 1830.

XLVIII. *On the Conversion of Right Ascension and Declination into Longitude and Latitude, and vice versâ.* By Prof. ENCKE*.

VARIOUS applications have been made to me, requesting that I would give in the Ephemeris, besides the geocentric right ascensions and declinations of the planets, also their latitudes and longitudes, as was usual in the former construction of the Ephemeris, it having been hitherto the custom to express the places of the planets by these latter co-ordinates. Without considerably enlarging the book, this wish could not be fulfilled; I have, however, endeavoured to supply this want, as far as it may still exist, by the following tables for approximately finding these quantities with an accuracy sufficient for all useful purposes. If only minutes of longitude and latitude are required for heavenly bodies moving in the zodiac, neither trigonometrical calculations nor logarithms will be wanted; a simple multiplication of two numbers will be sufficient. Combined with a small trigonometrical calculation they will give both conversions with nearly the same accuracy as can be obtained with logarithms having five figures of decimals.

Denoting the right ascension and declination of a heavenly body by α and δ, the longitude and latitude by λ and β, the obliquity of the elliptic by ε, the well-known formulæ for effecting both these mutual conversions are as follows:

$$\sin\beta = \cos\varepsilon \sin\delta - \sin\varepsilon \cos\delta \sin\alpha \qquad (1)$$
$$\cos\beta \sin\lambda = \sin\varepsilon \sin\delta + \cos\varepsilon \cos\delta \sin\alpha$$
$$\cos\beta \cos\lambda = \cos\alpha \cos\delta$$

* From his *Astronom. Jahrbuch*, 1831, p. 281.—The explanation of the tables for converting geocentric longitude and latitude into right ascension and declination, and *vice versâ*, is here given, but not the tables, agreeably to the intention we have always had in giving these translations, which was to render the work of Encke useful to such as are not acquainted with the German language. This paper concludes the list of those from Encke's Ephemeris for 1831, of which we have given whatever was capable of being translated, as we formerly did of that for 1830.—EDIT.

sin

$$\begin{aligned} \sin\delta &= \sin\varepsilon\cos\beta\sin\lambda + \sin\beta\cos\varepsilon \\ \cos\delta\sin\alpha &= \cos\varepsilon\cos\beta\sin\lambda - \sin\beta\sin\varepsilon \qquad (2) \\ \cos\delta\cos\alpha &= \cos\beta\cos\lambda \end{aligned}$$

They are general through the whole circumference of the circle, inasmuch as, by the nature of the case, $\cos\alpha$ and $\cos\lambda$ have always the same signs, or as α and λ are always at the same time between 90° and 270°, or 270° and 90°.

For facilitating the logarithmic calculation it is usual to introduce an auxiliary angle, by means of which the two parts on one side are changed into one only. For (1) put:

$$\begin{aligned} \sin\delta &= M\sin N \\ \cos\delta\sin\alpha &= M\cos N \end{aligned}$$

hence

$$\begin{aligned} \sin\beta &= M\sin(N-\varepsilon) \\ \cos\beta\sin\lambda &= M\cos(N-\varepsilon) \\ \cos\beta\cos\lambda &= \cos\delta\cos\alpha \end{aligned}$$

The value of M from the two first auxiliary formulæ being substituted in the latter three; viz. $M = \frac{\cos\delta\sin\alpha}{\cos N}$ we obtain

$$\tan N = \frac{\tan\delta}{\sin\alpha}$$

$$\tan\lambda = \frac{\cos(N-\varepsilon)}{\cos N}\tan\alpha$$

$$\tan\beta = \tan(N-\varepsilon)\sin\lambda$$

to which may be added as a check on the calculation

$$\frac{\cos(N-\varepsilon)}{\cos N} = \frac{\cos\beta\sin\lambda}{\cos\delta\sin\alpha}$$

These formulæ give every thing by means of tangents, consequently in the most accurate manner; and (with due regard to the remark above made) without any ambiguity; the geometrical signification of M and N will be easily found. For a single calculation they are beyond doubt the most convenient; but they do not admit of a table of single entry even for constant ε, and have besides the small disadvantage that for converting at the same time several places with regular differences, the auxiliary angle N is, in the vicinity of 0° and 180°, more irregular than the given principal quantities.

For the system (2) we have, in like manner:

$$\tan N' = \frac{\tan\beta}{\sin\lambda}$$

$$\tan\alpha = \frac{\cos N' + \varepsilon}{\cos N'}\tan\lambda$$

$$\tan\delta = \tan(N'+\varepsilon)\sin\alpha$$

$$\frac{\cos N' + \varepsilon}{\cos N} = \frac{\cos\delta\sin\alpha}{\cos\beta\sin\lambda}$$

The

The formulæ (1) and (2) may be rendered convenient for logarithmic calculation in another way, by assuming for each of the two systems a particular case as a foundation.

Designating in (1) by λ' and δ' the longitude and declination of a point whose right ascension $= \alpha$, and latitude $= 0$, we have

$$\begin{aligned} 0 &= \cos\varepsilon \sin\delta' - \sin\varepsilon \cos\delta' \sin\alpha \\ \sin\lambda' &= \sin\varepsilon \sin\delta' + \cos\varepsilon \cos\delta' \sin\alpha \\ \cos\lambda' &= \cos\alpha \cos\delta' \end{aligned} \qquad (3)$$

whence

$$\begin{aligned} \sin\lambda' \sin\varepsilon &= \sin\delta' \\ \sin\lambda' \cos\varepsilon &= \cos\delta' \sin\alpha \\ \cos\lambda' &= \cos\delta' \cos\alpha \end{aligned} \qquad (4)$$

Combining (1) and (3) by multiplying with $\cos\delta$ and $\cos\delta'$, as also with $\cos\lambda'$ and $\sin\lambda'$, we have

$$\sin\beta = \frac{\cos\varepsilon}{\cos\delta'} \sin(\delta-\delta')$$

$$\begin{aligned} \cos\beta \sin(\lambda-\lambda') &= \sin\varepsilon \cos\alpha \sin(\delta-\delta') \\ \cos\beta \cos(\lambda-\lambda') &= \cos\delta \cos\delta' \cos\alpha^2 + \sin\varepsilon^2 \sin\delta \sin\delta' \\ &\quad + \cos\varepsilon^2 \sin\alpha^2 \cos\delta \cos\delta' \\ &\quad + \sin\varepsilon \cos\varepsilon \sin\alpha \sin(\delta+\delta') \end{aligned}$$

where the latter formula may also be written thus:

$$\begin{aligned} \cos\beta \cos(\lambda-\lambda') &= \cos(\delta-\delta') \\ &\quad - \{\cos\varepsilon \sin\delta - \sin\varepsilon \cos\delta \sin\alpha\} \times \\ &\quad \{\cos\varepsilon \sin\delta' - \sin\varepsilon \cos\delta' \sin\alpha\} \end{aligned}$$

and as by the first equation of (3) the latter part is always $= 0$, we have

$$\begin{aligned} \sin\beta &= \frac{\cos\varepsilon}{\cos\delta'} \cdot \sin(\delta-\delta') \\ \cos\beta \sin(\lambda-\lambda') &= \sin\varepsilon \cos\alpha \sin(\delta-\delta') \\ \cos\beta \cos(\lambda-\lambda') &= \cos(\delta-\delta') \end{aligned} \qquad (5)$$

These three equations squared, give

$$1 = \left\{ \frac{\cos\varepsilon^2}{\cos\delta'^2} + \sin\varepsilon^2 \cos\alpha^2 \right\} \sin(\delta-\delta')^2 + \cos(\delta-\delta')^2$$

It will, therefore, be allowed to put

$$\frac{\cos\varepsilon}{\cos\delta'} = \sin\gamma, \qquad \sin\varepsilon \cos\alpha = \cos\gamma$$

where on account of the first equation of (5) γ must always be assumed less than 180°. The calculation is thus reduced to these six equations:

$$\begin{aligned} \tan\lambda' &= \tan\alpha \sec\varepsilon \\ \tan\delta' &= \sin\alpha \tan\varepsilon \\ \cos\gamma &= \cos\alpha \sin\varepsilon \\ \sin\beta &= \sin\gamma \sin(\delta-\delta') \\ \cos\beta \sin(\lambda-\lambda') &= \cos\gamma \sin(\delta-\delta') \\ \cos\beta \cos(\lambda-\lambda') &= \cos(\delta-\delta') \end{aligned}$$

in which $\cos\lambda'$ and $\cos\alpha$ must always be taken with equal signs

signs. In these equations λ', δ' and γ for a constant ε are functions of α only. If, therefore, λ', δ', $\sin\gamma$, $\cos\gamma$, be reduced into tables with single entry, the argument being α, it will only be required to calculate,

$$\text{tang}\, p = \text{tang}\,(\delta-\delta')\cos\gamma$$
$$\text{tang}\,\beta = \text{tang}\,(\delta-\delta')\sin\gamma\cos p$$
$$\lambda = \lambda' + p.$$

The geometrical signification of these auxiliary quantities will be found without trouble.

Exactly in the same manner we have for system (2),

$$\text{tang}\,\alpha' = \text{tang}\,\lambda\sec\varepsilon$$
$$\text{tang}\,\beta' = \sin\lambda\,\text{tang}\,\varepsilon$$
$$\cos\gamma' = \cos\lambda\sin\varepsilon$$
$$\sin\delta = \sin\gamma'\sin(\beta+\beta')$$
$$\cos\delta\sin(\alpha'-\alpha) = \cos\gamma'\sin(\beta+\beta')$$
$$\cos\delta\cos(\alpha'-\alpha) = \cos(\beta+\beta')$$

If, therefore, the following quantities are reduced to tables whose argument k:

$$\text{tang A} = \text{tang}\,k\sec\varepsilon$$
$$\text{tang B} = \sin k\,\text{tang}\,\varepsilon$$
$$a = \cos k\sin\varepsilon$$
$$b = \frac{\cos\varepsilon}{\cos \text{B}}$$

we shall find for $k = \alpha$

$$\text{tang}\,p = a\,\text{tang}\,(\delta-\text{B}) \qquad \lambda = \text{A}+p$$
$$\text{tang}\,\beta = b\,\text{tang}\,(\delta-\text{B})\cos p$$

and for $k = \lambda$

$$\text{tang}\,q = a\,\text{tang}\,(\beta+\text{B}) \qquad \alpha = \text{A}-q$$
$$\text{tang}\,\delta = b\,\text{tang}\,(\beta+\text{B})\cos q$$

With respect to the purpose for which the tables are here intended, viz. converting the geocentric α and δ into λ and β, for planets or such heavenly bodies as are within the zodiac, or for which $\beta < \pm 10°$, it will be sufficient, if an approximation is only required, to put: $\beta = b\,(\delta-\text{B})$

$$\lambda = \text{A} + a\,(\delta-\text{B})\sec\beta$$

where even the factor $\sec\beta$ may in most cases be neglected, because its value is only,

β	$\sec\beta$	β	$\sec\beta$
± 0°	1·000	± 6°	1·006
1	1·000	7	1·008
2	1·001	8	1·010
3	1·001	9	1·012
4	1·002	10	1·015
5	1·004		

The

The errors arising from neglecting this factor will always be within narrow limits. For the rigorous value of β we have $\sin\beta = b \sin(\delta - \delta')$ and by developing the series:

$$\beta = b(\delta - \delta') - \frac{1}{6} \sin\beta^3 \operatorname{cotang}\gamma^2 - \frac{3}{40} \sin\beta^5 \operatorname{cotang}\gamma^2 \frac{1 + \sin\gamma^2}{\sin\gamma^2} \ldots$$

where the last term when a maximum for $\beta = 10°$ does not amount to a second; the second, when a maximum, for

$$\beta = \;\; 5° \ldots\ldots\ldots\ldots \;\; 4''$$
$$= 10 \ldots\ldots\ldots\ldots 34$$

The error for λ would be somewhat greater: here the development of the series is

$$\lambda = A + a(\delta - \delta') \sec\beta - \frac{1}{6} \frac{\operatorname{tang}\beta^3}{\operatorname{tang}\gamma} \ldots$$

For $\beta = 5°$ the error is about $9''$; for $\beta = 10°$ about $82''$ when a maximum. For the following tables, ε has been assumed at $23° \; 27' 30''$.

As an example, let the longitude and latitude of Mars 1831, January, be required. The Ephemeris gives

$$\alpha = 0^h \; 58' \; 33''\cdot65 \qquad \delta = +6° \; 42' \; 0''\cdot0.$$

By these quantities we find from the table,

$$A = 15° \; 53'\cdot7 \qquad B = +\;6° \; 15'\cdot5$$
$$a = +\;0\;3852 \qquad b = 0\cdot9228$$
$$\delta - B = +\;26\cdot5 \qquad \beta = +\;0° \; 24'\cdot5$$
$$a(\delta - B) = +\;10\cdot2 \qquad \lambda = 16° \;\; 3'\cdot9$$

The rigorous calculation would have given $\beta = 0° \; 24' \; 25''$ and $\lambda = 16° \; 3' \; 54''$.

XLIX. *Notes on the Geographical Distribution of Organic Remains contained in the Oolitic Series of the Great London and Paris Basin, and in the same Series of the South of France.* *By* Henry T. De la Beche, *F.R.S. &c.*

[Continued from page 268.]

It cannot have escaped the reader's observation, that many fossils are common to several parts of the oolitic series;—to show the various beds in which the same organic remains may occur, and at the same time to convey an idea of the organic contents of the oolitic series in general, I have formed the following table from the authorities already mentioned, and from others that will be noticed in their places. Such a table must necessarily be very defective, and can only answer temporary purposes; but it is hoped that it may be useful.

Organic

Organic Remains of the Oolitic Series.

VEGETABLE REMAINS.

Algæ.

Fucoides furcatus (*Ad. Brong.*). *Stonesfield slate* (Ad. Brong.).*

Equisetaceæ.

Equisetum columnare (*Ad. Brong.*). *Lower carbonaceous series.* Yorks. (Phil.). Brora (Murch.).

Filices.

1. Pachypteris lanceolata (*Ad. Brong.*). *Coal, shale, &c. between inf. and great oolite.* Yorks. (Phil.).
2. ——— ovata (*Ad. Brong.*). *Coal, shale, &c. between inf. and great oolite.* Yorks. (Phil.).

1. Pecopteris Reglei (*Ad. Brong.*). *Forest marble.* Mamers (Desn.).
2. ——— Desnoyerii (*Ad. Brong.*). *Forest marble.* Mamers (Desn.).
3. ——— polypodioides (*Ad. Brong.*). *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).
4. ——— denticulata (*Ad. Brong.*). *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).
5. ——— Phillippi (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.).
6. ——— Whitbiensis (*Ad. Brong.*). *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).

1. Sphænopteris hymenophylloides (*Ad. Brong.*). *Stonesfield slate* (Buckl.). *Coal, shale, &c. between gt. and inf. oolite.* Yorks. (Phil.).
2. ——— ? macrophylla (*Ad. Brong.*). *Stonesfield slate* (Buckl.).
3. ——— Williamsonis (*Ad. Brong.*). *Coal, &c of the oolitic series.* Yorks. (Ad. Brong.).
4. ——— crenulata (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.).
5. ——— denticulata (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.).

1. Tæniopteris latifolia (*Ad. Brong.*). *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).
2. ——— vittata (*Ad. Brong.*). *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).

Cycadeæ.

1. Pterophyllum Williamsonis. *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).

1. Zamia pectinata (*Ad. Brong.*). *Stonesfield slate.* (Buckl.).
2. ——— patens (*Ad. Brong.*). *Stonesfield slate.* (Ad. Brong.).

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* Abbreviations of authors' names:

Beaum.	Elie de Beaumont.	Dufr.	Dufrénoy.
Bobl.	Boblaye.	Jæg.	Jæger.
Al. Brong.	Alexandre Brongniart.	Lam^x.	Lamouroux.
Ad. Brong.	Adolphe Brongniart.	Murch.	Murchison.
Buckl.	Buckland.	Phil.	Phillips.
Conyb.	Conybeare.	Sow.	Sowerby.
Cuv.	Cuvier.	Sternb.	Sternberg.
DeC. or De Cau.	De Caumont.	Y. & B.	Young and Bird.
Desn.	Desnoyers.	De la B.	De la Beche.

3. Zamia

3. Zamia longifolia (*Ad. Brong.*). *Coal, shale, &c. between cornbrash and great oolite.* Yorks. (Phil.).
4. ——— pennæformis (*Ad. Brong.*). *Coal, shale, &c. between great and inferior oolite.* Yorks. (Phil.).
5. ——— elegans (*Ad. Brong.*). *Coal, shale, &c. between great and inferior oolite.* Yorks. (Phil.).
6. ——— Goldiæi (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.)
7. ——— acuta (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.)
8. ——— lævis (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.).
9. ——— Youngii (*Ad. Brong.*). *Coal, shale, &c. between great and inferior oolite.* Yorks. (Phil.).
10. ——— Feneonis (*Ad. Brong.*). *Coal, &c. of the oolitic series.* Yorks. (Ad. Brong.).
11. ——— Mantelli (*Ad. Brong.*). *Coal, shale, &c. between great and inferior oolite.* Yorks. (Phil.).

1. Zamites Bechii (*Ad. Brong.*). *Forest marble.* Mamers (Desn.). *Lias.* Lyme Regis (De la B.).
2. ——— Bucklandii (*Ad. Brong.*). *Forest marble.* Mamers (Desn.). *Lias.* Lyme Regis (De la B.).
3. ——— lagotis (*Ad. Brong.*). *Forest marble.* Mamers (Desn.).
4. ——— hastata (*Ad. Brong.*). *Forest marble.* Mamers (Desn.).

Coniferæ.

1. Thuytes divaricata (*Sternb.*). *Stonesfield slate* (Buckl.).
2. ——— expansa (*Sternb.*). *Stonesfield slate* (Buckl.).
3 ——— acutifolia (*Ad. Brong.*). *Stonesfield slate* (Buckl.).
4. ——— cupressiformis (*Sternb.*). *Stonesfield slate* (Buckl.).

1. Taxites podocarpoides (*Ad. Brong.*). *Stonesfield slate* (Buckl.).*

MAMMA-

* Mr. Phillips has very kindly furnished me with the following synonyms of the plants noticed by him in the coal, shale, &c. of the Yorkshire oolitic series.

Phillips: *Geology of Yorkshire.*	Brongniart: *Prodrome, &c.*
Upper Shale, Coal, and Sandstone.	
Pl. 7. Fig. 17. Sphænopteris ? longifolia.	
18. ——— latifolia.	
19. Cycadites tenuicaulis.	
20. ——— comptus..........	Pterophyllum Williamsonis.
21. ——— sulcicaulis	Zamia longifolia.
22. ——— pecten.	
23. Unknown leaves.	
24. ——— leaf.	
25. Small vegetable bodies in groups.	
Pl. 8. Fig. 1. Strobilus?	
2. Winged seed.	
3. Lycopodites uncifolius	Lycopodites Williamsonis.
4. Aspleniopteris Nilsoni? *Sternb*.......	Tæniopteris latifolia?

5. Scolopen-

MAMMALIA.

Didelphys Bucklandi (*Broderip.*). *Stonesfield slate* (Buckl.).

REPTILES.

Pterodactylus macronyx (*Buckl.*). *Lias.* Lyme Regis (Buckl.).
——— species not known. *Stonesfield slate* (Buckl.).
Crocodilus Bollensis (*Jæg.*). *Lias.* Boll in Wurtemberg (Jæg.).
——— gavial, short-snouted. *Kim. clay.* Havre (Al. Brong.).
——— ——— long-snouted. *Kim. clay.* Havre (Al. Brong.).
Crocodile of Caen. *Great oolite* (De Cau.).
——— of Mans (*Cuv.*). *Great oolite* (Brong.).
——— remains, species not determined. *Lias.* Yorks. (Phil.). *Lias?* Lyme Regis (De la B.). *Cornbrash.* Engl. (Conyb.). *Stonesfield slate* (Buckl.). *Coral. oolite.* Yorks. (Phil.).
Megalosaurus Bucklandi. *Stonesfield slate* (Buckl.).
Megalosaurus, species not known. *Great oolite.* Normandy (De Cau.).
Geosaurus Bollensis (*Jæg.*). *Lias.* Boll (Jæg.).

	Phillips: *Geology of Yorkshire.*	Brongniart: *Prodrome, &c.*
	Upper Shale, Coal, and Sandstone—continued.	
Pl. 8. Fig.	5. Scolopendrium solitarium.	Tæniopteris vittata.
	6. Sphænopteris digitata.	
	7. Variety of ditto.	
	8. Pecopteris paucifolia.	
	9. Phyllites nervulosus. *Sternb.*	
	10. Pecopteris cæspitosa.	
	11. ——— crenifolia	Pecopteris polypodioides.
	12. ——— curtata.	
	13. Neuropteris lobifolia.	
	14. Pecopteris ligata............	Pecopteris denticulata.
	15. ——— recentior.	
	16. Pecopteris exilis.	
	17. ——— hastata	Pecopteris Whitbiensis.
	18. ——— branch.	
	Dicotyledonous wood.	
	Lower Shale, Coal, and Sandstone.	
Pl. 10. Fig.	1. Cycadites latifolius	Zamia Youngii.
	2. ——— gramineus ...	——— elegans.
	3. ——— lanceolatus...	——— Mantelli.
	4. ——— pectinoïdes...	——— pennæformis.
	5. Winged seed.	
	6. Sphænopteris lanceolata	Pachypteris lanceolata.
	7. Pecopteris curtata	Pecopteris Williamsonis?
	8. Sphænopteris stipata....	Sphænopteris hymenophylloïdes.
	9. Neuropteris lævigata....	Pachypteris ovata.
	10. Sphænopteris muscoides.	
	11. Thuytes expansa? *Sternb.*	Brachyphyllum.
	12. Flabellaria viminea? *Stern.*	
	13. Equisetum laterale	
	Equisetum columnare ...	Equisetum columnare.
Seed-vessel	1. Y. & B. Pl. 1. f. 2. } 2nd edit.	
	2. Y. & B. — f. 7. } 2nd edit.	
	3. Y. & B — f. 2. } 2nd edit.	

1. Plesiosaurus dolichodeirus (*Conyb.*). *Lias.* Lyme Regis, Bristol, &c.
2. ———— recentior (*Conyb.*). *Kim. clay.* Engl. (Conyb.). *Kim. clay.* Honfleur (Brong.).
3. ———— carinatus (*Cuv.*). *Great oolite.* Boulogne (Brong.).
4. ———— pentagonus (*Cuv.*). *Great oolite.* Ballon & Chaufour (Brong.).
5. ———— ? trigonus (*Cuv.*). *Great oolite.* Calvados (Brong.).
———— species not determined. *Oxford clay.* Stenay (Bobl.). *Oxford clay.* Calvados (De la B.).

1. Ichthyosaurus communis (*De la B.*). *Lias.* Lyme Regis, &c. Engl. (Conyb. &c.). *Lias.* Boll, Wurtemberg (Jæg.).
2. ———— platyodon (*De la B.*). *Lias.* Lyme Regis, &c. Engl. (Conyb. &c.). *Lias.* Boll, (Jæg.).
3. ———— tenuirostris (*De la B.*). *Lias.* Lyme Regis, &c. (Conyb. &c.). *Lias.* Boll (Jæg.).
4. ———— intermedius (*Conyb.*). *Lias.* Lyme Regis, &c. (Conyb. &c.). *Lias.* Boll (Jæg.).
———— species not determined. *Lias and inferior oolite.* Normandy (De Cau.). *Lias.* Yorks. (Phil.). *Oxford clay.* England (Conyb.). *Oxford clay.* Normandy (De la B.). *Great oolite.* Reugny (Brong.). *Coral. oolite.* Yorks. (Phil.). *Calc. grit.* Midl. Engl. (Conyb.). *Kim. clay.* Oxford (Buckl.). *Kim. clay.* Weymouth (De la B.). *Kim. clay.* Honfleur (Brong.).

Saurian bones occur in the *Kelloway rock and Bath oolite.* Yorks. (Phil.). in the *Portland stone* (Buckl. & De la B.).

Tortoise. *Stonesfield slate* (Buckl). *Lias.*? Engl. (Conyb.).

INSECTS.

Elytra of coleopterous insects (*Leach*). *Stonesfield slate* (Buckl.).

FISH.

Dapedium politum (*De la B.*). *Lias.* Lyme Regis (De la B.). *Lias and Oxford clay* of Normandy (De Cau.).

Fish, species not yet determined, several in the *lias.* Lyme Regis (De la B.). Barrow, Leicestershire (Conyb.).

Ichthyodorulites (*Buckl. & De la B.*). Different kinds. *Lias.* Lyme Regis, and elsewhere in Southern and Midland Engl. (Conyb. & De la B.). *Kimmeridge clay.* Near Oxford (Buckl.). *Stonesfield slate* (Buckl.). In the *great oolite.* Normandy (DeCau.).

Fish palates and teeth. *Lias.* Lyme Regis and Somersethire, &c. (Conyb.). *Stonesfield slate* (Buckl.). *Great oolite.* Normandy (De Cau.) *Cornbrash and forest marble.* North of France (Bobl.). *Coral. oolite. Oxford clay.* Yorks. (Phil.).

CRUSTACEA.

Astacus rostratus (*Phil.*). *Kelloway rock and coral. oolite.* Yorks. (Phil.).
——— species not determined. *Oxford clay* & *Lias.* Yorks. (Phil.).

Crustacea, not yet determined. *Lias.* Midl. and S. Engl. (Conyb.). Lyme Regis (De la B.). *Forest marble.* Normandy (De Cau.). *Stonesfield slate* (Conyb.). *Bradford clay.* North of France (Bobl.).

ZOOPHYTA.

1. Spongia floriceps (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
——— *Lower calc. grit.* Yorks. (Phil.). *Inferior oolite.* Midl. and S. Engl. (Conyb.).

Alcyonium. *Forest marble.* Normandy (De Cau.).

1. Turbinolia

1. Turbinolia dispar (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
——— Species not stated. *Inferior oolite. Lias.* N. of France (Boblaye).
1. Turbinolopsis ochracea (*Lam*x.). *Forest marble.* Normandy (De Cau.).
1. Entalophora cellarioïdes (*Lam*x.). *Forest marble.* Normandy (De Cau.).
1. Limnorea mamillaris (*Lam*x.). *Forest marble.* Normandy (De Cau.).
1. Caryophyllia cylindrica (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
2. ——— truncata (*Lam*x.). *Forest marble.* Normandy (De Cau.).
3. ——— Brebissonii (*Lam*x.). *Forest marble.* Normandy (De Cau.).
4. ——— convexa (*Phil.*). *Inferior oolite.* Yorks. (Phil.).
5. ——— like C. cespitosa (*Ellis*). *Coralline oolite.* Yorks. (Phil.). *Coral rag.* Midl. and S. Engl. (Conyb.).
6. ——— like C. flexuosa (*Ellis*). *Coralline oolite.* Yorks. (Phil.). *Great oolite.* Midl. and S. Engl. (Conyb.).
7. ——— approaching C. carduus (*Park.*). *Coral rag, great oolite.* Midl. and S. Engl. (Conyb.).

Caryophyllia, species not stated. *Inferior oolite.* N. of France (Bobl.). *Rochelle beds.* (Dufr.). Numerous remains, *Coral rag.* Normandy (De Cau.). *Inferior and great oolite.* Midl. and S. Engl. (Conyb.).

1. Millepora dumetosa (*Lam*x.). *Forest marble.* Normandy (De Cau.).
2. ——— corymbosa (*Lam*x.). *Forest marble.* Normandy (De Cau.).
3. ——— conifera (*Lam*x.). *Forest marble.* Normandy (De Cau.).
4. ——— pyriformis (*Lam*x.). *Forest marble.* Normandy (De Cau.).
5. ——— macrocaule (*Lam*x.). *Forest marble.* Normandy (De Cau.).
6. ——— straminea (*Phil.*). *Great oolite and cornbrash.* Yorks. (Phil.).
——— species not stated. *Cornbrash and forest marble.* N. of France (Bobl.). *Forest marble.* Mamers, Normandy (Desn.).

Favosites .... *Forest marble.* Mamers, Normandy (Desn.).

1. Astrea favosioïdes (*Smith*). *Coralline oolite.* Yorks. (Phil.). *Coral rag and great oolite.* Midl. and S. England (Conyb.).
2. ——— inæqualis. *Coral. oolite.* Yorks. (Phil.).
3. ——— micastron. *Coral. oolite.* Yorks. (Phil.).
4. ——— arachnoides (*Flem.*). *Coral. oolite.* Yorks. (Phil.).
5. ——— tubulifera (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
6. ——— resembling A. siderea. *Inferior oolite.* Midl. and S. Engl. (Conyb.).

Astrea, species not stated. *Coral rag.* Normandy, numerous (De Cau.). *Great oolite.* Midl. and S. Engl. (Conyb.). *Lias.* Western Islands, Scotl. (Murch.).

Cellepora .... *Inferior oolite.* Midl. and S. Engl. (Conyb.).

1. Fungia orbulites (*Lam*x.). *Forest marble.* Normandy (De Cau.).
——— species not stated. *Inferior oolite.* Midl. and S. Engl. (Conyb.).
1. Spiropora tetragona (*Lam*x.). *Forest marble.* Normandy (De Cau.).
2. ——— cespitosa (*Lam*x.). *Forest marble.* Normandy (De Cau.).
3. ——— elegans (*Lam*x.). *Forest marble.* Normandy (De Cau.).
4. ——— intricata (*Lam*x.). *Forest marble.* Normandy (De Cau.).

Cyclolites elliptica (*Lam.*). *Inferior oolite.* Midl. and S. Engl. (Conyb.).
——— species not stated. *Bradford clay.* Midl. and S. Engl. (Conyb.).

1. Eunomia radiata (*Lam*x.). *Forest marble.* Normandy (De Cau.).
——— or Tubipora. *Great oolite.* Yorks. (Phil.).
1. Chrysaora damæcornis (*Lam*x.). *Forest marble.* Normandy (De Cau.).
2. ——— spinosa (*Lam*x.). *Forest marble.* Normandy (De Cau.).
1. Theonoa chlatrata (*Lam*x.). *Forest marble.* Normandy (De Cau.).
1. Idmonea triquetra (*Lam*x.). *Forest marble.* Normandy (De Cau.).

 1. Alecto

1. Alecto dichotoma (*Lam*x.). *Bradford clay*. S. Engl. (Conyb.). *Forest marble*. Normandy (De Cau.).
——— species not stated. *Inferior oolite*. Midl. and S. Engl. (Conyb.).
1. Berenicea diluviana (*Lam*x.). *Bradford clay*. S. Engl. (Conyb.). *Forest marble*. Normandy (De Cau.).
1. Terebellaria ramosissima (*Lam*x.). *Bradford clay*. S. Engl. (Conyb.). *Forest marble*. Normandy (De Cau.).
2. ——— antilope (*Lam*x). *Forest marble*. Normandy (De Cau.).
Retipora? .... *Great oolite*. Yorks. (Phil.).
Madrepora .... various and abundant. *Bradford clay*. N. of France (Bobl.). *Coral rag*. Normandy (De Cau.). *Great oolite*. N. of France (Bobl.). *Portland stone*. Wiltshire (Conyb.). *Inferior oolite*. Midl. and S. Engl. (Conyb.). Mauriac beds, S. of France (Dufr.).
1. Cellaria Smithii. *Cornbrash*. Yorks. (Phil.).
Meandrina. *Inferior oolite and coralline oolite*. Yorks. (Phil.). *Inferior oolite?* (Conyb.).
Eschara......*Forest marble*. Normandy (De Cau.).
Thamnasteria Lamourouxii (*Le Sauvage*.). *Coral rag*. Normandy (De Cau.).
Explanaria mesenterina (*Lam*x.). *Inferior oolite*. Midl. and S. Engl. (Conyb.).
Polypifers, genera doubtful. *Lias* (rare). Lyme Regis (De la B.). *Lias* (rare). Yorks. (Phil.). *Lias* (rare). Normandy (De Cau.). *Coral rag* (numerous). N. of France (Bobl.). *Coral rag* (abundant). Burgundy (Beaum.). *Coral rag* (abundant). S. of France (Dufr.).

RADIARIA.

1. Cidaris florigemma (*Phil.*). *Coralline oolite*. Yorks. (Phil.).
2. ——— intermedia (*Park.*). *Coralline oolite*. Yorks. (Phil.).
3. ——— monilipora (*Y. & B.*). *Coralline oolite*. Yorks. (Phil.).
4. ——— vagans (*Phil.*). *Calcareous grit, cornbrash, and great oolite*. Yorks. (Phil.).
5. ——— papillata (*Park.*). *Coral rag*. Midl. and S. Engl. (Conyb.).
6. ——— diadema (*Park.*). *Coral rag*. Midl. and S. Engl. (Conyb.).
7. ——— subangularis (*Park.*). *Inferior oolite*. Midl. and S. England. (Conyb.).
8. ——— ornata. *Bradford clay*. N. of France (Bobl.).
9. Cydarites globatus (*Schl.*). *Coral rag*. N. of France (Bobl.).
Cidaris. *Inferior oolite*. Yorks. (Phil.). *Lias*. Lyme Regis (De la B.). *Cornbrash, Bradford clay, great oolite, inferior oolite, lias*. Midl. and S. Engl. (Conyb.). *Coral rag, forest marble*. Normandy (De Cau.).
——— spines. *Great oolite and lias*. Yorks. (Phil.). *Lias*. Midl. and S. Engl. (Conyb.). *Oolite beds*. Lower system. S. of France (Dufr.). *Coral rag*. Normandy (Desn.).
1. Echinus germinans (*Phil.*). *Coralline oolite, calcareous grit, and great oolite*. Yorks. (Phil.).
Echinus. *Coral rag*. N. of France (Bobl.).
1. Clypeus sinuatus (*Park.*). *Coralline oolite*. Yorks. (Phil.). *Coral rag, cornbrash, forest marble, great and inferior oolite*. Midl. and S. Engl. (Conyb.). *Forest marble*. Normandy (De Cau.).
2. ——— emarginatus (*Phil.*). *Coralline oolite*. Yorks. (Phil.).

3. Clypeus

3. Clypeus clunicularis (*Smith*). *Coralline oolite and cornbrash.* Yorks. (Phil.). *Coral rag, cornbrash, great oolite, inferior oolite.* Midl. and S. Engl. (Conyb.). *Forest marble.* Normandy (De Cau.).
4. ——— dimidiatus (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
5. ——— semisulcatus (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
6. ——— orbicularis (*Phil.*). *Cornbrash.* Yorks. (Phil.).
1. Spatangus ovalis (*Park.*). *Coralline oolite, calcareous grit, and Kelloway rock.* Yorks. (Phil.).
 Spatangus....... *Cornbrash and forest marble.* N. of France (Bobl.).
1. Clypeaster pentagonalis (*Phil.*). *Calcareous grit.* Yorks. (Phil.).
 Clypeaster...... *Coral rag.* Normandy (De Cau.).
1. Galerites depressus. *Coralline oolite. Calcareous grit. Cornbrash.* Yorks. (Phil.). *Oxford clay.* Normandy (Desn.).
2. ——— patella. *Oxford clay.* Normandy (Desn.).
1. Ananchites bicordata. *Oxford clay.* Normandy (Desn.).
1. Nucleolites scutata. *Oxford clay.* Normandy (Desn.).
2. ——— columbaria. *Cornbrash and forest marble.* N. of Fr. (Bobl.).
 Nucleolites. *Oxford clay.* N. of France (Bobl.).
 Echinites, genera not stated. *Inferior oolite.* Normandy (De Cau.). Spines of. *Coral rag.* Burgundy (Beaum.). Spines of. *Coral. rag.* N. of France (Bobl.). *Forest marble.* Mamers. Normandy (Desn.). Mauriac beds, S. of France (Dufr.).
 Asteria...... *Forest marble.* Normandy (De Cau.).
 Ophiura Milleri (*Phil.*). *Lias.* Yorkshire (Phil.).
1. Apiocrinites rotundus (*Mill.*). *Forest marble.* Normandy (De C.) *Bradford clay and great oolite.* Midl. and S. Engl. (Conyb.). *Forest marble* (Buckl.). *Great oolite.* Farque, Alsace (Brong.). *Forest marble.* Normandy (De Cau.).
2. ——— Pratii (*Gray*). *Great oolite.* Bath (Lonsdale).
1. Pentacrinites Caput Medusæ (*Mill.*). *Cornbrash and coralline oolite.* Yorkshire (Phil.). *Inferior oolite and lias.* Midl. and S. Engl. (Conyb.). *Lias.* Yorkshire (Phil.). *Lias.* Alsace, Gundershofen, Figeac (Brong.).
2. ——— subangularis (*Miller*). *Inferior oolite and lias.* Midl. and S. Engl. (Conyb.).
3. ——— Briareus (*Mill.*). *Lias.* Midl. and S. Engl. (Conyb.). *Lias.* Yorkshire (Phil.).
4. ——— subangularis (*Mill.*). *Lias.* S. Engl. (Conyb.).
5. ——— basaltiformis (*Mill.*). *Lias.* Midl. and S. Engl. (Conyb.). *Lias.* Alsace (Voltz).
6. tuberculatus (*Mill.*). *Lias.* Midl. and S. Engl. (Conyb.). *Lias.* Alsace (Voltz).
 Pentacrinites. *Forest marble.* Normandy (De C.). *Bradford clay.* N. of France (Bobl.). *Cornbrash, forest marble, great oolite.* Engl. (Conyb.). *Inf. oolite.* Wotton-under-Edge (Lonsdale).
 Crinoidea, genera not determined. *Inferior oolite and coral rag.* N. of France (Bobl.). Mauriac beds, S. of France (Dufr.). *Calc. grit.* Yorkshire (Phil.).

MOLLUSCA.

1. Pholas recondita (*Phil.*). *Coralline oolite.* Yorkshire (Phil.).
2. ——— ? compressa (*Sow.*). *Kim. clay.* Oxford (G. E. Smith).
1. Pholadomya Murchisoni (*Sow.*). *Sandstone, limestone and shale.* Invcr- brora,

brora, Scotl. (Murch.). *Coralline oolite? cornbrash.* Yorks. (Phil.). *Inf. oolite.* Normandy (De Cau.).

2. Pholadomya simplex (*Phil.*). *Calc. grit.* Yorks. (Phil.).
3. ——— deltoidea (*Sow.*). *Calc. grit.* Yorks. (Phil.). *Kell. rock and cornbrash.* Midl. and S. Engl. (Conyb.).
4. ——— obsoleta (*Phil.*). *Oxford clay and Kell. rock.* Yorks. (Phil.).
5. ——— ovalis (*Sow.*). *Cornbrash.* Yorks. (Phil.). *Portland-stone.* (Conyb.). *Oxford clay.* Normandy (De C.). *Kim. clay?* Angoulême. *Rochelle limestone* (Dufr.).
6. ——— acuticostata (*Sow.*). *Great oolite.* Yorks. (Phil.). *Kim. clay.* Cahors, S. of Fr. (Dufr.). *Kim. clay?* Angoulême (Dufr.).
7. ——— nana (*Phil.*). *Great oolite.* Yorks. (Phil.).
8. ——— producta (*Sow.*). *Great oolite?* Yorks. (Phil.) *Cornb., inf. oolite.* Midl. and S. Engl. (Conyb.).
9. ——— obliquata (*Phil.*). *Great oolite, inf. oolite, and lias.* Yorks. (Phil.).
10. ——— fidicula (*Sow.*). *Inf. oolite.* Yorks. (Phil.).
11. ——— lirata (*Sow.*). *Cornb.* Midl. and S. Engl. *Inf. oolite.* Dundry (Conyb.). *Lias.* Normandy (De C.).
12. ——— obtusa (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
13. ——— ambigua (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Oxford clay.* Normandy (De C.). *Lias.* S. of France (Dufr.). *Lias.* Alsace (Voltz).
14. ——— æqualis (*Sow.*). *Inf. oolite.* Normandy (De C.).
15. ——— gibbosa (*Sow.*). *Lias.* Normandy (De Cau.)
16. ——— Proteii (*Brong.*). *Rochelle limestone* (Dufr.). *Kim. clay.* Havre, and the Jura (Brong.).

1. Panopæa intermedia (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
2. ——— gibbosa (*Sow.*). *Great oolite?* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.).

1. Mya literata (*Sow.*). *Coralline oolite, calc. grit, Oxford clay, Kelloway rock, cornb., inf. oolite, and lias.* Yorks. (Phil.). *Shale, sandstone, and limestone.* Inverbrora, Scotl. (Murch.).
2. —— depressa (*Sow.*). *Oxford clay?* Yorks. (Phil.). *Kim. clay?* Angoulême (Dufr.). *Kim. clay.* Havre (Phil.). *Shale, limestone, and sandstone.* Inverbrora, Scotl. (Murch.).
3. —— calceiformis (*Phil.*). *Kell. rock, great oolite, and inf. oolite.* Yorks. (Phil.).
4. —— dilata (*Phil.*). *Inferior oolite.* Yorks. (Phil.).
5. —— æquata (*Phil.*). *Inferior oolite.* Yorks. (Phil.).
6. —— V. scripta (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Great oolite.* Alsace (Brong.). *Micaceous sandstone.* Western Islands, Scotl. (Murch.).
7. —— mandibulata (*Sow.*). *Kim. clay?* Env. of Angoulême (Dufr.).

1. Sanguinolaria undulata (*Sow.*). *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.). *Calc. grit, Oxford clay, and cornbrash.* Yorks. (Phil.).
2. ——— elegans (*Phil.*). *Lias.* Yorks. (Phil.).

Sanguinolaria. *Lias.* Ross and Cromarty, Scot. (Murch.). *Lias.* Yorks. (Phil.).

1. Crassina ovata (*Smith*). *Coralline oolite.* Yorks. (Phil.).
2. ——— elegans (*Sow.*). *Coralline oolite and inf. oolite.* Yorks. (Phil.).
3. ——— aliena (*Phil.*). *Coralline oolite.* Yorks. (Phil.).

4. Crassina

4. Crassina extensa (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
5. ——— carinata (*Phil.*) *Calc. grit, Oxford clay, and Kell. rock.* Yorks. (Phil.).
6. ——— lurida (*Sow.*). *Oxford clay.* Yorks. (Phil.).
7. ——— minima (*Phil.*). *Great oolite, inf. oolite, lias.* Yorks. (Phil.).
1. Amphidesma decurtatum (*Phil.*). *Cornb., great oolite.* Yorks. (Phil.).
2. ——— recurvum. (*Phil.*) *Coralline oolite? Kell. rock.* Yorks. (Phil.). *Kim. clay.* Havre (Phil.).
3. ——— securiforme (*Phil.*). *Cornb., inf. oolite.* Yorks. (Phil.). *Kim. clay.* Havre (Phil.).
4. ——— donaciforme (*Phil.*). *Lias.* Yorks. (Phil.).
5. ——— rotundatum (*Phil.*). *Lias.* Yorks. (Phil.).
1. Lutraria Jurassi (*Brong.*) *Forest marble.* Ligny, Meuse (Brong.).
1. Gastrochæna tortuosa (*Sow.*). *Inf. oolite.* Yorks. (Phil.).
1. Psammobia lævigata (*Phil.*). *Coralline oolite, great oolite, and inf. oolite.* Yorks. (Phil.).
1. Lucina crassa (*Sow.*). *Sandstone and rubbly limestone.* Braambury Hill, Brora (Murch.). *Calc. grit.* Yorks. (Phil.). Lincolnshire (Sow.). *Great arenaceous formation.* Western Islands, Scotl. (Murch.).
2. ——— lirata (*Phil.*). *Kell. rock.* Yorks. (Phil.).
3. ——— despecta (*Phil.*). *Great oolite.* Yorks. (Phil.).

Lucina, species not stated. *Coral rag and for. marb.* Norm. (De Cau.). *Inf. oolite.* Yorks. (Phil.). *Shale, &c.* Inverbrora, Scotl. (Murch.).

1. Unio peregrinus (*Phil.*). *Cornb.* Yorks. (Phil.).
2. ——— abductus (*Phil.*). *Inferior oolite and lias.* Yorks. (Phil.).
3. ——— concinnus (*Sow.*). *Lias.* Yorkshire (Phil.) *Inf. oolite.* Mid. and S. Eng. (Conyb.).
4. ——— crassiusculus (*Sow.*). *Lias.* Yorks. (Phil.).
5. ——— Listeri (*Sow.*). *Lias.* Yorks. (Phil.). *Inf. oolite.* Mid. and S. Eng. (Conyb.).
6. ——— acutus (*Sow.*). *Cornb.* Mid. and S. Eng. (Conyb.).
7. ——— crassissimus (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.). *For. marb.?* Mauriac, *and inf. oolite.* Uzer, S. of Fr. (Dufr.).

——— species not stated. *Portland stone.* (Conyb.).

1. Pullastra recondita (*Phil.*). *Great oolite.* Yorks. (Phil.).
2. ——— oblita (*Phil.*). *Inferior oolite.* Yorks. (Phil.).

———, species not named. *Lias.* Yorks. (Phil.).

Venus, species not named. *Coralline oolite, calc. grit, and lias.* Yorks. (Phil.). *Portland stone.* (Smith.). *Coral rag.* Norm. (De C.). *Sandst., shale, &c.* Inverbrora, Scotl. (Murch.).

Cytherea dolabra (*Phil.*). *Great oolite.* Yorks. (Phil.).

———, species not named. *Coralline oolite.* Yorks. (Phil.). *Lias.* N. of Fr. (Bobl.).

1. Corbis lævis (*Sow.*). *Coralline oolite? Kell. rock?* Yorks. (Phil.).
2. ——— ovalis (*Phil.*). *Kell. rock.* Yorks. (Phil.).
1. Tellina ampliata (*Phil.*). *Coralline oolite.* Yorks. (Phil.).

1. Astarte

1. Astarte cuneata (*Sow.*). *Portland stone.* S. Eng. *Inf. oolite?* Dundry (Conyb.).
2. ——— excavata (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
3. ——— lurida (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
4. ——— ovata (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
5. ——— planata (*Sow.*). *Inf. oolite.* Norm. (De C.). *Bradf. clay.* N. of Fr. (Bobl.).
6. ——— rugata (*Sow.*). *Inf. oolite.* Norm. (De C.).
7. ——— imbricata (*Sow.*). *Inf. oolite.* Norm. (De C.).
8. ——— orbicularis (*Sow.*). *Great oolite.* Bath (Sow.).
9. ——— trigonalis (*Sow.*). *Inf. oolite.* Dundry.
10. ——— orbicularis (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
11. ——— pumila (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson). *Rochelle limestone* (Dufr.).
12. ——— elegans (*Sow.*). *Rochelle limestone* (Dufr.). *Shell limest. and calc. grit.* Portgower, &c. *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).

Astarte ...... *Lias.* Mid. and S. Eng. (Conyb.).

1. Corbula curtansata (*Phil.*). *Coralline oolite and Kell. rock.* Yorks. (Phil.).
2. ——— depressa (*Phil.*). *Great oolite.* Yorks. (Phil.).
3. ——— ? cardioïdes (*Phil.*). *Lias.* Yorks. (Phil.).
4. ——— obscura (*Sow.*). Brora (Murch.).

1. Cardium lobatum (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
2. ——— dissimile (*Sow.*). *Kell. rock.* Yorks. (Phil.). *Portland stone.* Portland (Sow.). *Rocks of the oolite series.* Braambury Hill, Brora (Murch.).
3. ——— citrinoïdeum (*Phil.*). *Cornb.* Yorks. (Phil.).
4. ——— cognatum (*Phil.*). *Great oolite.* Yorks. (Phil.).
5. ——— acutangulum (*Phil.*). *Great oolite and inf. oolite.* Yorks. (Phil.).
6. ——— semiglabrum (*Phil.*). *Great oolite.* Yorks. (Phil.).
7. ——— incertum (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
8. ——— striatulum (*Sow.*). *Sandst., limest. and shale.* Inverbrora, Scotl. (Murch.). *Inf. oolite.* Yorks. (Phil.).
9. ——— gibberulum (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
10. ——— truncatum (*Sow.*). *Lias.* Yorks. (Phil.). *Sandst., limest., &c.* Inverbrora? (Murch.).
11. ——— multicostatum (*Bean*). *Lias.* Yorks. (Phil.).

1. Isocardia rhomboidalis (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
2. ——— tumida (*Phil.*). *Calc. grit.* Yorks. (Phil.).
3. ——— minima (*Sow.*). *Cornb. and great oolite?* Yorks. (Phil.).
4. ——— concentrica (*Sow.*). *Great oolite and inf. oolite.* Yorks. (Phil.). *Oxford clay.* Norm. (De C.). *Cornb.* Northamptonshire (Sow.).
5. ——— angulata (*Phil.*). *Great oolite?* Yorks. (Phil.).
6. ——— rostrata (*Sow.*). *Inf. oolite.* Yorks. (Phil.).

———, species not mentioned. *For. marb.* Norm. (De C.).

1. Cardita

1. Cardita similis (*Sow.*). *Coralline oolite, great oolite, and inf. oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.).
2. —— lunulata (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
3. —— striata. *Lias.* Norm.? (De C.).
—— species not mentioned. *Portland stone* (Conyb.).
1. Trigonia costata (*Sow.*). *Coralline oolite, great oolite, and inf. oolite.* Yorks. (Phil.). *Cornb., for. marb., and Brad. clay.* Mid. and S. Engl. *Inf. oolite.* Dundry (Conyb.). *Oxford clay, for. marb., and inf. oolite.* Norm. (De C.). *Oxford clay.* N. of Fr. (Bobl.).
2. —— clavellata (*Sow.*). *Coralline oolite, Kell. rock, and cornb.* Yorks. (Phil.). *Portland stone and cornb.* Mid. and S. Engl. *Inf. oolite.* Dundry (Conyb.). *Oxford clay.* Norm. (De la B.). *Oxford clay.* N. of Fr. (Bobl.). *Kim. clay?* Angoulême (Dufr.). *Sandst., shale, &c.* Inverbrora, Scotl. (Murch.).
3. —— conjungens (*Phil.*). *Great oolite.* Yorks. (Phil.).
4. —— striata (*Sow.*). *Inferior oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.).
5. —— angulata (*Sow.*). *Inf. oolite.* Yorks. (Phil.) *Inf. oolite.* Near Frome (Sow.).
6. —— literata (*Y. & B.*). *Lias.* Yorkshire (Phil.).
7. —— gibbosa (*Sow.*). *Portland stone* (Conyb.). *Forest marb.* Norm. (De C.).
8. —— duplicata (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.). *For. marb.* Norm. (De C.).
9. —— elongata (*Sow.*). *Oxford clay.* Norm. (De C.). *Oxford clay.* Eng. (Sow.). *Great oolite.* Alsace (Voltz).
10. —— imbricata (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).
11. —— cuspidata (*Sow.*). *Great oolite.* Ancliff (Cookson).
12. —— pullus (*Sow.*). *Great oolite.* Ancliff (Cookson).
—— species not stated. *Coral rag.* Mid. and S. Eng. (Conyb.). *Coral rag.* Norm. (De C.).
1. Hippopodium ponderosum (*Sow.*). *Coralline oolite and lias.* Yorks. (Phil.). *Lias.* Mid. and S. Eng. (Conyb.).
1. Nucula elliptica (*Phil.*). *Oxford clay.* Yorks. (Phil.).
2. —— nuda (*Y. & B.*). *Oxford clay.* Yorks. (Phil.).
3. —— variabilis (*Sow.*). *Great oolite and inf. oolite.* Yorks. (Phil.). *Great oolite.* Ancliff, near Bath (Cookson).
4. —— lachryma (*Sow.*). *Great oolite and inf. oolite.* Yorks. (Phil.).
5. —— axiniformis (*Phil.*). *Inferior oolite.* Yorks. (Phil.).
6. —— ovum (*Sow.*). *Lias.* Yorks. (Phil.).
7. —— pectinata (*Sow.*). *Oxford clay.* Norm. (De C.).
8. —— clariformis. *Lias.* S. of Fr. (Dufr.).
9. —— mucronata (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
—— species not stated. *Coralline oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry. *Lias.* Mid. and S. Eng. (Conyb.).

 1. Cucullæa

1. Cucullæa oblonga (*Sow.*). *Coralline oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.).
2. ——— contracta (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
3. ——— triangularis (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
4. ——— pectinata (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
5. ——— elongata (*Sow.*). *Coralline oolite? and great oolite.* Yorks. (Phil.). *Rochelle limestone* (Dufr.).
6. ——— concinna (*Phil.*). *Oxford clay and Kell. rock?* Yorks. (Phil.).
7. ——— imperialis (*Bean*). *Great oolite.* Yorks. (Phil.).
8. ——— cylindrica (*Phil.*). *Great oolite.* Yorks. (Phil.).
9. ——— cancellata (*Phil.*). *Great oolite.* Yorks. (Phil.).
10. ——— reticulata (*Bean*). *Inf. oolite.* Yorks. (Phil.).
11. ——— decussata (*Sow.*). *Inf. oolite.* Norm. (De C.).
12. ——— minuta (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
13. ——— rudis (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).

——— species not stated. *Lias.* Yorks. (Phil.). *Lias.* Mid. and S. Eng. (Conyb.).

1. Arca quadrisulcata (*Sow.*). *Coralline oolite.* Yorks. (Phil.).
2. —— æmula (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
3. —— pulchra (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson). *Rochelle limestone* (Dufr.).

—— species not stated. *Lias.* Mid. and S. Eng. (Conyb.).

1. Pectunculus minimus (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
2. ——— oblongus (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).

1. Crenatula ventricosa (*Sow.*). *Lias.* Yorks. (Phil.).

——— species not stated. *Portland stone* (Conyb.).

1. Inoceramus dubius (*Sow.*). *Lias.* Yorks. (Phil.).

1. Modiola imbricata (*Sow.*). *Coralline oolite? and great oolite.* Yorks. (Phil.). *Cornb.* Mid. and S. Eng. (Conyb.).
2. ——— ungulata (*Y. & B.*). *Coralline oolite, great oolite, and inf. oolite.* Yorks. (Phil.).
3. ——— bipartita (*Sow.*). *Calc. grit.* Yorks. (Phil.). *Sandstone and limestone.* Braambury Hill, Brora (Murch.).
4. ——— cuneata (*Sow.*). *Oxford clay, Kell. rock? and cornb.* Yorks. (Phil.). *Inf. oolite.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.). *Lias.* Western Islands, Scotl. *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).
5. ——— pulchra (*Phil.*). *Kell. rock.* Yorks. (Phil.). *Oolite.* Sutherland.
6. ——— plicata (*Sow.*). *Inf. oolite.* Yorks. (Phil.). *Cornb.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.).
7. ——— aspera (*Sow.*). *Inf. oolite.* Yorks. (Phil.). *Cornb.* Mid. and S. Eng. (Conyb.).
8. ——— scalprum (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* S. of Fr. (Dufr.).
9. ——— Hillana (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Mid. and S. Eng. (Conyb.).
10. ——— lævis (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.).

11. Modiola

11. Modiola depressa (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.).
12. ——— minima (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.).
13. ——— subcarinata (*Lam.*). *Oxford clay.* Norm. (De C.).
14. ——— elegans (*Sow.*). *For. marb.* Norm. (De C.).
15. ——— tulipea (*Lam.*). *Oxford clay.* N. of Fr. (Bobl.).
16. ——— palida (*Sow.*). *Shale and grit.* Dunrobin Reefs, &c. Scotl. (Murch.).
1. Mytilus cuneatus (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
2. ——— amplus. *Great oolite.* Norm. (De C.).
3. ——— pectinatus (*Sow.*). *Kim. clay.* Weymouth (Sedgwick). *Rochelle limestone* (Dufr.).
4. ——— sublævis (*Sow.*). *Cornb.* Eng. (Sow.).
5. ——— solenoïdes. *Kim. clay.* Cahors, S. of Fr. (Dufr.).
——— species not stated. *Coral rag, inf. oolite.* Mid. and S. Eng. (Conyb.). *Coral rag.* Norm. (De C.).
1. Trigonellites antiquatus (*Phil.*). *Coralline oolite.* Yorks. (Phil.).
2. ——— politus. (*Phil.*). *Oxford clay.* Yorks. (Phil.).
1. Mactra gibbosa. *For. marb.* Norm. (De C.).
1. Pinna lanceolata (*Sow.*). *Coralline oolite and calcareous grit.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Lias.* Norm. (De C.). *Oxford clay.* N. of Fr. (Bobl.).
2. —— mitis (*Phil.*). *Oxford clay and Kell. rock?* Yorks. (Phil.).
3. —— cuneata (*Bean*). *Cornb. and great oolite.* Yorks. (Phil.).
4. —— folium (*Y. & B.*) *Lias.* Yorks. (Phil.).
5. —— pinnigena. *Coral rag, for. marb., and inf. oolite.* Norm. (De C.).
6. —— tetragona (*Brocchi*). *Oxford clay.* Norm. (De C.).
7. —— granulata (*Sow.*). *Kim. clay.* Weymouth (Sedgwick). *Kim. clay.* Cahors., S. of Fr. (Dufr.). *Lias.* Skye (Murch.).
1. Perna quadrata (*Sow.*). *Coralline oolite, Kell. rock, and great oolite.* Yorks. (Phil.). *Cornb.* Bulwick (Sow.).
—— species not named. *Oxford clay.* Yorks. (Phil.).
1. Gervillia aviculoides (*Sow.*). *Coralline oolite.* Yorks. *Calcareous grit.* Oxfordshire (Phil.). *Oxford clay.* Mid. and S. Eng. *Inf. oolite.* Dundry Hill (Conyb.). *Oxford clay.* Norm. (De la B.). *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).
2. ——— acuta (*Sow.*). *Great oolite.* Yorks. (Phil.).
3. ——— lata (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
4. ——— pernoides (*Desl.*). *Oxford clay, for. marb., great oolite, and inf. oolite.* Norm. (De C.).
5. ——— siliqua (*Desl.*). *Oxford clay and for. marb.* Norm. (De C.).
6. ——— monotis (*Desl.*). *For. marb.* Norm. (De C.).
7. ——— costellata (*Desl.*). *For. marb.* Norm. (De C.).
——— species not stated. *Coral rag.* Norm. (De C.).
1. Avicula expansa (*Phil.*). *Coralline oolite, Oxford clay? Kell. rock, and great oolite.* Yorks. (Phil.).
2. ——— ovalis (*Phil.*). *Coralline oolite and calc. grit.* Yorks. (Phil.).
3. ——— elegantissima (*Bean*). *Coralline oolite.* Yorks. (Phil.).
4. ——— tonsipluma (*Y. & B.*). *Coralline oolite.* Yorks. (Phil.).
5. ——— Braamburiensis (*Sow.*). *Sandstone.* Braambury Hill, Brora (Murch.).

(Murch.). *Kell. rock, great oolite, and inf. oolite.* Yorks. (Phil.).

6. Avicula inæquivalvis (*Sow.*). *Inf. oolite and lias.* Yorks. (Phil.). *Kell. rock.* Mid. and S. Eng. (Conyb.). *Great oolite and inf. oolite.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.). *Great arenaceous formation.* Western Islands: and *shell limest. and grit.* Portgower, Scotland (Murch.). *Lias.* Lyme Regis (De la B.).
7. ——— echinata (*Sow.*). *Lias?* Yorks. (Phil.). *Cornb.* Mid. and S. Eng. (Conyb.). *For. marb.* Norm. (De C.). *Brad. clay, cornb., and for. marb.* N. of Fr. (Bobl.).
8. ——— cygnipes (*Y. & B.*). *Lias.* Yorks. (Phil.). *Lias.* Western Islands, Scotl. (Murch.).
9. ——— costata (*Sow.*). *Cornb. and Brad. clay.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.). *For. marb.* Norm. (De C.).
10. ——— lanceolata (*Sow.*). *Lias.* Lyme Regis (De la B.).
11. ——— ovata (*Sow.*). *Stonesfield slate* (Sow.).
1. Plagiostoma læviusculum (*Sow.*). *Coralline oolite.* Yorks. *Coral rag and calcareous grit.* Oxon (Phil.). *Coral rag.* Marthon, S. of Fr. (Dufr.).
2. ——— rigidum (*Sow.*). *Coralline oolite.* Yorks. *Coral rag.* Oxon (Phil.). *Inf. oolite.* Dundry (Conyb.). *Coral rag.* N. of Fr. (Bobl.).
3. ——— rusticum (*Sow.*). *Coralline oolite.* Yorks. *Calc. grit.* Oxon (Phil.).
4. ——— duplicatum (*Sow.*). *Coralline oolite, Oxford clay, and Kell. rock.* Yorks. (Phil.). *Inf. oolite.* Norm. (De C.). *Dunrobin oolite.* Scotl. (Murch.).
5. ——— rigidulum (*Phil.*). *Cornbrash.* Yorks. (Phil.).
6. ——— interstinctum (*Phil.*). *Cornb. and great oolite.* Yorks. (Phil.).
7. ——— cardiiforme (*Sow.*). *Great oolite?* Yorks. (Phil.). *Cornb. and for. marb.* N. of Fr. (Bobl.).
8. ——— giganteum (*Sow.*). *Inf. oolite and lias.* Yorks. (Phil.). *Inf. oolite.* Dundry? *Lias.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.). *Lias.* N. of Fr. (Bobl.). *Lias.* Western Islands, Scotl. (Murch.).
9. ——— obscurum (*Sow.*). *Kell. rock.* Mid. and S. Eng. (Conyb.).
10. ——— pectinoïdes (*Sow*). *Lias.* Yorks. (Phil.). *Shale and grit.* Reefs at Dunrobin, Scotl. (Murch.).
11. ——— punctatum (*Sow.*). *Inf. oolite.* Dundry. *Lias.* Mid. and S. Eng. (Conyb.). *For. marb. and inf. oolite.* Norm. (De C.). *Lias.* N. of Fr. (Bobl.). *Lias.* S. of Fr. (Dufr.). *Lias.* Western Islands, Scotl. (Murch.).
12. ——— sulcatum. *Lias.* S. of France. (Dufr.)
13. ——— ovale (*Sow.*). *For. marb.?* Mauriac, S. of Fr. (Dufr.).
14. ——— Hermanni (*Voltz*). *Lias.* Alsace (Voltz).
15. ——— obliquatum (*Sow.*). *Sandstone and limestone.* Braambury Hill, Brora. *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).
16. ——— acuticosta (*Sow.*). *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).

17. Plagiostoma concentricum (*Sow.*). *Lias.* Ross and Cromarty, Scotl. (Murch.).

——— species not stated. *Bradford clay and great oolite.* Mid. and S. Eng. (Conyb.).

1. Pecten abjectus (*Phil.*). *Coral rag.* Yorks. and Oxon. *Calc. grit, great oolite, and inf. oolite.* Yorks. (Phil.).
2. —— inæquicostatus (*Phil.*). *Coralline oolite.* Yorks. *Calc. grit.* Oxon (Phil.).
3. —— cancellatus (*Bean*). *Coralline oolite.* Yorks. *Oolite.* Sutherland? (Phil.).
4. —— demissus (*Phil.*). *Coralline oolite, Kell. rock, cornbrash, and great oolite.* Yorks. (Phil.).
5. —— lens (*Sow.*). *Coralline oolite, Kell. rock, great oolite, inf. oolite, and lias.* Yorks. (Phil.). *Coral rag.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.). *Coral rag and Oxford clay.* Norm. (De C.). *Cornb. and for. marb.* N. of Fr. (Bobl.). *Inf. oolite.* Alsace, and Stranen near Luxembourg (Al. Brong.). *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).
6. —— viminalis (*Sow.*). *Coral rag.* Yorks., Oxon., and Wilts (Phil.).
7. —— vagans (*Sow.*). *Coral rag.* Yorks. and Oxon. *Calc. grit.* Yorks. (Phil.). *For. marb.* Norm. (De C.). *Sandst. and rubbly limest.* Braambury Hill, Brora (Murch.).
8. —— fibrosus (*Sow.*). *Kell. rock and cornbrash.* Yorks. (Phil.). *Coral rag, Kell. rock, cornb., for. marb., Brad. clay, and inf. oolite.* Mid. and S. Eng. (Conyb.). *Coral rag.* Norm.? (De C.). *Cornb. and for. marb.* N. of Fr. (Bobl.). *For. marb.?* Mauriac, S. of Fr. (Dufr.). *Rubbly limestone, &c.* Braambury Hill, Brora (Murch.).
9. —— virguliferus (*Phil.*). *Inferior oolite.* Yorks. (Phil.).
10. —— sublævis (*Y. & B.*). *Lias.* Yorks. (Phil.).
11. —— equivalvis (*Sow.*). *Lias.* Yorks. (Phil.) *Inf. oolite.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.). *Lias.* Western Islands, Scotl. (Murch.).
12. —— lamellosus (*Sow.*). *Portland stone* (Conyb.).
13. —— arcuatus (*Sow.*). *Coral rag.* Mid. and S. Eng. (Conyb.).
14. —— similis (*Sow.*). *Coral rag.* Mid. and S. Eng. (Conyb.). *Coral rag.* Norm.? (De C.).
15. —— laminatus (*Sow.*). *Cornb.* Mid. and S. Eng. (Conyb.).
16. —— barbatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Lias.* Norm. (De C.).
17. —— vimineus (*Sow.*). *Oxford clay, for. marb., and inf. oolite.* Norm. (De C.). *Forest marble.* Malton (Sow.). *Rubbly limestone, &c.* Braambury Hill, Brora (Murch.).
18. —— corneus (*Sow.*). *For. marb., great oolite, and inf. oolite.* Norm. (De C.).
19. —— obscurus (*Sow.*). *For. marb.?* Mauriac, S. of Fr. (Dufr.).
20. —— annulatus (*Sow.*). *Cornb.* Felmersham (Marsh).

—— species not named, many in various beds.

1. Lima rudis (*Sow.*). *Coralline oolite, calc. grit, Kell. rock, and great oolite.* Yorks. (Phil.). *Coral rag.* Mid. and S. Eng. (Conyb.).

(Conyb.). *Coral rag.* N. of Fr. (Bobl.). *Rubbly limestone, &c.* Braambury Hill, Brora (Murch.).

2. Lima proboscidea (*Sow.*). *Inf. oolite?* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Oxford clay, for. marb., and inf. oolite.* Norm. (De C.).

3. —— gibbosa (*Sow.*). *Cornb. and inf. oolite.* Mid. and S. Eng. (Conyb.). *Great oolite and inf. oolite.* Norm. (De C.).

4. —— antiqua (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.). *Lias.* S. of Fr. (Dufr.).

1. Exogyra digitata (*Sow.*). *Kell. rock.* Mid. and S. Eng. (Conyb.).

1. Chama mima or Gryphæa mima (*Phil.*). *Coral oolite and calc. grit.* Yorks. (Phil.).

2. ——— crassa (*Sow.*). *Bradford clay.* Mid. and S. Eng. (Conyb.).

1. Plicatula spinosa (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.). *Inf. oolite.* N. of Fr. (Bobl.). *Great arenaceous formation.* Western Islands, Scotl. (Murch.).

1. Ostrea gregarea (*Sow.*). *Coral rag.* Yorks., Wilts, &c. *Calc. grit and great oolite?* Yorks. (Phil.). *Coral rag.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.). *Coral rag and Oxford clay.* Norm. (De C.). *Oxford clay and coral rag.* N. of Fr. (Bobl.). *Kim. clay.* Havre. (Phil.).

2. —— solitaria (*Sow.*). *Coral rag and inf. oolite.* Yorks., Oxon, &c. (Phil.).

3. —— duriuscula (*Bean*). *Coralline oolite.* Yorks. (Phil.).

4. —— inæqualis (*Phil.*). *Oxford clay.* Yorks. (Phil.).

5. —— undosa (*Bean*). *Kell. rock.* Yorks. (Phil.).

6. —— archetypa (Phil.). *Kell. rock.* Yorks. (Phil.).

7. —— Marshii (*Sow.*). *Kell. rock, cornb., and great oolite.* Yorks. (Phil.). *Cornb. and Fuller's E.* Mid. and S. Eng. (Conyb.). *Oxford clay, for. marb. and inf. oolite.* Norm. (De C.).

8. —— sulcifera (*Phil.*). *Great oolite.* Yorks. (Phil.).

9. —— deltoidea (*Sow. & Smith*). *Kim. clay.* Yorks. (Phil.). *Oxford clay.* N. of Fr. (Bobl.). *Kim. clay.* S. and Mid. England. (Conyb.). *Shell limest. and calc. grit?* Portgower, &c. Scotl. (Murch.). *Kim. clay.* Havre (Phil.). *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.).

10. —— expansa (*Sow.*). *Portland stone* (Conyb.).

11. —— Crista Galli (*Smith*). *Coral rag, for. marb., Brad. clay, aud great oolite.* Mid. and S. Eng. (Conyb.). *Great oolite.* Norm. (De C.).

12. —— palmetta (*Sow.*). *Oxford clay.* Mid. and S. Eng. (Conyb.). *Oxford clay and for. marb.* Norm. (De C.).

13. —— acuminata (*Sow.*). *Bradford clay and inf. oolite.* Mid. and S. Eng. (Conyb.). *Great oolite and Brad. clay.* N. of Fr. (Bobl.).

14. —— rugosa (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.).

15. —— minima (*Desl.*). *Coral rag and Oxford clay.* Norm. (De C.).

16. —— plicatilis. *Oxford clay.* Norm. (De C.).

17. —— carinata (*Lam.*). *Oxford clay.* Norm. (De C.).

18. Ostrea

18. Ostrea costata (*Sow.*). *Brad. clay.* N. of Fr. (Bobl.). *Great oolite.* Ancliff, near Bath (Cookson).
19. —— pectinata. *Oxford clay.* N. of Fr. (Bobl.).
20. —— pennaria. *Oxford clay.* N. of Fr. (Bobl.).
21. —— flabelloides (*Lam.*). *Oxford clay.* N. of Fr. (Bobl.).
22. —— læviuscula (*Sow.*). *Lias.* Eng. (Sow.).
23. —— obscura (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
1. Gryphæa chamæformis (*Phil.*). *Calc. grit.* Yorks. And *Oolite* Sutherland (Phil.).
2. —— bullata (*Sow.*). *Coral. oolite? Calc. grit?* (Phil.). *Oxford clay.* Lincolnshire (Sow.). *Oolite* of Braambury Hill, Brora (Murch.).
3. —— inhærens (*Phil.*). *Calc. grit.* Yorks. (Phil.).
4. —— dilatata (*Sow.*). *Kell. rock.* Yorks. (Phil.). *Oxford clay.* Mid. and S. Eng. (Conyb.). *Oxford clay and lias.* Norm. (De C.). *Oxford clay.* N. of Fr. (Bobl.). *Oxford clay.* Burgundy (Beaum.). *Great arenaceous formation.* Western Islands, Scotl. (Murch.).
5. —— incurva (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.). *Lias and inf. oolite.* N. of Fr. (Bobl.). *Lias.* S. of Fr. (Dufr.). *Lias.* Metz, Salins, Amberg. (Brong.). *Lias.* Western Islands, Scotl. *Lias.* Ross and Cromarty, Scotl. (Murch.).
6. ——? nana (*Sow.*). *Lias and Oxford clay?* N. of Fr. (Bobl.).

[To be continued.]

L. *On* M. Guinand's *Glass for Telescopes. By* M. Utzschneider*.

I ADDRESS you, Sir, as a friend to truth, in order to remove some unfavourable impressions which have got abroad relative to my manufactory of glass for optical purposes.

It is stated in the *Bibliothèque Universelle* (November 1828, page 175) that the 9-inch object glass, belonging to the Dorpat telescope, made by Utzschneider and Fraunhofer, came from the crucibles of M. Guinand.

Several other journals also have repeated, from the *Globe* (French newspaper) of November 1828, that MM. Thibeaudeau and Bontemps had, in concert with M. Guinand the son, re-discovered the secret of producing flint glass of any magnitude, highly favourable for optical purposes; a secret which they pretend has been lost since the death of Fraunhofer, and Guinand the father: and that amongst the pieces presented to the Academy of Sciences, there were some of fourteen inches diameter.

* From Schumacher's *Astron. Nachrichten*, No. 163.

I do

I do not wish to occupy the public attention about my own affairs; but I am nevertheless obliged, by the interest which is attached to this singular discovery, to state some particulars relative to the residence of M. Guinand in my glass-house at Benedictbeurn. I have already spoken of it in 1826, in my *Life of Fraunhofer:* but it is necessary to recur to it again, in order to refute the reports which are circulated injurious not only to my establishment, but also to the memory of Fraunhofer.

Before M. Pierre Louis Guinand entered my service, I made him communicate to me every thing he knew, up to that time, as to the art of making glass: I also obtained from him a description of the small castings made by him since 1775: and I was convinced that his efforts would not have been attended with any advantage either to science, or to his own interests. M. Guinand renewed unsuccessfully his attempts, but was not the less received by me. His efforts directed me in the path which he ought to have pursued to obtain his object: and I therefore resolved to continue to work with him, after a settled plan, and to take advantage of every moment of leisure I could spare from my public duties to assist at his castings. We obtained some pieces of flint glass, with which we made object glasses for instruments forming in the manufactories of Reichenbach, Utzschneider and Liebherr. Our labours were only discontinued when I attended my public duties: I then charged M. Fraunhofer with the direction of the castings which were undertaken at my expense: and this excellent optician always gave me a written report of the experiments and castings that he had made.

M. Guinand announced to me, on December 6, 1823, that domestic affairs required his presence at Brennets: in fact, he left me some time after, and never returned again to Benedictbeurn.

The description of the castings of M. Guinand, written with his own hand, and still in my possession, proves that in 1805 he could not then make perfect flint glass: and that he would not have succeeded but for the experiments made with me at Benedictbeurn, and at my expense. Still the glass of the last casting, which was made at the commencement of the year 1814, was not equal in quality to that which Fraunhofer made at a later period.

The flint glass for the object glass of the Dorpat telescope was not cast till four years after the departure of M. Guinand, in the thirty-third casting of December 18, 1817; as may be seen by M. Fraunhofer's journal: and it was I who furnished the principal materials for this and the thirty-second casting.

On the 11th of January 1816, M. P. L. Guinand wrote to me me that he was about to superintend an important glass manufactory: to which I replied that he ought so to do, and recommended him to undertake to instruct some one to make flint and crown glass. A short time afterwards (in a letter of February 10, 1816) he again offered me his services, stating "I have recently obtained some knowledge about making glass, and have lately put it in practice by two small castings." But, M. Guinand, at that time, still did not know how to produce glass for optical purposes.

After the departure of M. Guinand, my friend Fraunhofer made several large and excellent castings which succeeded to our utmost wishes. Since his death, I have myself undertaken the continuation of the manufactory of glass destined for optical purposes; and I believe that I can guarantee its excellence. The object-glasses recently constructed by my workmen, sufficiently well attest that the secret of making flint glass, of any size, for optical purposes, is not yet lost, as the *Globe* would have us believe. I assure you I shall be happy to see our neighbours follow us, or even surpass us, in an art which is so immediately connected with the interests of science. I shall take care, on my part, to continue the researches commenced by Fraunhofer on the theory of light, hoping that those who contribute thereto will receive the reward which they so justly deserve. *Suum cuique.*

UTZSCHNEIDER.

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE following are abstracts of papers which have been read before the Royal Society, during the present session.

Mr. Lister's paper on the Compound Achromatic Microscope of Mr. Wm. Tulley; with some account of the present state of the microscope, and suggestions for its improvement on a new principle. Communicated by Dr. Roget, Sec. R.S.

The principles on which the reflecting, and also the achromatic refracting telescope are constructed, have been recently applied with considerable success to the microscope, and have added much to the power of that instrument. The author speaks with much commendation of the peculiar construction adopted in Mr. Tulley's compound achromatic microscopes, consisting of a combination of object-glasses of short focus and large aperture, the curvatures of which are such as very nearly to equalize the refractions produced by each. As the magnitude of the aperture, he observes, is valuable only in proportion to that of the pencil of light which it admits, the latter circumstance is that which chiefly claims attention; and as it is often erroneously estimated, a method is pointed out of ascertaining it with sufficient exactness for every practical purpose. He then enters into a detailed description of the several parts of an instrument in his possession, constructed on the principles he recommends, referring to the drawings which accompany the paper. The magnifying power may be varied at pleasure, either by drawing out the tubes containing the eye-pieces, or by substituting an eye-glass of different power or differently combined; and by these changes an uninterrupted range of amplification is obtained from 35 to 800 diameters. No sensible loss as to distinctness is observable, whether the effect is produced by changing the eye-piece or by varying the length of the tubes. The construction of the instrument admits of the utmost variation of magnifying power, without the risk of losing sight of the object viewed; and every part which relates to the illumination being wholly detached from the stage, ample opportunity is afforded of rapidly moving the objects, and bringing into view a succession of them, while the light remains the same. Minute directions are given for the employment of the instrument, and its application to various purposes; and great stress is laid on the importance of a skilful management of the light. In stating the results of his experience on this subject, the author takes occasion to advert to some of the sources of fallacy, by which incautious observers with the microscope have so often been greatly misled. When a pencil of rays proceeding from an indefinitely small bright portion of an object is brought to a focus by the most perfect object-glass, the image thus formed is in reality not a point, but a small circle, and will always appear as such, if the eye-glass of the microscope be sufficiently powerful. These circles have a considerable analogy to the spacious discs of stars viewed through telescopes. Like the latter, they become much enlarged by diminishing the aperture of the object-glass; and they are also enlarged by increasing the intensity of the illumination. The overlapping of contiguous circles of diffusion has given rise to many fallacious appearances, (such as the spottiness which some surfaces assume,) which have been mistaken for globules. This optical illusion has been the basis of some ingenious but visionary speculations on the intimate structure of organic matter. The appearance, in certain directions of the light, of lines on the surface of an object where they do not really exist, may be traced to a similar cause.

The author proceeds to describe the method he uses for measuring the dimensions of the objects viewed; and notices different test-objects with reference to their affording the means of judging of the powers of the instrument. He next enters into a review of the comparative merits of various microscopes constructed by Cuthbert and Dollond in this country, and by Chevalier, Selligue, Amici, Utzschneider, and Fraunhofer, on the continent.

The concluding part of the paper is occupied by the development of a principle, from the application of which to the construction of the microscope, the author expects that a still greater extension of its powers will, ere long, be obtained. He remarks, that the circumstance which limits the magnitude of the pencil, admissible with high powers by a single achromatic object-glass, is, that the correction for spheri-
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cal aberration by the concave lens is proportionably greater for the rays that are remote from the centre, than for the central rays. The degree of confusion in the image hence arising, is, in similar glasses, inversely as the square of their focal lengths. It increases very rapidly with a small enlargement of the aperture, but may be rendered much less considerable by distributing the refractions equally among a greater number of lenses of smaller curvature. Hence the advantage obtained by certain combinations. The experiments made by the author have established the fact, that in general an achromatic object-glass, of which the inner surfaces are in contact, will have on one side of it two aplanatic foci in its axis, for the rays proceeding from which, it will be truly corrected, with a moderate operation; that for those proceeding from any part of the interval between these two points, the spherical aberration will be over-corrected; and that for rays beyond these limits it will be under-corrected. Methods are pointed out for ascertaining the situation of these aplanatic foci. The principle here explained furnishes the means of destroying both kinds of aberration in a large focal pencil, and of thus surmounting what has hitherto been a chief obstacle to the perfection of the microscope.

Experiments on the Influence of the Aurora Borealis on the Magnetic Needle. This paper is extracted from letters from the Rev. James Farquharson to Captain Sabine, Sec. R.S., by whom it was communicated..

In the first letter, dated from Alford, 15th December, 1829, the author gives a description of the instrument which was furnished to him by the Royal Society for measuring the variation of the magnetic needle, and also the magnetic intensity; and of his mode of using it. The needle was so delicately suspended as to render very sensible, changes in the declination as small as 10″. In his experiments on the magnetic intensity, the intervals of time occupied in the needle's performing 50 oscillations, commencing with an arc of 12°, were noted by a stop-watch, in which the stop, being applied on the balance, is instantaneous in its operation. The watch is again released from the stop at the commencement of a new observation; thus compensating, on the principle of the repeating circle, for any inaccuracy in the reading off, or any inequality in the divisions of the dial-plate.

The observations made on an Aurora Borealis which appeared on the night of the 14th of December, are particularly detailed. On that occasion the disturbance of the magnetic declination was so great, and so frequently changing from east to west, and the reverse, as to leave no doubt in the mind of the author of the reality of this influence. The needle, however, was affected at those times only when the fringes of the aurora were in a position such as to include the needle in their planes. It appeared to him also, that the side towards which the needle declined was the greater, where the aurora gave out the most vivid light.

His experiments on the oscillations of the needle have not yet enabled him to determine satisfactorily whether any change of magnetic intensity accompanied these changes of direction.

In a second letter, dated 26th December, he gives the results of

 later

later observations. From a comparison of his own with the observations of the Rev. James Paull, minister of Tully-nestle, he infers that the height of the particular aurora which was seen by them on the 20th, did not, at its upper extremities, exceed 4000 feet above the ground; and is led to the general conclusion, that the aurora borealis is situated in the region immediately above the clouds, and therefore varies much in height, according to the different states of the atmosphere. He believes it to be an effect of the development of electricity from the condensation of vapour. The position of the fringes which are constantly at right angles to the magnetic meridian, their progressive movements from the north magnetic pole, and their influence on the needle whenever they come into the plane of the dip, are all of them circumstances which establish the relation of this phænomenon to magnetism; while they at the same time illustrate the intimate connection subsisting between magnetism and electricity.

On the production of regular double Refraction, in the Molecules of Bodies, by simple Pressure; with Observations on the Origin of the doubly-refracting Structure; by David Brewster, LL.D. F.R.S. Lond. & Ed. The author has already shown, in former papers which have appeared in the Philosophical Transactions, that the phænomena of double refraction may be produced artificially, by effecting certain changes in the mechanical condition of hard and of soft bodies. In all these cases, he observes, the phænomena are entirely different from those of regular crystals; and in none of them is the doubly-refracting force a function of the angle which the incident ray forms with one or more axes given in position. In the year 1815 he noticed the depolarizing properties of a thin film of a mixture of resin and white wax, compressed between two pieces of glass. Accidentally meeting with the specimen which had originally been the subject of this observation, he found that after fifteen years it still retained this property of depolarization, and was induced to pursue the inquiry to which it led. He varied the proportions of the ingredients, and observed in the different cases the modifications produced in the phænomena by employing various degrees of pressure. He found that in every point there existed an axis of double-refraction perpendicular to the plane of the film; and that the doubly-refracting force varied with the inclination of the incident ray to this axis; just as happens with all regular uniaxal crystals. He infers from his observations, that the property of uni-axal double-refraction is communicated to the molecules simply by the agency of pressure; for in all cases where pressure has not operated, the aggregate does not exhibit this property. These effects are precisely the same as those which would be produced by subjecting elastic spheres to a regular compressing force; the axis of pressure becoming an axis of positive double refraction; while extension, on the contrary, produces a negative axis.

From the consideration of the preceding facts, the author is led to a very simple explanation of the origin and general phænomena of double refraction in regular crystals. He considers this property as not being inherent in the molecules themselves; but as resulting from their compression, either by an extraneous force, or by their

power

power of inherent attraction of aggregation. The phænomena of crystallization and of cleavage prove that the molecules of crystals have several axes of attraction, or lines along which they are most powerfully attracted, and in the directions of which they cohere with different degrees of force. Guided by the indications of hemitrope forms, and supposing the molecules to be spherical or spheroidal, it is inferred that these axes are three in number, and at right angles to each other, and that they are related in position to the geometrical axis of the primitive form. In like manner, the phænomena of double refraction are related to the same axis of the primitive form; and may be all rigorously calculated by a reference to three rectangular axes. The author pursues the consequences of these principles in their application to various kinds of crystals. It follows, from this theory, that the forms of the ultimate molecules of crystals existing separately, determines within certain limits the primitive form to which they belong, while the doubly-refracting structure and the precise form of the crystal are simultaneously produced by the action of the forces of aggregation. These views receive a remarkable confirmation in the doubly-refracting structure which the author discovered in chabasite; and they also enable us to understand the nature of that influence which heat produces on doubly-refracting crystals, as discovered by Professor Mitscherlich. The optical phænomena exhibited by fluids under the influence of heat and pressure, and by crystals exposed to compressing or dilating forces, are also in perfect conformity with the above views, and would in themselves have been sufficient to establish the principle that the forces of double refraction are not resident in the molecules themselves, but are the immediate result of those mechanical forces by which these molecules constitute solid bodies.

LINNÆAN SOCIETY.

April 6.—The President, Lord Stanley, in the chair.

A further description was read of the Anatomy of the Mammary Organs of the Kangaroo. By J. Morgan, Esq. F.L.S.

This paper is a sequel to one already printed in the Transactions of the Society, and contains some important additional information, subsequently derived from an examination of living and dead subjects.*

April 20.—The President in the chair.

A paper was read,—On Luminous Insects, by Mr. Richard Chambers, F.L.S.

The paper maintains, on the testimony of various authorities, (some selected from books, and some collected from original sources by the author,) that *Ignes fatui* are luminous insects; and supports this opinion by the facts often observed, that they alight on various objects, and bound over others.

There was exhibited the cuticle of the hand and foot of a person from whom its exfoliation had occurred five several times, after severe attacks of fever.

* An abstract of Mr. Morgan's former paper will be found in Phil. Mag. and Annals, vol. iii. pp. 375 & 440.

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GEOLOGICAL SOCIETY.

Feb. 19.—At the Annual General Meeting held this day, the following Officers and Council were elected for the ensuing year.

President: Rev. A. Sedgwick, M.A. F.R.S. Woodwardian Prof. Camb.—*Vice-Pres.* W. J. Broderip, Esq. B.A. F.R.S. L.S. & H.S.; D. Gilbert, Esq. M.P. M.A. Pres. R.S. Hon. M.R.S.E. F.S.A. L.S. & H.S.; Leonard Horner, Esq. F.R.S. L. & E. Warden of the Univ. of London; H. Warburton, Esq. M.P. M.A. F.R.S. L.S. & H.S.—*Sec.:* R. I. Murchison, Esq. F.R.S. & L.S.; E. Turner, M.D. F.R.S. L. & E. Prof. of Chem. in the Univ. of London.—*For. Sec.:* C. Lyell, Esq. M.A. F.R.S. & L.S.—*Treas.:* J. Taylor, Esq. F.R.S. & H.S.—*Council:* A. Aikin, Esq. F.L.S.; Rev. W. Buckland, D.D. F.R.S. & L.S.; F. Chantrey, Esq. D.C.L. R.A. F.R.S. S.A. & H.S.; Sir A. Crichton, K.S.W. M.D. F.R.S. & L.S.; H. T. De la Beche, Esq. F.R.S. & L.S.; Sir J. Franklin, Capt. R.N. D.C.L. F.R.S.; G. B. Greenough, Esq. F.R.S. L.S. & H.S. M.R.A.S; J. Lindley, Esq. F.R.S. L.S. & H.S.; Rev. J. H. Randolph, M.A.; P. M. Roget, M.D. Sec. R.S. F.L.S. M.R.I.A.; C. Stokes, Esq. F.R.S. S.A. & L.S. M.R.A.S.; J. Vetch, Esq. Capt. R.E. M.W.S.; N. A. Vigors, Esq. M.A. Sec. Z.S. F.R.S. S.A. L.S. H.S. & M.R I.A.; Rev. W. Whewell, M.A. F.R.S. Prof. Min. Camb.—*Curator & Librarian:* W. Lonsdale, Esq.

March 5.—Richard Smith, Esq. of Connaught Square; Sir Thomas Maryon Wilson, Bart. of Charlton House, Kent; Aristides Franklin Mornay, Esq. of Ashburton House, Putney; Rev. Counop Thirlwall, M. A. of Trinity College Cambridge; Rev. John Philip Higman, M. A. of Trinity College Cambridge, and William Parry Richards, Esq. of Queen Street, Bloomsbury, were elected Fellows of this Society.

A paper was read, entitled "On the Tertiary deposits of Lower Styria;" by the Rev. Adam Sedgwick, Pres. G.S. F.R.S. &c. and Roderick Impey Murchison, Esq. Sec. G.S. F.R.S. &c.

The region described in this memoir, is a great depression on the north-eastern watershed of the Alps, in which has been accumulated a very fine series of tertiary deposits terminating eastward in the plains of Hungary. This great trough or bay of Lower Styria, which is intersected by the river Mur, is bounded on the west by the Schwanberg Alp; on the north by the calcareous chain of Grätz and the primary mountains of Pettau, Vorau, and Hartberg; on the south and south-west by the Matzel and Bacher-Gebirge.

Two principal sections are offered, explanatory of the views of the authors:—The first from the Schwanberg Alp to Radkersburg, in a direction nearly east and west, develops in an ascending succession all the tertiary deposits:—The second, from south to north, is confined to the youngest zone of those deposits, and exhibits its relations to the volcanic rocks of Hungary.

I. Section in an ascending order of the Tertiary formations between Eibeswald on the west and Radkersburg on the east.

a. The lowest members of these deposits consist near Eibeswald, of micaceous sandstones, grits, and conglomerates, made up of the detritus

tritus of the primary slaty rocks on which they rest at high angles of inclination, and rise into the lofty mountain of the Radlberg.

b. Shale and sandstone with coal. There are various beds of lignite near Eibeswald, one of which is deposited on the grits of the Radlberg. At Scheineck, where the coal is extensively worked for use, it contains bones of anthracotheria, and in the shale are found gyrogonites (Chara tuberculata of the Isle of Wight), many flattened stems of arundinaceous plants, cypris, shells of paludinæ, scales of fish, &c. From the organic remains and position of the strata it is presumed by the authors that this coal is of about the same age as that of Cadibuona in Piedmont.

c. Blue marly shale, sand, &c. The carboniferous strata are surmounted by dark-coloured marls inclosing well preserved shells, many of which are identical with species found in the London clay and Calcaire grossier, amongst which are Lutraria oblata, Lucina mutabilis and renulata, Venus vetula, Cerithium thiara, Bulla cylindrica, &c. &c.

d. Conglomerate, with micaceo-calcareous sand and millstone conglomerate. This group is of very great development and occupies all the hilly region of the Sausal.

e. Coralline limestone and marl. The preceding group is seen, both at Ehrenhausen and Wildon on the Mur, to pass under a hard, mottled, coralline limestone of a yellowish white colour, which at the latter place forms a cap several hundred feet thick in beds nearly horizontal. The fossils seem to be of the age of the English Crag and middle Sub-apennine formations, and include many corals of the genera Astrea and Flustra, Crustacea, Balanus crassus, Conus Aldrovandi, Pecten infumatus, Pholas, Fistulana, &c. The authors compare this coralline limestone with the tertiary marble of Possagno near Bassano, and they also observe that it far exceeds in magnitude the secondary coral rag of England.

f. White and blue marl, calcareous grit, white marlstone, and concretionary white limestone. The Mur in its easterly course from Ehrenhausen, exposes all the members of this and the following group, although some of them are still better seen in transverse sections to the south. At Santa Egida concretionary, white limestone alternating with marls, contains Pecten pleuronectes, Ostræa bellovicina, Scalaria, Cypræa, &c. and in the Zirknitz-thal, Echinanthus marginatus with gigantic oysters and pectens. At St. Kunegund and Morgruben the white marls graduate into a compact building stone undistinguishable from the clunch or lowest chalk of Cambridgeshire. Near Mureck on the right bank of the Mur, the upper portion of this group is remarkable by containing a very white concretionary limestone made up of small tubular and concentric layers, several varieties of which, occurring in other parts of this tertiary series, very much resemble concretions in the magnesian limestone of England.

g. Calcareous sands and pebble beds, calcareous grits and oolitic limestone. These form the superior and youngest stratified deposits of the country. At Radkersburg, where the section terminates and the hills sink into the plains of Hungary, the sands, marls, and grits are

are charged with shells similar to those of the highest members of the basin of Vienna. Some of the beds pass into concretionary masses of an oolitic limestone similar to that which is described at other places in the next section.

II. Section from Radkersburg on the south to Riegersberg on the north, exhibiting the structure of the youngest zone of the tertiary deposits of Styria, and its relations to certain volcanic rocks.

Several lofty and serrated ridges of volcanic rocks range from Hainfeldt on the Raab towards Radkersburg, and a section made along their western face offers the following phænomena.

At Straden shelly sands and pebble beds are capped by irregularly columnar basaltic lava with olivine, &c.

The hill of Poppendorf exhibits in great detail the structure of this younger tertiary zone. Marls, sands, and conglomerates, occupy its lower and middle parts, together with many beds of calcareous shelly grits, indurated marlstone, limestone, &c. the whole being very micaceous, and the organic remains identical with those of Radkersburg. These are overlaid by micaceo-calcareous sand containing concretionary masses of a perfect oolite which is quarried as a building stone, and which differs from the great oolite of Bath only by its concretionary structure and the tertiary shells associated with it.

The fine-grained oolite passes upwards into other concretionary beds something like English cornbrash, and the whole is surmounted by micaceous sands and marls. In an adjoining hill near Gnaess, these beds inclosing shells, alternate with volcanic peperino made up of basaltic lava, scoria, vitreous felspar, olivine, pyroxene, the detritus of tertiary rocks and shells, &c.; and on the summit the peperino in a more compact state is quarried as a building stone. The conical hills of Gleichenberg, overlying the shelly sands, are entirely of volcanic origin, and were probably the centre of igneous eruption in these parts. Here the predominating rock is a coarse trachyte used for millstones (felspathic porphyry, probably analogous to the *Porphyre molaire* of Beudant), and with it are associated basaltic lavas, scoria, and fine peperino, which near Hainfeldt repose upon the sands. Considerably to the north of the Raab the volcanic conglomerate on which the castle stands, is also recumbent upon the shelly sands and pebble beds.

From these and several other examples in the neighbourhood, the authors infer, that no tests can be established by which the relative ages of these various igneous rocks can be fixed, since the same tertiary strata are in one place covered by basaltic lava, in a second by trachyte, in a third by volcanic conglomerate, whilst in a fourth they alternate with peperino.

In conclusion they remark:

That the lowest tertiary strata near Eibeswald must from their high inclination have been considerably elevated after their deposition.

That the various groups described, unquestionably represent,—1st, the Paleotherian and Calcaire grossier period:—2ndly, The crag and

and middle Sub-apennine formations :—3rdly, Newer deposits identical with those of the adjoining bay of Vienna, which is shown to have been connected with the bay of Grätz by the intervention of the great tertiary sea which once occupied all the plains of Hungary.

That the volcanic forces in this region, were first called into action during the most recent of these periods, and were probably continued in activity through the long succession of ages in which the sea was spread over these countries.

Lastly, That the volcanic rocks stand out in such prominent masses, as to offer emphatic proofs of the enormous degradation and waste of the surface of the country, since the formation of some of the newest regular strata known in geology.

ASTRONOMICAL SOCIETY.

Feb. 12.—Extract from the Report of the Council of the Society to the Tenth Annual General Meeting, held this day.

Amongst the deaths, the Society have to regret the loss of Mr. Edgeworth, a gentleman of the highest promise and attainment, and worthy of a name dear to science and to literature.

The exquisite transit circle by Troughton, of two feet diameter, presented by Dr. Lee, is now in the possession of Captain Smyth, after receiving from the hands of its maker some important additions and improvements. Upon these much time was necessarily expended; and since the instrument was placed in the Bedford Observatory, the weather has not allowed Captain Smyth to prosecute the delicate researches in which he is engaged. The Society will have remarked with satisfaction the excellent observations of occulted stars by this active astronomer, and by Mr. Maclear, to whom Dr. Wollaston's telescope was intrusted. The transit clock presented by Lieutenant Beaufoy, and now in the possession of Mr. Herschel, has been lent to the Rev. Michael Ward, and will be forwarded to him by the first opportunity. The Beaufoy circle has been safely received, but too recently to have enabled Mr. Ward to furnish the observations which may be expected from his zeal and attainments.

A library has been formed, of considerable extent and value, from the contributions of individuals and scientific bodies. The want of any means of consulting this has long been felt; but the Council have the gratification of stating, that Professor De Morgan has offered his valuable services to arrange and catalogue the books and manuscripts belonging to the Society,—a task which is now nearly completed. It is well known that the Council have long been endeavouring to procure more convenient apartments for the use of the Society, such as would be better adapted to their increasing demands, and under a more permanent tenure than those which they now hold. It is to be regretted that so desirable an object has not hitherto been attained.

Upon the increasing attention paid to our noble science, it is scarcely possible to speak in terms too flattering. In addition to the increased activity of public observatories, the exertions of private individuals are by no means slackened in this race of improvement. A magnificent achromatic object-glass, the masterpiece of Cauchoix, and of the largest dimensions, having $11\frac{3}{4}$ inches clear aperture, and 19 feet focal length, has been purchased by our spirited President. The task of mounting this gigantic telescope equatorially, has been undertaken by the most celebrated of our modern artists, and we may hope to see this active astronomer speedily engaged in pursuing, with unequalled means and unabated ardour, the path of discovery in which he is already so advantageously known*.

Your Council trust, that they have not been backward in promoting, to the utmost of their power, every attempt to advance the interests of the science of Astronomy.

Before the departure of Captain Ross on his Arctic expedition, that enterprising officer proposed to make any scientific observations, for which his situation might be considered favourable, should he unfortunately be detained upon his perilous route. An application was made by him to the Lords of the Admiralty, and supported by your Council, for a loan of the requisite instruments, which was received by their Lordships in a most gratifying manner. Captain Ross was amply supplied with the proper instruments; and, at the request of the Council, a list of moon-culminating stars, for the months of January, February, March, and April, 1831, was prepared for him by Mr. Henderson, the Astronomer Royal having undertaken that the corresponding observations should be made at Greenwich.

The numerous and excellent observations of Aldebaran, during the preceding year, which have been already communicated, are a satisfactory proof of the increased interest felt for the science, and of the importance attached to the views and wishes of the Society. In some instances the star disappeared instantaneously after the bisection of its disc by the moon's limb, while in others the projection lasted nearly six seconds. It does not appear that a sensible diminution of brightness was perceived previous to immersion. When a sufficient number of well-attested appearances has been collected, it may be possible to account for the anomalies which have hitherto been so perplexing. The attention of astronomers towards this interesting phænomenon is earnestly requested during the present year. Neither should the

* [Although this telescope was placed on a temporary stand, at the President's Observatory at Kensington, on January 29, the weather did not permit of any trial of its powers *previously* to the Anniversary. It may now, therefore, be interesting to state, that the first night on which an opportunity was afforded, viz. the 13th of February, Mr. Herschel discovered a *sixth* star in the trapezium in the nebula of Orion, a few degrees south following of, and about five or six seconds distant from, the star denominated A in that trapezium, in Mr. South's observations of the double stars. Its brightness is about one-third of that of the fifth star, discovered by M. Struve, which is as distinctly seen as the companion to Polaris is in a 5-feet achromatic. It was immediately after detected by Mr. South, and has since been seen by Mr. Baily, Mr. Troughton, Mr. Sharpe, and others, and repeatedly by myself. Saturn, with his Belts, Double Ring, and Seven Satellites, is beautifully defined, with powers of 130, 280, and 350. The object-glass is not in very perfect adjustment; it was placed in its cell, as now used, by mere accident, and, in consequence of unfavourable weather, it has not been deemed prudent hitherto to attempt any alteration.—W. S. S. *Sec.*]

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predicted occultations of other stars be neglected by observers. It is from them that the most accurate determinations of longitude are, in the generality of cases, to be obtained; and the tedious difficulties with which they were embarrassed, have been in a great measure removed. The Council have distributed from time to time monthly notices of the principal lunar occultations, for the meridian of Greenwich, computed by Mr. Henderson, to whom this branch of astronomy is under peculiar obligations. It is to be regretted that the avocations of this gentleman will not allow him to continue these useful lists much longer. Fortunately, this loss is greatly diminished by a recent memoir due to our excellent associate Professor Bessel, and inserted in *Encke's Ephemeris* for 1831, which gives a short and simple method of transferring to another meridian the time and circumstances of an occultation already computed for Berlin, along with the quantities required for the calculation. These subsidiary quantities have been prepared for the first six months of the present year by Professor Troeger of Dantzic, and inserted in the *Astronomische Nachrichten*, an early copy of which was transmitted to the Society by the accomplished editor. This has been printed for distribution, in the hope that the zeal of Mr. Henderson will find worthy successors. To mark their sense of obligation to this gentleman, and as a small tribute of respect, the Council have unanimously agreed, "That it be recommended to the general meeting to present to Mr. Henderson a copy of the Memoirs, handsomely bound, for the very valuable assistance which he has rendered to the cause of Astronomy in his various computations presented to the Society*." The numerous communications that have from time to time been received from Germany, and the valuable astronomical works that are constantly issuing from the press of that country, render an acquaintance with the German language almost essential to the prosecution of the science. The difficulty which the Council would otherwise have experienced has been greatly removed by the cordial and very valuable assistance of a gentleman deeply versed in every branch of the science, who has, on all occasions, been ready in favouring the Council with translations of such papers as they have from time to time required. To mark their sense also of this obligation, and as a small tribute of respect, they have agreed, "That it be recommended to the general meeting to present to Dr. Tiarks a copy of the Memoirs, handsomely bound, for his kind and ready assistance at all times in translating various foreign papers for the use of the members, and for other valuable services rendered to the Society*."

A simple and ingenious method of determining the mass of the moon, by observing the Æ of the bright limb of Venus, has been proposed by Professor Airy, with a request that the Society would use their influence in obtaining for it an extensive circulation. Copies have, accordingly, been forwarded to every public and private observatory in Europe, with a strong recommendation for its adoption.

* [The recommendation of the Council in each of these instances was unanimously approved of, and adopted by, the Members at the General Meeting.—*Sec.*]

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The fluctuating use of symbols in mathematical investigations more frequently occasions mistake and misapprehension than the inherent difficulty of the subject matter; and it is to be lamented that so little care is taken by the generality of authors to be consistent with each other, or even with themselves. An attempt has been made by Mr. Lubbock, under the auspices of the Society, to introduce a system of notation depending upon fixed rules. This paper is now in the press, and will be submitted before publication to the geometers of this and other countries. In this way, it seems probable that a considerable degree of accordance may be obtained, which it is hoped may be strengthened and confirmed by the sanction and practice of the Society.

The paper upon the Constant of Aberration, by Mr. William Richardson, to which such marked attention was drawn in the Report of last year, has subsequently been published in the Memoirs of the Society, and fully justified the high expectations excited. The immense labour bestowed upon this important investigation, the clearness with which it is developed, and the number of observations on which it is founded, must command the admiration of all astronomers. Your Council have unanimously awarded to Mr. Richardson the gold medal for the determination of this fundamental constant.

A gold medal has also been voted to Professor Encke for the superb Ephemeris of Berlin. It would be superfluous to dwell here upon the merits of this well-known work, which, far outstripping all rivalry, must be considered as the only ephemeris, on a level with the present wants of the science,—the manual and standard of practical astronomy wherever it may be cultivated.

The President will deliver the medals at the close of this meeting in the accustomed form.

Your Council trust that this exposition of the resources and progress of the Society will be satisfactory to the members at large; but it is not to be forgotten, that much remains to be done, and that more labourers are demanded. It is not sufficient to thank or to admire those who have toiled in the good cause,—the necessity of imitation must be strongly felt and inculcated. There is scarcely any instrument which may not be usefully employed, or any acquirements which may not perform good service. Phænomena may be predicted, preliminary computations furnished, reductions prepared, with little more knowledge than that of common arithmetic; occultations, eclipses, moon-culminating stars, planets, may be observed, and portions of the heavens examined and surveyed, with instruments of moderate price and of inferior dimensions. Indeed, in the present state of astronomy, the class of *differential observations* embraces far the greater portion of celestial phænomena even for the best instruments; and by the publication of the Berlin Ephemeris, computation is almost wholly reduced to simple interpolation. At no distant period it may be possible for the Council to provide skeleton forms, not merely for registering observations, but for their reduction and for all ordinary calculations.

It would also be highly desirable to collect and publish annually,

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in a condensed form, observations presented to them by British and foreign astronomers, and not printed elsewhere,—a repository and digest of the services of amateur observers. The Council confidently rely upon the energy and activity of the members, and of all friends to astronomical science, for the accomplishment of this important object. Thus, not only will a large mass of valuable information be rescued from oblivion, and preserved in a shape the best adapted to comparison and use, but a deeper interest will be felt by the contributor, and a stronger motive offered for that care and method, without which computation is rendered useless, and instruments degenerate into toys.

(*The President then addressed the Meeting on the subject of the award of the Medals, as follows :—*)

Gentlemen,—Ere we proceed to the distribution of our honorary rewards, I cannot but congratulate you on the general state of astronomical affairs ; whether we turn to the north or to the south, to the east or to the west, the astronomical horizon presents a more promising aspect than it has done for many years ; and what is peculiarly gratifying to us is, that amidst the general endeavours to advance astronomical knowledge, our own country this year stands indeed conspicuous. Since our last anniversary, the observations of Professor Airy have done honour to the University of Cambridge ; those of Dr. Robinson have rewarded the munificence of the Primate of Ireland ; and others made at Paramatta, in the observatory founded by Sir Thomas Brisbane, and which (thanks principally to the exertions of members of this Society) is now become an observatory in the service of the British government, give us an earnest of the value of those observations of Sir Thomas's, now in the course of reduction by Mr. Richardson, as also of the importance of others, which, on the authority of the government, we are henceforth regularly to receive from Paramatta. Again, within these few days has arrived a first series of standard transit observations, made by Mr. Fallows at the Cape Observatory. They are, as yet, unpublished. Knowing him, however, intimately as I do, and having witnessed his early astronomical career, I may be permitted to felicitate you on their arrival, confident that whatever comes from him will be honourable to his country, for it will be worthy of himself. But that these observatories have rendered to us their first fruits, is not all : others are in a state fast approaching to practical utility ; and when another year shall glide away, he who shall have the honour to fill the chair I now so unworthily occupy, will have, I hope, to applaud the activity of the observatories of Brussels and Cadiz, of Cracow and of Edinburgh, of Geneva and Madras*.

While exulting, however, in the accession of these new observato-

* This is not literally a new observatory ; the instruments, however, now about to be sent thither, are so far superior to those already there, that it may almost be regarded as a new establishment. The same applies to the Geneva observatory, but with greater force ; for not only are superior instruments ordered for it, but a new building is being prepared for their reception.

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ries, let us not forget honourably to notice, that from the Royal Observatory of Greenwich has issued, but a few weeks since, a Catalogue of 720 Stars;—a valuable treasure, and for which the astronomical world is indebted to this Society, as well as to the Astronomer Royal; inasmuch as the stars it embraces have been selected from, and reduced by, the Society's Catalogue. On examining the important facts resulting from this first series, and which the Astronomer Royal has exhibited by actual comparison with our own work, let us hope, now that his fundamental catalogue has reached a degree of accuracy probably unexampled in astronomical history, he will persevere in the task; assured as he may be, that no one thing would tend more to the credit of the Observatory, and to his own glory, than that his name should be attached to a large catalogue of stars, called, as in the good old times of Flamsteed, "*The British Catalogue.*"

The Royal Observatory of Paris is also an object of increasing interest; a splendid equatorial by Gambey has been added to its previous collection; and the transit instrument by the same distinguished artist, so long and so anxiously looked for by our *confrères* of Paris, is at length placed on its piers; whilst the observatory, *properly* so called, is about to be remodelled and rendered more convenient for astronomical labour. We hail the recommencement of the observations as an æra which will be worthy of the country of Cassini*, of La Caille, and of La Lande; of La Grange, Legendre, and La Place. The sentiments of the late distinguished Secretary of the Institute, that "Ce n'est pas le tout que de fonder un observatoire et de doter l'astronome, il faudrait faire un fonds annuel pour l'impression, et imposer à l'astronome l'obligation de n'être jamais en retard d'une année†," being adopted by the astronomers of Paris, we shall not have to regret that observations, made with such instruments and by such men, shall be available only to those who reside within the walls of the observatory. We have this year set our Parisian friends a splendid example, and doubtless they will follow it.

As to the improvement of the achromatic telescope, since our last anniversary, much cannot be said. The theory of Mr. Rogers has not, as far as I know, been put to the test of practice, in a manner at all commensurate with its merits; whilst Mr. Barlow's achromatic, of eight inches aperture and eleven feet focal length, owing to imperfections in the material of its exterior lens, does not define close sidereal objects so neatly as might be wished, but is, nevertheless, applicable to many useful purposes; and there is a hope that the labours of a sister society, although hitherto unsuccessful in furnishing us with

* Cassini was an Italian professor; he was, however, called to Paris by Colbert, whose comprehensive mind felt that not only military success, but scientific reputation could confer glory on his country. Not only did he place the observatory of Paris under the care of Cassini, but men of all countries who had distinguished themselves by their scientific acquirements were invited by that illustrious patron of science to reside in Paris. He was also the founder of the *Académie des Sciences*, and of the Observatory of Paris.

† Hist. de l'Astron. au XVIIIème siècle; par Delambre, p. 115.

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the mode of making good *flint* glass, will shortly put us in possession of at least a succedaneum for it.

Some of the members* of the Society, regretting that the late Board of Longitude was not remodelled instead of being dissolved, had, during the last spring, several interviews with His Majesty's ministers on the subject of a new one. A plan for the constitution and regulation of a new Board was shown to the First Lord of the Admiralty; and was *only* not introduced to the consideration of Parliament at the instance of the projectors of it, in consequence of a wish expressed by Lord Melville, that a bill drawn up in its spirit should be presented to the Legislature by the Admiralty officers. The question which at that time, and during the remainder of the session, so completely engrossed the attention of the government, being now disposed of, let us hope, that the pledge then given will be remembered, and that on our next anniversary we shall have to thank the government for having given us a Board of Longitude, which shall be worthy of the first maritime nation in the world.

As connected in some measure with astronomy, we see the geodetic operations in Ireland carried on with vigour, under the superintendence of Colonel Colby; whilst for triangulating our Indian empire (thanks to the scientific spirit of the Directors of the East India Company) such a battery of field apparatus is nearly prepared by Troughton and Simms, as, in even less able hands than those of Captain Everest, could scarcely fail to immortalize him who should have the happiness of using it.

Again, we have the pendulum investigations of our associate, Professor Bessel, showing that the corrections employed by British philosophers for the reduction to a vacuum are far from accurate; and, to come nearer home, our ever active member, Mr. Francis Baily, has demonstrated that certain imperfections and discordances exist in the apparatus employed by previous experimenters, which point out the absolute necessity of fresh inquiries being instituted, ere we can pretend to be possessed of accurate estimations either of measure or of weight †.

But our Society was not founded solely for the investigation of astronomical refinements—practical utility was to go hand in hand with it: geography was to be improved; and hydrography, so essential to the well-being of our maritime country, was not to be neglected. To advance our knowledge of the former since our last anniversary, British intrepidity, undaunted by previous defeat, has left our shores, and is now struggling in the Victory to achieve that, which the perseverance of a Parry, backed by the exchequer of Great Britain, was forced, after repeated trials, to abandon in despair. And the no less interesting part of the history is, that the expense of the attempt (which God grant may be successful!) has been paid by the princely

* Lord Ashley, Mr. Francis Baily, Captain Beaufort, and the President.

† A standard measure, on a new construction, has been ordered by the Council, since the delivery of this Address, and is now in course of completion by Troughton and Simms.

liberality

liberality of an English gentleman, whose modesty withholds his name from the public.

Again, as to hydrography: scientific surveys are gradually spreading to every quarter of the globe, under the influence of the noble Lord who has lately enrolled his name in this Society. But little does it matter if coasts are well surveyed, rocks detected, shoals discovered and currents ascertained, if the results are not digested so as to be practically useful to the seaman. It gives me pleasure therefore to announce, that since we last met, an appointment has been made by the Admiralty, which has given universal satisfaction; for it assures us, that the day is not far distant, when an Englishman shall visit the hydrographic department of the *Depôt de la Marine* of a neighbouring state, without feeling humiliated by the comparative inferiority of the corresponding establishment of his own country.

Proceed we now to the distribution of our medals. By the minutes read at the table, you have been informed that "one of them has been decreed to Mr. Richardson, for his investigation and determination of the constant of aberration, from observations made at Greenwich with the two mural circles;— the other to Professor Encke, for his *Berlin Ephemeris*."

Three hundred years have now elapsed since Copernicus proposed to the world that system which bears his name; and if we except the labours of Tycho Brahe, who, besides a catalogue of 800 stars, made attempts to determine the altitude of the pole star at different seasons of the year, little was done by practical observation to support or refute the ideas of Copernicus till the time of Galileo. Observations of the eclipses of Jupiter's satellites induced him to propose them as a means of determining differences of longitude, whilst his discovery of the phases of Venus removed a serious objection to the truth of the Copernican system, and which Copernicus himself predicted would be removed, though he had not the means of doing so himself. About the year 1665, Huygens, by his discovery of the pendulum clock, gave to astronomical observations an accuracy hitherto unknown; and Cassini, by means of the excellent glasses of Campani, accumulated a vast mass of observations of the eclipses of Jupiter's satellites, and deduced from them tables whereby astronomers could predict their occurrence.

Notwithstanding the powerful arguments advanced in its favour, the Copernican hypothesis was not generally embraced; for in the year 1669, nearly a century and a half subsequent to its promulgation by Copernicus, even the celebrated Hook, to use his own words, "would not absolutely declare for it*." To settle the matter, therefore, this extraordinary man, feeling that the instruments of Tycho, although magnificent beyond all others, were, from the nature of their construction, and from their being unprovided with telescopic sights, incompetent to detect minute alterations of sidereal positions, and

* An attempt to prove the motion of the Earth, from Observations made by Robert Hook, F.R.S., pp. 5 & 7.

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knowing that the laws which governed refraction were so little understood* as to render all observations in which that element was materially involved, liable to errors greater probably than the quantities he was in search of, invented the zenith sector. It was erected at Gresham College, and consisted of a telescope 36 feet long, a divided arc, and a plumb-line. The star selected for observation, and with reference to which, indeed, his instrument was entirely constructed, was one which passed within two or three minutes of the zenith of Gresham College, was visible in the day-time throughout the year, and was γ Draconis: by observing its zenith distance when the earth was in opposite points of her orbit, he found (as he erroneously concluded) a sensible parallax, amounting to about 20 seconds, and, consequently, determined that the Copernican system was the true one†.

In the mean time, the eclipses of Jupiter's satellites,—thanks to the facilities of predicting them afforded by Cassini's tables,—had been assiduously observed; and in the year 1675, the discordances found between the predicted and the observed eclipses enabled the celebrated Roëmer to demonstrate that light was not instantaneously propagated, and that the discordances between the tables and the observations might be considered as the measure of its velocity.

The year of Roëmer's discovery was further marked by another epoch in astronomical history, namely, the foundation of the Royal Observatory. Flamsteed, with his mural quadrant, detected a change of place in the pole-star, amounting to 35, 40, or 45 seconds, attributed it to parallax, and regarded it as confirmatory of Hook's discovery. Indeed, the observations of Hook, as well as of those who preceded him, although nominally in search of parallax, had for their object little else than the confirmation or verification of the Copernican system; and this arrived at, there seems to have been but little disposition to repeat them.

Hence it was that, the brilliant discoveries of Newton having placed the accuracy of the Copernican system beyond all possibility of doubt, the investigation of parallax was not resumed till the latter end of November, in the year 1725, at which time Molyneux erected his 24-feet zenith sector, by Graham, in his observatory at Kew‡. "On the 3d of December, γ Draconis was, for the first time, observed as it passed near the zenith, and its situation carefully taken with the instrument; and again, on the 5th, 11th, and 12th, when, no material change in the star's place having been detected, further observations seemed needless, since it was a time of the year when no sensible alteration of parallax could soon be expected." Bradley, however, being on a visit to his friend Molyneux, was "tempted by curiosity to repeat the observation on the 17th, and perceived the star pass a little more southerly than when it had been observed before:" sus-

* An Attempt to prove the Motion of the Earth, from Observations made by Robert Hook, F.R.S. pp. 10 & 11.

† Idem, p. 25.

‡ Philosophical Transactions, vol. xxxv. p. 639.

pecting that the apparent change of place might be owing to erroneous observation, it was observed again on the 20th, and he found the star still further south than in the preceding observations. This sensible alteration surprised himself and Molyneux, inasmuch as it was the contrary way from which it would have been, had it proceeded from an annual parallax of the star; but being incapable of accounting for it by want of exactness in the observations, and having no notion of any other cause from which such apparent motion could proceed, they suspected that some change in the materials of the instrument itself might have occasioned it. Under this apprehension they remained some time; but being at length fully convinced, by repeated trials, of the great exactness of the instrument, and finding, by the gradual increase of the star's distance from the pole, that there must be some regular cause which produced it, they examined nicely at the time of each observation how much it was; and about the beginning of March 1726, the star was found to be 20″ more southerly than at the time of the first observation. It now, indeed, seemed to have arrived at its utmost limit southward; for in several observations made about this time, no sensible difference could be detected in its situation. By the middle of April it appeared to be returning towards the north, and about the beginning of June it passed at the same distance from the zenith as it had done in December, when it was first observed. From the quick change in the star's declination about this time (it increasing a second in three days) they concluded that it would now proceed northward, as it before had gone southward of its present situation; and it happened as was conjectured; for it continued to move northward till September following, when it again became stationary, being then near 20″ more northerly than in June, and no less than 39″ more northerly than it had been in March. From September it returned towards the south, till it arrived, in December, at the very same situation it had been at that time twelvemonth, allowing for the difference of declination "on account of the precession of the equinox."

Such is a brief history of the Kew observations; commenced, indeed, for the determination of sensible parallax, but which, as subsequently in the hands of Herschel, led to a very different result. In reading it, we are at a loss whether most to admire the mode in which the observations were conducted, or the modest unassuming manner in which they are recorded: no possible source of error is allowed to pass without the most rigid examination—no theory suffered to embarrass the observers in their observations; the slightest anomaly became the subject of suspicion, till in presumed anomaly was found the most perfect regularity.

That observations so conducted, leading to results so unexpected, could be abandoned till the law which governed them should be unfolded, was impossible. But Bradley rejected all inquiries into the cause till the effects were accurately determined; and feeling that the apparent motion was obtained by observations only of one year,—by one instrument,—and by one star,—he erected at Wanstead, aided by his friend Graham, on the 19th of August 1727, his zenith sector of of $12\frac{1}{2}$-feet focus, formed, indeed, upon the same general plan of Molyneux's, but furnished with a divided arc of $6\frac{1}{4}$ degrees on each side of the zenith point, for the purpose of enabling him to ascertain, by direct observation, whether other stars than γ Draconis, would be similarly affected. The instrument's situation, when adjusted, "might be securely depended upon to half a second," and its telescope could be directed to "not less than 12 stars, bright enough to be seen in the day-time" throughout the year: the same changes were observed as had been previously detected with Molyneux's instrument. Inflexible, however, in his resolution not to generalise till sufficient means were collected to lead him to a "probably just conclusion," the year of probation was suffered to be completed before "the observations were examined and compared:" *then* it was that he satisfied himself of the general laws of the phænomena; and *then*, and not *till then*, did he endeavour to find out their cause. Convinced that the apparent motion of the stars which he had observed was not owing to nutation—persuaded that a change in the direction of the plumb-line with which the instrument was rectified, was insufficient to have occasioned it—and having appealed unsuccessfully to refraction,—he perceived "that if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest as when it is moving in any other direction than that of the line passing through the eye and the object; and that when the eye is moving in different directions, the apparent place of the object would be different." He therefore announced his discovery in these words: "That all the phænomena proceeded from the progressive motion of light and the earth's annual motion in its orbit," or, as he afterwards called it, aberration of light.

But he who determined its existence determined also its constant, and fixed it at 20″; giving us, therefore, the interval of time in which light travels from the sun to the earth, as eight minutes and seven seconds, differing from that deduced by Roëmer nearly three minutes of time, a circumstance not at all to the discredit of Roëmer, considering the imperfect knowledge of the theory of Jupiter's satellites at the time he made his important discovery.

The observations, however, which led Bradley to the discovery of aberration, and to the determination of its constant, being as yet unpublished, have given rise to insinuations certainly ungenerous, and probably unjust. Impelled by more honourable feelings, our illustrious associate Bessel, alluding to the observations of γ Draconis made by Bradley when the sector was removed to Greenwich, says*: "Cæterùm Bradleii observationes Wansteadianæ liberari possunt à sectoris mutabilitate, quum sæpiùs, eodem tempore, observatæ sint stellæ, in quibus aberrationi contraria fuerunt signa: quâ de causâ, et propter observationum præstantiam, optabile esset reperire ipsa Bradleii autographa." It will therefore be highly grateful to him, and to astronomers in general, to be informed from this chair, that the manuscripts of the Wanstead observations are found!—that, to the

* Fundamenta Astronomiæ, p. 124.

honour of the University of Oxford, twenty-three sheets of them are already printed; and that the volume will be presented to the public with as little delay as possible, under the superintendence of Professor Rigaud.

Till within these few years, the constant, as determined by Bradley, was universally employed in all our astronomical reductions; recently, however, astronomers have re-investigated it. Delambre, from the eclipses of Jupiter's satellites, regards it as 20″,25. Bessel, from Bradley's observations made at the Royal Observatory, after he was appointed Astronomer Royal, has deduced for the constant 20″,68. Lindenau, by comparisons of Bradley's, Maskelyne's, Bessel's, and Pond's observations of Polaris, has obtained for it 20″,61. Brinkley, from his own observations, considers it as 20″,37. Whilst Struve, by his observations, draws almost the same inference, namely 20″,35.

Such were the results most entitled to our confidence, when Mr. Richardson, in the hours allotted to him for repose or recreation, undertook those labours which form the subject of our present consideration, and of which the following is a brief outline. A second mural circle by Jones, after the model of that of Troughton's, having been erected at the Royal Observatory, in the April of 1825, corresponding observations with the two instruments were carried on simultaneously; they were confined, indeed, to a few stars, but every precaution to render them as accurate as possible was adopted. Thus, the index error of each instrument was ascertained by observing the same star alternately, by direct vision and reflection; each pair, therefore, giving to its respective instrument one horizontal point perfectly independent of astronomical tables, the final accuracy of the determination of the index error being directly as the number of pairs observed. Throughout the observations, the place of each star was arrived at by reference to the six microscopes of each circle; care also being taken to equalize, as nearly as possible, the temperature of the observatory with that of the external air, so that errors to any extent, arising from partial expansions or erroneous divisions of the instrument, were effectually excluded.

From such unexceptionable data, fourteen stars were selected by Mr. Richardson as the fittest for his purpose, being those the least affected by refraction, and the most affected by aberration, so that the errors of observation might have the minimum influence upon the results. Upwards of 4000 observations he separately discussed, and in no instance was the actual aberration of each observed star less than 14″; and the conclusion to which he has arrived is, from Troughton's circle, that the Constant of Aberration is 20″,505; whilst by Jones's it is 20″,502, the one differing from the other but three-thousandths of a second.

Hook, in searching for parallax, was misled by his instrument. Bradley, in detecting it, was unsuccessful, but discovered aberration. Hook's instrument was the work of his own hands; Bradley's was the work of Graham. Sensible that much of his astronomical glory was referable to the accuracy of his instrument, the amateur astronomer of Wanstead was ever ready to acknowledge it; and when we consider

consider that one hundred years' improvement in astronomical manipulations can alter the constant of aberration as determined by him but one half second *, we are almost led to exclaim "Quando ullum inveniemus parem?"

Our Graham is amongst us; to him we are indebted for the instruments with which results thus important have been obtained, and also for the mode of using them, through which the maximum of accuracy has been acquired. One of them was made with his own hands, the other under his direction; and it is not too much to say, that the disciple has shown himself worthy of his master. The benefits which Edward Troughton has conferred on science, are too well known to need enumeration. His Majesty the King of Denmark, not insensible to the importance of science, and feeling that for much of the accuracy to which astronomy and navigation have arrived, we are indebted to the genius of our revered member, has recently acknowledged his gratitude to him, by the presentation of his gold medal, inscribed with the word "MERITO." Never was inscription more appropriate. May he live long to enjoy this token of respect! alike honourable to himself and to the princely feelings of its royal donor.

On looking over the constant as determined by each star, nothing definitive, as Mr. Richardson justly observes, can be concluded, as to whether light emitted from different stars is propagated to us with different degrees of velocity: the idea is not irrational, but its validity future astronomers must determine.

(*The President then, addressing Mr. Richardson, continued thus:—*)

Mr. Richardson,—Brought up to an employment little allied to astronomy—residing in a part of the country remote from the metropolis—cut off from the society of scientific men, which operates so powerfully in stimulating to scientific exertions,—we see you quitting the place of your birth,—leaving the roof of your respected parents,—and presenting yourself in this great city, known only to a single individual; abandoning every sordid pursuit to follow that which, however noble, too often leads, in this country, to neglect and poverty,—we find you, in August 1822, at the Royal Observatory as an assistant. Here you soon distinguished yourself by your punctuality—your attention—and your zeal; accustomed to the business of an observatory†, you required to be instructed only in the established routine of that to which you were appointed. Soon did you gain the confidence of the Astronomer Royal; and whether he wished to establish the place of a star in the heavens, or of a spot on the earth, to no one could he intrust the one or the other with more confidence than to you, whom he emphatically styled "his right hand." Attached to astronomy, but unable to maintain your family upon the *miserable* pittance allowed you by the Government, we see you observing all day and computing all night. As an assistant of the Royal Observatory, I have only to say,—Go on as you have begun. But if a trust higher in the scale of importance awaits you, remember, sir, that you

* It is not impossible that much of this discordance will disappear when the Wanstead Observations shall be reduced with modern precision.

† The President's.

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have *now* a character to maintain; for your name, but a few years since confined to the narrow limits of a country village, is now known throughout Europe; and that too by your own exertions. Receive this medal, sir, as a pledge of our esteem—as, indeed, the highest honour we have to offer. May it stimulate you to fresh exertions in the cause of your favourite science; and on your return from India to your native home, may the Astronomer of Madras be as honourably distinguished, as is this day the Assistant of the Royal Observatory.

(*The President then resumed his address to the Members as follows:—*)

The second medal has been adjudged to Professor Encke, for the Berlin Ephemeriswhich bears his name. Experience has long shown, that an astronomical ephemeris is almost as necessary an appendage to an observatory as are the instruments themselves; accordingly we find that some one, more or less perfect, has been in the hands of every astronomer, since astronomy has been pursued in a systematic manner. The first which appears in this country at all deserving the name of an astronomical ephemeris was that of Vincent Wing, and was used by Flamsteed: it, however, was very imperfect, and Flamsteed, whilst at Derby, transmitted to the Royal Society supplements to it, containing notices of phænomena not inserted in Wing's Ephemeris, and which were published from time to time in the Philosophical Transactions. La Caille, about the year 1755, proposed one upon a larger scale; but in 1767, on the recommendation of Dr. Maskelyne, the Board of Longitude of Great Britain published the Nautical Almanac and Astronomical Ephemeris, Dr. Maskelyne himself taking the superintendence of it. Its object was evidently twofold, since it contained, even when first published, much information not wanted by the astronomer, and still *more* not required by the seaman. The *Connoissance des Tems* also was published much in the same style, and was, as well as the Nautical Almanac, a manual not only for the seaman, but also for the astronomer. The Berlin Ephemeris was purely astronomical, but, like the *Connoissance des Tems*, contained, in the shape of additions, much valuable astronomical information, inserted by its highly respected editor, the much-lamented Bode. But the *Connoissance des Tems*, compared with the Nautical Almanac, had so fallen into disrepute, that, in the year 1795, Grégoire, now Evêque de Blois, in the name of the Committees of Marine, of Finance, and of Public Instruction, proposed to the National Convention the establishment of a Board of Longitude, in imitation of the British Board, and to which should be confided the publication of the *Connoissance des Tems*. From that time to the present it has been published with greater regularity, and in general about the same time as the Nautical Almanac; for, from some cause or other, the latter has not appeared so many years in advance as in the times of Maskelyne. Considering that the work is sometimes wanted for long voyages of discovery, its publication three or four years in advance, instead of two years and a quarter, as at present, would certainly be desirable. From the time of Maskelyne's editing the Nautical Almanac till his decease,

decease, it gained the approbation of his countrymen and the eulogies of foreign astronomers. La Lande, speaking of it, says, "On a fait à Bologne, à Vienne, à Berlin, à Milan; mais le 'Nautical Almanac' de Londres est l'éphéméride la plus parfaite qu'il y ait jamais eu."

On the death of Maskelyne, the superintendence of it passed into the hands of "irresponsible" persons, and the character which it had so long sustained was lost. Compared with corresponding productions of other countries, it was not only inaccurate but incomplete. Astronomical knowledge was progressive; but the Nautical Almanac remained not only stationary, but retrograded. To restore it to its former rank, a new Board of Longitude was formed, and the superintendence of the Almanac was confided to the Secretary of the Board. To retrieve the character of a work which, from holding the highest rank in the ephemerides of Europe, had descended below mediocrity, was no easy task; but it is due to the memory of that eminent person to acknowledge, that the first volumes published under his superintendence did much to restore its character for accuracy. Other ephemerides, however, having, in an astronomical point of view, got the start of it, sustained their superiority; and there is scarcely an observatory in the country upon whose desks have not been found, for several years past, the ephemerides of France, of Berlin, and of Milan. As a small addition, a supplement has been recently published; this, however, gets into circulation not till its year is actually commenced, and therefore is comparatively of little use. Under these circumstances, so injurious to the astronomical character of the country, many have been the discussions relative to the Nautical Almanac, and the individual who has the honour to address you has not been unconcerned in them. His animadversions may have been occasionally severe, but he hopes never unjust;—they were intended to convince, not unnecessarily to wound. For the individual with whom he had the misfortune to differ, he entertained the highest respect; and by no one even of his scientific friends, was his premature removal more keenly felt than by him, who never differed from him but with regret. His acquirements were of no common order; and whatever may be our opinions of his superintendence of the Nautical Almanac, all suffrages will unite in pronouncing him an ornament to his country.

Since the decease of Dr. Young, the superintendence of the Almanac has been transferred to the Astronomer Royal, and the volume for 1832 has just been published under his direction. No important alteration distinguishes it from its predecessors. Almost on the same day appeared the corresponding volume of the *Connoissance des Tems.* The former gives us no notice of improvements contemplated for future volumes; the latter promises some important additions. I have, however, the satisfaction of announcing to the Society the following message from the First Lord of the Admiralty, transmitted to me through the Hydrographer; namely, "that the Admiralty has actually ordered some additions to the Almanac for 1833, and has it in contemplation to order further additions to that for 1834." What these additions are, it has not been thought proper to divulge, at least to me; let us therefore hope that they will be worthy of the country, and that

that ere long the Nautical Almanac and Astronomical Ephemeris of Great Britain, rising like a phœnix from its ashes, shall be as much superior to Encke's, as Encke's is now superior to it. That, however, will be no easy task: Encke, whose name stands already in the list of those to whom our medal has been *before* adjudged, is no ordinary rival. Well knowing what is wanted in an observatory—skilled in practice—profound in theory—and living under a Government by which science is *eminently* encouraged—he has given to us an ephemeris, which is invaluable. To enter into its individual merits would far exceed the time allowed me; suffice it to say, that for every purpose of astronomical investigation, its materials are sterling, and its arrangement excellent. With it, an observatory scarcely wants a single book; without it, every one.

(*The President then, addressing Mr. Baily, said:*—)

Mr. Baily,—To no one can I confide this medal with more propriety than to you; for no one, more than you, knows the importance of our Associate's labours. Transmit it to him as a second badge of our regard; and tell him, that for his Ephemeris we are under obligations to him, which we never can repay.

List of Officers for the ensuing year.

President: James South, Esq. F.R.S. L. & E. M.R.I.A. & F.L.S.—*Vice-Presidents:* Captain F. Beaufort, R.N. F.R.S.; Olinthus G. Gregory, LL.D. *Prof. Math. Roy. Mil. Acad. Woolwich*; J. F. W. Herschel, Esq. M.A. F.R.S.L. & E. F.G.S. & M.R.I.A.; Edward Troughton, Esq. F.R.S. L. & E.—*Treasurer:* Rev. William Pearson, LL.D. & F.R.S.—*Secretaries:* Rev. Richard Sheepshanks, M.A.; Lieutenant Wm. S. Stratford, R.N.—*Foreign Secretary:* Captain W. H. Smyth, R.N. F.R.S. & A.S.—*Council:* George Biddell Airy, Esq. M.A. *Plum. Prof. Ast. Univ. of Camb.;* Hon. Lord Ashley, M.P.; Charles Babbage, Esq. M.A. F.R.S. *Lucas. Prof. Math. Univ. of Camb.;* Francis Baily, Esq. F.R.S. L.S. & G.S. & M.R.I.A.; Augustus De Morgan, Esq. B.A.; Davies Gilbert, Esq. M.P. *Pres.* R.S. F.L.S. & G.S.; John Lee, Esq. LL.D.; John William Lubbock, Esq. M.A. & F.R.S.; John Lewis Tiarks, Esq. F.R.S.; John Wrottesley, Esq. M.A.

ROYAL ACADEMY OF SCIENCES OF PARIS.

July 6th.—*Manuscripts received*:—Memoir On the height of tides, by M. Simnonnin;—Notice respecting the use of acidulated water in removing the blackness of buildings, by M. Chevalier;—A sealed packet by M. Rigal, containing drawings of an apparatus for the destruction of calculi in the bladder;—Various memoirs from the members of the scientific expedition to the Peloponnesus, from the Minister of the Interior;—Considerations respecting the establishment of a Meteorological correspondence, by M. D'Hombres-Firmas;—Memoir by M. Grimaud On a method of curing madness and tetanus.

The Academy afterwards heard two memoirs by M. Sérullas; one relating to a new double chloride of phosphorus and sulphur; the other

other On iodide and chloride of azote;—A memoir On the family of the Rubiaceæ, by M. Richard;—And lastly, a memoir by M. Baudelocque, nephew, entitled *De la Céphalotriptie*, or a new process for practising embryotomy.

July 13.—*Manuscripts*:—A sealed packet by M. Le Gallois;—A sealed packet by M. Dauger;—Essay On duodecimal calculus, by M. Gauthier;—A letter from Dr. Larroque, which disputes the accuracy of an observation cited by M. Gannel in his memoir On the means of curing Phthisis pulmonaris by chlorine;—Memoir On stammering, by M. Malbouche.

The Memoirs read at this Sitting were:—Researches by M. Donné On the influences exerted by meteorological phænomena on the dry pile;—A memoir in which Dr. Antomarchi disputes the correctness of M. Lippi's observations On the communications of the lymphatic vessels;—And lastly, a favourable report by M. Cauchy respecting M. Ostrogradsky's memoir On the propagation of waves in a cylindrical basin.

July 20.—*Manuscripts*:—Memoir by MM. Audouin and Milne Edwards On the Annelides, accompanied with a work on the hair of these animals;—Extract of a letter from M. Berzelius to M. Dulong, On the discovery of Thorina, a new earth.

Reports and Memoirs read:—Report by M. Blainville on Dr. L. Company's memoir respecting a whale thrown on the shores of the Department of the Eastern Pyrenees, on the 27th Dec. 1828. M. Blainville agrees with the author, that the animal did not belong to a new species;—Memoir by M. Portal On the communications of the lymphatic vessels, in which the author speaks favourably of the Thesis of M. Lippi;—Memoir by M. Flourens On the reproduction of bones;—Result of the complete removal of a ring of bark, by M. Du Petit-Thouars;—Notice On the conversion of organic vegetable bodies into oxalic acid by the action of caustic potash, by M. Gay-Lussac;—Report by M. Cauchy On a memoir by M. Russel d'Inval On the employment of *baguettes arithmétiques* in division.

July 27.—*Manuscripts*:—Description of an instrument to supersede proportional scales, by M. Chauvin;—Description and model of a new statical lamp, by MM. Chapuy and Marsaux;—New observations On the motions of the radicles of some plants which germinated on mercury, by M. Jules Pinot;—Receipt for an indelible ink prepared in a solid form, by M. Dizé (a sealed packet);—Observations on the developement of the Crustaceæ, and on the changes of form which these animals exhibit before they arrive at maturity, by M. Milne Edwards;—Memoir by M. Sturm On the integration of a certain system of linear differential equations;—Memoir by M. Cauchy On the equation by the aid of which the secular inequalities of the motions of the heavenly bodies is determined;—Claim of Dr. Alex. Paillard On the employment of salts of iodine in the treatment of scrofula, by M. Lugol.

Reports and Memoirs read:—Verbal report by M. Damoiseau On M. Vincen's new elements of astronomy;—Verbal report by M. Flourens On the elements of veterinary pathology of Professor

 Vatel;

Vatel;—Verbal report by M. Cauchy upon a German work by Dr. Mellin, entitled *Decouverte dans le calcul integral*;—A Notice by M. Becquerel;—And lastly, a memoir by M. Rigal On a sound proper for facilitating the introduction of instruments employed in lithotrity.

August 3.—*Manuscripts*:—Detailed account of a new case of curing Phthisis pulmonaris by means of chlorine, by Dr. Cottereau; —Observations On the question respecting the communication of the lymphatic vessels with the veins, by M. Lauth;—Experiments upon the mode of curing hiccough, by M. Vanier;—Essay upon a new electro-dynamic phænomenon, by M. de Briche;—Observations On the experiments of M. Flourens relating to respiration, by M. Desportes;—Geographical questions, by M. Duvallier.

Reports and Memoirs read:—Report by M. Becquerel On the memoir of M. Donné, concerning the influence of meteorological phænomena upon the dry pile;—Report of M. Duméril On the laws of mortality of the rich and poor, established by M. Benoiston, of Chateauneuf;—Report of M. Duméril On a case of phthisis successfully treated with chlorine with Dr. Cottereau's apparatus;—Memoir of M. Becquerel On thermo-electric power;—Observations on fossil bones occurring in the *Calcaire grossier* on the road to Nanterre, by M. Cordier.

Aug. 10.—*Manuscripts*:—Letter On the torsion of the arteries, by Dr. Thierry;—Letter from M. Vannier On the nature of madness;—Letter from M. Parnard of Avignon, on certain improvements of which the instruments used in lithotrity are susceptible.

Reports and Memoirs:—Reports of M. Cassini On a very fine collection of artificial plants, formed by the late M. Robillard of Argentelle;—Memoir by M. Le Bœuf On the diurnal motion of the earth.

Aug. 17.—*Manuscripts*:—A letter from M. Tournal On fossil bone caves;—Meteorological observations made at Berne, by M. Trechsel;—Letters from M. Duleau, occasioned by M. Savart's last memoir, in which he refers to some analogous experiments which he had made;—Letter from M. Nel, of Avignon, containing an account of some new improvements in lithontriptic instruments; —Letter from M. Dabled On the direct communication of the lymphatic vessels with the veins;—Use of the calculus of residua for the valuation and transformation of products composed of an infinite number of factors, by M. Cauchy.

Reports and Memoirs read:—A very detailed report of the last voyage of the Astrolabe, by M. de Rossel;—Report of M. Duméril On the attempts made by M. Lugol to cure scrofulous diseases by Iodine. The cases are numerous and well related;—Report of M. Cauchy On a work by M. Gomès, entitled, *Nouvelle Arithmétique*; —Experiments made upon the vegetable alkalies, and the disorders which they occasion in the animal œconomy, by M. Donné.

Aug. 24.—*Manuscripts*:—A sealed packet by M. Alphonse Sanson;—Notice by MM. François and Caventon, On the medicinal properties which they found in the root of a Brazilian shrub of the family

family of the Rubiaceæ;—An Essay On the isochronism of the spiral springs of a chronometer, without steel, and various notes, by M. Houriet;—Letter from M. Bories, requesting to be placed on the list of candidates for the professorship of Pharmacy at Montpellier; —Remainder of the late M. Paulet's treatise On the manufacture of silks, presented by his son;—M. Cassini made a very favourable report respecting the great work of M. Achille Richard, On the general study of the family of the Rubiaceæ;—M. Duméril gave an advantageous account of M. Roullin's memoir relating to the effects of the ergot of Maize upon man and animals. According to M. Mathieu's report, it does not appear that M. Vaucher's instrument for tracing parallels, contains any thing remarkable;—M. Girard gave a verbal analysis of the new history of the external navigation of France, published by M. Dutems;—M. Blainville read a memoir on the Gauga. At the request of M. Amussat, his communication On the torsion of the arteries was read; it was deposited in a sealed packet on the 20th of last June.—The section of medicine and surgery presented the following list of candidates for the vacant place of Correspondent: MM. Meckel of Halle; Fodéré of Strasbourg; Bretonneau of Tours; Abercrombie of Edinburgh; Lallemand of Montpellier; Barbier of Amiens, and Braschet of Lyons.

Aug. 31.—*Manuscripts*:—Royal ordonnance of the 23d August, relating to the employment of the legacy of Monthyon;—A sealed packet by M. Cottereau;—Memoir by M. Monpensier On the quadrature of the circle. M. Meckel had a majority of votes for the place of Correspondent.—The Academy afterwards heard: Meditations on Nature, by M. Geoffroy-Saint-Hilaire;—A Memoir by M. Cauchy On the application of the calculus of residua;—And a memoir by M. Amussat On a new process for stopping hæmorrhages from arteries and veins.

LII. *Intelligence and Miscellaneous Articles.*

DISCUSSIONS IN THE FRENCH ROYAL ACADEMY OF SCIENCES, BETWEEN M. GEOFFROY-SAINT-HILAIRE AND THE BARON CUVIER, RESPECTING THE UNITY OF ORGANIZATION IN ANIMALS, AND THE ANALOGIES BY WHICH THEY ARE CONNECTED.

[As the subject of the following controversy appears to be exciting much attention upon the Continent, we think our zoological readers will be pleased with our devoting a few pages to it. We have only to add that it has been suggested to us that Mr. W. S. MacLeay's* views respecting the osculant position of the Cephalopoda between the Mollusca and the Vertebrata, will probably be found to reconcile M. Cuvier's statement with that of his opponent,

* A writer in the Quarterly Review, instead of fairly investigating or combating the views of Mr. MacLeay has, it is alleged, greatly misrepresented them. That our readers may the better judge for themselves, we shall probably in some future Number confront the representations of the Reviewer with the original.

 without

without impugning the validity of either; and that M. Cuvier's explanation of the term '*unity of composition*' appears to be merely an expression, in terms less definite than those used by Mr. MacLeay, of some of the more comprehensive relations of analogy existing in the animal kingdom. See *Horæ Entomologicæ*, p. 248 to 260, &c. EDIT.]

ON the 15th of February last MM. Geoffroy-Saint-Hilaire and Latreille made a report on the Memoir of MM. Meyraux and Laurencet, intitled "Considerations on the Organization of Mollusca," &c. The authors have devoted themselves for some time past to considerable anatomical labours; they have prepared for early publication plates of more than *three thousand* new facts of anatomy, relating to all the difficulties of the science. These designs give the zootomy of a number of animals of the middle and lower ranks of the zoological series, such as reptiles, tortoises, salamanders, fish, crustacea, insects, and mollusca of different orders. MM. Meyraux and Laurencet, thinking themselves sufficiently prepared by the labours of which we have just spoken, have adopted as general facts the following propositions:

1st, Every molluscum presents, under an envelope more or less destitute of solid parts and of sensitive apparatus attached to them, a vegetative system resembling that of one or more superior animals. 2nd, The viscera which compose these apparatuses are placed in the Mollusca in the same connections as in the superior animals, and their functions are exercised in them by a similar mechanism and moving organs. 3rd, The connections designated as disturbed are only so in appearance; the key for discovering their undeviating constancy is furnished by the consideration that the mollusca, whose trunk, keeping in other respects a longitudinal position, is found on the contrary bent towards its half, and that the two portions returning on each other, soldered the one to the other, are reversed sometimes on what is called the ventral face, sometimes on the face called dorsal. 4th, The folds in question discover themselves externally by the respective position of the orifices. 5th, Lastly, in the case of parts resisting and fixed in the skin, these earthy masses are also comparable to certain bony portions in vertebrated animals. Desirous of supplying the justification of these theoretic views, MM. Meyraux and Laurencet make an application of it to the order Cephalopoda, and indeed to explain their idea more clearly, to one of the species in particular; viz. the cuttle-fish (*Sepia officinalis*). After following the authors in their anatomical labours upon this molluscous animal, and the remarks by which they accompany them, the reporter, M. Geoffroy-Saint-Hilaire, proceeds with the following reflections suggested to him by the organization of the cuttle-fish: "If we find inclosed in the same integuments, organs so elevated by their structure as are two venous hearts and one arterial heart, a perfectly regular set of branchiæ, medullary matter chiefly concentrated at the nape of the neck, a very large liver, a series of vessels secreting urine; if in like manner we find associated and placed together an entire intestinal apparatus, a beak

beak constructed like that of the parrot, the œsophagus, all the organs of generation very nearly repetitions of those of fish,—can it be said of so many things that it is a whole quite differently put together, quite differently combined? In order to prove this proposition, and find in it a text in favour of the most surprising of anomalies, there would be more to be done than in order to sustain the contrary position. For it would be necessary to admit that these organs, which can exist only as produced by each other (*engendrés les uns par les autres*), and in consequence of the reciprocal congruity of the nervous and circulatory action, would cease to be connected, or to accord with each other. Now such an hypothesis is not admissible: from the moment that the harmony between the organs no longer exists, life ceases: then there is an end of the animal, it is not an animal. But if, on the contrary, life goes on, it is because all these organs have remained in their habitual and invariable relations, that they act on one another as usual;—then, following up the reasoning—this is because they are linked together by the same order of formation, subjected to the same rule, and because, like everything in animal composition, they are unable to escape from the consequences of the universal law of nature, the unity of organic composition. MM. Meyraux and Laurencet have known how to appreciate the wants of science, since they have endeavoured to lessen the hiatus observed between the Cephalopoda and the higher animals. Doubtless, they have not hoped to arrive all at once at a result completely satisfactory; but we owe them at least the justice to say, that theirs is a happy attempt at opening the way, and that they have already trodden some of its paths."

M. Cuvier remarked upon a passage of the above Report, that he had not changed his opinion as to the view to be taken of the animals in question.—*Rev. Encyc.*

On the 22nd of February M. Cuvier read a Memoir intitled "Considerations on the Mollusca, and on the Cephalopoda in particular."—The author brings to mind that it was he who, now thirty-five years ago, after having made the Mollusca better known than they had before been, proved the necessity of no longer leaving animals so richly provided with organs, confounded in a single class along with the polypi and other zoophytes; and that his views on this subject have been admitted in some way or other by all naturalists.

But, in showing how the organization of the Mollusca approached in the abundance and diversity of its parts to that of the Vertebrata, M. Cuvier was far from thinking this organization composed in the same way, or arranged on the same plan: his opinion, on the contrary, had always been, that the plan, which up to a certain point is common to the Vertebrata, is not continued in the Mollusca. And with regard to composition, he never admitted that it could reasonably be called *one*, even taking it only in a single class, and *à fortiori* in different classes.

Engaged in a discussion on which he wished to avoid employing the

the time which perhaps might be occupied more usefully in study, M. Cuvier first states the circumstances which have brought on this discussion.

Two young and ingenious observers studying the respective position of the viscera of the Cephalopoda, thought that perhaps among these viscera might be found an arrangement resembling that which exists among the Vertebrata, if we represented to ourselves the cephalopodous animal as a vertebrated one, whose trunk was folded on itself backwards at the height of the navel, in such a way that the pelvis should fall on the neck. "One of our learned colleagues," continues M. Cuvier, "eagerly seizing this novel view, has announced that it completely refutes all that I had said respecting the distance which separates the Mollusca from the Vertebrata. Going even much further than the authors of the memoir, he has concluded from it that zoology has not had until now any solid basis; that it has been nothing but an edifice constructed on the sand; and that its only indestructible base for the future is a certain principle which he calls *unity of composition*, and which he asserts to be capable of an universal application."

M. Cuvier, being determined to discuss the reality of this principle, began by examining the question in its particular relation to the Mollusca.

Now first of all it is best clearly to define the terms, and to determine what we are to understand by the expressions '*unity of composition*,' '*unity of plan*.' If the words were taken in their most rigorous acceptation, it could not be said that there is unity of composition in two kinds of animals, only except in so far as they are *composed* of the same organs: so likewise to prove that there is unity of *plan* in their organization, it would be necessary to show that these identical organs are disposed in the same order in both.

But it is impossible to suppose that the naturalists who speak of *unity of composition*, of *unity of plan*, in the whole of the animal kingdom, should have understood things thus, or that they should have wished to maintain *that all animals are composed of the same organs, arranged in the same manner.*

Now the terms being thus defined, the principle of *unity*, restricted as it ought to be, appears incontestably true; but it is very far from being new. It forms, on the contrary, one of the bases upon which zoology has reposed ever since its origin,—one of the principles upon which Aristotle, its creator, has placed it,—a basis which all zoologists worthy of that name have sought to enlarge, and to the confirmation of which all the efforts of anatomy have been devoted.

Thus every day might be discovered in an animal some part which was not before known to exist in it, and which enables us to seize on some additional analogy between that animal and those of different genera and classes. The same may be the case with respect to connections of newly-perceived relations. Labours undertaken in this direction are eminently useful, and those of M. Geoffroy-St.-Hilaire in particular are worthy of all the esteem of naturalists. When, for instance,

instance, he discovered that in comparing the head of a fœtus of a quadruped with that of a reptile or of an oviparous animal in general, relations were remarked in the number and the arrangement of the pieces, which were not to be perceived in adult heads,—when he proved that the *os quadratum* in birds, is analogous to the *tympanum* in the fœtus of the Mammiferæ, he made discoveries quite real and very important, to which M. Cuvier was the first to render justice in the report which he made on them to the Academy. These are additional traits which he has added to resemblances of different degrees which exist among the composition of different animals; but he has only *added* to the old and known bases of zoology, he has in no respect *changed* them. He has not at all proved either the unity or the identity of this composition, nor indeed any thing that can afford ground for the establishment of a new principle.

Thus all naturalists have long known that the Cetacea have on the sides of the anus two little bones called the rudiments of the pelvis. There is then here, and it has been known for ages, a slight resemblance of composition; but nothing can lead to the belief that there is unity of composition, since this rudiment of a pelvis supports none of the bones of the lower extremity.

In one word, if by unity of composition be meant *identity*, a thing is affirmed contrary to the plainest testimony of the senses.

If by it be meant *resemblance*, *analogy*, a thing is affirmed which is true within certain limits, but which is as old in its principle as zoology itself, and to which the newest discoveries have only added, in certain cases, features more or less important, without altering any thing in its nature.

Moreover, M. Cuvier (and it is in this especially that he differs from the naturalists whom he combats) is far from regarding this principle, so important and so long established, as a single principle; on the contrary, he sees in it only a principle subordinate to another, much more elevated and much more fruitful—namely, certain *conditions of existence*, *of the suitability of parts*, of their *adaptation for the part which the animal is to perform in nature*. Such is the true philosophic principle from which the possibility of certain resemblances may flow, and the impossibility of certain others. Such is the rational principle whence that of analogies of plan and of composition is deduced, and in which, at the same time, it finds limits which it would be vain to attempt to overlook.

The reality of a certain analogy of composition and of plan being recognised, naturalists have nothing else to do, (and they do in fact nothing else,) but to examine how far this resemblance extends; in what cases and on what points it stops; and if there are beings in which it is reduced to such a trifle that it may be said to be nothing. It is the especial object of comparative anatomy, which is far from being a modern science, since its first author was Aristotle.

M. Cuvier announces that, in the new edition of his "Lessons of Comparative Anatomy," which he is preparing, excited by a desire to reduce to just bounds what has been vaguely said on this subject, he will consider animals particularly under this point of view, taking care

to

to avail himself of all new discoveries, in order to mark as much as possible the extent and limits of the analogies that exist among animals. At present he is to attend especially to the Cephalopoda, a subject which he remarked had been happily chosen by his learned colleague, since there is none that can better show what truth there may be in the principles under discussion, and how far they are vague and exaggerated.

M. Cuvier here enters into the details of the discussion of the point of view set forth by MM. Laurencet and Meyraux, a point of view that consists in considering the Mollusca as species of vertebrated animals, bent backwards at the navel, so that the two portions of the spine of the back come in contact. To appreciate the justice of this view, M. Cuvier took, in the first place, a vertebrated animal, which he bent back, as was required, with the pelvis towards the nape of the neck, and removed all the integuments of one side, in order to show the interior parts well *in situ*. He then took a cuttle-fish, placed it by the side of the vertebrated animal, and examined the respective situation of the parts.

Passing in review successively the respective position of the head and of the different parts which compose it, the large vessels, and the organs of generation, the author concludes, from a very detailed comparative examination of these parts, that the analogy which the authors of the Memoir had thought that they observed, is illusory nearly throughout.

He even thinks that it would be more easy to establish some analogy of situation, by supposing the animal to be bent in a direction the reverse of that of the hypothesis; then, indeed, the brain, the liver, œsophagus, stomachs, and the great artery, would remain in the same respective position as in vertebrated animals; but the hearts, veins, branchiæ, and organs of generation, would still be differently placed, and the problem would not be solved; and further still, M. Cuvier thinks he may affirm that it is impossible that it ever should be, entirely. Those important organs, the heart and the branchiæ, always in connection with the œsophagus in vertebrated animals, are here at a great distance from it, and without any connection; and from this circumstance necessarily results an entirely different direction in the vessels. Now since the plan of an animal essentially depends upon the distribution of the vessels which convey nutrition and life to its organs, it may, *à priori*, be asserted that the identity of plan of the Cephalopoda and the Vertebrata will never be demonstrated except in a very partial manner.

Another generating element of the plan of animals, perhaps still more essential than their vessels, is their nervous system. But how can it be said that there is here the least analogy? The brain is inclosed in a cavity of the cartilaginous ring, which serves as a base to the tentacula. It furnishes in front the nerves of the buccal mass (cheek pouches), then an expansion which occupies the side of the cartilaginous ring, and supplies the nerves of the great tentacula, in order to produce the enormous ganglion of the eye; another branch swells out a little further on into a ganglion, whence the

the nerves of the sac divide and radiate; a third, joined to the corresponding one, descends into the abdomen, and is distributed among the viscera; a small thread goes to the ear. There is not the slightest trace of a spinal marrow, or of those numerous pairs of nerves which spring from it so regularly in vertebrated animals. So, likewise, there is neither spine of the back, nor any of the limb pairs or side pairs which are attached to it.

That which has misled the authors of the Memoir is the position of the ear on the side of the cartilaginous ring opposite to the brain: as in the Vertebrata the ear is towards the back of the head, they have supposed that it denoted the nape of the neck; but the ear in the Vertebrata is not only at the back of the head, it is also under that part, under the brain: in the cuttle-fish it is the same, because that part of the ring is the lower; only the two ears, instead of resting simply on the sides of the œsophagus, descend lower down, and embrace it on the lower side; but they are always underneath.

Thus, in short, the Cephalopoda have a brain inclosed in a separate cavity, eyes, ears in the form of two mandibles, a tongue, salivary glands, an œsophagus, a gizzard, a second stomach, an intestinal canal, a liver, branchiæ, hearts, arteries, veins, nerves, organs of the two sexes, ovaries, testicles, oviducts, epididymis, penis, all things which are common to them with certain Vertebrata; but all these differently disposed; nearly always differently organized.

At the same time they want all the bones of the cranium, all those of the face, true jaws, all the bones of the Hyoidian apparatus, and of the branchial apparatus; all the vertebræ, all the bones of the extremities, the sides of the sternum, the muscles attached to all these parts, the spinal marrow, all the nerves which spring from it, the pancreas, the kidneys, and the bladder.

At the same time, too, they have many parts, of which there is no trace in vertebrated animals; a muscular apparatus quite different and adapted to a form so extraordinary; frequently a shell, of a structure truly remarkable, and of which no vertebrated animal presents the slightest vestige; an excrementitial organ which produces that black liquor known under the name of *encre de seiche* or *sepia;* a spongy or glandulous apparatus, which communicates directly with their veins by a multitude of orifices.

The tentacula themselves, which it has been wished to compare to the cirrhi of fish, neither resemble them in organization nor in their connections. Their complication is prodigious: nerves enlarged from space to space into numerous ganglions, furnish innumerable threads; very distinct vessels, divided also into innumerable branches, pass through them and animate them; suckers of an admirable structure supply them with an armour of an unique kind: lastly, the principal cirrhus of fishes is but a prolongation of their maxillary bone, while the tentacula of the Cephalopoda are not even attached to the beak, which, without absolutely representing the jaws, nevertheless performs their functions.

"I ask," says M. Cuvier, "how, with these numerous, these vast differ-

differences,—how can it be said that between the Cephalopoda and the Vertebrata there is an *identity of composition, a unity of composition*, without perverting the terms of language from their most obvious sense? I shall bring back these facts to their true expression, when I say that the Cephalopoda have many organs which are common to them with the Vertebrata, and which in them perform similar functions; but that these organs are differently arranged with relation to each other, and are often constructed in another manner;—that in them they are accompanied by several other organs which the Vertebrata have not;—while the latter have also on their part several which are wanting in the Cephalopoda."

M. Cuvier announces to the Academy other communications in which he will examine various other principles, various other laws set forth by different naturalists. But in order that these readings may not be confined to idle metaphysical questions, he will take care always to connect them, like the present one, with some ascertainment of facts from which the science may derive advantage.

M. Geoffroy-Saint-Hilaire heard the Memoir of M. Cuvier with the greatest pleasure: he was delighted to see the discussion opened upon the grand principle whose existence he asserts. He will reply, and will state precisely what he means by *unity of organic composition*. In the mean time he thought it necessary to remark, that it was not he who had sought to apply this principle to the Mollusca, but MM. Laurencet and Meyraux; it is therefore for them to support their views against the observations of M. Cuvier. For himself, called on to form a judgement of this view, which appeared to him ingenious, he has only said what he thought of it, without wishing to take upon himself the responsibility of it. "I shall make," continued M. Geoffroy, "a single remark on the subject: You have heard the long enumeration of the organs which the Cephalopoda have in common with the Vertebrata. Now at first sight it seems to me much more difficult to conceive how animals which have so many similar organs could be arranged on different plans, than to comprehend how, in spite of the difference of distribution which appears at first sight to exist among them, it could be possible to conceive them as arranged on the same plan."—*Le Globe*, No. 10.

[The discussion has been continued at various sittings of the Academy, down to that of the 12th of April. We shall endeavour to present our readers with a view of the remainder in our next and following Numbers.—EDIT.]

ON KERMES MINERAL. BY M. GAY-LUSSAC.

According to the latest researches of M. Berzelius, and those of M. Rose, kermes mineral is merely common sulphuret of antimony, the colour of which is owing to its state of minute division.

Not being perfectly satisfied with the proofs adduced in support of this composition, I made some experiments, which have induced me to form a different opinion, and which approximates the idea that the greater number of chemists have entertained, particularly since the researches of M. Robiquet. These experiments are very ancient; and

and I should content myself with the publicity which I have given to them in my lectures, if M. Henry, jun., who has lately published an interesting memoir on kermes, had not left me some observations to add. I shall distinguish the precipitates formed by sulphuretted hydrogen in a solution of antimony from kermes, properly so called, because their natures are very different.

The orange red precipitate, obtained by passing sulphuretted hydrogen into a solution of emetic tartar, is an hydrated protosulphuret of antimony. In fact, neither weak muriatic acid nor tartar separates any oxide from it; and when solution is effected, it is always accompanied with the disengagement of sulphuretted hydrogen.

This sulphuret when dried at 212° Fahr. contains some water, but not in quantity sufficient to form an hydrosulphuret; it is gradually expelled up to 446°; at this point it contains no more, and becomes black; when rubbed on paper it gives black marks. It appears to me analogous to hydrated peroxide of iron, which loses its water gradually, becoming more and more brown, as the temperature rises, and assuming a red colour only when it has lost all its water.

Sulphuretted hydrogen produces also a red precipitate in the solution of permuriate of antimony, but it differs from that obtained from emetic tartar or the protomuriate; it is an hydrated persulphuret, which heat decomposes into sulphur, which is volatilized and black protosulphuret, similar to the preceding. It is to be observed, that the black sulphuret obtained by calcining the orange-red sulphuret, is less fusible than the native black sulphuret; it resists the action of a spirit-lamp.

It is well known that kermes varies in colour, at least, according to the mode adopted in preparing it. It is that obtained by the process of Cluzel (*Annales de Chimie*, tom. lxiii. p. 122), upon which my observations will be made. We shall be greatly deceived if we suppose that because kermes still yields something to water, after numerous washings, it is pure only when it ceases to do so; for if we were to wash the subacetate of copper, and many other salts, till water ceased to dissolve any portion of them, they would be completely decomposed. The fact is the same with kermes, too much washing alters its nature. At what point then ought we to stop? This is readily discovered by employing the smallest possible quantity of water in the washings, and in continuing them only until the residue, supposing the water to have no chemical action upon it, contains only one-thousandth or a ten-thousandth of foreign matter.

Kermes mineral, thus washed, has the following properties:—dilute muriatic acid, tartaric acid and bitartrate of potash, take protoxide of antimony from it without disengaging sulphuretted hydrogen; when dried for a long time at 7°, and even at 212°, it still contains water; heated by a spirit-lamp it becomes black, and yields water, which, as observed by M. Robiquet, is slightly ammoniacal. At a higher temperature it fuses and swells up, on account of a little sulphurous gas which is disengaged. When in layers upon glass, it gives it a deep red colour, and rubbed upon paper it gives a reddish-brown colour: it is more fusible than the black sulphuret obtained by the calcina-

 tion

tion of the hydrated orange sulphuret. If a current of hydrogen be passed at a low red heat over kermes deprived of moisture by heat, much water and sulphuretted hydrogen are obtained, and the antimony is reduced; but, as already observed, the residue possesses an alkaline re-action. After these various experiments, it is unquestionable that kermes contains oxide and sulphuret of antimony, and it ought to be considered as an oxisulphuret. The quantity of water obtained by decomposing it with hydrogen is variable; but it may be considered as composed of one proportion of protoxide of antimony and two proportions of protosulphuret. In fact, I obtained 0·9 of the proportion of protoxide; and M. Henry by another process found the difference still less.

It is equally certain that kermes mineral precipitated from the alkaline sulphurets which held it in solution, is an hydrate. It loses water gradually as the temperature is raised, and appears black when deprived of it; but in my experiments I did not obtain a definite proportion.

When potash, soda, or their carbonates act upon black sulphuret of antimony, their oxygen goes to the antimony, with which it forms protoxide, and the sulphur of the antimony takes the place of the oxygen of the alkali: thus it is that no kermes is obtained by boiling sulphuret of antimony with sulphuret of potassium saturated with sulphur; but by means of acid, a yellowish orange precipitate is formed in the solution, which when heated yields sulphur and becomes black. The golden sulphuret gives a similar result.—*Ann. de Chim. et Phys.* tom. xlii. p. 88.

THORINA AND THORINUM. BY BERZELIUS.

In the Phil. Mag. for November last, we noticed Berzelius's discovery of a new earth or rather metallic oxide, to which he gave the name of Thorina: we add the following particulars respecting it. Thorina hardens in the fire, and it becomes difficult to powder it. Its specific gravity is greater than that of any other earth, and almost equal to that of oxide of lead, being 9·402. It is infusible and unalterable by the blowpipe *per se*. With borax it dissolves extremely slowly and the resulting glass is not transparent, but it may be so saturated as to become milky on cooling. It dissolves slowly with salt of phosphorus, and carbonate of soda does not dissolve it at all.

One hundred parts of thorina appear to consist of

Thorina	88·16
Oxygen	11·84

and one hundred parts of hydrate of thorina of

Thorina	88·25
Water	11·75

The weight of its atom, according to Berzelius, is 884·9; hydrogen being unity, we may perhaps assume it to be 70.

Thorina is distinguished from other earths principally by its combination with sulphuric acid; in this combination, heat precipitates a salt, which by cooling slowly, but completely, redissolves. It is, however, to be observed with respect to this re-action, that it does not occur

cur when those bases are present, with which thorina forms double salts; the latter are but little precipitated by heat. It differs from alumina and glucina, because it is insoluble in caustic potash, which dissolves those substances ; from yttria, because it forms a double salt with sulphate of potash, which is insoluble in a saturated solution of sulphate of potash : this circumstance furnishes a method of separating it from yttria. It differs from zirconia, because the latter, after having been precipitated hot by sulphate of potash, is for the most part insoluble in water and the acids, and because thorina is precipitated by ferrocyanate of potash, and zirconia is not. It differs from the oxide of cerium, in not assuming the same colour when it is dried and burnt, and also in not giving with the blowpipe a coloured compound with salt of phosphorus, or borax, either hot or cold, provided it be not at all mixed with iron.

From titanic acid it is distinguished by its precipitation with sulphate of potash, or by the characteristic manner of that acid with the blowpipe.

From the metallic oxides, properly so called, and with which there is some inducement to place it on account of its great specific gravity, it is distinguished by not being precipitated with sulphuretted hydrogen.—*Bibliotheque Universelle*, Dec. 1829.

PREPARATION OF MAGNESIUM.

M. Bussy has announced to the French Academy, that he has obtained magnesium or the metallic base of magnesia, and he sent a specimen of the metal; it was procured by the decomposition of the chloride by a process similar to that employed by M. Wöhler.

Magnesium has the following properties :—it is brilliant, silvery white, perfectly ductile and malleable, fusible at a moderate temperature, like zinc, volatilized at a temperature a little higher than that of its melting point, and like that metal condenses into small globules. It does not decompose water at common temperatures ; it oxidizes at a high temperature, and is slowly converted into magnesia, when in small masses ; but when in filings, it burns with great splendour, throwing out sparks, like iron in oxygen.

M. Bussy is of opinion, that magnesium may be usefully employed in the arts ; and he is occupied with endeavouring to find means of procuring it in a simple and œconomical manner.—*Le Globe.*

NEW NATIVE COMPOUND OF CARBONATE OF LIME AND CARBONATE OF SODA.

This mineral differs from Gay-Lussite, which is a compound of the same salts; it was procured by M. Barruel at a mineral dealer's, who did not know its origin. The structure of this substance is laminated, has an easy threefold cleavage, and gives a rhomboid similar to that of carbonate of lime, as nearly as could be discovered by comparing the crystals of these two substances.

The fragments are perfectly transparent; the lustre is vitreous, resembling that of arragonite. It scratches carbonate of lime readily, and arragonite with difficulty. Its sp. gr. is 2·921, and the double refraction

refraction similar to that of Iceland spar. It dissolves entirely and with effervescence in nitric acid; before the blowpipe, *per se*, it first decrepitates a little, then becomes brown, and is eventually reduced to lime, but with more difficulty than pure limestone.

In order to analyse it, M. Barruel heated it to redness, by which it lost 46 per cent. of carbonic acid and water; it was then dissolved in dilute nitric acid, and filtered to separate the insoluble gangue; the solution was treated with ammonia to separate the iron, and carbonate of ammonia threw down carbonate of lime; it was ascertained to contain neither barytes nor strontia. The filtered liquors were evaporated to dryness, then heated to redness in a platina crucible, and an efflorescent residuum was obtained, which converted into a nitrate was slightly deliquescent; it was carbonate of soda. The analysis gave

Carbonate of lime..............	70
Carbonate of soda..............	14
Water........................	9·7
Peroxide of iron..............	1
Gangue.......................	5
	——99·7

Or, according to M. Barruel, the mineral consists of 11 atoms carbonate of lime, 2 atoms carbonate of soda, and 9 atoms water. —*Le Globe.*

CIRCULATED BY THE ASTRONOMICAL SOCIETY.

Lunar Occultations of some of the principal fixed Stars, for the months of May and June 1830. *Computed, for the meridian and parallel of Greenwich, by Lieut. W. S. Stratford, R.N., Secretary to the Society.*

Date.	Stars' Names.	Astr. Soc. Cat. No.	Mag.	Immersions.			Emersions.		
				Sidereal time.	Mean solar time.	Angle.	Sidereal time.	Mean solar time.	Angle.
1830.				h m	h m	°	h m	h m	°
May 1	48 Leonis	1256	5·6	13 53	11 15	118	14 59	12 21	274
5	77 Virginis	1548	7	11 35	8 43	351	12 15	9 23	289
11	P. xviii. 112	2144	7	below	the	horizon	14 50	11 33	247
—	——— 121	2149	7	14 26	11 9	60	15 39	12 22	238
12	P. xix. 180	2293	7	18 0	14 39	93	19 20	15 58	279
22	α Tauri	528	1	10 55	6 56	175	11 33	7 34	269
24	u Geminor.	828	5·6	13 7	9 0	127	14 0	9 52	297
June 2	y Virginis	1608	6·7	15 51	11 7	17	16 11	11 27	349
—	k ———	1495	4	17 45	13 1	134	18 47	14 3	264
4	η Libræ	1787	4·5	14 25	9 33	100	15 38	10 46	224
8	d Sagittarii	2230	5	16 25	11 18	46	17 34	12 27	278
25	o^1 Sextantis	1277	6	14 28	8 15	73	15 24	9 11	318
—	o^2 ———	1280	7	15 3	8 49	96	16 5	9 51	298
28	θ Virginis	1511	4·5	18 6	11 41	64	below	the	horizon

The *angles* are reckoned from the vertex of the moon's *image*, as viewed in an inverting telescope, towards the right hand, round the circumference.

Results of a Meteorological Journal for the Year 1829, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

By William Burney, *LL.D.*

Latitude 50° 47′ 45″ North: Longitude 1° 7′ West of Greenwich—In time 4′ 28″.

1829. Months.	Barometer.										Self-registering Thermometer.								Mean Temp. of Spring Water.	De Luc's Hygrometer.						
	Max.	Min.	Media.	Range.	No. of Changes.	Spaces described.	Greatest Variation in 24 hours.	Media at 8 A.M.	Media at 2 P.M.	Media at 8 P.M.	Max.	Min.	Media.	Mean Range.	Gt. Var. in 24 hours.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.		Max.	Min.	Mean Range of the Index.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Media at 8,2,& 8 o'cl.
	In.	In.	In.	In.		In.	In.	In.	In.	In.	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Jan.	30·20	29·00	29·708	1·20	15	6·08	0·56	29·695	29·695	29·722	49	21	36·13	28	19	38·90	34·35	35·93	52·12	88	52	36	68·2	75·9	74·2	72·7
Feb.	30·52	29·02	29·993	1·50	17	4·70	0·54	29·993	29·983	29·988	56	27	44·48	29	19	48·25	42·07	43·82	50·32	89	54	35	68·3	76·6	75·3	73·4
Mar.	30·24	29·16	29·763	1·08	17	4·28	0·42	29·767	29·750	29·758	60	30	44·47	30	22	49·29	41·61	43·00	50·00	85	43	42	56·4	69·4	65·5	63·8
April	30·00	28·86	29·497	1·12	23	6·68	0·44	29·498	29·488	29·498	65	31	48·68	34	24	53·17	46·13	46·17	49·94	90	42	48	59·3	67·9	71·8	66·3
May	30·32	29·54	29·996	0·78	23	3·05	0·43	29·997	29·995	29·996	73	44	57·00	29	26	63·93	56·13	55·52	50·64	82	36	46	49·7	56·6	59·2	55·2
June	30·37	29·36	29·998	1·01	15	3·45	0·40	30·009	30·003	29·988	76	42	61·02	34	24	67·20	60·90	59·27	51·76	86	35	51	50·2	55·6	59·6	55·1
July	30·31	29·36	29·889	0·95	21	6·10	0·47	29·890	29·888	29·887	74	47	61·97	27	21	66·77	61·84	60·22	53·15	88	42	46	63·5	67·2	74·6	68·4
Aug.	30·36	29·32	29·963	1·04	15	7·34	0·60	29·960	29·968	29·967	74	46	60·47	28	19	65·58	59·55	58·97	54·33	95	48	47	62·2	69·3	73·5	68·3
Sept.	30·28	29·24	29·821	1·04	28	7·71	0·66	29·823	29·832	29·819	69	39	55·87	30	19	61·37	54·10	54·16	54·96	97	52	45	61·8	73·8	78·8	71·5
Oct.	30·50	29·42	30·077	1·08	22	7·17	0·77	30·073	30·068	30·088	65	30	50·77	35	21	54·58	49·45	49·09	54·38	98	52	46	68·7	75·8	79·8	74·8
Nov.	30·44	29·55	30·021	0·89	21	4·84	0·66	30·027	30·009	30·018	58	30	43·70	28	20	46·80	41·40	43·56	53·24	98	52	46	71·7	78·1	78·5	76·1
Dec.	30·58	29·60	30·117	0·98	21	4·38	0·53	30·110	30·101	30·127	53	18	36·13	35	18	38·51	34·84	36·39	51·12	96	65	31	79·3	84·1	82·4	81·9
Aver.	30·58	28·88	29·903	12·67	238	65·78	0·77	29·903	29·898	29·905	76	18	50·06	30·58	26	54·53	48·53	48·84	52·16	98	35	43·2	63·3	70·8	72·8	68·9

TABLE (continued).

1829. Months.	Scale of the Winds. North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.
January ..	7	12	5	...	...	2	½	4½	31
February	4	3½	5	4	2	3½	2	4	28
March ...	1	14	7	5	1	1½	½	1	31
April	2	3	1	3	6	6	3	6	30
May	2	10	2½	3	1½	5	5	2	31
June	2½	2½	2	8	2	4	3	6	30
July	1	1	1½	2	4	13	6	2½	31
August ..	3½	½	½	3	2	8	8	5½	31
September	5½	2½	...	1	3	8½	6½	3	30
October ..	6	4	1	½	...	8	4	7½	31
November	3	9½	2½	1	1½	3½	3	6	30
December	5	14½	4½	1½	2½	1	...	2	31
Results for 1829.	42½	77	32½	32	25½	64	41½	50	365

1829. Months.	Modifications of Clouds. Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbi.	Weather. A clear Sky.	Fair, with Clouds.	An overcast Sky.	Foggy.	Rain, &c.	Total Number of Days.
January ..	9	4	30	1	11	14	14	2½	11	14	...	3½	31
February	7	3	26	1	4	11	17	2	6	13	1	6	28
March ...	13	3	30	1	12	22	12	4	9½	13½	...	4	31
April	27	13	30	...	22	28	27	3	12	7	...	8	30
May	22	10	25	...	21	10	7	6	18	5	1	1	31
June	26	16	29	...	24	22	17	3	17	6	...	4	30
July	25	16	28	...	24	28	26	2½	13	7½	...	8	31
August ..	21	16	28	...	21	27	19	2	14	8	½	6½	31
September	25	10	28	...	25	22	21	4	14	4	...	8	30
October ..	20	6	29	...	14	17	15	4½	13½	8½	...	4½	31
November	11	3	27	1	9	8	11	4½	6	15	½	4	30
December	10	5	30	2	2	13	13	2	9	15	1	4	31
Results for 1829.	216	105	340	6	189	222	199	40	143	116½	4	61½	365

1829. Months.	Atmospheric Phænomena. Anthelia.	Parhelia.	Paraselene.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.	Evaporation in Inches, &c.	Rain in Inches, &c.
January ..	...	...	...	...	...	...	...	...	...	0·60	1·390
February	...	...	...	...	1	...	2	...	...	0·70	0·900
March ...	...	...	...	2	1	...	2	...	...	1·70	0·940
April	...	1	...	5	2	4	2	...	1	1·60	6·465
May	...	5	...	4	3	...	...	1	...	5·05	0·295
June	...	...	...	6	2	3	2	2	3	4·35	2·270
July	...	2	...	3	...	7	...	1	4	3·30	5·385
August ..	...	...	...	2	1	3	6	2	2	2·85	3·330
September	...	...	...	5	1	5	3	4	2	1·70	4·590
October ..	...	...	1	1	2	...	5	...	...	1·55	1·470
November	...	...	...	...	...	1	2	...	...	1·00	1·670
December	...	...	...	...	1	...	4	...	...	0·80	1·200
Results for 1829.		8	1	28	14	23	28	10	12	25·20	29·905

ANNUAL RESULTS FOR 1829.

Barometer.	Inches.
Greatest pressure of the atmosphere, Dec. 31st. Wind N.E.	30·580
Least ditto ditto April 14th. Wind S.W.	28·880
Range of the quicksilver	1·700
Annual mean pressure of the atmosphere	29·903
Mean pressure for 180 days with the moon in North decl.	29·917
———— for 174 days with the moon in South decl.	29·878
Annual mean pressure at 8 o'clock A.M.	29·903
———— at 2 o'clock P.M.	29·898
———— at 8 o'clock P.M.	29·905
Greatest range of the quicksilver in February	1·500
Least range of ditto in May	0·780
Greatest annual variation in 24 hours in October	0·770
Least of the greatest variations in 24 hours in June	0·400
Aggregate of the spaces described by the rising and falling of the quicksilver	65·780
Number of changes	238·

Self-registering Day and Night Thermometer.	Degrees.
Greatest thermometrical heat, June 3rd. Wind N.W.	76
———— cold, December 27th. Wind N.E.	18
Range of the thermometer between the extremes	58
Annual mean temperature of the external air	50·06
———— at 8 A.M.	48·53
———— at 8 P.M.	48·84
———— at 2 P.M.	54·53
Greatest range in October and December	35·00
Least of the greatest monthly ranges in July	27·00
Annual mean range	30·58
Greatest monthly variation in 24 hours in May	26·00
Least of the greatest variations in 24 hours in December	18·00
Annual mean temperature of spring water at 8 o'clock A.M.	52·16

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the atmosphere, in October and November	98
Greatest dryness of ditto, June 5th	35
Range of the index between the extremes	63
Annual mean state of the hygrometer at 8 o'clock A.M.	70·8
———— at 8 o'clock P.M.	72·8
———— at 2 o'clock P.M.	63·3
———— at 8, 2, and 8 o'clock	68·9
Greatest mean monthly humidity of the atmosphere in Dec.	81·9
———— dryness of ditto in June	55·1

Position of the Winds.	Days.
From North to North-east	$42\frac{1}{2}$
—— North-east to East	77
—— East to South-east	$32\frac{1}{2}$
—— South-east to South	32
—— South to South-west	$25\frac{1}{2}$
—— South-west to West	64
—— West to North-west	$41\frac{1}{2}$
—— North-west to North	50
	——365

Clouds, agreeably to the Nomenclature, or the Number of Days on which each Modification has appeared.

	Days.		Days.
Cirrus	216	Cumulus	189
Cirrocumulus	105	Cumulostratus	222
Cirrostratus	340	Nimbus	199
Stratus	6		

General State of the Weather.	Days.
A transparent atmosphere without clouds	40
Fair, with various modifications of clouds	143
An overcast sky without rain	$116\frac{1}{2}$
Foggy	4
Rain, hail, and snow	$61\frac{1}{2}$
	——365

Atmospheric Phænomena.	No.
Parhelia, or mock-suns on the sides of the true sun	8
Paraselene, or mock-moon	1
Solar halos	28
Lunar halos	14
Rainbows	23
Meteors of various sizes	28
Aurora Borealis	1
Lightning, days on which it happened	10
Thunder, ditto ditto	12

Evaporation.	Inches.
Greatest monthly quantity in May	5·05
Least monthly quantity in January	0·60
Total amount for the year	25·20

Rain.	
Greatest monthly depth in April	6·465
Least monthly depth in May	0·295
Total amount for the year, near the ground	29·905

The instruments with which the observations were made are the same, and were placed in the same situation as they have been for many years past.

BAROME-

Barometrical Pressure.—The mean pressure this year is $\frac{3}{100}$ of an inch higher than the mean of the last fourteen years; but the number of changes is less than in any former year of that period.

The mean pressure for December is high, but that for April, which was a very wet and stormy month, was remarkably low.

The mean of the pressures at 8, 2, and 8 o'clock, coincides with the annual mean.

Temperature.—The mean temperature of the external air this year, is lower than that of any year since 1816: it is, however, only $\frac{7}{100}$ of a degree lower than that of 1820, and 4·43 degrees lower than that of 1828, which is the greatest difference that has yet occurred here between any two consecutive years.

The mean temperatures of January and December were low, and they exactly agree with each other: the only remarkable anomaly that happened is, that the mean of March was one-hundredth of a degree lower than the mean of February.

It may be seen by the table that the yearly *maximum* temperature of the external air did not exceed summer heat, which is the least that has occurred since the year 1823, and that the *minimum* was fourteen degrees below the freezing point in December; but a thermometer that was exposed on a bed of snow the same night receded four degrees lower.

The mean yearly temperatures at 8 A.M. and 8 P.M. coincide within about three-tenths of a degree.

The mean yearly temperature of *four* observations each day; namely, the *maximum*, *minimum*, at 8 A.M. and 8 P.M., is only 49·14 degrees. This should be considered the truest yearly mean temperature, because the mean of the greatest number of thermometrical observations made every twenty-four hours, must unquestionably approach nearest to the true mean temperature of any place where a meteorological journal is kept,—a circumstance that has not been duly considered, nor practically applied by those who have written extensively on meteorology in this country, to obtain the annual mean temperature. By this method of ascertaining the mean temperature of the external air here for the last fourteen years, it appears to be *seven-eighths* of a degree lower than the mean of the *maxima* and *minima* or daily extremes, for the same period of time.

The yearly mean state of De Luc's whalebone hygrometer indicates that the atmosphere was three-tenths of a degree drier than in 1828.

Winds.—The scale of the prevailing winds shows that their continuance from the eastern side of the meridian is nearly equal to those from the western side. This sometimes happens in comparatively cold years, unless the North-west wind be of long duration.

The continuance of the North-east wind is beyond all former observations, which appears to have lessened the duration of the wind from the opposite point. This circumstance may have caused the deficiency in the mean yearly depth of rain, and the slow evaporation;

poration; as the North-east winds, except those in May, were mostly humid.

The number of strong gales of wind, or the days on which they have prevailed this year, is as in the following scale:

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Gales.
10	26	6	5	8	27	5	7	94

The year 1828 was unprecedented in its high annual mean temperature, and 1829 was the coldest since the memorable year 1816.

January was seasonably cold, mostly frosty, and rather dry, with a dark atmosphere and frequent gales of wind.

February was also dry, calm, remarkably cloudy and humid, insomuch that the sun, moon and stars were seen only a few days, and was equal to March in its mean temperature.

March was generally dry, particularly the first fortnight, rather cold, with prevailing winds from the N.E. and E., which kept back the spring considerably.

April was cold, boisterous, and very wet throughout, which in many shaded places injured the bloom of the fruit trees.

May was very dry, warm, and a fine spring month, attended with a great evaporation towards the end.

The first part of *June* was also dry and fine, and the maximum temperature of the external air for the year occurred on the 3rd! The latter part was distinguished by very seasonable showers of rain, and intervals of strong sunshine, which produced astonishing effects in the growth and maturation of the corn and fruits; but the frequency of rain was inconvenient for the making of hay, consequently much of it was spoiled in this and the following month before it could be put in ricks.

July was very wet and stormy, there being only six days without rain. The month altogether was appalling to the grower, and furnished a bad omen for the fate of the corn harvest.

The first part of *August* being fine, the wheat harvest was in many places got in tolerably well; but in places where it was not sufficiently ripe, and where every advantage was not taken of intervening fine weather, the greatest difficulties were experienced from the rains and heavy gales of wind, in securing even average crops from most abundant ones. This furnished an instructive lesson to those agriculturists who were careless about the precariousness of the harvest weather, and remunerated the forethought and industry of others.

September was also wet and stormy, and unusually cold, with the exception of a few days, which afforded good opportunities for getting in the barley, oats, &c. More abundant crops of almost every description of fruit, except wall fruit, had not occurred for many years, notwithstanding much was blown off the trees before it was

was ripe. The second crops of grass were good and plentiful, but they were very generally spoiled by the continual rains.

October was tolerably fine, with frequent light showers of rain and hail, accompanied with heavy gales of wind. Ice and snow appeared on the ground on the 8th in several parts of Hampshire,—a very early beginning of wintry weather.

November was rather dry and fine, and alternately mild and cold, with great changes in the temperature of the atmosphere, and blusterous winds towards the end. In the night of the 23rd, and on the 24th, three inches in depth of snow fell here, which was unusually early for so great a quantity.

December was rather dry, cold and cloudy, except a few days; the piercing N.E. winds closed the year by an induction of hard frosts, and thus gave an early advantage to skaters and sliders to pursue a pleasant exercise on the ice-bound marshes, moats and ponds.

LIST OF NEW PATENTS.

To F. Westby, Leicester, cutler, for his improved apparatus for the purpose of whetting or sharpening razors, penknives, or other cutting instruments.—Dated the 26th of November 1829.—2 months allowed to enrol specification.

To J. Marshall, Southampton-street, Strand, Middlesex, tea-dealer, for his method of preparing or making an extract from cocoa, which he denominates, Marshall's Extract of Cocoa.—10th of December.—2 months.

To B. Goulson, Pendleton, Lancashire, surgeon, for his improvements in the manufacturing of farina and sugar from vegetable productions.—14th of December.—6 months.

To C. Derosne, Leicester-square, Middlesex, gentleman, for certain improvements, communicated from abroad, in extracting sugar or syrups from cane-juice and other substances containing sugar, and in refining sugar and syrups.—14th of December.—2 months.

To W. Hale, Colchester, Essex, mechanist, for his method of raising or forcing water for propelling vessels.—12th of January 1830.—6 months.

To J. Carpenter, Willenhall, Wolverhampton, Stafford, and J. Young, Wolverhampton, locksmiths, for their improvements on locks and other securities applicable to doors and other purposes.—18th of January.—6 months.

To W. Parr, Union-place, City-road, Middlesex, gentleman, for his method of producing a reciprocating action to be applied to the working of pumps, mangles, and all other machinery to which reciprocating action is required or may be applied.—18th of January.—4 months.

To E. Dakeyne, and J. Dakeyne, Darley Dale, Derby, merchants, for their machine or hydraulic engine for applying the power or pressure of water, steam, and other elastic fluids to the purpose of working machinery and other uses requiring power, and applicable to that of raising or forcing of fluids.—21st of January.—6 months.

To

To J. Yates, Hyde, Chester, calico-printer, for his method or process of giving a metallic surface to cotton, silk, linen, and other fabrics.—26th of January.—6 months.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·62. Mar. 26. Wind N.W.—Min. 29·50. Mar. 15. Wind S.W.
Range of the mercury 1·12.
Mean barometrical pressure for the month .... 30·123
Spaces described by the rising and falling of the mercury............ 6·400
Greatest variation in 24 hours 0·640.—Number of changes 15.
Therm. Max. 62° Mar. 26. Wind W.—Min. 34°. Several times.
Range 28°.—Mean temp. of exter. air 46°·01. For 30 days with ☉ in ♓ 44·50
Max. var. in 24 hours 23°·00.—Mean temp. of spring-water at 8 A.M. 47·23

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere in the evening of the 22nd ... 96°
Greatest dryness of the atmosphere in the afternoon of the 26th... 46
Range of the index 50
Mean at 2 P.M. 77°·7.—Mean at 8 A.M. 66°·8.—Mean at 8 P.M. 81·3
—— of three observations each day at 8, 2, and 8 o'clock .... 75·3
Evaporation for the month 2·10 inches.
Rain in the pluviameter near the ground 0·62 inch.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 7½; fine, with various modifications of clouds, 12; an overcast sky without rain, 9; rain, 2½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
21	9	24	4	7	12	9

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1	1	3	6½	2½	8	6½	2½	31

General Observations.—This has been a fine and very dry month; indeed so small a quantity of rain has not fallen here in any March for the last ten years. The March of 1820 was also fine and dry, but the mean temperature of that period was three degrees lower than that of the present month. A finer time for agricultural purposes could not be desired, as no interruption occurred to field labour; and so far as the leafing of the trees and the bloom have advanced, every thing bears a promising aspect.

If there be any truth in the moon's influence over the weather when her phases happen at certain hours, we may anticipate fine growing weather till the end of July. In August it may probably be changeable, in which case some good may be derived by agriculturists who are acquainted with the principles of the barometer, and will regulate their harvest-work by its indications, in connection with the state of the hygrometer, and the prevailing modifications of clouds.

Lunar halos appeared in the evenings of the 4th and 6th, and were exactly 45 degrees in diameter to their interior edges.

On

On the 16th, 17th and 18th, the equinoctial gales blew fresh from the Westward. In the evening of the 19th, at sunset, the clouds *cirri*, *cirrocumuli*, and *cirrostrati* passed through a gradation of prismatic colours, particularly the last modification.

The mean temperature of the air this month is one degree above the average of March for many years past: and a high atmospheric pressure has prevailed during the last fortnight.

Spring water was brought to its *minimum* temperature for this year so early as the 20th of February, and remained stationary till the middle of March, when it began to increase.

The atmospheric and meteoric phænomena that have come within our observations this month, are, three solar and three lunar halos; six meteors; and four gales of wind, namely, three from the South-west, and one from the West.

A very low ebbing of the Tide at Portsmouth, March 27th, 1830.—On the information of an active Portsmouth pilot, and others interested in the unusually low ebbing of the tide this morning, it was found at least six feet lower at Hamilton's Bank on the western side, and near the mouth of Portsmouth harbour, than they ever saw it before at the same age of the moon. Thirty years ago it is said to have been about as low.

The Bank on this occasion was completely dry for 160 feet, and on which a few hours before vessels of large tonnage passed over, persons were seen walking. His Majesty's ship Victory grounded in Portsmouth harbour; and the wreck of the old Boyne, which blew up about thirty-five years ago near Southsea Castle, made its appearance above water. This singular phænomenon in the reflux or ebbing of the tide, scarcely three days after the new moon, may probably turn out a prognostic of some distant convulsion of nature.

REMARKS.

London. — March 1. Cloudy: fine. 2—5. Very fine. 6. Slight fog: very fine. 7, 8. Very fine. 9. Drizzly. 10. Cloudy morning: very fine. 11. Very fine. 12. Very fine: stormy at night. 13. Clear, with stormy wind. 14. Very fine. 15. Stormy, with showers. 16. Stormy, with slight hail-showers. 17, 18. Fine. 19. Cloudy, with stormy wind. 20. Fine. 21. Very fine. 22. Cloudy, with stormy wind. 23. Fine. 24—30. Very fine. 31. Fine morning: cloudy, with drizzly rain at night.

Penzance.—March 1, 2. Misty. 3. Clear. 4. Clear: rain. 5. Rain: fair. 6. Misty: rain. 7. Fair: clear. 8, 9. Rain. 10. Fair: rain. 11, 12. Fair. 13. Clear. 14. Clear: fair. 15. Fair: showers, hail and rain. 16. Showers. 17—19. Fair. 20. Clear. 21. Fair. 22. Misty: rain. 23—26. Fair. 27—30. Clear. 31. Fair.

Boston. — March 1. Misty. 2, 3. Cloudy. 4—7. Fine. 8, 9. Cloudy. 10. Rain. 11, 12. Cloudy. 13. Stormy. 14. Fine. 15. Cloudy and stormy. 16. Stormy. 17. Cloudy. 18. Stormy. 19—23. Fine. 24, 25. Cloudy. 26—30. Fine. 31. Rain.

N.B. This month has been unusually warm and dry.

Meteoro-

Meteorological Observations made by Mr. Booth *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* Giddy *at Penzance, Dr.* Burney *at Gosport, and Mr.* Veall *at Boston.*

Days of Month, 1830.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Gosport. Max.	Barometer. Gosport. Min.	Barometer. Boston 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Gosport. Max.	Thermometer. Gosport. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Penz.	Wind. Gosp.	Wind. Bost.	Evap. Gosp.	Rain. Lond.	Rain. Penz.	Rain. Gosp.	Rain. Bost.
☽ Mar. 1	30·407	30·380	30·22	30·20	30·40	30·36	29·78	54	47	53	47	53	44	45	NE.	SW.	SW.	NW.	...	...	...	0·010	...
2	30·447	30·435	30·25	30·25	30·43	30·41	29·87	52	45	51	45	52	42	49	NE.	SW.	S.	NW.	...	...	...	...	...
3	30·445	30·342	30·28	30·25	30·40	30·33	29·90	58	32	50	45	50	35	46	E.	SE.	SE.	calm	0·10	...	...	...	...
4	30·245	30·040	29·95	29·85	30·18	30·00	29·83	50	32	48	44	44	34	38·5	NE.	SE.	E.	calm	...	...	...	...	...
5	30·046	29·973	29·90	29·80	29·97	29·95	29·60	54	31	47	43	44	36	38	E.	SE.	SE.	calm	...	...	0·105	...	...
6	30·050	30·018	29·90	29·90	30·02	29·98	29·74	54	32	48	43	51	34	36	E.	SE.	E.	calm	...	...	·130	...	...
7	30·082	30·038	29·90	29·88	30·00	29·98	29·77	45	32	49	42	45	34	37·5	SE.	SE.	NE.	calm	·15	...	·020	...	...
8	30·068	29·910	29·85	29·75	30·00	29·91	29·71	52	42	48	43	47	41	36	S.	S.	SE.	calm	...	...	·360	·185	...
○ 9	29·708	29·616	29·50	29·35	29·63	29·52	29·30	48	45	50	44	48	44	42	S.	SE.	SE.	S.	...	0·09	·530	·220	...
10	29·708	29·679	29·76	29·75	29·74	29·67	29·26	55	50	50	40	51	44	45·5	NW.	NW.	N.	calm	·10	...	...	·020	...
11	30·037	29·712	30·00	29·80	30·06	29·78	29·16	57	49	52	43	58	42	50	NW.	NW.	NW.	SW.	...	...	·090	...	0·02
12	30·139	29·938	30·15	30·12	30·17	30·01	29·56	59	40	53	47	53	38	51·5	W.	NW.	SW.	calm	...	...	...	...	...
13	30·331	30·081	30·32	30·30	30·37	30·26	29·63	54	37	50	42	52	37	43	SW.	NW.	W.	W.	·20	...	...	...	...
14	30·076	29·726	30·10	29·70	30·20	29·92	29·63	53	40	51	39	50	39	46	SW.	SW.	SW.	W.	...	...	...	...	...
15	29·649	29·498	29·50	29·50	29·57	29·50	29·02	50	36	48	44	54	36	43	S.	W.	SW.	W.	...	·07	·270	·010	...
16	29·874	29·743	29·80	29·70	29·90	29·73	29·10	52	38	50	39	48	41	41	SW.	NW.	SW.	W.	·20	...	·060	·030	...
☾ 17	30·076	29·910	30·05	29·95	30·11	29·92	29·26	57	47	54	45	53	43	48·5	W.	NW.	W.	W.	...	...	...	...	...
18	30·254	30·158	30·20	30·10	30·29	30·19	29·55	57	48	54	48	53	45	54	S.	W.	SW.	W.	...	...	...	...	...
19	30·264	30·187	30·20	30·20	30·29	30·21	29·69	55	48	55	48	56	44	51	SW.	W.	SW.	calm	·25	·02	...	...	...
20	30·259	30·149	30·20	30·18	30·30	30·18	29·57	55	39	54	42	54	40	45	SW.	NW.	W.	W.	...	...	...	...	...
21	30·385	30·315	30·30	30·28	30·40	30·37	29·83	60	47	54	45	53	41	48	SW.	NW.	SW.	calm	...	...	...	...	...
22	30·196	29·936	30·05	30·05	30·21	30·00	29·62	51	39	54	45	50	39	47·5	S.	SW.	S.	calm	·20	...	·070	·020	...
23	30·081	30·063	30·10	30·10	30·12	30·10	29·47	54	48	53	44	50	42	43	W.	W.	W.	calm	...	...	...	·025	...
● 24	30·282	30·187	30·15	30·08	30·30	30·18	29·56	57	38	54	45	57	38	52	W.	W.	W.	calm	...	...	...	...	...
25	30·466	30·388	30·20	30·10	30·47	30·38	29·97	65	40	55	46	55	39	48	SW.	NW.	W.	W.	·10	...	...	...	...
26	30·614	30·567	30·25	30·22	30·62	30·56	30·00	73	34	56	39	62	41	52·5	SW.	W.	NW.	calm	...	...	...	...	...
27	30·611	30·545	30·21	30·20	30·61	30·56	30·03	75	32	61	45	60	41	51	SW.	NE.	W.	N.	...	...	...	...	...
28	30·486	30·327	30·19	30·06	30·48	30·37	29·87	68	33	61	46	56	39	51	NE.	NE.	SE.	calm	·40	...	...	...	...
29	30·239	30·093	30·10	30·10	30·26	30·15	29·65	72	34	58	44	54	38	51	SE.	NW.	SE.	calm	...	...	...	...	...
30	30·063	29·942	30·00	29·95	30·08	30·00	29·46	66	35	58	43	58	39	50	E.	SE.	SE.	E.	...	...	...	...	...
☽ 31	29·824	29·730	29·80	29·75	29·86	29·70	29·23	62	33	58	43	56	36	49	E.	S.		NW.	·40	...	...	·100	·10
Aver.	30·614	29·498	30·32	29·35	30·62	29·50	29·60	75	31	61	39	62	34	46·1					2·10	0·18	1·635	0·620	·12

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JUNE 1830.

LIII. *Chemical Examination of a Crystalline Substance from the Zinc-foundry at Filisur; together with an Account of some ancient Glass Beads coloured by Oxide of Copper.* By *the Rev.* WM. V. VERNON, *F.R.S.**

THE substance of which I propose to give an account was obtained, by my friend Mr. Marshall, from a zinc-foundry belonging to M. Hitz, at Filisur in the Canton of the Grisons. It is said to occur oozing out of the crucibles while the metal is in a state of fusion; and yet the specimen resembles a natural mineral so much in its general appearance, that, without this information, it might have been supposed to have come from the cavity of a rock or the interstice of a mineral vein. The structure is crystalline, and a few of the crystals in the hollow parts of the specimen are extremely perfect; they are regular hexagonal prisms, terminated by similar pyramids†, transparent, amber-coloured, splendent, yielding with difficulty to the knife: the specific gravity of the more perfectly crystallized parts is 6·0, but that of inferior pieces is as low as 5·6.

I placed a grain of the crystals, in powder, on platinum foil, and heated it to redness. There was no loss of weight; the powder grew yellow when hot, and whitened again on cooling. When a crystal was exposed to the reducing flame of the blowpipe, it evaporated by degrees, and a white film spread

* Read before the Yorkshire Philosophical Society, April 6, 1830; and communicated by the Author.

† The pyramidal summit is replaced in some of the crystals by a plane at right angles to the axis of the prism. The angles which the lateral planes of the prism make with each other were measured by Mr. John Phillips with the reflective goniometer, and found to be 120°.

over the platinum; when ignited with carbonate of soda in the exterior flame of a candle, the globule remained uncoloured *.

Acids dissolved the powder entirely and without effervescence, though with a perceptible smell of sulphuretted hydrogen. The excess of acid being driven off, water redissolved the whole †. The solution was precipitated white by ammonia, an excess of which again took up the precipitate; it was also precipitated white by the ferro-prussiate of potash, and by the sulphuret of ammonia. A solution of a grain, from which the sulphuret had been thrown down, was evaporated and ignited, and when the ammoniacal salt was driven off, there was no residue.

From these experiments I was led to conclude that the substance is oxide of zinc in a state of purity, and uncombined with any of those substances with which as a mineral it has been hitherto found united, excepting only a minute and inappreciable quantity of sulphur; and I was confirmed in that conclusion by the following experiment with which I terminated my examination. I put a crystal into a solution of bisulphate of potash; it dissolved quietly and entirely; I left the solution to crystallize, and found all the crystals that were formed to be right rhombic prisms, which I recognised as the compound sulphate of potash and zinc.

It is worth while to observe how nearly this substance and the natural ore of zinc, in which the oxide, though not absolutely pure ‡, has been found in the simplest state, resemble one another in specific gravity and in crystalline form. The specific gravity of the red oxide of zinc found at New Jersey in America, is stated to be 6·2; and the late Mr. William Phillips has deduced from the cleavage of this mineral, (as it is met with in an incompletely crystallized state,) the form of a regular hexagonal prism. So that the mineral-laboratory of nature seems to have been employed in forming the same crystal which the crucible has here produced in a more perfect form; and thus the substance before us adds another instance to those already known, of the mineralogical presumption, that the agency of fire may have been concerned in the formation of mineral veins, or in the structure of at least some of the rocks.

I will take this opportunity of communicating also to the Society the result of some experiments which I have made on the ancient glass beads, lately presented by the Rev. Mr. Stillingfleet.

* It contained therefore no manganese.

† It contained therefore no silica, nor any notable quantity of sulphur.

‡ It is coloured by a small quantity of manganese.

lingfleet.—I have found the colouring matter of these beads to be copper. At the present day cobalt, I believe, is exclusively employed to produce every shade of *blue* glass, copper being only used in staining it red or green; and I imagine that a modern artist would be much at a loss to obtain the fine azure of these beads from copper. I find that a blue glass may be obtained from copper by employing a large proportion of carbonate of soda in the composition, and I suppose that the blue colour in this case is given by the *carbonate* of copper, as the red is by the *protoxide*. I did not succeed, however, in obtaining a good permanent glass, and have reason to think that considerable nicety is required in the proportions, to bring the process to perfection. Now we know that the Romans possessed the art of making a blue glass coloured by copper. Among the most curious remains found at Pompeii were the contents of a colour-shop, which were analysed by Sir Humphry Davy; and in the blue frit which he examined, that lamented philosopher found the colouring matter to be copper. I have lately had occasion to show in another instance, that antiquarian pursuits, however far they may appear to be disjoined from experimental philosophy, may sometimes derive aid from an alliance with it. I pointed out, in a piece of black pottery, angular and unaltered fragments of calcareous spar forming a part of its constituent ingredients, and inferred from hence that this pottery could not have been baked in the usual degree; and I stated that this was that black pottery of the Romans which we meet with at many of their stations, and which a German chemist has shown to have been probably made of clay kneaded with bitumen or pitch, and afterwards charred in a moderate heat. Now it is obvious that wherever an antiquary should discover the fragments of a pottery manufactured in so peculiar a manner, he would not require to know the form of the vessel, or to decypher the name of the artist, but could pronounce at once that the arts of Rome had penetrated to that place. An inference of the same kind I am disposed to draw from the colouring of these glass beads. The date of the tumuli in which the beads were found is in question. The peculiar manner in which the glass is coloured, determines me to believe that the tumuli, together with the implements which they contain of a more equivocal character, and with whatever peculiarities in the mode of sepulture we may discover in them, are of a date posterior to the introduction of the Roman arts into Britain.

Since this paper was read, I have found among the ashes contained in a Roman cinerary urn, of black pottery, a fragment of a blue glass bead precisely similar to those which were discovered in the tumuli by Mr. Stillingfleet. I have

 also

also seen a Roman lachrymatory and an image of one of the divinities of Egypt, which seem to be of the same manufacture. It is probable that this blue glass was originally an Egyptian invention, for Pliny ascribes the discovery of a mode of fabricating a factitious lapis lazuli to a king of Egypt*.

LIV. *On the Detection of Baryta or Strontia when in union with Lime.* By Mr. J. Andrews†.

THE best method which has yet been proposed to distinguish strontia when combined with lime, is perhaps that by which Stromeyer first succeeded in determining the composition of arragonite; and I believe, it alone has been employed in the analyses that have been made by different chemists of the varieties of that mineral. It is founded upon the insolubility of nitrate of strontia in pure alcohol, which at the same time dissolves with facility the nitrate of lime. In theory the process is absolutely perfect; and when properly conducted, it seems susceptible in practice of very great precision; but it must at the same time be confessed that its failure in the hands of some of the most expert chemists proves it to be a far from satisfactory mode of analysis. As nitrate of baryta is insoluble in alcohol, baryta may likewise in this manner be separated from lime.

Bucholz having endeavoured unsuccessfully to verify the analysis of Stromeyer, suggested another process, which, though he failed himself in executing, was afterwards performed by Gehlen. It consisted in separating the strontia from the greater part of the lime (the mixture being previously reduced to the pure state) by solution in boiling water, and afterwards distinguishing the presence of the former by the crystallization of its hydrate on allowing the solution to cool. It is evident that although in this way considerable quantities of strontia may be detected, yet when it exists in small proportion, the process will entirely fail, in consequence of the solubility of strontia in cold water. If baryta be substituted for strontia, this method will be still more defective, since that earth is much more soluble in water than strontia.

Mr. Brande has proposed to precipitate directly the sulphate

* In the 23rd volume of the Annals of Philosophy, p. 116, are some remarks by Mr. Smithson, on the colours found on the tomb of the Egyptian King Psammis: "The blue is what most deserves attention; it is a smalt or glass powder, but its tingeing matter is not cobalt, but copper. Many years ago I examined the blue glass with which was painted a small figure of Isis brought to me from Egypt, and found its colouring matter to be copper. I am informed that a fine blue glass cannot at present be obtained by means of copper:—what its advantages would be above that from cobalt it is for artists to decide."

† Communicated by the Author.

phate of strontia from a solution of the earths in nitric acid, and after washing it with repeated affusions of boiling water to determine its weight. This mode of analysis appears to me, with much deference to so high an authority, to be much more exceptionable than any that has yet been proposed. I have in vain endeavoured to throw down sulphate of strontia in this manner, although present to the amount of 5 per cent; for when the solution was made so dilute that no precipitate at first appeared on the addition of sulphate of soda, a voluminous deposit of crystallized sulphate of lime slowly formed, without any previous precipitation of sulphate of strontia. Nor will boiling water effect a separation of the sulphates of lime and strontia, as it is nearly impossible to wash away any considerable quantity of the former sulphate by means of that liquid, while the latter is at the same time sensibly soluble in it. It is possible, however, that there may be some circumstance on which the success of this experiment depends, of which I have not been aware; for Mr. Brande obtained by this means a precipitate which upon examination proved to be pure sulphate of strontia, and the weight of which corresponded exactly with the original quantity of carbonate of strontia that he employed. It will certainly be less liable to objection in the case of baryta; but it appears to me very doubtful whether it will effect a complete separation, or distinguish a small quantity, even of that earth.

The following method, which is merely an extension of that proposed by Bucholz, will be found not less sensible in its indications, while it is at the same time more easy of execution, and more certain in its results, than the process of Stromeyer.

Dissolve the carbonates of lime and baryta or strontia in nitric acid, evaporate the solution to dryness, and decompose the nitrates by heat; to the dry mass add boiling water (pure in the case of baryta, but saturated in the cold with sulphate of strontia in the case of that earth), and boil it for a few minutes, keeping the crucible at the same time loosely covered with its lid. Throw the whole on a covered filter; and to the liquid which passes through add sulphuric acid or a soluble sulphate: a white powder will precipitate if either baryta or strontia be present; but if not, the liquid will retain its transparency.

I decomposed in this manner 99·75 grains of nitrate of lime and ·25 grain of nitrate of baryta; a white precipitate immediately appeared on the addition of sulphuric acid to the filtered liquid. A similar experiment made with equal proportions of the nitrates of lime and strontia was attended with the same result; the precipitate, however, did not appear quite so soon as before. Hence it appears, that $\frac{1}{400}$dth part at least of baryta

ryta or strontia united to lime may be in this way rendered evident. It is almost unnecessary to add, that when pure lime was treated in a similar manner, no precipitate fell, since sulphate of lime is nearly four times as soluble in boiling water as lime itself. If however the liquid be raised to the boiling point after the addition of the sulphuric acid, one or two crystals of sulphate of lime will sometimes form; but, as these are transparent and of a distinctly crystalline appearance, it is impossible to mistake them for the sulphate either of baryta or strontia*. In these experiments, I have in general preferred the employment of sulphuric acid to that of a sulphate, as I have found it to be a rather more delicate test, particularly of the presence of strontia. It must of course be carefully purified from sulphate of lead.

It is generally believed, on the authority of Dr. Hope, that sulphate of strontia is sensibly soluble in water, so that the addition of a barytic salt or an alkaline carbonate to a solution of it, will indicate the presence of both its constituents; and this circumstance has been proposed as a distinguishing test between the sulphates of baryta and strontia. On the other hand, the late Mr. Smithson states that "water or solution of sulphate of soda, in which sulphate of strontia had long lain, did not produce the least cloud on the addition of what is commonly called subcarbonate of soda;" and Dr. Thomson was unable to detect any trace of strontia in the supernatant liquid which remained after the mutual decomposition of solutions of sulphate of soda and chloride of strontium. I digested for some hours sulphate of strontia, obtained by precipitation, in pure water, in a solution of sulphate of soda, and in water acidulated with sulphuric acid; carbonate of soda was added to each liquid, and heat applied; a precipitate appeared in all of them: it was most considerable in the first, while in the last the opalescence was scarcely perceptible; 900 grains of the solution in pure water were evaporated to dryness, and left a residue of sulphate of strontia which weighed ·25 grain; from which it follows that one part of sulphate of strontia requires about 3600 parts of water, at the temperature of 60°, for solution. It also appears that the presence of sulphate of soda and sulphuric acid (in small quantity) diminishes its solubility. I have not examined whether chloride of sodium would produce a similar effect; but the experiments of Dr. Thomson would lead to this conclusion.

* I added to three different portions of lime-water saturated at 60°, sulphate of soda, sulphate of ammonia, and sulphuric acid: no precipitate appeared at the end of some weeks in the first two liquids; but a large quantity of gypseous crystals was slowly deposited in the latter in the course of some days.

LV. *On*

LV. *On a simple Method of exhibiting the Combustion of the Diamond.* By Mr. W. HERAPATH.

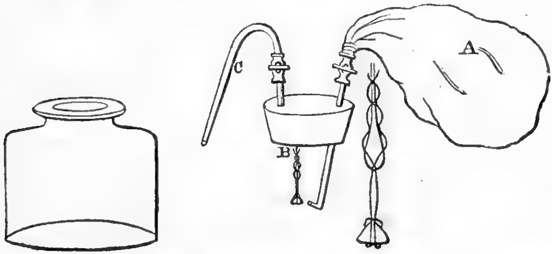
To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I AM not aware that any simple method of exhibiting the combustion of the diamond has been published; I therefore send you a description of an apparatus by which it may readily be effected in oxygen gas.

Provide a wide-mouthed gas receiver for the oxygen, with a ground upper surface so as to be closed by a glass plate; fit it with a cork, in which make two holes to pass tubes through; one to convey hydrogen gas from a bladder, the other to allow a passage to the carbonic acid.—Perhaps the hasty sketch which accompanies this will explain better what I mean.

A is the bladder of hydrogen gas capable of transmitting a stream upon the diamond by means of a bent jet at the end



of the glass tube. The diamond is supported by a fine platinum wire passed over it as a running noose, the ends being left an inch long to twist round the flattened end of a stout platinum wire, B, stuck in the cork. By holding the diamond thus it may readily be detached after the experiment, and the fine wire will not conduct away so much caloric as to prevent the combustion from continuing. C, a recurved tube with a stopcock attached, to permit the transmission of the carbonic acid gas into lime-water.

Bring the diamond up to a fine white heat by directing a stream of the ignited gas upon it (pressing the bladder under the arm); plunge it while at this heat into the oxygen gas, taking care to turn off the hydrogen when in the act of introducing it. Combustion will begin, and continue as long as a fragment remains. I think it the most splendid experiment in chemistry, as no flame, smoke or scintillation occurs, while the diamond glows like a little sun.

The

The most strange part of the experiment is, that although it is a simple solution of carbon in oxygen, the gas not being condensed, consequently the solid carbon must expand to become gaseous; yet caloric is liberated not only in sufficient quantity to maintain the combustion, but also to fuse the supporting platinum wire, many times the weight of the diamond. I generally use flat or rose diamonds, as they are more readily secured in the wire; one of the value of two or three shillings is quite large enough. In some instances in which I have interrupted the experiment to examine the gem, its surface was dull, retaining traces of the original facets; the edges were rounded, and elevated ribs were observed where the platinum wire had prevented the free access of the gas. When the experiment is allowed to terminate, and the oxygen is pure, all the diamond is consumed, except a particle as large as a pin's point which remains imbedded in the fused platinum.

I conceive this experiment goes some way to account for animal heat; for if solid carbon, by merely dissolving in oxygen to form carbonic acid, gives out so much caloric, there is no difficulty in supposing carbon, in the liquid form of blood, to give out caloric under the same circumstances.

Bristol, 56 Old Market-street, March 15, 1830. WM. HERAPATH.

LVI. *Letter from* Mr. E. GALLOWAY *respecting the new Paddle-Wheel, the Invention of which is claimed by* Mr. King Williams: *with Affidavits relating to that Subject.*

To Richard Taylor, Esq.

Sir,

HAVING a few days ago seen a Number of your valuable publication, in which an improvement in propelling steam-vessels is described as the invention of Mr. King Williams, I am induced to trouble you with the inclosed copies of affidavits, which will instantly show you the degree of merit belonging to the claimant. These affidavits form but a small part of the evidence I possess on the subject.

I am, Sir, most respectfully yours,

7 Holland-Street, Blackfriars, April 14, 1830. E. GALLOWAY.

Affidavits.

(COPY.)

Benjamin Chapman, Engineer, of Russell-Place, Bedford-Row, Borough of Southwark, maketh oath, and saith,

That he was present during part of a conversation which took place in November 1829, between Mr. E. Galloway and Mr. King Williams, when the latter showed a model of a paddle-wheel, which he stated to be his invention;

vention; that the model shown corresponded in all its proportions with certain paddle-wheels manufactured by Mr. W. J. Curtis about the middle of the year 1829, under the superintendence of Mr. E. Galloway; that in the conversation above alluded to, Williams at first denied either having seen the paddles made by Mr. Curtis, or any plan resembling the model shown (this he asserted with repeated solemn protestations, calling on God to witness their truth); and that he shortly afterwards admitted having seen the paddles made by Mr. Curtis, and having copied therefrom the essential parts of the machine—to wit, the radius collar, fixed crank, and the lengths of the radius rods:

That deponent remembers having seen Williams at the shop of Mr. Curtis when the paddles were fixed and ready for exportation, and before Williams had commenced to make the model which he now asserts to be his invention. Benj. Chapman.

Witness to the signing, Wm. Sappo.
Sworn before me this 23rd day of January 1830,
P. Laurie, Alderman.

(Copy.)

Alexander Park, Mechanical Draughtsman, of Nelson-Street, Borough of Southwark, maketh oath, and saith,

That on the eleventh of January 1830 he was in conversation with Mr. King Williams, machinist, who professes to be the inventor of a certain modification of paddle-wheels similar to the system recently patented by Mr. E. Galloway; that the said Williams then admitted to deponent, that he had copied certain parts of Mr. Galloway's machine from a set of paddles manufactured by Mr. W. J. Curtis about the middle of the year 1827; that he had gained admittance to Mr. Curtis's premises by the connivance and assistance of a smith named Samuel Laing or Lang; and that he, Williams, had then and there measured the above-named paddle-wheels, for the purpose of making therefrom the model now shown as the invention of the said Williams. That Williams also admitted to deponent, that those parts of the paddle-wheel which deponent knows to constitute Mr. Galloway's patent had been accurately copied in the model; and that their dimensions were obtained by inspection and measurement of the paddles made by Mr. Curtis.

Deponent further saith, that Williams solicited deponent to give plans of the subsequent improvements of Mr. Galloway, for the purpose, as Williams stated, of transmitting them, as his own invention, to the Editor of the Register of Arts and Sciences. Alexander Park.

Witness to the signing, Wm. Sappo.
Sworn before me this 23rd day of January 1830,
P. Laurie, Alderman.

LVII. *Determination of the Dimensions of the Earth, from the principal Measurements of Arcs of Meridians.* By Dr. Edward Schmidt*.

On the supposition of the earth being an elliptical spheroid, the figure of the generating ellipsis may be found from two arcs of meridians measured in different latitudes; but the applica-

* From Prof. Schumacher's *Astronomische Nachrichten.*

application of the formulæ derived from this assumption, to the different measurements which have been executed, has proved that every combination of any two of them gives a different result for those dimensions. It became, therefore, necessary to apply to the whole mass of the best measurements, a principle accordant with the nature of the subject, by which the most probable figure of the earth's surface, resulting from all of them, might be elicited. This has partly been done in an unfinished paper by Walbeck, entitled "*Dissertatio de forma et magnitudine telluris, ex dimensis arcubus meridiani definiendis.*" Walbeck has founded his researches on the principle that the sum of the squares of the differences between the measured and calculated arcs should be a minimum. But he neglected in these calculations the second power of the ellipticity, and likewise the partial arcs determined between the extreme points of a measurement; and his results were therefore susceptible of some improvements by a more accurate investigation.

Having been induced by Professor Gauss to take up this subject, I have taken into account the second power of the ellipticity, and have so far changed the above-mentioned principle, as to make the sum of the squares of the differences of the calculated and observed latitudes a minimum, instead of that of the squares of the differences of the measured and computed arcs of the meridian. The results of my calculation have been published by Prof. Gauss in his book entitled "Determination of the Difference of Latitude of the Observatories of Göttingen and Altona." But on a revision, a small error in calculation was discovered; and I became acquainted with a small correction which is to be applied to the English and East Indian measurements (see Phil. Trans., where Capt. Kater has proved that the lengths of the arcs in the East Indies are to be multiplied by $1-0\cdot000018$, and those in England by $1+0\cdot000007$). A new calculation therefore became necessary, which I here communicate.

Let f denote the length of the mean degree (the three-hundred-and-sixtieth part of the terrestrial meridian), α its ellipticity; so that if a represent the semiaxis major, and b the semiaxis minor, we have $b = (1-\alpha)\,a$. Let it be assumed, that

$$f = \frac{57009\cdot76}{1+\frac{u}{1000}} \text{ toises, and } \alpha = \frac{1+y}{302\cdot78}\text{; then the following}$$

measurements give these final equations for determining u and y:

$$72\cdot13 = 2100\cdot90\,u + 1763\cdot16\,y$$
$$200\cdot77 = 1763\cdot16\,u + 9348\cdot66\,y$$

Hence

Hence we obtain

$u = +\ 0{\cdot}01937669$ Weight $= 1768$

$y = +\ 0{\cdot}01782113$ ——— $= 7869$

and the errors of the single latitudes are then as follows:

		°	′	″	″
Peruvian Measurement.					
Tarqui....................	−	3	4	30·83	+1·79
Cotchesqui	+	0	2	37·88	−1·79
First East Indian Measurement.					
Trwandeporum	+	11	44	52·59	−0·59
Paudree		13	19	49·02	+0·55
Second East Indian Measurement.					
Punnac		8	9	38·39	−1·73
Putchapollian		10	59	48·93	−1·21
Dodagoontah		12	59	59·91	+3·50
Nantkabad		15	6	0·64	−0·57
French Measurement.					
Formentera		38	39	56·11	+3·39
Montjouy		41	21	45·45	+2·56
Barcelona		41	22	47·16	+0·83
Perpignan		42	41	58·01	−4·15
Carcassone		43	12	54·31	−1·01
Evaux......................		46	10	42·19	−5·88
Pantheon		48	50	48·94	+0·36
Dunkerque		51	2	8·74	+3·88
English Measurement.					
Dunnose	+	50	37	7·81	−1·88
Greenwich		51	28	39·60	+0·95
Blenheim		51	50	27·50	+3·01
Arbury Hill		52	13	27·79	+1·83
Clifton		53	27	31·59	−3·89
Hanoverian Measurement.					
Göttingen		51	31	47·85	−2·74
Altona		53	32	45·27	+2·74
Swedish Measurement.					
Mallörn		65	31	31·06	+1·33
Pahtawara		67	8	51·41	−1·33

The sum of the squares of these errors is $157''{\cdot}78$; and hence results the mean error of a determination $= 3''{\cdot}140$. The mean errors are: of $u = 0{\cdot}074684$

of $y = 0{\cdot}035401$

Substituting

Substituting the above values of u and y in the expressions for α and f, we find:
The three-hundred-and-sixtieth part of the terrestrial meridian = 0·57008·655 toises; the ellipticity = $\frac{1}{297\cdot479}$.
The mean error to which the value of f is subject, is = 4·26 toises; and the mean error to which the value of α is subject, is = 10·5 unities of the denominator. We have next,

$$a = 3271852\cdot32 \text{ toises}; \quad b = 3260853\cdot70 \text{ toises}.$$

With regard to the formulæ applied in these calculations, I refer to the first volume of my "Mathematical and Physical Geography," the printing of which is nearly finished.

EDWARD SCHMIDT.

LVIII. *On the Figure of the Earth.* *By* JAMES IVORY, *Esq. M.A. F.R.S. &c.**

THE 8th volume of the *Mémoires de l'Academie des Sciences*, published in 1829, contains a paper on the figure of the earth by M. Biot, who has had so great a share in the experimental researches relating to that subject. Accompanied with his son and assisted by him, M. Biot determined in 1824 the lengths of the pendulum at Milan, Padua, and Fiume, upon the parallel of 45°; and as the same parallel passes through the old stations of Bourdeaux, Figeac, and Clermont, we are thus made acquainted with the lengths of the pendulum at the six principal stations of the great arc perpendicular to the meridian, extending from Bourdeaux to Fiume, which has lately been measured geodetically with so much ability and exactness. We are indebted to the same observers for the pendulums at the two new stations of Lipari and Barcelona; and for a remeasurement of the pendulum at Formentera, the old length in the 4th volume of the *Base Métrique* being for some reasons deemed exceptionable. According to the new measurement, the pendulum at Formentera, in a vacuum and reduced to the level of the sea, is $993^{mm}\cdot0697$; the length as stated in the *Base Métrique* is equivalent to $992^{mm}\cdot9761$; the difference $0^{mm}\cdot0936$ is very considerable, and answers to about four vibrations in a day.

The most curious part of M. Biot's paper is an attempt to prove that the lengths of the pendulum in different latitudes are not accurately represented by the formula usually employed: for he finds that the coefficient of the term proportional to the square of the sine of the latitude, is not an invari-

* Communicated by the Author.

able

able quantity, as usually assumed, but a quantity decreasing gradually from the pole to the equator. As this inference which M. Biot has drawn from a comparison of the experiments is opposed to the usual theory, and would lead us to entertain new notions concerning the figure of the earth, it seems important to examine the grounds of it with attention.

In what follows I shall use the lengths of the pendulum in M. Biot's paper without reducing them to English measures. This will save calculation, and besides it has this advantage, that all the pendulums are reduced to the level of the sea by one uniform rule. If A denote the length of the equatorial pendulum, and l the length at any latitude λ, we shall have, by the usual formula,

$$l = A \times (1 + f \sin^2 \lambda);$$

from which we get,

$$f = \frac{l - \Lambda}{\Lambda \sin^2 \lambda}.$$

Now, having assumed some known value for A, if we begin at the greatest north latitude, and substitute for l the successive observed pendulums, we shall obtain a series of values of f, which will show whether that coefficient is constant or variable in the supposition of the assumed equatorial pendulum. The formula must not be applied to latitudes less than 45°: for in such cases, the difference of the pendulums in the numerator is sensibly affected by the errors of observation; and, the effect of these being increased by the small divisor $\sin^2\lambda$, the results would be too irregular to lead to any certain conclusion. But we shall be able to form an opinion respecting the new system advanced by M. Biot without going to latitudes less than 45°; because the whole decrease of the coefficient f from the pole to the equator attains half its magnitude at the parallel of 45°.

The last four columns of the following Table exhibit the values of f for all the observed pendulums between the parallel of 45° and the pole, on the supposition of four different equatorial pendulums, denoted by L, L′, L″, L‴. Of these $L = 990^{mm}\cdot 8876$ is the observed pendulum at Maranham, reduced to the equator by a small deduction; $L' = 990^{mm}\cdot 9466$ is the observed pendulum at Rawak on the equator; $L'' = 991^{mm}\cdot 0270$ is the mean equatorial pendulum adopted by M. Biot; and $L''' = 991^{mm}\cdot 1106$ is the observed pendulum at St. Thomas, on the equator.

TABLE.

TABLE.

Stations.	Latitude.	Pendulums.	Values of *f*. L	L′	L″	L‴
	° ′ ″	mm.				
Spitzbergen	79 49 58	996·0359	·00536	·00530	·00522	·00513
Greenland	74 32 19	995·7465	·00528	·00521	·00513	·00504
Hammerfest	70 40 5	995·5312	·00526	·00520	·00510	·00501
Drontheim	63 25 54	995·0132	·00520	·00513	·00503	·00492
Unst	60 45 25	994·9421	·00537	·00530	·00519	·00508
Stockholm	59 20 43	994·7880	·00532	·00524	·00513	·00501
Portsoy....	57 40 59	994·6906	·00537	·00529	·00518	·00506
Leith	55 58 38	994·5331	·00536	·00527	·00515	·00503
Clifton	53 27 43	994·3016	·00534	·00524	·00512	·00499
London	51 31 8	994·1234	·00533	·00523	·00510	·00496
Arbury Hill	52 12 55	994·2275	·00540	·00530	·00517	·00503
Dunkirk ...	51 2 10	994·0804	·00533	·00523	·00510	·00496
Shanklin...	50 27 24	994·0470	·00534	·00524	·00510	·00496
Paris	48 50 14	993·8607	·00529	·00519	·00505	·00490
Bourdeaux	44 50 26	993·4529	·00521	·00509	·00492	·00475
Figeac......	44 36 45	993·4578	·00526	·00514	·00497	·00480
Clermont..	45 46 48	993·5823	·00529	·00518	·00502	·00486
Milan	45 48 1	993·5476	·00528	·00517	·00501	·00484
Padua	45 24 3	993·6073	·00541	·00530	·00514	·00497
Fiume......	45 19 0	993·5841	·00538	·00527	·00510	·00494

The 4th column of the table contains the values of *f* for the equatorial pendulum observed at Maranham. The mean of the first three numbers is ·00530, the value of *f* at the latitude of 75°; the mean of the last six numbers is ·00531, the value of *f* at the latitude of 45°; and the mean of all the numbers in the column is ·00532, the value of *f* at the latitude of about 60°. If we compare the mean number ·00532 with the values of *f* at all the stations where the pendulum has been so determined as to inspire most confidence in the accuracy of the operations, the discrepancies will be found very small: as at Unst and Leith, at which stations the results of Captain Kater and M. Biot agree, although obtained separately, and by different methods; and at Stockholm, London, Dunkirk, and Paris, at all which places every precaution was used to attain the utmost precision in the experiments. It cannot therefore be said that when L is assumed for the pendulum at the equator, the value of *f* either increases or decreases from the pole to the parallel of 45°. No other inference can be drawn than that *f* is constant, the discrepancies in the table being unavoidable in a series of results deduced directly from such experiments.

And because the value of *f* is found to be constant when the

the equatorial pendulum is L, it is a necessary consequence that *f* will be variable, and will decrease from the pole to the equator when we assume an equatorial pendulum greater than L. On the other hand, when we assume an equatorial pendulum less than L, the value of *f* will be variable and will increase from the pole to the equator. All this readily follows from the formula for *f*.

In the 5th column, answering to the equatorial pendulum observed at Rawak, the mean of the first three numbers is ·00524, the value of *f* at the latitude of 75°; the mean of all the numbers in the column is ·00522, the value of *f* at the latitude of 60°; and the mean of the last six numbers is ·00519, the value of *f* at the latitude of 45°. In this case it can hardly be said that *f* either increases or decreases, the variation of the equatorial pendulum being too small to produce a sensible effect.

By taking the like mean numbers as before in the 6th column, which is computed from the equatorial pendulum adopted by M. Biot, it will be found that the values of *f* at the latitudes of 75°, 60°, and 45°, are respectively ·00515, ·00510, and ·00503. Here there is a regular decrease amounting to ·00012 between the latitudes of 75° and 45°. This quantity is however very small, and within the limits of the discrepancies observable in the table; and it might be reduced to zero, or greatly altered in magnitude, by small variations in the pendulums at the extreme latitudes. Besides, even if its magnitude were well ascertained, we could safely draw no conclusion from it respecting the physical constitution of the globe, because it might be occasioned merely by the supposed length of the equatorial pendulum.

In the last column the values of *f* at the latitudes of 75°, 60°, and 45°, are ·00506, ·00497, and ·00486, the decrease between the extreme latitudes being ·00020. The greater decrease of *f* is caused solely by assuming an equatorial pendulum of a greater length.

It is therefore very possible that the inference M. Biot has drawn respecting the variability of *f* from the pole to the equator, is no more than a consequence of small unavoidable errors or anomalies in the experiments, and of the equatorial pendulum he has assumed.

It seems clearly to follow from what has been said, that the experiments with the pendulum, numerous as they are, leave the question of the figure of the earth, and the quantity of its oblateness, in some degree indeterminate; and that this will continue to be the case until the precise length of the equatorial pendulum is decisively ascertained. Various equatorial

torial pendulums represent all the experiments nearly equally well; but every individual length gives to the earth a different degree of oblateness.

But if we review the experiments that have already been made on or near the equator, what notion can we have of an equatorial pendulum, which seems to vary irregularly at every different station? The difference in the pendulums observed at Maranham and St. Thomas answers to about nine vibrations a-day; and if we compare the pendulum at Maranham with the experiments at the Isles of France and Guam, the difference amounts to twelve vibrations a-day. To ascertain the exact quantity of such anomalies, and to detect their cause, is assuredly at present the most important and interesting part of this research. Until this be done, the theoretical question can never be placed on a sure foundation.

May 10, 1830. JAMES IVORY.

LIX. *On the Census.* *By* J. W. LUBBOCK, *Esq. F.R.S. &c.**

THE importance of questions connected with the theory of population renders it much to be regretted that we possess so few data for their solution. The bills of mortality are now published only in London, Northampton, and a few great towns, and the parish registers from which these are taken are not, I fear, kept with sufficient accuracy. The Act of the 52nd Geo. III. for the better regulating and preserving parish and other registers of births, baptisms, marriages and burials in England, provides many excellent regulations, but attaches no penalties to the non-performance of them.

If we refer to the censuses of the population, we find anomalies which it is by no means easy to account for. It seems doubtful whether censuses made at such distant intervals of time as once in ten years can be accurate, as the experience furnished by one must in many cases be lost before a second is commenced. I hope that something may be done to remedy these defects, and to give to these researches an accuracy equal to that which has been attained in almost every other branch of science.

Two methods present themselves of ascertaining the ratio of the births and the deaths to the population. The first, which is the more obvious, is by comparing the actual numbers of the living, found by any enumeration or census, with that of the births and deaths as deduced from registers of baptisms and burials.

But these ratios can also be found when the law of mortality

* Communicated by the Author.

which

which obtains in the given country, and the rate of increase, are known; and we have this equation of condition, which enables us to check the accuracy of the results.

$$\frac{\text{Births}}{\text{Population}} - \frac{\text{Deaths}}{\text{Population}} = \text{rate of increase} - 1.$$

According to the Population Report 1821, the baptisms in England in 1820 were 328,230, and their mean annual rate of increase 1·0149; this would give 332,820 for the baptisms in 1821, and adding 20,696 for the unentered baptisms, according to Mr. Rickman, we have 353,516 for the births in 1821. The burials in 1820, according to the same Report, were 198,634; supposing their mean annual rate of increase to have been also 1·0140, we have 201,410 for the burials in 1821, and adding 8770, according to Mr. Rickman, for the unentered burials, we have 210,180 for the deaths in 1821. The population of England according to the census of 1821 was 11,261,437.

$$\frac{353{,}516}{11{,}261{,}437} = \frac{1}{31{\cdot}854} = {\cdot}031392.$$

$$\frac{210{,}180}{11{,}261{,}437} = \frac{1}{53{\cdot}576} = {\cdot}018664$$

$${\cdot}031392 - {\cdot}018664 = {\cdot}012728.$$

But the population, according to the census, of 1811 was 9,538,827, which gives the annual rate of increase = 1·0167 instead of 1·0127, as given above. This error is not considerable; it must be recollected that the equation of line 4 is only rigorously true for the actual rate of increase in 1821.

The small proportion of births and deaths to the population is very remarkable. I have found by calculation that in a population, of which the annual rate of increase is 1·015, according to a table of mortality which I deduced from Dr. Haygarth's observations at Chester, and which is published in the last volume of the Transactions of the Cambridge Philosophical Society *, the ratio of the births to the population is $\frac{1}{24{\cdot}660}$, and the ratio of the deaths to the population $\frac{1}{38{\cdot}544}$.

I have found the same ratios, according to Mr. Milne's Carlisle Table, to be $\frac{1}{25{\cdot}654}$ and $\frac{1}{40{\cdot}085}$. In fact, these tables of mortality agree very closely; and no table of mortality would give results approaching to $\frac{1}{31}$ and $\frac{1}{53}$.

* On the Comparison of various Tables of Annuities: Table VII.

In these calculations I supposed the rate of increase to be uniform; but I repeated similar calculations with the rate of increase of the baptisms (which may be considered the same as that of the births) as given in each year, by the Report before alluded to, and I found from the table of mortality, deduced from the Chester observations, the ratio of the deaths to the population $= \frac{1}{36{\cdot}897}$ or ·0271: if we suppose the rate of increase to be 1·0167, this gives ·0438 for the ratio of the births to the population, or $\frac{1}{22{\cdot}83}$ nearly; and I constructed the following Table, which offers a comparison of the proportions which the number of persons at different ages, as given by the census and by theory, bears to the population.

Age.	Males.			Females.			Age.
	Census 1821.	Theory.	Errors of Theory.	Census 1821.	Theory.	Errors of Theory.	
0 to 5	·01538	·01572	+·00034	·01444	·01458	+·00014	0 to 5
5 to 10	·01343	·01200	−·00143	·01268	·01146	−·00122	5 to 10
10 to 15	·01169	·01077	−·00108	·01056	·01044	−·00012	10 to 15
15 to 20	·00988	·00978	−·00010	·00995	·00944	−·00051	15 to 20
20 to 30	·01470	·01509	+·00039	·01684	·01491	−·00193	20 to 30
30 to 40	·01155	·01227	+·00072	·01210	·01215	+·00005	30 to 40
40 to 50	·00941	·00959	+·00018	·00933	·00982	+·00049	40 to 50
50 to 60	·00665	·00670	+·00005	·00653	·00761	+·00108	50 to 60
60 to 70	·00447	·00446	−·00001	·00458	·00540	+·00090	60 to 70
70 to 80	·00222	·00231	+·00009	·00228	·00309	+·00081	70 to 80
80 to 90	·00056	·00080	+·00024	·00065	·00100	+·00035	80 to 90
90 to 100	·00004	·00016	+·00012	·00006	·00023	+·00017	90 to 100
Total	1·00000	·00000		1·00000	1·00000		

The result of theory agrees pretty well with observation. It may be observed that the age from 20 to 30 appears decidedly the favourite with females; for while the error of theory between 30 and 40 is only +·00005, the error between 20 and 30 is −·00193; the error for males at the same epoch is +·00039.

Considering the limits of the errors of which these calculations are susceptible, I do not think the ratio of the deaths to the population can differ much from $\frac{1}{37}$ or ·027; if, therefore, we take the census to be correct, and that 11,261,437 was the number of the population in 1821, we have for the deaths in 1821, 304,350, instead of 201,410, making the unentered burials amount to 102,940, or nearly a third of the total number.

Some writers, and particularly Dr. Hawkins in his valuable work on medical statistics, have inferred from the small proportion of the deaths to the population given by the parliamentary report, and from its being so much less than what obtains

tains in other countries, the pre-eminent salubrity of our country. However consolatory this opinion may be, I fear it will not stand the test of examination. The disagreement of the results given by the census with those given by the best tables of mortality is so great, that either the one or the other must be grossly inaccurate: this assertion, however, rests upon my own calculations, which I shall be very glad to see verified or disproved. Considering the manner in which these tables have been formed, the error will not, I think, be found in them. The table of Mr. Finlaison does not differ widely from Mr. Milne's; and this, which can hardly be suspected of being unfavourable, particularly when applied to the population generally, would give a ratio far greater than $\frac{1}{53}$.

In France the ratio of the legitimate male births to legitimate female births is about 1·068, the ratio of illegitimate male births to illegitimate female births is about 1·048; this difference has been also remarked in other countries: see a paper by Professor Babbage, in Dr. Brewster's Journal of Science, New Series, No. 1. But we have no means, as far as I know, of ascertaining whether it exists in England; the parliamentary reports do not separate legitimate and illegitimate births, nor do the bills of mortality. It would be interesting to observe the differences presented by different counties with regard to the number of illegitimate births; and it is much to be regretted that no information on this subject is to be found.

In a pamphlet entitled "A Statement of the Principles and Objects of a National Colonization Society," it is supposed, p. 17, that in a population which increases at the rate of four per cent. per annum, the number of couples who attain the age of puberty is as one to one hundred in proportion to the whole population; and hence that in Britain, taking the population to be twenty millions, "the procreative power every year brought into action would be 200,000 couples."

According to the Chester table of mortality, to which I have before referred, Of 10,000 males born, 5765 } reach the
10,000 females born, 6302 }
age of 20. Therefore the males aged 20 are to the male births 20 years ago, as $\frac{5765}{10000}$. If we suppose the population to be increasing annually in a geometrical progression, of which the common ratio is 1·016 (according to the comparison of the censuses of 1810 and 1820), the males aged 20 are to the male births at present as $\frac{5765}{10,000 \times (1·0167)^{20}}$. The male births are to the female births as 1·0435 : 1.

 With

With these data it is easy to find that the males of the age of 20 now, are to the total number of births now, as 2548 to 10,000, and the females of the age of 20 now, are to the total number of births now, as 25,53 to 10,000; hence, if the total number of births annually be to the population as 1 : 31·854 (see p. 417, line 17), the number of males aged 20 now, is to the whole population as 80,000 : 10,000,000 nearly, and the number of females aged 20 now, is to the whole population as 80,180 : 10,000,000 nearly; hence in a population of twenty millions there are about 160,000 couples aged 20.

If the total number of births annually be to the population as 1 : 24·660 (see p. 417, line 31), the number of males of the age of 20 is to the population as 103,360 : 10,000,000; and the number of females of the age of 20 is to the population as 103,560 : 10,000,000; hence in a population of twenty millions there are about 206,720 couples aged 20. According to the census of 1821, p. xv. there are 988 males between 15 and 20 to a population of 10,000 males. Dividing this by 5 we have 197·6; the ratio of males to females in the population is ·95754 : 1. Therefore $\left(197{\cdot}6 \times \frac{{\cdot}95754}{2} = 94{\cdot}6\right)$ according to these data there are about 189,200 couples aged 20 in a population of twenty millions.

The rate of increase in England has never amounted to 1·04, nor do I think it has ever exceeded 1·017; during the greater part of the last century it was about 1·004. I have found that the ratio of the deaths to the population is very little affected by the rate of increase of the population; while the ratio of the births to the population varies very much. If the population increased at the rate of four per cent. annually, this ratio would be greater than $\frac{1}{20}$, and the number of couples aged 20 in the population of 20,000,000 would exceed considerably 200,000.

As the census is now under the consideration of Parliament, it is to be hoped that some means may be devised of obtaining more extensive information than has hitherto been given: the bills of mortality ought to be published for the whole empire every year; the ages at which deaths take place being distinguished for at least every five years. This is done, I believe, in almost every other country in Europe but our own, not excepting Russia; for I have the bill of mortality before me for that empire for the year 1827, including thirty-eight provinces, which are kept distinct, and which present altogether a total of more than 600,000 deaths.

It would be desirable also that the diseases should be stated, from

from which the deaths have originated, as is done in the London bills of mortality, distinguishing the sexes.

It seems necessary that a general registry of births, marriages and deaths should be established and enforced throughout the empire, including persons of all religious persuasions: if this were done and the results published, tables of mortality would furnish the means of deducing the actual number of the population more accurately than any census or direct enumeration, and at all events one method would be a check upon the other.

The following example will serve to show how a table of mortality might be deduced, if the number of persons alive in the population at every age were known. According to the table, page xv. of the Report of the Census of 1821, there are 1470 males between the ages 20 and 30, and 1155 between the ages 30 and 40, in a population of 10,000 males, since the tenth of these numbers will be very nearly the number of males between 25 & 26, and 35 & 36;

147 : 115·5 : : male births 25 years previously multiplied by the probability of a male at birth living 25 years: male births 35 years previously multiplied by the probability of a male at birth living 35 years; therefore

$$\frac{\text{probability of a male at birth living 35 years}}{\text{probability of a male at birth living 25 years}}$$
$$= \text{probability of a male aged 25 living 10 years}$$
$$= \frac{1155 \times \text{male births in 1796}}{1470 \times \text{male births in 1786}}$$
$$= \frac{1155 \times \text{male baptisms in 1796}}{1470 \times \text{male baptisms in 1786}}, \text{ nearly.}$$

The male baptisms in 1796 were 117,100 and in 1786, 100,895: with these numbers we get ·82971 for the probability of a male aged 25 living 10 years. The same probability, according to the table of mortality before referred to, p. 417, line 28, is ·8882; and according to the table of mortality given by Mr. Finlaison in the Parliamentary Report on Friendly Societies, 1827, p. 28, is ·8777. Similarly if the number living at each age were known, the probability of an individual at every age living one year, might be found, and hence a complete table of mortality.

LX. *On the Origin of the Improvements which have been made in the Performance of Steam-Engines in Cornwall, within the last seventeen Years. By Mr.* Farey.

To Richard Taylor, Esq.

Dear Sir,

IT is asserted in a communication at p. 323 of your last Number, that in my evidence on the patent laws, there are statements

statements injurious to some improvers and builders of steam-engines; and three passages, marked with inverted commas, are put forth as the substance of the objectionable statement.

In the evidence printed by the Committee, those passages are in the following words, respecting Woolf's patent:

"*Mr. Woolf's invention of working steam-engines by high pressure steam, acting expansively* (*either in one or in two cylinders*);" at your page 323, the words in parentheses are omitted. Again, in the printed evidence, speaking of deep mining in Cornwall, my words are, "*The difference in cost, between the quantity of coals consumed by the engines now in use* (*which are all on Mr. Woolf's system*) *and by an equal force of engines, such as were in use before he went into Cornwall in* 1813, *would absorb the profit of all the deep mining that is now carried on in Cornwall;*" this passage is altered at your p. 323, so as to become very indefinite.

What your correspondent calls the substance of my statement, is made the basis of an insinuation,—that in speaking to the Committee, I ascribed to Mr. Woolf the invention of expansive working, although in writing my treatise on the steam-engine, I had before ascribed it to Mr. Watt.

The true quotation, as above, shows what I really did ascribe to Mr. Woolf as his invention, for which he had a patent, four years after Mr. Watt had retired from business altogether; and it was not until after having made my definition, as above, that I stated the engines now in use for deep mining in Cornwall, to be all on Mr. Woolf's system. The meaning of the words I used, admits of no mistake, and the statement they convey is true and exact. If it be intended to deny that Mr. Woolf was the first who invented the system of working steam-engines by high pressure steam acting expansively, either in one or in two cylinders *, or to deny that he was the first person who put that system in execution, with engines of both kinds, first in London for turning machinery, and then in Cornwall for draining mines†, the origin of that system, with particulars of the person, time and place, should be stated by the objector.

The engines now in use for deep mining in Cornwall, are strictly within the terms of my definition of Mr. Woolf's invention; for they are worked by high pressure steam, acting expansively in one cylinder. The engines Mr. Woolf first made in Cornwall, with two cylinders, have gone out of use, not to resume Mr. Watt's system of working by low pressure steam acting expansively, but because engines with one cylinder

* See the specification of his patent of 1804. Philosophical Magazine, vol. xxiii. p. 335.

† See Philosophical Magazine, vol. xlvi. p. 463.

worked

worked on Mr. Woolf's system (that is, by high pressure steam acting expansively), are found better for draining deep mines, than those with two cylinders. Mr. Woolf's engines with two cylinders, for turning machinery, continue in use in London, and are very common in France; they execute their work with less fuel than any engines used for turning machinery.

It is asserted at p. 324, that variation in the elasticity of the steam employed by no means affects the invention, meaning Mr. Watt's invention of expansive working; also that evidence seems to preponderate against the use of steam of very high tension. And yet the column there entitled *Highest of Watt's*, beginning with 40·6 millions in 1823, and ending with 79· millions in 1829, relates solely to engines which are worked on Mr. Woolf's system; that is, by high pressure steam acting expansively. Let it be shown when and where any engines, not strictly within the meaning of those words, ever raised any thing like 40·6 millions, which is the least in the column entitled Highest of Watt's; or when and where Mr. Watt ever worked, or proposed to work, his engines by high pressure steam acting expansively.

Mr. Watt's engines are said at p. 324 to have done about 25 millions average duty, whilst superintended by his own agents. All the water-works in London are now served by Mr. Watt's engines, made by his successors in business at Soho, and good specimens of execution: they are worked by low pressure steam, acting expansively in one cylinder, and the performance of the best of them is about 25 millions; hence no improvement has been made in the performance of what are really Mr. Watt's engines since his time.

Notwithstanding this, your correspondent has put the title Highest of Watt's, to performances increasing each year from 40·6 to 79· millions, without explaining to your readers how such a great increase has been effected in the performance of what he calls Watt's engines. Any explanation that could have been given, must have brought out the fact which is kept back, viz. that they are worked by high pressure steam acting expansively in one cylinder; a system which Mr. Watt never did propose or execute; but as Mr. Woolf did both propose and execute it, and brought it into common use in Cornwall, how can my evidence, that those engines are on Mr. Woolf's system, be objected to? Or with what propriety can they still be called Watt's engines, when they are worked on such a different system as high pressure steam, and low pressure steam, and with such a very great advantage in cost of fuel, beyond all that Mr. Watt ever did accomplish?

If variation in the elasticity of steam no way affects the invention

vention of expansive working, why is high pressure steam used in Cornwall? explosions have been frequent enough* to deter the miners from continuing the use of such a dangerous agent, if they could carry on deep mining without its aid.

The obvious intention of the communication at p. 324 is to lead your readers to conclude that Mr. Woolf's patent was for no invention at all, and that the system of working steam-engines by high pressure steam acting expansively (which he alone introduced into use) has had nothing to do with the remarkable improvement of the performance of engines in Cornwall, within the last seventeen years.

The mention I made of Mr. Woolf's invention, arose incidentally in answering the various questions put by the Committee. I cited Mr. Woolf as an example of the case that has so often occurred, where an inventor who has rendered important service to the public, has derived no benefit from his patent, because it expired before the invention came into use; and I also cited Mr. Watt, as an example of the rare case of an inventor who did derive an adequate recompense for his public services, by the operation of an Act of Parliament, which prolonged the term of his patent.

If any member of the Committee had felt doubtful of the accuracy of my statement, further questions would certainly have been put, either to me or to others. I was fully prepared to substantiate all I advanced; and if you think it will prove interesting to your readers, you can reprint from the evidence reported by the Committee all that relates to Mr. Woolf, and I will supply you with the explanation which I should have given in support of my statement, if it had been called for.

I am, Dear Sir, yours truly,

JOHN FAREY.

37, Howland-Street, Fitzroy-Square,
May 10, 1830.

[We shall gladly avail ourselves of Mr. Farey's offer of further information on this important subject. It is due to the memory of our estimable predecessor, Dr. Tilloch, to state that, so far as pecuniary means have contributed to the completion of Mr. Woolf's invention, the public are in a great measure indebted to him for it.]—EDIT.

LXI. *On the Duty of Steam-Engines in Cornwall.* *By* JOHN TAYLOR, *Esq. F.R.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE attention of the Admiralty has, it appears, lately been drawn to the subject of the duty performed by the steam-engines in Cornwall. This is said to have been done by Cap-

* See Mr. John Taylor's paper on this subject in vol. i. of the present series of the Philosophical Magazine, p. 126, also p. 403.

tain

tain King, superintendant of the packets at Falmouth, and in consequence Mr. Rennie has been desired to investigate the facts.

This gentleman lately visited Cornwall, and selected for experiment Wilson's engine at Wheal Towan, erected by Capt. Samuel Grose, which has for two years past stood highest in the monthly reports, and which is so justly celebrated for its performance and for its general excellence as a pumping-engine.

The following are the particulars and dimensions of the most important parts, as noted by parties present at the mine at the time of the experiment.

Cylinder 80 inches diameter.
Length of stroke in the cylinder 10 feet.
in the pumps 8 feet.
Usual rate of working about $6\frac{1}{2}$ strokes per minute.
Draws 5 lifts of pumps of the following lengths and diameters.

	Heights.			Diameters.	Weight.
	Fath.	Ft.	In.	Inches.	lbs.
House lift	44	1	9	13	15313
Tye lift	43	5	9	$15\frac{7}{8}$	22656
Rose lift...............	32	5	9	$16\frac{1}{8}$	17525
Crown lift............	18	5	8	$17\frac{7}{8}$	12381
Puppy lift	9	4	2	12	2856
	149	5	1		70731

The water contained in this column is 1131·7 cubic feet; weight 70731 pounds; being at the rate of 11·15 pounds per square inch of the piston.

During the experiment made by Mr. Rennie, the engine made 979 strokes and consumed 6 bushels of coal, which gives for duty, according to the preceding data, 92,327,000 pounds lifted one foot by each bushel.

A former experiment on this same engine was made in 1828, in the presence of several mine agents and engineers, to which I alluded in my paper on the duty of steam-engines, published in the "Records of Mining*," and in which the result was 87,210,000 pounds lifted one foot by each bushel. I find that the duty reported monthly in the printed duty papers for 1829, is on an average 76,234,307; and it was 77,290,000 in the preceding year, as appears from the statements in the publication to which I have referred.

It is satisfactory to perceive that at length this subject seems likely to receive the attention, and undergo the investigation it deserves. The facts have been long before the public, but they have been much controverted, and rather treated as the statements of visionary or interested persons, than as substan-

* Murray, 1829. Phil. Mag. and Annals, vol. v. p. 297

tive results which merited examination. It has been preferred to suppose all kinds of error in the mode of computation or in the data rather than admit their consequences; and this I have reason to think has been done very often where a little inquiry would have produced a satisfactory explanation.

I published my paper in the "Records of Mining," with a view to promote such inquiry: I traced the history of the duty of steam-engines from the earliest period of their application; I endeavoured to show that the standard of comparison, as between one pumping-engine and another, was a fair one, notwithstanding some unimportant imperfections; and that it was to this standard that Messrs. Boulton and Watt appealed for proof of the superiority of their engines when engaged in legal disputes respecting their patent rights, or in claiming their allowance for the use of them.

I attempted to correct a misapprehension that I believed to exist as to the purpose of the monthly reports of Duty of Engines in Cornwall, and as to the parties by whom they are caused to be made. I had often heard, and I continue to hear, that they are published by persons concerned in making engines, and that the object might therefore be to exaggerate the value of particular improvements: it has sometimes on the other hand been stated, that they are carelessly or ignorantly done. I showed, in reply to this, that these reports are procured by the adventurers in the Cornish mines, who use engines so extensively as to render them especially concerned in finding out who make them the best, and whose only partiality that I know of, to any construction of an engine is, to that which will do their work at the least expense. The history of the steam-engine will exhibit the same fact; and hence it has been that Newcomen, Smeaton, Watt, Bull, Hornblower, Woolf, Grose, and others, have found in Cornwall the principal field for their first efforts.

The adventurers in deep mines in Cornwall may well desire to know accurately the value of the different applications of steam power; their interest in that question is perhaps greater than that of any other individuals. One concern with which I am connected, employs 8 engines for drawing water, of which 4 have cylinders of 90 inches in diameter; the monthly consumption of coal is about 14,000 bushels, and the expenses near 12,000*l.* a-year. But if we take all the mines in the county into consideration, there will appear abundance of reason for great interest in this matter; and the fact being so, how absurd would it be to permit any mode of self-deceit! and how much more absurd to pay liberally for information, on which, according to some, no reliance is to be placed!

But

But are all the adventurers in Cornwall likely to be so ignorant? and are they not in a situation to have acquired more experience in engines than most other persons? They have witnessed each step of improvement, they have been parties to the strifes and contentions respecting them, and they can certainly tell by their cost-books whether their saving in coal and in money corresponds or not with the calculations of duty which they receive from the agents whom they employ and pay to furnish them with this information; and they or their managers can judge whether such agents are competent to the task, and perform it correctly. It has been suggested also, as I hear, that the data are taken in a negligent manner, that pumps are not correctly gauged, that the coal is not measured, and that all is vague and indefinite. Now I have already endeavoured to show how important it is thought by the owners of these engines to have a very correct account of their duty. I have stated that they pay liberally for such an account, and that their agents at least are competent to judge of the manner in which it is furnished by the parties specially appointed for the purpose. It is therefore rather too much to suppose gratuitously that the means of obtaining correctness are totally neglected.

I assert, however, that the greatest possible care is taken in all the steps of the process; and this I will endeavour to explain more fully. The duty of the Cornish engines is taken and computed by Messrs. Thomas and John Lean, who publish separate lists of the engines which they respectively have the charge of; the counters are carefully under lock, and no one has access to them but themselves. At some mines other counters are attached for the inspection of the agents, and at the Consolidated Mines a daily computation of duty is made from them, which is posted up in every engine-house on the concern. The average of these, I may observe, is compared with the monthly report, and found generally to coincide as nearly as possible.

The calibre of the pumps is taken occasionally by the reporter; and I have been assured by Captain Thomas Lean, that he does not rely upon any account given to him, but on his own measurements; they are subject to some alteration by wear, which is attended to from time to time. The experiment upon Wheal Towan furnishes some evidence on this point; the diameters of the pumps which I have given above were taken, I presume, to satisfy Mr. Rennie, and they are stated to eighths of an inch; they agree very nearly with those in the report of March by Mr. John Lean. The shortest way, however, of comparing, is by taking the computed weight

of water by each method, which, calculated for Mr. Rennie's experiment, was.................................. 70,734 pounds.
Stated in duty paper for March 70,605

So that taking diameters and lengths together, they were found rather to exceed the printed account, but differ only by 129 pounds in the whole.

Those who are acquainted with the management of the mines know that the coal is very accurately measured to each engine; both that the duty of the engine may be ascertained, and that the drawback allowed by the Custom-House may be obtained, which is granted on the oath of the parties that the measurements are correct.

I will explain with how much precision this is conducted at the Consolidated Mines. In each engine-house a paper, printed in columns to serve for one month, is placed, called *The Engine-men and Coal-measurers' List.* The engine-men are required to place in the column, No. 1, a statement of the quantity of coal wanted for the next 24 hours, which is to be done between the hours of 7 and 8 in the morning for every respective day of the month, and where the coal-measurer may refer to know the quantity required. This person delivers the quantity required daily, by accurate measurement, and places the account of what he actually delivers in column, No. 2; and further, one of the engine-men on duty must see the coal delivered into the sheds, keep an account of the measurement, and certify the quantity by marking it in column, No. 3. A penalty is imposed for any neglect of this order.

The engine-men, being to a certain extent responsible for the duty of their engine, ought to see, as they do by this regulation, that they have their proper quantity of coal; and it is the express purpose of the coal-measurer's office that no more than what is accurately registered be delivered.

But here, again, the experiment on Wheal Towan engine may be quoted; the duty was found to be 92,331,899. The experiment in 1828 gave a result of 87,000,000, and the average duty of 12 reports published last year was 76,234,307. Now as the performance of any engine under experiment will be free from those stoppages or hindrances which we know diminish duty, and which cannot be avoided in the work of a month, we may expect that what would be found in the one case would exceed that of the other; and so it proves to be. Now as doubtless Mr. Rennie, and the gentlemen who were with him, took care to see the coal properly measured and applied, it follows that the measurement for the 12 months, of which the duty was reported, was very nearly correct also.

Wheal Towan is a mine which was in full work previous to

1814,

1814, when it was given up. It was undertaken again by the present adventurers in 1826; it is not situated near other mines, by which it can be suspected that water is drained from it, or that there is now less to draw than formerly. It is worked deeper than before, and the engines are more loaded in that proportion. As greater steam power must therefore be required than in 1813; is that obtained by the use of a smaller quantity of fuel? and if so, is the diminution in quantity proportionate to the improvement in engines, which we say has taken place; and has that improvement been correctly represented in the duty papers? If it should appear to be so, then the measurement of coal, the diameter of the pumps, and all other data, have been correctly assumed and used, and for a long series of years.

I have given in the "Records of Mining" several instances to show that the actual quantities of coal used by different mines have diminished proportionately with the reported improvement in the engines; and I presume it will not be supposed that the account books are incorrect, which show the quantity of coals bought and paid for. I will repeat the case of Wheal Towan, as it appears to me decisive, and as it is one with which I am, personally, totally unconnected, and can have therefore no partialities to bias me.

When the mine ceased to work in 1814, the average monthly consumption of coal had been for some time 9360 bushels; the average duty of engines in Cornwall was then about 20,000,000, according to the tables which I have given, and which we may assume as the performance of those engines for this calculation.

When I wrote my paper in the latter part of 1828, the monthly consumption of coal at Wheal Towan was but about 2600 bushels, and the average duty of the two engines working was 66,000,000, as reported in the duty papers.

Now 66,000,000 is to 20,000,000 as 9360 is to 2830, which is sufficiently near to prove the case; and in taking a larger average of the coal, and since my paper was published, I find the monthly quantity is increased to 2900 bushels, the time including winter months, when there is more water to draw, and the depth of the mine is somewhat increased. The average duty of the engines has in the same time advanced to near 70,000,000; and calculating as before, 70,000,000 is to 20,000 as 9360 is to 2674, which shows a very near approximation to what would be inferred from the actual quantity of coal supplied.

There is therefore every kind of proof that the application of steam has been improved so as to œconomize fuel in Cornwall,

wall, and that the rate of improvement has been well expressed by the printed reports. The same system and the same mode of estimating duty have been adopted in the lead-mines in Flintshire, and the advantages are sufficiently manifest. I can add that I now receive a regular duty paper from Mexico, showing the great advantage that the steam-engines at Real del Monte have derived from the application of the later improvements to them.

The evidence has been long before the public; and your predecessor Dr. Tilloch, who indeed was interested with Mr. Woolf, repeatedly published statements in the early numbers of the Philosophical Magazine. Mr. Farey visited Cornwall with a view to investigate this subject, and this he did as long ago as 1818. He conducted an experiment on one of Woolf's engines, then working at Wheal Abraham, and the result was in his opinion conclusive. The learned President of the Royal Society, who has had longer acquaintance with the progress of improvement of steam-engines in Cornwall than almost any other person, and who was in the year 1798 one of five individuals to whom disputes relative to the real performance of Mr. Watt's engines were referred, has illustrated the subject in two papers read before the Royal Society: one printed in the Transactions for 1827; and the second in the present session, just published. He quotes in both the duty papers as authorities to be relied on; and in the latter, states that the best engine, which is the same that Mr. Rennie has taken for experiment, performed a duty in the whole month of December 1829, exceeding the average of 17 engines on Mr. Watt's construction in 1793, by a proportion of nearly 4 to 1.

My paper in the "Records of Mining" was published in the early part of last year; and as it was noticed in your fifth volume, I shall only say that I considered the account there given of the progress of improvement from year to year as the most interesting part of it, particularly as a great portion is taken from documents which are not in the hands of many. The results are quoted in your pages.

I trust and hope Mr. Rennie's opinion and conclusions on this interesting subject may also be communicated in some manner to the public, and that it may be strictly investigated by those who may continue to doubt.

As much has been said about the motives of those who publish statements on this subject, I must be allowed to repeat for myself and other adventurers in the mines in Cornwall, that we have no interest whatever in inducing others to give credit to those statements. We know that we are reaping

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ing great advantage from the use of certain improvements, and we might be content to keep this knowledge to ourselves.

We are not the manufacturers of the engines, but the purchasers of them; we have no monoply to maintain, or any other advantage to enjoy, but what may be shared by every individual who may wish to employ similar machines; but we do feel it to be an act of justice to those ingenious men, by whose labours we have benefited, to bear testimony to the success of their efforts: and as, since the time of Boulton and Watt, no one who has improved our engines has reaped pecuniary reward, it is at least fair that they should have credit for their skill and exertion. We are not the partisans of any individual artist, we avail ourselves of the assistance of many; and the great scale upon which we have to experiment, makes the result most interesting to us.

In the last year, besides many smaller engines for winding and stamping, the mines in which I am interested in Cornwall and Wales, employed 25 steam-engines for pumping, of which 17 have cylinders from 60 to 90 inches in diameter: their consumption of coal was 495,434 bushels; and a due regard to the œconomy of the application of so vast a power is sufficient to interest me in every thing by which it may be promoted.

London, May 25, 1830. JOHN TAYLOR.

LXII. *On the Dying Struggle of the Dichotomous System. By* W. S. MACLEAY, *Esq. M.A. In a Letter to* N. A. VIGORS, *Esq. F.R.S.**

My Dear Vigors,

SOME years have now elapsed since a gentleman, the sable hue of whose vesture, if not the smile on his countenance, betokened that he should be in peace with all men, came up from the North to London, and announced himself to me as the Rev. John Fleming, D.D., Minister of Flisk, N. B. Considering him to be entitled to my services, as being a labourer in the same vineyard with myself, I of course showed him all the attention in my power. I knew him indeed at the time only by two or three articles in the Supplement to the Encyclopædia Britannica, which, if they be not fair specimens of a Scotch D.D.'s usual quantum of Greek, will at least remain a monument of his talent for writing on animals that he not only never saw, but would not even now know if he saw them. In addition to these truly *novel* specimens of Entomological

* Communicated by N. A. Vigors, Esq.

knowledge,

knowledge, I knew him also by a subsequent compilation, called with much modesty "The Philosophy of Zoology;" the first volume of which contains nothing new but some miserable plates, and the second, little original except a genus termed *Homea*, and some names which have been framed in a proper independent spirit, and with a noble contempt of Priscian*. These works indeed have been somehow or other praised in certain journals; but as, I repeat, I thought our Reverend Doctor to be an industrious man whose love of natural history was struggling with various obstacles, I left the development of his true merits to time. As his works were seldom or never quoted by naturalists, it mattered little to me how they were lauded; and a learned Oxford Professor having stript him of some of his borrowed plumes, I thought that small credit was to be acquired by plucking him of his last feather, while at all events there was charity in the forbearance. Since I left England, however, I have been informed that he has written some book on British Animals, in which he has distributed them, as proposed in his former work, in double Indian file; that is, by two and two; or in short, in some way that he fancies will render him immortal. Now two or three years before the publication of "The Philosophy of Zoology," and before my having any idea of Dr. Fleming's splitting on this rock, I had exposed the absurdity of a system which I termed "Dichotomous." I pointed out its deficiencies in the *Horæ Entomologicæ*, p. 188, and afterwards in a paper read before the Linnæan Society in 1826, and since printed in their Transactions. Ignorant apparently of these hints of mine, that might have saved him from much disgrace, and chuckling over his discovery of a mare's nest, Dr. Fleming unadvisedly staked his reputation on the Dichotomous System. *Hinc illæ lachrymæ!* Some good-natured friend seems lately to have shown him that his system was annihilated by anticipation; when what does this worthy clergyman do, but in the most orthodox spirit of theological hate vent his rage, through the medium of the Quarterly Review, on me, who never so much as thought of him! How he got his article inserted there I know not; but

* Thus we have *Trochusidæ*, *Gordiusidæ*, and *Ptinusidæ*; *Gyrinedæ*, *Cicindeladæ*, and *Scirtesidæ*, cum multis aliis in *dæ* of similar calibre. Having *two* Ds tacked to the end of his own name, the worthy minister doubtless thinks that he has a right to clap *one* to the tail of any thing. On the other hand, there is no end to old friends disguised under such appellations as *Thaphozus*, *Ornithorinchus*, or *Ophisthocomus*, &c.; which might pass for slovenly printing, were they not accompanied by such new words as *Aluco*, *Cistuda*, *Coriudo*, and some hundred of equally classical value. Luckily, however, no great harm is done; for few naturalists place the Doctor's names even in their list of synonyms.

but, I suppose, that, trusting to his universally applicable binary system, he knew that editors must be either asleep or awake, and with cautious modesty preferred to catch the King of the Quarterly napping. So far in this respect he seems to have succeeded, that a considerable part of his article is composed of verbatim extracts from "The Philosophy of Zoology." As for me, I certainly would have consigned the author of this Review to the contempt he deserves, if he had not contrived to get it inserted in so respectable a periodical. But now I must confess, that the minister's venom becomes more likely to poison the minds of persons not versed in natural history, than it could have done if conveyed through the pages of "The Philosophy of Zoology."

Dr. Fleming's scientific history seems to be this:—Confined to his parish in a remote part of the kingdom, he took up zoology to be a resource against *ennui*, as many others have done before him; and measuring Nature by his own capability of observing her, he stumbled on the Binary System, probably because the ins and outs of the pulpit appeared to him to be the most interesting division of his flock; and because the minister and his precentor furnished him with the most obvious subdivision of the contents of the pulpit. With the scanty museum afforded by his glebe, he came in due process of time, as may be seen from the Quarterly, to distinguish accurately the hare from the rabbit, and the dwarf from the common elder. Practical knowledge being thus acquired, and some theory from such rare authors as Linnæus, Cuvier, and Lamarck, our naturalist set up as a contributor of Natural History articles to the Edinburgh Encyclopedias; a class of works, that, so far as we have yet seen, has presented us with nothing in zoology but stale compilations miserably travestied. Flattered by a success in this drudgery, which few naturalists would have contested with him, he then compiled his "Philosophy of Zoology;" a book which rests its sole claim to distinction on the Doctor's formerly supposed discovery of the Dichotomous System. It so happened, as before mentioned, that by two or three words I had previously demolished this system; and now the "philosophy" of our zoologist, on finding it out, will not allow him to forgive me. Had this man more depth, my task in quenching him might have been a difficult one; for you know I have no extensive libraries to refer to: but as no part of his attack that is not marked with the stamp of ignorance claims a more respectable character than that of superficial ribaldry, I am not afraid to take up the gauntlet he has so foolishly thrown down.

In the first place I must inform him, that it is a matter of

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the most perfect indifference to me, whether he advocates the binary, the quinary, or the centenary system. I write not, and I have never written, but to amuse myself. I have never degraded natural history into book-making, nor considered the science as a mode of making money by puffing. If I had, the *Horæ Entomologicæ*, the principal part of the copies of which was burnt in the bookseller's hands, with his house, almost immediately after the publication, would long ago have been reprinted. My work, therefore, has remained in few hands; and if it has attained public notice, it is certainly not owing to any extraordinary trouble having been taken by its author for that purpose. I cannot indeed be blind to the changes it has effected in the English school of zoology. These are evident on the slightest comparison of the zoological works published in England previous and subsequent to 1822. But I can assure Dr. Fleming I never puffed the work: I have neither unblushingly reviewed it, nor attempted to burnish it brighter by detracting from the just merits of other works. And, in short, although repeatedly called upon by my bookseller, as well as by eminent naturalists, to republish, I have left it to its fate on the strength of some 80 or 100 copies, which I believe are all that exist, in the hands of naturalists. This conduct, however, which in a great measure has resulted from the irksomeness of going twice over the same ground, I now perceive to be attended with the great inconvenience, that it allows any malicious or dishonest person to misquote me as he pleases; for few have the means of detecting the crime. To those who have seen my work, I appeal whether the author of this article in the Quarterly has not written in malice. This is evident from his mode of quoting; from his disgracefully pirating some of my observations and making them pass for his own; and, lastly, from the obvious desire he has to distinguish between MM. Lamarck and Macleay, by the former being the " Author" of the *system of Progressive Developement*, and the latter only the " Advocate" of the *system of Circular Affinities*. While on this head, I must remind him, that notwithstanding his once supposed " discovery," the minister of Flisk is himself only the advocate of the Dichotomous System; with this difference indeed, that when every other person has taken it up, tried it, and spurned it, he alone remains to admire the beauties of this stale mode of division. Indeed, Dr. Fleming now says, " As the dichotomous method is the exhibition of a process of thought universally practised by the human mind, we may well be surprised that any other mode of arrangement for establishing a distinction among species ever usurped its place. We" (i. e. Dr. Fleming) " disclaim all idea of regarding it as a modern invention, or that Peter Ramus has the merit of its establishment." Now be it observed, that I never said that Peter Ramus had the merit of its establishment. My words are, "Those divisions so much insisted on by Peter Ramus, which consist of two members, one of which is contradictory of the other, are sure to be complete; but, unfortunately, one or both are always too comprehensive." Dr. Fleming adds, "In the earliest writings of the world, those of the Jewish legislator, positive and negative characters are used in classifying organized beings, exhibiting not indistinctly the rudiments of the dichotomous method." No doubt. It is precisely because his writings are the earliest in the world, that positive and negative characters are used by Moses. But, observe, he does not use them always; and, therefore, if it be Scripture that is to afford us rules for natural history, no absolute rule is laid down on this particular subject. It is really surprising how this eleventh chapter of Leviticus is shovelled into our faces by various writers. Another clerical naturalist has supported the quaternary division, because in this very chapter grasshoppers are divided into four kinds, the word translated "beetle" in the Bible being considered by Hebrew scholars to allude to a kind of locust. Dr. Harris, on the other hand, in his "Natural History of the Bible," gives the Mosaic arrangement of natural history as into triads, thus.

1. Vegetables	1. Grass. 2. Herbage. 3. Trees.
2. Aquatic Animals ...	1. Animalcules. 2. Fishes. 3. Birds.
3. Terrestrial Animals	1. Cattle. 2. Wild beasts. 3. Reptiles.

Another clergyman afterwards, looking also to Scripture, declared seven to be the correct number. Where Doctors differ, who shall decide? Clergymen, it must be allowed, whether Catholic or Protestant, have too great a propensity to silence all inquiry with a text. It is a very convenient mode of getting rid of an antagonist; as they have only to raise the hue and cry against him for disputing a Bible truth, and the affair is settled. Here, however, are an English, Scotch, and American divine, quoting Scripture against each other, each for his favourite number, seven, two, or three. Solomon moreover distributes animals by a quaternary division. Am

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I not therefore, unlearned layman as I am, permitted to doubt that Scripture has laid down any positive rule on the subject? I suspect indeed, that, whatever Moses may have himself known, he was aware that he was writing the Pentateuch for men who were neither chemists, astronomers, or naturalists; and that if he had addressed the Israelites in the language of science, his laws on the subject of clean and unclean animals would have remained incomprehensible to them. In the very chapter which we are now told exhibits not indistinctly the rudiments of the dichotomous method, we find the following law: "And these are they which ye shall have in abomination among the fowls; the eagle, &c. and the stork, the heron after her kind, and the lapwing, and the bat." Are we then, upon the authority of Moses, to place the bat among fowls? For heaven's sake, if clergymen be anxious, and *properly anxious*, not to forget their profession, let them at the same time bear in mind that the cause of religion is more hurt than aided by absurd and inconsiderate zeal. The Bible was intended to direct our moral conduct and religious belief. No one but a madman, a fanatic, or an interested knave, can pretend to tell us that it is an encyclopædia of science. But Dr. Fleming, not content with the Mosaic authority, says, "In the writings of Aristotle equally obvious traces of this method may be perceived, not only in the construction of his different classes, but in the numerous accurate subdivisions which he announced." Let us here dichotomize. Either Dr. Fleming has read Aristotle or he has not. If he has read him, he has stated what he knew to be not true; since the Stagyrite has not, as may be seen from the synopsis I have given of his System in the Linnæan Transactions, considered it more necessary to divide animals into twos, than Lamarck, Cuvier, or Linnæus: and if the Doctor has not read him, what shall we say to the modest assurance of this minister of Flisk? Although Aristotle, however, has not deemed it requisite to construct his different classes and numerous subdivisions necessarily on this method, he was perfectly aware of its merits and defects, as has been shown in the Linnæan Transactions, from which I quote the following words: "Secondly, Aristotle says, Organs may be arranged according to their excess and defect (καθ' ὑπεροχην και ελλειψιν). This being entirely a consideration of quantity and not of form, his mathematical axiom comes into play. His opinion is accordingly correct, that animals are capable of a binary distribution, depending entirely on the excess or defect of particular organs; as where he instances birds being divisible into those with long and those with short beaks, into those with crests and those without crests, &c. &c. This is the most arbitrary, and

and therefore, I suppose, the oldest of all modes of arrangement: and, as Aristotle expressly says, so easy that any one may adopt it. I have said a few words on its merits in the *Horæ Entomologicæ;* but the truth is, that, proceeding entirely on the notion of division, and not of affinity, it is a method which is applicable to all sciences whatsoever as much as to zoology. It has nothing to do with the Natural System, which must of course depend upon affinities." In fact, it is the principle of distinction carried to the extreme; and Aristotle therefore was perfectly aware that it must completely fail when our object is to connect animals by their affinities.

Dr. Fleming boldly adds: "More recently, in the writings of Lister, Willoughby, and Ray, the dichotomous system occupied the situation which it merited." Now Willoughby have I not got with me in the Havana to refer to; but his arrangement ought to be that of Ray; and both Ray and Lister have given the binary method the situation it merited, that is, they have disregarded it; and nothing but the most consummate courage could ever have induced Dr. Fleming to bolster up the child of his adoption by such *equivocal* assertions. Oh! but Aristotle has τα ζωα and τα φυτα, &c., and Ray his *Metamorphota* and *Ametamorphota*, &c. No doubt they have. Every naturalist has thought it proper to divide some group or other into two parts; but it is quite another thing to have no other method of dividing groups, and this it is which our D.D. is driving at.

Immediately on the publication of my work, a respectable naturalist, Mr. Haworth, took up this method in the Philosophical Magazine, and even gave afterwards, in the same periodical, a division of *Crustacea* founded upon it; so that the minister of Flisk has not even the credit of reviving dichotomy. Indeed I may myself lay claim to its revival, although I raised it from the dead only for the purpose of pointing out its defects. Mr. Haworth, I believe, like every naturalist of sense, soon abandoned it; and Dr. Fleming, as has been said, remains its sole admirer.

This dichotomizing Doctor takes the "*Divisio Naturalis Animalium*" of Linnæus; and exclaiming no doubt with his brother Dominie, "*Prodigious!!!*" he transforms it into the following table of classes, containing every animal created.

1. Cor biloculare,	biauritum; sanguine calido rubro:	Mammalia. Aves.
2. Cor uniloculare,	uniauritum; sanguine frigido rubro:	Amphibia. Pisces.
	inauritum; sanie frigida albida:	Insecta. Vermes.

Prodigiously clever, indeed! Now to carry on the dichotomous method,

method, either Dr. Fleming believes this to be a correct mode of distributing animals, or an incorrect one. If he thinks it correct, he by means of his binary system makes the notable discovery, that insects and worms have *all* a heart and white blood! And if he thinks it incorrect, it surely is as little a proof of the beauty of the binary system as of the brightness of his brains, that it should chime so readily in with error.

A similar dichotomous dilemma may be applied to the Doctor's botany: for in the same way this frisky and "frolicksome" minister "makes a great leap" to plants; and, to prove his intimate acquaintance with natural orders, chooses, κατ' εξοχην, the Linnæan class *Diandria* and order *Monogynia*, a group which most naturally lumps the ash, the speedwell, the sage, and the duckweed, all together. The duckweed however running rather restive, our botanist very conveniently leaves it out, and then divides his "Assemblage of approximations" thus, in what he calls "their proper subordination."

1. Flores inferi monopetali.
   - α. Regulares.
   - α α. Irregulares.
   - β. Fructus capsularis.
   - β β. Fructus gymnospermi.
2. Flores superi.

Methinks I hear the veriest clodpoll that ever rang the kirk-bell of Flisk here exclaim—

"Minister, why I'll rime you so, eight years together; dinner and supper and sleeping hours excepted: It is the right butter-woman's rank to market. For a taste.

"Man.
- 1. Scotch.
  - 1. Breeched.
    - 1. Dominies.
      - 1. Of Flisk.
        - 1. D.D.
          - 1. Fleming.
            - John.
            - Not John."
          - 2. Not Fleming.
        - 2. Not D.D.
      - 2. Not of Flisk.
    - 2. Not Dominies.
  - 2. Not Breeched.
- 2. Not Scotch.

Thus there is no clodhopping lout in his parish that may not always obtain, according to Dr. Fleming's wonderful plan, two groups "or inferior divisions, the first distinguished by a positive character, and the second by a negative;" and who may not thus ultimately make out John Fleming, Doctor of Divinity and Dominie of Flisk, to be a breeched Scotchman, or or any thing else, provided always that the said lout does not fall into "the error which Linnæus committed, in neglecting to arrange his groups so as to preserve their subordinate relations." This evil Dr. Fleming thinks not likely to be removed "until distinct conceptions shall prevail respecting positive and negative characters." Although there be too much reason to fear that his own distinct conceptions are deferred *ad Græcas calendas*, I still indulge some hope of his granting that in the above "dichotomous arrangement" a very distinct conception of positive and negative characters has been shown. Man in this system may be compared to the trunk of a tree, Dominies and D.D.s to the branches, and John Fleming to the bud or leaf on the spray.

But jesting apart, it is almost incredible that any person pretending to be a naturalist should at this time of day palm such stuff upon us as the *summum bonum* of natural history. "How different," cries the Reverend D.D. in the very ecstasy of self-approbation, "How different is this mode of aiding the student, from those cumbrous methods in common use! But there are obvious reasons why it is so generally neglected. The labour of an author who pursues the dichotomous method is greatly increased." Increased indeed the labour is by the distinction of *yes* and *no* on every possible point of habit and structure being manifestly infinite; and nevertheless such is the degrading nature of this useless toil that it can be undertaken by every person who can count his five fingers. But if the labour be thus *greatly increased*, according to the Doctor's confession, how is the Dichotomous method *less cumbrous* than the methods in common use? "Aye, that is a question," as his countrymen say.

I never have had the sleepiness of knowing whether Dr. Fleming be one of the tribe of spin-texts, but he now gives ample proofs of his being a very able Twist-text. He excells even his Joe Miller brother of the cloth, who extinguished ladies' top-knots by the well-selected text "Top not come down." Our D.D. deserves credit certainly for lugging me in, of all persons in the world, to favour, by hook and by crook, his dichotomous system. As he had so preposterously cited Aristotle, Lister, and Ray, he might certainly as well throw me in to swell his lump of authorities. "Instead of having discovered the circular arrangement in the organized kingdom" (here by the way is another new kingdom) "as a whole, and accomplished a quinary distribution at the very commencement of the scheme—*the author* has found it necessary to consider animals and vegetables as constituting only *two* distinct circles." To be sure I found it necessary, because Nature points

points out those divisions as two out of five. Nothing would have surprised me more than to have discovered organized matter forming a circular group as a whole, or to have accomplished a quinary distribution of it at the outset. This worthy, therefore, according to his favourite dichotomy, must either not have read the following passage of my work, or, having read, have been too dull to understand it. My words are as follow: "We have two natural, but I fear somewhat arbitrary, divisions of matter, into organic and inorganic. No person denies the existence of this division in nature, still less is the use of it to be despised. The truth however is, that the first great division of matter is not yet ascertained, and the knowledge of it, to say nothing of the celestial bodies, must in a great measure depend on the labours of the chemist, who has hitherto so little elucidated the nature of heat, light, and many other of those subtle substances which are possibly forms of matter." Who does not see that I was here hinting at the quinary division of matter, as much as if I had expressed it tabularly thus?

MATTER.

Normal group. *Organic.*	1. Animals.
	2. Vegetables.
Aberrant group. *Inorganic.*	3. * * * *
	4. * * * *
	5. * * * *

If the Reverend Doctor be still in doubt that this was my meaning, let him turn to vol. xiv. of the Linnæan Transactions, p. 60. There, speaking of M. Fries, I say, "Consequently in every circle he admits the existence of two central groups, and three radial; that is, in all five natural groups. Thus organized matter is the centrum of matter, and is composed of animals and vegetables. And so on, we shall ever find a natural group to be a circle of five minor groups, and that two of these minor groups form what M. Fries would call a *centrum*, or, more correctly, have some character in common which distinguishes them from the other three." So much for the first citation from the *Horæ Entomologicæ* in favour of Dichotomy.

Next he says: "Even animals themselves are considered as having *been created on two distinct plans*." This, a quinarian may be allowed to say, is the very quintessence of quoting. I shall give the whole passage as it occurs in the *Horæ Entomologicæ*: "Animals *appear* to have been created on two distinct plans; or, to make use of an idea frequently adopted in this work, Nature seems in the Animal Kingdom to have set out from inorganic matter by two different routes, which meet

meet together, and complete the circle among the *Annelida* and *Crustacea.*" In short, the two routes form the circle;—but the latter part of the sentence did not suit the Doctor's purpose. So much then for his second quotation to prove me adopting his binary division in opposition to the circular arrangement.

"The distinction between these two plans is strangely inaccurate," adds the Dominie, "since among the *Annulosa* of the second division, organs of circulation and respiration are exhibited as perfect as among the *Mollusca* of the first division." Indeed! Who told him so? One would really think that he had caught me tripping; but, unfortunately, our worthy has here, with his usual fairness, appropriated to himself the discovery of my inaccuracy; when, after stating the above two plans, I myself immediately say, "This difference of construction may be thus represented, *although speaking properly, the rule only applies to* the greatest part of the *Annulosa*, and not to the *Crustacea* and *Arachnida.*" And yet, pretending to detect my strange inaccuracy, he kindly offers to overlook it, although I verily believe he would never have been aware of the objection if I had not laid it before him.

The truth is, if our worthy D.D. had been wise, he would never have stirred up this puddle, for no better proof can be given of the absurd inutility of his Dichotomous System. I pointed out as above a binary distinction, and at the same time showed its inaccuracy. The Dominie most honourably states this inaccuracy as a discovery of his own; most Christianly charges the fault on me; and, nevertheless, in spite of all inaccuracies, having a strong stomach for number two, and an inordinate love of number one, he greedily swallows this very self same binary division of animals. There is no straw but what he catches at in favour of Dichotomy. Why, if it was thus, not with propriety, but with impropriety, that I intimated that the animal kingdom may be divided by positive and negative characters into two groups, how comes it that Dr. Fleming so readily adopts the division? I will answer. He cannot omit the occasion of falsely charging me with inaccuracy, but is at the same time too blind to see that he thus defiles his beloved mare's nest. He ought to have recollected his country's definition of *an ill-bird.*

"In the course of the efforts which have been made to establish the Quinarian System," the Doctor says, "we have witnessed a classification of animals founded on the characters of their circulating and respiring organs sacrificed, with scarcely the shadow of apology, to hypothetical views." This I imagine indeed to be the whole secret of the Review. Dr. Fleming has witnessed his dichotomy of British animals founded on their circulation spurned, scoffed at, and sacrificed to that quinary system, which takes notice of circulation and every thing else, and which in the vexation of his soul he is pleased to call hypothetical views. Not a shadow of apology has been vouchsafed on the occasion. "Even the division of animals into vertebral and invertebral has been rejected, because it does not state enough; and that the young naturalist, placing full reliance on it, may be led to conceive that animals have been formed on only two distinct plans." Thus I have declared that the *Vertebrata* form one natural group, and that so far all other animals may be correctly called *Invertebrata*; but it is said that these *Invertebrata* do not present one natural group of equal degree with the *Vertebrata*, but four. "This statement however," says the Dominie, "exhibits a very inaccurate view of the subject; for while the vertebral group is declared as formed on a Plan (and this no one will deny), the invertebral group is distinguished only by the negative mark, being destitute of a vertebral column and concomitant characters." This means, in plain English, that the above statement is inaccurate, because what is not vertebral must be invertebral,—because Dr. Fleming is not the Pope.

Although it does not bear on the question under discussion, the truth, *that what is not vertebral must be invertebral* is not very difficult to be believed. But it does not therefore follow that no animal out of the natural group called *Vertebrata* can possess vertebræ. Such a notion, it is to be hoped, is not a good test of the state of zoological knowledge on the north side of the Tweed. Here is a D.D. who has been for years manufacturing books on Natural History, and he appears not yet to know what every zoologist since Aristotle could have told him, namely, that *Annulosa* possess a vertebrated column, but differ from the *Vertebrata* in having it on their outside. Perhaps he will say that he knows this also, and has therefore grouped the *Annulosa* with the *Vertebrata* under the common title of *Articulata* * in his late work on British Animals. I know not whether he has done this; but if so, how comes he in another part of his article to be angry at the *Mollusca* being proved inferior to the *Annulosa*?

But I proceed with his quotations from myself in favour of number two. It seems that Mr. MacLeay "acknowledges that the *Vertebrata* are the perfection of one plan of organization, as the *Annulosa* may be of another." I do indeed acknowledge it, and much more my surprise, that this most brilliant genius has not also scraped up in his own behalf

* See *Linn. Trans.* vol. xiv. p. 60.

passages

passages out of my works to much greater purpose. For instance, in *Hor. Ent.* p. 134, he will find "En, naturæ arboris dichotomi corpora omnia proferentis terrestria organica ex ramusculis extremis aspicias unum!" In page 200 he will find that "the great object of my essay is to trace the ramifications of this *dichotomous tree* * to its extreme fibres," &c. &c. The Doctor indeed, as usual, appropriates to himself this last idea, and clothing it in his own milk-and-water language passes it off on the poor editor of the Quarterly as his own. But the strongest argument in his own favour that he could possibly have taken from my works, he has most unaccountably altogether omitted; to wit, that to this day I always dichotomize a group of five into two *Normal* and three Aberrant groups, which two divisions are distinguished from each other by a positive and negative character. "Ah!" says the Dominie, "after all, at last I have caught you; you remain binary to the backbone." So far indeed I do, and that for a very simple reason, which will show him how he and all other admirers of positives and negatives fall so invariably on the number two.

Linnæus said, "Scias characterem non constituere genus, sed genus characterem." This rule indeed is in direct opposition to the Dichotomous System, which first fixes on some positive character, and so forms the second group by antithesis. Nevertheless I adopt the above Linnæan maxim, and having followed my circle of affinities, and found two out of five groups to be what Fries called a *centrum*, I then seek for some character that will insulate this *centrum*. Having discovered this, I find then, without the aid of the least magic, that the other three of the five groups *must remain in the opposite category*. And this is precisely what I have over and over again said: "No person denies the existence of the binary division, still less is the use of it to be despised." Dr. Fleming may therefore make what use of the above quotations from me he pleases. If he thinks I now deny the binary divisions there stated, he is mistaken. They are sound, they are solid, with the one exception I have stated; but really I did not think that they deserved to be set forth with such magnificent solemnity, or that the wonderful, thrice-wonderful conviction of the author of the *Horæ Entomologicæ*, as to "the existence of *only two* plans in the animal kingdom," required

* The expression "dichotomous tree" alludes here to that binary division of organized matter into animals and vegetables which is explained *Hor. Ent.* p. 199. It never was meant that the tree ought to be considered as naturally dichotomous *ad infinitum*, in order to understand its affinities and analogies.

the word *two* to be printed in italics. The little word *only* is, it may be observed, foisted in here with his usual honesty by Dr. Fleming; and as these four letters involve the whole of the question, I shall cite the passage from which this worthy quotes, at full length. "Animals have been divided into vertebrated and unvertebrated. Now this division, as M. Cuvier perceived, errs more in its nature, of which we have already exposed the defects, than in its particular relation to zoology. The objection to it is not that it is contrary to truth, but that it does not state enough, and that the young naturalist, placing full reliance on it, may be led to conceive that animals have been formed on only two distinct plans. Had the animal kingdom however been divided into *radiated* and *not radiated*, or into *annulose* and *not annulose*, both of these methods would have been equally applicable with that proposed by the celebrated author of the *Histoire Naturelle des Animaux sans Vertèbres*," (p. 208). I repeat that our D.D. is a most able Twist-text.

But animals are only vertebral and invertebral, says the Dominie; and the proof is, that you, Mr. MacLeay, are "convinced of the existence of only two plans in the animal kingdom, because you acknowledge that the *Vertebrata* are the perfection of one plan of organization, as the *Annulosa* may be of another." Now granting this to be a perfectly legitimate Scotch *sequitur;* why may not the two plans I am thus proved to be convinced of, be ANNULOSE and NOT ANNULOSE, as well as vertebral and invertebral? Let the Reverend Padre tell me why. To talk therefore of there being *only two* certain divisions in the animal kingdom, however convenient for the dichotomizing Minister of Flisk, is rank nonsense; when we can just as truly and as easily divide animals into asses and not asses.

The editor of the Quarterly indeed seems to have been bewildered by Dr. Fleming's positives and negatives, and the high-sounding title of "The Dichotomous System." Yet the secret of this wonderful discovery lies in the compass of a nutshell, as this editor will perhaps comprehend when he happens to consider, that if a law were passed extinguishing all quarterly publications, those that would remain would not be quarterly.

Discriminate objects this method manifestly does; but Dr. Fleming says, moreover, "that it accomplishes as much" in the way of exhibiting the affinities of objects, as one system can effect. He might with equal reason have said that it accomplishes more; for can any man in his senses honestly state his belief, that a system proceeding entirely and essentially on the plan of division can favour conjunctions at all? Why the very

very object of it is to break up natural affinities, and as far as it lies in the power of art to effect, to destroy them. Surely here is a naturalist, to use his own words, who has "confounded two objects which ought to be regarded as perfectly distinct,—the classification of organized beings so as to distinguish them, and their classification with the view of exhibiting their affinities."

In the "Philosophy of Zoology" it is said, that "the employment of the twofold method of division by positive and negative characters is so *easy* of application, that the reluctance which many naturalists seem to display in using it may well excite our surprise." The fact is, that the system is so trifling and easy, that it excites their contempt. In other words, to use a colloquial expression, it will not take. What therefore does the D.D. do; but now, perceiving that he was formerly on the wrong scent, and could not get his twos into notice by calling them *easy*, he adroitly changes ground, and, as a forlorn hope, states in the Quarterly Review, that "the labour of an author who pursues the dichotomous method is greatly *increased*." He might have remembered, that of old, whether he flogged the urchins of his parish-school high or low, he never could make his labours perfectly agreeable to them.

I shall now endeavour to precipitate a little of the mud that clouds the Doctor's cranium on another subject. It is, as we have seen, clear, that animals may be primarily divided into viviparous and not viviparous, oviparous and not oviparous, vertebrated and not vertebrated, winged and apterous, or, in short, in as many different ways as they present points of structure and habits. It is therefore equally clear, that it is a matter of mere option on our parts, to select any one of these modes of division,—such as, for instance, "viviparous and not viviparous;" and having so selected it, we find each of these two primary groups to be dichotomous again, in as many different ways as it presents points of structure and habits. Selecting again one of these new divisions, the same thing occurs, and so on. Dichotomous systems may therefore be considered as almost infinite in number; and, consequently, to talk of "*The Dichotomous System*" means, in fact, nothing else than *that one Dichotomous system* which Dr. Fleming has been pleased to decorate with Fliskian-Greek names. Our worthy apparently has some indistinct perception of this absurdity of his system; for, taking the bull boldly by the horns, he denies—and, be it remarked, this denial is the most *original* of all his cyclopædia or non-cyclopædia manœuvres,—the existence of a single natural method.

[To be continued.]

LXIII. *Observations on the Comet in Pegasus, April and May 1830, made at Geneva by* Mr. WARTMANN; *and its Elements calculated by* C. RUMKER, *Esq. M. Astr. Soc.**

THE following observations have been made of this comet, at Marseilles, Geneva, and Chougney, a villa near Geneva.

1830.	Morning.	Right Ascen.	Declin. N.	Place and Observer.	
April 21	$4^h 15^m$ A.M.	317° 27′ 0″	8° 37′ 0″	Marseilles.	Gambart.
22	3 50 24^s	317 33 30	9 30 18	Marseilles.	Gambart.
27	3 0 0	318 0 0	13 22 0	Geneva.	Wartmann.
30	2 0 0	318 20 0	15 15 0	Chougney.	Gautier.
May 3	1 30 0	318 35 0	17 45 0	Geneva.	Wartmann.
4	1 30 0	318 43 0	17 51 0	Geneva.	Wartmann.
5	1 30 0	318 51 0	17 56 0	Geneva.	Wartmann.
6	1 30 0	318 59 0	18 1 0	Geneva.	Wartmann.

The places of the comet given by Mr. Wartmann are only approximated ones, he not having reduced his observations as yet. The comet was visible to the naked eye, and appeared like a star of the fifth magnitude, with a nebulous and circular nucleus of from 9″ to 10″ diameter, and a tail of about 1½ degree. After the 10th April the comet diminished gradually in magnitude and splendour, although still visible to the naked eye. The nucleus measured but 4′ or 5′ diameter, and the tail only ¾ of a degree. On the 6th May the nucleus could no longer be distinguished, and barely some traces of the tail.

From the above observations I have deduced the following elements.

Transit over the perihelion, 14th April, at 8^h 30′ 54″ M. T. Greenwich.

Longitude of { Perihel. 215° 11′ 56″ upon the orbit.
Asc. Node 204 53 9.

Logarithm of perihelion distance 9·97·37·568.

Inclination of orbit 18° 39′ 44″.

Motion direct.

LXIV. *Letters from the President and Secretary of the Royal Society, in refutation of an alleged Inaccuracy in the Minutes of the Council*†.

To the President of the Royal Society.

Dear Sir,

IT having been publicly asserted by a Fellow of the Royal Society that a minute of the Council held on the 26th of

* Communicated by Mr. Rumker. † Communicated by Dr. Roget.

November

November last, preparatory to the anniversary meeting, was not correctly entered, as far as regards the recommendation of gentlemen proper to compose the Council for the ensuing year, and that, consequently, the minutes generally are unworthy of confidence, I cannot suffer such assertions to pass uncontradicted; and take this mode of appealing to you in confirmation of the correctness of that minute. You cannot but remember that at the meeting in question the eligibility of the several persons proposed by yourself was canvassed; that some of those actually nominated were suggested by other members of that Council; and that, in filling up the last vacancy, their choice lay between Captain Beaufort and Sir John Franklin; it being perfectly agreed upon that either the one or the other of these two gentlemen should be placed upon the list of those who were to be recommended for election at the anniversary. The sense of the Council having been so clearly and unequivocally manifested in favour of placing the name of Sir John Franklin on that list, in preference to any other except that of Captain Beaufort, it became my duty, upon being verbally informed by yourself that Captain Beaufort had declined the honour intended him, to make out the list as I did; that is, conformably to the views of the meeting, substituting for his name that of Sir John Franklin; more especially as you, at the same time, intimated to me that you did not consider it worth while (even had there been time, during the two days which intervened previous to the anniversary on the 30th, and one of which days was Sunday,) to go through all the formalities of issuing notices to all the members of the Council, and of holding another meeting, in order to ascertain what, upon this particular contingency, had already been sufficiently ascertained. I am warranted, therefore, in maintaining that the minute as it stands was substantially correct; and that in ordering it to be so entered I have on this, as on all other occasions, faithfully discharged the duty I owe to yourself and the Council, of representing the real sense of that body as expressed at their meetings.

That any minute of the proceeding was made at all, I acknowledge to be my own act, and I am willing to bear the whole responsibility attaching to it. I conceived it to be my duty to put upon record the result of the deliberations of the Council upon that occasion, which it seems had not hitherto been done. This reference to the Council of the selection of members to compose the future Council is, indeed, the very measure strongly recommended by Mr. Babbage in the scheme which he has proposed for reforming the Society.

As a check upon the accuracy of the minutes of each Council, they

they are always read at the following Council; and as a further precaution against the occurrence of mistakes, and against the improper use that might be made of them, I usually defer having them entered by the Assistant Secretary until just before the meeting at which they are to be read. This practice also is blamed by the very person who professes such extraordinary regard to the most punctilious accuracy, and who, in the eagerness of his zeal to discover faults, complains that the minutes of the Council of the 11th of February were not sooner entered. Had he chosen to take the slightest pains to inquire into the truth of the accusation before he made it, he might have learned that the simple reason why there exist no records of that Council, is that *no Council was held on that day*, but only a Committee, the minutes of which were duly entered in their proper place.

Such being the only instances which the author of these statements, after the immense time he has spent in ransacking the records of the Society, with an industry worthy of a better cause, for materials of accusation, has been able to adduce in support of his charge of inaccuracy in the minutes; and such being the data whence he draws the sweeping conclusion that the whole of the minutes are unworthy of the least confidence, and can never hereafter be appealed to as authentic documents,—may it not, with greater fairness, be inferred, now that their accuracy has been established in these very instances, that they are accurate throughout? His own arguments, indeed, might well be retorted upon himself with reference to his own statements: but, however strong the temptation, it would perhaps be ungenerous further to turn against himself the same weapons which he has been so little scrupulous in employing to impugn the conduct of the Officers of the Royal Society. I remain, Dear Sir,

Your most obedient and faithful Servant,

Bernard Street, Russell Square, May 21, 1830. P. M. Roget.

To Dr. Roget.

My dear Sir,

Having this morning seen your statement respecting certain minutes of the Council made previously to the last annual meeting of the Royal Society, in relation to the ensuing election, I bear very full testimony to its accuracy.

Believe me, my dear Sir,

Your's ever most faithfully,

Bridge Street, Westminster, May 22, 1830. Davies Gilbert.

LXV. *Pro-*

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE following are abstracts of papers which have been read before the Royal Society during the present session.

Feb. 4.—"Remarks on several Icebergs which have been met with in considerably low latitudes in the Southern Hemisphere." By Capt. Horsburgh, hydrographer to the East India Company.

The journals of the ships belonging to the East India Company, the author observes, during the whole of the last century, contain no accounts of icebergs having been seen in the course of their navigation in the southern hemisphere, although several of these ships proceeded into the parallels of latitude 40°, 41°, and 42°. But, during the last two years, it appears that icebergs have occasionally been met with by several ships in their passage, very near the Cape of Good Hope, between the latitudes of 36° and 39°. The particulars relating to these observations are detailed in the paper. The most remarkable occurred in the voyage of the brig Eliza from Antwerp, bound to Batavia, which on the 28th of April, 1828, fell in with five icebergs in latitude 37° 31′ S., longitude 18° 17′ E. of Greenwich. They had the appearance of church steeples, of a height from 250 to 300 feet; and the sea broke so violently against these enormous masses, that it was at first suspected they might be fixed on some unknown shoal, until, on sounding, no bottom could be discovered.

It is remarkable, that, in general, icebergs seem to be met with in low latitudes nearly at the same period of the year, namely, in April or May, in both the northern and southern hemispheres, although the seasons are reversed in these two divisions of the globe. In order to account for the origin and accretion of the southern icebergs, the author thinks it probable, that there exists a large tract of land near the antarctic circle, somewhere between the meridian of London and the 20th degree of east longitude, whence these icebergs have been carried in a N. and N.E. direction, by the united forces of current, winds, and waves, prevailing from S.S.W. and S.W. Bouvet's and Thompson's islands are not of sufficient magnitude; and Sandwich land and Kesgnelin's island are too remote to be the source of the icebergs lately observed in the vicinity of the Cape. From their unprecedented descent during the last two years, it is most probable that the disruption of these masses of ice from the places of their formation, was the effect of some powerful cause, of rare occurrence, such as an earthquake or volcano, which has burst forth and convulsed the inaccessible regions of the south, leaving no other testimonials of the event, than some few fragments of ice, scattered at a distance in the Indian Ocean.

March 4.—"On the progressive improvements made in the efficiency of steam-engines in Cornwall; with investigations of the methods best adapted for imparting great angular velocities." By Davies Gilbert, Esq.

The practical adaptation of the steam-engine to mechanical purposes is considered by the author as due to Mr. Newcomen, whose inquiries were

were introduced into Cornwall very early in the last century, and soon superseded the rude machinery which had, till then, been employed for raising water from the mines by the labour of men and of horses. The terms of Mr. Watt's patent in 1769, which secured to him, until the year 1800, the receipts of one-third of all the savings in fuel, resulting from the adoption of his improvements in the construction of the engine, rendered it necessary to institute an accurate comparison between the efficiency of his with former engines. A copy of the report drawn up on this occasion, in October 1778, is given in the paper; but as the dynamic unit of one pound avoirdupois, raised through a height of one foot, by the consumption of one bushel of coal, had not yet been established as the measure of efficiency, the author, proceeding upon the data furnished by that report, calculates that the duty performed by Watt's engine on that occasion was 7,037,800. In the year 1793 an account was taken of the work performed by seventeen engines on Mr.Watt's construction, then working in Cornwall. Their average duty was 19,569,000, which exceeds the performances of the former atmospheric engines in the standard experiment in the proportion of 2·78 to 1. Some years afterwards, disputes having arisen as to the real performance of Mr. Watt's engines, the matter was referred to five arbiters, of whom the author was one; and their report, dated in May 1798, is given, as far as relates to the duties of the engines. The general average of twenty-three engines was 17,671,000. Since that period, so great have been the improvements in the economy of fuel, and other parts of the machinery, that in December 1829, the duty of the best engine, with a cylinder of eighty inches, was 75,628,000, exceeding the duty performed in 1795 in the proportion of 3·865 to 1, and that of the atmospheric engine of 1778 in the proportion of 10·75 to 1.

The remainder of the paper relates to the friction in machinery, and the different modes of obviating its effects. With a view of reducing the amount of friction, the author is led to consider what are the most proper forms for the teeth and cogs of wheels, and through what intermediate steps a given increase of angular velocity may be most advantageously communicated. Equability of velocity is obtained, though at the expense of some degree of sliding friction, when the outline of the teeth of the wheels are involutes of circles. Friction, on the other hand, is wholly prevented when their form is logarithmetic spiral; but the angular velocities will then be variable. Hence these two advantages are incompatible with one another; but, on the whole, the author gives the preference to the involute, which produces an equability of angular motion. The most advantageous mode of increasing velocity by a series of wheels, is to adjust them so that the multiplication of velocity shall proceed in a geometrical progression.

April 1.—"Statement of the principal circumstances respecting the united Siamese Twins, now exhibiting in London." By George Buckley Bolton, Esq. member of the Royal College of Surgeons. Communicated by the President.

The twin brothers, of whom an account is given in this paper, were born of Chinese parents, in 1811, at a small village in Siam, distant about

about sixty miles from Bankok, the capital of the kingdom. When the intelligence of their birth reached the ears of the King of Siam, he gave orders that they should be destroyed, as portending evil to his government; but he changed his intention, and suffered them to live, on being assured that they were harmless, and would be capable of supporting themselves by their own labour. About six years ago, Mr. Robert Hunter, a British merchant resident at Siam, saw them for the first time in a fishing-boat on the river, in the dusk of the evening, and mistook them for some strange animal. It was only in the spring of last year that permission could be obtained from the Siamese Government to bring them to England. They were taken to Boston in the United States, where they landed in August last, and six weeks afterwards embarked for England, and arrived in London in November.

They are both of the same height, namely, five feet two inches; and their united weight is 180lbs. They have not the broad and flat forehead so characteristic of the Chinese race; but they resemble the lower class of the people of Canton in the colour of their skins and the form of their features. Their bodies and limbs are well made. The band of union is formed by the prolongation and junction of the ensiform cartilages of each, which meet in the middle of the upper part of the band, and form movable joints with each other, connected by ligamentous structures. Underneath the cartilages there appear to be large hernial sacs opening into each abdomen; into which, on coughing, portions of the intestine are propelled as far as the middle of the band; though, in ordinary circumstances, these herniæ are not apparent. The entire band is covered with common integument; and when the boys face each other, its length at the apex is one inch and three quarters, and at the lower edge not quite three inches. Its breadth from above downwards is four inches, and its greatest thickness nearly two inches. In the centre of the lower edge there is a cicatrix of a single navel. It possesses little sensibility, and is of great strength; for upon a rope being fastened to it, the twins may be pulled along without occasioning pain; and when one of them is lifted from the ground, the other will hang by the band alone, without sensible inconvenience. For the space of about half an inch from the medial line of the band, the sensibility of the skin appears to be common to both. The following experiment was tried upon them by Dr. Roget. A silver tea-spoon being placed on the tongue of one of the twins, and a disk of zinc on the tongue of the other, the moment the two metals were brought into contact, both the boys exclaimed "*Sour, sour*:" thus proving that the galvanic influence passed from the one to the other through the connecting band. Another simple but clever experiment (which we need not detail, as all philosophical inquiries are not fit for publication) proved that the sanguineous inter-communion was not common to the two.

Their strength and activity are very remarkable. They can throw down with perfect ease a powerful man. They run with great swiftness, bend their bodies in all directions, and in their sports often tumble head over heels without the least difficulty or inconvenience. In all

all the bodily actions in which the concurrence of both is required, such as running, jumping, playing at battledore and shuttlecock, they exhibit a wonderful consent, or agreement, without the appearance of any previous communication of their intentions. The intellectual powers of each are nearly equal; and they have both attained the same degree of proficiency in the games of chess, draughts, and whist. They both possess great powers of imitation. In their respective physical constitutions, however, several differences are observable. Chang, as the boy on the left is named, has more vigorous health, and greater regularity of functions, than his brother, whose name is Eng. In general they take their meals, and obey the calls of nature, at the same time. The author details the circumstances of a catarrhal complaint which attacked both of them in December last, the symptoms and progress of which were similar in each; and from which they both recovered in the same manner and at the same time. In their healthy state their pulses are generally alike, and are easily excited; but that of the one may be accelerated, while that of his brother continues calm.

In their habits they are very cleanly and delicate; in their dispositions affectionate and grateful for every kindness shown to them. There exists between them the most perfect harmony. They always fall asleep at the same moment; and it is impossible to wake the one without also waking the other. The author adverts in the course of the paper to the question, whether they were the produce of a single or double ovum, and also to the possibility at some future time of effecting their separation with safety to themselves; and he concludes, by bearing testimony to the uniformly kind treatment they have received from Captain Coffin, Mr. Hunter, and Mr. Hall, who have evinced on all occasions the greatest anxiety for their welfare and happiness; and to the liberal manner in which they have always afforded access to men of science, for promoting any object of philosophical inquiry*.

GEOLOGICAL SOCIETY.

March 19th.—Henry Rowland Brandreth, Esq. of the Royal Engineers, Woolwich; Sir Thomas Phillips, Bart. of Middle Hill, Worcestershire; and Robert Alfred Cloyne Austen, Esq. of Lincoln's Inn,—were elected Fellows of this Society.

Extracts were read from a paper entitled "Reference to a Geological Map and Section of Pembrokeshire," by Alfred Thomas, Esq., Mineral Surveyor, Haverfordwest.

The author accompanies the map and section with geological and economical remarks. The map comprehends all that northern part of Pembrokeshire not described by Mr. De la Beche, and the section is drawn from St. Gowan's Head on the south to Cardigan on the north. The alternations of the different formations in the county are detailed in a series of descriptive sections: the chief masses are coal measures, including culm and coal grits, mountain limestone, old red sandstone and conglomerate, transition

sition limestone, grauwacke, grauwacke slate. All these, in the central and southern parts of the county, are traversed by, or alternate with trap rocks which are of various kinds, some being syenitic, others hornblendic and amygdaloidal, whilst near Fishguard they are columnar and basaltic. The beds of the stratified deposits are frequently contorted, and their nature altered in contact with the intrusive rocks. The transition limestone contains trilobites.

The first of two letters addressed to R. I. Murchison, Esq., Sec. G.S. F.R.S. &c. "On the Lacustrine Basins of Baza and Alhama in the province of Granada, and similar deposits in other parts of Spain," by Col. Charles Silvertop, F.G.S., was then read.

The Sierra Nevada, rising to the height of 11,000 and 12,000 feet above the sea, is the culminating point of a number of subordinate mountain groups which form a lofty chain stretching from Andalusia on the W.S.W. to Murcia on the E.N.E. and bisecting in its range the kingdom of Granada.

This chain is composed of a central axis of gneiss and mica schist, with successively overlying zones on each flank of transition and secondary rocks, which on the south and along the shores of the Mediterranean are, here and there, covered with patches of tertiary marine deposits containing Sub-Apennine shells; whilst on the northern flank of the chain, or towards the interior of Spain, the secondary rocks are succeeded by formations of lacustrine origin, which in the kingdom of Granada occupy two large and separate basins, one near Baza, the other near Alhama. These great and elevated depressions in the secondary rocks, though at little distances from the Mediterranean, are so cut off from that sea by the Sierra Nevada, that their drainage is effected in a north-westerly direction, into the Guadalquivir, and thence into the more distant Atlantic. The author describes in detail the basin of Baza, which, traversed by an insignificant stream called the Rio Baza, is surrounded upon three of its sides by a secondary nummulite-limestone; the precise age of which he does not pretend to determine, although he states that it very much resembles certain varieties of the younger Alpine limestone.

Unconformably deposited on this and other older rocks, within a district the average diameter of which is about thirty-five miles, there are spread out formations of considerable thickness, the organic remains of which are exclusively lacustrine and tertiary. These in the immediate neighbourhood of Baza are divided into two principal groups; the lowest, consisting of marls with laminated gypsum, sulphur and brine springs, is zoologically distinguished by the presence of cypris; the uppermost is a compact, cream-coloured limestone, charged with many small Paludinæ of a species identical with one which is found in the lacustrine formations of Central France. The united thickness of these freshwater groups in the neighbourhood of Baza cannot be estimated at less than 300 and 400 feet; they are generally horizontal, but the face of the country everywhere exhibits striking proofs of immense degradation,

degradation, the gypsiferous marls being denuded throughout the greater part of the centre of the basin, and but rarely exhibiting caps of the compact paludina-limestone. On the southern, eastern and south-western flanks of the basin, particularly near Gaudix, there are vast accumulations of pebble beds, conglomerate, &c., the exact relations of which to the marls and limestone the author could not satisfactorily determine, owing to the obscurity of the sections; although he is of opinion that there are conglomerates which in some places pass under the marls, whilst in others they are decidedly overlying.

The reading of the letter on the Basin of Alhama was deferred until another evening.

April 2nd.—William Hallows Miller, Esq., M.A., of St. John's College, Cambridge; Lloyd Baker, jun., Esq., of Hardwick, Gloucestershire; William Granville Eliot, Esq., Lieut.-Col. late of the Royal Engineers, Hastings; Rev. Henry Engleheart, of Caius College Cambridge, and Seal, Kent; Josias Lambert, Esq., of Liverpool Street, London; and Thomas Morgan, Esq., of Thames Ditton, Middlesex,—were elected Fellows of this Society.

The reading of a paper on the Geology of Weymouth, and the adjacent parts of the coast of Dorsetshire, by the Rev. William Buckland, D.D., F.G.S., F.R.S. &c., and Henry Thomas de la Beche, Esq., F.G.S., F.R.S. &c., was begun.

April 16th.—John Rennie, Esq., of 15, Whitehall Place; George Rennie, Esq., of 21, Whitehall Place; Alfred Thomas, Esq., of Haverfordwest, Pembrokeshire; Charles Mundy, jun., Esq., of Burton Hall, Loughborough; and Alexander Turnbull Christie, M.D., of the East India Medical Service,—were elected Fellows of this Society.

The reading of a paper on the Geology of Weymouth, and the adjacent parts of the coast of Dorsetshire, by the Rev. Dr. Buckland, and Henry Thomas De la Beche, Esq., begun at the last Meeting, was concluded.

The Authors take up the history of the Geology of the Coast of Dorset at the point where Mr. Webster terminates, viz. at the chalky promontory of White Nore, about eight miles E.N.E. of Weymouth, and continue their account of the coast thence westwards to the lias at Charmouth. The Memoir is accompanied by a map and many sections both of the cliffs and of the adjacent inland district, including the space intermediate between the escarpment of the chalk downs of Dorsetshire and the sea. The authors divide this district into two compartments, viz. the Vale of Weymouth and the Vale of Bredy.

The structure of the Vale of Bredy is comparatively simple, being chiefly composed of chalk, greensand, Kimmeridge clay, Oxford oolite, forest marble, and inferior oolite, dipping for the most part to the E. and N.E. and divided by thick beds of clay.

The Valley of Weymouth is more complicated, comprehending tertiary strata, chalk, greensand, Purbeck and Portland beds, Kimmeridge

meridge sand and clay, Oxford oolite, Oxford clay, cornbrash and forest marble. To the forest marble belong the lowest strata that form the axis of this district. Nearly all these strata are highly inclined, and dip respectively in two opposite directions from an anticlinal line which runs through a saddle of forest marble from E. to W.

The uppermost of these strata on the N. side constitute the chalk escarpment of the ridgeway, capped with patches of plastic clay; whilst on the S. no strata appear above the sea more recent than those which form the Isle of Portland.

Between the ridgeway-chalk-escarpment and the Isle of Portland, the strata are disposed in a succession of long and narrow belts of clay and stone, the clay constituting valleys, and the stone rising into ridges between the valleys; all these belts are terminated eastward by the bay of Weymouth, and westward by the Chesil Bank.

The formations composing this district are described in the following order.

1. Plastic clay and sands, with blocks of puddingstone, and beds of angular flints forming a breccia in place, occur on the surface of the chalk.

2. Chalk presenting no remarkable peculiarities.

3. Greensand formation exhibiting no distinct traces of gault. The Wealden formation terminates a little W. of Lulworth Cove.

4. Purbeck beds appearing in two long insulated patches at Osmington and Upway.

5. Portland stone occurring not only throughout the island of that name, but forming a high and narrow ridge parallel and immediately subjacent to the escarpment of the chalk along nearly the whole north frontier of the Vale of Weymouth.

6. Between the Purbeck and Portland formations there is a very remarkable bed of black earth called the "Dirt Bed," already described by Mr. Webster as being mixed with slightly rolled pebbles of Portland stone*, and containing, in a silicified state, long prostrate trunks of coniferous trees and stems of Cycadeoideæ. These trunks lie, partly sunk into the black earth, like fallen trees on the surface of a peat bog, and partly covered by the incumbent limestone. Many stumps of trees also remain erect, with their roots attached to the black soil in which they grew, and their upper part in the limestone; and show that the surface of the subjacent Portland stone was for some time dry land, and covered with a forest, and probably in a climate such as admits the growth of the modern Zamia and Cycas. This forest has been submerged; first beneath the fresh waters of a lake or estuary, in which were deposited the Purbeck beds and sands and clays of the Wealden formation, (amounting together to nearly 1000 feet), and subsequently beneath the salt water of an ocean of sufficient depth to accumulate all the great marine formations of greensand and chalk.

* Geol. Trans., Second Series, vol. ii. p. 42.

7. Below

7. Below the Portland stone, and dividing it from the Kimmeridge clay, the authors establish a deposit, hitherto unnoticed, of sand and sandstone 80 feet thick, which they call the Kimmeridge sandstone; it is full of grains of green earth, and scarcely distinguishable, except by its fossils, from the greensands immediately below the chalk: they also have ascertained that the pseudo-volcano still burning on the north of Weymouth is in the bituminous beds of the Kimmeridge clay, and that there has been at some unknown former period a similar combustion of the same clay on the shore near Portland ferry.

8. The Oxford oolite is very fully developed near Weymouth, as it is near Scarborough, passing into beds of sand, sandstone, and clay at its upper and lower extremities; containing Ostrea deltoidea in the upper, and Gryphæa dilatata in the lower beds; and gradually passing into Kimmeridge clay above, and into Oxford clay below: its thickness exceeds 150 feet. The history and character of this oolite formation at Weymouth have been fully described in all their details, and accompanied by a valuable list of its fossils, in a paper on the strata of the Yorkshire coast, by Professor Sedgwick; Ann. Phil., May 1826.

9. The Oxford clay is about 300 feet thick, and contains large septaria, which are cut into beautiful tables, under the name of Turtle Marble. This clay abounds throughout with shells of Gryphæa dilatata.

10. The cornbrash and forest marble form the axis of the Valley of Weymouth, and occupy much of the Valley of Bredy. The forest marble formation abounds in beds of clay, and is often composed of clay without the marble. The Bradford Encrinite (Apiocrinites rotundus) is found in several parts of it, e. g. at Abbotsbury, at Bothenhampton, and in the cliff west of Bridport Harbour.

11. There is no Bath oolite stone in Dorsetshire, but the inferior oolite occupies a large extent near Bridport, affording coarse limestone, like that of Dundry in its upper, and micaceous sand with beds and concretions of calcareous sandstone in its lower part. Its total thickness is about 300 feet. Near its middle region are masses of breccia, containing slightly rolled fragments of the lower strata, and having the entire circumference of these fragments drilled all over by some small lithodomous shells; these fragments attest the consolidation of the lower strata before the deposition of the central beds, and mark an interval in the formation sufficient for the fragments to have been rounded and perforated.

12. The lowest strata, within the district described, are the upper marl beds of the lias formation on the east of Charmouth; these are loaded with belemnites, and may represent the *Calcaire à Belemnite* of the French geologists; as the lower stony beds of lias at Lyme are equivalent to their *Calcaire à Gryphite.* On the shore east of Charmouth the marl beds present an almost continuous pavement of belemnites, and also contain saurians.

13. The elevation which has raised all the component formations of the Valley of Weymouth towards an anticlinal axis, has been accompanied

companied by extensive faults, the most remarkable of which are parallel to the anticlinal axis, and appear to have been contemporaneous with the general elevation of the district. One of these faults is continuous nearly 15 miles along the escarpment of the chalk of the ridgeway, on the north of Weymouth, and at various places brings up strata of oolite, Portland stone, and Purbeck stone into contact with chalk and greensand; many sections are given illustrating the effects of these faults, not one of which appears to be anterior to the deposition of the most recent strata in the district.

14. Subsequently to, or perhaps contemporaneously with the elevation of the strata and production of the faults, the surface has been ravaged by a tremendous inundation which has swept away all the ruins and rubbish of the elevated masses, and has excavated valleys of many hundred feet in depth on the surface of the strata that remain. Outlying summits, composed of residuary portions of strata which are continuous along the escarpments on the north and east of the Vale of Bredy, indicate the original continuity of these strata over large portions of that district, from which they have been removed.

15. Small deposits of diluvium are scattered over many of the hills as well as the valleys, but there are no very thick and connected accumulations of gravel; the force of the water that could produce such enormous excavations must have been far too great to allow the excavated materials to subside so near the rocks from which they were torn, and must have drifted them far away into the continuation of these valleys, in the bottom of the English Channel.

The authors conclude that they have sufficient evidence to establish the following succession of changes, in the state of that small portion of England which occupies the coast of Dorsetshire and Hants.

1st. There is a continuous succession of marine deposits from the lias upwards through the oolites, terminating in the deposition of the Portland stone:—during the period of all these formations the district must have been the bottom of an ancient sea.

2ndly. Some part of the bottom of this sea appears for a certain time to have become dry land, and whilst in that state, to have been covered with a forest of large coniferous trees and cycadeoideous plants which indicate a warm climate. We have a measure of the duration of this forest in the black earth which is accumulated to the thickness of more than a foot from the wreck of its vegetation: the regular and uniform preservation of this thin bed of black earth over a distance of many miles, shows that the change to the next state of things was not accompanied by any violent denudation or rush of waters, since the trees that lie prostrate on this black earth would have been swept away by any such violent catastrophe. Dr. Buckland has found this same black earth on the surface of the Portland stone near Thame in Oxfordshire. It has also been found by Dr. Fitton in the Boulonnois.

3rdly. The dry land on which this forest grew, in Dorsetshire, became converted to something like an estuary, in which the lowest deposits contain freshwater shells, succeeded by a thick bed of oyster shells; and above the oyster bed, by strata containing an admixture of freshwater shells with shells that are marine. This freshwater formation,including both the Purbeck and the Wealden strata, extends with certain interruptions from Upway on the N. of Weymouth to the E. extremity of Purbeck, and reappears in the Isle of Wight and the Weald of Sussex and Kent; but of the boundaries of the estuary or estuaries in which these fresh-water strata were deposited we have no indications beyond those afforded by the area of the strata themselves. Its breadth probably extended about 30 miles from Purbeck to Tisbury on the west of Salisbury, across the intermediate portion of Dorset and Wilts, which is now covered up with chalk.

4thly. We have a return of the sea over the estuary, and in this sea an accumulation of the successive and thick marine deposits which constitute the greensand and chalk formations.

5thly. Although no fresh-water formations occur in the tertiary strata above the chalk on the coast of Dorset, we have on the adjacent coast of Hants and the Isle of Wight, a re-appearance of fresh-water deposits above the chalk, mixed and alternating with others that are marine.

6thly. All these deposits appear to have been succeeded by powerful convulsions, producing elevation and depression of the strata, intersecting them with tremendous faults, and followed by an inundation competent to excavate deep valleys of denudation, and to overspread the country with diluvial gravel.

7thly. This inundation has been succeeded by a state of tranquillity, which has continued to the present hour.

A paper entitled "Description of a New Species of Ichthyosaurus," by Daniel Sharpe, Esq., F.G.S., was then read.

This specimen of Ichthyosaurus was found in a quarry of lias limestone about four miles from Stratford-upon-Avon. The whole length of the animal must probably have been about seven feet; the parts of it which remain exhibit the upper portion of the head from the nostrils backwards, in a very crushed state, a continuous series of 52 vertebræ, from the atlas to the commencement of the tail, with nearly all the spinous processes; one scapula, and nearly the whole of one fore paddle. The teeth (by which the four species formerly described have been chiefly distinguished) are entirely wanting in this individual; the author, however, considers it to be a new species, from the following peculiarities of character.

1. The length of each vertebra is uniformly three-fifths of its breadth, a proportion not found to exist in any hitherto described species.

2. The paddle is of great size, and including the humerus must have been equal to one-fifth of the length of the whole animal. In the ulna or radius (it is difficult to say which) there is a notch on

on the outer edge, and all the other bones of the paddle are very nearly circular or oval; thus differing essentially from the angular shaped phalanges of I. communis, tenuirostris, and intermedius.

On account of the large size of its paddle, the author names this species "Ichthyosaurus grandipes."

LINNÆAN SOCIETY.

At the meeting on the 4th of May, A. B. Lambert, Esq. V.P. in the chair, there was read "An Examination of M. Virey's Observations on Aëronautic Spiders," published in the *Bulletin des Sciences Naturelles.*

May 24.—This day, being the Anniversary, the following Officers and Council were elected for the ensuing year.

President: Edward Lord Stanley, M.P.—*Vice-Presidents:* A. B. Lambert, Esq. F.R.S.; W. G. Maton, M.D. F.R.S.; E. Forster, Esq. F.R.S., and R. Brown, Esq. F.R.S.—*Treasurer:* Edward Forster, Esq. F.R.S. — *Secretary:* J. E. Bicheno, Esq. F.R.S. *Assistant Secretary:* Richard Taylor, Esq. F.S.A.—Also to fill the five vacancies in the Council: George Bentham, Esq.; John, Earl Brownlow, F.R.S.; Rev. W. Buckland, D.D. F.R.S.; Charles Stokes, Esq. F.R.S.; Wm. Yarrell, Esq. Many of the members afterwards dined together at the Freemasons Tavern, Lord Stanley in the chair.

ASTRONOMICAL SOCIETY.

March 12, 1830.— The following communications were read :—

1. Extract from a letter of Professor Harding to Dr. Tiarks, dated Gottingen, December 19, 1829.

"I observed the occultation of Aldebaran on the 9th of December, and the sky being perfectly clear, I obtained the moments of immersion and emersion with extreme precision. The first creeping out of the star at the emersion was easily perceived, by the contrast of its red light with the white light of the moon's limb; and I think that the observation of the emersion is as accurate as that of the immersion. I devoted all my attention to the observation, with a view to the well-known phænomenon of the star's remaining visible some seconds on the moon's disc, which has been repeatedly observed in this star, and which I have myself seen several times. I remained, at the immersion, as well as at the emersion, 10 seconds before the telescope without turning my eye off, but nothing of the kind was seen."

Occultation of α Tauri, Gottingen, Dec. 9, 1829.

Immersion	$23^h\ 37^m\ 33^s\!\cdot\!3$	Sidereal time.
Emersion	$0\ \ 37\ \ 58\ \cdot3$	

2. "On Mr. Pond's recent catalogue of 720 principal stars." By Francis Baily, Esq.

3. "On the method of computing the longitude from an observed occultation of a fixed star by the moon." By Edward Riddle, Esq.

 LXVI. *In-*

LXVI. *Intelligence and Miscellaneous Articles.*

SPECIFIC HEAT OF ELASTIC FLUIDS.

THIS subject, which has been under investigation at various times by MM. De la Roche and Berard, Haycraft, De la Rive and Marcet, has been taken up by M. Dulong, who has applied to it a new method of investigation dependent upon the velocity of sound in the different gases. La Place showed that the velocity of the sound in air or other elastic media was importantly influenced and increased above the expected velocity by the heat evolved, as the vibrations producing sound passed through the air; and M. Dulong, by examining and comparing the sounds produced by different gases, has endeavoured to ascertain whether this element is the same in all of them. He arrives at this general law, remarkable for its simplicity, 1. That equal volumes of all elastic fluids taken at the same temperature and pressure, when compressed or expanded by a fraction of their volume, disengage or absorb the same *absolute quantity of heat.* 2. That the variations of *temperature* which result, are in the inverse ratio of the specific heat of a *constant volume.*—*Annales de Chimie,* tom. xli. p. 113. *Royal Instit. Journal.*

EXPERIMENTS ON GOLD. BY DR. THOMSON.

Berzelius and Javal have determined from their experiments that peroxide of gold contains 3 atoms of oxygen. To ascertain the correctness of this statement, Dr. Thomson dissolved 25 grains of gold in nitro-muriatic acid, and to the solution was added one of 104·25 grains of crystallized sulphate of iron; the distilled water was boiled for half an hour before it was used, and the protosulphate of iron crystals were thrown into the boiling hot liquid, which was added to the solution of gold as quickly as possible: the gold obtained weighed 24·9 grains; so that the loss was only 0·1 grain.

As six times 4·5 grains of protoxide of iron which the 104·25 of protosulphate contained were converted into peroxide, by uniting with 3 grains of oxygen, Dr. Thomson considers it as demonstrated that peroxide of gold is composed of

1 atom of gold	25
3 atoms of oxygen........	3
	28

Dr. Thomson, from the facts stated in his "*Attempt,*" vol. i. p. 440 considered it as extremely probable that muriate of gold is a compound of 2 atoms muriatic acid, and 1 atom peroxide of gold. To determine this by experiment, a solution of gold in nitro-muriatic acid was evaporated to dryness, and 24·2 grains of the metal yielded 42 grains of dry muriate.

To determine the quantity of muriatic acid, it was necessary first to get rid of the gold; for nitrate of silver being dropt into the undecomposed salt, both the gold and muriatic acid precipitate along with the silver; the gold was therefore thrown down by copper, the copper by potash, and the excess of potash was neutralized by nitric acid, and then

then the muriatic acid precipitated by silver: the chloride of silver weighed 34·65 grains, equivalent to 8·453 grains of chlorine, or 8·78 grains of muriatic acid. "Thus it appears," says Dr. Thomson, "that 24·2 grains of gold, in the state of peroxide, had been combined with 8·87 grains of muriatic acid. Consequently 25 grains of gold in the state of peroxide must be united with 9·11 grains of muriatic acid. This is only 0·14 grain less than 9·25, the equivalent for 2 atoms of muriatic acid. From this result it is obvious, that muriate of gold is a compound of 2 atoms muriatic acid, and 1 atom peroxide of gold: the weight of the dry salt having been 42·8 grains, it is clear that it must have contained 5 atoms of water, and that muriate of gold is composed as follows:

2 atoms muriatic acid........	9·25
1 atom peroxide of gold......	28·
5 atoms water..............	5·625
	42·875

"The precipitation of the gold by protosulphate of iron seems to show, that the gold in this salt is in the state of oxide, and consequently combined, not with chlorine, but with muriatic acid. It is equally clear that, in the sodium chloride of gold, the metal is not oxidized but in the metallic state, and united to chlorine. Hence the reason why it is so difficult to reduce the gold from the sodium chloride by heat, while it is so easy, by a very moderate heat, to reduce the gold from the muriate.

"Gold furnishes a striking example of the want of coincidence in the proportions of oxygen and chlorine, which unite with bodies, and of the danger of being misled, when we infer the composition of a chloride from that of an oxide. The peroxide of gold, containing 3 atoms of oxygen, one would have been disposed to infer, that the chloride would also contain 3 atoms of chlorine; yet it contains only 2 atoms. This want of coincidence between the peroxide and chloride of gold, is probably the reason why the muriate of gold cannot be converted into a chloride by heat; at least all my attempts to obtain a chloride by this process have ended in disappointment. In what manner the change takes place in the atomic proportions, when common salt is added to the muriate, it is not easy to conceive; but the experiments which I have related in this paper, and in my "*Attempt*," leave, I conceive, no doubt that the conversion from muriate to chloride actually takes place."—*Trans. Royal Soc. Edinburgh*, vol. xi. p. 23.

SUPPOSED NEW VEGETO-ALKALI—CHINIOÏDIA.

In the last Number but one of the Phil. Mag. we noticed Dr. Serturner's supposed discovery of a new vegeto-alkali, to which he gave the name of Chinioïdia. MM. Henry, jun. and Delondre have made numerous experiments to determine whether such an alkali really exists, and they have arrived at the following conclusions:—

1st. That there remains little doubt as to the non-existence of chinioïdia, and that it appears demonstrated that it is only a modification of quina and cinchonia combined and rendered uncrystallizable by a peculiar yellow matter. These modifications cease, when after a long time

time and much care it is either separated or destroyed and crystallization takes place.

2nd. That the yellow resinous matter which accompanies quina more than cinchonia, appears to change its properties much; this yellow matter the authors succeeded in destroying, but without being able to collect it separately in a perfect state. It appears to differ from the yellow colouring matter of the bark, which is fixed by alumina, oxide of lead and of tin.

3rd. That this yellow matter especially influences the crystallizations.

4th. That the most certain method of clearing the mother waters from it, are the addition of turpentine, repeated precipitation and solutions in the acids, and concentration by cold.

The experiments were made by MM. Henry and Delondre, and always with the same results; they operated upon the mother waters remaining after the treatment about two hundred thousand pounds of yellow bark, and they always separated from this suspected matter the portion of quina and cinchonia, the crystallization of which it had prevented.—*Journal de Pharmacie,* March 1830.

CHEMICAL ACTION OF LIGHT.

The following facts are cited by M. Fischer, as proper to be added to those which demonstrate the chemical action of light upon organic matter. If a solution of ferro-prussiate of potash be precipitated by alcohol, and the precipitate be quickly collected and dissolved in water, the solution, exposed to light, will pass rapidly from yellow to green, and at length prussian blue will be deposited. The solution becomes at the same time alkaline; and if the experiment be made in a close vessel, on examining the liquid, the odour of hydrocyanic acid will be perceived. The salt is indeed partly decomposed in this action. Prussian blue, sometimes with excess of oxide of iron, is formed and deposited, and the hydrocyanate of potash remains in solution. The same modifications may take place in a common solution of ferro-prussiate, but time is in that case necessary. It cannot take place without the presence of light. In darkness this salt (the ferro-prussiate of potash) crystallizes in large quadrangular plates; but exposed to a vivid light, it loses gradually the power of assuming this figure, and becomes pulverulent, and is deposited in dendritic forms.

The presence of organic matters, such as gum, starch, sugar, alcohol, &c. greatly increases the action of light in solutions of gold and silver. —*Bib. Univ., June* 1829.

DR. WEBSTER'S DICTIONARY.

The very valuable Dictionary of Dr. Webster lately published in America, and now reprinting with additions in this country, will at some future time probably claim a fuller notice as an important contribution to the philosophy of language. From the three Parts which have appeared, it will be seen that, among other recommendations, it contains, what cannot be found in other English Dictionaries, an explanation of Terms in the various branches of Science and Art adapted to their state at the present period.

LIST OF NEW PATENTS.

To G. Stocker and A. Stocker, both of the parish of Yeovil, Somerset, gunsmiths, for a cock for drawing liquor from casks, which produces a stop superior to that which is effected by common cocks, and will continue in use for a longer period of time.—Dated the 26th of January 1830.—2 months allowed to enrol specification.

To J. Arnold, of Sheffield, Yorkshire, powder-flask maker, for an improved spring latch or fastening for doors.—26th of January.—2 months.

To G. F. Johnson, of Canterbury, Kent, Tunbridge-ware manufacturer, for a machine or apparatus, which is intended as a substitute for drags for carriage wheels, and other purposes.—26th of January.—6 months.

To T. Bulkeley, of Richmond, Surrey, doctor of physic, for a method of making or manufacturing candles.—26th of January.—6 months.

To J. Cobbing, of Bury St. Edmund's, cordwainer, for certain improvements on skaits.—26th of January.—6 months.

To S. Weight, of Shelton, Staffordshire potteries, for a manufacture of ornamental tiles, bricks, and quarries, for floors, pavements, and other purposes.—26th of January.—6 months.

To R. Busk, of Leeds, Yorkshire, gentleman, for certain improvements in apparatus used for distilling and rectifying. Communicated by a foreigner.—26th of January.—6 months.

To J. Revere, of New York, in the United States of America, now residing in the parish of St. James, Westminster, M.D., for a new alloy or compound metal applicable to the sheathing of ships, and various other useful purposes.—28th of January.—6 months.

To J. Lambert, of Liverpool-street, London, esquire, for an improvement in the process of making iron applicable at the smelting of the ore, and at various subsequent stages of the process up to the completion of the rods or bars, and a new process for the improving of the quality of inferior iron.—4th of February.—2 months.

To G. Pocock, of Bristol, gentleman, for improvements in making or constructing globes for astronomical, geographical, and other purposes.—4th of February.—2 months.

To J. Gray, of Beaumorris, Anglesea, gentleman, for a new and improved method of preparing and putting on copper sheathing for shipping.—4th of February.—2 months.

To C. T. Miller, of Piccadilly, Middlesex, wax-chandler, for certain improvements in making or manufacturing of candles.—4th of February.—6 months.

To J. C. Daniell, of Limphey Stoke, in the parish of Bradford, Wilts, clothier, for certain improvements in the machinery applicable to the manufacturing of woollen cloths.—6th of February.—6 months.

AURORA BOREALIS.

In the evening of the 19th, soon after sunset, as bright a light appeared in the horizon about the magnetic north as the crepuscule immediately

immediately above the sun; and as the twilight withdrew, the aurora increased in brightness. At 9 P.M. it showed a steady flame colour, and was comprised between the N.W. and N. by E. points of the horizon, and about nine or ten degrees in altitude.

At 11 o'clock a vertical line of light, whose bearing was N. by E. $\frac{3}{4}$ E. emanated from the aurora, and in a few minutes afterwards other coruscations emanated from it between the N.W. and N., but they often disappeared, and rose again to an altitude exceeding that of ε Cassiopeiæ. At a quarter before 12, seven columns of light of various widths appeared at once, and continued several minutes: the wind blowing fresh from the westward seemed to give them a slight inclination from a perpendicular towards the East, and they did not finally disappear till between one and two A.M. In the course of the evening several bright meteors descended from over the aurora, and in a few hours afterwards a heavy gale came on from the S.W. and continued nearly four days. This meteoric phænomenon was also seen in Scotland, but from the interposition of clouds it did not display any vertical columns there, only faint coruscations in the horizon. Whatever gaseous quality an aurora borealis may possess, whether hydrogenous, electric, or magnetic, or a mixture of any of these, here it is very generally, if not the cause, a prognostic of a strong gale of wind from some quarter.

ON THE DIFFERENCE IN THE HEIGHT OF SPRING TIDES.

The first, second and third tides after the new moon on the 24th of April were considerably higher in Portsmouth Harbour than the first three tides after the new moon on the 24th of March; yet the new moon in March was nearer to the earth's equator than the new moon in April, and of course her attraction of the water was greater in the former month: the sun in March was also nearer the earth than he was in April, and his attraction proportionably greater. The moon's horizontal parallaxes in the Nautical Almanac at the time of these new moons, are the same within *one second,* and the greatest for the year till the last day of October; yet the difference in the height of the spring tides at these times was fifteen inches greater immediately after the last new moon. It would be difficult under these nearly coinciding circumstances to account for this unusual swell of the tides, without referring to, and taking into consideration the state of the weather, and the position and strength of the wind which influenced it. In March only three-fifths of rain fell here; and the evaporation was nearly as great as that of the present month, and the weather remarkably calm. In April between three and four inches of rain fell, and a S.W. gale from over the Atlantic blew strong two days before and two days after the last new moon, which in connection with this depth of rain, must have caused the swell and comparative difference in the last spring tides on our shores. A remarkably low ebbing of the tide, six feet lower than is usual at the same age of the moon, occurred here the third day after the new moon in March.

OBSERVA-

OBSERVATIONS ON THE PLANET VENUS IN THE DAY-TIME.

1830.	True Mer. Alt. of Venus.	App. Dist. of Venus from the Sun's centre.
Feb. 5th.	40° 24′	37° 1′ 15
10th.	41 48	33 21 15
18th.	43 19	25 55 10
25th.	43 36	———*

METEOROLOGICAL OBSERVATIONS FOR APRIL 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·26. April 26. Wind S.W.—Min. 29·26. April 3. Wind N.W.
Range of the mercury 1·00.
Mean barometrical pressure for the month .... 29·807
Spaces described by the rising and falling of the mercury........... 6·680
Greatest variation in 24 hours 0·840.—Number of changes 20.
Therm. Max. 66°. April 29. Wind E.—Min. 28°. April 4. Wind. N.E.
Range 38°.—Mean temp. of exter. air 49°·73. For 30 days with ☉ in ♈ 47·47
Max. var. in 24 hours 22°·00.—Mean temp. of spring-water at 8 A.M. 47·86

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere in the evening of the 22nd ... 100°
Greatest dryness of the atmosphere in the afternoon of the 4th ... 48
Range of the index 52
Mean at 2 P.M. 67°·8.—Mean at 8 A.M. 75°·7.—Mean at 8 P.M. 81·7
—— of three observations each day at 8, 2, and 8 o'clock .... 75·1
Evaporation for the month 2·70 inches.
Rain in the pluviameter near the ground 3·235 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 6; fine, with various modifications of clouds, 13; an overcast sky without rain, 5; rain, 6.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
18	12	24	0	18	19	17

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
0	4	2	3½	½	11	3	6	30

* The distance was not correctly ascertained, in consequence of the interposition of a *cirrus* cloud, through which Venus was but faintly seen. These observations were made here merely to show that Venus may be seen with the naked eye in the day-time, while passing to or from her inferior conjunction when within 20 degrees of the sun; and that her altitude and distance from his centre can at the same time be correctly taken with a telescope and sextant.

The 5th being a fine sunny day, sixteen spots of various sizes were seen on the eastern side of the sun's disc in the form of a right angle, six of which were inclosed by two umbræ, and two of them were each 3000 miles in diameter.

General Observations.—This month has been alternately fine and showery, with frequent gales of wind; and although the cold nights, with hoar frost in the first part, gave a check to vegetation, the warm rains, followed by clear sunny days in the latter part, caused a rapid growth.

After a fruitful year and a severe winter, Nature has spread out her gifts more profusely in the bloom of the trees than could be expected: in sheltered situations the blooming and the formation of the fruits have prospered; but in open situations in the country they have been much blighted by the cold winds and prevailing frosts; nor have the easterly winds been pure and favourable to the human constitution.

On the 1st instant we had a snow-storm from 6 till nearly 10 A.M., which covered Portsdown Hill, although it was mixed with heavy rain. In the neighbourhood of London the snow is said to have been four inches deep, and in Shropshire six inches deep at the beginning of the month. The sudden change of temperature which brought on the snow at this late period, was occasioned by opposite winds from S.W. and N.E., and the cold increased till the morning of the 5th, when the moats of the fortifications were frozen over.

In the afternoon of the 8th, vivid flashes of lightning emanated from black clouds from the S.E., accompanied with loud claps of thunder. In the morning of the 18th, the first pair of swallows made their appearance here.

The mean temperature of the external air this month is about equal to the mean of April for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are five parhelia, four paraselenæ, one lunar and three solar halos, one rainbow, four meteors, an aurora borealis, and twelve gales of wind, or days on which they have prevailed, namely, two from the North-east, one from the East, seven from the South-west, and two from the North-west.

REMARKS.

London.—April 1, 2. Stormy and wet. 3. Cold and stormy. 4, 5. Clear and cold, with sharp frost at night. 6, 7. Fine. 8. Very fine, with much lightning at night. 9. Very fine: showery. 10—12. Showery. 13. Fine. 14. Very fine. 15—17. Cloudy, with showers. 18. Very fine. 19. Showery, with thunder at noon. 20. Showery. 21. Stormy, with strong gale at night. 22, 23. Stormy, with rain and boisterous gale. 24. Stormy morning: fine. 25—30. Very fine.

Penzance.—April 1, 2. Rain. 3. Hail, rain, snow. 4. Fair. 5, 6. Clear. 7. Misty: rain. 8. Fair: rain. 9. Fair: showers. 10. Showers. 11. Clear: rain. 12. Fair. 13. Clear. 14. Fair: rain. 15. Rain. 16. Rain: fair. 17. Fair. 18. Clear. 19—20. Fair. 21, 22. Fair: rain. 23. Fair: showers. 24—26. Clear. 27. Fair. 28. Clear. 29. Clear: shower. 30. Clear: fair.

Boston.—April 1. Cloudy. 2. Snow: snow P.M. 3. Snow: snow great part of the day. 4. Fine: frost early A.M. 5, 6. Fine. 7. Cloudy. 8, 9. Fine. 10. Rain. 11. Cloudy. 12. Cloudy: rain early A.M. 13, 14. Cloudy. 15. Rain. 16. Fine: rain early A.M. 17. Cloudy. 18. Cloudy: rain early A.M. 19. Rain: beautiful rainbow 6 P.M. 20. Rain: about noon very stormy. 21. Rain. 22. Windy: rain P.M. 23. Cloudy. 24. Stormy: rain early A.M. and P.M. 25. Fine. 26. Cloudy. 27—30. Fine.

Meteoro-

Meteorological Observations made by Mr. BOOTH at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
April 1	29·763	29·747	29·60	29·58	29·71	29·70	29·30	41	35	46	43	41	37	42	E.	SE.	NE.	E.	...	0·29	0·400	0·720	...
2	29·626	29·381	29·40	29·20	29·47	29·34	29·33	39	36	53	41	45	39	35	NE.	SE.	NE.	E.	...	1·19	·470	·630	...
3	29·981	29·516	29·95	29·50	29·86	29·26	28·85	41	27	43	38	41	31	35·5	E.	NE.	NW.	E.	0·10	...	·330	·005	0·67
4	30·283	30·050	30·10	30·05	30·18	30·10	29·74	44	24	45	34	44	28	34·5	S.	NE.	NE.	NW.	...	...	...	...	·17
5	30·243	30·052	30·10	30·05	30·20	29·98	29·77	52	26	45	32	44	30	36	S.	E.	NE.	W.	...	...	...	...	...
6	29·886	29·823	29·90	29·85	29·90	29·84	29·25	56	42	50	33	52	39	46·5	SW.	NW.	W.	W.	·30	...	...	...	...
7	29·922	29·840	29·90	29·87	29·93	29·84	29·40	60	43	56	38	55	44	47	SW.	SW.	SW.	calm	...	...	·505	...	...
○ 8	29·664	29·622	29·50	29·40	29·65	29·53	29·13	70	44	55	46	58	45	53	S.	S.	SE.	calm	...	...	·300	·080	...
9	29·443	29·372	29·32	29·30	29·45	29·40	28·90	67	44	54	46	58	45	55·5	S.	SW.	SW.	calm	·20	·28	·090	·750	...
10	29·585	29·506	29·50	29·40	29·67	29·54	28·85	57	40	55	44	55	44	44	SW.	W.	SW.	W.	...	·04	·040	·060	·36
11	29·728	29·671	29·60	29·55	29·67	29·67	29·15	58	43	55	45	54	47	50	SW.	W.	SW.	calm	...	·17	·490	·220	·20
12	29·780	29·504	29·70	29·55	29·70	29·50	29·00	59	45	54	46	56	45	50	NW.	W.	W.	calm	·15	·12	...	·030	·05
13	29·952	29·706	30·00	29·85	29·97	29·86	29·16	58	36	55	46	59	39	51	W.	NW.	NW.	W.	...	...	...	...	...
14	30·040	29·943	30·00	29·95	30·08	30·00	29·50	63	49	53	44	56	48	51	SW.	W.	NW.	calm	...	...	·210	...	...
15	29·812	29·688	29·75	29·72	29·82	29·75	29·30	63	51	57	48	60	49	50	S.	SW.	SW.	calm	·15	·13	·470	·110	·17
☾ 16	29·748	29·669	29·67	29·67	29·77	29·70	29·05	63	54	56	49	58	50	58·5	W.	SW.	SW.	S.	...	·01	·045	·020	·06
17	29·772	29·654	29·77	29·67	29·80	29·74	29·01	62	43	56	49	59	44	56	W.	SW.	SW.	W.	...	·08	...	...	...
18	29·866	29·809	29·80	29·78	29·90	29·82	29·85	64	44	58	46	57	45	54	SW.	S.	SW.	calm	·15	...	...	...	·15
19	29·836	29·584	29·98	29·72	29·90	29·70	29·12	51	39	55	46	58	45	49	W.	W.	NW.	calm	...	...	...	...	·20
20	30·014	29·660	30·00	30·00	30·00	29·84	29·20	61	41	56	47	57	46	43·5	SW.	W.	NW.	S.	...	·12	...	...	...
21	29·923	29·623	29·90	29·70	29·96	29·70	29·40	59	50	54	47	57	49	46·5	SW.	W.	SW.	W.	·30	·03	·490	·050	·18
● 22	29·587	29·415	29·60	29·40	29·64	29·50	28·98	59	53	54	49	54	50	53	W.	W.	SW.	W.	...	·23	·980	·370	·16
23	29·344	29·267	29·30	29·30	29·38	29·31	28·68	58	49	56	49	56	48	56	NW.	SW.	SW.	W.	...	·15	·175	·140	·14
24	29·761	29·246	29·60	29·40	29·76	29·41	28·52	58	38	53	48	56	41	54·5	SW.	NW.	W.	W.	·30	...	...	·030	·03
25	30·203	30·014	30·02	30·00	30·19	30·06	29·46	62	42	57	44	56	46	52	SE.	SW.	W.	W.	...	...	...	...	·09
26	30·240	30·226	30·05	30·05	30·26	30·24	29·65	66	36	58	47	58	44	53	E.	SW.	SE.	calm	...	...	...	...	...
27	30·251	30·181	30·00	29·98	30·24	30·15	29·70	68	40	58	47	60	50	56·5	E.	SE.	SE.	calm	·45	...	...	...	...
28	30·151	30·063	29·90	29·80	30·11	30·03	29·61	73	48	62	49	64	53	58	E.	SE.	E.	calm	...	...	...	...	...
☽ 29	30·029	29·927	29·75	29·70	30·01	29·92	29·43	76	46	62	54	66	54	62	SE.	SW.	E.	N.	...	...	·090	...	...
30	29·947	29·877	29·72	29·70	29·92	29·90	29·26	77	50	60	50	64	51	65	SE.	S.	SE.	calm	·60	...	...	·020	...
	30·283	29·246	30·10	29·20	30·26	29·26	29·58	77	24	62	32	66	28	49·9					2·70	2·84	5·085	3·235	2·63

INDEX TO VOL. IV.

END OF THE SEVENTH VOLUME.

LONDON:

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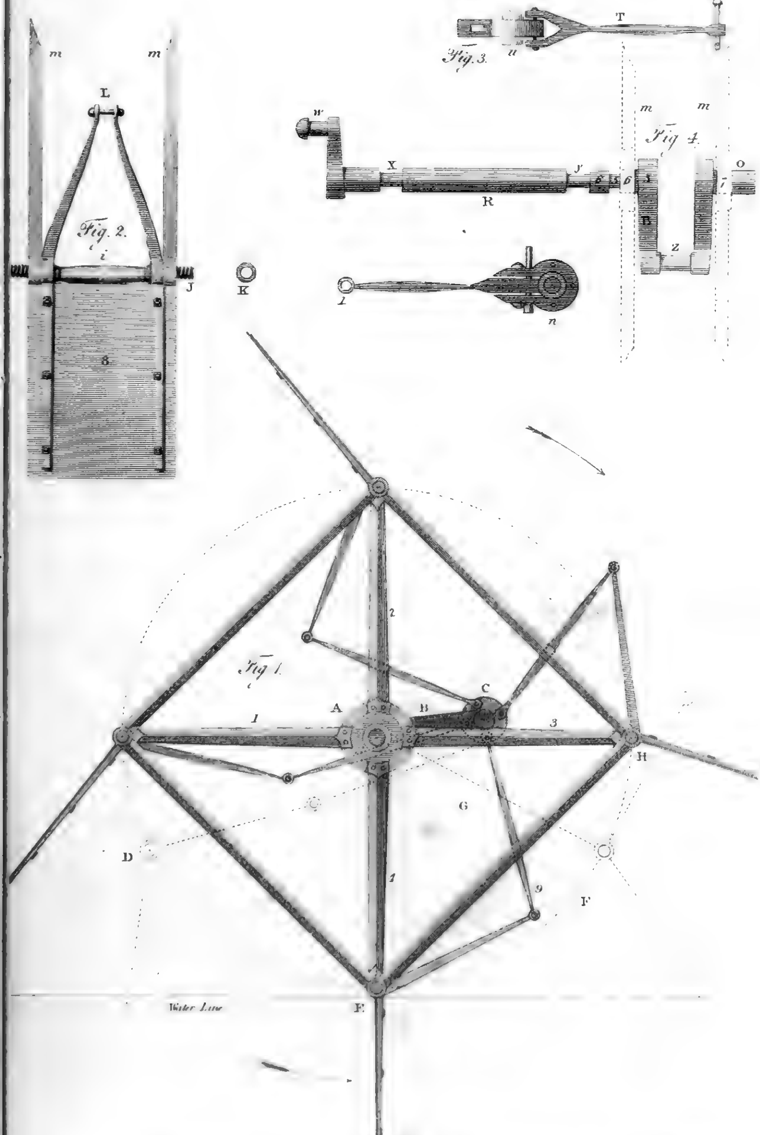
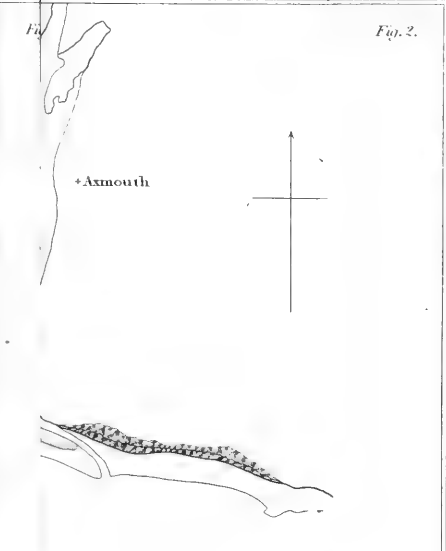
1830.



King William's Paddle Wheel.

Fig. 2.



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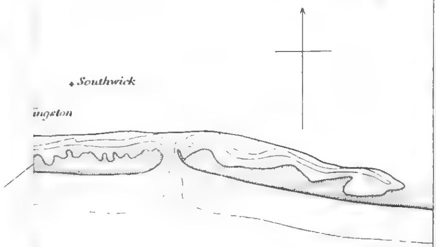
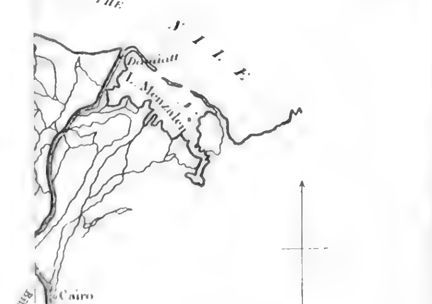
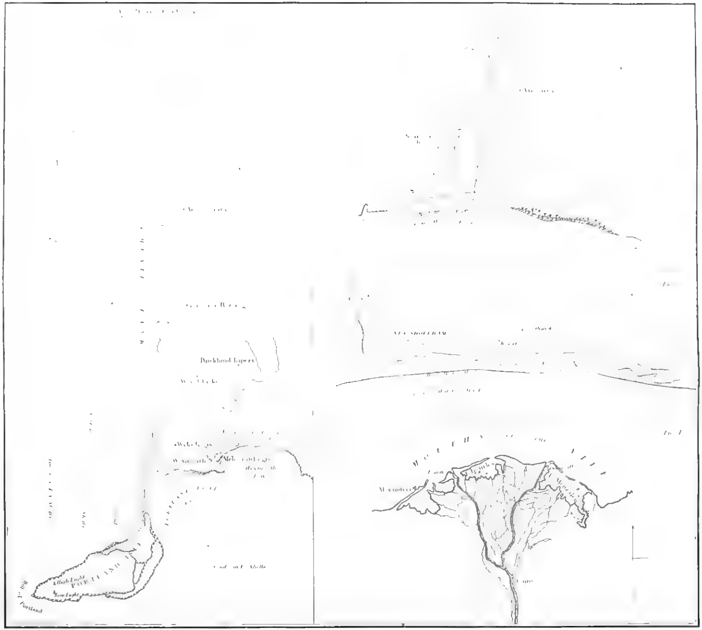


Fig. 4.




Portland

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Vol. I.—*New Series.* January—July 1827.

CONTENTS OF Nº 39.—*New Series.*

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