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COMPREHENDING

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

NEW SERIES.

N° 7.—JULY 1827.

WITH A PLATE,

Illustrative of Mr. DAVIES's communication on the Symmetrical Properties of Plane Triangles.

BY

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

AND

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

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TO CORRESPONDENTS.

Communications have been received from Mr. EXLEY, Mr. JOHN PHILLIPS, Mr. REID, and Lieut.-Col. MILLER.

Mr. MOYLE's Meteorological Register and Mr. EDMONSTONE's paper in our next.

*** *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 15th day of the month, or they will be too late to appear in the ensuing Number.*

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NEW SERIES.

No 9.—SEPTEMBER 1827.

BY

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

AND

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

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TO CORRESPONDENTS.

The Communications from Messrs. MOSELY and BRONWIN will be inserted as early as possible.

Mr. ROSE's paper is under consideration.

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NEW SERIES.

N° 10.—OCTOBER 1827.

WITH A PLATE,

Illustrative of Mr. R. C. TAYLOR's Remarks on the Natural Embankments of the Norfolk and Suffolk Coast of the German Ocean.

BY

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

AND

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TO CORRESPONDENTS.

The Communications from the Rev. Mr. BRONWIN and from Mr. HAWORTH will appear in our next Number.

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NEW SERIES.

No 11.—NOVEMBER 1827.

WITH A PLATE,
To illustrate Professor Hansteen's Observations on the Polar Lights.

BY
RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.
AND
RICHARD PHILLIPS, F.R.S. L. & E. F.L.S. &c.

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TO CORRESPONDENTS.

We regret that the typographical difficulties arising from the notation employed by the Rev. B. BRONWIN in his second and third communications is at present an obstacle to their insertion.

We have received Meteorological and Astronomical Observations from Yarmouth, but are not favoured with the name of the observer.

Communications from Mr. WOOLMER and Mr. H. STOKES have been received.

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NEW SERIES.

N° 12.—DECEMBER 1827.

WITH A PLATE,
Illustrative of Mr. JOHN TAYLOR's Rain-gauge.

BY
RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.
AND
RICHARD PHILLIPS, F.R.S. L. & E. F.L.S. &c.

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TO CORRESPONDENTS.

The Minerals of Haytor are described in terms so unintelligible, that we must decline inserting the paper on them.

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THE

PHILOSOPHICAL MAGAZINE,

OR

ANNALS

OF

CHEMISTRY, MATHEMATICS, ASTRONOMY, NATURAL HISTORY, AND GENERAL SCIENCE.

BY

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

AND

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

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

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AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JULY 1827.

I. *Collections in Foreign Geology.*—[No. I.] *By* H. T. De la Beche, *Esq. F.R., L., and G.S. &c. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

SHOULD it suit the plan of your Journal, I propose sending you, from time to time, such accounts or translations of the labours of continental geologists, as it is hoped may be found useful and interesting to those of our own country who may not always be within reach of libraries containing foreign scientific works. Though our own country, is, perhaps, unequalled, (when extent of surface is considered,) for the facilities it affords the study of geology, by the variety and importance of its rocks and by its natural and artificial sections, it cannot alone enable us to take a comprehensive view of the general structure of the earth's surface; this can only be effected by combining the labours of numerous individuals in different parts of the world. The structure of by far the greater part of our planet's surface, is, and will long continue to be, unknown; yet much has already been done, more particularly in Europe and North America: distinguished geologists of France, Germany, Sweden, North America, and Italy are continually adding to our knowledge by their respective labours; and if the following communications should tend to direct more general attention to these labours, their object will be fully answered.

H. T. De la Beche.

1. *On the* Calcaire Grossier *of Paris**.

The calcaire grossier does not always rest immediately on the (plastic) clay, being often separated from it by a bed of sand. We cannot state whether this sand belongs to the formation of the limestone or that of the clay. We have not, it is true, found fossil shells in it, in the few places where we have observed it,—which circumstance would seem to refer it to the (plastic) clay formation; but as the lowest calcareous bed usually contains sand, and as it is always full of shells, we do not yet know whether this sand differs from the former, or belongs to the same deposit. We may be led to suppose it different, since the sand of the clay that we have seen is in general tolerably pure, though of a red or blueish-gray colour; it is refractory (*réfractaire*), and often very-large-grained. This sand sometimes contains masses or beds of tolerably pure and solid sandstone.

The calcaire grossier, after quitting this sand or sandstone, is composed of alternating beds of coarse limestone more or less hard; of argillaceous marl, often in very thin beds, and of calcareous marl: it must not however be supposed that these various beds occur without order; they are always found in the same order of superposition throughout the considerable extent of country that we have examined. Many of them are occasionally either wanting or very thin; but that bed which is inferior in one district is never superior in another.

This constancy in the superposition of even the thinnest beds, over an extent of at least twelve myriameters, is, in our

* From the *Description Géologique des Environs de Paris*, by Baron G. Cuvier, and M. Alex. Brongniart.

"This rock, the equivalent of our London clay, forms such an important part of the superior or tertiary rocks, that the excellent general account here given of its mode of occurrence, and of its characteristic fossils, becomes highly valuable. We should indeed expect very considerable modifications in these respects, in such a comparatively modern rock, at points distant from each other; and we do accordingly find them, more particularly in its mineral composition, as for instance, in the case of the London clay; yet keeping in mind the more essential characteristics here detailed, we are enabled to recognise strata, formed at the same geological epoch with this, amid the superior or tertiary rocks of other countries. This has already been done not only in our own country, but also in Switzerland, Italy, the Netherlands, Austria, Hungary, Poland, &c. It is also stated to exist in the north of Africa. Humboldt (*Essai sur le Gisement des Roches*) considers some of the rocks of Equinoctial America as equivalent to this formation. However this may be, it is certain that tertiary rocks are not wanting in the West Indian Islands; and I have myself observed them occupying a considerable extent of country, and of great thickness, in Jamaica."—*Trans.*

estimation,

estimation, one of the most remarkable facts that our researches have brought to light.

The mode we have employed to recognize a bed already observed in one district, is the nature of the fossils contained in each bed: these fossils are always of the same general character in corresponding beds, and present very observable differences in species from one system of beds to another. This mode of recognition has not as yet deceived us.

It must not however be supposed that the difference between one bed and another is as marked as that between the chalk and calcaire grossier. If it were so, we should have so many distinct formations; but the characteristic fossils of one bed become less numerous in the bed above it, and either disappear in those above the latter, or are gradually replaced by others which had not before appeared.

We now proceed to describe the principal systems of beds observable in the calcaire grossier. The first and most inferior beds are the best characterized; they are very sandy, often more sandy than calcareous. When they are solid, they decompose in the air and fall into powder: the stone also which they furnish is only capable of being employed under particular circumstances.

The shelly limestone of which they are composed, and even the sand which sometimes replaces it, almost always contains green earth, either in grains or powder. This earth appears, from the experiments we have made, analogous in its composition to the *chlorite baldogée*, or Verona-earth, and to owe its colour to iron.

The following is an analysis that M. Berthier has made of the Verona-earth and of the green grains found in the lowest beds of the calcaire grossier.

	Verona Earth.	Green grains of the limestone beneath Paris.	Green grains of the limestone in the environs of Paris.
Silica	0·68	0·46	0·40
Protoxide of Iron	0·17	0·22	0·25
Alumina	0·01	0·07	0·02
Lime	0·00	0·03	0·03
Magnesia	0·07	0·06	0·16
Potash	0·00	0·00	0·02
Water	0·06	0·15	0·12
	0·99	0·99	1·00

It will be seen that this earth is in general a silicate of iron; and it is probable, if it could be obtained more pure, that the analogy between it and the Verona-earth would be found more complete.

This green, earthy, and granular silicate of iron is only found in the lower beds: it is not seen either in the upper or white chalk, in the (plastic) clay, or in the middle or upper beds of the calcaire grossier, and its presence may be regarded as a sure sign of the vicinity of the plastic clay, and consequently of the chalk. The prodigious quantity of fossil shells, is, however, the principal characteristic of this system of beds; the greater part of these shells are more unlike existing species than those contained in the upper beds.

Nummulites are found in this system of beds. They occur either alone or mixed with madrepores and a few shells. We have found them near Villers-Cotterets, in the valley of Vaucienne, at Chantilly, and at the descent of Morlaye. They are mixed with well-preserved shells, and with large grains of quartz which form this rock into a kind of conglomerate; as at Mont Ganelon near Compiègne, Mont Ouin near Gisors, &c.

Another characteristic of the shells contained in this system of beds, is, that the greater part are entire and well preserved, that they are easily separated from the rock, and that many of them have still preserved their nacre.

The other systems of beds are less distinct. The middle beds still contain an abundance of shells. We should notice a bed, sometimes soft and often possessing a green tint, whence the name *banc vert* given it by the workmen, and sometimes hard and of a yellowish gray colour. Brown impressions of leaves and stems of vegetables are frequently found in the lower part of this bed mixed with *Cerithia*, thick *Ampullariæ*, and other marine shells. The greater part of these vegetables, which are very delicate and varied, cannot be referred to any marine plant; the bed containing them is seen at Chatillon, St. Nom, Saillancourt, &c. that is to say, over an extent of nearly ten leagues.

The third or upper system contains fewer shells than the two preceding. In this we can often recognize, 1. Yellowish or gray beds, sometimes soft, at others very hard and containing *Lucinæ*, *Ampullariæ*, and *Cerithia*, which sometimes occur in vast quantities. The upper and middle of this part is often very hard, affords an excellent building-stone, and is known by the name of *roche*. 2. Near the top, a bed of little thickness, but hard, and remarkable for the prodigious quantity of small elongated and striated *Corbulæ*, found in the horizontal fissures. These Corbulæ occur in an horizontal position, pressed closely against each other. They are generally white.

Above the last beds of the calcaire grossier, the hard calcareous

careous marls appear. These divide into fragments, the faces of which are commonly covered by a yellow bloom and black dendrites. They are divided by soft calcareous marls, by argillaceous marls, and by calcareous sand containing chert. We refer the bed in the Neuilly quarries to this system, in which are found quartz crystals, rhomboidal crystals of carbonate of lime, and small cubical crystals of fluor spar.

This fourth and last system contains very few shells: in fact the upper beds do not usually contain any.

Each of these systems may be characterized by the fossils contained in the following list:

First System.

Lower Beds.

Nummulites lævigata ——— *scabra* ——— *numismalis* ——— *rotundata*	These are always found in the lowest parts: they are not discovered at Grignon; the Grignon strata appear to belong rather to the middle than the lowest beds.
Madrepora	Three species at least.
Astræa	At least three species.
Turbinolia elliptica	A. Br.
——— *crispa*	Lam. Enc. pl. cdlxxxiii. fig. 4.
——— *sulcata*	Lam.
Reteporites digitata	Lam^{x}., Polyp. pl. lxxii. fig. 6—8.
Lunulites radiata	Lam^{x}., Polyp. pl. lxxiii. fig. 5—8.
——— *urceolata*	Lam.
Fungia Guettardi	Guettard, 3, pl. xii. fig. 1—8.
Cerithium giganteum	Scarcely any other than this species of *cerithium* is found in the true inferior beds.

Lucina lamellosa.
Cardium porulosum.
Voluta Cithara.
Crassatella lamellosa.
Turritella multisulcata.
Ostrea Flabellula.
——— *Cymbula.*

Second System.

Middle Beds.

Nearly all the Grignon fossils belong to this system. The following appear to be the most characteristic:

Orbitolites plana.
Cardita avicularia.
Ovulites elongata, Lam. . . . Lam^{x}., pl. lxxi. fig. 11, 12.

Ovulites

Ovulites Margaritula . Lam^x^., pl. lxxi. fig. 9, 10.
Alveolites Milium . . . Bosc, Bull. des Sc. No. 61. pl. v. fig. 3.
Turritella imbricata.
Terebellum convolutum.
Calyptræa trochiformis.
Pectunculus pulvinatus.
Citheræa nitidula.
——— *elegans.*
Miliolites . . . . These are extremely abundant.
Cerithium? . . . . There may be some species; but we do not find *C. lapidum*, *C. petricolum*, &c., or *C. cinctum*, *C. plicatum*, &c. The latter belong to the second marine formation.

The number of the species of fossil shells found in these two first systems amounts to nearly six hundred. They have nearly all been collected by M. Defrance and by us, and have been described by M. de Lamarck.

Third System.

Upper Beds.

The species contained in these beds are much less numerous than those found in the middle beds.

Miliolites . . . . These are here more rare.
Cardium Lima or *obliquum.*
Lucina saxorum.
Ampullaria spirata, &c.
Cerithium tuberculatum .
——— *mutabile* . . .
——— *lapidum* . . .
——— *petricolum* . .
And nearly all the other *Cerithia*, with the exception of *C. giganteum.*
Corbula anatina?
——— *striata.*

Fossil Vegetables of the Calcaire Grossier.

Endogenites echinatus . . .	Ad. B. . . .	Environs of Soissons.
Culmites nodosus	Ad. B. . . .	Montrouge.
——— *ambiguus*	Ad. B. . . .	Grignon.
Phyllites	(many species) . . .	Montrouge, &c.
Flabellites Parisiensis . .	Ad. B. . . .	St. Nom.
Pinus Defrancii	Ad. B. . . .	Bagneux.
Equisetum brachyodon . .	Ad. B. . . .	Montrouge.

Beds of sandstone and masses of chert, full of marine shells, are found among the strata of the second and third systems. The limestone beds are even, in some places, entirely replaced by this sandstone, which is sometimes friable and of an opaque whitish

whitish gray colour; at others shining, almost translucent, and of a more or less deep gray colour. The fossils which often occur in it, in prodigious quantities, are white, calcareous, and well preserved, though thin and occasionally mixed with rolled pebbles.

This sandstone, the second above the chalk, and also the chert with marine shells, which appears occasionally to replace it, sometimes occurs on or in the marine limestone, as at Triel, at Frêne, at St. Jean les Deux Jumeaux, &c. They sometimes seem entirely to replace the limestone, as in the environs of Pontoise, at Essainville, and at Beauchamp near Pierrelaie.

Among the various shells contained in this sandstone, there are many which appear to be of the same species as those found at Grignon; others differ a little.

The following is a list of those shells which are the most constantly found in this sandstone, and which, as it were, characterize it by their presence:

Calyptræa trochiformis? . . .	Quarries of Beauchamp near Pierrelaie.
Oliva laumontiana	Pierrelaie, Triel.
Ancilla canalifera	Triel.
Voluta Harpula?	Triel.
Fusus bulbiformis?	Pierrelaie.
Cerithium serratum	Pierrelaie.
——— *tuberculosum*	Essainville.
——— *coronatum*	Pierrelaie.
——— *lapidum*	Pierrelaie.
——— *mutabile*	Pierrelaie.
Ampullaria acuta or *spirata* . . .	Pierrelaie, Triel.
——— *patula?* (very small)	Pierrelaie.
Nucula deltoidea?	Pierrelaie.
Cardium Lima?	Pierrelaie, Triel.
Venericardia imbricata	Pierrelaie, Triel.
Cytherea nitidula	Triel.
——— *elegans?*	Triel, Pierrelaie.
——— *tellinaria*	Pierrelaie.
Venus callosa?	Pierrelaie.
Lucina circinaria	Essainville.
——— *saxorum.*	

Two species of oyster still undetermined, one approaching *Ostrea deltoidea*, and the other *O. Cymbula*.

It will be seen by this enumeration; 1. that the species are considerably fewer than in the Grignon beds; 2. that it is with doubt that we have referred the greater part of these species to the names given by M. Lamarck to those of Grignon.

It

It is in this sandstone and at Beauchamp to the E. of Pierrelaie that MM. Gillet de Laumont and Beudant have observed terrestrial and fresh-water shells (well characterized *Limnei* and *Cyclostomæ*) mixed with the above-named marine shells.

It results from the preceding observations: 1. that the fossils of the calcaire grossier have been deposited slowly and in a tranquil sea, since these fossils occur in regular strata, are not mixed, and the greater part are in a perfect state of preservation; notwithstanding the delicacy of their structure, even the points of the spiny shells being entire; 2. that these fossils differ entirely from those of the chalk; 3. that as the beds of this formation were deposited, the species changed, many disappeared, while new fossils took their place, which would render a long series of generations of marine animals probable; and lastly, that the number of species continually decreased, until they finally disappeared.

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2. *On the Position of the Fossil Bones of* Mont de la Molière (*Switzerland*), *by the late* Mons. P. F. M. Bourdet*.

The author commences with a description of the rocks in the vicinity of the mountain in question, which he visited in June 1823. "The beds," he says, "are composed, 1. of vegetable soil; 2. of nine feet of a soft sandstone (*nagel-fluh* sand), which easily decomposes in the air; 3. of eight inches of a species of coloured fuller's-earth-marl, employed by the clothiers of the country; 4. of the same thickness of a hard calcareous sandstone; and 5. of a chocolate-coloured and hard argillaceous marl. The remainder is concealed by fallen portions of the upper beds; the debris of a molasse sandstone. Advancing towards the hill, the slope of which is very gradual, two distinct rocks are seen beneath the vegetable soil; the first is

* From a notice in Baron de Ferrusac's *Bulletin des Sciences*, (a very valuable periodical publication,) of the Author's paper in the *Ann. de la Soc. Linnéenne de Paris*, Sept. 1825.

"This memoir is highly interesting, as it presents us with a well authenticated instance of the bones of the hyæna, the elephant, the rhinoceros, &c., having been found lower down in the series of formations than the diluvium, mixed up with marine, terrestrial, and fresh-water shells; a very valuable geological fact, in addition to that, now some time known, of the teeth of the mastodon (not the great species) having been discovered among the lignite of some of the Swiss sandstones. Specimens of the latter I had the pleasure of seeing at Prof. Meisner's at Berne a few years since. As my friend Dr. Buckland intends elucidating this subject from observations made in Italy, &c., I shall abstain from further remark. M. Bourdet considers the compact sandstone-rocks of Monte de la Molière as referable to the marine? gypsum formation of the environs of Paris."—*Trans.*

is a sandy deposit, containing rocks of the first formation; the second is a greenish-gray and very fragile *molasse* sandstone, containing no fossils. The two rocks constitute the upper bed of a much harder *molasse*, occurring nine feet lower down. The calcareous sandstone, which is very hard, contains marine shells of the genera *Venus*, *Tellina*, and *Pyrula*; among which terrestrial and fluviatile shells are found, belonging to the genera *Helix*, *Planorbis*, *Lymnea*, *Cyclostoma*, &c. Here also a ferruginous sand is observed mixed with stems and roots highly impregnated with iron, emitting a vegetable odour when burnt.

Before we reach Mont de la Molière, we arrive at Haut-Mont, a hill composed, beneath the vegetable soil, of a hard and brown calcareous sandstone, in which no fossil occurs, with the exception of a lignite, susceptible of a high polish. Beneath this rock, occurs one which differs from the preceding, both in its nature and the substances it contains; it is a kind of conglomerate (*nagel-fluh*) formed of small rounded pebbles, of compact limestone, whitish flint (*silex*), siliceous sand, and compact felspar (*eurite*), strongly cemented by a substance, entirely calcareous, the interstices of which are filled with small scales of siliceous carbonate of lime, which effervesce in nitric acid, and strike fire with steel. This conglomerate alternates with a *molasse* sandstone on which it rests; mill-stones are made of it, and it constitutes a great part of the high land between Vreissens, Correvon, Ogens and Combremont, the environs of Estavayer, and extends from thence to beyond the eastern shore of the lake of Neufchâtel. After an hour's journey, we arrive at the highest point of Mont de la Molière; a hill situated in the Canton of Fribourg, on the south of Estavayer, near the lake of Neufchâtel. The summit is shaded by trees, from the centre of which an old ruined tower rises; it is 1018 English feet above the Lake of Geneva, and 2220 English feet above the Mediterranean Sea. It is composed of a very solid compact sandstone, which has been long worked for millstones. The quarry, thirty feet deep, is worked in such a manner that the direction of the beds can be easily observed; their inclination, from S.W. to N.E. is inconsiderable. These beds, of a blueish colour, are composed of thin seams intimately connected together, and contain the fossils about to be noticed.

The abundance of these fossils is the first thing that strikes us in this part of the mountain; we cannot raise a specimen of rock without meeting with them; this profusion is not however the same every where. The bones which do not appear

appear to have been rolled, are in a great measure broken. M. Bourdet describes the various specimens met with, a considerable part of which he found in the collection of M. Fontaine of Friburg, in those of MM. Meisner and Wyttenbach, and in the museum of Berne. The remainder were either obtained by himself or others, and have been deposited, partly in the collection of Christian Frederick, hereditary prince of Denmark, and partly in his own. Three plates, comprising twenty-three figures of specimens, accompany the memoir. We shall not here follow him through the anatomical details which he has entered into respecting each, but present the following results at which he has arrived.

I. *Carnivorous Mammiferæ.* A species of *Hyena*, differing from those at present known.

II. *Pachydermata.* An *Elephant*, of two different ages, approaching that of India;

A *Hog*, which though young, was of greater size than those which at present exist;

A *Rhinoceros*, probably nearly approaching the Unicorn Rhinoceros of Java.

III. *Ruminants.* An *Antelope*,—as it would appear.

IV. *Various Bones.*

"The other remains of the bones of quadrupeds found in this place are," says M. Bourdet, "too much mutilated to be recognized; the collection of Berne, however, contains some fragments of gallinaceous ornitholites, such as the remains of the tibia and femur."

"The remains of a land tortoise are here met with; as also those of fish, the best preserved of which are the teeth of sharks, &c., the palates of unknown rays, and *cestracions*. This is the first instance of the teeth of the latter fish, a native of New Holland, having been found fossil. The other remains of fish consist of vertebræ and ribs."

"The remains of shells are the most abundant: those which we have observed belong to the marine genera *Cytherea*, *Venus*, *Tellina*, *Cardita*, *Pecten*, *Cama*, *Buccinum*, *Voluta*, *Turbo*, *Strombus*, *Cerithium*, *Bulla*, &c. Among the terrestrial and fresh-water shells are *Helix*, *Planorbis*, *Limneus*, &c. M. Studer, jun., has described this class with equal sagacity and precision in his Monography of the Molasses of Switzerland."

H. T. D. B.

II. *Obser-*

II. *Observations relatives à un Article de* Mr. Ivory, *inséré dans le No. 5. du* Philosophical Magazine, *&c. N. S. pour Mai* 1827. *Par* M. Poisson, *F.R.S. &c.**

L'ARTICLE dont il s'agit a principalement pour objet de critiquer un passage de mon Mémoire sur l'attraction des sphéroïdes, qui fait partie des Additions à la *Connoissance des tems* pour l'année 1829. Pour répondre à cette critique, je supposerai qu'on aît mon Mémoire sous les yeux, ainsi que l'Article de Mr. Ivory dont je conserverai les notations, d'après lesquelles la formule qu'il s'agira d'examiner sera:

$$X = \frac{1}{4\pi}\int \frac{(1-\alpha^2)\,y'ds}{f^3};$$

où l'on doit intégrer par rapport à deux variables θ' et ψ', depuis $\theta' = 0$ et $\psi' = 0$, jusqu' à $\theta' = \pi$ et $\psi' = 2\pi$; et où l'on a: $ds = \sin\theta'\,d\theta'\,d\psi'$; y' est une fonction donnée de θ' et ψ'; f est la distance mutuelle de deux points dont les coordonnées polaires sont α, θ', et ψ', pour l'un; et l'unité, θ, et ψ, pour l'autre; y sera la valeur de y' qui répond à $\theta' = \theta$, et $\psi' = \psi$; on supposera, pour fixer les idées, $\theta > 0$ et $< \pi$, $\psi > 0$ et $< 2\pi$, et la constante α moindre que l'unité.

J'ai dit qu' à la limite, où cette constante diffère infiniment peu de l'unité, on a: $X = y$, pourvû que y' ne devienne pas infinie dans l'étendue de l'intégration. Pour le prouver, j'ai remarqué que, dans cette hypothèse, l'intégrale s'évanouit avec la différence $1 - \alpha$, excepté pour les valeurs de θ' et ψ' qui rendent le dénominateur f^3 infiniment petit. Lorsque la différence $1 - \alpha$ est infiniment petite, il suffit donc d'étendre l'intégrale à ces valeurs de θ' et ψ', lesquelles diffèrent infiniment peu de θ et ψ; alors le facteur y' peut être regardé comme constant et égal à y dans toute l'étendue de l'intégration; et il en résulte:

$$X = \frac{y}{4\pi}\int \frac{(1-\alpha^2)\,ds}{f^3}.$$

D'ailleurs cette dernière intégrale est égale à 4π; on aura donc: $X = y$, à la limite $\alpha = 1$.

J'avois déjà donné cette démonstration à la suite de mon 1er Mémoire sur la distribution de la chaleur dans les corps solides; mais Mr. Ivory ayant contesté que l'on puisse regarder le facteur y' comme constant, ainsi que je l'ai pratiqué ici,

* These observations have been transmitted to us by the author; and, though contrary to our usual custom, we think it due to M. Poisson under all the circumstances of the mathematical discussion to which they relate, to insert them in the original French, exactly as they were placed in our hands.—Edit.

ici, et dans beaucoup d'autres cas semblables, j'ai pensé qu'il falloit insister sur ce point qui est, en effet, le principe essentiel de mon raisonnement. Après avoir reproduit dans le No. 2. de mon Mémoire sur l'attraction des sphéroïdes, la démonstration précédente, telle que je l'avois donnée autrefois, j'ai donc ajouté l'observation suivante qui forme le No. 3. de mon Mémoire.

Si l'on fait: $y' = y + \zeta$, on aura:

$$X = \frac{y}{4\pi}\int\frac{(1-\alpha^2)\,ds}{f^3} + \frac{1}{4\pi}\int\frac{(1-\alpha^2)\,\zeta\,ds}{f^3}.$$

Entre les limites de ces intégrales qui ne doivent s'étendre qu'à des valeurs de θ' et ψ' infiniment peu différentes de θ et ψ, la variable ζ est infiniment petite; si donc on désigne par β sa plus grande valeur, cette constante β sera aussi infiniment petite; et comme on aura évidemment

$$\int\frac{(1-\alpha^2)\,\zeta\,ds}{f^3} < \beta.\int\frac{(1-\alpha^2)\,ds}{f^3},$$

il en résulte que la seconde intégrale comprise dans la valeur de X, sera infiniment petite et pourra être négligée par rapport à la première; ce qui réduira cette valeur à $X = \frac{y}{4\pi}.\int\frac{(1-\alpha^2)\,ds}{f^3}$, ainsi qu'auparavant.

Je ne vois pas qu'on puisse élever aucune difficulté contre ce raisonnement, réduit à un tel degré de simplicité; et si Mr. Ivory n'a pas été convaincu de son exactitude, c'est sans doute qu'il aura mal saisi le sens de mes expressions.

Quoiqu'il en soit, Mr. Ivory dit d'abord, dans l'Article auquel je réponds, que l'équation $\int\frac{(1-\alpha^2)\,\zeta\,ds}{f^3} = 0$, étant la même chose que: $y = \frac{1}{4\pi}\int\frac{(1-\alpha^2)\,y'\,ds}{f^3}$, c'est une *pétition de principe* de s'appuyer sur la 1ere équation pour démontrer la 2de. Cela seroit vrai, effectivement, si l'on se contentoit de *poser* la 1ere équation; mais on la *démontre*, comme on vient de le voir; et il en résulte que la 2de se trouve démontrée en même tems, ce qui n'a rien de contraire à la logique.

Mr. Ivory ajoute ensuite que je prends l'intégrale qui renferme ζ, en regardant ce facteur infiniment petit comme constant. Cela n'est point exact: j'appelle β la plus grande valeur de ζ; c'est cette quantité β que je traite comme constante, et que je fais passer en dehors du signe $\int$; et, de cette manière, j'obtiens, non pas la 2de intégrale contenue dans X, mais une quantité supérieure à cette intégrale: ce qui suffit à l'objet que je me propose.

Enfin,

Enfin, Mr. Ivory cherche la valeur même de cette intégrale. Pour cela, il prend entre ses limites:

$$\zeta = \mathrm{A}h + \mathrm{B}k,$$

A et B étant des constantes, et h et k des quantités infiniment petites. Il réduit alors le second terme de la valeur de X à

$$\frac{1}{2\pi}\int \frac{g \mathrm{A} \sin\theta . h dh dk}{(g^2 + h^2 + k^2 \sin^2\theta)^{\frac{3}{2}}} + \frac{1}{2\pi}\int \frac{g \mathrm{B} \sin\theta . dh k dk}{(g^2 + h^2 + k^2 \sin^2\theta)^{\frac{3}{2}}},$$

g désignant une constante infiniment petite. On pourroit d'abord observer qu'il seroit permis de prendre pour limites de ces intégrales, des valeurs de h égales et de signes contraires, et de semblables valeurs de k; ce qui suffiroit pour rendre ces deux intégrales identiquement nulles. Mais selon Mr. Ivory leur somme est exprimée par

$$-\frac{g}{2\pi}\left\{ \mathrm{A} \log. \frac{\sqrt{g^2 + h^2}}{g} + \frac{\mathrm{B}}{\sin\theta} \log. \frac{\sqrt{g^2 + k^2 \sin^2\theta}}{g} \right\};$$

résultat qui suppose que l'une des intégrales est prise depuis $k = 0$ jusqu'à $k = \infty$, l'autre depuis $h = 0$ jusqu'à $h = \infty$; et qu'en outre, la 1[ere] s'évanouit avec h et la 2[de] avec k. Or, contre le sentiment de l'auteur, cette quantité est infiniment petite en même tems que g, quelque rapport que l'on établisse entre g et les quantités h et k, comprises sous les logarithmes. En effet, si l'on suppose ces quantités infiniment petites à l'égard de g, celles dont on prend les logarithmes se réduisent à l'unité, et ceux-ci à zéro. Si l'on fait $h = gh'$, $k = gk'$, et qu'on regarde h' et k' comme des quantités finies, les logarithmes ci-dessus seront aussi des quantites finies, et multipliés par g, les produits seront infiniment petits. Enfin, si l'on veut que h et k soient infinies par rapport à g, et qu'on fasse, par exemple,

$$h = \frac{h'}{g^a},\ k = \frac{k'}{g^b},$$

a et b étant des exposans positifs, et h' et k' des quantités finies, la formule que nous considérons se réduira à la forme:

$$\mathrm{C}. g \log. g,$$

C désignant un facteur indépendant de g: elle sera donc encore infiniment petite en même tems que g; car le produit g log. g diminue indéfiniment et s'évanouit avec g; ce qui tient à ce que le logarithme d'une fraction décroissante, augmente dans un moindre rapport que cette fraction ne diminue.

On arriveroit donc aussi par cette discussion à conclure que le second terme de la valeur de X, tel que Mr. Ivory l'a formé, s'évanouit avec la différence $1 - \alpha$; mais on est dispensé de tout ce détail, en considérant, comme je l'ai fait, une quantité supérieure à cette intégrale, et correspondante à la plus grande

valeur

valeur ε de la variable ζ. On n'a pas besoin alors d'avoir égard à la forme de cette variable dans l'étendue de l'intégration ; et il n'est pas nécessaire qu'elle soit infiniment petite par rapport à f, comme Laplace le suppose dans la démonstration de son équation relative à la surface d'un sphéroïde peu différent d'une sphère. Il peut même arriver, pour des valeurs particulières de θ et ψ, que ζ soit au contraire infinie par rapport à f; ou, autrement dit, que sa valeur en fonction des variables h et k, soit exprimée par des puissances dont les exposans soient moindres que l'unité : cette circonstance n'empêchera nullement l'intégrale $\int \frac{(1-\alpha^2)\zeta ds}{f^3}$, de devenir infiniment petite ou de s'évanouir en même tems que la différence $1-\alpha$.

Généralement, si l'on a : $X = \frac{c}{\pi . 2^{c+1}} . \int \frac{(1-\alpha^2)^c . y' ds}{f^3}$,

c étant un exposant positif quelconque, et les autres notations demeurant les mêmes que précédemment ; on en conclura

$$X = \frac{cy}{\pi . 2^{c+1}} . \int \frac{(1-\alpha^2)^c . ds}{f^3},$$

à la limite où α diffère infiniment peu de l'unité. La démonstration sera exactement la même que dans le cas précédent où l'on avoit $c = 1$; et quant à l'intégrale qui reste à obtenir, on la réduira d'abord à

$$\int \frac{2^c . g^c . \sin\theta \, dh \, dk}{\{g^2 + h^2 + k^2 \sin^2\theta\}^{1+\frac{1}{2}c}},$$

g désignant une constante infiniment petite. Quoiqu'il suffise d'étendre cette intégrale à des valeurs infiniment petite de h et k, on pourra aussi, sans en altérer la valeur, la prendre depuis $h = -\infty$ et $k = -\infty$, jusqu' à $h = \infty$ et $k = \infty$, à cause qu'elle est nulle dès que l'une des variables h ou k a acquis une grandeur finie. Si l'on fait alors

$$h = g . r \sin\omega, \qquad k \sin\theta = g . r \cos\omega,$$

On aura en même tems : $\sin\theta . dh \, dk = g^2 . r \, dr \, d\omega$,

l'intégrale précédente deviendra : $\int \frac{2^c . r dr d\omega}{\{1+r^2\}^{1+\frac{1}{2}c}}$;

et comme elle devra être prise depuis $r = 0$ et $\omega = 0$, jusqu'à $r = \infty$ et $\omega = 2\pi$, elle aura $\frac{\pi . 2^{c+1}}{c}$ pour valeur. Celle de X sera donc : $X = y$, quelque soit l'exposant positif c, et quelle que soit aussi la fonction y'.

Le surplus de l'Article de Mr. Ivory est relatif à la figure d'une masse fluide homogène tournant autour d'un axe fixe. L'auteur

L'auteur pense, comme on sait, que dans le cas où les molécules fluides sont soumises à leur attraction mutuelle suivant une fonction quelconque de la distance, la condition ordinaire de l'équilibre est insuffisante; mais la nécessité d'aucune condition nouvelle n'a été admise par les Géomètres; et j'ai refuté, il y a près de trois ans, l'opinion de Mr. Ivory sur ce sujet, dans un petit écrit * qui auroit pu mériter son attention. Si l'on excepte le cas où l'attraction est en raison inverse du carré des distances, et où il s'agit d'une couche fluide comprise entre deux surfaces elliptiques semblables et semblablement placées, l'auteur n'en citeroit peut-être pas un autre où sa nouvelle condition d'équilibre fût satisfaite; et même, dans ce cas particulier, il faut observer qu'elle a lieu comme une propriété des ellipsoïdes quelconques, solides ou fluides, et non pas comme une condition propre à l'état d'équilibre des fluides. Depuis la publication de l'écrit que je viens de rappeler, et auquel Mr. Ivory n'a pas répondu, je n'ai eu aucune raison de changer d'avis. Je pense encore aujourd'hui que personne n'a démontré jusqu'à présent que l'ellipsoïde fût *la seule* figure d'équilibre, excepté dans le cas du sphéroïde peu différent d'une sphère qui a été résolu par M. Legendre. En restreignant la question à cette forme particulière, on peut alors développer le rayon du sphéroïde en série convergente, ordonnée suivant les puissances de la force centrifuge divisée par l'attraction à la surface; et l'équation d'équilibre fournie par les principes ordinaires de l'hydrostatique, suffit pour déterminer successivement autant de termes qu'on veut de cette série. Si l'on s'arrête aux deux 1ers, comme dans la *Mécanique Céleste*, on reconnoit immédiatement qu'ils appartiennent à la figure elliptique; mais il deviendroit de plus en plus difficile de s'en assurer à mesure qu'on pousseroit plus loin les approximations successives; ce qui n'est pas, ainsi que Mr. Ivory paroit le croire, une difficulté qui s'élève contre l'hydrostatique, ni une preuve de son insuffisance; c'est seulement une difficulté d'analyse qui se rencontre le plus souvent quand on fait usage de la méthode des séries. Pour la résoudre, j'ai eu recours à une considération fort simple: j'ai remarqué que l'éllipsoïde satisfaisant dans tous les cas à l'équation d'équilibre, il en résulte que le développement du rayon qui résout cette équation, ne peut appartenir qu'à l'ellipsoïde, quelque loin que la série soit prolongée.

P. S. Le No. 4. du *Philosophical Magazine*, renferme des observations de Mr. Ivory relatives à mon Mémoire sur la vitesse du son qui se trouve dans les Additions à la *Connois-*

* *Annales de Physique et de Chimie*, tome xxvii. page 225.

sance

sance des tems pour l'année 1826. A' cette occasion, j'ai relu ce Mémoire et un autre qui en est le développement (*Annales de Physique et de Chimie*, tome xxiii. page 337); et j'avoue que je n'y ai rien trouvé d'inexact. Mr. Ivory en comparant la formule (6) de mon Mémoire à une équation qu'il a trouvée d'une autre manière, pense qu'elles sont différentes l'une de l'autre, et que c'est la mienne qui est en défaut. J'attendrai qu'il fasse voir en quoi le raisonnement qui m'a conduit à cette formule seroit erronné ou incomplet; et jusques-là je persisterai à croire qu'elle exprime la variation de température qui accompagne une très petite compression ou dilatation de l'air, quand sa quantité de chaleur propre reste la même, ainsi qu'on le suppose dans le phénomène du son, à cause de la rapidité des vibrations du fluide; ou bien encore, comme cela a lieu lorsque l'air est comprimé dans un vase fermé dont la matière n'absorbe pas sensiblement la chaleur.

Paris, le 19 Mai 1827. POISSON.

III. *A Letter to* Professor Airy, *in reply to his Remarks on some Passages in a Paper by* Mr. Ivory. *By* J. IVORY, *Esq. M.A. F.R.S.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I HAVE to beg the favour of you to insert in your next publication the following short letter occasioned by Professor Airy's remarks in your last.

To G. B. Airy, Esq. A.M., Lucasian Professor of Mathematics in the University of Cambridge.

Sir,

I have examined the note in your paper in the Phil. Trans. 1826, but I confess, without seeing much reason to alter my opinion. In my paper to which the note refers, I limited my investigation to homogeneous fluids, expressly setting aside those of variable density as matter for future discussion. Your paper is confined to the case of variable density, and your reference to a law restricted to homogeneous fluids appears inconsiderate and misplaced, and looks as if you had gone out of your way to find fault with what I had written. I do not conceive that Science could in any respect be bettered by the note, and ultimately it will be found that no advantage has resulted from it. You barely assert an opinion without condescending to allege any reason on which it is founded, and I am not aware that such a procedure is very unaptly characterized

terized by the terms I have used. I should be sorry to be found a great trespasser on this occasion; but, for many years, I have been very frequently attacked without rhyme or reason, and with no regard to scruples: and this will, I hope, be deemed a sufficient apology for any warmth of language, more especially to a gentleman who, it appears from his paper, knows full well to do himself justice.

What I read in the last Phil. Mag., p. 447, induced me to examine my investigation in the preceding Number, pp. 327, 328; and I find that I have drawn a wrong inference from my analysis. The evanescent fractions $\frac{h}{g}$, $\frac{k}{g}$ (h, k, g all vanish together with f) are in every case equal to zero; so that my reasoning is entirely favourable to M. Poisson's proposition. As my investigation is free from precarious assumptions, I will briefly state the steps of it, referring to the place cited for an explanation of the symbols. We have,

$$y' = y + \mathrm{A}h + \mathrm{B}k;$$

consequently,

$$\frac{1}{2\pi}\int\frac{gy'ds}{f^3} = \frac{1}{2\pi}\int\frac{gyds}{f^3} + \frac{1}{2\pi}\int\frac{g(\mathrm{A}h+\mathrm{B}k)\,ds}{f^3}.$$

Now, $ds = \sin\theta'\,d\theta'\,d\psi' = \sin\theta\,dh\,dk$; and, the value of ds being substituted, the second term on the right hand integrated as at the place cited, is always equal to zero; so that we have

$$\frac{1}{2\pi}\int\frac{gy'ds}{f^3} = \frac{y}{2\pi}\int\frac{gds}{f^3} = y.$$

Admitting therefore that the proposition is proved in the most general sense, let us examine the consequence.

The development is likewise proved in the most general sense. For there is no objection to the process by which the following formula is deduced from the general theorem, viz.

$$4\alpha\pi a^2 y = \frac{\mathrm{U}^{(0)}}{a} + \frac{3\,\mathrm{U}^{(1)}}{a^2} + \frac{5\,\mathrm{U}^{(2)}}{a^3} + \&c.$$

Now this is the development. For we have,

$$\mathrm{U}^{(i)} = \alpha a^{i+3}\int y'\,d\varpi'\,d\theta'\sin\theta'\,\mathrm{Q}^{(i)};$$

and as $\mathrm{Q}^{(i)}$ is a function of μ, $\sqrt{1-\mu^2}.\cos\omega$, $\sqrt{1-\mu^2}.\sin\omega$, it is plain that $\mathrm{U}^{(i)}$ will be a like function, the coefficients alone being changed by the integrations. The development may always be found, namely, by performing the requisite integrations for every term. We must admit too that it is unique; for every term is a definite integral, involving nothing precarious and contained between given limits. I conclude, therefore, that on the grounds we now go upon, we have,

$$y = Y^{(0)} + Y^{(1)} + Y^{(3)} + \&c. \qquad (A)$$

with all the generality that Laplace and Poisson have asserted.

Supposing that α is less than unit, let us expand the integral expression, thus *,

$$\frac{1}{4\pi}\int\frac{(1-\alpha^2)\,y'\,ds}{f^3} = Y^{(0)} + \alpha Y^{(1)} + \alpha^2.Y^{(2)} + \&c.$$

Here there is numerical equality between the finite expression on one side and the infinite series on the other: for, α being less than unit, the terms of the series continually decrease and finally become insensible. But, when $\alpha = 1$, the principle by which the equality was before proved disappears, and we can no longer affirm that there is an equation. If we suppose α to become ever so little greater than unit, there is a disruption of the continuity, the quantity on one side becoming negative, and the series, on the other side infinitely great. In many cases it is certain that the series, in the circumstances mentioned, is absurd and insignificant in respect of numerical value. How are we to separate such cases from those in which the analysis may be employed as a legitimate means of investigation? It may be argued that the theorem alone is not sufficient; because, in the demonstration the quantity y is considered as finite; and some check in respect of numerical quantity is always requisite when a finite is changed into an infinite expression. How comes it that a series, which is interminable, and in which no principle of convergency has been pointed out, nevertheless represents a finite quantity with numerical exactness?

Suppose that y is a rational function of $\cos\theta$, $\sin\theta$, $\cos\omega$, $\sin\omega$, or of μ, $\sqrt{1-\mu^2}$, $\cos\omega$, $\sin\omega$; and put,

$$t = \sqrt{1-\mu^2}.\cos\omega, \qquad s = \sqrt{1-\mu^2}.\sin\omega;$$

then $$\cos\omega = \frac{t}{\sqrt{1-\mu^2}}, \quad \sin\omega = \frac{s}{\sqrt{1-\mu^2}}.$$

By substituting these values it is evident that y will be converted into a function of μ, s, t; and, by expanding the radicals wherever they occur, it will be changed into an infinite expression which is a rational function of μ, s, t. This expression is unique; for the algebraic operations can be performed only one way. We have now two infinite expressions of y in terms of the same quantities μ, s, t; namely, that (A)

* *Conn. des Tems* 1829, p. 333.

resulting

resulting from Laplace's development, and that obtained by the algebraic process. Each of these expressions is obtained in a manner that is unique: there is nothing uncertain in either; and it must be admitted that they are identical. Taken *in toto*, they both consist of the same simple quantities connected with the same signs; but in one, these quantities are distributed in groups possessed of a general property; and in the other, there is no artificial arrangement. If one be numerically equal to the finite quantity y, the other must be so too; and if the first may be substituted for y in any investigation, so likewise may the second.

It is evident that the radicals expanded in the algebraic value of y produce, converging serieses only; and that, by extending the portions of the serieses taken in, we may approximate to the value of y indefinitely. When $\mu = \pm 1$, the approximation is not disturbed by that part arising from the expansion of $\sqrt{1 - \mu^2}$ and its powers; and the part produced by $\frac{1}{\sqrt{1 - \mu^2}}$ and its powers, which might be infinitely great, vanishes, because every term is multiplied by s, t which are equal to zero when $\mu = \pm 1$. Thus the approximation holds good for all values of μ between the limits ± 1. I therefore conclude generally that every function of two arcs, which is always finite between the prescribed limits, may be changed into a finite and rational function of three coordinates of a sphere, that shall approximate in any required degree to the given function.

There is no difficulty attending this analysis when y is accurately a finite and rational function of three coordinates of a sphere; and we may certainly comprehend in the same conclusion all cases in which we can approximate indefinitely to the value of y by expressions of the same kind. We thus obtain the theory in all its generality, and we place it on its right basis, which is the nature of the development.

I have never found fault with the demonstrations of the theorem, except on just grounds. In the *Mécanique Céleste*, livr. ii. No. 10, the thickness of the molecule at the attracted point is not considered; which occasions a difficulty of which Lagrange has treated: and in livr. xii. the same thickness is made ultimately divisible by the square of its distance from the attracted point; which would limit the analysis to a very particular class of functions. The demonstration of M. Poisson amounts only to this,

$$y = \frac{y}{2\pi} \int \frac{(1 - \alpha^2)\, ds}{f^3},$$

which seems an identical proposition, the quantity into which

y is

y is multiplied on the right-hand side, being equal to unit. But the theorem alone, without considering the nature of the development, cannot be a proper foundation for this doctrine; it does not put the analyst in possession of the true principles of the method. All the difficulties, and, I may add, a great part of the celebrity of this kind of investigation, have arisen from the manner in which it has been presented to the public.

I have no room left to add any thing respecting the equilibrium of fluids. From what you say, I apprehend there is not much between us. It would greatly contribute to throw light on this subject if any one would attempt to demonstrate synthetically Laplace's proposition, namely, That a homogeneous planet in a fluid state, will be *in equilibrio*, when the resultant of the accelerating forces acting upon a particle in the surface is perpendicular to that surface. I am of opinion he would be forced to adopt my conditions of equilibrium, in which case the task is easy; but I maintain that it is impossible he could prove any thing contrary to them. I have the honour to be, sir,

Your obedient servant,

June 12, 1827. J. Ivory.

IV. *On Exceptions to the Law that Salts are more soluble in hot than in cold Water; with a new Instance. By* Thomas Graham, *M.A.* *

THE bodies which have been observed to possess this anomalous solubility are the hydrate of lime and the sulphate of soda: its detection in the first case we owe to Mr. Dalton, and in the latter to M. Gay-Lussac. The phosphate of magnesia, a body like the hydrate of lime of sparing solubility, appears from our experiments to belong to the same class.

To form phosphate of magnesia, phosphate of soda and sulphate of magnesia in crystals were separately dissolved in water, in the proportion of 21 parts of the former to 15·375 parts of the latter, or of an integrant particle of each. These solutions were mixed and set aside. Within twenty-four hours the phosphate of magnesia had precipitated, generally in tufts of short acicular crystals, while sulphate of soda remained in solution. According to Dr. Thomson this salt is composed of

One atom phosphoric acid	3·5
One atom magnesia	2·5
Seven atoms water	7·875
	13·875

* Communicated by the Author.

It

It is *efflorescent*, rapidly losing its water of crystallization when exposed to the air, and falling down in a white powder.

The crystals were drained, purified with great care by repeated agitation with water, and finally thrown upon a filter with more water, and allowed to dry. Solutions were obtained by occasionally agitating the salt with pure water during three or four days, in the proportion of 2 ounces phosphate of magnesia to 1 pint of water. The solutions were then decanted off and filtered. Although the water drained from the crystals upon the filter was nearly tasteless, yet the solutions thus obtained were of a sweetish taste, which was sufficiently perceptible.

A quantity of the solution, in the preparation of which distilled water only had been employed, was gradually heated by immersion in the water-bath. Before the bath had arrived at 120°, the solution became turbid, and it assumed more and more of a milky appearance as the heat increased, till the temperature settled at 212°, when a cloudy precipitate slowly subsided, and the supernatant liquid became nearly transparent. The precipitate was found not to differ in its sensible properties from phosphate of magnesia deprived of its water of crystallization.

To determine the solubility of this salt at different temperatures, a solution was prepared by repeated agitation with water, for more than a week, of a quantity of the salt from which already three solutions had been derived. The temperature was about 45°.

8000 grains of this solution, carefully filtered, were evaporated to dryness on the sandbath. The residue was found to be 10·75 grains anhydrous phosphate of magnesia. Hence 744 grains water dissolve 1 grain of the anhydrous salt.

8000 grains of the same solution, in a glass stoppered phial, were heated to 212° in the water-bath and retained for some time at that temperature. When the precipitate had subsided, a large portion of the transparent liquid was decanted off, and the remainder with the precipitate thrown upon a filter while still hot. It weighed when accurately dried 3·8 grains. Hence 8000 grains water at 212° retain in solution $10{\cdot}75 - 3{\cdot}8 = 6{\cdot}95$ grains; or 1151 grains water retain in solution 1 grain of anhydrous phosphate of magnesia. Hence 1 part water dissolves of anhydrous phosphate of magnesia,

at 45° . . . . $\frac{1}{744}$;

at 212° . . . . $\frac{1}{1151}$.

Of the hydrate, or phosphate of magnesia in the state of crystals, 1 part water will therefore dissolve

at 45° . . . . $\frac{1}{322}$;

at 212° . . . . $\frac{1}{498}$.

The

The precipitate by heat was exceedingly bulky and not crystallized. It did not amount in general to so much as 3·8 grains from 8000 grains of the solution. Indeed the mean of seven experiments made upon different solutions was 2·5 grains of precipitate. But it was found that the amount of the precipitate depended much upon the time and agitation employed in effecting the solution, as it is difficult to saturate water with this salt. It is evident, therefore, that not the mean but the greatest result will approach nearest to the truth.

Phosphate of magnesia boiled in water for several hours, afterwards yielded solutions possessing this property. By the heat, the crystals assumed the appearance in the water of having effloresced.

Phosphate of soda and sulphate of magnesia were added separately to solutions of phosphate of magnesia, in the proportion of 10 grains to 1000 grains solution, without influencing in the slightest degree the amount or appearance of the precipitate.

Phosphate of magnesia appears to be much more soluble in the acids than in water; at least it was observed to dissolve with facility in the following acids, even when in a very dilute state—acetic, oxalic, phosphoric, muriatic, nitric, and sulphuric. The addition of the smallest quantity of any of these acids to the aqueous solution, prevents the appearance of the usual precipitate by heat, by increasing the solvent power of the menstruum*.

In prosecuting this subject I had occasion to make several observations.

Mere continuance of the heat had no effect in increasing the amount of the precipitate, in the solutions of hydrate of lime or of phosphate of magnesia, provided no part of the solution was at any time converted into vapour. When filtered solutions of lime and phosphate of magnesia, which had formerly been heated, were again subjected to a temperature of 212° by complete immersion in the water-bath in close vessels, and retained at that temperature for several hours, no additional precipitate appeared. But when heat of greater intensity was applied to elevate the temperature of the solution to 212°, this was seldom the case. When such a solution was heated by the flame of a spirit-lamp, even in a close vessel, a slight precipitate generally appeared. When the vessel, although

* The experiment of the partial precipitation of phosphate of magnesia in solution, by heat, has been repeated successfully by my friend Mr. A. Steel, in the laboratory of Dr. Thomson, with great care and very pure materials.

close,

close, was only occupied in part by the solution, the precipitate was greater; and when the space occupied by the solution bore so small a proportion to the whole capacity of the vessel, that the solution might be made to boil, and be condensed in the upper part of the vessel and returned without loss,—the precipitate might be increased *ad libitum*, particularly in the case of lime-water. The cause of the precipitate appears to be the same in all these cases. The moment a drop of the solution is converted into vapour, it deposits the quantity of lime or salt that it held in solution; and in the case of bodies which dissolve so sparingly and with so much difficulty, as the hydrate of lime and phosphate of magnesia, although the water be returned again to the solution, it is incapable of re-dissolving what it has deposited. We know that it would be a hopeless task to form a saturated solution of lime, by agitating with the water no more than the few grains which it is capable of dissolving; and in the case before us, when the lime is once deposited the same difficulty should be experienced in taking it up.

These observations show the advantage of employing the water-bath in heating the solutions,—a procedure which was always followed by the author, and by which he regularly obtained precipitates of hydrate of lime as well as of phosphate of magnesia. They also account for a phænomenon in the solubility of lime observed by Mr. Richard Phillips, which otherwise appears anomalous*.

Mr. Phillips heated a quantity of lime-water in a flask, the neck of which was elongated by a tube, to prevent the access of carbonic acid gas from the atmosphere, and made to boil till 1-13th part was dissipated in vapour. From the deposition which mere elevation of temperature would occasion without any evaporation, the quantity of lime in solution would be reduced to $\frac{1}{1270}$th part, but it was found to amount to no more than $\frac{1}{1505}$. But much more of the solution would be converted into vapour during the boiling, than what actually escaped, the cool sides of the long tube being singularly adapted to condense the rising vapour and return it to the solution, supposing that the tube had any elevation; while the hydrate of lime, which had been deposited in hard crystals, would not admit of being re-dissolved in an appretiable degree.

It is evident that this effect of *cohobation* will take place, not only in lime-water and the solution of phosphate of magnesia, but to a certain extent in all bodies of difficult solubility. I have observed it to a considerable extent in the solution of sulphate of lime, even when greatly diluted, and believe

* Annals of Philosophy, N. S. vol. i. p. 109.

lieve that the deposit from slight boiling observed in many mineral waters, and generally attributed to the dissipation of carbonic acid gas, depends in some instances upon this cause. However weak the solution may be, it is evident that a portion of the salt may be deposited in this way.

It had occurred to us as a method of determining the relative solubility at different temperatures of bodies of this class, to form a saturated solution at the lowest temperature, and dilute it with water till it ceased to deposit at the high temperature. But this method was found inconvenient from the difficulty of incorporating the solution with the water added.

4000 grains lime-water were diluted with 2000 grains water, agitated and set aside for two hours. Upon being then heated in the water-bath to 212°, a precipitate appeared, which being received upon a filter and dried was found to amount to nearly 2 grains hydrate of lime. Phosphate of magnesia similarly treated gave 12 grains of precipitate.

4000 grains lime-water diluted with an equal quantity of pure water, and occasionally agitated for three days in a stoppered phial, became slightly turbid upon being carefully heated in the water-bath, and deposited a small quantity of hydrate of lime, of which 0·15 grain was recovered. The solution of phosphate of magnesia in the same circumstances yielded a precipitate, which although it rendered the solution much more turbid, did not amount to so much.

It was found, as might be expected from the previous experiments, that the deposit by heat from lime-water was not diminished sensibly by being allowed to remain in the solution till it became cool, or was not re-dissolved upon cooling. Hence it is unnecessary to filter the solution while hot. The phosphate of magnesia, however, appeared to be re-dissolved in a more sensible degree, probably from the state of extreme division in which it is deposited. At least 2·3 grains of precipitated phosphate of magnesia were obtained by filtering at 212°, while an equal quantity of the same solution, allowed to cool with occasional agitation before filtration, gave a precipitate which did not exceed 2 grains. In appearance, the precipitate had suffered a very great reduction.

The rapidity with which phosphate of magnesia *effloresces* when exposed to the atmosphere, led us, from theoretical considerations, to look for this anomaly in its solubility. Efflorescence in the hydrates of the salts certainly indicates a weak affinity for water at the atmospheric temperature—an attraction or affinity, too, which is much diminished by slight elevation in temperature. If the attraction subsisting between the salt and water, when in solution, be of the same nature as that between

tween the base and water when in the state of a solid hydrate, we might expect the striking power of heat in weakening the affinity or attraction, to affect the solubility of the salt at different temperatures. Even supposing that the solvent power of water increased to a certain extent with rise in temperature, yet this rapid diminution of the attraction of the salt for water as the temperature rose, might counteract and eventually overcome the increasing power of the solvent, in salts so efflorescent as phosphate of magnesia or sulphate of soda. Hence the solubility of such salts might begin to lessen, when the temperature was raised beyond a certain point.

As the hydrates of all the salts, whether they may be efflorescent at the temperature of the atmosphere or not, are decomposed by heat, the cause assigned, as counteracting the increase of the solvent power of water with temperature, if it exists, must be general, and influence to a greater or less extent the solubility at different temperatures of all salts whatever. In fact, the consequence necessarily follows from it, that for every salt there is a point in the scale of temperature above which it ceases to become more soluble in water, and diminishes in solubility. In the case of the efflorescent salts, whose affinity for water, when in the state of hydrate, is much impaired by slight elevation of temperature, this point of temperature appears to be low—in some cases under 212°; in the case of hydrates which retain their water with more force it will be higher, and in hydrates which require a considerable heat to decompose them, the maximum point of solubility will be proportionally high, and such as would require the retention of the solvent in the liquid state by vast pressure, in order to be exhibited.

In that extensive class of salts which do not form solid combinations with water, we do not possess such a clue to their solubility at different temperatures. They may, therefore, be subject in some cases to this anomaly in solubility as well as the efflorescent salts. Indeed the theory is not applicable even to all the hydrates without distinction. There is a class of hydrates, in which the combination between the base and water appears to differ essentially from that of the ordinary hydrates of the salts. This class comprehends the hydrates of the alkalies, the earths and metallic oxides, and these appear not to be subject to the law.

Many salts, oxides and earths of this class are known to be deprived of solubility by exposure to a considerable heat. This arises from the loss of the water with which they were previously combined, and not, as it is often supposed, from the action of heat in hardening and increasing the cohesion of the particles of such bodies. For if we examine the solubility of these bodies, we shall find it necessary to suppose, that at no time is the simple substance itself dissolved, but always an original and intimate compound of the substance with water. These compounds are of a higher order than the common hydrates, and frequently require peculiar circumstances for their formation. Silica is a good instance. Dried and destitute of water it is altogether insoluble, and cannot be made again to form a solid combination with water, but in a state of previous and intimate combination with water it is soluble. It is evident then that the solution should be viewed not as a solution of silica, but as a solution of hydrate of silica. The alkalies are in the same situation; and the fact strikingly illustrates our position, that when the alkalies dissolve in alcohol they are still in the state of hydrates. The combination between water and lime in slaked lime is of this superior kind, so that lime-water may be considered not as a solution of lime, but as a solution of hydrate of lime. The water appears to be in more close union with the lime, than the water of crystallization of salts to which efflorescence is confined. It is therefore no objection to the theory that hydrate of lime is more soluble in cold than in hot water, and yet does not effloresce. Were the hydrate of lime to form a loose compound with an additional quantity of water, like the water of crystallization of ordinary salts, then if that hydrate did not effloresce, the circumstance would be inimical to the theory.

The coincidence of efflorescence with diminished solubility at high temperatures, in the case of sulphate of soda, is favourable to the view taken of the connection between these properties. If the solubility of the efflorescent salts were examined particularly, more of them probably would give indications of the same property.

Carbonate of magnesia in crystals is very efflorescent, and according to Butini * it is more soluble in cold than in hot water impregnated with carbonic acid.

V. *Symmetrical Properties of Plane Triangles. By* T. S. Davies, *Esq. of Bath.*†

[With a Plate.]

THE following properties of Plane Triangles, though intimately connected with a very popular course of geometrical inquiry, and though far from difficult either as to investigation or demonstration, do not appear to have been noticed

* "Sur le Magnesie." *Vide* Thomson's System, under Salts of Magnesia.
† Communicated by the Author.

ticed by mathematicians. The most fertile source, indeed, of the properties of the triangle yet explored, is when it is considered in relation to its circumscribing circle and its four circles of contact: and though this class of relations had always had a place in the theoretical arrangements of geometrical science, yet it owes its present extent principally to the labours and example of Professor Thomas Simpson*. Still, there has been till lately so much attention bestowed upon those relations which result from taking a single side as base, that the symmetrical properties arising from considering all the sides alike, though affording conclusions equally interesting, have not been cultivated in any degree worthy of its importance. A small number of such properties are, however, already known; and the following series will add a few not inelegant ones to the number.

It may be proper here to remark, that some of the most beautiful of the symmetrical properties of the triangle, admit of more simple demonstrations than those commonly given. Let, for example in Plate I. fig. 2. the triangle ABC have its four circles of contact whose centres are e, E, F, G′; let also S signify semiperimeter; and r, radius of inscribed circle; r_1, r_2, r_3, those of external contact.

* The most ample series of properties in which the triangle is viewed in this connection that has yet appeared, is "*The Modern Geometry*," published in "*The Student*," a little work of great merit, which was issued by the late Mr. Hilton, of Liverpool. This work, of which but a very few copies appear to have been printed, is become extremely scarce; and the majority of those mathematicians who know anything of its contents, have transcribed it from the printed copies, and in some cases from MS. copies at *third or fourth hand*. The inquiries have in several cases been successfully followed up in those different periodicals which are principally devoted to mathematics, but in so unconnected a form (which is unavoidable in those works) that they are comparatively little known, and their relations are as little perceived. Besides, excellent as Mr. Hilton's "Modern Geometry" is, it cannot be disguised that the arrangement is extremely defective; as classes of inquiry very different in many respects are combined in the same series of propositions, and referring to the same diagrams. It is likely, therefore, that a very useful work might be produced (even were there but little original matter in it), by giving a systematic arrangement to all the properties at present known, and classing them according to some distinctly marked mode of division, and demonstrating them in the brief form adopted in "The Student." Such a work would much facilitate geometrical reference, and be of great service in a course of investigation, as well as very convenient in stating the authorities for propositions which we have occasion to quote. It is a plan which I have some years formed, and towards accomplishing which I have taken considerable steps: but other pursuits have so often intervened as to much impede my progress, and especially in that very important part,—A minute examination of the different periodical works which have appeared at various times and in several parts of the country. Indeed, it is no easy matter to procure several of them, and often when procured they prove to be of little use.

 Then

Then similar triangles give us

$$\begin{aligned} VA \,.\, Aa &= GV \,.\, Fa, \text{ or } VA \,.\, S = r_3 \,.\, r_2, \\ WA \,.\, AT &= EW \,.\, FT, \text{ or } WA \,.\, S = r_1 \,.\, r_3, \\ GX \,.\, CU &= EX \,.\, GU, \text{ or } CX \,.\, S = r_1 \,.\, r_2; \end{aligned}$$

and therefore by addition of equivalents

$$r_1 \,.\, r_2 + r_1 \,.\, r_3 + r_2 \,.\, r_3 = \{VA + AW + CX\} \,.\, S = S^2:$$

or the sum of the rectangles, under the radii of external contact, taken two and two, is equivalent to the square of the semiperimeter. A theorem due to Professor Lowry, of the R. M. College.*

Again, for the common expression for the area in terms of the sides, we have

$$\begin{aligned} BO \,.\, EW &= eO \,.\, BW, \text{ by sim. trians. } BOe, BWE; \\ WA \,.\, AO &= eO \,.\, EW, \text{ by sim. trians. } EWA, AeO; \\ BW &= BW; \text{ hence by compounding, we obtain,} \end{aligned}$$

$$BO \,.\, WA \,.\, AO \,.\, BW = eO^2 \,.\, BW^2 = (eO \,.\, BW)^2 = \text{area}^2.$$

Also, by similar triangles, we have

$$\begin{aligned} AP \,.\, PC &= ES \,.\, eP, \text{ and} \\ VA \,.\, AC &= GV \,.\, Fa; \text{ whence, compounding} \end{aligned}$$

$$AP \,.\, PC \,.\, VA \,.\, AC = ES \,.\, GV \,.\, Fa \,.\, eP = \text{area}^2.$$

Or, the continued product of the four radii of contact is equal to the square of the area.—Hamilton's Analytical Geometry, p. 45 : or Gent. Math. Comp. No. 22.

To resume,—by sim. trians.

$$\begin{aligned} VA \,.\, AP, \text{ or } BO \,.\, AP &= CV \,.\, eP = r \,.\, r_2, \\ BP \,.\, PO, \text{ or } CZ \,.\, BO &= FP \,.\, eZ = r \,.\, r_3, \\ CX \,.\, CZ, \text{ or } AP \,.\, CZ &= EX \,.\, eZ = r \,.\, r_1. \end{aligned}$$

And by addition of equivalents,

$$BO \,.\, AP + CZ \,.\, BO + AP \,.\, CZ = r\,\{r_1 + r_2 + r_3\}.$$

Also,
$$\begin{aligned} AP : PE &:: Aa : aF, \text{ or } AP : r :: S : r_3, \\ CP : PE &:: CA : VG, \text{ or } CP : r :: S : r_2, \\ BO : OE &:: BW : WE, \text{ or } BO : r :: S : r_1. \end{aligned}$$

Or, compounding,

$$AP \,.\, CP \,.\, BO : r^3 :: S^3 :: r_1 \,.\, r_2 \,.\, r_3.$$

Compounding, again; these analogies in pairs, we shall obtain

$$\begin{aligned} AP \,.\, CP &: r^2 :: S^2 : r_2 \,.\, r_3, \\ AP \,.\, BO &: r^2 :: S^2 : r_1 \,.\, r_3, \\ CP \,.\, BO &: r^2 :: S^2 : r_1 \,.\, r_2; \text{ and hence} \end{aligned}$$

* See Leybourn's Repository, No. 16. p. 5.

$\left\{ \frac{r^2}{AP \,.\, CP} \right.$

$$\left\{ \frac{r^2}{AP.CP} + \frac{r^2}{AP.BO} + \frac{r^2}{CP.BO} \right\} . S^2 = r_2 . r_3 + r_2 . r_1 + r_1 . r_3$$
$$= S^2 \text{ (as before);}$$

or, $\frac{r^2}{AP.CP} + \frac{r^2}{AP.BO} + \frac{r^2}{CP.BO} = 1$;

or, $\frac{1}{AP.CP} + \frac{1}{AP.BO} + \frac{1}{CP.BO} = \frac{1}{r^2}$.

The last three I believe are original; and it is obvious that a similar mode of proceeding is calculated to develop a considerable number of neat properties, as well as to demonstrate very concisely several of those already known.

But I must proceed to the main object of my paper.

Prop. I. Plate I. (fig. 1.)

Let ABC be any plane triangle, and let perpendiculars Ab, BF, Ca be demitted from each angle upon its opposite side, and prolonged to meet the circumscribing circle in k, h, i: then if the triangle $k\,h\,i$ be formed, its angles will be bisected by the said perpendiculars.

For, since the lines Ca, Ab are perpendicular to the lines AB, BC, and the angle ABC common to the two triangles AbB, CaB, the angle BAb is equal to the angle BCa. Hence they stand on equal arcs iB, Bk. But the angles Bhi, Bhk stand on the same two arcs, and therefore they are equal; or the angle ikh is bisected by Bh.

In the same manner, the angles hik, ikh are proved to be bisected by Ca, and Ab respectively.

Cor. 1.—The angles of the triangle abF are also bisected by the same perpendiculars. For each side of this triangle is manifestly parallel to a corresponding side of ikh.

Cor. 2.—Each of the triangles Bab, bCF, and FAa, is similar to the original triangle. For the angles aFB, aCB being equal, their complements aFA and aBC are equal. In like manner it may be shown that FaA is equal to BCA; and therefore the triangle aFA is similar to ABC. And so of the others.

Or this corollary may be thus deduced: Because AaC, AbC are right angles, a circle will pass through AabC; and, therefore, the angles Bab and Bba are equal respectively to BCA and BAC. And so of the others.

Prop. II. (fig. 1.)

A circle described through the feet of the perpendiculars a, b, F will also bisect the sides of the triangle.

For, let the circle cut OB in f, AO in e, and OC in c; AC in D, AB in L, and BC in J. Also, join fD, eL, and cJ.

Then,

Then, since DFf, Lac, Jbe are right angles, the lines fD, Lc, eJ are diameters of the circle abF. They also pass through the middles of the arcs ab, bF, Fa; and are, consequently, perpendicular to the middles of the chords ab, bF, Fa, which respectively subtend those arcs. But ab is also a chord of the semicircle $AabC$; and as fD is a chord perpendicular to the middle of it, it passes through the centre of the semicircle, and therefore bisects the diameter AC. Hence D is the middle of AC.

In the same manner it may be proved that AB, BC are bisected in L and J.

Prop. III. (fig. 1.)

Let O be the point of intersection of the perpendiculars Ab, Ca, BF; then the distance of O from each of the angles A, B, C, is bisected by the circle abF.

For join Le. Then $eLab$ is a quadrilateral in a circle, and the angle eLA is equal to the opposite angle abA. But $aBbO$ is also a quadrilateral in a circle, and therefore the angle abO is equal to aBO. Hence the angle eLA is equal to ABO, or eL is parallel to BO. Consequently we have

$$AL : BO :: Ae : AO :: 1 : 2; \text{ or}$$

AO is bisected in the point e.

In the same manner it appears that f and c are the middles of BO and CO.

Cor.—Let H be the centre of the circumscribing circle, and the perpendiculars HD, HL, HJ drawn; we shall have BO equal to twice the perpendicular HD, AO to twice HJ, and CO to twice HL.

For by the above demonstration De, Le are parallel to CO and BO respectively, and consequently to LH and DH respectively: whence eL is equal to DH. But eL is half BO, or BO is equal to twice eL or to twice DH.

The same reasoning applies to the other stated equalities.

Prop. IV. (fig. 1.)

Let G be the centre of the circle abF, H that of the circle ABC, and O the intersection of the perpendiculars;—these three points G, H, O are in one straight line.

For, since HD is parallel and equal to fO, the lines fD, HO bisect each other in their point of intersection, or HO passes through the middle of fD, the diameter of the circle abF, and therefore through its centre G.

Cor. 1.—The centre G of the circle abF is midway between O and H.

Cor. 2.—It is known (Bland's Geometrical Problems, sect. iii. Pr. 24.)

Pr. 42.) that the centre of gravity of the triangle is also in OH. Whence *four* important points belonging to the triangle are in one line.

Cor. 3.—The diameter of the circle abF is half the diameter of the circle circumscribing the triangle ABC.

For, join LJ, JD, DL. Then this triangle is similar to the triangle ABC and has half its linear dimensions. Hence the diameter of a circle about LJD (viz. the circle abF by Prop. 3.) is half the diameter of that about ABC.

Prop. V. Plate I. (fig. 2.)

Let ABC be a triangle; e the centre of its inscribed circles; E, F, G the centres of the circles which touch the sides *externally:* and let M, b, s be the external points of contact of the circles with the sides *unprolonged.* Then the radii Es, Fb, and GM meet in one point.

For, as is taught in most elementary books,

$$\mathrm{A}s^2 + \mathrm{C}b^2 + \mathrm{BM}^2 = \mathrm{S}c^2 + b\mathrm{C}^2 + \mathrm{MA}^2; \text{ and}$$

therefore by the converse of a well-known theorem (Bland *, Prop. xxxii. sect. 4.), the perpendiculars from M, s, b meet in the same point d; and these are identical with the radii of external contact, which radii therefore meet in one point d.

Prop. VI. (fig. 2.)

Let the radii of external contact which meet the sides *prolonged*, also meet each other in three points (that is say, let

GV, EW meet in k,
GU, FT meet in l, and
EX, Fa meet in m);

then d is the centre of the circle described through E, F, G.

Since GE bisects the angle BAV, and its vertical angle CAW; and in the triangles GVA, GMA the angles GVA, GMA are right angles, the remaining angle VGA (or hGE) is equal to the remaining angle MGA (or dGE). Also, because Gk Ed are both perpendicular to AC, they are parallel to one another; and because both Ek and Gd are perpendicular to AB, they are parallel to one another. Whence the figure GkEd is a parallelogram; and having the angle kGd bisected by the diagonal GE, it is a rhombus.

The like course of reasoning will show that GdFl and dFmE are also rhombi.

The consequence is, that Gd, dE being sides of a rhombus

* The converse of this proposition in Bland is not necessarily true, except in case of the triangle; but it is to the triangle we apply it. It is too simple to need formal demonstration here.

are

are equal, and that Ed, dF being sides of a rhombus are equal; and hence that dG, dE, and dF are equal, and d is the centre of the circle which passes through E, F, G.

PROP. VII. (fig. 2.)

The figure GkEmFl is an equilateral hexagon whose sides are parallel.

The first part of this theorem follows immediately from the last; the three rhombi which meet in d being equilateral, and the hexagon in the proposition being composed of the exterior sides of these equilateral rhombi.

The hexagon has also its opposite sides parallel, for each pair of opposite sides is respectively perpendicular to the same line; viz. one pair to one line, another to another, and the third to a third—(Gk and Fm to AC, Gl and Em to BC, and Fl and Ek to AC)—whence these three pairs of opposite sides have the stated property of parallelism.

PROP. VIII. (fig. 2.)

The triangle klm is equal in all respects to the triangle EFG, and the sides of the one are parallel respectively to the sides equal to them of the other.

For, Gk is equal and parallel to Fm (Pr. 7.); and therefore the line km is equal and parallel to GF. In the same manner it appears that kl is equal and parallel to EF, and lm to GE. Hence the two triangles have their sides equal and parallel each to each.

PROP. IX. (fig. 2.)

Let the points of contact of the inscribed circle be O, P, Z; its centre e is the centre of the triangle klm.

For, since GlFd is a rhombus, the line dK is perpendicular to GF, and dl is equal to twice dK; but Ee is also equal to twice dK (Cor. Pr. III.). Whence dl is equal to Ee.

The lines dl and Ee are also parallel, being both perpendicular to GF. Hence le is parallel and equal to dE.

By similar reasoning it can be proved that dG is equal to em, and ek to dF. But $d\mathrm{G} = d\mathrm{E} = d\mathrm{F}$ (Prop. 6.); and, therefore
$$le = me = ke;$$
and hence e is the centre of the circle klm.

PROP. X. (fig. 2.)

Let eE meet the circumscribing circle in I, eF in Y, eG in N, GF in K, GE in Q, and EF in R. Then, first, the diagonals dl, dm, dk of the rhombi pass through K, R, Q.

For the circle ABC passing through the feet of the perpendiculars bisects the sides FG, GE, EF in K, Q, R. But these sides

sides are the diagonals of the rhombi $dFlG$, $dGkE$, $dEmF$; and consequently the other diagonals dl, dm, dk, also bisecting them, pass through the points K, Q, R.

Secondly, The sides of the triangle klm will pass through I, Y, N and be bisected there.

For, KI being perpendicular to AC, it is parallel to Gk and Fm, and it is also equal to them. Hence kIm is one straight line, and it is bisected in I.

Cor. 1.—The side of the hexagon $EmFlGk$ is equal to the diameter of the circle circumscribing the triangle ABC.

Cor. 2.—The lines le, eP coincide in direction, both passing through e and being perpendicular to AC. Hence the radius of the inscribed circle Pe passes through l; as do likewise the radii Ze, Oe through k and m.

Cor. 3.—Let the triangle klm meet the circle ABC again in p, o, n; then kd, ld, md will pass through these points.

For ld is perpendicular to GF, and GF is parallel to km; hence ld is perpendicular to km. In like manner md is shown to be perpendicular to kl, and kd to lm. Hence ld, md, kd are the three perpendiculars from the angles k, l, m, upon the opposite sides of the triangle klm. But the circle ABC bisects the sides km, ml, lk of this triangle (Pr. X. part 2); and therefore (by Pr. III.) it passes through the feet of the perpendiculars; or, ld, md, kd pass through o, n, p.

Prop. XI. (fig. 2.)

Join op, pn, no. These lines shall form a triangle whose sides are respectively parallel and equal to those of the primitive triangle ABC.

Because $KoIB$ is a rectangle (Ko and IB being perpendicular to the two parallels GF, km), the two sides KB, Io are equal. Also, because KF is equal to kI, the two remainders BF and ko are also equal to one another.

Similarly we find kn equal to FC. And because the angle BFC (or GFE) is equal to the angle okp (or mkl), the side on is equal to the side BC.

Again, because no, BC make equal angles with the parallel lines km, GF, they are parallel to one another. In like manner it will appear that op is parallel and equal to AB, and np to AB.

Prop. XII. (fig. 2.)

The point d is the centre of the circle inscribed in the triangle nop.

For no being parallel to BC, op to AC, and ol to BE, the angle nol is equal to EBC and pol to ABE. But the angle EBC is equal to ABE, and therefore nol is equal to *p o l*; or *n o p* is bisected by the line *o l*. In the same way it may be shown that *o n p* and *n p o* are bisected by *m n*, *k p*; and hence the intersection *d* of these lines is the centre of the circle inscribed in *n o p*.

PROP. XIII. (fig. 2.)

The centre H of the circle circumscribing any triangle ABC is equidistant from the centre *e* of the circle OPZ inscribed in the triangle, and the centre *d* of the circle FGE passing through the three centres of the circle of external contact.

For *d* being the centre of the circle EFG, and *e* the intersection of the perpendiculars from its angles upon its opposite sides, it follows from Pr. IV. and its corollaries, that if *e d* is bisected in H, the point H is the centre of the circle ABC. Whence the proposition is obvious.

Cor.—It is thence also plain that if ABC be any triangle whatever inscribed in a given circle, the circle through the three centres of the external contact is of an invariable magnitude, viz. having its diameter double of that of the circumscribing circle.

With these remarkable properties of the triangles ABC, EFG, and their respective *reciprocals*, *n o p*, *k l m*, I shall close the present paper. I intend, however, so soon as I can command sufficient time to arrange my materials, to select from the memoranda which have been accumulating upon my hands for some years, two or three other series of propositions, which I flatter myself will not be altogether uninteresting to geometers.

Bath, Jan. 15, 1827.

VI. *On the Semi-arcs of Vibration of a Clock with a Dead Beat Scapement and Deal Pendulum. By* THOMAS SQUIRE, *Esq.**

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

IN a paper dated Dec. 15, 1824, and which appeared in vol. lxv. page 38. of the Philosophical Magazine, I have made mention of having registered several hundred observations of the semi-arcs of vibration of a clock with a dead beat scapement and deal pendulum. Since that time I have considerably extended this series; and from which it appears that these arcs are at times so unaccountably variable, that for this reason I am induced to trouble you with some remarks

* Communicated by the Author.

marks on the subject, hoping at the same time that they may be the means of calling the attention of your readers to this curious circumstance, and which appears to be general in a greater or less degree, under certain atmospheric variations; and is moreover too important to be overlooked, in every case where great accuracy is required.

These variations in the supplementary arc of vibration, are in many cases of much greater consequence to the accurate going of a clock, than theory would seem to point out; and what is worse, perhaps no two clocks can be found, that, under the same circumstances of angular vibration, would have their rates affected in an equal degree; as the thickness and length of the suspension spring, the nature and accuracy of the scapement, the weight of the pendulum, and the power applied to keep the clock in motion,—all have a tendency, more or less, to affect the result.

With a given power, weight of pendulum, and dead beat scapement, might not a suspension spring be made of such a length and thickness as perfectly to neutralize the effect on the rate, which would arise from an increase or decrease in the arc of vibration?

Although I have in the paper above alluded to given some account of the clock on which these observations were made, yet perhaps it may not be amiss to say something more in this place respecting it.

This clock has a very perfect dead beat scapement, and the train is of the very best workmanship; the balance-wheel is of brass, and the pallets of steel: there is no jewelling in any part. The pendulum weighs twelve pounds, and is so delicately hung, that a very small maintaining power at the pallets is sufficient to keep it in motion; there are two tangent screws for adjusting the beat, and a graduated nut and index for regulating the rate.

Now from the observations which I have made on the vibrations of the pendulum of this clock, I have found that they are at times regular, or nearly so, for several days together; and on the other hand, they are frequently very irregular, varying several times in the day, and that too, without any apparent cause; but in general I have observed the arcs most variable, when much *cirri*, *cirrostrati*, and highly electrified *cumuli* appear in the atmosphere, and also on the approach of stormy weather; yet this is not always the case, for sometimes the pendulum has a very unsteady motion even in fine and serene weather. I have also found that the same anomaly takes place in clocks with recoiling scapements, and probably in others of the most highly finished workmanship, a circumstance which seems not to have attracted that notice it really deserves.

I will now copy a few observations from my journal; and I may just remark, that in the following table, the hour of the day is according to mean solar time, civil account, and that the thermometer and hygrometer are hung inside the clock-case.

Table of Semi-arcs of Vibration, &c. Power $3\frac{1}{2}$lbs.

Month and Day.	Hour.		Semi-arc of Vibration.		Thermometer.	De Luc's whalebone Hygrom.	State of the Weather.
Sept. 17.	8^h	20^m	2°	$8'$	60	61	Bright, yet sultry, max. temp. of external air 70°. Bar. sinking, wind E. ∖. ∖– and ⌒.
	9	35	2	0	60	61	
	11	0	2	3	62	61	
	14	45	2	1	67	60	
	19	35	2	6	67	61	
18	8	20	2	5	64	61	Showery, wind N.E. and rather brisk. Heavy rain at night.
	12	0	2	9	65	61	
	13	0	2	3	65	61	
	14	0	1	58	65	62	
	17	0	2	5	65	62	
	21	45	2	8	65	62	
19	8	30	2	11	64	62	Fair, bright & warm, max. temp. 69°.
	20	0	2	6	69	62	
20	8	16	2	9	66	62	Rain A.M., afterwards fair and windy.
	11	0	2	11	66	62	
	14	15	2	16	66	62	
	21	15	2	18	66	62	
21	7	35	2	10	61	62	Fair, and mostly bright.
	12	0	2	13	61	61	
	14	10	2	10	63	62	
	21	0	2	0	61	61	
22	8	0	2	15	56	61	A cloudless sky, some wind middle of the day.
	11	36	2	18	58	61	
	15	0	2	12	63	61	
	20	25	2	1	61	61	
23	7	0	2	7	56	61	Fair, with drying winds.
	21	0	2	5	61	61	
24	8	10	2	2	59	61	Early A.M. dark ∖–, rain middle of the day; dark ⌒ W. with light^g and distant thunder.
	16	35	2	13	63	61	
	19	0	1	57	63	61	
	20	35	2	6	62	61	
25	7	45	2	4	61	62	∖–, ⌒ and ⌒–.
26	14	0	2	11	67	62	Mostly fair.
27	9	15	2	4	64	64	Some rain early A.M. afterwards fair and mostly bright.
	12	0	2	7	66	62	
	17	10	2	4	68	62	
	20	20	2	8	67	62	

I shall now advert to the valuable series of observations made by Col. Beaufoy in 1819, 1820, 1821 and part of 1822, on the going of a clock with a straight-grained deal pendulum rod. From these observations I am led to believe that the small and sudden irregularities in the rate of this clock are not referable to changes in the length of the pendulum, but to those in the arc of vibration: for had the changes of the atmosphere affected the length of the pendulum, whilst the arc of vibration remained constant, the clock would have gradually gained upon its mean rate as the summer advanced, and lost as the winter approached, which does not seem to have been the case from these observations. So that we may conclude this pendulum rod to have undergone little or no change during the period of three years and upwards that the daily rate of this clock was registered.

But if we take the differences of each succeeding daily rate, as given in the tables for the deal pendulum, and compare them with like differences of the daily rate, as found by a subsequent series of observations made also by Col. Beaufoy in 1824 and 1825, with a teak pendulum, we shall find that in the latter year, though the clock gradually gained from the beginning of March, when its daily rate was about two seconds, to the 6th of August, when its daily rate was 9·5 seconds; yet the *maximum* difference of the rates with the teak pendulum, only exceeded that of the deal pendulum ·31 of a second.

From the great increase of the daily rate (as observed above) during the spring and summer months, we must infer that there was a contraction of the teak pendulum, and which came to its *maximum* about the beginning of August: nevertheless the rate was not much affected by this cause, from day to day, as the second differences are not found to be more dissimilar than those obtained from the observations made with the deal pendulum.

As it is possible to render the deal pendulum perfectly constant under all changes of the atmosphere,—which is all that is wanted in this respect, for the purpose of accuracy; it is therefore unnecessary to have recourse to the more expensive mode of compensation, which from the uncertainty in the ratio of expansion of the different metals of which the gridiron and similar pendulums are composed, together with the nicety required in the adjustment of the different parts, so as to insure a perfect balancing of effects in the centre of oscillation, must be a work that requires more time and attention for insuring a requisite degree of accuracy than is usually bestowed on such occasions: whilst after all, if the arc of vibration be in a state of fluctuation, a doubt may arise whether to ascribe the

the variation in the rate to this cause, or an imperfect compensation. Since (as has been observed before) a pendulum of deal may be made constant under all the vicissitudes of the seasons, there is therefore nothing remaining but to render the arc of vibration also constant, with a given maintaining power in all cases of atmospheric influence; and then we shall be able to give to that useful machine the clock, that equable motion which constitutes its intrinsic value, for measuring time with accuracy.

I remain, Gentlemen,
most respectfully yours,
THOMAS SQUIRE.

Epping, Dec. 14th, 1826.

VII. *On Mechanical Science.**

IT is a very common opinion that there is an essential difference between the principles of Geometrical and of Mechanical science. The former is supposed to be independent of matter, the latter to be entirely dependent on it. Geometrical science is imagined to be derived without aid from experience, while mechanical science is said to be founded wholly on observation and experiment. These notions appear to have had much influence in retarding the progress of pure science, by limiting its extent, and causing the important doctrines of mechanics to be mixed with matters of mere observation; that is, instead of a general and unlimited investigation of the relations that might exist among forces, times, and spaces, it is confined to those relations only which are observed to take place in the ordinary natural phænomena; a few cases excepted, which are evidences of the general nature of the science.

In no instance have I seen a more distinct announcement of this narrow opinion regarding mechanics than in the excellent discourse which commences the "Library of Useful Knowledge" now publishing for the diffusion of science, and having an equal feeling of interest for the progress of pure and of practical science. I shall not scruple attempting to show that those opinions, which are not peculiar to that work, are not perfectly correct.

It will be admitted that an idea of a third dimension is necessary to geometry. Now I contend that the mind cannot obtain this idea without a previous one of power; and that it is simply power acting unresisted, which gives us our knowledge of space as being bounded by a figure of three dimensions. Deprive the mind of the aid of power, and it must become wholly incapable of conceiving the idea of space, or figure of three dimensions; for the organ of sight gives no

* Communicated by the Author.

aid

aid in conceiving outward distance or space; it is the power of unresisted motion alone which enables us to conceive it, while resisted motion renders us sensible of the presence of matter.

It may be said that by means of sight we can observe figure, and consequently form an idea of the boundary of figure, on a flat surface; and external motion adds the idea of variation of distance and of figure; but our own motion and feeling of power is essential to our perception of distance from us, and consequently to the perception of the idea of space.

Now since the idea of power, and its effect—motion, precedes the idea of space, the science which treats of the possible circumstances and relations under which power may operate, is as much a pure and elementary science as that which treats of space and figure, and it will be found that it may be followed to an equal extent without reference to matter.

If example be required of the possibility of a science of mechanics being formed independently of nature and natural phænomena, I would quote the doctrines of fluxions * as a partial attempt to form such a system; and if ever its first principles be clearly illustrated, it will be from the consideration of forces and velocities. The first book, and the greater part of the second, of Sir Isaac Newton's "Principles of Natural Philosophy," are of this species of mechanical science,—not quite so detached from existing nature as our systems of geometry; but not the less capable of being detached, generalized, and made more strictly systematic. The advantages of such a system would be similar to that of the elements of geometry. Natural philosophy and the practical application of science may then be formed by the reference of matters of experience and observation to the principles which solve them, as in the third book of Newton. Recent treatises have been formed only for the purposes of explaining natural phænomena; but even for this purpose a system of pure science, perfect in itself, would be vastly superior.

* I see with regret that my countrymen are gradually abandoning the Newtonian notation, and even the name which was bestowed on this doctrine by its great author; and that they are adopting both the name and notation given by not more than the second inventor; when it is a fact which cannot be denied, that both the name and notation of Leibnitz convey a false idea of the nature of the process. A small argument is raised in favour of the change, amounting to, I believe, one solitary instance of infrequent occurrence when the Newtonian notation is not perfectly convenient; while the glaring absurdity of using a representative of quantity, for a characteristic mark, is entirely overlooked.

Would any other nation on the face of the earth except this have paid such a tribute of respect to such a genius as Newton ! ! !

It has been usual to refer to motion, instead of force or power; but this is clearly beginning at the wrong end, motion being merely the result of the action of unresisted power. Our sense of feeling informs us that motion is a consequence of our power being exerted so as to exceed the resistance.

Force, denoting the intensity of the action of power, may be considered to flow equably or unequably with time; it may also be considered in relation to space, and to space and time conjointly. The variation with regard to time, I do not recollect having seen considered. Natural phænomena should only be looked into for suggesting branches of research, not as the objects and end of them; and whatever advantage it may give to ordinary readers, their use in illustration should be avoided; the Elements of Euclid in this respect should be an example to imitate.

Might not the sciences relating to the mind equally profit by attending to the mode by which we obtain our ideas of motion? Could the system of Hume stand the test of such an inquiry? even with the ponderous support which Brown has endeavoured to place under it? Have we not as distinct a perception of the possession of power as we have of the order of sequence? The eye and the ear are not the only organs of sense capable of affording us correct ideas; indeed, neither of them could inform us of the existence of matter; and besides, these organs themselves, when examined, appear to convey impressions to the mind by an operation analogous to that which conveys the perception of feeling, showing that no impression whatever is transmitted to the mind except by the action of force, or power. I will go a step further, and question the possibility of our knowing of the existence of any part of the external world, unless we can either touch it directly or indirectly, by the intervention of a medium between it and our organs of sense.

He who reasons respecting events by the order of sequence, confines himself to an imperfect view of the subject. We must advance another step, and inquire by what means forms of matter endure: it then becomes necessary to consider causes and effects as they exist, and not as they succeed one another; for they must exist together, to constitute a cause and an effect; the order of events being only the circumstances necessary to vary causes in their action.

He who began his researches by saying, "I think, and therefore I am," might with still greater propriety have said, "I have power, and therefore I am:" and he certainly indicated a true doctrine in referring to the human mind itself for the first elements of sound philosophy. D——T.

VIII. *A further*

VIII. *A further Account of the great Mass of Native Iron of Bitburg. By Messrs.* Steininger *and* Nœggerath: *with Observations, by Dr.* C. F. F. Chladni*.

THERE is no doubt that the mass of native iron found in the vicinity of Bitburg, near Treves, weighing from 3300 to 3400 pounds (mentioned in this Journal for 1825)†, which was afterwards through ignorance melted down, was of the meteoric kind; since Colonel Gibbs, of New York, has found it to be similar to other meteoric iron, and to contain nickel. For the first intelligence of the discovery of this iron I am indebted to my very worthy friend Dr. Nœggerath, of Bonn, who after my communication respecting the further investigations given on this subject in the American Mineralogical Journal, examined it more closely on the spot, caused the melted mass—which had been buried, on account of its unfitness for being wrought, and from a fear of its giving the establishment a bad name—to be dug up again; and he was kind enough to distribute some fragments of it to myself and other individuals. The original crystalline structure had been destroyed by the process of melting the mass, so that by etching a polished surface, no regular figures either did, or could appear; nevertheless there appear to be traces of its former crystalline structure on some of the projections and small imperfectly melted lumps. Professor Bischof and Counsellor Karsten have also found nickel in it: Counsellor Stromeyer, 81·8 of iron, 11·9 of nickel, 1·0 of cobalt, 0·2 of manganese, 5·1 of sulphur; = 100·0: and Professor John, 78·82 of iron, 8·10 of nickel, 3·00 of cobalt, 4·50 of sulphur, 0·08 of silicium, 5·50 of silica, alumina, oxide of iron, as well as a trace of selenium,—differences of analysis which may have been produced in part by the agents used in the process. Counsellor Stromeyer intends to examine this iron for chrome, which he has not yet done. It was however regretted that with the exception of some fragments that might perhaps be found in America, none was left for investigation in its natural state. Having therefore learned that some of it was still extant in a collection at Treves, I wrote to M. Steininger, (teacher of natural history and mathematics in the Gymnasium of that city,) a gentleman distinguished by several works he has written on the geology of the country along the Rhine, &c. and especially by his knowledge concerning the former volcanos of the Eifel.

* From Schweigger's Journal, N.R. Band xvi. p. 385.

† See Phil. Mag. vol. lxv. p. 401.

He was also kind enough to lay my request before the Society for Useful Investigations, in whose possession those fragments were, and to send me, with their consent, some parts of them, together with an earlier essay of his, on the subject, the publication of which he left to my option.

M. Steininger writes:

"After the communications made by Messrs. Bischof and Nœggerath in Bonn, concerning the problematic meteoric mass of Bitburg in Schweigger's Journal, it may not be uninteresting to state that two pieces of this mass in its natural and unaltered condition are still extant in the cabinet of the Society for Useful Investigations at Treves; which, being sufficiently large to show the original physical characters and external appearances of that mass, will in some measure console us for the loss sustained by science of the mass itself, which was melted down at the forge of Pulwig.

"At first sight they appear like a tolerably pure kind of iron, the produce of art; and I also find the pieces at Treves, which had been presented by Dr. Schmitz, of Hillesheim, in the Eifel, to the late dean H. Castello, to have been marked in the latter gentleman's catalogue of minerals, as problematic meteoric native iron; a designation which was subsequently erased, with the observation that this mass had been produced by art,—and this opinion was founded on the supposition of a distinguished mineralogist.

"The specific gravity of the pieces at Treves, is 6·14 in a temperature of about 61°·25 Fahr.; and to judge by the impressions the fragments received on being knocked off, and the experiments made on a third fragment at Treves, they are rather of a tough nature. They are much corroded and perforated with holes, and the small cavities are in part covered with oxide of iron, and lined with small grains of quartz, some of which may be distinguished by the naked eye. But it may be seen by means of a lens, and in parts acted upon by an acid, that these grains of quartz are more or less mingled with the whole mass, which in many parts has the appearance of peroxide of iron, and has only a metallic glimmering lustre in detached places, but on the whole has an earthy look and dense structure. In parts where the mass of metal is pure, it is either hackly or granular. The colour of the larger metallic granules is a bright white, and their brilliancy perfectly metallic. They do not seem to be oxidated readily in atmospheric air, as they appear quite fresh although exposed to it for many years. In some parts may be recognized through a lens, a black dross with small cavities in it, the sides of which

are

are vitrified, and the pure metallic mass seems only to form a sort of skeleton, in which the oxidated mass, mixed with granules of quartz, is distributed.

"In muriatic or nitric acid, even when heated, the mass dissolves slowly; but in aqua-regia, quickly, and with a development of nitrous acid gas. Sometimes I observed during the process of solution, that a turbidity was occasioned round the metal, and yellowish white flakes came off it. The surfaces of the metallic pieces then showed in some parts the colour of iron-pyrites, although the flakes were too small in quantity and number to allow me to collect and distinguish them separately; and I am of opinion that they clearly indicate the presence of sulphur in the mass. For the rest, the mass of metal was completely dissolved, with the exception of the grains of quartz intermixed with it.

"By separating the oxide of iron from the solution by saturation with caustic ammonia, the solution will pass colourless through the filter. But if it is condensed in a watch-glass, it assumes a greenish colour, and on being completely evaporated, leaves a grayish brown residuum. By volatilizing the sal ammoniac, the residue turns into a yellowish red; and at one end the colour turned to a greenish yellow. The residuum still somewhat warm, heated with a drop of caustic ammonia, crumbled down and dissolved, assuming different hues, at first blue, then violet, and at last bright red, forming however a blue solution. When evaporated, this solution left a residuum of a pomona green with a somewhat brownish mass in the centre. A drop of caustic ammonia rapidly dissolved the green border, turning the colour of the brown mass in the centre, first into a bluish, and subsequently into a reddish violet*, producing also a brown powder, but finally completely dissolving it.

"I now again caused the ammonia to evaporate, poured on the residuum a solution of chlorine, caused it to evaporate in its turn, and brought the residuum, now nearly red hot, in contact with ammonia, by which the green oxide of nickel was dissolved, and a brown residuum was left. Collecting this residuum on a filter, and adding some water to the fluid, after it had passed, white flakes, subsequently turning brown, were produced, which showed also the presence of manganese, besides the nickel. I then dissolved the residuum on the filter by means of nitric acid, and added cautiously some ammonia to the solution, which remained perfectly clear. I again made the nitro-ammoniacal solution evaporate, dissolved the brown

* "A circumstance which seems to indicate the presence of cobalt, pure manganese not furnishing these appearances."

 oxide

oxide in muriatic acid, and added prussiate of potash to the solution, whereby I obtained the same result as is obtained from the ferriferous oxide of manganese of Wadern, under similar circumstances; viz. a white precipitate is formed slightly of a prussian-blue tint, yet so that the white substance may be distinguished as the precipitate of the solution, which is at first of a greenish, and afterwards of a dark blue colour, and continues so distinguished till it is mixed with the rest by a continued stirring; at the same time pellicles of a purple violet show themselves on the surface of the blue liquor. I entertain, therefore, no doubt but that the meteoric mass of Bitburg contains manganese.

"I could, however, discover neither lime nor magnesia in the solution of the meteoric mass, freed from oxide of iron. It appears therefore that metallic oxides only constitute the scoria mixed with it.

"Thus the mass of Bitburg would appear to consist of iron, manganese, nickel (cobalt?), and sulphur, with a considerable intermixture of grains of quartz.

"Would it not appear from the latter constituent that the mass in falling to the ground was in a state of fusion, and became mixed with those grains of quartz on the surface only."

Thus far M. Steininger.—On exhibiting the fragment of the Bitburg iron to Dr. Nœggerath, during my residence in Bonn, he was kind enough to communicate to me the following observations for publication.

"There can be no doubt of the genuineness of the fragment of meteoric iron received by my friend Dr. Chladni from M. Steininger, of Treves, even with respect to the spot where it is said to have been found; since it came from Dr. Schmitz at Hillesheim, the same gentleman from whom I received, in 1814, the first verbal account of the existence of this mass near Bitburg.

"The rough condition of the fragment shows that the mass had been filled with cavities; a circumstance which may also have caused the comparatively low specific gravity of 6·14, mentioned by M. Steininger.

"The melted mass, although still porous, according to the experiment made by Professor Bischof and myself, has a specific gravity of 6·859. The irregular cavities formed by a less perfect ramose form than in the Siberian meteoric iron, show on their surface but few shining metallic points, on which the pure metallic mass appears; most of it is black, in part rough and dull, or smooth and of the ordinary lustre. The surfaces of the cavities combine the character of the crystalline with that of the melted substance. One may distinctly perceive

ceive detached surfaces, angles, &c. of crystals no longer definable, but probably octahedral, which cover the surfaces of the cavities in groups, and are partly melted themselves, and covered over by a scoriaceous substance. The latter forms the black dull surface, which is perceived in the above-mentioned state of brilliancy, wherever it became attached during the original fusion in a purer form, and in the shape of small hemispheres.

"The light-grayish-white substances which appear either singly or in small groups incorporated in the surface like splinters in the shape of small angular grains, appear to me to be too soft for quartz, and may perhaps be the same granular body as that which forms the principal part of meteoric stones. There are, however, too few of them in the fragment before me, to allow of any certain decision on this point.

"The pure meteoric iron of Bitburg itself is somewhat lighter in colour than the melted mass. It is malleable and softer than the latter; it may be splintered a little with the knife. It is possible that this difference arises from the unmelted substance having no *admixture* of sulphur, which may have been added during the fusion by the intermixture of pyrites. There is, however, no appearance of such a mixture in the specimen before me: perhaps the mixture was not equal throughout the mass, from which circumstance it may be entirely missing in some fragments; a supposition that seems to be confirmed by the great differences in the quantity of sulphur found in the various analyses made of the melted mass of Bitburg."

I beg now to add a few observations to the above.

I assume as a fact, that all masses of native iron which may actually be considered as meteoric, must, as far as we are acquainted with them, be divided into two principal classes*: viz. 1st, The ramose specimens, the cavities of which are filled with a substance resembling olivine or chrysolite; and 2ndly, The solid specimens, the structure of which is for the most part crystalline. The mass of Bitburg belongs to the second of these classes. Several other masses of this kind show, like

* I saw many years ago, when I was less acquainted with masses of native iron, a piece which I do not know what to think of, at Würzburg, with the late estimable Bonavita Blanc, whose collection has been since acquired by the University of that city. If I recollect it right, it had a flattish round form, and was of about two ounces in weight, and seemed a rough mixture of a gray mineral, more resembling some kinds of meteoric stone than olivine and meteoric iron, and unlike any other kind of native iron I had ever seen. It will probably be found in the cabinet of minerals of the university of Würzburg, by the side of a fragment of the Siberian mass; and it would be very desirable that some one should undertake to investigate it.

like this, greater and lesser cavities, on the surface of which some crystalline appearance is found; but never, as far as I know, a filling-up of olivine or any other mineral substance. Among all the fragments of meteoric iron in my possession, or that I have ever seen, I found none so greatly resembling the fragment of Bitburg described here, as that from the Montanna de Santa Rosa in Colombia, first made known by Mariano de Rivero; a piece of which, weighing several ounces, was presented to the university of Berlin by de Humboldt, and of which I possess a fragment of about three drachms in weight. They fully correspond as to colour, softness and porosity. On a smooth and etched surface of my fragment of the Colombian metal, no regular figures are seen, but only a slight indication of a crystalline structure; nor would they, as I think I may conclude from its external appearance and porous state, appear in the fragment of the Bitburg iron: nor indeed can it be expected from such porous pieces of the extremities of the mass; but certainly towards the interior, where the structure is closer and more crystalline.

Concerning the grains of an earthy substance found in the cavities of the Bitburg iron, either singly or in aggregates, I imagine that they may have existed before its fall, and have an affinity with meteoric stones.

The difference of the quantities of sulphur in natural and melted pieces, may originate from a mixture of iron pyrites with the latter; and also from the circumstance that in several masses of native iron, the sulphureous iron is added to the remaining mass in distinct parts, as I have shown in the *Annalen der Physik* for 1822, which sometimes render it difficult to saw, and may cause a difference in the quantities of sulphur found in the analyses.

IX. *On Meteoric Iron from Mexico. By* Dr. J. Nœggerath. *In a Letter to* Dr. Chladni*.

Bonn, 25th of June 1826.

I AM now enabled to give you a better account of the place where that meteoric iron was found which I received during your late visit to Bonn, and of which I had a small fragment cut off for you. It was sent to the directors of the German-American Mining Association at Ebberfeld, accompanied by a letter from their principal agent in Mexico, Mr. W. Stein.—Mr. S. expresses himself in the following terms:

* From Schweigger's Journal, N. R. Band xvii. p. 74.

"Among

"Among the minerals which you will receive with the present letter, is a piece of native iron from Jiquipilco, ten leagues north of Toluca. The existence of this iron, of which as yet nothing is known, deserves to be further investigated. For myself, although I have taken great pains respecting it during my journey to Jiquipilco, I have not been so fortunate as to find any of it in its original situation. It is however known, that a rather large quantity was formerly found there in ploughing the fields, and that it was used for various instruments and tools. The piece in question was given to me by a North-American of the name of Gould, who had found it on the spot."

This account therefore confirms generally what you have said respecting the place where this iron is found, in your larger work on Igneous Meteors, (Vienna 1819,) page 338, on the authority of the *Gazeta de Mexico.*

I have etched a polished surface of a very close and solid portion of this iron, and obtained very distinct geometrical figures. The lines of structure intersect each other, although in a rather irregular manner, generally in two directions only, pretty nearly in a rectangular figure. The indications of crystallization are more distinct, but most like that shown on the polished surface of the small piece in the Imperial Cabinet of Natural Curiosities at Vienna, represented in Von Schreiber's *Beiträgen*, (Vienna 1820, in folio,) tab. viii. If the mass of iron of Zacatecas should not entirely coincide with that of Jiquipilco, it may be supposed that the fragment at Vienna, taken from a larger one in Klaproth's collection, also comes from Jiquipilco.

I have seen, since I last saw you, another specimen of that iron, cut from the mass sent by Mr. Stein. It had this advantage over the very solid and close-grained specimen, that its structure was evident without its being etched. One flat surface appears as if it had split in two directions corresponding with the veins in the other. There are on this surface some larger angles which likewise indicate veins. One course of veins is more distinct than the other; a circumstance which is also remarked on the etched surface of the first piece, as the lines in one direction appear less interrupted than those in the other. Several parallel clefts run in the direction of this first course of veins in the second piece, which clefts so weaken the mass that it would require a much smaller power, in proportion, to break the specimen in this direction into several slices,—a character which I have never before noticed in any meteoric iron.

X. *On*

X. *On the Method of the Least Squares, as employed in Determining the Figure of the Earth, from Experiments with the Pendulum, as well as by the Measurements of Arcs.* By W. Galbraith, *Esq. A.M.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN numerous papers that have lately appeared on the determination of the figure of the earth, from experiments by the pendulum, as well as by the measurements of arcs, the method of the *least squares* has been much employed, as best calculated to arrive at the truth. The application of this method to such a purpose I think may perhaps be questioned. On investigating the principles on which it is founded, it will be observed that each individual observation is nearly of equal authority. It is generally supposed that the errors are irregular and fortuitous, and have no part constant, and that each and every observation contributes its share of accuracy to the general conclusion. But it does not, I apprehend, guard against the effects of a favourable or unfavourable situation in any or several of the observations, by which a greater or less effect may be produced by the amount of error, whatever it may be in magnitude, upon the final result. By combining, as that method sometimes requires, observations made under favourable circumstances with those in unfavourable, a result may be produced less accurate than if a judicious selection of decisive observations had been made, by which means the method of the least squares would have contributed by the manner in which it must be employed to have vitiated the conclusion.

On combining observations for various astronomical purposes, there is often no criterion from which it can be concluded that any one among a very considerable number has the least advantage over any of the rest; though on some occasions it is quite well known that an observation made under peculiar circumstances, may be more decisive than all the others, or at least the greater part of them.

Now this is precisely the case with regard to the compression of the earth; to determine which, two measurements of distant arcs are much more conclusive than a considerable number near one another, or at least, whose distances are not considerable. But when the method of the least squares is employed, the whole are combined, both those in favourable and in unfavourable situations; so that the advantages of the distant ones are in some degree counterbalanced by the disadvantages

advantages of those adjacent. If two arcs, for example, are chosen near the equator, a very small error in the measurement of the length of a degree may be productive of a considerable error in the compression. On comparing a degree measured in India, in latitude 13° 6′ 31″ N., with that in Peru, in latitude 1° 31′ 0″ N., the compression by the usual formula given for that purpose will be $\frac{1}{368}$, much less than that usually received as most entitled to confidence. The difference of the length of a degree measured in these two parallels is only about 25 fathoms, and consequently a small error committed in the measure of either, amounts to a considerable part of the whole, and must, by that means, affect the compression in a similar manner. In fact, the increase of this difference by a single fathom would increase the compression considerably, or from $\frac{1}{368}$ to $\frac{1}{354}$. To avoid this circumstance, distant degrees have frequently been chosen, where the difference of their lengths is considerable, and consequently a small error in the measure of either has but little effect on the resulting compression. But this artifice cannot be employed when recourse is had to the method of the least squares; for in that case all the arcs are combined, the favourable with the unfavourable, and it is difficult to estimate what effect this has on the final result. In general I have remarked that the conclusions derived from it lately, give a greater compression than that usually received, and it would require some consideration to show why this has been the case.

Again, if the compression be deduced from the English and French degrees, it will be about $\frac{1}{333}$; and it would require the difference of the lengths to be increased about five fathoms to bring it to $\frac{1}{310}$, the more usual quantity. In truth, it may be readily inferred by consulting any table of lengths of degrees, such as those given by Cagnoli and Lambton, that a small error committed in the measurement of arcs either near the equator or near the poles, when compared together may have a considerable effect on the accuracy or inaccuracy of the resulting compression.

If two arcs are therefore compared at no great distance from the equator, such as one at about 10° S. with another at the same distance nearly N., a very small error in the measurements of the arcs will have a great influence on the compression, by whatever mode it may be obtained. The same thing nearly will happen when a degree upon the equator is compared with one about ten or fifteen degrees from it. This inference also applies to those similarly situated with respect to the pole. It also appears that the lengths of degrees vary most rapidly at 45°, and consequently it is on this account that

 the

the compression derived from a comparison of the English and French arcs is less affected by any small error that may be supposed to exist in either or both of the measures of these arcs, than equal or even smaller errors in those of India and Peru, though the distance between the parallels of the latter is greater than that between those of the former; and the same thing is true of those in similar circumstances with respect to the pole. Arguing on similar principles, it will appear that the ellipticity derived from arcs bisected by the tropics and polar circles will be entitled to the greatest confidence, as the length of a degree varies considerably at those parallels, and they are sufficiently distant to counteract any small error arising from a similar error in the measures of the degrees. A great deal of discussion has taken place upon this subject lately, and from which it may be supposed an improved solution of the figure of the earth, whether from the measurement of arcs or from experiments with the pendulum, will be obtained. Mr. Ivory has given in the Philosophical Transactions for 1824, a new and, so far as I am able to judge, an improved solution of equilibrium of a homogeneous body revolving about an axis. Mr. Airy has endeavoured to extend the celebrated proposition called Clairaut's theorem, by including the terms of the second order. In this investigation, however, Mr. Airy has not included the second condition of equilibrium, which Mr. Ivory has thought requisite, and assigns as a reason the insufficiency of that gentleman's arguments.

Several of the French mathematicans have also controverted some of Mr. Ivory's views on that subject, in which perhaps prejudice and the authority of the late Laplace have not been without their influence. Indeed, a very few only of the first mathematicians are competent to enter with effect upon such a difficult subject; and as Mr. Ivory has apparently succeeded in solving the question in its most general form, without the rejection of the terms involving quantities of the higher orders, which was required in former solutions, this seems to be a strong argument in his favour.

Mr. Airy, after having given his new solution of the problem, proceeds to compare theory with observation, and selects Captain Sabine's series of experiments with the pendulum as the best for his purpose. The compression comes out 0·003474 $= \frac{1}{287 \cdot 8}$, nearly the same as what Captain Sabine himself had deduced from Clairaut's solution: so that according to Mr. Airy's conclusion, not much more accuracy is obtained when quantities of the second order are retained, than when, according to Clauraut's method, they are rejected: and it is not probable that even by adopting Mr. Ivory's second condition

dition of equilibrium the difference would be great, though it is likely a slight difference might take place. Indeed, before I saw Mr. Airy's paper, I was in the habit of employing the reduced or geocentric latitudes in the solution of this problem, instead of the apparent or observed latitudes which have been generally used. The difference, however, is not great in a spheroid of small compression, such as our earth, and the employment of the apparent latitudes increases in a small degree the lengths of the computed pendulum near 45°; but the effect upon the compression is almost insensible. I shall not, however, at present direct my attention to this method by the pendulum of obtaining the compression, but proceed to consider that derived from the measurement of arcs.

Mr. Airy gives the following table of the measure of arcs, which are certainly the most extensive and accurate:

Place.	l	l'	Length of Arc in English fathoms.
Peru . . .	$-0°\ 2'\ 31''\cdot22$	$3°\ 4'\ 31''\cdot9$	188510
India . . .	8 9 38·39	18 3 23·6	598630
France . .	38 39 56·11	51 2 9·2	751567
England .	50 37 5·27	53 27 29·89	172751
Sweden . .	65 31 30·27	67 8 49·55	98870

The method of comparing large arcs has been said to be the more accurate plan of arriving at a correct value of the ellipticity, than by a comparison of single degrees; though it is not likely to have any great advantage over a comparison of degrees derived from the measurement of large arcs.—As a comparison of single degrees is more easily made than of arcs, I have deduced from the above arcs the value of a degree at the middle latitude of each.

Place.	Latitude.	Length of a degree in fathoms.
Peru . . .	1° 31′ 0″ N.	60468
India . . .	13 6 31	60493
France . .	44 51 3	60756
England . .	52 2 18	60824
Sweden . .	66 20 10	60955

 Now.

Now, as has frequently been demonstrated, if $\frac{e}{t}$ be called the compression denoted by ε, then

$$\varepsilon = \frac{d' - d}{3\, d \sin (l' + l) \times \sin (l' - l)} \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

in which e is the excess of the equatorial radius above the polar semiaxis, t the equatorial radius, d' the measure of a degree furthest from the equator, d that of the nearest, and l' and l the corresponding latitudes of the middle of these degrees. Also $t = \frac{r^{\circ}\, d}{1 - \frac{\varepsilon}{2} - \frac{3}{2}\varepsilon \cos 2\, l} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$

in which $r^{\circ} = 57^{\circ}{\cdot}2957795$, the length of an arc in degrees equal to the radius.

After determining the compression from formula (1), the radius of the equator and polar semiaxis may be obtained from formula (2).

On combining the Swedish with the Indian degree we shall get $\varepsilon = 0{\cdot}003233$

The Swedish	with the French	$= 0{\cdot}003197$
	with the English	$= 0{\cdot}003304$
	with the Peruvian	$= 0{\cdot}003203$
The English	with the Peruvian	$= 0{\cdot}003162$
	with the Indian	$= 0{\cdot}003199$
The French	with the Peruvian	$= 0{\cdot}003196$
	with the Indian	$= 0{\cdot}003250$
	Mean of the whole	$= 0{\cdot}003218$

or, $\frac{1}{310{\cdot}75}$.

In all these combinations it is obvious that the agreement is close, and admits of considerable confidence being placed in the general mean, since the discrepancies from it are on the whole very slight.

No doubt I have avoided such combinations as were likely from their situation to have an improper influence on the final result, as has been more particularly noticed already.

It may also be remarked, that the comparison of the Indian degree with the French, gives a compression almost the same as that adopted by Laplace, he having concluded that it is equal to 0·00326, while that stated above is 0·00325. As these are the largest and most accurate arcs hitherto measured, it may be supposed that the error of the length of a degree derived from them must be very accurate; and as their situation with respect to each other is favourable, it may be confidently expected that those stated above are both very accurate values of the compression.

This

This is also corroborated by Mr. Ivory's value of it, derived from some extensive series of pendulum experiments detailed in several numbers of the Philosophical Magazine, in which he finds from

Captain Sabines's experiments, ε	$= 0\cdot00333$
Captain Kater's	$= 0\cdot00329$
M. Biot's	$= 0\cdot00332$
Mean of the whole . . .	$= 0\cdot00331$

or, a little greater than that obtained above.

This is the more remarkable, as the result deduced by Mr. Airy from Captain Sabine's series of experiments by an application of the method of the least squares, according to deductions from his analysis, gives $\varepsilon = 0\cdot003474$, and from the measures of arcs, $\varepsilon = 0\cdot003589$. Mr. Airy concludes that if the Indian and French arcs are supposed to be quite accurate, $\varepsilon = 0\cdot003269 - A \times 2\cdot139$.

Now, from a previous analysis, he finds $A = -0\cdot000157$, which, if applicable here, would give $\varepsilon = 0\cdot003269 \pm 0\cdot000336$, a result (in whatever way we take the sign), he observes, that cannot be reconciled with the values of ε and A, which were $\varepsilon = 0\cdot003474$, and $A = 0\cdot000064$*, as deduced from the pendulum experiments. However, if A in this case were nothing, or very small, the compression, as I have also found it from these arcs, would very nearly agree with that deduced from various sources by Laplace.

He remarks, that "the measures of arcs of the meridian which have hitherto been made are, I imagine, insufficient for the determination of the figure of the earth." Now, from the consistency in the various measures of the compression which I have found above, it seems hardly possible that by increasing the number of the measures of arcs, a more near coincidence can, on the whole, be expected; and therefore it cannot be supposed that a better agreement among individual determinations could take place.

But still the question occurs, How does it happen that there is such a discrepancy between the method I have employed for individual observations, and that of the least squares, which connects the whole? Is it possible by any means to reconcile them? And which deduction is most worthy of credit? Is there any peculiarity in the application of the method of least squares to the present question, that makes it deviate so far from the other? I am disposed to think that there is, and that it consists in this: that there is not any regular series of

* I have supposed this to be the real value in page 566, Phil. Trans. 1826, though the decimal point has been from some oversight omitted.

observations

observations of pendulum experiments, or of measurements of arcs from the equator to the pole, by which the relative excesses and defects may exactly counteract each other, when all those both placed in favourable or in unfavourable circumstances are grouped together; and that in consequence of which, the ellipticity derived from the measurements of degrees considerably distant from each other, is more likely to be accurate than that obtained from the method of the least squares from arcs under such circumstances.

P.S. If $\varepsilon = 0{\cdot}00325$, then from an application of formula (2) to the degrees of France and India, the radius of the equator or t, the semitransverse axis of the ellipse will be 20921178 feet. Also $\frac{e}{t}$ being equal to 0·00325, $e = 0{\cdot}00325\ t$. Taking the value of t in feet, then $e = 67993{\cdot}8$ feet, or 12·878 miles. Hence also the semiconjugate or polar semiaxis, designated by c, $=$ 20853184 feet. The radius of curvature at 45° being represented by $t - \frac{e}{2}$ will be 20887181 feet $=$ 3955·905 miles. Whence the mean diameter of the earth is 7911·81 miles, and the mean circumference 24855·686 miles.

I am, &c.

Edinburgh, May 7, 1827. Wm. Galbraith.

XI. *A Synopsis of the Genera of Saurian Reptiles, in which some new Genera are indicated, and the others reviewed by actual Examination.* By J. E. Gray, *Esq. F.G.S. &c.**

In the Annals of Philosophy, (N. S. vol. x. p. 193.) I collected together the genera of Reptiles; but since that time several new ones having been established, and having lately been enabled to examine most of them, as well as several others, especially some from India, collected by General Hardwicke, of the Bengal Artillery, whose fine collection of drawings illustrative of the Natural History of India is well known, I am enabled to add several new and very curious genera which have not hitherto been published.

A. *Tongue long, end deeply two-cut. Teeth denticulated; on the inner side of the jaw.* Sauræ.

Fam. I. UARANIDÆ.

Tongue retractile; head and body scaly, femoral pores none; palate toothless.—*Only found in the Old World.*

* Communicated by the Author.

Uaranus

UARANUS, *Merrem.* (Monitors.) Tail compressed, beneath rounded, above mostly serrated. Aquatic.

Lacerta varia, *Shaw.*

DRACŒNA, *Merrem.* Tail round. Terrestrial.

Lacerta Dracœna, *Linn.*

FAM. II. TEIIDÆ.

Tongue contractile; head and abdomen shielded; throat with two plaits; palate toothless.—*New World.*

ADA. Teeth conical; scales of the back large-keeled; tail end compressed.

Lacerta Dracœna, *Bonn.*

TEIUS, *Merrem.* Teeth denticulated; scales of abdomen long; tail end compressed.

Lacerta bicarinata, *Linn.*

AMEIVA, *Cuv.* Teeth denticulated; scales of abdomen short, broad; tail round.

Lacerta Ameiva, *Gmel.*

EXYPNESTES, *Caup.* Head and abdomen with small plates; back scaly.

E——, *Caup.*

FAM. III. LACERTINIDÆ.

Tongue contractile; head and abdomen shielded; throat with a collar of large scales; palate toothed.—*Old World.*

LACERTA, *Linn.* Head and abdomen shielded; femoral pores distinct.

* Subanal scales entire.

L. agilis, *Linn.*

** Subanal scales imbricate.

TACHYDROMUS, *Oppel.* Head, back and abdomen shielded; pores none.

T. sexlineatus, *Daud.*

B. *Tongue end slightly nicked.*

† Teeth denticulated, internal, submarginal.

FAM. IV. GECKOTIDÆ.

Head and body depressed; scales small; toes beneath mostly furnished with scales; throat simple. Palate toothless.

HEMIDACTYLUS. Toes not webbed, scales in a double series, last joint free, compressed.

Gecko tuberculosus, *Daud.*

PLATYDACTYLUS. Toes not webbed, dilated the whole length, last joint keeled, attached; scales entire in one broad series; femoral pores none; toes mostly clawless.

* Toes clawless.

Gecko inunguis, *Cuv.*

** Two middle fingers clawed. *Tarentola.*

Lacerta Mauritanica, *Linn.*

GECKO.

GECKO. Toes not webbed (like Platydactyli), femoral or subanal pore distinct; thumb clawless.

Lacerta Gecko, *Linn.*

Toes all clawless. *Phelsuma.*

Gecko cepidianus. *Merrem.*

PTEROPLEURA. Toes webbed, otherwise like Platydactyli; femoral pores none; sides of body and members furnished with a membranaceous expansion.

P. Horsfieldii. *n. s.* from India by General Hardwicke, and in the India House museum from Java.

THECADACTYLUS. Toes half-webbed, dilated their whole length, scales divided by a longitudinal groove inclosing the claws; pores none. Madagascar.

* Uroplates, *Daud.* Toes 5-5; tail and sides fringed. Stellio fimbriatus, *Schneid.*

** Caudiverba, *Laur.* Toes 5-5; tail fringed.

Lacerta Caudiverba, *Linn.* America.

*** Sarouba. Toes 4-5; tail fringed.

Salamandra Sarube, *Bonn.*

**** Toes 5-5; thumb clawless; tail round.

Lacerta rapicauda. *Gmel.* America.

PTYODACTYLUS. Toes 5-5, slender, compressed, last joint with 2-3 scales, divided in the centre, and sheathing the claws; femoral pores none.—*Africa.*

Lacerta gecko, *Hasselquist* not *Linn.*

PHYLLURUS. Toes 5-5, subequal, slender, compressed, without scales, clawed; pores none; tail depressed, fringed.—*New Holland.*

Lacerta platyura.—*Shaw*, *White's Journal.*

EUBLEPHARIS. *n.* Toes 5-5, subequal, conical, without any scales; short clawed; subanal pores in a lunate series; tail round.—*India.*

E. Hardwickii *n. s.* General Hardwicke.

CYRTODACTYLUS *n.* Toes 5-5, subequal, compressed, without any scales, clawed, end compressed, bent, and arched, femoral pores none, but situation marked by large scales. —*India.*

C. pulchella *n. s.* General Hardwicke.

FAM. V. IGUANIDÆ.

Palate toothed (except *Cyclura*); head and body compressed; toes mostly simple; throat pendulous.

* *Femoral pores none; teeth 3-lobed.*

ANOLIUS, *Cuv.* Anolis, *Merrem.* Head shielded; back crested; toes with last joint but one dilated, pear-shaped.

Lacerta bullaris, *Linn.*

BASILISCUS, *Daud.* not *Cuv.* Head shielded, hooded; back and tail with an erect fin; toes margined.

Lacerta Basiliscus, *Linn.*

LOPHYRUS

LOPHYRUS *Oppel.* (part). Uranascodon, *Caup.* Head scaly; back toothed; toes simple.

Lacerta superciliosa, *Linn.?* *Seba. t.* 109. *f.* 4.

** *Femoral pores distinct; toes simple.*

IGUANA, *Daud.* Teeth denticulated; back crested; toes inequal.

Lacerta Iguana, *Linn.*

AMBLYRHYNCHUS, *Bell.* Teeth 3-lobed; toes subequal; back and tail crested.

A. cristatus, *Bell.*

POLYCRUS, *Cuv.* Teeth 3-lobed; head shielded; back simple; tail long, cylindrical.

Lacerta marmorata, *Linn.*

CYCLURA, *Harlan.* Teeth 3-lobed; head shielded; back denticulated; tail with whorled spiny scales; palate toothless.

Lacerta acanthura, *Shaw.*

†† *Teeth simple, marginal, entire.*

FAM. VI. CHAMELEONIDÆ.

Toes united into two groups, to the claws; tail prehensile; body compressed.

CHAMELEON.—*Africa and India.*

Lacerta chameleon, *Linn.*

FAM. VII. AGAMIDÆ.

Toes free, long, unequal; head depressed, behind swollen; tail simple.

* *Femoral pores none.*

AGAMA, *Oppel.* Neck and sides simple.—*Old and New World.*

* Trapelus, *Cuv.* Scales smooth.

** Scales keeled; head scaly.

*** Scales keeled; head with large occipital plate.

**** Scales keeled; head with plates.

CLAMYDOSAURUS. Neck with a plaited frill on each side. —*New Holland.*

C. Kingii.—*King's Voyage.*

DRACO, *Linn.* Neck with two plaits; side of body with wing-like expansion.—*India.*

D. volans, *Linn.*

* *Femoral pores distinct.*

LOPHURA. Tail with a raised fin-like dorsal crest; toes fringed.—*India.*

Lacerta Lophura, *Shaw.* Lacerta Amboinensis, *Schlosser?*

ZONURUS, *Gronovius.* Tail verticillately spinous; head and abdomen shielded.

Lacerta cordylus, *Linn.*

*** *Femoral and subanal pores distinct.*

UROMASTIX. Tail verticillately spinous; head and body scaly.

U. acanthinurus, *Bell.*

British Museum, June 1, 1827.

XII. *Corrections in a Work entitled "Chemical Manipulation."* *By* MICHAEL FARADAY, *Esq. F.R.S. &c.**

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IF not inconsistent with the object of your Journal, may I beg your insertion of the following errors and corrections which have been pointed out to me by Captain Kater. They belong to a work of mine, entitled "Chemical Manipulation," which being intended for instruction ought not to contain such errors uncorrected. Your insertion of the corrections will tend greatly to prevent any harm arising from the errors, and will much oblige, Gentlemen,

Your very obedient servant,

Royal Institution, June 18, 1827. M. FARADAY.

Page 83 line 22, *for* "39.37039," *read* "39.37079."
—— 83 — 23, *for* "3.93704," *read* "3.93708."
—— 83 — 76, *for* "39.12929," *read* "39.13929."
——140 — 8, *for* "high," *read* "low."

XIII. *Notices respecting New Books.*

Chemical Manipulation; being Instructions to Students in Chemistry, on the Methods of performing Experiments of Demonstration or of Research, with Accuracy and Success. By MICHAEL FARADAY, *F.R.S. F.G.S. &c. &c.*

WHILE treatises on Chemistry have been multiplying with rapidity, and while every form which ingenuity could devise, has been given to the facts and theories of the science, there did not, as far as we know, exist, until the appearance of the present volume, any work expressly on manipulation, and we need, therefore, hardly remark, that directions given with plainness and accuracy and without the fear of appearing to enter too deeply into minutiæ, have hitherto been a desideratum which but very few have the means, the patience, or the talent to supply.

* Communicated by the Author.

We

We shall not dwell at any length on the numerous proofs which might be adduced to support an opinion which is, we believe, universal in the chemical world,—that the author of the present work possesses every requisite which the execution of the work demanded. We cannot, however, refrain from informing the tyro (and he only needs the information) that to Mr. Faraday are owing many of the most brilliant and valuable discoveries with which the science of Chemistry has of late years been enriched: by his experiments we have been taught, that various aëriform bodies, which were previously considered as permanently elastic, are reducible to the liquid state; and to him also the philosophical world is indebted for the first instrument for exhibiting electro-magnetic rotation.

That a work of the present kind was wanted, we think every one must admit, who has witnessed the attempts of a tyro to perform even the most simple experiments proposed by chemical authors; and this difficulty arises not merely from a deficiency of directions, but from such as are inadequate or improper:—Take as an instance of this the directions given by one of the most distinguished chemists of the present day, who in preparing oxygen advises that the oxide of manganese and sulphuric acid should be mixed in the glass retort by means of a glass rod; an operation which in nine cases out of ten would terminate in perforating the bottom of the retort.

The objects which the author has in view, are clearly and concisely stated in a few pages of introduction: he then divides his work into twenty-four sections, in all of which he has displayed that adaptation of means to ends which constitutes power, and evinced the patience of research and accuracy of observation, which are shewn in his various and excellent papers contained in the Philosophical Transactions, and the Journal of the Royal Institution.

Our limits will not permit us even to enumerate the subjects of the sections which the work contains, and we find it extremely difficult to select any one part in preference to another, for the purpose of exhibiting the author's talents for invention and description; for although the work is elucidated by numerous wood-cuts, it will be found that the instruments which they are intended to represent, are mostly original, and not such as have appeared in every chemical work from the origin of pneumatic chemistry to the present day.

We cannot, perhaps, more fairly exhibit Mr. Faraday's method of treating the various subjects which constitute his work, than by selecting what relates to a very interesting part of that branch of the science which he terms *tube chemistry*, and from this the reader will have an opportunity of judging of the minute accuracy and nice observation that characterize every part.

" Frequent occasion has occurred in the preceding parts of this volume for the description of apparatus formed partly or altogether of glass tube. The object of this section is to show the important uses of apparatus of that description. The facility with which it supplies the absence of many complicated instruments; the consequent economy and readiness of chemical practice; and the peculiar advantages of it when rare and valuable substances are under examination, are the inducements to collect the information upon this subject into one focus.

"The material required for the construction of this kind of apparatus is glass tube of half an inch in diameter or less, and of different degrees of thickness. The most useful sort is quill tube, the glass being of the thickness of card or thin pasteboard. Three-square or edge files are required for cutting the tube into lengths. If the table blow-pipe and lamp be not at hand, most and indeed all the apparatus may be made by a spirit-lamp and a mouth blow-pipe. To these should be added a drawer full of tubes, closed at one end, of all diameters and all lengths, from one inch to five or six. The fragments of tubes, which are continually occurring, should be worked up into these forms at every opportunity, according to the direction to be given in Section xix. and are then ready for use.

"These tubes answer all the purposes of test-glasses, and in the small way precipitates are made, preserved, and washed very conveniently in them. They are easily supported in a tumbler or wine-glass, or they may be supplied individually with stands by inserting them in perforated corks.

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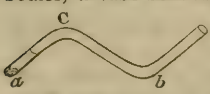
"During long digestions, as in the solution of difficultly soluble bodies, a tube bent into the form represented in the figure is very



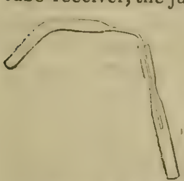
advantageous. The acid or other fluid which is volatilized and distilled over into the part at *b*, is easily returned upon the substance at *a*, by elevating the open end of the tube, and is made to re-act upon it; a little piece of moistened paper may be applied at *b*, or that part may be cooled by a refrigerating mixture, or by immersion in water. This arrangement is most frequently useful in the solution of substances but slowly acted upon in acids, as certain metals or metallic ores.

"The above process also illustrates the use of tube apparatus in distillation; the part *a* answers to the retort, and the part *b* to the receiver of the usual apparatus. The fluid to be purified or distilled may be poured into the tube, and the latter being held upright, and the finger placed over the aperture, heat should be applied below and vapour raised; this will condense upon the sides of the tube and flow down, carrying with it that portion of the fluid which, in pouring it in, adhered to the side; this should be done till it is observed that the vapour rises nearly to the top before it condenses, and insures the cleansing of the whole tube. This preliminary operation is intended simply to wash the adhering portion of the introduced fluid to the bottom of the apparatus, that nothing may remain at *b* to contaminate the distilled products. The tube is then to be placed as in the figure, the proportion of the vessel and the charge being such, that the latter should not occupy more than half that part of the tube. Heat being then gradually applied near the top of the fluid, the latter should be distilled over, into the angle at *b*, which is now to be cooled by wet paper, water, or some other

other means. If the distillation be unsatisfactory, it is easy to return the product and repeat the operation; if satisfactory, then by applying a file at *c* the tube is readily divided, and the rectified portion obtained in the closed part, constituting a separate vessel.

" Distillation is frequently performed in a tube-apparatus, precisely similar to the ordinary retort and receiver. A piece of tube sealed at one end, and then bent as in the figure, forms what is called

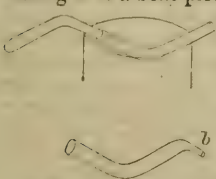
a tube-retort. Fluid substances are easily introduced into it through a little tube funnel, made by heating the middle of a piece of tube about two inches long, and half an inch in diameter, by the lamp, and then drawing it out into a capillary tube and separating it of a proper length. A receiver for such a retort is made of a piece of straight tube of larger diameter closed at one end. The beak of the tube-retort is merely inserted an inch or more into the tube-receiver, the junction is left open, and the latter is cooled, if re-



quired, in any of the usual ways. Occasionally it is advantageous to draw out the beak of the retort into a capillary form, as has been before described; it will then enter into vessels having small apertures and necks. Sometimes it is very useful to contract the necks of tube-receivers in a similar manner, as will hereafter be more evident.

" When a larger tube-retort is made use of, it is often useful to

draw out and contract the neck, for the purpose of diminishing its capacity, and consequently the quantity of vapour which it can contain; a common narrow-necked phial then makes an excellent receiver. The tube-receiver is frequently varied in form with advantage, by making it of a bent piece of tube open at both ends, and when one

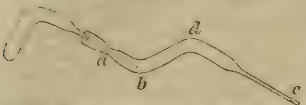


end of it is formed as at *b* in the following figure, it is exceedingly convenient for pouring out minute portions of the liquid contained in it without waste; for by bringing the small extremity *b* against a glass rod or a plate, and inclining the receiver, as little or as much of the fluid may be delivered as is required.

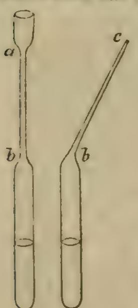
" These tubular vessels may be supported with facility, sometimes upon the table across two or three pieces of glass tube, or rod, or upon bent rings, or in the air upon the edges of glasses placed side by side, or upon retort stands. The arrangements are so simple that no difficulty can occur with respect to them. The receivers may be cooled by wet paper, or by placing them in water in a glass or Wedgwood's dish, or by putting them into a hole in a piece of ice, into which a little salt may be occasionally introduced.

" In cases of distillation upon a small scale, where, besides a fluid product,

product, a gas is also expected and required to be collected, the tube may have the accompanying form given to it. If it be required to distil the first product a second time for its further purification, it should at first be distilled into a receiver of the second form, and that operation finished, the retort is to be removed, a small flame applied by the blow-pipe to the narrow part of the receiver at *a*, which when soft is to be drawn out and sealed hermetically. The second rectification is to be made either by applying heat to *b*, and placing another receiver at *c*; or by turning the tube into the position it would take if this page were inverted, applying heat at *a*, and distilling into the bend at *d*.



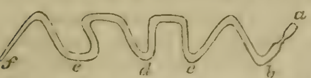
"A very convenient vessel, answering the purpose both of receiver and bottle, may be made of tube. For this purpose a piece of tube about the third of an inch in diameter, four inches long, and sealed at one end, is to be softened in a flame at about an inch from the open extremity, and when uniformly heated all round, it is to be removed from the flame and drawn out, so as to form a long narrow neck. The substance to be distilled into it, such as sulphurous acid, or chloride of phosphorus, is to be conducted by a fine tube, similar to that already described, which is to terminate the distillatory apparatus, being either drawn out upon the end of the retort or joined with it by a caoutchouc connecter. When sufficient fluid has been distilled into the receiver, the capillary neck of the distillatory apparatus should be withdrawn, the tube softened about *a* by a small flame and drawn off, so as to leave the termination there with a fine aperture; then it is to be softened again at *b*, and bent as in the second figure. The aperture at *c* is easily closed by holding it for a moment in the edge of a flame, and the contents of the vessel, however valuable they may be, are securely retained. When a portion is wanted for experiment, the extreme point should be nipped off so as to make an aperture, and the tube should be inclined until *b* becomes the highest part; so much of the fluid as may be required should be thrown by a little agitation into the neck about *b*, where it will remain in a short column; but by applying the hand to the thicker part of the tube, the air will expand and force out the fluid in the neck, on to any spot to which the aperture at *c* may have been directed. In this manner the smallest quantity of the fluid, or the whole, may be used at once; and enough having been removed, the tube is again to be placed in a more upright position, its extremity sealed, and the whole put aside until again wanted.



"These receivers are very useful for retaining valuable and volatile fluids, and are the best that can be used for such bodies as sulphurous acid. If that substance be confined in ordinary bottles, a great

great quantity is suddenly volatilized each time the bottles are opened, and from the instantaneous cold produced, the bottoms generally break and fall out: but if it be preserved in vessels like those described, they may be of sufficient thinness to bear this sudden depression of temperature without fracture, and may even be cooled previously with facility by a piece of ice and a little salt. Sulphurous acid may be preserved in such tubes in small portions for single experiments, or if in large quantity, it is easy to distil or transfer it as has been described. When used for sulphurous acid, they must of course be retained in a refrigerating mixture during the distillation, they must be continued in this mixture whilst the top piece is withdrawn, and also whilst the bend is given to them, if that be required. It is also necessary that they be sealed when thus cooled, for it cannot be done after they are exposed to the air. The best method is to prepare the small aperture by drawing off the extremity, to lift the tube into the air, then to apply the flame of the lamp, which will not as yet seal it, and afterwards to lift up the freezing mixture, or depress the tube in it, still applying the flame of the lamp: as the cold condenses the internal vapour the current outwards will cease, and the extremity will close; instantly withdraw the lamp so that the glass shall harden, and then the receiver may be taken out of the cold mixture, and preserved in a glass or tumbler, in a place at ordinary temperature. Should there be a doubt of the sealing being perfect, bring a little ammonia near the extremity; if no fumes are produced all is secure, if there be fumes the same operation of sealing as that just described must be resorted to.

"Successive rectifications may be made in the same tube, by bending it with several angles as in the annexed figure; such an apparatus was found of great service in experiments upon the fluids obtained by the compression of oil gas. The fluid is to be introduced

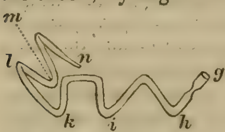


at the open extremity *a*, so as to lie in the angle *b*, then applying a small blow-pipe flame, the glass should be softened at the neck, drawn out and sealed, the capillary termination at *f* being open. On moderately heating *b* and cooling *c*, a distillation of the more volatile part will take place, the latter collecting at *c*: after a time by keeping *b* and *c* warm and cooling *d*, a rectification of the product at *c* may be effected, and this distillation may be again repeated upon the product at *d* by condensing in *e*. By forming the angles of the tube as in the figure, the results may be returned and redistilled; for upon raising the end *f*, the product in *c* will first return to *b*, then that in *d*, and finally that in *e*; so that if the substance be sufficiently freed from the denser parts only after the third or fourth distillation, the products in *c* and *d* may be returned to *b* and redistilled as before, that in *e* being retained separate: during such experiments *e* should be preserved very cold.

"In experiments with the oil gas liquor, distillations of this kind were often to be performed in close vessels, that dissipation of the more volatile parts might be prevented. In such cases after having introduced

introduced the fluid to *b* and sealed the end *a*, the end *f* was raised till it was the highest point, the fluid in the lower extremity heated until combustible vapour issued at *f*, and then a small flame applied whilst the temperature of the other parts was allowed to fall; the vapour within soon condensed, the extremity was instantly closed by the lamp, the lamp itself removed, and the tube left hermetically sealed. Then collecting all the fluid to the end *b*, the distillations and rectifications were performed, and when fluid had collected in *c*, it was easy by opening the end *f* under mercury, to ascertain whether it was sufficiently volatile to rise as gas at ordinary pressure, and when it did so the gas was easily collected in jars.

"It will be unnecessary to refer minutely to the capability of transferring backwards afforded by different inclications of the parts of the tubes; by angles different to those mentioned, the fluid may be first returned from *e* to *d*, then from *d* to *c*, and so on. By bending the tube at *l*, as is represented in the accompanying wood-cut, so that the tube from *l* to *n* shall be in a plane perpendicular to that which includes the part from *g* to *l*, the



power is obtained of returning the products from *k* to *h*, or from *m* to *k* independently of each other; and thus the more fixed and more volatile parts may both be returned and be re-distilled without mutual interference. The student will easily comprehend these forms of tubes and their advantages by bending a piece of wire into the directed or desired shape, and observing the position of its parts as he inclines it in different directions."

Would room admit of it, there are various other parts of the work which we should have great pleasure in presenting to the reader; but we must now conclude with a few and only a few very miscellaneous observations.

First, as to filters and funnels:—It would, we think, be well to warn the tyro that if a single drop of water remain in the funnel, the filter is extremely liable to tear at that spot. The subject of filters is one which is extremely puzzling to the young experimentalist, and we think a little more might have been advantageously said with respect to double filters,—as to the mode of cutting, weighing, and drying them, and allowing for the quantity of any product which adheres to the filter. We believe it is not generally known, or if known not adverted to, that if a sheet of filtering paper be divided even quite accurately into equal portions, in the direction in which it is folded, one half is heavier than the other,—a circumstance which arises from the manner in which the frame is held during the manufacture of the paper. Now it is extremely convenient and saves much weighing and cutting, that filters of equal weights should as nearly as possible be of equal sizes: with this intention it is better not to divide the sheet into halves, but to cut the portions of which the double filter is to be made, *across* the sheet of paper.

In p. 172. excellent directions are given on the very simple subject of glass stirrers, to which however we would add one hint more; —they are extremely apt to roll from the experiment-table. Now this

this is easily prevented by softening them near the centre in the spirit-lamp, and then very slightly bending them. In washing precipitates an inverted bottle of water is extremely useful: it supplies the fluid as fast as it is wanted; and as it requires no watching, filtration may not only be performed during the absence of the operator, but that cracking of the precipitate which occurs in many cases when the filter becomes dry, is prevented.

From the long extract which we have made, the reader will have a sufficient opportunity of observing that Mr. Faraday's style is at once simple and correct: there are however two passages which contain (as we think) a figure of speech which we will not name, but which the author will probably guess at, and in a future edition alter. The first occurs in the Introduction, p. iii. "There are also two parts in an experiment; first, it has to be devised" &c. Now as a thing does not exist until it is devised, we do not see how the devising of it is a "part" of it. In p. 174. we are informed that "the simplest step in the application of heat is to obtain a solution saturated when cold." To us it appears on the contrary that the obtaining of a cold solution is no step at all in the application of heat.—We observe also that the author uses the term "lute" in two and very different senses: first, in its proper sense, that of stopping the orifice between a retort and receiver; and secondly, in that of coating. Now *luting* a retort and *coating* one are two different operations.

There are few philosophers, as we shall probably show, more particularly, on a future occasion, whose discoveries have been more frequently attributed to others than Dr. Priestley. This observation is occasioned by a remark by Mr. Faraday, in which he ascribes to Lavoisier the first use of oxygen, as a means of increasing heat. The following quotations will, we think, unquestionably evince the priority of Dr. Priestley.

In his "Experiments and Observations on different kinds of Air," vol. ii. edit. of 1784, p. 100, Dr. P. remarks, "Nothing would be easier than to augment the force of fire to a prodigious degree by blowing it with dephlogisticated air instead of common air. This I have tried in the presence of my friend Mr. Magellan, by filling a bladder with it and puffing it through a small glass tube upon a piece of lighted wood; but it would be very easy to supply a pair of bellows with it from a large reservoir.

"Possibly much greater things might be effected by chemists in a variety of respects, with the prodigious heat which this air may be the means of affording them. I had no sooner mentioned the discovery of this kind of air to my friend Mr. Mitchell, than this use of it occurred to him.—He observed that platina might be melted by means of it."

And in the edition of 1790, vol. ii. p. 168, he adds to the above: "These conjectures have been abundantly verified by the experiments of M. Lavoisier and others."

In concluding these remarks, and after a very careful perusal of the work, we strenuously recommend it, as containing the most complete and excellent instructions for conducting chemical experiments: there are few persons, however great their experience,

 who

who may not gain information in many important particulars; and, for ourselves, we beg, most unequivocaly to acknowledge that we have acquired many useful and important hints, on subjects even of every-day occurrence.

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 3.—A PAPER was read, entitled, "Rules and Principles for determining the dispersive ratio of glass, and for computing the radii of curvature for achromatic object-glasses: submitted to the test of experiment. By Peter Barlow, Esq. F.R.S."

May 10.—A paper was read, entitled, "Some observations on the effects of dividing the nerves of the lungs, and subjecting the latter to the influence of voltaic electricity. By A. P. Wilson Philip, M.D. F.R.S."

When the nerves of the eighth pair, supplying the lungs, are divided, the animal breathes with difficulty, and speedily dies of suffocation. If the lungs are examined after death, their cells are found so completely filled with a viscid fluid as to obliterate them entirely, as well as the air-tubes. They sink in water, and from a description by Mr. Cutler, which is stated at length by Dr. Philip, it appears that they are rendered impermeable to injections. These particulars having been described, the author states, on his own testimony and on that of various other gentlemen who have witnessed the fact, that if the due degree of voltaic electricity be transmitted through the lungs by those portions of the nerves which remain attached to them, no affection of the breathing supervenes, and the lungs, after death, are found quite healthy, unless the electricity has been applied of such power, or for so long a time, as to cause inflammation, in which case the appearances on dissection are those of inflammation, and not those produced by mere division of the nerves.

A paper was also read, entitled, "On the change in the plumage of some Hen Pheasants. By W. Yarrell, Esq. F.L.S." Communicated by W. Morgan, Esq. F.R.S.

The last shooting season having been unusually productive of hen pheasants, which have assumed more or less the plumage and appearance of the male, much discussion has in consequence arisen on the cause of this change; and the author, having had many opportunities of examining the facts, as respecting both the pheasant and the domestic fowl, was induced to notice the internal peculiarities which invariably accompany this transformation. According to an opinion of John Hunter and of Dr. Butter, this change only takes place at an advanced age; but Mr. Yarrell considers the facts in his possession as at variance with this idea, and that the appearances in question may occur at any period of life, and may even be produced artificially.

In all the instances examined by him the sexual organs were found diseased, and to a greater or less extent in proportion with

the

the change of plumage. The ovarium was shrunk, purple, and hard; the oviduct diseased, and the canal obliterated at the upper part, immediately preceding its infundibuliform enlargement at the bottom of the ovarium. Having opened a hen pheasant in her natural plumage, for the sake of comparison, he found a similar diseased state of the organs to exist; thus proving that the disease must exist some time before the corresponding change of feather takes place. He observes, that it is no uncommon thing to find among numerous broods of pheasants reared by hand, some females, which, at the age of only four months, produce the brightest plumage of the male; and in two instances of birds shot in a wild state, the nest feathers had not been shed, proving them to have been birds of the year.

A partridge, having a white bar across the breast, and the first three primaries in each wing white, being opened, exhibited the same sort of organic disease; and, from circumstances adduced, it appears that this was also a bird of the year.

But all variations in plumage are not traceable to this cause. In most of the excepted instances, however, the individuals are dwarf birds; and the author attributes their variety of plumage to defective secretion—the effect of weakness.

When the sexual organs are artificially obliterated in the common fowl, so soon as the operation is performed in the male bird, he ceases to crow, the comb and gills do not attain their full size, the spurs remain short and blunt, and the feathers of the neck assume an appearance intermediate between the hackled character of the cock and the ordinary web of the hen. When the oviduct of the female is obliterated, the ova cease to enlarge; she makes an imperfect attempt to crow; the comb increases in size, and short and blunt spurs make their appearance. The plumage alters in colour and in form, approaching to that of the cock, the bones of the lower part of the back never acquiring the enlargement requisite for giving a proper breadth to the pelvis. In short, the two sexes approximate so nearly in character by this process, that it frequently becomes difficult to determine the sex.

Hen pheasants assume the plumage of the male at best but imperfectly, and it is probable that they do not live many years after the change.

It appears to be a general law, that where the sexes of animals are indicated by external characters, these undergo a change, and assume a neutral appearance, whenever original malformation, subsequent disease, or artificial obliteration, has deprived the sexual organs of their true influence.

ROYAL INSTITUTION OF GREAT BRITAIN.

May 25.—On the different methods and principles of ship-building, with particular illustrations of those due to Sir Rob. Seppings, by Mr. Holdsworth.—The different methods of building were explained by very fine models and clear descriptions; illustrations both historical and practical, being drawn from various nations: the peculiar advantages of the late improvements were pointed out.

Models of ships; of a ventilator; of Italian banditti, and various works of art, were placed in the Library.

June 1.—Mr. Turrell described the various useful and œconomical applications of the diamond; with the methods of cutting, polishing, and setting it, either for ornamental purposes, or in chronometers, or for engraving, &c. &c., and illustrated his details with numerous specimens of the gem in various states, and many works performed by means of it.

In the Library were a series of insects from the Caucasus, and several birds which had been presented to the Museum: also engravings on wood by the celebrated Tobias Stinimus, and some rare literary works.

June 8.— Mr. Millington took a rapid but clear survey of the steam-engine, and the recently proposed improvements; and also of the means for obtaining motive power from the gases. The latter part principally referred to Brown's gas-engine, of which a large working model was set in operation in the Lecture-room.

Specimens in Natural History which had been presented during the week to the Museum, and new literary works, were laid upon the Library table.

June 15.—Mr. Faraday gave An account of the progress and present state of the Thames Tunnel. It was illustrated by Mr. Brunell's drawings and models, and comprised a particular account of the recent entrance of water upon two occasions, and the means by which the ingress of the fluid had been stopped.

In the Library were two Guanches from Teneriffe, belonging to Mr. Brettell. Specimens of rock-salt, being a complete series, from Cheshire. New literary works, and works of art.

These meetings were then adjourned until the next season.

XV. *Intelligence and Miscellaneous Articles.*

FLUOR SPAR AND ANHYDROUS SULPHURIC ACID.

M. KUHLMANN, professor of chemistry at Lille, has discovered that fluor spar cannot be decomposed by anhydrous sulphuric acid. This is an additional fact in favour of the opinion that this substance is a fluoride of calcium.—*Ann. de Chim. et de Phys.* Feb. 1827.

RHEINE,—A PECULIAR SUBSTANCE IN RHUBARB.

M. Vaudin, by treating one part of rhubarb with eight parts of nitric acid at 35° (Baumé) with a gentle heat, and then reducing it to the consistence of a syrup and diluting it with water, observed that a peculiar substance was precipitated, which he has called *Rheine,* and which has the following properties when dried. It is of a yellowish orange colour, devoid of any peculiar smell; its taste is slightly bitter, and it is almost entirely soluble in alcohol and æther: these solutions become yellow by acids, and of a rose-colour by alkalies. Rheine burns like other vegetable bodies, especially like starch. Rhubarb treated directly by sulphuric æther yields a substance which is perfectly similar:—this proves that the new substance exists

exists ready formed in the rhubarb, and that it is not acted upon by the nitric acid.—*Journ. de Chim. Méd.* ii. 286.

ZANTHOPICRITE,—A NEW VEGETABLE COLOURING PRINCIPLE.

MM. Chevallier and G. Pelletan have given this name to a crystallized substance which they extracted from the bark of the *Zanthoxilum* of the Caribbee Islands. After having made a spirituous extract and washed it with small quantities of water to separate a red colouring matter, a resinous matter is dissolved by æther. The residuum redissolved in alcohol, yields by spontaneous evaporation groups of diverging acicular crystals, which are silky, of a yellow colour with a shade of green, and suffer no alteration by long exposure to the air: these crystals, which are Zanthopicrite, are readily soluble in water, more so in alcohol, but not at all in æther; they are extremely bitter, inodorous, excite the secretion of saliva, and do not act upon stained papers. When heated in a glass tube, a portion of zanthopicrite is volatilized, and another portion is decomposed with the usual results of vegetable decomposition. The aqueous solution of this substance is yellow; animal-charcoal removes the colouring principle perfectly, but it may be obtained again by boiling alcohol. It is not precipitated by the greater number of re-agents when diluted with water; some of them when concentrated produce this effect, but the precipitate is redissolved on the addition of water. A very small quantity of gold precipitates it entirely; the compound formed is soluble in alcohol and ammonia.—*Journal de Chim. Méd.*

ALTHEIN,—A PECULIAR SUBSTANCE IN MARSHMALLOW.

M. Bacon, professor of chemistry at Caen, obtained the following substances from the *Althea officinalis*:—water, gum, sugar, fat oil, starch, albumen, lignin, different salts, a transparent substance which is not acid, and crystallizes in octahedrons (althein), malate of althein. Althein is obtained by the following process:—Prepare a cold watery extract of marshmallow root, treat it with boiling alcohol, which dissolves acidulous malate of althein, oil, &c. Mix all the spirituous decoctions, which become turbid as they cool, pour off the clear solution, treat the crystalline deposit with water, filter the solution, and evaporate it with a gentle heat to the consistence of a syrup, and set it aside to crystallize. The crystals obtained are to be washed with a small quantity of water, and dried upon paper. These crystals appear to the unassisted eye in the form of grains, needles and feathers, and stars, but when examined with a glass they exhibit the hexahedral form. They are of a magnificent emerald-green colour, remarkably distinct, inodorous and unalterable by exposure to the air; they redden litmus paper, are soluble in water, but insoluble in alcohol. The aqueous solution of these crystals, treated in the cold with magnesia, and filtered, restores the colour of litmus which has been reddened by an acid, and renders syrup of violets green: by evaporation the althein is obtained separate from the malic acid, and it possesses the following properties:

It

It crystallizes in regular hexagons or in rhombic octahedrons; it converts syrup of violets to a green colour, restores the colour of litmus reddened by an acid; it is transparent, inodorous, nearly insipid, unalterable in the air, very soluble in water, insoluble in alcohol, soluble in acetic acid, with which it forms a crystalliziable salt.—*Ibid.*

METEORITE WHICH FELL NEAR FERRARA IN 1824.

This stone, like that of l'Aigle and most of the same nature, contains a great number of small globules of iron, which are ductile and flatten under the pestle; it differs however in appearance from other meteoric stones in containing small white grains, which are supposed to be a peculiar substance, probably amphigène.—It was impossible to separate them by mechanical means from the other substances which compose the stone. M. Laugier analysed the meteorite with the following results:

Peroxide of Iron	43
Silica	41·75
Magnesia	16·
Oxide of Chrome	1·50
——— Nickel	1·25
Sulphur	1·
	104·50

From this amount there must be deducted 9·2 of oxygen, by which the 30 parts of metallic iron that had been separated from the stone by the magnet, are in the analysis converted into peroxide;—this reduces the total to 95·3. This analysis shows that this stone is composed of the same elements as the greater number of meteorites, but that the sulphur and nickel are in smaller quantity than usual.—*Ann. de Chim. et de Phys.* Feb. 1827.

COMPOSITION OF IRON SCALES.

M. Berthier in analysing the above substances, concluded that they are composed of 2 atoms of protoxide of iron, and 1 atom of peroxide. M. Mosander has examined the scales produced by exposing a bar of iron to a red heat: his results differ from those of M. Berthier;—the mean of three analyses gave

	External layer.			*Internal layer.*		
Protoxide of Iron	64·97	Oxygen	14·79	64·21	Oxygen	16·90
Peroxide of Iron	34·65	———	10·63	34·77	———	7·56
Silica	0·38	———	0·20	1·02	———	0·53
	100·00			100·00		

M. Mosander remarks, that deducting the silica and the protoxide of iron with which it forms a silicate, the oxygen of the protoxide in the external layer is to that of the peroxide : : 4 : 3, and in the internal layer : : 2 : 1.—Consequently, the composition of the external layer may be considered as 2 atoms of protoxide and 1 atom of peroxide, and the external layer as 3 atoms of protoxide and 1 atom of peroxide. The composition of the first agrees exactly with that given by M. Berthier for the entire scales: this coincidence may be explained by the impurity of the scales, which

which sometimes contain a considerable proportion of silica. Having ascertained the composition of the two layers which form the scales, M. Mosander examined whether they were similar throughout: he found the outer part of the external layer contained more peroxide than the entire scale, viz. 52·77 instead of 34·65, but the second layer, in contact with the iron, was found to be homogeneous throughout. It results from these experiments that these scales are not homogeneous; that the first layer contains more peroxide in proportion to its nearness to the exterior surface; but the second layer is homogeneous, and may be considered as formed of 3 atoms of protoxide with 1 atom of peroxide.—*Ann. de Chim. et de Phys.* Feb. 1827.

QUANTITY OF SILVER AND GOLD RAISED IN GUANAXUATO.

The following statement, drawn from the registers at the Assay Office, show a considerable decrease in the amount of silver and gold raised in Guanaxuato, before and after the year 1810; the produce of silver being reduced to a fourth, and that of gold to a fifth of what it had been in the year 1809.

Before the year 1810.			*After the year* 1810.		
Years.	Silver Marks.	Gold Marks.	Years.	Silver Marks.	Gold Marks.
1801 ..	342,608	1,457	1810 ..	511,445	1,412
1802 ..	502,497	1,676	1811 ..	270,206	550
1803 ..	750,887	1,538	1812 ..	357,930	907
1804 ..	755,861	2,228	1813 ..	292,211	462
1805 ..	723,789	2,495	1814 ..	337,795	708
1806 ..	618,417	2,188	1815 ..	275,905	841
1807 ..	578,735	2,396	1816 ..	269,711	694
1808 ..	617,474	1,842	1817 ..	199,706	523
1809 ..	620,012	2,189	1818 ..	155,112	401

Report of the United Mexican Mining Association, March 1827.

NATIVE IRON FOUND IN CANAAN. (CONN. U. S.)

The following notice is from Silliman's *Journal*, March 1827.—"We are informed by Mr. W. Barrall, in a letter dated August 16, 1826, that his father was surveying a piece of land on the mountain, about three years since, and by accident noticed a black vein in a quartz rock; he pounded upon it sometime with a stone, and with considerable difficulty got out two small pieces, the largest of which is in our possession.—He has never been at the place since; and probably no other person has discovered it or knows where it is. It is surrounded by woods one or two miles on every side, and it is on the top of a mountain 700 or 800 feet above the common average of the land in the town. Mr. Barrall says there is evidence in that quarter of masses of iron or its ores, of considerable extent, as his compass was materially affected; but the particular vein from which he obtained the pieces appeared to be of no great extent; and the width is the same as that of the piece in our possession, which measures two inches wide and two thick.—It weighs eight ounces.

The following notice of the same facts has been received from Mr. C. A. Lee.

Native iron, on Canaan mountain, a mile and a half from the South Meeting-house. This is particularly interesting, as it is the first instance in which native iron, not meteoric, has been found in America. It was discovered by Major Barrall of Canaan, while employed on surveying several years ago. It formed a thin stratum or *plate*, in a mass of mica slate, which seemed to have been broken from an adjoining ledge. It presents the usual characters of native iron, and is easily malleable. For some distance around the place where it was found the needle will not traverse, and a great proportion of the tallest trees have been struck with lightning. Whether these phænomena are connected with the existence of a large mass of native iron, I leave for others to determine; the facts, however, may be relied on.

Physical and Chemical properties of the native iron of Canaan, ascertained in the laboratory of Yale College, by Mr. C. U. Shepard, at the request of the Editor.—In its first appearance to the eye, the native iron of Canaan resembles highly crystalline plumbago; being every where invested with a thin coating of this mineral, which completely defends it from oxidation. Its structure is visibly crystalline; separating with considerable readiness into pyramidal masses, and more usually into oblique tetrahedra. This cleavage however never takes place without the intervention of thin scales of plumbago. It falls considerably short of meteoric iron in malleability, toughness and flexibility; as well as the silvery whiteness of its colour, which in part is no doubt due to the plumbago diffused through it. In hardness and magnetic properties it does not differ perceptibly from pure iron. Its specific gravity varies from 5·95 to 6·72.

Intermingled with it, occasionally, is *native steel*. One angular fragment, weighing about eight grains, was perfectly brittle, sufficiently hard to scratch glass, and possessed of the characteristic granular structure and silvery white colour of steel. With the microscope no scales of plumbago were noticeable in it. Dissolved in dilute nitric acid, it afforded an evident quantity of black carbonaceous matter, upon the surface of the solution.

A fragment of the native iron, weighing 100 grains, was dissolved in dilute nitro-muriatic acid. The plumbago attached to it being left behind, was separated, and found to weigh six grains. To the solution was added in excess, perfectly caustic liquid ammonia, by means of which the iron was thrown down; the ammoniacal solution was then examined for lead, copper, or any other metal which might be present, by adding to it hydro-sulphuret of ammonia. No precipitate nor change of colour was produced, though suffered to remain for several days, which leads to the conclusion that our mineral is unalloyed by any metal. In this respect, therefore, it differs from the native iron of Saxony, in which Klaproth found lead 6·0, and copper 1·50. The iron being washed and heated, weighed 127 grains; which being in the state of a peroxide, according to Mr. Children indicated 88·90 metallic iron, or according to Klaproth, 92·21 metallic iron.

To secure greater accuracy, the process was repeated with 50 grains

grains of the mineral, from which were separated 3·50 grains plumbago: the iron was precipitated as before; and after being heated weighed 63 grains, which according to Children indicated 44.10 metallic iron, or by Klaproth's rule 45 90.

Remarks by the Editor (*Dr. Silliman*).—There can be no question but the native iron above described is a genuine production of the earth; and that it holds no connection with meteoric iron.—The mass bears the marks of a true metallic vein, it has smooth sides (saalbandes), and small specks of a blue and white quartz are sticking in it; nickel, constantly found in the meteoric irons, is absent from this specimen; and if it were a question whether native iron be a true production of mines, this discovery decides it.—Y. C. Feb. 15th, 1827.

DISCOVERY OF FOSSIL HYÆNAS IN KENT.

A most interesting discovery has, within these few days, been made in this county, by John Braddick, Esq. of Boughton Mount, of the fossil remains of an extinct species of hyæna and some other antediluvian animals, in the extensive quarries of Boughton, about three miles south of Maidstone. These quarries appear to have been worked for many centuries; and there is a tradition that many of the materials of Westminster Abbey, and other ancient buildings in London, were brought from hence; they have lately been extensively wrought by Mr. Braddick, for the purpose of erecting buildings on his estate. The stone is designated most commonly by the name of Kentish Rag: it consists of a succession of beds of limestone and coarse flint dispersed in irregular thickness through a matrix of sand and sandstone; its geological position is in the lowest region of the green-sand formation immediately above the weald clay. The remains in question consist of the jaws, teeth, and broken portions of the skull, together with bones of the fore and hind legs of a very large hyæna, and a few other teeth and bones apparently of the ox and horse. All these were found nearly together, within the space of a few feet in one of the numerous cracks or fissures (locally called *vents*) that intersect the strata at this place, and are usually from one to twenty feet broad: on the sides of many of these vents are hollow apertures of various sizes, some of which occasionally expand themselves into caves: two such caves have lately been destroyed in the quarries on the north side of the valley, at Boughton Mount. These fissures or vents cut through the strata from the bottom of the quarries to the surface, and are filled with diluvial loam, interspersed with fragments of the adjacent rocks, and numerous chalk-flints; these last must have been drifted hither from some distant hills, and have fallen into the fissures at the same time with the loam. This loam at its upper extremity becomes united to that which covers the surface of the quarry and the adjacent fields. The bones were discovered at about fifteen feet deep in one of these fissures; and from the manner in which they were scattered amongst the loam and stony fragments, they appear to have been drifted to their present place at the same time with the diluvial matter, amongst which they lay occupying a position precisely

cisely similar to the bones of hyænas and other animals, that were discovered in the fissures of the break-water limestone rock, near Plymouth, embedded in similar diluvial loam and pebbles. It is highly probable that at Boughton, as was the case at Plymouth, the caves communicating with these fissures will be found to contain an abundance of similar bones. Mr. Braddick's workmen say they have frequently found them in his quarries, but always neglected to preserve them; one fine head was thus lost but a few weeks ago:—enough, however, has already been done to show that the hyæna was among the antediluvian inhabitants of Kent, as it has been proved to have been among those of Yorkshire and Devon; and it is highly probable that if the proprietors of quarries in this country will reward their workmen for preserving whatever teeth, or bones, or fragments of bones, they may dig up in the course of working their stone, many similar discoveries will soon be made. Professor Buckland and some other gentlemen of the Geological Society of London have this week visited Mr. Braddick's quarries, and entertain the most sanguine expectations that his further researches therein will be attended with success. Mr. B. has added materially to the value of his discovery, by communicating information of it immediately to the Geological Society of London, as well as by presenting the specimens to their museum.—*Maidstone*, June 12, 1827.

ON REGISTER RAIN-GAUGES. BY B. BEVAN, CIVIL ENGINEER.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

Observing on the cover of your Magazine for the present month a request by one of your readers, relative to my rain-gauges, I take the first opportunity of complying therewith, by giving the following specification of the several parts.

The part usually called the gauge, we may distinguish by the name of the *collecting* vessel, which is in the form of an inverted cone, with a base of 12 inches diameter: from the bottom of this collecting-vessel passes a tube of about $\frac{1}{4}$ of an inch diameter to the *receiving*-cylinder of 6 inches diameter and 36 inches depth. In the receiving cylinder is a copper float, of about $5\frac{1}{2}$ inches diameter and about 2 inches height, having a socket on the middle of the upper side, to support a light rod of deal about 5 feet in length, near the upper part of which is fixed a small frame with friction rollers to support a black-lead-pencil; the pencil is kept upon the rollers by a small weight, and is also pressed forwards by another small weight, against a sheet of paper which is fastened upon a brass cylinder of 2 feet in length and about 5 inches in diameter; the brass cylinder is connected by a line and pulley wheel with a time-piece, so as to revolve uniformly at any pace that may be required. The whole of the apparatus, except the first-mentioned conical vessel, is placed under cover, where most convenient: the deal rod which carries the pencil is about 4 inches wide and $\frac{1}{4}$ inch thick, and passes between two vertical guides, to insure the parallel position of the pencil.

From this description I presume it will be easy to comprehend the

the operation. Thus the moment the rain begins to fall into the conical collector, it is conducted by the tube into the receiving-cylinder, and begins to raise the float, and with it the deal rod, with its pencil, which makes an oblique line upon the paper, compounded of the vertical motion of the pencil and the horizontal motion of the surface of the brass cylinder, and shows the quantity fallen by the total height of the oblique line, and the rate of falling by the angle of obliquity, and the time of commencing and termination of each shower by the distances along the line.

All the attention requisite is to wind up the time-piece from time to time, and to take off the paper from the cylinder and replace it with a fresh sheet, marking the time on the paper when it is put on.

The following table is an abstract of one of my gauges for the year 1817: in the course of this year, there were 21 days in which the time-piece was more or less out of order, or omitted to be wound up; there were, however, eight complete months.

It appears by the following abstract, that in 344 days there were but $614\cdot\frac{1}{10}$ hours *actual rain*, being at the rate of $1^h\ 47^m$ per day.

The greatest *rate* of raining, I found to be on the 30th of June, which for a few minutes was at the rate of $42\frac{1}{4}$ inches per day.

Yours, truly, B. BEVAN.

Leighton, June 11, 1827.

Abstract of Registering Rain-gauge, 1817, at Leighton Buzzard. Latitude 51° 54′ 56″. Longitude $2^m\ 39^s$ West: the collecting vessel about 10 feet above the surface of the ground, and about 300 feet above the level of the sea.

1817.	Number of days the time-piece was in action.	Number of wet days.	Number of separate showers.	Longest time of a single shower.	Greatest amount of one shower.	Average time of each shower.	No. of hours rain in the time regist^d	Depth of rain in same time.	Depth of rain in the whole month.
				h	inch.	h.	h.	inch.	nch.
January......	23	8	8	11	0·11	2·6	20·8	0·22	2·01
February	27	11	12	7·5	0·19	2·4	28·7	0·75	0·85
March	31	14	15	10·2	0·47	2·8	42·2	1·40	1·40
April........	30	2	2	3·3	0·06	2·2	4·4	0·07	0·07
May	21	9	8	16·0	0·22	7·1	56·7	0·42	2·87
June........	30	14	17	13·3	0·34	3·5	59·4	1·98	1·98
July	31	16	24	7·5	0·54	2·4	57·2	2·83	2·83
August	31	21	34	13·5	0·59	2·8	94·7	4·35	4·35
September ..	30	9	9	7·2	0·12	4·4	39·2	0·50	0·50
October	31	11	11	8·0	0·21	3·9	42·4	1·21	1·21
November....	30	11	11	18·5	0·36	6·5	71·6	1·42	1·42
December....	29	17	21	20·6	0·52	4·6	97·1	2·32	2·72
Time omitted.	21								
Quantity not registered..	......	....	....	....	....	....	....	4·74	
Total	365	....	....	....	....	....	....	22·21	22·21

MR.

MR. SQUIRE'S COMPARATIVE OBSERVATIONS OF THE SOLAR ECLIPSE OF NOV. 29, 1826.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

In No. I. of the New Series of the Philosophical Magazine and Annals, you were so good as to insert my observations of the solar eclipse of November last; but having now the Greenwich observations of the end of this eclipse, and being desirous of comparing the observations taken at the two places, I have for this purpose made a computation of the *end* at this place, and by which I find that my observation agrees so well with the mean of those taken at Greenwich, that I am induced to trouble you with the results. In calculating the end for Epping, (latitude 51° 41′ 41″·6, longitude 27 seconds E. of Greenwich,) I have taken into consideration the spheroidical figure of the earth, and applied every other correction that the present refined state of astronomy requires, the same as was done in the computations for Greenwich.

In the following summary of the calculations and observations, the instants are given in mean solar time, according to the meridian of the Royal Observatory.

	d	h	m	s
End of the eclipse at Epping, by calculation, Nov.	29	0	0	11·74
Ditto at Greenwich by ditto	28	23	59	58·84
Difference				12·9

	d	h	m	s
End at Epping, by observation..............	29	0	0	27
Ditto at Greenwich by ditto................	29	0	0	14·55
Difference				12·45

Hence it appears that, owing to the effects of the lunar parallax, the termination of this eclipse at Epping was absolutely later than at Greenwich by 12·9 seconds in the former case, and 12·45 in the latter, making a second difference of only ·45 of a second between the computed and observed times at the two places; a nearer approximation could hardly have been expected.

Again,

		d	h	m	s
Epping..	Observed time of the end..	29	0	0	27
	Calculated time of ditto ..	29	0	0	11·74
	Difference				15·26
Greenwich	Observed time of the end ..	29	0	0	14·55
	Calculated time of ditto ..	28	23	59	58·84
	Difference				15·71

From which it appears that the observed time at Epping was later than

than the computed by 15·26 seconds, and at Greenwich 15·71 seconds,—making a difference as before of ·45 or 27 thirds.

Yours respectfully,

Epping, June 12, 1827. THOMAS SQUIRE.

Observations of the end* of the Solar Eclipse, Nov. 29, 1826, at Greenwich Observatory.

		h	m	s	
With 25-inch achromatic by	Mr. Rogerson	0	0	11·5	Mean Time.
46 ———	Mr. Taylor, Sen.	0	0	12·4	
5 feet ———	Mr. Richardson	0	0	13·4	
5 —— equatorial —	Mr. Pond	0	0	15·5	
	or	0	0	17·5	
30-inch achromatic by	Mr. Taylor, Jun.	0	0	16·5	
5-feet Newtonian —	Mr. Henry	0	0	18·0	

NEW PATENTS.

To W. J. H. Hood, of Arundel-street, Strand, Lieut. R. N. for improvements on pumps, chiefly applicable to ships.—Dated the 26th of May, 1827.—6 months allowed to enrol specification.

To G. Burges, of Bagnigge Wells, for improvements in the construction of wheeled-carriages.—26th of May.—6 months.

To T. Clarke, of Market-Harborough, for improvements in manufacturing carpets.—26th of May.—4 months.

To Malcolm Muir, of Glasgow, for machinery for preparing boards for flooring and other purposes.—1st of June.—2 months.

To J. W. Clarke, of Tiverton, for his improved mode of attaching, fixing, or securing the dead eyes to the channels and sides of ships.—8th of June.—6 months.

To J. C. Daniell, of Stoke, Wiltshire, for improvements in preparing wire cards, and dressing woollen and other cloths.—8th of June.—6 months.

To C. Phillips, esq. of Rochester, Capt. R. N. for improvements on capstans.—8th of June.—6 months.

To Hugh Evans, of Great Surrey-street, Surrey, Lieut. of Marines, and W. R. Hale King, of No. 66, Snow Hill, for their new table apparatus to promote the ease, comfort, and œconomy of persons at sea.—12th of June.—6 months.

To S. Robinson, of Leeds, flax-dresser, for improvements in machinery for hackling or dressing and clearing hemp, flax, and tow.—16th of June.—6 months.

METEOROLOGICAL OBSERVATIONS FOR MAY 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·15 May 21. Wind NW.—Min. 29·09 May 6. Wind SW.
Range of the mercury 1·06.

Mean barometrical pressure for the month . . . . 29·758
Spaces described by the rising and falling of the mercury . . . 5·430
Greatest variation in 24 hours 0·570.—Number of changes 19.

* The beginning could not be observed in consequence of clouds.

Therm.

Therm. Max. 72° May 21. Wind W.—Min. 40° May 7 & 12. Wind NE. Range 32°.—Mean temp. of exter. air 56°·31. For 31 days with ☉ in ♉ 53·19 Max. var. in 24 hours 21°·00—Mean temp. of spring water at 8 A.M. 50°·22

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 5th . . . . 95°
Greatest dryness of the air in the afternoon of the 13th 33
Range of the index 62
Mean at 2 P.M. 54°·1—Mean at 8 A.M. 60°·1—Mean at 8 P.M. 70·9
—— of three observations each day at 8, 2, and 8 o'clock . . 61·7
Evaporation for the month 3·05 inch.
Rain near ground 2·125 inch.—Rain 23 feet high 1·965 inch.
Prevailing Wind S.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 14½; an overcast sky without rain, 7; foggy ½; rain, 6.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
20	9	28	0	22	21	18

Scale of the prevailing Winds.

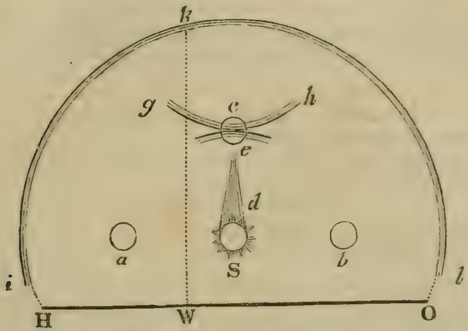
N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1	3½	3	2½	3	10	4½	3½	31

General Observations.—This month has been alternately dry and wet, with frequent gales of wind; and although it has rained more or less on eighteen different days, yet the aggregate quantity has only been sufficient to keep the herbage and vegetation in a good growing state, the light showers having been quickly dried up by the sunshine and winds. As April was rather dry, the showers in the first part of the period were very beneficial; for they in some measure counteracted the blighting effects of the easterly winds, and hoar-frosts on several mornings. From the 21st to the end of the month the atmosphere was generally loaded with moist clouds, which for several days were in contact with the earth's surface, and in their passage afforded frequent irrigations, insomuch that the roots of the grass have been much thickened, and bear a striking contrast to the appearance last year; the wheat plants changed to a fine verdant colour, and vegetation brought forward surprisingly. The bloom of the trees, in general, was fine and luxuriant, and the state of the period, excepting two or three cold days, fair growing weather: the temperature of the air, however, has often been variable, and trying to delicate constitutions, and caused agues, colds, rheumatism, &c.

From six till after seven o'clock in the evening of the 25th, unusual atmospheric *phænomena* presented themselves here, as exhibited in the annexed figure, where the line HO represents the western horizon, S the sun, *a b c* three *parhelia*, each 22½ degrees distant from the sun's centre; they were nearly the apparent size of the sun, and composed of intermixed prismatic colours. *d e*, a broad whitish column of light reflected upwards and terminated in a point; *g h* part of an inverted arc of coloured vapour; *i k l* a solar halo 90 degrees in diameter, bounded by red, the interior prismatic colours being faint; and W *k* the prime vertical. When it is stated that the solar halo is generally 45 degrees in diameter, the present one being 90 degrees made the phænomenon the more singular, especially as no halo of the common extent could be traced within the parhelia; only

two

two halos of this large diameter have been seen here the last eleven years. All these phænomena appeared in lofty attenuated cirrostrative vapour, lowered agreeably to the sun's descent, and slowly disappeared as the vapours moved off out of the refracting angles of the sun's rays.



The atmospheric and meteoric *phænomena* that have come within our observations this month, are five *parhelia*, four solar halos, three meteors, one double rainbow, lightning and thunder on three different days; and eight gales of wind, or days on which they have prevailed; namely, one from N.E., one from S.E., five from S.W., and one from N.W.

REMARKS.

London.—May 1—3. Fine. 4. Cloudy. 5, 6. Rainy. 7. Fine. 8. Overcast, cold. 9. Cloudy, fine. 10—13. Fine. 14. Showery. 15. Fine. 16. Cloudy, fine. 17. Showery. 18—21. Fine. 22, 23. Showery. 24. Showery: hail about noon. 25. Rainy. 26, 27. Showery. 28. Cloudy. 29. Showery. 30, 31. Fine.

Boston.—May 1, 2. Cloudy. 3, 4. Fine. 5, 6. Cloudy. 7. Fine. 8, 9. Cloudy. 10—13. Fine. 14, 15. Cloudy. 16, 17. Fine. 18. Rain. 19—21. Fine. 22. Rain. 23—25. Fine. 26—30. Cloudy. 31. Stormy.

Penzance.—May 1. Fair, misty. 2. Misty. 3, 4. Fair. 5. Cloudy, rain. 6. Fair. 7. Cloudy: rain. 8. Fair: clear. 9. Clear. 10. Fair: clear. 11, 12. Clear. 13. Fair. 14. Clear. 15. Fair. 16. Rain. 17. Cloudy: rain. 18. Clear. 19. Fair: clear. 20. Misty: rain. 21. Misty: clear. 22, 23. Misty: rain. 24, 25. Showers. 26. Showers: hail. 27. Coudy: rain. 28. Rain: showers. 29. Fair. 30. Fair: rain. 31. Showers: fair.

RESULTS.

London.—Winds, N. 1: N.E. 1: E. 7: S.E. 1: S. 1: S.W. 9: W. 3: N.W. 5: var. 3.

Barometer: Greatest height for the month	30·26 inch.
Least	29·32
Mean	29·9338
Thermometer: Greatest height for the month	79°
Least	30
Mean	52·0967
Rain	2·07 inch.
—— in a second gauge	1·96
Evaporation	2·80

Meteoro-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.										
May 1	30·18	30·17	29·84	29·84	30·04	30·02	29·63	74	47	61	46	66	48	57	var.	SW.	W.	calm	...	...	...	...	...	...
2	30·17	30·10	29·78	29·76	30·00	29·96	29·70	64	40	59	49	63	51	49·5	E.	SW.	S.	calm	...	...	...	...	...	...
3	30·10	30·06	29·76	29·74	29·94	29·92	29·54	74	42	59	49	68	50	62	NW.	W.	W.	NW.	...	0·30	...	...	...	...
☽ 4	30·06	29·82	29·70	29·68	29·90	29·79	29·43	68	50	59	49	65	51	61·5	NW.	W.	SW.	calm	...	...	...	...	...	...
5	29·82	29·50	29·38	29·30	29·66	29·52	29·20	60	42	58	51	60	51	59	SW.	SW.	SW.	W.	...	...	0·40	0·700	0·250	...
6	29·82	29·32	29·20	28·92	29·42	29·09	29·00	60	43	56	49	61	45	56	SW.	SW.	NW.	calm	...	·20	·27	0·300	·520	0·08
7	30·02	29·82	29·36	29·20	29·68	29·55	29·40	58	30	56	48	53	40	48.5	E.	N.	E.	E.	...	...	...	...	·225	·12
8	30·02	29 98	29·55	29·55	29·81	29·80	29·64	55	35	54	46	53	46	46.5	E.	SE.	NE.	calm	...	...	...	...	·010	...
9	29·98	29·93	29·50	29·50	29·76	29·75	29·60	54	36	58	42	60	45	42	NE.	E.	NE.	S.	...	·15	...	...	...	...
10	29·98	29·94	29·54	29·50	29·75	29·74	29·46	60	32	56	43	64	43	50	E.	NW.	S.	W.	...	...	...	...	...	·06
○ 11	30·25	29·98	29·70	29·58	29·92	29·75	29.46	64	34	54	40	64	44	54·5	W.	NW.	NW.	calm	...	...	...	...	·010	...
12	30·25	30·13	29·76	29·75	30·07	30·05	29·83	62	36	58	44	57	40	53·5	SE.	SE.	NE.	E.	0·96	·35	...	...	...	...
13	30·13	30·02	29·68	29·60	29·96	29·84	29·66	63	42	56	44	59	44	50	E.	SE.	NE.	E.	...	...	...	...	...	...
14	30·02	29·97	29·70	29·68	29 83	29·80	29·55	55	46	61	43	56	48	54	N.	NE.	N.	NE.	...	...	·12	...	·055	...
15	29·97	29·77	29·52	29·38	29·77	29·67	29·35	64	43	59	44	67	48	50	NW.	SE.	S.	W.	...	·40	...	...	...	·46
16	29·77	29·71	29·20	29·00	29·51	29·34	29·30	69	49	55	50	65	50	57	E.	SE.	E.	E.	...	...	·11	1·030	·085	...
☾ 17	29·78	29·73	29·15	28·96	29·54	29·51	29·17	68	54	58	46	62	53	57	S.	S.	SE.	S.	...	...	·37	1·100	·240	·08
18	30·05	29·78	29·50	29·30	29·77	29·60	29·22	70	50	61	47	64	50	57	SW.	W.	SW.	SE.	...	·20	...	0·770	...	·31
19	30·19	30·05	29·70	29·64	30·01	29·90	29·50	74	50	63	49	65	47	57·5	var.	S.	NW.	S.	...	...	...	...	...	·05
20	30·25	30·19	29·72	29·69	30·05	30·04	29·56	78	43	61	52	67	51	65	E.	SW.	W.	SE.	·88	...	...	0·170	...	...
21	30·26	30·25	29·90	29·90	30·15	30·14	29·63	79	50	63	53	72	53	60	NW.	W.	NW.	NW.	...	·50	...	...	...	...
22	30·25	30·12	29·90	29.88	30·14	30·06	29·52	66	52	63	53	65	53	56·5	SW.	W.	W.	NW.	...	...	·06	0·055	·065	·09
23	30·12	29·59	29·80	29·45	30·00	29·68	29·45	72	51	61	53	71	51	63·5	SW.	W.	SW.	W.	...	...	·12	...	·260	...
24	29·59	29·53	29·30	29·28	29·43	29·37	28·95	60	44	57	49	63	46	58	NW.	NW.	NW.	W.	...	·30	·15	0·200	·130	·26
● 25	29·60	29.54	29·26	29·24	29·39	29·37	28·90	62	44	56	48	61	50	55·5	var.	NW.	SW.	W.	...	...	·30	0·210	·085	·13
26	29·74	29·60	29·30	29·24	29·50	29·39	29·03	65	46	56	46	66	51	57·5	W.	NW.	SW.	calm	...	...	·09	0·170	·010	·03
27	29·85	29·74	29·40	29·40	29·66	29·60	29·15	61	52	58	50	65	54	56	W.	NW.	NW.	W.	...	·25	·05	0·100	·040	·28
28	29·92	29·85	29·42	29·40	29·81	29·70	29·16	71	56	60	52	69	54	61	SW.	W.	SW.	W.	...	...	...	0·080	·030	·11
29	30·02	29·92	29·44	29·42	29·78	29·75	29·22	64	50	63	53	65	54	56	SW.	SW.	SW.	W.	...	...	·03	0·305	·110	...
30	30·07	29·85	29·50	29·40	29·86	29·74	29·39	75	54	56	53	70	56	60	SW.	S.	SE.	calm	...	...	...	...	...	...
31	29·86	29·85	29·50	29·34	29 78	29·67	29·13	70	48	61	52	67	51	64	SW.	SW.	SW.	W.	·96	·40	...	0·125	...	...
Aver.:	30·26	29·32	29·90	28·92	30·15	29·09	29·37	79	30	63	40	72	40	55·9					2·80	3·05	2·07	5·315	2·125	2·06

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

AUGUST 1827.

XVI. *On the Royal Observatory at Palermo.*

A NEW volume has just appeared of the Astronomical Observations made at this celebrated observatory, edited by M. Cacciatore. The preceding volumes were published under the superintendence of the late illustrious conductor of that observatory, M. Piazzi: and the present volume, which may be considered as a continuation of that work, is the production of his distinguished successor. The work contains not only the observations made by M. Cacciatore himself, but also the result of many observations made by his predecessor not hitherto published. It extends therefore over several years. As the *form*, in which the observations of Palermo are published, differs from that of every other observatory, and possesses many advantages over the ordinary method, it may be acceptable to our readers to see a brief abstract of the contents of the present volume.

Instead of presenting a *regular* journal of daily observations, in the order in which they were recorded, we are here furnished with a number of interesting dissertations on various branches of the science, arising out of the observations that have, from time to time, been made. The observations, relative to each subject, are selected and brought together under one head: first, the observations as they were made, and as they stand in the observation book; secondly, as *reduced* either to the day of observation, or to some other given epoch. In this latter shape they are presented to astronomers in an useful and tangible form; and can readily and instantly be made use of, as occasion may require.

Thus, in the case of the *planets* (which is the subject of the first chapter), all the observations relative thereto are taken out of the observation book; and, in general, some star in the vicinity of the planet is also observed and recorded: a method which tends to correct any error that may inadvertently have occurred in the register, or in the observation. After this, a table is given wherein the planets are arranged, according to their position in the system, and all the observations relative to each planet are *reduced* to the day of observation. This table consists of five columns: 1° the mean time of the transit; 2° the *reduced* right ascension; 3° the *reduced* declination; 4° the computed longitude; and 5° the computed latitude corresponding thereto. Another table then follows, containing the mean time of the *opposition* of the superior planets, together with their deduced longitudes and latitudes at those respective periods. The utility and convenience of these arrangements are too apparent to be insisted on.

The second chapter consists of *eclipses* and *occultations*. The original observations are here also recorded and printed, together with the name of the observer, the power of the telescope, and the *particular clock* used in the observation. And, in order that the time may be correctly determined, the transit of some well-known star, near the time of observation is also given. The *correct mean time* of each observation is afterwards given in a subsequent table: and the whole is thus *again* presented to our view in *another* and a more perfect form.

The next division of the work contains observations of the *sun*, near the solstices and near the equinoxes, from the year 1817 to the end of 1825. The observations near the solstices are afterwards presented in *another* form, by grouping all those which relate to each solstice separately; and *reducing* them to the moment of the sun's passing the solstitial colure. The computations are given in detail: the table of refraction employed, together with the constant of nutation, and other quantities are distinctly pointed out: and the reader is not left in doubt as to any of the corrections. The result of the investigations is, that there appears to be a difference in the obliquity of the ecliptic, according as the summer or winter observations are used in the computations. The mean of 104 observations made in *summer* gives the mean obliquity for January 1, 1820, equal to 23° 27′ 48″,08: and the mean of 50 observations made in *winter*, gives 23° 27′ 42″,73. The *mean of the whole* (regard being had to the number of observations of each kind) is 23° 27′ 46″,35.

M. Cacciatore next discusses the cause of this singular phænomenon, which is one indeed that has generally been noticed at other observatories; and which has been supposed to arise from one or other of the following circumstances: 1°

a bending

a bending of the *metallic* wires in the focus of the telescope: 2° a bending of the telescope itself; each varying according to the temperature of the atmosphere: 3° a want of information as to the true value of the *mean* refraction, or the *corrections* to be applied thereto: 4° a modification of the rays of light by the action of heat. The first two he gives up as groundless: and to the last he does not attach much importance. With respect to the refraction, he has shown that it cannot, under any circumstances, account for the whole of the difference; although it is possible that a portion of that discordancy may be occasioned thereby. In order to set this in a clearer point of view, he has given a small table (which we shall here subjoin), in which is contained the amount of refraction according to the several tables of Piazzi, Carlini, Delambre, Young, and Bessel, on the day of the summer and winter solstices respectively, for the several observatories of Palermo, Milan, Paris, Greenwich and Konigsberg. The altitude of the sun at each place, on those days, is added; together with the assumed mean state of the thermometer. The barometer has been assumed equal to 29·6 inches. These elements are sufficiently correct for the comparisons proposed.

Summer Solstice.

Observatory =	Palermo.	Milan.	Paris.	Greenwich.	Konigsberg.
Thermom. =	75°	72°	76°	62°	60°
Altitude . . =	14° 39′	22° 0′	25° 21′	28° 0′	31° 13′
Piazzi . . . =	15″,02	23″,10	27″,12	30″,60	34″,78
Carlini . . . =	15 ,14	23 ,40	27 ,45	30 ,80	35 ,11
Delambre . =	15 ,05	23 ,24	27 ,31	30 ,66	34 ,91
Young . . . =	14 ,91	23 ,28	27 ,26	30 ,58	34 ,83
Bessel . . . =	15 ,01	23 ,19	27 ,19	30 ,53	34 ,78

Winter Solstice.

Observatory =	Palermo.	Milan.	Paris.	Greenwich.	Konigsberg.
Thermom. =	58°	35°	43°	38°	30°
Altitude . . =	61° 33′	68° 53′	72° 15′	74° 53′	78° 6′
Piazzi . . . =	1′ 46″,42	2′ 28″,85	2′ 58″,92	3′ 30″,30	4′ 26″,45
Carlini . . . =	1 46 ,52	2 29 ,00	2 59 ,23	3 31 ,43	4 28 ,77
Delambre . =	1 46 ,03	2 28 ,07	2 58 ,00	3 30 ,33	4 27 ,28
Young . . . =	1 45 ,71	2 27 ,58	2 57 ,83	3 29 ,77	4 26 ,20
Bessel . . . =	1 45 ,56	2 27 ,50	2 57 ,34	3 29 ,21	4 25 ,64

It is evident, therefore, that the difference between the two solstices cannot wholly arise from the tables of refraction employed: and M. Cacciatore was consequently induced to ex-

tend

tend his investigations as to other probable causes of this phænomenon.

Amongst the various causes that might be assigned for this remarkable anomaly M. Cacciatore imagined that the heat of the sun on the circle might have the greatest effect: and he has entered into an elaborate investigation of the subject with a view to determine this curious point. We have not room for a detail of the steps of the process pursued by this learned astronomer: but it may suffice at present to state that, on the presumption that the circle was divided at the temperature of 54° Fahr. (which, he informs us, was the case) the corrections for the readings of the microscopes on the limb will be

For the summer solstice $= (t - 54°) \times 0'',06931$
For the winter solstice $= (t - 54°) \times 0'',29133$

By the help of these formulæ, and the application of the method of minimum squares, he finds the mean obliquity for January 1, 1809, to be as under: viz.

From the summer solstice	= 23° 27′ 51″,57	340 obs.
From the winter solstice	= 23 27 51 ,75	182 obs.

whence he deduces the mean obliquity, regard being had to the number of observations, to be on January 1, 1809, equal to 23° 27′ 51″,64. He then compares this result with the determinations by other astronomers, reduced to the same epoch, which are as follows; viz.

	Mean Obliq. Jan. 1, 1809.
Cacciatore	23° 27′ 51″,64
Piazzi	52 ,04
Oriani	51 ,34
Arago	51 ,50
Pond	51 ,32
Bessel	50 ,10
Arithmetical mean	= 23° 27′ 51″,33

M. Cacciatore next investigates the subject of the annual diminution of the obliquity of the ecliptic, and deduces the following results from a comparison of the observations of the under-mentioned astronomers: viz.

Piazzi	= 0″,4546
Maskelyne	= 0 ,4355
Mayer	= 0 ,4620
Bradley	= 0 ,4437
La Caille	= 0 ,4650

The mean of these is 0″,4522, but M. Cacciatore is disposed to reject the result deduced from La Caille's observations; which will therefore reduce the value rather below 0″,45.

He

He next discusses the subject of the nutation of the obliquity of the ecliptic; and by comparing the several observations of the solstices from 1791 to 1821, he obtains the quantity 9″,12. By a different mode however of investigating the subject (that is, by arranging the whole 522 solstitial observations into 52 equations of condition, and by the application of the method of minimum squares), he subsequently obtains 9″,3555 for the nutation, and 0″,4683 for the annual diminution of the obliquity of the ecliptic. The mean of the two results for the nutation is 9″,238; which is almost identical with that deduced by Dr. Brinkley, and which has been assumed by the Astronomical Society of London, in the formation of their recent Tables.

From the solstices M. Cacciatore proceeds to the determination of the equinoxes: and here he finds the same anomaly (and arising partly from the same source) as in the observations of the solstices. For, the length of the year, as deduced from the autumnal equinoxes, is always greater than when deduced from the vernal equinoxes: even after the correction has been made, as already alluded to, for the expansion of the circle. The mean of six years observations gives the length of the tropical year, as deduced from the

Vernal equinox	$=365^{d}\cdot2421514$	$=365^{d}$	5^{h}	48^{m}	$41^{s},872$
Autumnal equinox	$=365\ \cdot2423099$	$=365$	5	48	55 ,575
Mean	$=365\ \cdot2422306$	$=365$	5	48	48 ,724

which is exactly one second less than the quantity assumed by M. Laplace.

From the sun and the planets M. Cacciatore passes to the subject of the fixed stars; there being no mention made of any observations on the moon. This part of the work is confined to a comparison of the observations of 173 stars, taken from different parts of the heavens. The observations of these stars, as made by M. Cacciatore, are compared with the observations made by Flamsteed, Bradley, Mayer, La Caille, Piazzi, Zach and others: and in these comparisons some singular anomalies are discovered. For instance, in comparing 104 *Piscium*, M. Cacciatore deduces the following proper motion in declination: viz.

From 1755 to 1798 = + 0″,058
1798 to 1808 = − 0 ,430
1808 to 1824 = + 0 ,156

Again, the comparison of δ *Trianguli* at different periods, affords the following results for its proper motion in declination: viz.

From

From 1755 to 1798	=	− 0″,256
1798 to 1806	=	+ 0 ,188
1806 to 1812	=	− 0 ,767
1812 to 1824	=	+ 0 ,800

Take another instance in the case of η *Eridani:* the proper motion in declination appears to be as follows: viz.

From 1755 to 1798	=	+ 0″,135
1798 to 1806	=	− 0 ,125
1806 to 1810	=	+ 1 ,500
1810 to 1812	=	+ 1 ,250
1812 to 1824	=	+ 0 ,050
1824 to 1825	=	+ 1 ,000

Several other similar instances might be adduced of an apparently *variable* proper motion: we shall select however only one more, which seems to be the most remarkable. It is the case of 67 *Ursæ Majoris:* the proper motion of which appears to be as under: viz.

		Æ *in arc.*	D.
From 1755 to 1798	=	− 0″,367	0″,000
1798 to 1800	=	− 2 ,150	+ 2 ,600
1800 to 1809	=	− 0 ,710	− 0 ,520
1809 to 1813	=	+ 1 ,150	+ 0 ,675
1813 to 1819	=	− 1 ,480	0 ,000

This subject is certainly a very curious and interesting one, and is deserving of more minute inquiry and investigation: but we very much doubt whether the observations of Bradley or Piazzi (setting aside those of Mayer, La Caille, &c.) were made with sufficient accuracy to serve as the foundation for such a minute investigation. These remarks are not made with a view to diminish the fame of those excellent observers, whose characters stand too high to be thus affected; but when we see that the best practical astronomers of the present day, with the most perfect instruments that ever were made (far superior to any that could have been possessed by their predecessors), differ from each other, not only one or two, but sometimes as much as four and five seconds, in the position of a star, both in right ascension and declination, we may fairly hesitate in assenting to the conclusions which are intended to be inferred from these comparisons. There are moreover too many discordant elements in the *reduction* of observations, too many circumstances to be attended to in the steps of that process, to induce us to assent *instanter* to the results which are thus laid before us. The detail of the method pursued in such cases cannot be too explicit, where the differences are so minute, and the results so important. It is not enough to compare one reduced catalogue with another reduced catalogue, unless

unless we know that the *same mode of reduction* has been adopted in each, and the *same elements* employed in the computations.

The next subject treated of by M. Cacciatore is that of the six comets which he observed at Palermo. These are the comets of 1821, 1822, and 1824, the comet of Encke in 1825, and another comet in 1825, the return of which he also observed in 1826. The same plan is followed here as in the case of the planets. First, the observations, as taken from the observation-book, are given in the order in which they were made. The place of the comet is then reduced, and a separate table is given of those reduced places: and lastly, the elements of the comet are inserted.

The work is closed with numerous meteorological tables, from the year 1791 to 1825, both inclusive. The state of the barometer and thermometer is taken four times in each day: viz. in the morning, at noon, in the afternoon and at midnight; and the *names of the observers* are given. We have also the maximum, the minimum, and the mean state of each instrument for each month in the year, together with the days of the month on which each case occurs. Amongst the most remarkable phænomena recorded in the meteorological journal, we may notice several destructive *earthquakes;* in many of which the concussions were so great as to stop the pendulum of the clock: and in one instance, in 1823 (when several houses were thrown down and fourteen persons were buried in the ruins), the floor of the observatory *opened*, and threatened destruction to the building.

Upon the whole, this work reflects great credit on the distinguished astronomer who now conducts the observatory at Palermo. The plan, although nearly the same as was pursued by M. Piazzi, and therefore not altogether new, is different from the printed observations of other observatories: and is in many respects worthy of imitation. For, although we would not wish to be considered as discouraging the printing of observations in the order, and in the manner in which they are actually made, yet we are certainly desirous, in common with many others that are fond of astronomy, of seeing the *reductions* of those observations a little more frequently than we do; and, at all events, of seeing the *principal results* given at the end of each year. It is in this manner only that the vast mass of observations that annually issue from the press can be made available and useful to the purposes of astronomy, and that we can expect to derive any advantage from the exertions of public or private observers.

B.

XVII. *Letter*

XVII. *Letter to* G. B. Airy, *Esq., Lucasian Professor of the Mathematics in the University of Cambridge. By* J. Ivory, *Esq. M.A. F.R.S.*

Sir,

IN the letter I did myself the honour to address to you in the last Number of this Journal, I showed that the analysis employed by Laplace in the investigation of the figure of the planets is confined to rational and finite expressions of three rectangular coordinates, or to such functions as can be expanded, by converging series, into expressions of the kind mentioned. I proved this from the nature of the development, which appears to me to be the proper foundation of this analytical theory. But it has been usual to rest this doctrine on Laplace's equation in partial differentials, *Méc. Céleste*, liv. 3^me^, No. 10, or on M. Poisson's proposition, *Conn. des Tems*, 1829, p. 330 *, which are supposed to be demonstrated with I know not what degree of generality. In order to remove every difficulty, it therefore seems necessary to reconcile the two different views that have been taken of this subject, and to prove that the equation of Laplace, and the theorem of M. Poisson, are restricted by the same limitation which the nature of the development requires to be adopted. To accomplish this end is the purpose of the present letter, which I address to you as a person not indifferent to the progress of this branch of science, and, I may add, as the only one in England who seems to have bestowed upon it the least degree of attention.

The analytical theory, in the view we now take of it, is stated in the proposition of M. Poisson very clearly, and free from any particular application: I shall therefore confine my attention to it. I suppose that $y' = f(\theta', \psi')$ is a function of two variable arcs, and that $y = f(\theta, \psi)$ is what y' becomes when the particular values θ and ψ are assigned to θ' and ψ': further, put

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

$$f = \sqrt{1 - 2\alpha p + \alpha^2} = \sqrt{(1-\alpha)^2 + 2\alpha(1-p)},$$

$$ds = d\theta' \sin\theta' \, d\psi',$$

α being supposed less than unit, but tending to it as a limit; then, form the expression,

$$X = \frac{1}{4\pi} \int \frac{(1-\alpha^2)\, y'\, ds}{f^3},$$

the integral extending to the whole surface of the sphere, or from $\theta' = 0$, $\psi' = 0$, to $\theta' = \pi$, $\psi' = 2\pi$. Now, in the particular case

* First published, *Journal de l'Ecole Polyt.* 19^me^ cahier, p. 145.

when

when $\alpha = 1$, I say that $X = y$, whenever $y' - y$ is divisible by $\sqrt{1-p}$, or whenever the quotient of the same division has always a finite value; but, if this condition do not hold, then X is indeterminate.

It is to be observed that if we make y' equal to unit, or to a constant quantity in the foregoing formula, X will be equal to unit, or to the same constant quantity. This arises from the nature of the fluent, which can be integrated without difficulty by the ordinary rules. M. Poisson employs a particular mode of integration, which, as it leads to the same result, makes no difference. Write $y + (y' - y)$ for y'; then, according to what is just observed, we shall get,

$$X = y + \frac{1}{4\pi}\int \frac{(1-\alpha^2)(y'-y)\,ds}{f^3}. \qquad \text{(A)}$$

We have next to consider the term under the sign of integration, which I write in this manner,

$$\int \frac{y'-y}{\sqrt{2\alpha(1-p)}} \cdot \frac{\sqrt{2\alpha(1-p)}}{f} \cdot \frac{1-\alpha^2}{f} \cdot \frac{ds}{f}.$$

Beginning on the left, the first factor is always finite in the hypothesis laid down: the last factor too is never infinite; it is a constant differential multiplied by a finite quantity: the second factor never exceeds 1, and the third never exceeds 2. But although the third factor is exactly equal to 2, when $\alpha = 1$ and $p = 1$, yet, when $\alpha = 1$, it is evanescent for every value of p less than 1. It is evident, therefore, that the integral, as it receives no accumulation by the increase of the arcs θ' and ψ', is equal to zero. Wherefore we have $X = y$.

The demonstration now given rests entirely on the supposition that $y' - y$ is divisible by $\sqrt{1-p}$. For instance, if $y' - y$ were divisible by $(1-p)^e$, e being less than $\frac{1}{2}$, the first factor on the left would be infinitely great, and nothing could be affirmed concerning the value of the integral, or of X. It is therefore essential in this analytical theory that $y' - y$ be divisible by $\sqrt{1-p}$.

I must here remark, that the strictures I made in this Journal for May last, pp. 326, 327, 328, on M. Poisson's demonstration, are perfectly correct. These strictures are founded on the condition I there deduced by following his analysis; and that condition I have here made the principle of the foregoing demonstration. On examining what I had written, I found it was liable to be misunderstood; and, despairing to make

make out any clear demonstration, to which it should be impossible to oppose any objection, by a process of investigation depending upon very delicate considerations, I chose to found my reasoning, in the letter I addressed to you, on the nature of the development. In that letter I have expressed myself rather unguardedly with respect to M. Poisson's theorem: but it would serve no purpose to enter upon any explanation of these points; because the demonstration I have now given is equally clear and simple, and ascertains with precision the nature and extent of this analytical theory.

As it is now proved that the functions which come under the method of Laplace are not entirely arbitrary, we have next to inquire, how we are to distinguish them. And first we may suppose that y' is a function of three rectangular coordinates, that is, of $\cos\theta'$, $\sin\theta'\sin\psi'$, $\sin\theta'\cos\psi'$. On the surface of the sphere conceive a spherical triangle of which θ' and θ are two sides including the angle $\psi-\psi'$; then if γ be the third side, we shall have $p=\cos\gamma$. Further, let ϕ be the angle of the same triangle contained by the sides θ and γ: and, by the rules of spherical trigonometry, every one of the three quantities, $\cos\theta'$, $\sin\theta'\sin\psi'$, $\sin\theta'\cos\psi'$, may be expressed by a linear function of $\cos\gamma$, $\sin\gamma\sin\phi$, $\sin\gamma\cos\phi$; and consequently, y' may be converted, by substitution, into a rational and finite expression of the three latter quantities. But it is easily proved that every such function is reducible to this form, viz.

$$y' = \mathrm{F}(p) + \sqrt{1-p^2}\,.\,\mathrm{M},$$

F being the mark of a rational function and M an algebraic quantity having a finite value. Now y is what y' becomes when $\theta'=\theta$ and $\psi'=\psi$, that is when $p=1$; wherefore $y=\mathrm{F}(1)$. Consequently,

$$y'-y = \mathrm{F}(p) - \mathrm{F}(1) + \sqrt{1-p^2}\,.\,\mathrm{M}.$$

This expression is divisible by $\sqrt{1-p}$; and therefore the method of Laplace applies to rational and finite functions of three rectangular coordinates. But, strictly speaking, this is its whole extent: because we have no means of ascertaining that $y'-y$ is divisible by $\sqrt{1-p}$, or of proving that the quotient is a finite quantity, but the actual performing of the transformations I have shortly described. There is no doubt an extensive class of functions, not originally in the form of rational expressions of three rectangular coordinates, but which may be reduced to such expressions by converging series, as I showed in my former letter to you; and it will be admitted

that

that to this class the method likewise extends. But this happens, not because the analysis is more extensive in its principles than I have represented it to be, but because whatever is generally true of an indefinite number of approximations tending to a limit, must be true of that limit.

The purpose of this letter is now accomplished by what I have written, which fully unfolds the principle and the extent of the analytical method of Laplace; but, before concluding, it may not be improper to add a few words concerning the demonstration which the author of the *Méc. Céleste* has given of his fundamental equation.

The demonstration in the *Méc. Céleste*, liv. 3[me], No. 10, rests upon this formula,

$$\frac{1}{2} \cdot \frac{1}{f} + \alpha \frac{d \cdot \frac{1}{f}}{d\alpha} = 0 \qquad \text{(B)}$$

which, it is assumed, is *always* equal to zero when $\alpha = 1$. Now if we put $V = \int \frac{dm}{f}$, all the molecules being near the surface of the sphere, and observe that the differential equation is true of every individual molecule, we shall have,

$$\frac{1}{2} V + \alpha \frac{dV}{d\alpha} = 0.$$

This demonstration is independent of the nature of the molecule. It is true that Laplace makes the sphere touch the spheroid at the attracted point; but no stress is laid upon this construction in the process of finding the equation: and it seems only intended by it to show that the gravitation perpendicular to the two surfaces at the point of contact, may be reckoned equal to the gravitation tending to the centre of gravity of the spheroid.

It is certain at least that Lagrange understood the demonstration as I have explained it. For he supposes that all the molecules have their thickness equal to a small constant quantity, and he finds that the equation is not exact. He discusses this point at some length, with his usual clearness and precision*; he calls it, *une difficulté singulière;* and he expresses some admiration, as well he might, that a number of zeros should produce an aggregate not equal to nothing.

But there is a defect in the demonstration of Laplace, which is also the origin of the difficulty of Lagrange; namely, their supposing that the formula (B) is always exact. It is true for every value of p less than 1; but when $p = 1$, instead of being evanescent, it is infinitely great. If we substitute the value

* *Journal de l'Ecole Polyt.* 15[me] cahier, p. 57.

 of

of $\frac{1}{f}$ and perform the operations indicated, it will appear that this equation is exact, viz.

$$\frac{1}{2}\,V + \alpha\,\frac{d\,V}{d\,\alpha} = \int \frac{2\alpha\,(1-p)}{f^2} \cdot \frac{d\,m}{2\alpha\,(1-p)} \cdot \frac{1-\alpha^2}{f}.$$

In the particular case when $\alpha = 1$ and $p = 1$, the last factor but one on the right side is infinitely great on the supposition that $d\,m$ has a constant thickness; and, although the last factor is evanescent for all values of p less than 1, no conclusion can be drawn respecting the value of the integral without a particular examination of the case. This sufficiently accounts for the difficulty of Lagrange. It follows too that the integral will be evanescent and the equation exact, when the thickness of $d\,m$ is divisible by $1-p$, or by the square of the evanescent distance from the attracted point: for the factor, which was before infinite, will now have a finite value. In reality this is the limitation laid down in the demonstration, *Méc.Céleste*, liv. 11^{me}, No. 2, which was first published in 1816. It is remarkable that the author did not perceive, or at least does not notice, that the equation, which was first supposed to be unbounded in its application, is confined, by the new demonstration, within very narrow limits. But we shall attain the utmost generality of which the equation is capable, if we write $(y'-y)\,d\,s$ for $d\,m$, $y'-y$ being the thickness of the molecule: then,

$$\frac{1}{2}\,V + \alpha\,\frac{d\,V}{d\,\alpha} = \int \frac{y'-y}{\sqrt{2\alpha\,(1-p)}} \cdot \frac{\sqrt{2\alpha\,(1-p)}}{f} \cdot \frac{1-\alpha^2}{f} \cdot \frac{d\,s}{f};$$

and the equation is true when $y'-y$ is divisible by $\sqrt{1-p}$, and in no other case whatsoever.

It will not be necessary to notice particularly the investigation which Lagrange has given of the differential equation: because it depends upon a transformation, and an integration by parts, which cannot be executed unless y' be a rational function of three rectangular coordinates.

May we now venture to conclude, that this analytical method, although it has passed through the hands of Laplace, Lagrange and Poisson, has never been well understood; that it has been founded on inconclusive demonstrations; and, when its principles are clearly unfolded, that it falls entirely within the ordinary rules of investigation?

I have the honour to be, &c. &c.

July 9, 1827. James Ivory.

XVIII. *Letter*

XVIII. *Letter from* Mr. Ivory *to the Editors of the Philosophical Magazine and Annals of Philosophy.*

Gentlemen,

I SENT you a short paper a few days ago, and since that time I have looked into your last publication, which contains M. Poisson's remarks. I shall answer all his remarks when I find leisure and inclination; but, in order to lessen the work cut out for me, it may be proper to add here, as a supplement to my letter to Professor Airy, what I have further to say of Laplace's method of investigation.

In M. Poisson's demonstration the arbitrary function y' is, in reality, treated as a constant quantity. Assuming that the said function is always finite within the limits of the integral, he makes it constant while θ' and ψ' vary a little from their initial values, after which all the increments of the integral are regarded as zeros; so that, in fact, the demonstration is the same whether y' be constant or variable.

Now the proposition to be proved is not true unless $\frac{y'-y}{\sqrt{1-p}}$ be a finite quantity in the whole extent of the integral. But it may happen in some cases, that this quotient may be infinite even in the nascent state of the arcs θ' and ψ'; which would be contrary to the procedure of M. Poisson. It is the finite value of the quotient mentioned within the limits considered, and not any limited portion of the integral, that makes the demonstration conclusive.

What is said at p. 14 of this Journal for June, is rather beside the purpose, and seems contrary to the meaning of Laplace. I understand that the quotient alluded to, *Méc. Céleste*, livr. 11^{me}, No. 2, p. 26, is $\frac{y'-y}{1-p}$, and not $\frac{y'-y}{f^2}$, the value of α in f^2 being extremely near 1. This latter view of the case does not appear to answer the intention of the author; for the quotient would be evanescent when $y'-y=0$, independently of the nature of the functions. In my view, and especially if the quotient is finite in the whole extent of the integral, the demonstration of Laplace is exact, although very limited.

I have always maintained that the functions to which this analytical method can be applied, are determined by the method itself. I have ascertained the nature of these functions in two different ways in my two letters to Professor Airy. The fundamental proposition, or equation, is true only when

$\frac{y'-y}{\sqrt{1-p}}$ is finite in the whole extent of the integral, which limits the method to functions, or portions of functions, that can be expressed rationally in terms of three rectangular coordinates. M. Poisson confines his demonstration to functions that are finite within the limits of integration, and he uses the expression *fonctions entièrement arbitraires.* This is rather vague and indefinite; but if he will allow that he means no more than rational expressions of three rectangular coordinates, either in a finite form or in a converging series, every ground of dispute will be removed.

Nothing can be more unprecise or unsatisfactory than what we can gather in different authors respecting the extent of this analytical method. In the *Méc. Céleste*, livr. 3me, the utmost generality seems to be aimed at. In a Memoir published by the same author in 1816*, and afterwards in the 11th book of the *Méc. Céleste*, the demonstration of the fundamental equation is modified so as to fall far within the real limits of the method. Professor Airy has since republished this last demonstration of Laplace†, with some explanations; and, as he takes no notice of its limited application, we must infer that he conceives it to be general. The Professor seems to have been led into this peccadillo against geometrical rigour by his deference to the highest authorities. The process of Lagrange‡ is not liable to objection; but it is imperfect, inasmuch as he has not shown that all the operations which it necessarily supposes, whether they finally stop or run on indefinitely, may be performed without introducing any small divisors that would vitiate the reasoning. Had this been accomplished, the nature and extent of the method would have been fixed with precision.

I cannot quit this subject without remarking the strangeness of this dispute. Whatever general words are employed, or whatever undefined symbols are used, yet, in all the applications of the calculus, these words or symbols stand for nothing but rational expressions of three rectangular coordinates.

I remain, Gentlemen,

Your obedient servant,

July 16, 1827. JAMES IVORY.

* *Mémoires de l'Acad. des Sciences.*

† Transactions of the Cambridge Philosophical Society.

‡ 15me cahier *du Journal de l'Ecole Polyt.*

XIX. *Collections*

XIX. *Collections in Foreign Geology.*—[No. II.] *By* H. T. De la Beche, *Esq. F.R., L., and G.S. &c. &c.*

[Continued from p. 10.]

3. *Notice on the Geology of the Environs of Antwerp; by* M. de la Jonkaire*.

After some preliminary observations, in which the little attention that had been paid to the superior or tertiary rocks of Belgium is stated, the author proceeds;

"The deepest excavations made (in the environs of Antwerp) are those for the docks; the lowest bed entered is not well known to me, and I could not obtain exact information on this head. I however presume that it is composed of a calciferous clay.

"Above this is a gray clay, sometimes mixed with sand, commonly possessing considerable tenacity, and approaching the *plastic clay* in its mineral characters. It appears that fossil shells were not met with, or at least so rarely as to have escaped observation. Still rising, a second bed is observed; this is very thick, composed of a chloritic and quartzose sand, and filled with shells, among which we may remark the genera *Cyprina*, *Pectunculus*, *Turritella*, and especially many species of *Astarte*. It is also in this bed, and towards its lower part, that the bones of *Cetacea* are met with. From thence to the vegetable soil was a bed of sand without shells, about thirty feet thick, often containing siliceous pebbles. From this it appears the dock exposed four distinct thick beds. The same beds were found when digging a pond at Deure, a village situated a league from Antwerp; the sandy and shelly bed being raised, and within seven feet of the surface. Here also bones were found in the deepest part of the work; and as this was not carried beyond the clay, it appears probable these bones were, like those in the docks, in the lower part of the shelly sand.

"At Stuyvenberg, on another side of the town, fossil shells have been found, which, instead of being contained in a quartzose sand, were in a conglomerate of small pebbles, agglutinated by a calcareous cement, and sometimes replaced by calcareous nodules resembling certain non-decomposed parts of the lower beds of the *calcaire grossier*. It is near this latter place that we have seen those sections which expose the shelly sand, and that without shells, which so generally covers this country. The former bed is formed of a quartzose sandstone

* From the *Mémoires de la Société d'Histoire Naturelle de Paris*, vol. i.; this is interesting, as it contains an account of the prolongation of part of the tertiary beds of London and the E. of England upon the continent.

coloured

coloured by iron, containing a great quantity of those green grains so common in the lower part of the *calcaire grossier*, which are the silicate of iron of M. Berthier. I have only collected the following fossil shells; they are doubtless far from being the whole of those here found, yet their enumeration may be useful in determining the formation containing them.

Turritella triplicata.
——— *tornata.*
Natica.
Ostrea.
Pecten plebeius.
—— 4 undescribed species.
Pectunculus pulvinatus.
——— *nummiformis.*
Astarte obliquata.
Astarte. 4 or 5 undetermined species.
Isocardia Cor.
Cardium.
Lucina circinnata.
Venus. 2 undescribed species.
Cyprina islandicoides.
——— another species.
Nummulites.

" Fragments of silicified fossil wood, which appear to belong to genera approaching the Palms.

" Having enumerated the beds seen at Antwerp, we shall now endeavour to refer each to its equivalent of the environs of Paris.

" *The Lower Clay.*—The lowest clay bed, that which we first mentioned, appears to contain shells; but notwithstanding all our efforts we were unable to procure any, consequently we are unable positively to determine its equivalent. Nevertheless, if what we advance respecting the following beds shall be confirmed, it can only belong to the plastic clay, which in many places, even in the Paris basin, contains beds that would appear to be charged with shells.

" *The Second Clay.*—The following bed, though still not well characterized, is however more so by its mineral structure, its predominant colour, so often shown in the plastic clay, and by its lignite, which seems to occur at a short distance, and in a similar position.

" In fact, strata which appear to be the continuation of those we are describing, and which represent them in other points of the Pays-Bas, contain beds of bituminous clay, and even true lignites. Of these are the lands of Huisduinen, and probably the pretended peat containing the cocoa and areca nuts figured by Burtin. It should be remarked on this head that we should mistrust this term *peat*, as applied so generally in Holland to all those places from whence a combustible substance is extracted; for it certainly is not in peat properly so called, that we should meet with a mixture of fresh-water and marine shells, or with amber. Now these are found in the pretended peat, and particularly the amber, which has become

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in consequence of recent discoveries, one of the characteristic minerals of the plastic clay, and which has been collected in Holland at a very remote period, and since at different times in the bituminous beds, to which the authors who mention these discoveries assign in general the name of peat. These beds form, without doubt, the continuation of the bituminous beds of Low Germany, where M. Coquebert de Montbret and others have also observed amber, and are consequently lignites.

"If therefore true plastic clay exists in the same basin, in places in the vicinity, and in a similar position; and if beds possessing the mineral character of this rock are found at Antwerp,—it appears to us, that the latter can only be referred to the clay formation (plastic clay) beneath the *calcaire grossier* of Paris.

"*Shelly Sands.*—These resting upon the above would therefore, according to our opinion, represent the *calcaire grossier*. This we shall endeavour to establish.

"We should remind the reader in the first place, how deceptive is the apparent nature of a rock, when employed as a character in determining a formation; and that notwithstanding these sands may not afford true *calcaire grossier* to the mineralogist, they may so to the geologist. A greater difficulty exists in the perfect resemblance of its fossils to those of Italian tertiary rocks, at present regarded by many authors as above the Paris gypsum. At Antwerp, as in Italy, bones of *Cetacea* are found; the shells are of the same species, and there exists no well marked difference between the two places: yet we cannot, notwithstanding this analogy, consider the rocks of Antwerp as referable to the second marine formation of Paris. The sharks' teeth, so frequently found at Antwerp, are common in *calcaire grossier;* whether those of the genus *Scillium*, or those of the genus *Carcharias.* The Nummulites and Turbinolites found at Ghent in the same sands, resemble those of the *calcaire grossier.* Burtin has given, as coming from the same beds as at Antwerp, *Voluta Harpula*, *Voluta Cythyra*, *Hippocrenes*, and perhaps *Cerithium Gigas*, a fossil so essentially characteristic of the lower marine formation. Lastly, and this appears to us the most important fact, nearly a fifth part of the bed we are describing is composed of silicate of iron. This, we say, is very important; in fact the lowest beds of the Paris *calcaire grossier* are so highly charged with it as to pulverize entirely; while a mechanical analysis which we have made of the second marine formation of different places, has never afforded us a single grain of this substance."

The author then presents us with some reasons for supposing that this equivalent of the *calcaire grossier* has taken some of the characters of the plastic clay, and proceeds;

"That the *calcaire grossier* has here partly preserved the character of plastic clay is proved by the *Cyprinæ*, which are common, and which are found in the latter in the Isle of Wight; the *Astarte*, a characteristic fossil at Antwerp, is frequently found in clays, referred for the most part to the plastic clay. Many of the shells of the *calcaire grossier* of Antwerp belong equally to the plastic clay, or rather to the point of contact or mixture of the two formations.

"Beds, therefore, exist in Belgium analogous to the lower part of the first marine formation (*calcaire grossier*) of the Paris basin, and these beds are in general characterized by the presence of silicate of iron, by sharks' teeth, *Nummulites*, many species of *Astarte*, *Pectina*, *Cyprinæ*, *Pectunculi*, &c."

The author then describes the *upper sands without shells*, which he refers to diluvium, and notices the finding of elephants' bones in it at Antwerp, Vilvorde, &c.; and after mentioning the alluvial deposits, he adds:

"A tertiary basin exists in Belgium, bounded by chalk hills, which, quitting the coast of France, form a zone, as M. Prévost has noticed, that passes near Mons, Liége, Maestricht, &c. This basin appears to resemble that of the E. of England*, and probably contains many beds equivalent to those of the environs of Paris. At Antwerp we have examined beds which appear referable to the plastic clay, the lower part of the *calcaire grossier*, diluvium, and alluvium."

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4. *Geological Observations on Sieland and the Neighbouring Islands (Denmark); by* Dr. G. Forchammer†.

Two cretaceous formations are observable in these islands: one the same with the chalk of England and France; the other apparently analogous to the plastic clay or *calcaire grossier*. The limestone of Saltholm and Limhamn, which resembles chalk in its mineral structure, has a N.N.W. or S.S.W. direction, in which the following beds agree. The fragments of limestone found round Fursöen, and which abound in the con-

* The term *basin* is objectionable either as applied to the superior or tertiary deposits of the East of England or Belgium, as both only form the western continuation of the great mass of tertiary rocks which traverses the centre of Europe, and bend down towards the Black Sea.—*Trans.*

† From a notice in Baron de Férussac's *Bulletin des Sciences* for March 1826, of the author's remarks in Oersted, Oversigt over det K. Danske Videnskab. selsk. Forhandl., 1825; and shows the character under which the chalk, plastic clay, and *calcaire grossier* exist in Denmark.—*Trans.*

continuation of the direction of Saltholm, belong to this limestone; they appear to have been derived from some compact bed situated at a greater depth.

The following rock occurs at the Klint, or cliff of Stevn. It is a true chalk, and constitutes the last bed in the cliff; it contains flint (*silex*) in a nearly horizontal position: there is however a slight inclination to the W.S.W. The most perfect regularity reigns throughout this formation, the beds of which are completely parallel to each other. To the chalk succeeds a bed of clay three or four inches thick, and then a limestone bed one foot thick. The clay bed contains carbonaceous matter in its lower part, and sharks' teeth, *Pectens*, and imperfect impressions of plants are found in it. The limestone is hard, and full of green particles; it passes insensibly into a cretaceous rock and coral-limestone. Its fossils differ from those of the chalk, and consist of the genera *Cerithium*, *Trochus*, *Natica*, *Cypræa*, *Arca*, *Mytilus*; and in zoophytes, *Favosites* and *Turbinolites*. To this limestone, which the author names *Cerithium limestone*, succeeds another consisting of corals united by a marly mass; it is the *corallite limestone* of the author. It contains siliceous nodules, which sometimes take an ellipsoid form; the siliceous nodules resemble hornstone, and the beds are compact. The fossils found in this limestone are the same with those of the true chalk. It is covered by an irregular mass of angular fragments of the same limestone and flint (*silex*) cemented by chalk.

The limestone in the quarries of Faxoe is very pure, and is formed of slaty and compact limestone, with porous coral beds. Varieties similar to the corallite limestone and marly chalk are here also found. Generally speaking, the Faxoe limestone is analogous to the cerithium limestone of Stevns-Klint; part of the same fossils are found in it, but the Faxoe limestone also contains other species, some of which have only been found in rocks above the chalk: for example, *Cypræa*, *Fusus*, and *Solarium*.

The cliffs of the isle of Möen are composed, according to the author, of a marly cretaceous rock with beds of flint (*silex*), a conglomerate of chalk and white sand cemented by a cretaceous substance, a gray clay, yellow white and red sand, yellow clay mixed with sand, and of conglomerates of primitive rocks. The clay and sand contain rolled pieces of primitive rocks; a circumstance of frequent occurrence in Denmark. In the gray clay they are small, and rarely more than two or three inches in diameter; in the yellow clay and sand there are some larger. The cretaceous rock in many places contains nodules of strontian, of many pounds weight, with corals and bivalves.

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The author draws the following conclusions from his observations.—The cretaceous rock of Stevn is true chalk, and the clay and cerithium limestone, plastic clay and *calcaire grossier*. He regards the limestone of Faxoe as a local development of cerithium limestone; the beds of clay in the limestone of Stevn and Faxoe, and in the cretaceous rock of Möen, as subordinate strata, developed in the great formation of rolled stones, and formed at one and the same period,—the commencement of the tertiary epoch.

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5. *On the Coal Deposits of the Prussian States**.

The coal measures of Upper Silesia form masses on greywacke; the Tarnowitz limestone, in many places covering the coal measures, is probably muschelkalk. There are four series of coal-measure strata in Silesia, which have not been yet found to agree with each other: the first and richest is the eastern part; it extends from Sabrze, between Gleiwitz and Beuthen, N.W. as far as Kostow, near Bremse. These beds, having various dips, occupy a basin, and have a direction from N.W. to S.E.; they are exposed for three miles and a half in length and one in breadth†. A second series passes to the W. of the preceding, from Nicolai on the W. to a mile E. of Rybnik; it only occupies an extent of half a mile square, and is situated in a basin. The coal measures reappear near Hultschin, on the left bank of the Oder; they are highly inclined, with a direction from N. to S. There is a coal basin in Lower Silesia to the S.E. of the Riesengebirge, and to the W. of the Eulengebirge. Its limits are half a mile to the N.E. of Landshut, Altwasser, Charlottenbrunn, Rudolphswalde, the S.E. foot of the Eulengebirge, Volpersdorf, Liebau, Shatzlar and Straussenei to the N. of Lewin. A line drawn from Landshut to Wunschelburg would about divide the basin into two; the N.E. portion alone belongs to Prussia. Sandstones and porphyries cover the upper beds. The coal district of Waldenburg rests on submedial or transition rocks as far as Altwasser; more S. it is supported by the gneiss and mica slate of the Eulengebirge. The dip of the beds varies from 20° to 70°. Porphyry disturbs the direction of the upper beds, as between Landshut and Waldenburg. The coal measures here appear to envelop the porphyry. The coal is altered in the vicinity of the latter rock; it has taken the character of an anthracite,

* From a notice in Baron de Férussac's *Bulletin des Sciences* for September 1826, of the account contained in Karsten's *Archiv für Bergbau*. part I. vol. xii.

† It is presumed that Prussian miles are here understood, one of which is equal to about four and two-fifths English.—*Trans.*

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or species of coke, in the lowest bed of the gallery of Friedrich Wilhelm Erbstollen, in consequence of a porphyritic mass which rises from the interior of the earth, and covers it in an irregular manner*. The author mentions two similar examples in the fifth coal bed of Lauragrube, and in the second of the Gnade-Gottesgrube mine.

The coal deposit of the circle of Saale is situated to the N. and N. W. of Halle, fills cavities, presents saddle-shaped strata, and has many dips and directions. The secondary red sandstone covers the coal measures, porphyries rise through it, and raise the coal beds to the surface. The coal measures of Saarbruck are situated at the bottom of a bay formed by the Hunsdruck and the Vosges. The beds run from S.W. to N.E. and incline to the N.W. They are rather irregular, especially in the N.E. part, and present many contortions. The dip of the Saarbruck beds is in general at about 20°. The coal deposit on the Worm, N. and N. E. of Aix-la-Chapelle, forms a separate basin in the submedial or transition rocks, and contains thirty-four known beds of coal. The coal basin of Eschweiler resembles the preceding, contains forty-six beds of coal, is 5500 yards long, and 2250 yards wide. The thickest coal bed is from four to six feet in depth. The coal deposits of La Mark occupy three basins, run from S.W. to N.E. Many mines traverse the cretaceous chlorite marl, which covers the coal at Unna, &c. These rocks rest on submedial or transition rocks, and are separated from them by a thick mass of coal sandstone, without workable coal †. The coal deposits of Tecklenburg-Lingen are situated near Ibbenbühren; there are three beds of coal; the first and second run E. and W., and dip N. at an angle of 12° or 15°; and the third runs from E.N.E. to S.E., and dips N.W. at 6° or 10°. The first is three feet thick, the second from five feet to five feet seven inches, and the third a foot and a half. Its geological relations are unknown ‡.

Lastly; The coal deposit of the Shaumburg, near Minden, lies parallel to the Teutoburgerwald, in the chain of the Wichengebirge, which extends to the S. and W. of Minden, on

* These additions to similar facts already known are important, as they tend to confirm, if that were necessary, the now almost generally received opinion of the igneous origin of trap rocks.—*Trans.*

† May not this be the equivalent of our millstone grit?—*Trans.*

‡ The coal deposits, enumerated as far as this in the text, would appear to be referable to the coal measures; that of Tecklenburg-Lingen (which follows), stated to occur in the lias marls, is important, when coupled with Mr. Murchison's very interesting communication to the Geological Society, respecting

on the N. of the Teutoburgerwald. The Wichengebirge is composed of *keuper*, lias, and the marl and sandstone of the lias, which rest on muschelkalk. In the lower part of the lias marls there are many coal beds, running E. and W., and dipping N. The principal bed is two feet thick. Many coal beds are found above the principal bed at Trotheim and Tappenstadt.

6. *Observations on Fossil Shells; by* M. B. de Basterot*.

The geographical distribution of organized beings over the surface of the globe, has long since furnished a subject for the meditation of the naturalist. The limits of regions occupied by different families of quadrupeds, oviparous animals and plants, have been pointed out in a series of ingenious observations. The animals of a less elevated order have not been observed, in this point of view, with the same attention. The molluscous animals, so very abundant, so interesting by the variety, the beauty, and the number of the fossil remains which they have left on our continents, have been entirely neglected in this respect. Let us in imagination transport a naturalist on an unknown shore, covered with the remains of arborescent ferns, palms, gigantic *Gramineæ*, the bones of elephants, tigers, hyænas, &c.; would he not immediately recognize the productions of the torrid zone? But let this coast be alone covered by the productions of the sea, by molluscous and zoophytic animals; would they reveal to him the climate in which he was placed? I should think not; for though the frequency of univalves may have been remarked between the tropics, as among fossils, we are far from possessing general and certain knowledge on this subject.

I would wish to fill up this blank, and endeavour to sketch the geographical distribution of the *Mollusca*, but the time does not appear to be yet arrived. "Let us not forget," says M. Decandolle, speaking of botanical geography, "that this science can only commence when the study of species has become

respecting the Brora coal-field (an abstract of which appears in the Phil. Mag. for March), and which he considers equivalent to the coal of the Eastern Moorlands of Yorkshire. It may not be improbable that the coal of Tecklenburg-Lingen is the equivalent of the Brora and Yorkshire coal; for the oolitic series is not in general well understood in Germany, and much confusion seems to exist respecting the equivalents of muschelkalk, quadersandstein, keuper, &c. Be this as it may, the fact contained in the text is important; as it presents us with another instance of a useful carbonaceous deposit being found in the oolite formation.—*Trans.*

* Extracted from the author's *Mémoire Géologique sur les Environs de Bordeaux*, inserted in the *Mém. de la Soc. d'Histoire Nat. de Paris*, vol. ii.

come sufficiently advanced to furnish us with numerous and well-authenticated facts." Now this knowledge of species is almost entirely wanting as respects the Mollusca. It is but a short time since, that the search for shells has become, from a simple amusement, the study of scientific men: it was only after the period when it was perceived that geology and ancient zoology were destined to be enlightened by their fossil remains, that this research passed from the hands of amateurs into those of naturalists. From this period, the number of fossil species observed by naturalists and inscribed in their catalogues, has increased with astonishing rapidity.

The thirteenth edition of the *Systema Naturæ* cites only fifty-three species as found in a fossil state, ten of which were analogous to those at present existing. At the moment I write (1824) I possess a MS. list of the fossil species described by authors, the number of which amounts to more than 2500, —the distribution of which will be seen in the following table.

		Number of genera the species of which are found fossil.	Total No. of fossil species.	No. of species resembling existing species.
SHELLS.	Chambered	29	297	6
	Univalves	81	1141	151
	Multivalves and bivalves	111	1091	107
		221	2529	264

I am unable to present equally exact details on the augmentation of living species known to naturalists, but it no doubt forms a much less considerable proportion. The same edition of the *Systema Naturæ* does not quite contain 2400 species, —a number smaller than that at present presented by the fossil species, collected almost entirely in France, England, and Italy. This should lead us to hope, that when the attention of travellers is awakened to this subject, they will bring us from distant countries an abundance of species as yet unknown in our collections.

What has been above stated will be sufficient to prove, that our knowledge of the species of the molluscous class is not yet sufficiently advanced to allow us to hazard generalities on their geographical distribution. I may, I hope, nevertheless be pardoned in adding some observations which may add new interest to this kind of research.

In fact, the geographical distribution of living families, genera, and species, compared with the actual distribution of fossil species, may throw light on many of the most important geological

geological phænomena. Curious resemblances will, I have no doubt, be found to exist between these two distributions; and though fossil shells do not lead (as has been observed) to equally exact results in solving the different problems relating to the ancient zoology of our planet, with the remains of large quadrupeds, they may, it appears to me, afford a firm support for those deductions, which arise out of the examination of the other classes. Shells are, moreover, indefinitely more abundant than bones in the beds of the earth; they are often the only remaining documents to enlighten us respecting those changes which have taken place in the existence of living creatures since the formation of the globe. Shells have, in fine, the advantage of being in a great measure found in those situations which have given them birth, and thus remain the incontestible proofs of the presence and retreat of the sea. A striking example of this will be seen in the fresh-water limestone, noticed in the course of this memoir. These limestones, formed in the first instance in waters filled with *Limneæ* and *Planorbes*, have been abandoned by them; the sea has afterwards covered them, and the inhabitants of that sea, *Pholades*, *Saxicavæ*, and other similar shells, have fixed their dwellings in it: a third revolution has arisen, the sea has retired, and left them dry in that place where we now study them.

The knowledge of fossil shells, as it at present exists, serves almost exclusively to enable us to recognize different portions of the same formation in different places. These remains are nearly infallible guides in the examination of a limited district, notwithstanding the mineralogical differences often presented in the same formation in different parts of it. Let us take, for example, a system of beds, formed as are those in the vicinity of Paris, of alternating beds of limestone, sand, gypsum, and clay. When one of these beds is alone found, it is almost impossible to assign it its true place without having recourse to its zoological characters. How, for instance, can we without these characters distinguish the sands of the plastic clay, from those of the *calcaire grossier*, and those above the gypsum?

When, however, the observer compares distant countries and geological basins separated by mountain chains or seas, he does not find the same facility. If, at first sight, he considers he has met with species already known, he may also find many others new to him. If he afterwards regards the species, which he at first considered identical, he will remark, after careful examination, a crowd of differences between their forms and those of the species which he had previously collected in in a neighbouring basin. He will ask himself whether these species are distinct or are mere varieties, and he will find very few means of answering this question.

The difficulty here offered is of a very special nature, and deserves particular attention. This difficulty consists in the variation in form observable among shells, both living and fossil, when we examine the same species obtained from different situations.

We never, it may be affirmed, observe perfectly identical species, either on the surface of the earth (at least in the more recent beds) or on the coasts of our present seas, in situations separated by considerable horizontal distances, or even simply distinct from a difference in the nature of the rock or waters; a little greater or a little less elongation throughout all its parts, prominence in the striæ and the tubercles, size in the folds, &c. are always met with, and render the determination of species a very difficult task.

In well characterized species, the habit of exact observation is necessary to seize these differences; but among a number of other species these differences are so remarkable, that different names have often been given to the same shell, from characters which were in fact merely accidental*.

There must however be limits to these local variations; and I am far from being of the opinion of those, who consider that all species may merge into each other: at the same time I am much inclined to believe that the limits between the species and the variety, so difficult to assign in the higher classes of animals, is still less easily fixed among shells. This, in fact, should occur, for "the differences constituting the variety depend on determined circumstances, and their extent augments with the intensity of these circumstances†." Now shells, more than any other class, are the slaves of circumstances; they cannot withdraw themselves from the action of exterior influences. If a change in food and habitation can alter, as

* This may be easily observed in many genera: the following is an instance taken from the *Pleurotomæ.* All the assumed following species are probably only modifications of a single real species, produced by the circumstances under which the animals inhabiting these shells were placed.

Pleurotoma oblonga......	Brocchi, pl. viii. fig. 5..	Lives in the Mediterranean.
———— *ejusd. var.* ...	Id. .... pl. ix. fig. 19..	Fossil in the Duchy of Reggio.
———— *acuminata*...	Sowerby, pl. 387, fig. 2.	Foss. Muddyford.. England.
———— *brevirostris*	Id. .... pl. 146, fig. 4.	Foss. Highgate.
———— *multinoda* ...	Lamarck..........	Foss. Grignon.
———— *ejusd. var.*...	Nob.	Foss. Environs of Bordeaux.
———— *Monneti*....	Defrance .. M.S. ...	Foss. Fleury la Rivière, near Epernay.

† Cuvier, *Rech. sur les Oss. Foss., disc. prélim.*, 2d edit. p. 58.

they do, the forms of our domestic animals and cultivated plants; would not the differences of bottom, depth, temperature and agitation of the waters in which they live, vary the inhabitants of the sea?

It may not, perhaps, be useless to state an opinion here, which an examination of many shells contained in the more ancient beds has enabled me to entertain: I have observed an uniformity in their exterior characters not found among those from the more recent rocks, an uniformity which becomes the more striking, as the total number of species diminishes; in other words, *that identical species are the more generally spread in direct proportion to the antiquity of the rock containing them**.

In fact, the transition limestone of England, France, and North America, presents us with the same species of *Trilobite.* Impressions, very remarkable for their forms and association, are found with the same characters in the transition slates of Wales, Northumberland, Finisterre, the Cotentin, the Ardennes, the Hunsdruck, the Hartz, the county of la Mark (Colombia), New York (Pennsylvania), and the environs of lake Oneida, in North America. I should not, however, omit noticing that an attentive examination of fossils from the carboniferous or mountain limestone, in places distant from each other, has not presented me so perfect a resemblance; yet identical species are still found in Ireland, England, the Pays-Bas, France, and Norway. The species should, however, be more varied in this than in a more ancient rock, even according to the rule I have proposed.

The catalogue which I have formed of the fossil shells of the environs of Bordeaux, is not, doubtless, perfectly complete; it is however sufficiently so, to give rise to some remarkable approximations in ancient zoology.

It is there seen, that, notwithstanding the loose nature of the beds in these environs, notwithstanding the resemblance they still bear to a coast recently abandoned by the waters,—the Bordelais soil owes its origin to an order of things anterior to the last revolution of our planet. The action of our present seas accumulates sand-hills on one side of this basin, which advance slowly into the interior. These are, however, much circumscribed, and possess no analogy with the great sandy deposit of the Landes. In fact, the catalogue here presented, shows, that out of three hundred and thirty species of shells, found fossil in the latter sands, forty-five only are analogous

* This is not exactly a new opinion: every additional confirmation of it is, however, valuable.—*Trans.*

to those existing in the neighbouring seas, even comprising the Mediterranean.

It is in the more recent fossil deposits, but distinct from the products of our present seas, that we find a much greater analogy as to species. It will be seen that even in these more ancient times horizontal distances exerted an appreciable influence. Taking the Gironde basin as a centre, it will be observed that the shells of similar basins the more resemble each other the shorter the distance of these basins. Thus out of three hundred and thirty species collected in the environs of Bordeaux, ninety are found in similar rocks of Italy; sixty-six in those of the environs of Paris; eighteen in those of Vienna, and only twenty-four in the tertiary basins of England.

Table of the distribution of the fossil shells of the environs of Bordeaux.

Fossil shells of the environs of Bordeaux 330.	Anal. living species	In the Adriatic, the Mediterranean, the Ocean, and the English Channel . . .	45
		In other seas	21
	Anal. fossil species	In the basin of Italy	91
		In the Paris basin	66
		In England	24
		In the environs of Vienna .	18
	Species peculiar to the Bordeaux basin		110

It should be observed, that some species have at the same time analogous living and fossil species, which explains the circumstance of the total number of species, classed in this table, being greater than the real number mentioned as found at Bordeaux.

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7. *On the Lignite or Plastic Clay Formation of Mount Meissner; by* M. Alex. Brongniart*.

The fundamental rock, on which the lignite formation rests, is a compact smoke-gray limestone, containing *Ammonites nodosus* Schlot., and which I consider as of the same formation as the Alpine limestone†, consequently a much older rock than chalk.

This fossil-combustible deposit is very thick, and is composed: 1. of the combustible itself, presenting many minera-

* This account of the carbonaceous deposit of this long celebrated Hessian hill is extracted from that part of the description *Géologique des Environs de Paris*, by MM. Cuvier and Brongniart, which contains an account, by the latter, of the plastic clays known beyond the limits of the Paris basin.—*Trans.*

† The equivalent of our magnesian limestone.—*Trans.*

 logical

logical varieties, in some of which it becomes so far removed from bituminous wood and approaches coal, that it was long considered as of the same species, and bore that name. It is an important example of the too great influence mineralogical characters have been permitted to have on geology. In fact, confining ourselves to the principal varieties, we find true anthracite in this carbonaceous mass, that is to say, a dense carbon without bitumen, sometimes with a dull, sometimes with a shining fracture. We here find a thick bed of compact, solid, fossil bituminous carbon, having a nearly straight fracture, burning with facility, and presenting many of the characters of true coal. None of the exterior characters of lignite are observable in these varieties. But their manner of burning, the odour then emitted,—and what is better than this, the considerable mass of ligneous stems, some perfectly recognisable and scarcely changed, others so much altered that the ligneous structure has almost entirely disappeared, are facts which leave no doubt on the origin of this mass of fossil combustibles. The absence of every vegetable of the Fern family, and of all others belonging to the ancient coal formation, are negative characters, which, added to the former, contribute to distinguish this formation from the coal measures.

Plastic clay, *i. e.* non-effervescent and infusible, is found beneath the lignite; that occurring between the beds of lignite is sandy and less pure, and is sometimes accompanied by beds of sandstone; so that the relations of the two clays to each other is the same here as on the *plateau* of Arcueil and Vanvres near Paris,—a circumstance which completes the characters of this formation. It is in this clay that the spathose nacreous limestone named *Schaumerde* occurs; and it is from the same clay, worked at the foot of the hill, near the village of Grossalmerode, that the celebrated Hessian crucibles are made.

This formation is covered at Mount Meissner by a mass of basalt, celebrated on account of the discussions that have taken place respecting its origin.

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8. *Observations on the Porphyry of Tœplitz, and the Clinkstone of the Schlossberg; by* Dr. C. Naumann*.

The Mittelgebirge appears, says the author, to have been the central situation of those great eruptions which have principally acted along the southern side of the Erzgebirge, and

* From a notice in Baron de Férussac's *Bull. des Sciences,* for June, 1826, of the memoir contained in Leonhard's *Zeitschrift für Mineralogie,* Oct. 1825.

which

which have left, in the innumerable basaltic summits of Bohemia, Saxony, and Lusatia, the proofs of their activity. The Mittelgebirge is entirely formed of basalt and clinkstone; no other kind of igneous rock is met with. A red felspathic porphyry occurs at Tœplitz, at the foot of these mountains, which appears to form the extremity of a continuous range extending from Altenberg to Tœplitz. From this porphyry issue the celebrated springs of Tœplitz: they carry to the surface not only fragments of porphyry, but also of quartz, basalt, granite, and gneiss. A marly limestone formation, analogous to the *plänerkalk* of Saxony, and horizontally stratified, occurs round the porphyry on which its beds rest, in such a manner as in some places to show, that, from being at first horizontal, they have become inclined, in consequence of the porphyritic masses having been forced up: in other situations, this elevation of the inferior rock has broken some, without deranging the position of those which remained. In the vicinity of the points of contact, the porphyry is traversed by numerous hornstone veins, containing fragments of porphyry, whilst fragments of hornstone are seen in the porphyry next their sides. These veins penetrate into the limestone from the porphyry, to the depth of from six to eight feet, sometimes changing their nature, so as to become either true flint or a more or less siliceous marl; but in either case, they always contain the fossils of the limestone. A singular mixture of porphyry, hornstone, and siliciferous limestone sometimes forms a kind of crust, with a nearly scorified appearance. Thin seams of heavy spar often occur in the porphyry near the junction of the two rocks. It appears evident to the author, that these two rocks have been at the same time in a soft or fluid state, the one being igneous, the other aqueous; and that contact under these circumstances could have alone produced the singular facts he mentions*.

The mountain of the Schlossberg near Tœplitz, is basaltic at its lower part; but the cone on its summit, the sides of which are more steep, and which rises high above all that surrounds it, is entirely formed of clinkstone, in thin stratified beds, presenting on all sides a dip, corresponding with the sides of the mountain, whilst they are horizontal at the top of the cone. This kind of *bell* structure appears to the author to show the manner in which the mountain was formed.

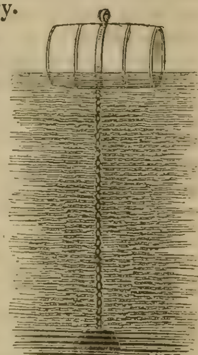
H. T. D. B.

* Here M. de Bonnard, who, by the signature, noticed this paper in the *Bulletin*, seems to have added, "Many of these circumstances are altogether analogous to those presented by the contact of the granite and lower parts of the oolite formation in Burgundy."—*Trans.*

XX. *Plan*

XX. *Plan for Mooring Ships in Roadsteads. By Lieut.-Colonel* Miller, *F.R.S.**

IN a country whose attention has been so steadily directed to her naval prosperity as that of England has been, it may perhaps appear surprising, that some attempt has not been made, or rather that any means should have been left untried, for effectually securing ships in roadsteads; as every heavy gale of wind is sure to be followed by an account of vessels driven from their anchors, on some part of the coast or other, and too frequently accompanied with a melancholy catalogue of loss of life and property. The only grand national works that have been undertaken to guard against so great a calamity, are the breakwaters at Plymouth and Kingstown; but however creditable these works may be to the nation, and however well they may answer the purpose intended, their great expense must be a serious obstacle to their ever being generally adopted throughout the country.



The following plan is therefore submitted with due deference to the public; and if it should be the means of saving a single vessel from shipwreck, the object of the writer will be attained. It is very simple, and will easily be understood from the figure. It consists merely in securing a large buoy, by means of a block of cast-iron, so that it cannot be moved by stress of weather, to which a vessel can make fast, instead of letting go her anchor.

Construction.

Length of buoy	16 feet.
Diameter of ditto at the middle	9
Ditto at the ends	$7\frac{1}{2}$
Length of chain	36
Diameter of cast-iron block at top . . .	3
Ditto at bottom	$5\frac{1}{2}$
Height of ditto	$2\frac{1}{2}$
Weight of ditto	7 tons.

The buoy must be made abundantly strong, bound with iron and coppered, as it will be subject to a heavy strain, and may frequently be drawn under water. A strong iron hoop also passes round its centre, to which the chain and ring are

* Communicated by the Author.

attached,

attached, and the chain must be of sufficient length to allow the buoy to rise to the surface at high water.

In most anchorages the weight of the cast-iron block will sink it sufficiently deep to prevent its being moved; but in stiff ground, where that may not take place, piles must be driven round it, by means of the diving-bell, so as effectually to secure it.

It is conceived that a buoy of these dimensions would be sufficient to hold a ship of 500 tons under any circumstances; but for a very large vessel, the size of the cast-iron block and also that of the buoy would require to be considerably increased, as the larger the buoy, with the greater ease would the vessel ride.

The principal cause of a vessel dragging her anchor, or parting her cable in a gale of wind, is the jerk that is produced by a heavy sea striking her when the cable is on the stretch. By the proposed plan this would be guarded against; as the tendency of the buoy to rise perpendicularly, while the vessel pulled horizontally, would cause a spring on the cable so as to prevent any sudden jerk. A vessel moored in this manner would probably not require to veer out more than 20 or 30 fathoms of cable: and the manner of bringing up would be, to make fast a hawser to the buoy: heave upon it until the latter came under the bows of the vessel, then pass the (chain) cable through the ring of the buoy, and bring the end on board. The cable would thus be double, and a vessel could get under weigh in an instant by letting go one end of it.

Buoys of this description might be laid down (in the Downs for instance) in lines at different distances from the shore; and a vessel, instead of looking out for good holding ground, might then bring up as near to the land as her draught of water would permit her, and thus facilitate her communication with it. They might also be laid down in rocky ground, where ships cannot anchor at all, by attaching them to bolts fixed in the rocks, by means of the diving-bell.

XXI. *On Writing-ink, and on the Effects which are produced upon it by Paper and Parchment. By Mr.* John Reid.*

IT has often been remarked that old writings retain their colour better than those of a later date; and it has been supposed in consequence, that formerly, ink of a superior quality to that now in use had been employed. But though much depends

* Communicated by the Author.

upon

upon the ink, equally as much depends upon the material upon which it is written; for as ink readily suffers decomposition by the application of chemical agents, and as paper and parchment contain ingredients which affect its composition, it is necessary to attend to the circumstances connected with the latter subject, as well as those which have reference to the chemical constitution of the ink itself.

With regard to the composition of ink, it appears that formerly galls, gum, and sulphate of iron had been used in manufacturing it. A decoction of the galls was generally made, to which the gum and sulphate of iron were afterwards added. After some time a sediment falling down, the fluid was poured off, which gradually absorbed oxygen from the air; when it was fit for use.

In this manner, when the proper proportions are observed, may writing-ink be made—of which it may be affirmed, that if it does not keep its colour so well as we see that ink on old writings does, it is owing entirely to some circumstance connected with the material upon which it is written, the nature of which I shall afterwards endeavour to describe; and as this process forms the basis of those still employed, I shall avail myself of it to point out what appears to me erroneous in the opinions commonly entertained on the subject, and how it may be improved so as to increase the quantity of ink which may be obtained from a given portion of the galls.

If to a solution of galls, sulphate of iron with or without gum is added, and the compound is excluded from the air, it is dissolved; but no change of colour results, and no precipitate is deposited. Hence it is to be inferred, that oxygen is necessary in order that that change may take place from which results the peculiar compound called ink. But when the air is admitted, the precipitate begins to be formed in the course of a minute, and the colour gradually deepens. What is the nature of the compound which remains? Chemists have described it as consisting of minute particles of colouring matter formed of tannin and gallic acid, combined with sulphate of iron suspended in the fluid by means of the gum. But in opposition to this opinion, it may be remarked, that no colouring matter is deposited from it when gum is not used. It may be filtered again and again, without the loss of any part of it. It is therefore a solution. But though the gum does not serve the purpose which has been supposed, it is notwithstanding a useful ingredient. Ink made without gum, sugar, or some other ingredient of a similar nature, is pale; but any of them, when present, combining with it, gives to it intensity

intensity of colour. This fact enables us to explain the circumstance, that ink which does not contain gum becomes of as intense a colour as that in which it is present, after being written with upon paper,—the paper serving the same purpose of affording a substance with which the ink may combine and develop the colouring properties of it.

Ink is considered as a compound of tannin and gallic acid with sulphate of iron, but I believe it does not contain any tannin whatever. It has been already remarked, that a copious precipitate falls down when ink is made; this appears to contain the tannin. When a solution of gelatine is added to a decoction of galls so as to precipitate the tannin, the fluid still affords an equal quantity of ink with sulphate of iron, and without the formation of any additional precipitate. When a decoction of galls is kept exposed to the action of the atmospheric air, oxygen is absorbed and carbonic acid gas is extricated; the decoction loses its astringent taste, becomes acid, and no longer precipitates gelatine; the tannin has in fact been converted into gallic acid. The addition of sulphate of iron does not now cause a precipitate till after the interval of a day or two, and then is very scanty. I infer therefore that it is a triple compound, a gallo-sulphate of protoxide of iron.

In consequence of the change which galls thus undergo, the quantity of ink which they afford is increased nearly threefold; 413 grains of galls require for combination 144 grains of sulphate of iron, but when the tannin has been converted into gallic acid the same quantity requires 336 grains.

When the persulphate of iron is combined with a decoction of galls, an intense blue compound results, but after a short time the colour changes to a dirty green. It is unfit for any practical purpose; but it deserves notice, because it enables us to ascertain the requisite quantity of protosulphate which a given quantity of galls requires. The persulphate is made by combining the protosulphate with nitric acid. For this purpose expose 64 minims of the latter and an ounce of the former together, using an increase of temperature as long as the fumes of nitric oxide gas are extricated. This compound may for the sake of convenience be dissolved in a certain proportion of water, and after the subsidence of the superabundant oxide is fit for use. Having made a solution of galls or gallic acid, add of this solution gradually as long as it causes an increase of colour, which, owing to its great intensity, requires very considerable caution. The method I have found to answer best, is to agitate the compound in a glass vessel so as to wet the sides of it, and while wet to touch it with a wire dipped in the persulphate. The quantity of persulphate re-

 quired

quired to produce the deepest intensity of colour is exactly three times as great as the quantity of protosulphate which is required for making ink. This salt is recommended for this purpose by Berthollet, but he has not given all the circumstances which it is necessary to attend to.

Since the time of Dr. Lewis, logwood has been used as an ingredient in making ink, by which an increase of colour is effected at an inconsiderable expense, without *in any considerable degree* injuring the properties of it. As the phænomena which logwood presents with sulphate of iron are in some respects peculiar, it is proper to allude to them in this place. A solution of logwood when recently made, absorbs oxygen from the air; and accordingly as it has absorbed more or less, there is a difference in the colour of the compound which results with the sulphate. With the decoction when recently made it forms a compound of a greenish-blue colour; when it has been exposed for some time (a day or two) to the air, a blue compound results; and when it is saturated with oxygen, the compound is of a brownish-black colour. There is no increase of colour along with these changes; on the contrary, the blue compound has a much deeper as well as a much richer colour than the last. A precipitate falls down in each case, and is equally as copious in the last as in the first instance. It cannot be used alone, therefore, for making ink, and ought not to be used in more than a certain proportion along with galls or gallic acid. When galls are used, the proportion is usually three parts of galls and one and a half of logwood; and the latter ought not to exceed this quantity. When gallic acid is used, one part of it may be added to one and a half of logwood.

In making ink with galls without the addition of logwood, the following directions may be observed:

Take of galls, one pound.
——— sulphate of iron, three ounces sixty-four grains.
——— gum, ditto ditto
——— water, three quarts.

Boil the galls, when bruised, with three pints of water till a quart of decoction remains; pour it off, and add the remainder of the water, and again boil till a quart remains. Mix both decoctions and dissolve the other ingredients in it; let them stand for twenty-four hours, and pour off the fluid ink from the precipitate, when it may be kept for use.

If it should be wished to convert the tannin of the galls into gallic acid, a decoction may be made in the manner above directed. Let it stand exposed freely to the air for ten days, agitating every day for a few minutes two or three times a day. By this means it may be converted into gallic acid; and as it thereby

thereby forms a greater quantity of ink, it must be diluted with water. To a quart of decoction add three pints and a half of water, and to this add sulphate of iron and gum, of each, nine ounces. The sediment which forms may, after three days, be separated, when it is fit for use.

Should gallic acid and logwood be used, the following proportions may be observed:

Take of galls, one pound.
——— logwood, one pound and a half.
——— sulphate of iron, eighteen ounces.
——— gum, ditto.

Make a decoction of galls as formerly directed, convert it into gallic acid in the manner described above. When the process is ended, make a decoction of logwood, by boiling it in five quarts of water till it is reduced to seven pints. Mix this decoction with the solution of gallic acid, and dissolve the sulphate of iron and gum. Let them stand for two or three days, and separate the ink from the sediment.

For the reasons formerly given, it is proper to use the recent decoction of logwood, instead of one which has been kept exposed to the air.

Dr. Lewis's formula for making ink with galls and logwood is so well known that it is unnecessary to give it here.

Ink, when made, ought to be kept as much as possible excluded from the action of the air, because it gradually suffers a change, in consequence either of the gum or the gallic acid absorbing oxygen, by which its colour is injured; and independent of this circumstance, its watery part evaporates and leaves it too thick for use. It ought to be kept in a glass vessel, or if in an earthenware one, it ought to be well glazed; for I have often observed that black earthenware inkstands destroy its colour,—an effect probably to be attributed to the action of the argillaceous matter upon the gallic acid.

Ink suffers decomposition from the action of alkaline substances and their carbonates, the sulphuric acid of the sulphate being attracted, and the oxide of iron being precipitated in combination with gallic acid, forming a gallate of iron*.

Before the early part of the eighteenth century, alum was not used in the manufacture of paper, since which time it has been constantly employed. From repeated observations I am convinced that on paper manufactured without it, the ink

* It has been said that good writing-ink may be made by dissolving iron in galls; but this is a mistake. Iron is acted upon by gallic acid; and as long as there is an excess of acid the compound remains in solution; but when a neutral compound is formed, it is insoluble, and falls down, leaving the water almost colourless.

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retains its colour much better than on that which is now in use; and that the difference which has been observed in this respect, so far as respects writings upon paper, is to be attributed to this cause, and not to any difference in the ink. I shall not attempt any explanation of the phænomenon, but merely state it as a fact*.

Ink made with either a diminished or an increased quantity of sulphate of iron does not keep its proper colour when written with upon paper, changing to a brownish black of diminished intensity. When an excess of the sulphate has been used, and the writing has been suffered to remain till the colour has been thus impaired, it may be in some measure restored by a dilute sulphuric acid. Half a drachm of the *acidum sulphuricum dilutum* of the London Pharmacopœia, added to two ounces of water, brushed over the paper, produces the effect. But if it is used in too large a quantity, or if a stronger acid is used, it injures or destroys the colour altogether.

With regard to parchment, the skin from which it is made naturally contains a considerable quantity of oil, which prevents the ink from fixing upon it. In order to obviate this inconvenience, chalk is used in the manufacture of it, which though it enables us readily to write upon it, produces a very injurious effect upon the ink. The sulphuric acid being abstracted, an insoluble crust is formed which lies upon the surface, but does not penetrate or combine with the substance of the skin. This crust after some time loses much of its colour, adheres so loosely to the parchment that it may be rubbed off with a wet cloth, leaving but little or no mark upon it,—thus affording a ready means of injuring or altering the writing. Nay, the effect is so readily produced, that accidental circumstances, such as rolling and unrolling it, sometimes cause part of the writing to scale off.

This is a defect of much importance as it appears to me, and one which I have no doubt might be remedied. I have endeavoured to obtain the assistance of those who are practically acquainted with the manufacture of it, to make a series of experiments on the subject, but unsuccessfully. I shall not, however, fail to avail myself of the first opportunity which presents itself to endeavour to discover the means by which the fault may be obviated.

* The presence of alum is easily ascertained by the test of infusion of roses. When it is not present, the infusion (made without acid) causes a pink stain to remain upon the paper:—when it is present, the stain is green. Sulphuric acid prevents this change when used in a certain quantity. Thus when twice the quantity ordered by the London College, in preparing *infusum rosæ*, is added to the simple infusion, the stain is pink; but if *infusum rosæ* itself is used, it assumes a green colour.

XXII. *On*

XXII. *On the Volcanos of Guatemala.* *By* M. de Humboldt.*

THE volcanos of Central America are ranged successively between the mountains of Veragua, and Oaxada, latitude 11° to 16°. The gneiss and mica slate of Veragua connect them with the western chain of New Grenada; the granitic gneiss of Oaxada unites them to the Mexican ridge: this connection, however, is formed not by the volcanos themselves, but by the mountainous land which surrounds them. During my voyage from Lima to Acapulco, I collected from the Spanish manuscript charts of John Morabda and other navigators various particulars, which throw light on the situation of the burning mountains of Guatemala with respect to the sea. Most of these volcanos are inserted by Bauza, with an accuracy peculiar to himself, in the *Carta esferica del Mar de las Antillas*, 1805, and in the *Carta esferica desde el Golfo Dulce hasta San Blas*, 1822: yet Von Buch very properly remarks in his classical work on the Canary Isles (1825, p. 406—409), that William Furnel, Dampier's mate, discovered at the early period of his voyage almost all that we know of them at present.

I shall pursue the series from S.E. to N.W., as they are placed by Arago in the *Annuaire du Bureau des Longitudes* 1824, according to the materials which have been imparted to me. Wherever my information does not correspond with the charts, or these do not correspond with one another, I shall state their variations with exactness, that future voyagers may determine the geographic results arising from them. Many of the volcanos have several names, which vary with the variation of the Indian idioms, and are borrowed from those of the neighbouring places. Thus in New Spain, *Popocatepetl* and *Iztaccí huatl*, are called sometimes *Volcanes de Puebla*, sometimes *Volcanes de Mexico ;* and from not understanding this, two mountains may be turned into six. Another source of error is, that in America the name of *Volcano* is not only applied to the mountains whose eruptions extend beyond the age of history, but also to masses of trachyte, which it is certain have never burnt, and are not connected with the interior of the earth by permanent aper-

* The above article relating to the volcanos of Guatemala, or, as it is now called, Central America, is extracted from a paper in a recent number of the *Hertha*, by Alex. de Humboldt. The entire paper relates to the present condition of this free state; but the portion we have selected alone comes within the scope of our Journal, and is sufficiently distinct from the rest for separate publication.—Edit.

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tures. Southernmost stands the *Volcan de Barua*, lat. 8° 50′, in the interior of the country, seven miles N.E. of Golfo Dulce; it is called in the English maps *Volcan de Varu*, and placed, I believe incorrectly, far more to the east (under 84° 52′ west long., and 8° 25′ lat.) in the province of Veragua. Next to the Volcan de Barua comes the Volcan de Papagayo (lat. 10° 10′), not on the mountain of Santa Catalina, but five leagues more to the north, scarcely more than 4¾ English miles from the coast.

East of the Volcan de Papagayo are three old burning mountains, near the south shore of the lake of Nicaragua; viz. the Volcan de Orosi, between Rio Zabales and Rio Terluga; the Volcan de Tenorio; and the Volcan del Rincon de la Vieja,—the last of which is in lat. 10° 57′, and only 1° 35′ west of the mouth of the Rio San Juan in the Atlantic Ocean. The great crater-lake of Nicaragua seems to me to have been produced by some phænomenon connected with this peculiar eastern site of the Volcan de la Vieja.

North of the city of Nicaragua, on the isthmus between the Lake and the sea-coast, between 10° 30′ and 12° 30′ lat. some uncertainty still prevails in the synonomy of the volcanos. Juarros the historiographer of Guatemala, and Antonio de la Cerda, Alcalde de la Ciudad de Granada, whose manuscript maps I possess, adduce merely; 1. Volcan Mombacho, on a mountain a few leagues south-east of the city of Grenada; 2. Volcan de Sapaloca, in the lake of Nicaragua, opposite Volcan de Mombacho; 3. Volcan de Masaya, between Ciudad de Granada and Ciudad de Leon, near the little lake Masaya west of the Rio Tepetapa, which connects the Laguna de Leon or Managua with the Laguna de Nicaragua; 4. Volcan de Momotombo, at the north end of Laguna de Leon, rather to the east of Ciudad de Leon. According to this nomenclature, the Volcan de Granada, of which Funnel and Dampier speak, describing it as being in the form of a beehive, is omitted in all the Spanish sea-charts. From a passage in Gomara (*Historia de las Indias*, fol. 112), it may be concluded that Volcan de Masaya and Volcan de Granada, are synonymous. The chart of the *Deposito Hydrografico* mentions: 1. Volcan de Bombacho, probably the Mombacho of the Alcalde of Granada; 2. Volcan de Granada, west of Ciudad de Granada; 3. Volcan de Leon, clearly from its situation the celebrated Volcan de Masaya, 20′ south of Ciudad de Leon. I repeat that, in my opinion, the mountain which in the Spanish charts is called *Volcan de Granada*, is either Volcan Bombacho, or Volcan Masaya,—for both lie in the neighbourhood (south and east) of Ciudad de Granada. Volcan Masaya, situated nearer the village of

Nindiri

Nindiri than the village of Masaya, was in the first ages of the conquest of the country the most active of all the burning mountains of Guatemala. "The Spaniards," says Juarros, "called it Hell, *el Infierno de Masaya*." Its crater was only from twenty to thirty paces in diameter, but the melted lava seethed and rolled in waves as high as towers; the light from it spread very far, as well as its frightful bellowings. At the distance of twenty-five miles the flames of Masaya were visible. This volcano peculiarly allured the monks of the 16th century, in their thirst for gold. A Dominican, Blas de Iñena, as Gomara relates, descended into the crater by a chain of 140 brazas long, armed with an iron ladle; with the ladle he intended to have taken up the gold in fusion (the fluid lava!); the ladle melted, and the monk escaped with difficulty. The secondary circumstances of this story are certainly fabulous; but it is more than probable that Iñena ventured into the crater, and that his unsuccessful enterprise induced the *Dechan* (dean) of the spiritual Chapter of Leon to obtain permission from the king to open the volcano of Masaya, and to collect the gold which was hidden in its interior. Juarros speaks of another volcano close to that of Masaya, the volcano of Nindiri or Nidiri, which had a great eruption in 1775, when a stream of lava (*rio de fuego*) flowed into Laguna de Leon, or Managua, and destroyed a great many fish. From the situation of the village of Nindiri it may be supposed that this phænomenon was an eruption from the side of the Masaya. In Teneriffe also I have often heard the Volcano de Chahorra spoken of as if it was a different mountain from the Peak. It is very common in all volcanic countries for the volcanos, properly so called, to be confounded with the sites of minor eruptions from their own sides. In travelling from the Volcano de Masaya along the Laguna of Tiscapa across Nagaroti to the city of Leon, east of the city you see, at the north end of Laguna de Leon or de Managua, the lofty volcano of Momotombo; further down, between lat. 12° 20′ and 13° 15′, or between the city of Leon and the Gulf of Amapala or Fonseca, appear the four volcanos of Felica, the Viejo, Giletepe, and Guanacaure. The volcano of Felica is still active, like Mombacho and Momotombo: persons also who visited the harbour of Rialejo last year, saw the Volcano del Viejo smoking considerably. The volcano of Giletepe is named in the Spanish manuscript charts *V. de Cosiguina*, from the neighbouring Punta de Cosiguina, as has been correctly conjectured by M. von Buch.

West of the Gulf of Amapala rise, as it were from the same cavernous bed, the volcanos of San Miguel Bosotlan (Usulutan?), Tecapa, San Vincente or Sacatecoluca, San Salvador, Isalco, Apaneca

Apaneca or Zonzonate, Pacaya, the Volcano de Agua, the two volcanos de Fuego or Guatemala, Acatenango, Toliman, Atitlan, Tajamulco, Sunil, Suchiltepegues, Sopotitlan, the Hamilpas (properly two contiguous volcanos of this name), and Soconusco. Of these twenty burning mountains, those of San Miguel, San Vincente, Isalco, San Salvador, Pacaya, the Volcano de Fuego or de Guatemala, Atitlan, and the Volcano de Sapotitlan, have hitherto been the most active. The volcano of Isalco had great eruptions in April 1798, and from 1805 to 1807, when the flames often came in sight. It is particularly rich in ammonium*.

The volcano of Pacaya lies three miles from the village of Amatitlan, and consequently east of the Volcano de Agua. It is not so isolated as the latter, but is prolonged into a vast ridge with three apparent summits. Streams of lava (which the inhabitants here as well as in Mexico call desert-land (*mal pays*), pumice, scoriæ and sand, have laid waste the surrounding country. At the end of the sixteenth century (according to the *Cronista Fuentes*, tom. i. liv. 9. cap. 9.) the Pacaya emitted day and night not only smoke but flames. The greatest and most celebrated eruptions of the volcano of Pacaya were those of 1565, 1651, 1664, 1668, 1671, 1677, and of the 11th of July 1775. The last eruption was not from the summit itself, but from one of the three lower lateral peaks.

The Volcan de Fuego, or as it is also called Volcan de Guatemala, is situated five miles west of the water-volcano and two miles south-west of the city of Antigua Guatemala. It still at times evolves flame and smoke. Its greatest eruptions since the arrival of the Spaniards were in 1581, 1586, 1623, 1705, 1710, 1717, 1732, and 1737. It is in the shape of a beautiful cone, near its top, however, disfigured by several hills of scoriæ, the remains of lateral eruptions.

The order of succession in which the extinguished volcanos arise south of Laguna de Atitlan, between Nueva Guatemala and Zapotitlan, seems to me, as a fact in geological science, very remarkable. They stand on two chasms east and west, and look as if they had slidden; so that the more western row lies

* We believe that the theory of volcanos held by M. de Humboldt, is a modification of that proposed by Sir H. Davy, in which the phænomena they present are ascribed to the decomposition of water by the metallic bases of the earths and alkalies existing in the earth. We presume, therefore, that when he states the volcano here mentioned to be rich in ammonium, it is to be understood that it evolves a large quantity of muriate of ammonia, from which circumstance M. de Humboldt infers that *ammonium*, the supposed metallic base of *ammonia*, is abundant in its interior.
E. W. B.

four

four leagues more to the north. On the eastern chasm are the volcanos of Pacaya, the Water-volcano, the two volcanos of Fuego, and the volcano of Acatenango; on the western, nearer the lake of Atitlan, are the volcanos of Toliman, Atitlan, and Sunil, with several isolated mountains, the names of which are unknown to me.

The Water-volcano (*Volcan de Agua*) is, in comparison with the twenty-one partly extinguished and partly still burning volcanos of Central America, one of the highest and most celebrated. It lies twenty miles east of the great Laguna de Atitlan, between Antigua Guatemala and the populous villages of Mixco Amatitan and San Christobal. As the altitude of not a single mountain of the Guatemalan Andes has been measured, I draw my opinion of its height merely from the circumstance that the mountain often remains covered for many months together with rime, with ice, and perhaps even with snow. In so southern a latitude the height cannot be less than 11,000, nor above 15,000 English feet. Mountains which exceed the latter are real *Nevados*, that is, covered with eternal snow. Capt. Basil Hall estimates the two volcanos of Guatemala at 14,331 and 14,562 feet, an admeasurement taken at the distance of forty leagues, and which cannot therefore be much relied on. Pater Remesal, (*Hist. de la Provincia de San Vincente*, liv. iv. cap. 5.) who plays with numbers in the old-fashioned way, asserts, that in 1615 the Water-volcano, as it is called, was still three leagues (*leguas*) high, although it lost its crown (*coronilla*), which was one league high, by the eruption of the 11th of September 1541, when Almolonga, or Ciudad Vieja, was destroyed! The geognostic relations of this water-eruption are wholly unknown. Juarros relates, that neither burnt stones nor any traces of volcanic eruptions were discoverable in the declivity of the mountain; perhaps however ashes and lava are covered by the vegetation; perhaps not merely subterranean caverns have been filled for centuries by rain-water which has flowed into them, but a crater-lake also may have existed in the summit itself.

In the province of Quito, I have been told, that the volcano of Imbabaru, which has been extinguished longest, near the Villa de Ibarra, from time to time (probably after earthquakes) casts forth water, slime, and fishes; thus much, however, is certain, that the Volcano de Agua, which lies between the Volcano de Pacaya and the Volcano de Fuego, has the form of a blunted cone. Two-thirds of the slopes of this great mountain-root, which is said to be eighteen leagues in diameter, are cultivated as a garden; further upwards arise majestic woods, and on the top there still exists an elliptical cavern, the diameter of which from N. to S. is 400 feet long. This is without doubt a crater (*caldera*); and Juarros, although he denies that there are any traces of fire in the water-volcano, describes (tom. ii. p. 351) it himself exactly as several intelligent natives of Guatemala have described it to me.

North of the group of the volcanos closely ranged between Pacaya and Sunil, at the western extremity of the lake of Atitlan, the heat-exhaling cavern of Central America seems gradually to close. The volcano of Soconusco, of which Juarros makes no mention (in Bauza's chart, 15° 59′ lat., and 95° 41′ long.), is the limit imposed to the series of volcanic eruptions on the western edge of the granitic-gneiss mountain of Oaxada: on the shore of the South Sea there is no volcano for 220 leagues till you arrive at the Volcan de Colonia. After having named, in these pages, between the parallels of 8° 50′ and 16°, in a direction from S. E. to N.W., five-and-thirty conical mountains, which are considered on the spot as volcanos, and of which fifteen have undoubtedly emitted smoke or flames within the last half century, I may safely repeat my assertion,—that in no part of the globe, not even in Chili, or the Indian Archipelago and the Alentes, is there so lasting a communication by means of caverns between the interior of the earth and the atmosphere. Future travellers will ascertain which among the thirty-five so-called *Volcanes de Centro-America* are cones of trachyte, and which are real open burning mountains.

XXIII. *Notice of some remarkable Crystals of Quartz, imbedded in the Limestone of the Black Rock, near Cork.* By W. Phillips, *F.L.S. F.G.S. &c.**

I LATELY received a letter, dated from Bristol, and signed "Samuel Peter, Dealer in Minerals, &c." accompanied by some specimens of limestone containing prismatic crystals of quartz, terminated at each end by the ordinary pyramid of that substance. These specimens, as the letter informed me, were obtained from a quarry at Black Rock near Cork; and the writer observed that he sent them on account of the (apparently) quartz crystals not having hitherto been publicly noticed, as consisting of alternate concentric layers of quartz and limestone, and that he had sold them under the name of *pseudolite*, which, however, is not very appropriate.

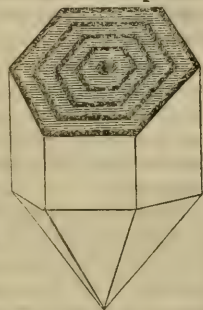
These crystals are generally about one-fourth of an inch long, rarely attain half an inch, and their width is mostly equal to about half their length; they are smooth externally for the

* Communicated by the Author.

most

most part, and sometimes are considerably bright; they are externally of that colour which is termed smoky or brown quartz, and may generally be separated from the limestone readily, leaving a cavity exhibiting their precise form.

In order to observe as accurately as possible the interior of these crystals, I attempted to break them across the prism, at right angles to the axis. I found, however, that they would yield only in directions parallel, or nearly so, to one or other of the planes of the pyramid, as is the case with common quartz. The annexed sketch exhibits a cleavage in that direction, and, generally speaking, also the aspect of the plane produced, which appeared to consist of alternate and concentric prisms of smoky quartz, which in thin fragments is highly translucent, and of gray, opaque, and somewhat granular limestone.



On applying a drop of diluted muriatic acid to the fractured surface, a brisk effervescence immediately ensued along the parts which are of a gray colour, satisfactorily proving the presence of limestone, and inducing a hope that it might thus be possible to dissect the crystal, by separating the smoke-coloured layers of quartz from each other: but in this I was disappointed; for the effervescence soon ceased, and I could not procure a renewal of it for any considerable time, by replacing the first by a second quantity of diluted acid; and the result of leaving the fragments in it for a week or two was, that the gray parts of the crystal became visibly cellular, and so soft as to admit of being scraped away by the point of a knife, leaving a little channel between the lines of brown quartz.

The annexed sketch, though not an absolutely faithful portrait of a crystal in my possession, shows the fact, that the whole consists of a minute central crystal of quartz surrounded by four thin layers of the same mineral, separated by a gray substance constituted in part of limestone. The outer coat of brown quartz is generally thicker than the layers within it, of which however the sides towards the exterior of the crystal are almost always better defined than the interior, which mingle with the gray substance, so as sometimes to prevent the occurrence of a complete line of separation between them; and the lines of brown quartz are not always so perfectly defined as is represented in the sketch. I have observed several crystals in which the lines of quartz are far more numerous,

 and

and though possessing a tendency to the same form as the outer coat of quartz, not disposed with regularity; while on the contrary others consist of a mere external coating of smoky quartz, the interior being wholly gray, which in all cases affords brisk effervescence on the application of acid. A portion of the limestone taken from the immediate neighbourhood of a crystal was wholly dissolved, with very brisk effervescence.

It seems reasonable to conclude that such part of the gray substance as does not yield to the action of the acid, is siliceous or quartzose; and that the prime difference between it and the smoky quartz surrounding it, consists in the different circumstances of crystalline aggregation under which they were deposited.

These crystals are at least curious, if not instructive, and may hereafter, in connection with the somewhat analogous case of the Fontainbleau sandstone, as it is termed, serve to assist in the illustration of some points relative to the laws of affinity as operating in the formation of crystals, when a sufficient number of facts shall have been collected. Meantime, it may perhaps be permitted to observe, that the fact of their including a proportion of limestone would lead to the notion of their formation having been contemporaneous with the deposition of the limestone inclosing them. And it seems not less certain that the menstruum from which the whole was deposited, differed at different and alternating periods; now permitting the free exercise of that law by which the particles of silex coalesced and were deposited with crystalline regularity, and then affording interruptions, by which minute portions of limestone were involved mechanically among the siliceous matter.

XXIV. *Some Remarks on* Capt. Sabine's *Pendulum Observations. By* Thomas Henderson, *Esq.**

In the Quarterly Journal of Science, Literature and Art, New Series, No. II. p. 382, Capt. Sabine has taken notice of an error which he had committed in estimating the value of the divisions of the level of the repeating circle employed in his pendulum experiments, each division having been assumed to denote single seconds in place of ten, the correct quantity. The circle having been used for determining the rate of the chronometer at New York, he has computed anew the observations made there, and has found that the results of the pendulum experiments at that station are affected to a very

* Communicated by the Author.

trifling

trifling extent by the error alluded to. But he has not mentioned what effect this error has occasioned upon the experiments at other stations, which depend upon the accuracy of the observations made with the circle. Neither has he taken any notice of the corrections to be applied to his computations of latitude, although it is evident from the slightest inspection that in many instances they are considerable, and must destroy the remarkable accordance between the single results at the same station.

Several of the latitudes were obtained from observations of southern stars, and depend on the accuracy of their calculated positions. Capt. Sabine seems to have taken their right ascensions and declinations from Mr. Lax's Nautical Tables; but at the time when that useful work was published, the positions of the southern stars invisible in Europe were not accurately known, as they had not been observed since the time of La Caille. We are now in possession of more correct catalogues of these stars, founded on the observations of Mr. Fallows at the Cape of Good of Hope (Philosophical Transactions for 1824), and of Mr. Rumker in New South Wales (*Astronomische Nachrichten*, No. 82). From the subjoined comparison it will be seen that the apparent places assigned by Capt. Sabine to the first two stars, differ very materially from those obtained by computation from the catalogues of those astronomers.

It is hoped that Capt. Sabine will see the propriety of revising his computations and publishing the results; for it is not improbable that the alteration which has been made upon one of the principal elements of his experiments may exercise a considerable influence upon the deductions made from them: at any rate it must be admitted that at present a certain degree of doubt attends his observations, which ought to be removed. In the mean time it may be said that he has much overrated the accuracy attainable by the six-inch repeating circle, the performance of which it is now seen does not support the eulogium pronounced upon it. And if the observations of latitude at the island of St. Thomas be held as a test of the accuracy of the repeating reflecting circle, it cannot be considered so perfect an instrument as would appear from the published accounts; for the two observations of latitude (when the declination of *α Crucis* is corrected) will be found to differ half a minute from each other.

			Apparent Æ.	Appt. Decl.
α Crucis,	June 7, 1822,	Sabine,	$12^h 16^m 52^s \cdot 4$	$62° \; 6' \; 44''$
		Fallows,	12 16 47 ·1	62 7 19
		Rumker,		62 7 17

α Centauri,

			Apparent Æ.			Appt. Decl.		
α Centauri, June 26, 1822,	Sabine,	14^h	28^m	13^s	$60°$	$6'$	$33''$
	Fallows,	14	27	40 ·8	60	5	54
	Rumker,				60	6	3
α Pavonis, July 31, 1822,	Sabine,	20	11	36	57	17	34
	Fallows,	20	11	37 ·3	57	17	30
α Gruis, Sept. 2, 1822,	Sabine,	21	57	3	47	48	35
	Fallows,	21	57	4 ·2	47	48	42
Achevnar, Sept. 30, 1822,	Sabine,	1	31	7 ·5	58	8	7
	Fallows,	1	31	9 ·3	58	8	11
	Rumker,				58	8	8

Edinburgh, July 11, 1827.

XXV. *On the Origin of the Power of suspending Respiration, possessed by Aquatic Mammalia and Birds.* By LAWRENCE EDMONSTON, *Esq.**

TO determine the nature and conditions of that power which certain species of aquatic animals possess of suspending respiration for a considerable time, has long been an interesting object of physiological investigation; but the most accurate observations and ingenious experiments seem hitherto to have failed in removing the obscurity in which this singular phænomenon is involved. Can this have arisen from the erroneous direction in which research has been conducted; or can it result from the insurmountable difficulty of the subject? Has the obvious suggestion been sufficiently attended to, that this anomaly of respiration may constitute one of those ultimate facts in the laws of vitality which, in its nature, may be independent of peculiarity of organization?

We observe certain diving animals that breathe through lungs, as whales, seals, and water-birds, remain long under water at one time, and not merely in a state of quiescence, but often expending great muscular exertion. We know that a much shorter period than this, without respiring, must be necessarily and immediately fatal to land-animals similarly formed; and we are then too hastily disposed to infer, that this singular faculty which aquatic animals possess, must depend on some undetected peculiarity of structure. The problem was supposed to have advanced towards solution, when it was asserted that, in aquatic animals, the *foramen ovale* remained open, and that in them there was something analogous to fœtal circulation.

But in the first place, it is unproved that in them the *foramen ovale* is oftener open than in land-animals; and, in the

* Communicated by the Author.

numerous

numerous individuals of the two species of seal (*Phoca barbata* and *P. vitulina*) which I have dissected, I have never found it open, but in fœtuses. But secondly, though this were the fact, it affords no solution of the question,—of the faculty which these animals possess of supporting life, not merely like the fœtus, with the external senses dormant, and supplied with oxygenated blood through the medium of the mother, but in all the play and vigour of their faculties, without the action of the lungs or the access of oxygen. The *foramen ovale* remaining open may cause less blood to circulate through the lungs, and more to be thrown on other organs; but the fact by no means accounts for the animal being able to sustain vigorous and perfect life, while venous blood must be circulating through the brain, and the vital function of respiration totally interrupted. If it be conceded that provisions may be found, in the increased size of the liver, spleen, or veins of the abdominal viscera, for receiving an additional quantity of blood, diverted from the lungs during the suspension of respiration, still this points out only a change in the distribution of the circulating fluid.

It can hardly be assumed that the moment the animal dives, those processes, which exhaust the blood of its oxygen, are interrupted; for still, at the moment of suspending respiration, much venous blood must necessarily be existing in the circulation. And can we suppose that, until respiration is renewed, the venous blood remains in the pulmonic system without being carried to the systemic heart? Such a hypothesis, not more visionary than some others, only, like them, multiplies difficulties; it supposes, for instance, a power of suspending the processes which exhaust the blood of those vivifying principles which it receives from respiration. It supposes the pulmonic and systemic hearts to be, not merely in function different, but in action separate from and independent of each other. It presumes that the left ventricle contracts on vacuity, or that the aorta and its tributaries can, at the will of the animal, become substitutes for the action of the heart.

Can we assume, that some other organ is vicarious of the function of the lungs, analogous to what we observe to a certain extent to obtain in some secretory functions, as those of the skin and kidneys; or that the system enjoys the power of accumulating an additional stock of oxygen to supply its diving exigencies? Or can we imagine that the liver or any other organ deprives in a great measure the venous blood of those qualities which render it so deleterious to life in terrestrial animals, when carried through the arteries to the brain, as the experiments of Bichat especially, and of other eminent physiologists

siologists luminously demonstrate? To all these suppositions the same general objection applies,—that they are not only entirely destitute of proof, and inadequate to the solution of the difficulty, but multiply others equally unaccountable. While therefore the most minute and laborious anatomy has failed to unravel any peculiarity of structure adequate to account for this phænomenon, the doubt naturally arises, whether in every case of difference or modification of function we are entitled to presume difference of structure; whether the faculty in question may not be one of those modifications of the vital principle independent of tangible peculiarity of organization? And under this impression, the hypothesis which appears to me to be the simplest, and to be supported by analogy, is, that the conditions of the nervous system of aquatic animals are such, that venous blood requires a much longer period to circulate through their brain than in that of land-animals, to produce the same deleterious effects. Why should it be matter of surprise, that the brain of aquatic animals should have peculiar relations to certain fluids circulating through it; or that their nervous system should be less or more susceptible of sedative or stimulating influence? Different animals, and even different organs in the same animals, are very differently affected by similar causes; and in such cases we do not always necessarily presume difference of structure. Is it even wonderful that the excitability of aquatic animals should be such as to render them unable permanently to bear the same highly oxygenized blood as land-animals; much in the same way as we suffer, and should at length die, by breathing air in which there is more than its usual proportion of oxygen? Different species of terrestrial animals require, *cæteris paribus*, unequal quantities of oxygen; and even different individuals of the same species, both in health and disease, are dissimilar in this respect.

Why may not the natural and healthy state of the blood in aquatic animals have a tendency to the venous, as that of terrestrial animals has to the arterial standard? And this indeed I believe to be the fact; for in all amphibious mammalia and birds which I have examined, the blood has much more of the venous than arterial appearance. If this fact be established by a sufficiently ample induction, it would seem decisive of the question; and it would furnish a satisfactory reason why aquatic animals decline and at length die, when placed in situations where they are long deprived of the power and the necessity of diving.

I have also observed, that if seals, after remaining on the rocks for an hour or two, are killed before returning to the water,

water, the blood is more florid than under opposite circumstances; and that after long reposing on land and then betaking themselves to their native element, they in no instance, when they first dive, remain so long under water as in most other occasions. These facts may be obviously accounted for by supposing, that respiration in these situations being longer continued without interruption than usual, the blood had become more highly oxygenized; and that hence the brain being temporarily more highly excited, could not at once bear so long a continuance of the venous influence. I have repeatedly ascertained that the dark venous blood of the seal when exposed to oxygen, as speedily assumes the arterial hue as that of any other animal.

To a certain extent also, this capacity of suspending respiration is under the influence of habit, as we see exemplified by those individuals, even of our own species, engaged in the pearl-fisheries. And moreover, this faculty even in aquatic animals is limited, and differs in the different species; for beyond a certain period they, like land-animals, are unable to remain under water. I have repeatedly seen seals taken in a net; and when not allowed to come to the surface to breathe, life became extinct generally in less than a quarter of an hour. In this case, it is true, the violent struggling to get loose would naturally abridge the period during which they could remain under water in a tranquil state, which I should believe to be somewhere about twenty minutes.

The young of the great seal, like the young of the other species of *Phoca*, is brought forth on land, and must remain there a month or six weeks before it can live in the water. If before this period it be thrown into the sea, it exhibits as much anxiety and awkwardness as a young dog, and cannot remain longer, without inconvenience, under water. The young of the common seal, on the contrary, follows the mother to the water immediately it is born, and swims and dives with ease and perfection; a difference so strongly marked and so decidedly established, as in my opinion sufficient to constitute among others a most important feature of specific distinction between them.

With respect to whales, I have been often assured by intelligent Greenland shipmasters, that they have been frequently known to remain upwards of an hour under water; and this also accords with my own observation. The difference therefore in this faculty which we remark in different kinds of aquatic animals, and between these and land-animals, seems more to consist in degree than in kind.

If a land-bird, as a crow or a pigeon, be thrown into the water

water, even while the head is kept up it is as speedily *drowned* as a cormorant under it. For this fact we can give no other adequate solution, than by referring it to some peculiarity of the nervous system. It may indeed be argued, that the cause of death in this instance results proximately from the spasmodic closure of the glottis, produced by the shock of immersion. But why should this occur in the land-bird more than in the cormorant?

Not only is the quality but also the *quantity* of blood in aquatic animals different from that in land-animals. In the former, the quantity is much greater in proportion to their size. In whales and seals for instance, it is excessive; and their capacity of great and continued muscular exertion is also strikingly conspicuous. Can this depend chiefly on the different quality or increased quantity of the vital fluid; or on some particular condition of the nervous energy?

On a review of what I have here advanced on this subject, it appears to me that we may be justified in assuming, that the nervous system of aquatic animals breathing through lungs is so constituted, that the venous blood requires a much longer period to circulate in the brain before it produces death than in land-animals. That the natural and healthy state of their blood is *subarterial;* and that it is not necessary, in accounting for the superior power which they possess of suspending respiration, to presume any peculiarity of organization.

These views I have long entertained; and they are founded on an ample experience of many years, both as a zoologist and a sportsman, in situations peculiarly favourable for accurate observation on the œconomy and structure of aquatic animals.

Balta Sound, Zetland, April 1827.

XXVI. *Notice of some Miscroscopic Observations of the Blood and Animal Tissues. By* Dr. Hodgkin *and* J. J. Lister.*

THE powerful compound achromatic microscope in the possession of J. J. Lister, being, as I have reason to think, far superior to any thing of the kind yet produced in this country, a short account of its application to animal structures will probably be considered not altogether uninteresting. This microscope is the only one, which, up to the present time, has borne a comparison with the justly celebrated instruments of Amici. After repeated comparative trials, with the most

* Communicated by Dr. Hodgkin.

most delicate test objects, it was impossible to decide the question of superiority between J. J. Lister's microscope, and that which the profound and skilful optician of Modena, had with him during his late visit to this country; although the professor most obligingly afforded every facility for the trial.

Most of the observations of which I am about to speak, were made in the course of the last spring, when my friend not merely favoured me with the use of his instrument, in order to ascertain the accuracy of some recent microscopic examinations, but took upon himself a very active part in the inquiry.

As we hope, before long, to give a detailed account of our investigations, I shall on the present occasion confine myself to an outline of the facts.

Particles of the Blood.—In our examination of these corpuscles, we have in vain looked for the globular form attributed to them, not only by the older authors Leeuwenhoeck, Fontana, and Haller, but still more recently by Sir Everard Home and Bauer. Our observations are also at variance with the opinion long since formed by Hewson, that these particles consisted of a central globule inclosed in a vesicle composed of the coloured part, and which, though refuted by Dr. Young, has since, in a modified form, been revived by Sir Everard Home and Bauer in this country, and by Prevost and Dumas on the Continent. We have never been able to perceive the separation of the colouring matter, which our countrymen have described as taking place in a few seconds after the particles have escaped from the body; nor can we with Prevost and Dumas, consider the particles as prominent in the centre.

The particles of the blood must unquestionably be classed amongst the objects most difficult to examine with the microscope; partly from the variations of form to which their yielding structure renders them liable, but still more from their being transparent and composed of a substance which, as Dr. Young has remarked, is probably not uniform in its refractive power.

These causes of error we have endeavoured to counteract by varying the mode of observation. We have viewed the particles both wet and dry, both as opaque and as transparent objects, under every variety of power and light, and we lay no stress on observations which have not been confirmed by frequent repetition.

To us the particles of human blood appear to consist of circular flattened transparent cakes, which, when seen singly, appear to be nearly or quite colourless. Their edges are

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rounded, and being the thickest part, occasion a depression in the middle, which exists on both surfaces. This form perfectly agrees with the accurate observation of Dr. Young, that on the disks of the particles there is an annular shade, which is darkest on that side of the centre on which the margin is brightest. Though the Doctor drew the obvious conclusion that the disks were concave, he does not consider the fact as demonstrated; since the appearance might be produced by a difference in the refractive power of different parts of the corpuscle.

This objection we think completely met:

1st. By their reflecting the erect image of any opaque body placed between them and the light, precisely as a concave lens would do.

2dly. By the appearance presented by the particles when viewed dry, as opaque bodies. When illuminated by the whole of the Leiberkuhn, the entire margin is enlightened, and in most of the particles there is besides a broad inner ring of considerable brightness; whilst the centre, and the space between the two rings, is completely dark. On half the Leiberkuhn being covered, the rings are reduced to semicircles, the outer one being opposite to the light side, and the inner to the darkened side of the speculum.

3dly. When fluid blood having been placed between two slips of glass, the particles happen to be at right angles to the surfaces of the glass, so as to be seen in profile, the two concave surfaces are visible at the same time, or alternately, but more distinctly, if the particles slightly vacillate.

The concavity of the disks is, however, extremely trifling; and under particular circumstances, in a few of the particles, the surface is to all appearance quite flat.

Notwithstanding the great uniformity in the size of the particles of the blood, so long as they retain, unimpaired, the form which they possess on escaping from the body, their real magnitude has been so variously estimated, that we judged it worth while to attempt a new measurement. In doing so, we adopted a method somewhat different from those hitherto employed. A camera lucida is adapted to the eye-piece of the microscope in such a manner that the distance of the paper being ascertained, the object may be drawn on a known scale. Tracings of several of the images being made, they were applied to, and compared with, the images of other particles until their accuracy was established.

The diameter of the particles obtained in this manner may be pretty correctly stated at $\frac{1}{3000}$ of an inch.

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The following measurements by former observers are given for the sake of comparison.

Jurine	$\frac{1}{3240}$
Jurine in a 2d measurement	$\frac{1}{1940}$
Bauer	$\frac{1}{1700}$
Wollaston	$\frac{1}{5000}$
Young	$\frac{1}{6060}$
Kater	$\frac{1}{4000}$
Ditto	$\frac{1}{6000}$
Prevost and Dumas	$\frac{1}{4076}$

The thickness of the particles, which is perhaps not so uniform as the diameter of the disks, is on an average to this latter dimension as 1 to 45.

The form and size of the particles of the blood of other animals have frequently been compared with those of man. Many observations were made for this purpose by Hewson; but while some of them appear tolerably accurate, others are decidedly far from the truth. Those which have recently been made by Prevost and Dumas, are the most extensive and complete which as yet exist. Our attention having been chiefly taken up with the blood of man, we have not as yet carried our investigation of that of other animals so far as we design doing; we have, however, examined the blood in all the classes of vertebrate animals, and in different species of most of them. Our observations completely accord with those of Prevost and Dumas, as to the particles having a circular form in the mammalia, and an elliptical one in the other three classes. There are varieties both in the size and proportion of the particles in different species. Thus for example, in the pig and rabbit, the particles have a less diameter, but a greater thickness than in man. We have hitherto invariably found the elliptical particles larger than the circular, but they are proportionably thinner. In birds, the particles are much more numerous, but smaller than in either reptiles or fishes.

There are numerous interesting phænomena which present themselves when the particles lose their integrity and assume new forms. Changes of this description are occasioned by the spontaneous decomposition which the blood undergoes a longer or shorter time after its escape from the body, by mechanical violence, and by the addition of various substances, which appear to exert a chemical action on the matter of which the particles are composed. To these appearances we have been induced to devote the more attention, from their seeming calculated to throw some light on the composition and structure of the particles. We were also desirous of not hastily or rashly denying the existence of those colour-

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less central globules which have been strongly insisted on by Sir Everard Home and Bauer, and by Prevost and Dumas, and which have been regarded not merely by themselves, but by other distinguished and intelligent physiologists, as constituting by their varied combination the different organic tissues. The separation and detection of these globules is stated to be facilitated by some of the means which effect the changes to which I have alluded; but, as I have already stated, we have in vain looked for these globules.

After blood taken from the living body has been kept a sufficient length of time for an alteration in the form of the particles to commence, and this according to circumstances will be from a very few hours to one or more days, the first change which we have noticed is a notched or jagged appearance of the edge of a few of the particles. The number so modified continues to increase: some of the particles lose their flattened form, and appear to be contracted into a more compact figure; but their outline continues to appear irregular and notched, and their surfaces seem mammillated. Hewson and Falconar appear to have accurately noticed this change, and have compared the particles in this state to little mulberries. When more time has elapsed, most of the particles lose this irregularity of surface and assume a more or less perfectly globular form, and reflect the image of an interposed opaque body as a convex lens would do. Some of the particles resist these changes much more obstinately than others.

If a small quantity of blood be placed between two pieces of glass, which are afterwards pressed together with some force, several of the particles, however recent the blood, will be materially altered. The smooth circular outline is lost, and as in the former case, they appear notched. A few seem to be considerably extended by the compression. When the surface of the particles has in this way been broken into, the ruptured part exhibits an adhesive property capable of gluing it to another particle or to the surface of the glass; but the particles in their natural state, though often drawn together or applied to the surface of the glass by the force of attraction, seem to be nearly or quite void of adhesiveness.

There is scarcely any fluid except serum which can be mixed with the blood without more or less altering the form of its particles, probably in consequence of some chemical change. In this general result our observations accord with those of Hewson and Falconar, whose experiments of this kind were very numerous. We differ in some of the particulars, but I reserve the detail of these for a future occasion. There is no fluid which, when mixed with the blood, produces a more remarkable and sudden alteration in the appearance of the particles than water does. With a rapidity, which in spite of every precaution, the eye almost invariably in vain attempts to follow, they change their flattened for a globular form, which from the brightness and distinctness of the images which they reflect as convex lenses, must be nearly perfect.

Contrary to Sir Everard Home's remark, that the particles in their perfect and entire state are not disposed to arrangement, it is in this state only that we have found them run into combinations, which they assume with considerable regularity. In order to observe this tendency of the particles, a small quantity of blood should be placed between two slips of glass. In this way the attraction exerted by one of the pieces of glass, counteracts that of the other, and the mutual action of the particles on each other is not interfered with, as is necessarily the case when only one slip is employed.

When the blood of man, or of any other animal having circular particles, is examined in this manner, considerable agitation is at first seen to take place amongst the particles; but as this subsides they apply themselves to each other by their broad surfaces, and form piles or *rouleaux* which are sometimes of considerable length. These *rouleaux* often again combine amongst themselves, the end of one being attached to the side of another, producing at times very curious ramifications.

When blood containing elliptical particles is examined in the same manner, it exhibits a not less remarkable but very different mode of arrangement. Though they are applied to each other by some part of their broad sides, they are not so completely matched one to another, as is the case with circular particles; and instead of placing themselves at right angles to the glass, with their edges presented to its surface, they are generally seen nearly parallel to it, one particle partially overlaying another, and their long diameters being nearly in the same line. The lines thus formed are subjected to a kind of secondary combination, in which several assume to themselves a common centre, whence they diverge in radii. It is by no means rare to see several of these foci in the field of the microscope at one time. The particles at these points appear crowded, confused, and misshapen. This tendency to arrangement is perhaps not to be wholly attributed to the ordinary attraction existing between the particles of matter, but is probably to a greater or less degree dependent on life; since we have not only observed that the aggregating energy is of different force in the blood of different individuals, but that in the blood of the same individual it becomes more feeble the longer it has been removed from the body. At the same time,

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we are very far from believing that these or any other mode of aggregation which the particles of the blood may be observed to assume, ought to be regarded as at all analogous to the process which nature employs in the formation of the different tissues.

I some years ago briefly stated this opinion, which I was induced to form *a priori**; but I may now appeal to facts in support of it.

In proceeding to offer a very short sketch of the result of our inquiries into the microscopic appearances of some of the animal tissues, I do so with one painful feeling, which I shall perhaps be excused from expressing. It is, that I am under the necessity of differing from my excellent and intelligent friend Dr. M. Edwards. It was the knowledge of his talents and address, and of the patience and care with which he made those investigations, which he has related, which induced me to enter into the examination of a question, which I had already regarded as settled in the negative. And though J. J. Lister and myself, in repeating the observations of Dr. M. Edwards, have arrived at widely different conclusions, I am confirmed in the conviction, that he described what he saw, and that he only saw amiss through the imperfection of his instruments. The idea of the globular structure of the different tissues is however by no means peculiar to Dr. Edwards, and to those micographers to whom I have already frequently alluded. Dr. Edwards, in the papers to which I refer, has employed much erudition to show that similar views had been taken, with respect at least to some of the tissues, by Hooke, Leeuwenhoeck, Swammerdam, Stuart, Della Torre, Prochaska, Wenzel, Dutrochet, and Clocquet.

Muscle.—The muscular tissue may be easily seen with the naked eye, or with the assistance of a comparatively feeble lens, to be composed of bundles of fibres, held together by a loose and fine cellular membrane, and these fibres are again seen to consist of more minute fibrillæ. It is difficult to push the mechanical division much further; for the softness of the muscular substance is such, that it either crushes or breaks off, rather than admit of further splitting. If a piece of one of the most delicate of the fibrillæ last arrived at be placed on a piece of glass in the field of the microscope, lines may be seen parallel to the direction of the fibre, which show a still further division into fibres. Although no trace of globular structure can be detected, innumerable very minute, but clear and fine, parallel lines or striæ may be distinctly perceived, transversely mark-

* *Vide* "Thesin de Absorbendi Functione." Edin. 1823.

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ing the fibrillæ. In some instances they seem to be continued nearly or quite at right angles completely across the fibril; but frequently the striæ in one part are opposite to the spaces in another, by which arrangement a sort of reticulated appearance is produced. The striæ are not in all specimens equally distant, but this may perhaps be owing to the elongation or contraction of the fibre. We have discovered this peculiar and very beautiful appearance in the muscles of all animals which we have as yet examined; and as we have seen it in no other tissue, we have been induced to view it as a distinguishing feature of muscle.

Nerves.—These appear to be essentially composed of fibres, but their structure is looser than that of muscle. Though the fibres of nerves do not form such intricate plexuses as those of some other tissues, their course is by no means straight. We have looked in vain for globules, as well as for any trace of medullary matter, which has been somewhat gratuitously supposed to be inclosed in the nerves.

Arteries.—The middle coat of these vessels being still regarded by some persons as muscular, we were desirous of discovering whether its minute structure was at all more favourable to such an opinion than its chemical composition. Its subdivision may be carried as far as that of any tissue; and it evidently consists essentially of long, straight, very delicate and even fibres, which offer no more trace of those transverse striæ, which we have regarded as the peculiar characteristic of muscle, than they do of elementary globules.

The inner coat, when completely detached from other structures, and presenting the appearance of a very thin uniform and almost transparent membrane, is also, by the aid of the microscope, seen to be composed of fibres, which are extremely delicate, smooth and uniform, but very tortuous and matted together, in the form of an intricate plexus.

Cellular Membrane.—This also appears to be very much if not wholly composed of fibres. Our observations respecting this tissue are not as yet complete.

Brain.—If there is any organized animal substance which seems more likely than another to consist of globular particles, it is undoubtedly that of the brain. Our examination of it has as yet been but slight; but we have noticed that when a portion of it, however fresh, is sufficiently extended to allow of its being viewed in the microscope, one sees instead of globules a multitude of very small particles, which are most irregular in shape and size, and are probably more dependent on the disintegration than on the organization of the substance.

The structure of some other parenchymatous parts appears equally indeterminate, presenting neither globule nor fibre.

Pus.—As far as we have yet examined this secretion, its particles appear to be as irregular in size and figure as those observed in the brain, and bear no resemblance to those of the blood.

Milk.—In this fluid the particles appear to be perfect globules. But, far from being uniform, they present the most remarkable varieties in respect to size. Whilst some are more than double, others are not a tenth part of the size of the particles of the blood, to which they bear no resemblance.

I forbear at present offering any remarks which the preceding observations would suggest.

XXVII. *On the Direction of the Diluvial Currents in Yorkshire. By* John Phillips, *M.Y.P.S. Lecturer on Geology*.*

MR. WILLIAM SMITH has been represented by Mr. Greenough as favouring the opinion that in our island the waters of the deluge moved generally from east to west. Mr. Smith, I believe, when he used these expressions in conversation, had in view merely to explain the occurrence of chalk gravel in Warwickshire and Lincolnshire, and of chalk flints on the oolite hills near Bath.

Dr. Buckland, who in his "*Reliquiæ Diluvianæ*" has so well expressed and enforced the general sentiments of English geologists, as to be with justice regarded as the great advocate and interpreter of the diluvial theory, has adduced evidence by which he thinks he has made probable the opinion of a general southward current.

In the remarks which I have now to offer to the Society, it is not my intention to travel over so wide a field. I rather wish to confine my observations to some facts which I think tend to show the direction of the diluvial waters across part of Yorkshire, and of some neighbouring counties. For this object I shall extract from my Journal some localities where boulders have been seen of rocks so peculiarly characterized as to allow no doubt of the spots from whence they were dislodged.

Shap and Birkbeck fells in Westmoreland, between Penrith and Kendal, are elevated in some parts about 1500 feet above the sea. Lying on the eastern skirts of the mountain group which incloses the lakes of Cumberland, Westmore-

* Read to the Yorkshire Philosophical Society, Nov. 7, 1826; and communicated by the Society.

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land, and Lancashire, they are environed by high ground on the south-east, south, and west sides: toward the north-east and east are lower ranges of mountain-limestone; beyond them is spread the wide vale of Eden, and the distance is formed by the long range of mountain-limestone which, from Ingleborough to Castle Carrock fell, presents to the west an escarpment varying in height from 1500 to 1900 feet. Stainmoor Forest, the lowest part of this great range, lying east of Shap fells, is 1500 feet above the sea.

Shap fells, like all the contiguous mountain tracts, are composed principally of various argillaceous slate rocks; but on the side of the Penrith and Kendal road appears a particular kind of porphyritic granite, composed of light-coloured felspar, grayish quartz, and dark mica, and inclosing abundance of large well-defined crystals of red felspar. The aspect of the rock is very characteristic, and its fragments are recognized at first sight.

No such granite is found *in situ* in any other part of the Cumbrian lake district: and there is no danger of confounding with it any Scotch granite, that I am acquainted with, except some varieties that may be collected in Ben Nevis; and these may be distinguished on careful inspection.

The granite of Shap fells seems therefore particularly worthy of their attention, who would trace practically from point to point the direction of diluvial currents.

Last summer (1826) I observed large boulders of this granite in the low ground about a mile south of Carlisle, and on an elevated red-sandstone fell north-west of Kirk Oswald. Abundance of boulders of this granite, lie some miles north of their native site, near the villages of Shap and Great Strickland.

In a southern direction, such large boulders are seen lying on the hills between Kendal and Sedbergh: they are common in the vale of the Lune, with many pebbles seemingly derived from the neighbouring hills of argillaceous slate; and were found of a ton weight in the vast heaps of diluvial matter cut through in the canal south of Lancaster.

Eastward from Shap fells, this granite has been rolled by the towns of Orton and Brough toward the hollow in the great summit ridge at Stainmoor, on the top of which pass the blocks remain, to attest the direction and force of the transporting waters. From this point the granitic boulders seem to have been dispersed in different directions; as we find them in Teesdale, at Cotherstone, below Barnard-castle near Greta Bridge, and Darlington, at Scotton, south of Richmond, at Catterick, and Leeming, at Stokesley, and Thirsk, and several places be-

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low the range of oolitic rocks between Thirsk and Pocklington, and commonly in gravel-pits and on the surface of the country around York. But this is by no means their eastern limit; for I have observed them of great size at several points on the Yorkshire coast between Redcar and Scarborough, where they are very plentiful on the shore, and are occasionally found inland, as about 300 feet high in cutting a new road toward Hackness, on the hill side above Scalby: and, to complete the evidence, they are seen lying on the cliff at Flamborough head, above 100 miles from their native situation.

An observer stationed on Shap fells sees the country to the north and east a good deal lower than the point where the granite appears *in situ*, and may therefore feel no particular surprise that blocks should have been transported into the vale of Eden; nor does it appear strange that they should have followed the vale of Lune to Lancaster. These effects might possibly have happened in consequence of the bursting of an elevated lake, according to Dr. Fleming's view; but that the bursting of such a lake, in the vicinity of Shap fells, could effect the transport of granite blocks across the deep vale of Eden, over the summit of Stainmoor, and down the valleys and over the hills of Yorkshire, to Scarborough and Flamborough head, I find myself incapable of admitting. This case has always appeared to me decisive in favour of the opinion, that the dispersion of gravel over large tracts is attributable to the deluge.

From the facts above mentioned, I am induced to suppose that the flood of waters, which rolled away blocks of granite from Shap fells, moved principally in an eastward or rather south-eastward direction; and that the dispersion of fragments toward other points may have happened, in consequence of contemporaneous or subsequent minor currents, down the valleys of Eden and Lune.

The next rock, whose bouldered fragments I propose to notice, occurs *in situ* on the precipitous sides of Carrock fell, one of a group of Cumberland mountains lying to the north-east of Skiddaw. It is a compound of white compact felspar and large-grained dark hornblende, in variable proportion, with here and there some magnetic iron-ore.

To the north, bouldered blocks of great size are seen somewhat thinly scattered between Hesket, Newmarket, and Bolton. I have noticed them near Kirk-Oswald, on high ground west of the Eden: they abound about Lowther Castle; and I broke one large mass, which at the time I considered to be a pebble of this rock, near Lord Darlington's smelt-mill at the west pits in Durham.

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The third rock is fully as remarkable and local as either of the others. It consists of pebbles of light-coloured mountain-limestone, united together by red sandstone into a conglomerate provincially called *brockram*. This occurs *in situ*, sometimes alternating with red sandstone, abundantly about Kirkby Stephen, in Westmoreland, as at Stenkrith Bridge, where the Eden has forced through it a romantic and beautiful channel.

I have observed pebbles of this curious rock at Scotton, south-east of Richmond, and at several places on the Yorkshire coast, especially at Scarborough, Bridlington, and south of Owthorne, in Holderness.

Clearly as these examples seem to indicate that over parts of Cumberland, Westmoreland, Yorkshire, and Durham, the principal force of the diluvial waters was exerted in an eastward or rather south-eastward direction, they are not the only ones which lead to such a conclusion. Analogous instances will occur to every one who examines the great variety of boulders which are found in several parts of Yorkshire, especially on the coast of Holderness, and in the neighbourhood of York.

But in both these localities we find fragments of rocks that can only be supposed to have been transported by a different current flowing from the north. The large blocks of limestone, trap, millstone-grit and other sandstones, coal, and magnesian limestone, which appear on the coast of Holderness, may well be admitted to have been derived from north-western Yorkshire, where such rocks abound. But mica slate with garnets, which nowhere occurs in the Cumbrian mountains, is probably derived from Scotland; the radiated fetid limestone clearly claims origin from Building hill near Sunderland, and the lias fossils have been rolled from Whitby: these effects seem to require the admission of a powerful southward flow of waters; and, in the Vale of York, lias fossils from the north, chalk and oolite fragments from the north-east, are mixed with the debris of rocks washed from the north-western part of the county.

Perhaps, on a future occasion, I may offer some additional statements on this subject, not undeserving the attention of a Society which, in consequence of its possessing a fine series of the remains of antediluvian animals procured in the neighbourhood of its museum, must feel a particular interest in the progress of geological opinions respecting the deluge.

XXVIII. *Pro-*

XXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 17.—W. H. WHITE, Esq. was ejected from the Society: and a paper was read, "On the secondary deflexion produced in a magnetized needle by an iron shell, in consequence of an unequal distribution of magnetism in its two branches; discovered by Capt. Wilson, R.N.; by Peter Barlow, Esq. F.R.S."

A paper was also read, entitled, "On the difference of Meridians of the Royal Observatories of Greenwich and Paris; by Thomas Henderson, Esq." Communicated by Mr. Herschel.

Mr. Henderson has detected an error of one second in the reduction from mean to sidereal time of some of the observations officially made for the purpose of determining the difference of the meridians of these two observatories, in July 1825. This connection redeems the result of the observations of July 21 from the suspicion that attached to it; produces a change of one-tenth of a second in the final result of the whole operation, giving $9^{m}\ 21^{s}\cdot 5$ for the most probable difference of longitudes between the two observatories; and also triples the value of the result obtained by narrowing the extreme range of the experiments, from $0^{s}\cdot 65$ to $0^{s}\cdot 21$.

A letter was read from Mr. C. Rumker, of Paramatta, giving an account of several series of observations made at the observatory there; consisting of the following:

1. Original and correctly reduced places of the comet of 1826, as observed from September 4 to October 5.
2. Mean places of the stars with which the comet was compared.
3. Elements of the comet as computed by Mr. Rumker.
4. A catalogue of mean places of stars near the orbit of the comet, about 58 in number, for Jan. 1, 1827, mostly of the 7th magnitude.
5. The latitude of the observatory, $33^{\circ}\ 48'\ 50''\cdot 45$ S., as found by reflexion with the mural circle.

May 24.—The Right Hon. C. Watkin W. Wynne was elected into the Society: and a paper was read, entitled, "On destroying the fire-damp in mines by the chloride of lime; by F. Fincham, Esq." Communicated by Mr. Children.

The very low price at which chloride of lime is produced in this country, led the author, who is a manufacturer of that substance, to several useful applications of it in large quantities. The first which occurred to him was the destruction of the stench of bilge-water in ships, and the correction of the confined air in their holds; both which he has successfully effected in the dock-yards at Deptford and Chatham. After describing the experiments on this subject, he proceeds to relate a series instituted for the purpose of destroying fire-damp in coal-mines.

On March 17th, in a part of the Bradford colliery, where the workmen, during the foregoing part of the week, had been obliged to use the safety-lamp, he sprinkled chloride of lime. On the 19th, though the damp had been collecting during the intermediate Sunday,

day, they were able to work with candles, which they could not have done had no good been done by the application. They threw in a large quantity of the chloride when they began to work; but finding themselves annoyed by the gas evolved, they neglected further application of it for the rest of the week: in consequence of which, on the following Monday, a man going in with a candle was severely burnt, and is since dead. Next day the author sprinkled more chloride; and the day following, a candle burnt in safety at the spot where the man had been killed. The same plan was continued to be used in that spot, which was the only one in the mine infested with the fire-damp. On April 6th, the chloride was disused. On the 10th, a candle caused a strong explosion. On the 12th, the chloride being still disused, an explosion was again produced by a candle. On the 12th and 13th, the chloride was applied. On the morning of the 14th, no explosion could be produced by a candle. The chloride was again discontinued till the 18th, but no explosion occurred. On the 20th, a weak explosion took place, the chloride being still discontinued. The men not having been at work in the interval from the 6th to the 20th, the experiments were fairly tried. The paper concludes with describing the method of using the chloride for this purpose.

A paper was also read, entitled, "On some properties of Heat; by R. W. Fox, Esq." Communicated by Davies Gilbert, Esq. M.P. V.P.R.S.

May 31.—E. W. Pendarves, Esq. M. P., Lieut.-Col. Miller, Major Gen. Wavell, and Dr. Harwood, were respectively admitted Fellows of the Society.

A paper was read, entitled, "On the resistance of fluids to bodies passing through them; by James Walker, Esq." Communicated by Mr. Davies Gilbert.

A paper was also read, entitled, "Corrections of the Pendulum, depending on the value of the divisions of the level of the small repeating circle, as recently ascertained by the experiments of Capt. Kater; by Capt. E. Sabine, R.A. F.R.S."

The alteration in the value of the divisions of the level, resulting from Capt. Kater's observations, as stated in the Philosophical Transactions by Lieut. Forster, led Capt. Sabine to a re-computation of the rates of his chronometer, as taken at Bahia, Maranham, Trinidad, Jamaica, and New York, and of their influence on the length of the pendulum deduced at those several stations. Capt. Sabine states the final result of these re-computations to be as follows:

At Bahia, an augmentation of $\frac{8}{100,000}$ of an inch in the length of the pendulum.

At Maranham, a diminution of $\frac{1}{100,000}$.

At Trinidad, an augmentation of $\frac{4}{100,000}$.

At Jamaica, a diminution of $\frac{7}{100,000}$.

At New York, a diminution of $\frac{4 \text{ or } 5}{100,000}$.

The effects of the correction on the latitudes of the several stations are next stated. These are very small; in one case only amounting to 8″.

The

The author concludes by noticing these accidental errors in his tabulated results, and by a statement, in a tabulated form, of the correct latitudes and lengths of the pendulum at the several stations.

A paper was also read, entitled, "On the effects produced on the air-cells of the lungs when the circulation is too much increased; by Sir E. Home, Bart. V.P.R.S."

June 14.—W. J. Guthrie, Esq. was admitted a Fellow of the Society: and a paper was read, entitled, "On the ultimate composition of simple alimentary substances; with some preliminary remarks on the analysis of organized bodies in general; by W. Prout, M.D. F.R.S."

This is the first of a series of communications on the same subject which Dr. Prout intends to present to the Royal Society; the object of the whole being to determine the exact composition of the saccharine, oily, and albuminous divisions in which the alimentary substances of the higher animals may be comprehended; and then to inquire into the changes which are induced in them, during the subsequent stages of assimilation, by the stomach and other organs. In the present paper are given some preliminary observations on the analysis of organized bodies in general, together with the composition of the saccharine substances. Dr. Prout observed, whilst stating the difficulties attending the use of oxide of copper, as now employed in the analysis of organic substances, that it is not only hygrometric, but, like many other powders, also condenses air. He also found that when it was removed from the tube in which the combustion had been effected, and re-triturated and re-burnt, it almost invariably gained in weight; a circumstance which he ascribed partly to the combination of the oxygen of the air contained in the tube with the partially reduced oxide of copper.

These sources of error Dr. Prout has been enabled to obviate by means of the following well-known chemical facts. When we burn in a given quantity of oxygen gas a substance composed of hydrogen, carbon and oxygen, one of three things must happen. Either the volume of the gas will remain unchanged, in which case the hydrogen and oxygen must exist in the substance in the proportions in which they form water; or the volume may be augmented, in which case the oxygen must be in excess; or lastly, the volume may be diminished, when the hydrogen must exceed that proportion. The author then proceeded to describe the apparatus he employed for the purpose of determining the composition of vegetable substances on these principles; and also detailed some precautions necessary to be observed in the process, pointing out at the same time some of its peculiar advantages, the chief of which is that it is not liable to be affected by moisture. Dr. Prout next considered the composition of the saccharine principle, under which term he included all those substances in which hydrogen and oxygen exist in the proportions in which they form water. These were stated to be all alimentary, or capable of becoming so, and were termed by way of distinction *vegetable* aliments. Sugar was first examined, of which at least two distinct varieties, and probably many more exist. The most perfect form of this principle is sugar-candy prepared from cane-

cane-sugar; identical in composition with which are the purest specimens of the loaf-sugar of commerce. This contains carbon 41·38, and water 58·62; but the principle in the abstract was regarded as consisting of carbon 44·44, and water 55·55. The other variety of sugar was obtained from Narbonne honey, and consisted of carbon 36·36, and water 63·63. Between these two extremes, sugars of almost every possible grade occur, analyses of some of which were given. The next class of bodies considered, was the amylaceous; and the author commenced with some remarks on the sense in which he employed the term *merorganized*. He stated that he had satisfied himself from many observations that the minute quantities of foreign bodies found in all organic products, instead of being mechanically mixed with them, as usually supposed, perform the most important functions; in short, that organization cannot exist without them: that when a crystallized substance passes into the organized state, its chemical composition frequently remains essentially the same, and that the only difference which can be traced in it, is the presence of a little more or less of water, and invariably of minute portions of some of the foreign bodies above alluded to; these appearing not only to destroy its power of crystallizing, but usually to change entirely its sensible properties. This subject he promised fully to illustrate hereafter, but proposed in the mean time to adopt the word *merorganized*, to designate all those substances formed essentially on the principles of crystallized bodies, but not capable of assuming the crystalline form, probably on account of the presence of the foreign bodies above alluded to *.

Starch from wheat appears to be the most perfect form of this principle, and yields from 37 to 43 per cent of carbon, according to the degree of its desiccation. Arrow-root contains still more water capable of separation; and it is to the want of attention to these circumstances that the author assigned the different results obtained by chemists with respect to the composition of starch, which, in the abstract, or free from water, he considered to be identical with cane-sugar similarly circumstanced.

Vinegar, the next principle considered, has in all ages and countries been more or less used as an aliment. Dr. P. had long since seen reason to suspect that the hydrogen and oxygen in this acid existed in the proportions in which they form water; and by means of the apparatus described in the paper, he was at length enabled to satisfy himself on this point. He found that acetate of copper produced no change of bulk in the oxygen employed. This acid consists, by his experiments, of carbon 47·05, and water 52·95. It

* The author observed in a note, that though his attention had been long forcibly directed to the fact alluded to, namely, the singular degree in which all bodies are liable to be affected by the presence of minute quantities of foreign matters (from living animal bodies by the subtle matters of contagion and miasmata on the one hand, down to common crystalline bodies on the other), yet that he could form no distinct notion of their *modus operandi* till the publication, in the Phil. Trans. for 1824, of Mr. Herschel's admirable paper "On certain motions produced in fluid conductors when transmitting the electric current," which threw an entire new light on the subject.

is not known to exist in the merorganized state, except the acid found in almost all animal matters, and hitherto called the lactic acid, be deserving of that appellation.

The woody fibre, or *lignin*, was the last substance belonging to this series, which was considered; and in proof of its alimentary qualities, the author quoted the experiments of Prof. Antenrieth, showing it to be capable, by certain processes, of acquiring the property of gelatinizing like starch when boiled in water, and also of forming bread. Sugar of milk, manna, and gum, were next considered, and afterwards the oxalic, citric, tartaric, and saccholactic acids, the composition of all which was stated; and the author concluded by observing that he refrained purposely from chemical observations on the subject, until all the facts in his possession were laid before the Society.

LINNÆAN SOCIETY.

June 5.—A paper was read, entitled, "Observations and experiments made with a view to ascertain the means by which the spiders that produce gossamer effect their aërial excursions; by John Blackwall, Esq. F.L.S. of Crumpsall Hall near Manchester."

After noticing that in the absence of accurate observation the ascent of gossamer spiders through the atmosphere had been conjecturally ascribed to several causes, such as the agency of winds, evaporation, electricity, or some peculiar physical powers of the insects, or from their webs being lighter than the air, Mr. Blackwall states that the ascent of gossamer takes place only in serene bright weather, and is invariably preceded by gossamer on the ground. He then details the phænomena of a remarkable ascent of gossamer, Oct. 1, 1826, when a little before noon the ground was everywhere covered with it, the day being calm and sunny. A vast quantity of the fine shining lines were then seen in the act of ascending, and becoming attached to each other in various ways in their motion, and were evidently not formed in the air but on the earth, and carried up by the ascending current caused by the rarefaction near the heated ground; and when this had ceased in the afternoon, they were perceived to fall. An account is added of two minute spiders that produce gossamer, and of their mode of spinning, and particularly when, impelled by the desire of traversing the air, they climb to the summits of various objects, and thence emit the viscous threads in such a manner as that it may be drawn out to a great length and fineness by the ascending current, until, feeling themselves sufficiently acted upon by it, they quit hold of the objects on which they stood, and commence their flight. Some of these insects, which were taken for the purpose of observation, when exposed to a slight current of air, always turned the thorax to the quarter from whence it came, and emitted a portion of glutinous matter, which was carried out into a line.

June 19.—Descriptions were read of two quadrupeds inhabiting the South of Africa, about the Cape of Good Hope; by Andrew Smith, M.D. Superintendent of the South African Museum, Assistant Surgeon to the Forces.

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The first of these is the Strand Wolf or Strand Jut of the colonists; and is named by the author *Hyæna villosa.* Some of its habits are noticed in confirmation of the conjectures of Professor Buckland.

An account was also read of a pair of hinder hands of an orang outang of unusual size, deposited in the collection of Trinity House, Hull; by J. Harwood, M.D. F.L.S. &c.

In this paper the author corrects the statements of Dr. Abel (Phil. Mag. and Annals, vol. i. p. 213) respecting the height of the orang outang; and maintains that the Pongo is not the *Simia Satyrus* Linn. as supposed by Cuvier and others, but in reality a distinct species. The Pongo at the College of Surgeons has five vertebræ, while all the skeletons of *Simia Satyrus* have but four: there are also material differences in the cranium and scapulæ.

GEOLOGICAL SOCIETY.

April 20.—Lieut.-Gen. Sir Rufane Donkin, K.C.B. &c. of Park Street, Grosvenor Square; Major T. L. Mitchell, of the Quarter Master General's department, Assistant Surveyor General of New South Wales; and the Rev. W. Whewell, M.A. F.R.S., Fellow of Trinity College, Cambridge, were elected Fellows of the Society.

The reading of Professor Sedgwick's paper, on the Magnesian Limestone, was continued.

A paper was read giving an account of the discovery of a number of fossil bones of bears, in the Grotto of Osselles, or Quingey, near Besançon in France, by the Rev. Dr. Buckland, Professor of Geology in the University of Oxford. The author visited this cave in October 1826, for the purpose of applying to it the method of investigation, which his experience in other caverns had taught him to adopt with success in the pursuit of fossil bones.

The Grotto of Osselles is of vast extent, nearly a quarter of a mile in length, and made up of a succession of more than thirty vaults, or chambers, connected together by narrow passages, and running almost horizontally into the body of a mountain of Jura limestone, on the left bank of the Doubs near Besançon.

The only entrance to the grotto is by an irregular aperture about the size of a common door, in the slope of the hill about 60 feet from the river. The abundance and beauty of the stalactite in many parts of this cavern, have rendered it one of the most celebrated and most frequented of any in France; but before Dr. Buckland, no one had ever sought for bones beneath the crust of stalagmite, which in most of the chambers covers the floor.

On breaking for the first time through the stalagmite, the guides were much surprised to find the author's prediction verified, as to the existence of a thick bed of mud and pebbles, beneath what they had considered to be the impenetrable pavement of the cave, and still more so, to see that in every one of the only four places which he selected for investigation, this diluvium was abundantly loaded with the teeth and bones of fossil bears. These lay scattered through the mud and gravel, in the same irregular manner as the bones of bears lie in the caves of Franconia and the Hartz, and are, like them, the remains of animals that lived and died in these caverns before the introduction of the diluvium. They were found no where in entire skeletons, but dispersed confusedly through the mud. They were from bears of all ages, and none bore marks of either having been rolled by water, or gnawed by the teeth of hyenas, of which last-named animal Dr. Buckland found no traces in this cave, in the few spots which he examined.

Insulated teeth, ribs, and vertebræ, separate fragments of skulls, and epiphyses detached from their bones, lay scattered through the mud and pebbles.

In one extensive grotto called the "Salle à danser," which from its size and dryness is selected by visitors to eat and dance in, there is neither stalactite on the roof, nor stalagmite on the floor, but simply a thick deposit of diluvial mud, containing the same bones as in the other chambers; this mud being very dry is intersected by narrow crevices descending from its surface, and the shells of eggs and nuts, and the bones of chickens, &c. that are carelessly thrown aside by visitors, have sometimes fallen into these fissures, where they lie in juxtaposition with the antediluvian bones. Some of these modern remains are also dragged by rats into holes made in the mud by themselves, or by rabbits, badgers, and foxes.

The author concludes by stating that the best rule to follow in pursuit of antediluvian remains in caverns, is to select the lowest parts in which any diluvium can have accumulated, and there dig through the stalagmitic crust, and seek for teeth and bones in the mud and pebbles that lie below. He also proposes, as a test for distinguishing bones of this antiquity,—their property of adhering to the tongue (happer) if applied to them after they are dry, a property apparently derived from the loss of animal gelatine they have sustained,—without the substitution of any mineral substance, such as we find in bones imbedded in the regular strata. This test extends equally to the bones of the osseous breccia of caverns and fissures, and to those in all superficial deposits of diluvium, excepting such as are too argillaceous to have admitted the percolation of water; but the property of adhesion is rarely found in bones from recent alluvium, or from peat bogs, nor does it exist in human bones, which the author has examined from Roman graves in England, and from the druidical tombs of the ancient Britons, nor in any of the human bones which he has discovered in the caves of Pairland and Wokey Hall.

Dr. Buckland proposes to apply this test to the much disputed case of human bones, said by M. Sclotheim to have been discovered in the cave of Kostriz in contact with those of the rhinoceros and other extinct animals.

Dr. Buckland also found, in the collection of Professor Fargeaud of Besançon, some teeth of fossil bears from a mine of Pea-iron-ore in that neighbourhood; but could not visit the spot to ascertain whether this ore was extracted from a bed of superficial diluvium or from a fissure. Such iron-ore abounds in the diluvium of the east of France; and in fissures at Plymouth, and near Spa.

May

May 4.—Thomas Bell, Esq. of New Broad Street, was elected a fellow of the Society.

The reading of Professor Sedgwick's paper on the Magnesian Limestone was continued.

ASTRONOMICAL SOCIETY.

May 11.—There was read a paper "On the approximate places and descriptions of 295 new double and triple stars, discovered in the course of a series of observations with a 20-feet reflecting telescope; together with some observations of double stars previously known." By the President. The author prefixes a few remarks to this catalogue of stars, in which he first explains why the places of the stars which it contains are probably materially more exact than those of his former catalogue, especially in right ascension. He then records some results of his observations, either in themselves considered, or in comparison of preceding observations of Mr. South, and some other astronomers. Thus the curious double star, Æ $17^h\ 52^m\ 1^s,5$, so remarkably situated in the midst of a very large and conspicuous nebula, affords a striking instance how easily the latter class of objects may be overlooked in the usual mode of conducting astronomical observations. Mr. South measured this star in the 5-feet equatorial, but the nebula, which forms so interesting an appendage to it, entirely escaped his notice. The observations of ξ *Ursæ Majoris* supply a satisfactory confirmation of its motion, in conjunction with those of Mr. South in the spring of 1825, its angular motion being found to be about 7° per annum. Since Mr. Herschel's first observation of this star, in conjunction with Mr. South in 1823, it has described nearly a twelfth of its revolution. This star, therefore, is earnestly recommended to astronomers, as frequent and careful observations upon it can scarcely fail to develope the law of gravitation in that remote system. Both the stars in α *Capricorni* are double: and that usually designated as α^2, Mr. H. characterizes as one of the most beautiful and delicate objects in the heavens. The introductory remarks terminate with an account of a curious meteorological phænomenon, which occurred on the 19th of April last. The paper concludes with a catalogue of new double stars, arranged in the manner of the author's former catalogue.

There was next read a paper from Mr. Curnin, the Superintendent of the Observatory at Bombay, communicating a regular series of observations of moon-culminating stars, on the several previously arranged days, for the lunations in the months of February, March, April, May, and a few detached observations on similar stars in August and September, in the year 1825. These observations were made with a transit instrument of only two feet focal length, and little more than two inches aperture, placed in a temporary observatory. The deviation of this instrument from the meridian, in the early part of the observations, was nearly one minute in time; but it is not mentioned whether the deviation was easterly or westerly: and Mr. Curnin remarks that the error of its position on each day

is

is stated " till the smallness of the deduced deviation and the op- " posite direction of it, were thought to be the effect of the errors " of observation; and within narrower limits I have not yet attempted " to bring the instrument, as I have not a catalogue on which I could " rely for the mean place of stars that are well adapted for the pur- " pose, nor a time-piece of sufficient regularity to justify my depen- " dence on it, nor a meridian mark to guide me in the execution of " it." It cannot be expected therefore, that observations made under such unfavourable circumstances can throw much light on the practical solution of that difficult problem, the determination of the longitude; where the greatest accuracy is necessary. It is understood, however, that a new set of instruments has lately been forwarded to the Observatory at Bombay, at the expense of the East India Company; and the public therefore may soon expect to reap the benefit of so laudable an exertion of patronage and public spirit.

This was followed by the reading of a paper by Professor Littrow, Director of the Imperial Observatory at Vienna, " On the determination of azimuths by observations of the pole-star." The principal object of this communication is to show that the observations of the pole-star, not merely at the times of its greatest elongation, but at any points in its diurnal revolution, may be advantageously employed in the determination of azimuths. After Mr. Littrow has explained his process of observation he investigates the theorems requisite in the computation. Let t be the mean of the times of observation, T the corresponding sidereal time, the horary angle of the pole-star $s =$ T$-$ apparent Æ, p be the apparent polar distance, ϕ the elevation of the pole, A the arithmetical mean of the azimuths of the star read off the instrument, while O is the mean of the corresponding azimuths of the terrestrial object chosen. Then the theorem for the azimuth ω of the star, at the time of the middle of the observations, is

$$\omega = \frac{p \sin s}{\cos \phi} + \delta \omega + \mathrm{M}\, \delta^2 \omega$$

and the azimuth of the terrestrial object is $\alpha = \omega \pm (\mathrm{A} - \mathrm{O})$.

The determination of the values of $\delta \omega$ and $\delta^2 \omega$, which depend partly upon series, is explained in the paper, as well as the mode of determining them by a small subsidiary table easily computed. The application of the method is then shown by some examples from observations in July last year, in which two distinct results of observations with the face of the circle towards the east and towards the west, agree within half a second.

A communication from G. Dollond, Esq. was then read, giving an account of a singular appearance observed during the solar eclipse on the 29th of November last. The morning was cloudy, but soon after the commencement of the eclipse there was a partial opening in the clouds, through which Mr. D. saw *a considerable part of the limb of the moon which had not yet entered on the disc of the sun.* Continuing his observations, after a short time as the clouds passed on, he again saw both the sun and a portion of the moon's border *which was off the sun's disc.* The sky then became cloudless, and he could no longer discern any part of the moon's limb, except that

that which eclipsed the sun. This unexpected occurrence Mr. D. thinks may be turned to advantage, as it seems to show that the reduction of the sun's light, by the intervention of an opaque substance, may enable an observer to see the moon when she is very near the sun.

There was next read, A letter from Mr. Reeves of Canton, giving an account of a comet seen at sea, Oct. 3d, 4th, 5th, 10th, and 30th, 1825. When it was first observed the ship was in lat. 7° 20′N. Long. 110° 20′ E., the comet was in a line between η *Eridani* and π *Ceti*, about one-third of the distance from the former star. It then passed π *Ceti*, advanced toward σ *Ceti*; and at length on the 30th of October was seen so near to α in the western wing of the Crane, that that star appeared to form the nucleus of the comet. Shortly afterwards the comet entirely disappeared.

Lastly; A letter was read from M. Gambart to the President, containing new elements of the comet which passed across the sun's disc in Nov. 1826, with a more precise determination of the moment of its leaving the disc; from which he concludes that both himself and M. Flaugergues of Viviers must have observed the sun before the comet left it. As, however, neither of them saw the comet, the conclusion drawn is, that it was too small or too rare to be visible in that situation.

XXIX. *Intelligence and Miscellaneous Articles.*

ATOMIC WEIGHT OF NICKEL.

In consequence of a paragraph contained in Dr. Turner's Elements of Chemistry, p. 418, Dr. Thomson has instituted some new experiments to ascertain the atomic weight of nickel. Pure oxide of nickel was obtained from speiss by the following process: It was first dissolved in a mixture of sulphuric and nitric acids; the crystals of sulphate obtained by evaporation contained neither arsenic, iron, bismuth nor antimony, but were contaminated by a little copper and cobalt; the former was precipitated by sulphuretted hydrogen, and the oxide of nickel, precipitated by carbonate of soda, had chlorine gas passed through it while moist, by which the oxide of nickel was dissolved and that of cobalt left: the muriate of nickel thus obtained was then converted into sulphate; it appeared to be absolutely pure, and by analysis it appeared to be composed of

1 atom sulphuric acid	5·
1 atom protoxide of nickel	4·25
7 atoms water	7·876
	17·126

Dr. Thomson did not make any experiments on the peroxide of nickel; but he concludes, as he has before shown, that the atomic weight of nickel is 3·25; and he states his opinion to be, that the Protoxide

	Nickel.		Oxygen.
Protoxide is composed of	3·25	+	1
Peroxide is composed of	3·25	+	1·7

Edinburgh Journal of Science, No. 13, p. 158.

In stating the composition of the peroxide, 1·7 is, I suppose, a misprint; it should be 1·5, which will agree with Dr. Thomson's statement in his "Attempt," vol. i. p. 361. I shall probably offer some observations on the composition of sulphate of nickel in the next number of the Phil. Mag. and Annals.—R. P.

NEW COMBUSTIBLE GAS.

A paper by Dr. Thomson has been read before the Royal Society of Edinburgh on a new inflammable gas. It was obtained from pyroxylic spirit, formed by the distillation of wood, and manufactured by Messrs. Turnbull and Ramsay of Glasgow. Pyroxylic spirit has a specific gravity of 0·812, and an agreeable smell, and is used in lamps instead of alcohol. Dr. Thomson found that the gas extricated from a mixture of aqua regia and pyroxylic spirit, consisted of

New inflammable gas	29
Nitrous gas	63
Azotic gas	8
	100

the specific gravity being 1·945, that of air being 1. The specific gravity of the new gas was 4·1757, and it was composed as follows:

1 atom hydrogen	0·128
1 atom carbon	0·750
$1\frac{1}{2}$ atom chlorine........	6·750
	7·625

Its atomic weight is 7·625. Hence Dr. Thomson calls it the sesquichloride of carbo-hydrogen.—*Ibid.* No. 13, p. 182.

OXIDES OF GOLD.

Dr. Thomson has read another paper before the Royal Society of Edinburgh, entitled some *Experiments on Gold.* The object of this paper was to determine whether the peroxide of gold contained *two* or *three* atoms of oxygen. The evidence from the analysis of Berzelius and Javal was in favour of *three* atoms; and hence chemists had considered the peroxide of gold as a *teroxide.* This result is confirmed by Dr. Thomson, who finds that peroxide of gold is composed of

1 atom gold	25
3 atoms oxygen	3
	28

In this paper Dr. Thomson also determines that the muriate of gold consists of

2 atoms

2 atoms muriatic acid	9·25
1 atom peroxide of gold	28·
5 atoms water	5·625
	42·875

Dr. Thomson then proceeds to show, in opposition to the views of Berzelius, that the permuriate of tin, like the muriate of gold, is more probably a muriate than a chloride.—*Ibid.* p. 182.

OXAHEVRITE—A NEW MINERAL.

This substance was brought from the hot-spring of Oxhaver in the north-east of Iceland; it occurs in petrifactions, in which the wood has been replaced by calcareous spar of a fine ochre-yellow colour, and more or less crystallized; it occurs in thin veins, in amorphous masses, in aggregated groups of crystals, and sometimes in insulated crystals implanted in the calcareous spar.

Dr. Brewster states that the crystals are acute octahedrons, with a square base; the angles at the base are truncated by planes parallel to the axis of the octahedron, and equally inclined to the adjacent sides of the base, so as to form, when enlarged, the faces of a square prism. Dr. Brewster observes a very remarkable property in the crystals of this substance, and which he has never before seen. Every face of the octahedron is a surface of double curvature, in consequence of which the maximum angle of the two opposite faces of the pyramid is 58°, while the minimum angle is 42°, giving a change of inclination of no less than 16°. The maximum inclination occurs at the base, and at the vertex of the pyramid, and the minimum inclination at an intermediate point. The colour of the crystals is light-gray, leek-green, olive-green, and reddish-brown; they are nearly as hard as apatite, sp. gr. 2·218. Their general size is about one-tenth of an inch in length; their surfaces are even, but not brilliant, and the small truncations of the angles at the base of the pyramid are more imperfect than those of the octahedron, the imperfections having the direction of the axis. They cleave with some facility perpendicular to the axis, but in no other direction; the plane of cleavage is considerably rounded, the convex surface being turned towards the apex of the pyramid.

Dr. Turner has analysed this mineral with the following results:

Silica	50·76
Lime	22·39
Potash................	4·18
Peroxide of iron........	3·39
Alumina	1·00
Fluoric acid	a trace.
Water................	17·36
	99·08

Dr. Turner observes, that as the proportions of silica, lime, potash, and water, are so nearly the same as those obtained by Berzelius from the apophyllite and tesselite, it admits of doubt whether the

 iron

ron and alumina, combined perhaps with a little water, are not to be regarded as accidental impurities, rather than as essential parts of the mixture. In this view the oxahevrite would be a variety of apophyllite.—*Ibid.* p. 118.

ON THE IRON CONTAINED IN THE BLOOD.

M. Englehart has shown that when an aqueous solution of the colouring matter of the blood is treated with chlorine, it is decomposed; a flocculent substance is deposited, which is insoluble in water, and which, when separated by the filter, yields all the iron of the colouring matter, by treating it with the commonly employed re-agents.

Mons. H. Rose has repeated and verified the experiments of Englehart. He found, however, that the iron precipitated by ammonia contains a small quantity of phosphate of lime and subphosphate of iron, and also that if excess of ammonia be added without separating the flocculent matter, it is re-dissolved; the liquid becomes of a deep brownish-red colour, and the iron does not precipitate. After a long time it is indeed true that flocculent matter is deposited, but it contains scarcely any iron, almost the whole of it remaining in the ammoniacal solution. It appears, therefore, that the red colouring matter and the flocculent matter derived from its decomposition by chlorine, possess the property of hindering the precipitation of the iron by the alkalies; indeed, a considerable quantity of solution of iron may be added to the colouring matter, without its being precipitated by the alkalies. If the colouring matter be destroyed by chlorine, and the insoluble matter be separated, all the iron may be precipitated by re-agents; but this is not the case if the insoluble matter be left in it. There is however a limit to these facts; for when the iron is in too great proportion to the colouring matter, the iron is then partly precipitated.

M. Rose found that when the serum of human blood, or of the ox or sheep, was mixed with a considerable quantity of solution of iron, it presented the action of the usual re-agents upon the metal. Ammonia and the other alkalies, although added in great excess, produced no precipate: they even re-dissolved that occasioned by the addition of albumen to a solution of peroxide of iron; and neither hydrosulphuret of ammonia, nor tincture of galls, occasions any precipitation in the solution.

In general, peroxide of iron and other oxides are not precipitable by alkalies, when an organic substance soluble in water is added to the solutions, provided it be of such a nature as to be entirely decomposable at a high temperature. On the contrary, when an organic substance soluble in water is totally or mostly volatilized without decomposition, it does not prevent the iron from being precipitated by the alkalies. The first effect is produced in a hot solution of gelatine or starch, in gum arabic, linseed, mucilage, sugar, sugar of starch and of diabetes; the pectic, kinic, malic, citric, and tartaric acids; in fact, this last acid has long been known to produce this effect. The contrary property was found in the following acids;

acids; viz. the oxalic, acetic, formic, pyro-tartaric, pyrocitric, pyromucic, succinic, benzoic, &c., and by alcohol and sulphuric ether. —*Annales de Chimie et de Physique*, tome xxxiv. p. 268.

COMPOSITION OF NATIVE ARGENTIFEROUS GOLD.

M. Bousingault having had occasion to examine a great number of samples of argentiferous gold found in Colombia, has observed that these metals are united in definite proportions; his process consisted simply in dissolving the specimen in nitromuriatic acid, separating the chloride of silver, and then precipitating the gold in its metallic state by protosulphate of iron. Mons. B. remarks that hitherto he has met with one atom of silver combined with 2, 3, 5, 6, and 8 atoms of gold; but as he reckons the weight of an atom of silver at double that which is usually admitted, these views will require modifying; and this we shall take the liberty of doing, in giving the results of his analysis.

Native gold of Marmato, near Vega de Sapia in the province of Popayan, sp. gr. 12·666.

	By analysis.		By theory.
Gold	73·45	3 atoms gold	73·17
Silver	26·48	2 atoms silver ..	26·83
Loss	00·07		100·00
	100·00		

Native gold of Titiribi.

	By analysis.		By theory.
Gold	74	3 atoms gold	73·17
Silver	26	2 atoms silver ..	26·83
	100		100·00

Native gold of Malpaso, near Mariquita, sp. gr. 14·706.

In analysing this and the remaining specimens, cupellation was employed instead of aqua regia.

	By analysis.		By theory.
Gold	88·24	4 atoms gold	87·9
Silver	11·76	1 atom silver	12·1
	100·00		100·0

Native gold of Rio Sucio, near Mariquita.

	By analysis.		By theory.
Gold	87·94	4 atoms gold	87·9
Silver	12·06	1 atom silver	12·1
	100·00		100·0

Native gold of Otra-Mina, near Titiribi,—crystallized in octahedrons.

	By analysis.		By theory.
Gold	73·4	3 atoms gold	73·17
Silver	26·6	2 atoms silver....	26·83
	100·0		100·00

Native gold from the mine of Guamo, near Marmato.

	By analysis.		By theory.
Gold	73·68	3 atoms gold	73·17
Silver	26·32	2 atoms silver....	26·83
	100·00		100·00

Native gold of El Llano; in small flattened grains, with a peculiar red colour, and hence called Oro colorado.

	By analysis.		By theory.
Gold	88·58	4 atoms gold	87·9
Silver	11·42	1 atom silver	12·1
	100·00		100·0

Native gold of La Baja, near Pamplona.

	By analysis.		By theory.
Gold	88·15	4 atoms gold	87·9
Silver	11·85	1 atom silver	12·1
	100·00		100·0

Native gold of Ojas-anclias, from an alluvial mine in the province of Antioquia. (It is in leaves of a yellowish red colour.)

	By analysis.		By theory.
Gold	84·5	3 atoms gold	84·5
Silver	15·5	1 atom silver	15·5
	100·0		100·0

Native gold of Transylvania; in very pale cubic crystals.

	By analysis.		By theory.
Gold	64·52	1 atom gold	64·5
Silver	35·48	1 atom silver ..	35·5
	100·00		100·0

This is the *Electrum* of Klaproth, whose analyses gave Gold 64, Silver 36.

Native gold of Santa-Rosa de Osos, province of Antioquia, sp. gr. 14·149. (Colour pale, with a green tint.)

	By analysis.		By theory.
Gold	64·93	1 atom gold	64·5
Silver	35·07	1 atom silver	35·5
	100·00		100·0

LIST OF NEW PATENTS.

To T. Don, of No. 9, Lower James-street, Golden-square, mill-wright, and A. Smith, of No. 28, Wells-street, Oxford-street, builder, for their methods of making shutters and blinds of iron or steel, or any other metals or composition thereof, and improved methods of constructing and fixing shutters and blinds of iron or steel or any other metals or materials and methods of uniting in shutters

shutters the double properties of shutters and blinds.—Dated the 15th of June 1827.—2 months allowed to enrol specification.

To L. Dexter, of King's Arms yard, Coleman-street, London, esquire, for improvements in machinery, communicated from abroad, for the purpose of spinning wool, cotton, and other fibrous substances.—16th of June.—6 months.

To Rear-Admiral Henry Raper, of Baker-street, Mary-le-bone, for a new system of signals; first, for communicating by day, by means of flags, &c. in which system the colours of the flags which have heretofore served to distinguish the signals, and are subject to be mistaken, may be dispensed with; and secondly, for communicating by night, by means of light, and which system of signals is more conspicuous, expeditious and certain than any hitherto employed.—21st of June.—2 months.

To Lieut. James Marshall, of Chatham, Kent, for improvements in mounting guns or cannon.—26th of June.—6 months.

To John Felton, of Hinkley, Leicestershire, for a machine for an expeditious and correct mode of giving a fine edge to knives, razors, scissors, and other cutting instruments.—28th of June.—2 months.

To Thomas Fuller, of Bath, for improvements on wheel carriages.—28th of June.—2 months.

To Walter Hancock, of Stratford, Essex, for his improvements on steam-engines.—3d of July.—6 months.

To William Wilson, of Martin-lane, Cannon-street, for extracting spirits and other solvents used in dissolving gums and other articles employed for stiffening hats, &c. and converting such spirit (after rectification) into use.—4th of July.—2 months.

To René Florentin Jenar, of Bunhill-row, for improvements in lamps.—4th of July.—6 months.

To George Poulton, of Stafford-street, Old Bond-street, for an instrument for writing, which he denominates a self-supplying pen.—4th of July.—6 months.

To Thomas Sowerby, of 'Change Alley, Cornhill, for improvements in the construction of ships' windlasses.—4th of July.—2 mon.

To René Florentin Jenar, of Bunhill-row, for his method of filling up with metal or other suitable material the holes or interstices in wire, gauze, or other similar substances, which he denominates metallic linen.—4th of July.—6 months.

To John Snelton Shenton, of Husband Bosworth, Leicestershire, for improvements in the mechanism of water-closets.—12th of July.—2 months.

To Edward Barnard Deeble, of St. James's-street, Westminster, for a new construction and combination of metallic blocks for forming caissons, jetties, piers, quays, embankments, light-houses, foundations, walls, &c.—12th of July.—6 months.

To Robert Vazie, of York-square, St. Pancras, Middlesex, for improvements in processes, utensils and apparatus, applicable to the preparing, extracting, and preserving various articles of food, the component parts of which are of different dimensions proportionate to their uses.—12th of July.—6 months.

METEORO-

METEOROLOGICAL OBSERVATIONS FOR JUNE 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·33 June 9. Wind N.—Min. 29·64 June 2 & 29. Wind SW.
Range of the mercury 0·69.
Mean barometrical pressure for the month . . . . 29·958
Spaces described by the rising and falling of the mercury . . . 4·080
Greatest variation in 24 hours 0·320.—Number of changes 26.

Therm. Max. 75° on three diff. days.—Min. 45° June 6 & 7. Wind NW.
Range 30°.—Mean temp. of exter. air 60°·75. For 31 days with ☉ in ♊ 59·79
Max. var. in 24 hours 24°·00—Mean temp. of spring water at 8 A.M. 51°·35

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the morning of the 1st . . . . 82°
Greatest dryness of the air in the afternoon of the 23rd 36
Range of the index 46
Mean at 2 P.M. 51°·4—Mean at 8 A.M. 58°·0—Mean at 8 P.M. 61·3
—— of three observations each day at 8, 2, and 8 o'clock . . 56·9
Evaporation for the month 3·00 inch
Rain near ground 1·66 inch.—Rain 23 feet high 1·525 inch.
Prevailing Wind S.W.

Summary of the Weather.

A clear sky, 4; fine, with various modifications of clouds, 14; an overcast sky without rain, 7; rain, 5.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
17	11	25	0	25	23	15

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1½	5	½	1½	½	13½	1	6½	30

General Observations.—This month has been rather dry, with intervals of refreshing showers and brisk gales of wind. The thermometer in the shade has not yet risen to summer heat; and the first seven nights were cold, and blighted much of the young fruit.

The mean temperature of the external air this month is about one-third of a degree under the mean of June for the last eleven years.

The changes in the mercurial column, though not very great, are many for June: and the continuation of the wind from the S.W. the last fortnight was remarkable. At twenty minutes before 12 P.M. on the 9th instant, an unusually large meteor fell obliquely towards the earth from an altitude of about 15 degrees in the N.E. part of the horizon: it was nearly the apparent size and colour of the full moon, which was then shining a few degrees eastward of the meridian.

Between five and six o'clock in the morning of the 22nd, two *parhelia* appeared, one on each side of, and each 22° 40′ distant from, the sun's centre. The first was observed on the northern side of the sun in a thick *Cirrus,* which was descending to the horizon and passing to a *Cirrostratus:* it had a whitish train ten or twelve degrees long, which was even perceived through a thin cloud that was passing under it. When this parhelion had nearly

nearly disappeared, the other began to form nearly in the prime vertical (East) in a long horizontal plumose *Cirrus*, which was changing from a silvery colour to a light brown; it was more beautifully adorned with prismatic colours than the first, as only one perceptible stratum of cloud was in its vicinity, and it reflected a deep red, light yellow and light blue from its circular part, and showed a whitish horizontal train the same length as the first. They alternately appeared and disappeared as the vapours became more dense or more attenuated, till the latter had descended beneath the rising sun. A faint solar halo was traced at intervals within the parhelia.

In the evening of the 2nd, *three* winds prevailed at the same time: a fixed mark on the top of a house, and two strata of cloud, the upper one moving from S.S.E. and the under one from N. by W., determined the two upper winds; and the lowest from the West was pointed out by a lofty vane: indeed, during the month the clouds and vane showed that the winds were often either at right angles, or opposite to each other.

The wheat in this neighbourhood came into ear the first week of this period, and now looks promising, although there will be much straw, for fair and early crops. Hay-making commenced here about the 15th, with dry weather, but not hot sunshine, from the frequent interposition of clouds. The quantity of hay will not be so great as was generally expected and anxiously wished for, in consequence of its failure last year; for the year so far has been comparatively dry: however, there are good bottom roots for a second cutting, should the weather prove favourable to its growth.

Insects of almost every description were, perhaps, never more numerous, nor more destructive to vegetation, &c. than at present.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two *parhelia*, four solar halos, three meteors, light thunder during the morning of the 14th; and five gales of wind, namely, three from the S.W., and two from the N.W.

REMARKS.

London.—June 1—3. Showery. 4. Cloudy: with showers. 5. Showery. 6. Cloudy: with showers. 7. Fine: a shower 6 P.M. 8—14. Fine. 15. Overcast. 16. Cloudy. 17—20. Fine. 21. Cloudy: a heavy shower of rain at 2 P.M. 22. Showery. 23—26. Fine. 27. Cloudy. 28. Rainy. 29. Cloudy and fine. 30. Cloudy.

Boston.—June 1. Rain. 2. Cloudy: rain A.M. and P.M. 3. Fine. 4—5. Cloudy: rain P.M. 6. Cloudy: heavy storm with rain and hail, P.M. 7, 8. Fine. 9, 10. Cloudy. 11—13. Fine. 14—17. Cloudy. 18. Fine. 19. Fine: rain P.M. 20—22. Fine. 23, 24. Cloudy. 25, 26. Fine. 27. Cloudy. 28. Rain. 29. Fine: rain A.M.

Penzance.—June 1. Fair, misty. 2. Fair: showers, 3, 4. Fair. 5. Rain: clear. 6—8. Fair. 9—12. Clear. 13. Fair. 14. Thunder showers. 15. Showers. 16. Misty. 17. Fair. 18. Clear. 19. Fair. 20. Fair: clear. 21. Showers. 22—25. Clear. 26. Fair. 27. Fair: rain. 28. Rain. 29, 30. Clear.

RESULTS.

London.—Winds, NE. 6: E. 1: SE. 1: S.W. 10: W. 1: N.W. 11.

Barometer: Mean of the month	30·087 inch.
Thermometer: Mean of the month	60·950°
Evaporation	3·74 inch.
Rain	0·75 inch.

Meteoro-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.										
June 1	29·86	29·83	29·56	29·48	29·80	29·71	29·13	67	48	61	52	64	53	56	SW.	W.	SW.	W.	...	...	0·05	...	0·160	...
» 2	30·00	29·83	29·53	29·48	29·67	29·64	29·25	64	44	58	53	62	46	57	SW.	W.	SW.	W.	...	...	·06	...	·100	...
3	30·07	30·00	29·70	29·68	29·92	29·88	29·35	66	46	58	50	62	48	57·5	NW.	W.	W.	W.	...	0·20	...	...	·260	0·12
4	30·07	30·00	29·79	29·74	30·00	29·95	29·45	68	52	59	48	66	50	56	NW.	NW.	NW.	W.	...	...	...	...	·025	...
5	30·00	29·82	29·65	29 61	29·92	29·68	29·33	67	50	59	49	60	47	54	SW.	NW.	SW.	W.	...	...	·08	...	·055	·04
6	30·11	29·82	29·75	29·70	29·88	29·73	29·10	62	42	57	49	62	45	54	NW.	NW	NW.	W.	...	·35	·02	...	...	·05
7	30·36	30·11	29·84	29·83	30·08	30·00	29·50	64	36	59	49	64	45	53·5	NW.	NW	NW.	NW.	...	...	·01	...	...	·43
8	30·45	30 36	30·00	29·90	30·28	30·22	29·72	78	40	62	54	64	50	57·5	NW.	N.	S.	W.	0·92	...	...	0·200	...	...
○ 9	30·45	30·43	30·04	30·03	30 33	30·30	29·80	79	41	68	51	74	55	62·5	NE.	NE.	N.	calm	...	·30	...	...	...	...
10	30·43	30·33	30·02	29·96	30·30	30·22	29·80	80	43	71	57	72	52	58	NE.	NE.	NE.	calm	...	...	...	...	...	...
11	30·33	30·25	29·93	29·90	30·20	30·12	29·73	77	44	69	56	73	52	63·5	NE.	NE.	NE.	SW.	...	...	...	...	...	...
12	30·25	30·24	29·90	29·86	30·12	30·11	29·66	78	48	69	54	71	53	61·5	NE.	NE.	NE.	NE.	...	·50	...	...	...	...
13	30·24	30·09	29·80	29·70	30·07	29·98	29·70	79	49	68	56	71	56	60	NE.	SE.	NE.	E.	...	...	...	...	...	...
14	30·09	29·92	29 60	29·54	29·87	29·80	29·55	78	43	60	56	75	58	57	NE.	SE.	NE.	E.	·95	...	...	0·720	·080	...
15	29·92	29·90	29·50	29·50	29·72	29·72	29·36	79	56	60	56	75	58	56	E.	SE.	SE.	NE.	...	·30	...	...	·020	...
☾ 16	29·96	29·92	29·52	29·50	29·75	29·72	29·25	78	56	64	56	69	57	60·5	SW.	SE.	SW.	calm	...	...	...	0·130	...	...
17	30·10	29·96	29·60	28·96	29·90	29·81	29·28	77	58	65	56	74	55	65	NW.	NW.	SW.	calm	...	...	...	...	...	...
18	30·10	30·06	29·74	29·30	30·00	29·96	29·42	78	54	64	55	73	53	69	W.	NW.	SW.	W.	...	·15	...	...	...	...
19	30·06	30·00	29·74	29·70	29·99	29·92	29·35	72	52	67	53	73	54	64·5	NW.	W.	SW.	W.	...	...	...	...	...	...
20	30·04	30·00	29·70	29·68	29·90	29·88	29·30	73	46	62	49	69	49	57	NW.	W.	SW.	NW.	...	...	...	...	...	...
21	30·15	30·04	29·70	29·68	29·96	29·94	29·40	72	46	62	48	67	49	57	SW.	W.	SW.	NW.	...	·35	·11	...	·025	...
22	30·24	30·15	29·86	29·84	30·10	30·08	29·50	68	44	62	49	66	49	57·5	NW.	NW.	SW.	NW.	...	...	...	...	·055	...
23	30·25	30·24	29·92	29·92	30·23	30·15	29·60	69	43	61	49	68	49	56	NW.	N.	NW.	W.	·98	...	...	0·190	...	...
● 24	30·25	30·25	29·92	29·92	30·15	30·13	29·65	73	53	64	51	69	51	59	NW.	NE.	N.	calm	...	·30	...	...	...	...
25	30·25	30·19	29·91	29·90	30·15	30·11	29·64	77	45	64	50	70	54	60	SE.	NW.	NW.	calm	...	...	...	...	...	...
26	30·19	30.10	29·88	29·70	30·08	30·05	29·55	78	55	66	51	75	57	61	SW.	S.	SW.	W.	...	...	...	...	...	...
27	30·10	29·82	29·66	29·50	29·97	29·80	29·40	72	58	66	56	73	59	61·5	SW.	SW.	S.	NW.	...	·40	·13	...	·370	...
28	29·82	29·81	29·40	29·40	29·71	29·68	29·10	72	58	66	54	68	59	60	SW.	SW.	SW.	NW.	...	...	·23	0·600	·400	·53
29	29·92	29·82	29·48	29·40	29·76	29·64	29·06	77	55	67	54	70	56	65	SW.	W.	SW.	W.	...	...	...	...	·055	·21
30	29·95	29·92	29·60	29·54	29·90	29·83	29·24	76	54	68	56	70	57	64	SW.	NW.	SW.	NW.	·89	·15	·06	0·320	·055	...
Aver. :			30·04	29·40	30·33	29·64	29·43			71	48	75	45	59·3						3·00		2·160	1·660	

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

SEPTEMBER 1827.

XXX. *On the Figure of Equilibrium of a Homogeneous Planet in a Fluid State; in reply to the Observations of* M. Poisson, *published in this Journal for July last. By* J. Ivory, *Esq. M.A. F.R.S.**

THE two letters which I have addressed to Professor Airy in the last Numbers of this Journal, fully ascertain the nature and extent of the analytical method employed by Laplace in the investigation of the figure of the planets. I now proceed to the conditions which I have found to be necessary for solving the problem of a homogeneous mass of fluid revolving upon an axis, to which M. Poisson objects. The subject is one of considerable importance; it involves a capital question in the system of Newton, towards the solution of which nothing has in reality been added since the time of Maclaurin and Clairaut. This is the more unaccountable, because, in the long interval elapsed, the problem has continued to occupy the attention of all the great mathematicians; the difficulties attending the computation of the attractive forces, which embarrassed the first inquirers, have been overcome by the progress of mathematical science; and we are in possession of a theory of the equilibrium of fluids, which is supposed to be exact and complete. Yet these accessions to our knowledge have been made not only without advancing the problem of the figure of the planets, but even without reaching what had previously been investigated by a more simple geometry. What reasons can be assigned for this total inefficiency? Any attempt to elucidate this matter, and to detect

* Communicated by the Author.

any misapprehension or error that may have impeded the progress of this branch of mechanical philosophy, is at least deserving of serious attention, and ought not to be fastidiously thrown aside merely because it is opposed to what has received the sanction of great names.

In what follows, homogeneous fluids only are understood; and it is supposed that the accelerating forces which urge the particles, are expressed by functions of the coordinates that satisfy the criterion of integrability. It will also be proper to notice a distinction depending on the nature of the accelerating forces, inattention to which has been the cause of no little perplexity in this theory. The forces mentioned may be either explicitly given, so as to be entirely and absolutely known when we know the coordinates of a molecule of the fluid upon which they act; or the same forces may be relative to the unknown figure of the mass of fluid.

If the particles of a fluid are urged by attractions to fixed centres and by centrifugal forces, the problem of equilibrium falls under the first division. In such cases all the level surfaces have the same differential equation, and they are different only in the different pressures they sustain. When any one of those surfaces is known, all the rest are derived from it merely by varying the pressure, and the figure of the whole mass *in equilibrio*, is necessarily determined. At the outer surface of the fluid there is no pressure; and, therefore, when the outer surface satisfies the differential equation common to all the level surfaces, or, which is the same thing, when the resultant of the accelerating forces urging the particles is perpendicular to that surface, all that is required for solving the problem is fulfilled.

When the particles of a fluid attract one another, the accelerating forces urging them will depend upon the place they occupy and upon the figure of the whole mass. As the attractive forces upon points differently situated must vary, all the level surfaces will not, in this case as before, have the same differential equation. These surfaces will now vary from one another on two accounts; namely, the different pressures they sustain, and the variation of the attractive forces according to the situation of the surfaces in the mass. It appears, therefore, impossible in this case to establish any general relation between the several level surfaces without the special consideration of the figure of the fluid. We cannot infer that all the level surfaces will exist in the interior of the fluid, and will satisfy, each its peculiar equation, merely because gravity acts in directions perpendicular to the outer surface. There

appears

appears to be no absurdity in supposing that the latter condition may be fulfilled, at the same time that no level surfaces exist in the interior, and consequently when there is no equilibrium of the mass of fluid.

It has been shown that there is a plain distinction between a fluid *in equilibrio* when the accelerating forces acting upon the particles are explicitly given, and when the same forces depend upon the figure of the mass. Yet, according to the received theory there is no difference in the two cases with respect to the conditions necessary to insure the equilibrium. Supposing that the algebraic expressions of the forces possess the criterion of integrability, nothing further is required in either case, than that the outer surface of the fluid be a level surface cutting the direction of gravity everywhere at right angles. But if the determination of the equilibrium be the same in both cases, we might expect that the demonstration would be different in circumstances so essentially distinguished. It must be confessed, however, that we meet with nothing in the shape of demonstration, except what is vague and unsatisfactory and applies alike to both cases. The principle seems to be this: granting that the exterior surface is a level surface, it is always possible to trace the level surfaces in the interior; because, since there is no distinction of density, we may adopt any surfaces we please as level surfaces*. When the accelerating forces are explicitly given, there is no difficulty nor ambiguity; because the equation of all the level surfaces are nowise different except in the constants introduced by integration, which vary from one surface to another. But the case is not the same when the level surfaces depend upon the figure of the fluid. Taking a point in the interior of the mass, we may indeed conceive a surface to pass through it and to be extended on all sides, so as everywhere to cut the resultant of the accelerating forces at right angles, and we may call this a level surface: but we may thus fall into error; because it is not clear that the surface so traced will return into itself and completely inclose a portion of the fluid; and without this it would neither be a level surface, nor would there be an equilibrium of the fluid. In order to place the theory on a solid foundation, such gratuitous assumptions must be set aside; and it must be proved, by means of the equations which accurately determine the level surfaces, that they do necessarily exist in the interior of the fluid, and produce the equilibrium in question. But this has not been done; and, to say

* *Mécan. Célestc*, Liv. 1re, No. 17; & Liv. 3^{me}, No. 22.

the least, it is very doubtful whether it can possibly be accomplished.

Being dissatisfied with the usual equations for finding the figure of a homogeneous planet in a fluid state, I have sought to deduce a solution of the problem by a strict analysis, without admitting any gratuitous assumption. By analysis I do not here mean an algebraic calculation; I understand by it a process of reasoning such as the ancient geometers employed to derive the construction of a geometrical problem from the conditions to be fulfilled. In the following investigation I confine my attention to a homogeneous planet in a fluid state; that is, to the equilibrium of a fluid mass of uniform density, the particles attracting one another inversely as the square of the distance, and being urged by a centrifugal force caused by rotation about an axis that passes through the centre of gravity; and in order that the train of reasoning may be more easily examined, I divide it in distinct propositions.

Prop. 1.—If two particles be similarly placed, in two bodies, exactly similar in their figure and composed of the same homogeneous matter; the attractive forces of the bodies upon the particles will act in similar directions, and will be proportional to the linear dimensions of the bodies.

Particles here mean infinitely small portions of the two bodies proportional to the whole masses. The proposition is proved, *Prin. Math.* lib. i. Prop. 87.—*Maclaurin's Fluxions,* § 629.

Prop. 2.—If a homogeneous mass of fluid revolving upon an axis be *in equilibrio* by the attraction of its particles in the inverse proportion of the square of the distance; any other mass of the same fluid having a similar figure and revolving with the same rotatory velocity about an axis similarly placed, will likewise be *in equilibrio,* supposing that its particles attract one another by the same law.

Take two particles similarly situated in the two bodies. By Prop. 1. the resultants of the attractive forces acting on the particles have similar directions, and are proportional to the linear dimensions of the bodies. Further, the centrifugal forces urging the particles to recede from the axes of rotation, are proportional to the respective distances from those axes, that is, to the linear dimensions of the bodies. Wherefore the joint effect of all the forces is to urge the particles in similar directions with intensities proportional to the linear dimensions of the bodies. And as the same thing is true of all particles similarly situated in the two bodies, it follows that if there be an equilibrium in one case, there will likewise be an equilibrium

equilibrium in the other: for the forces which urge the particles of one body are different in no respect from the forces which urge the particles of the other, except in being all increased, or all diminished, in the same given proportion *.

Prop. 3.—If a homogeneous mass of fluid revolve about an axis which passes through the centre of gravity, and be *in equilibrio* by the attraction of its particles in the inverse proportion of the square of the distance; any surface in the interior, similar to the outer surface and similarly posited about the centre of gravity, will be a level surface.

For, by the hypothesis, the whole mass of fluid and the portion of it bounded by the interior surface are similar in their figures; and they both revolve in the same time about the common axis which cuts them similarly: wherefore the first of these two bodies being *in equilibrio*, it follows from the last proposition, that the other would likewise be *in equilibrio* if it revolved by itself, the exterior matter being taken away or annihilated. Thus the interior fluid body is *in equilibrio* in two different states: first, when it revolves by itself, in which case the only forces in action are, the attraction of its particles and their centrifugal force; and secondly, when it is a part of the whole fluid mass, in which case there is superadded to the former forces, the action of all the exterior matter. Now these two states of equilibrium cannot consist with one another, if we suppose that the exterior matter has any other effect than to produce an equable pressure upon the surface of the interior body; that is, unless the same surface be a level surface.

* In the *Annales de Physique et de Chimie*, tom. xxvii. p. 234, M. Poisson makes use of a very curious argument with respect to this proposition:—"Proposition dont l'inverse ne serait pas vraie; car on sait que pour une même densité et une même vitesse de rotation, qui ne dépasse pas une certaine limite, il y a deux ellipsoïdes dissemblables qui satisfont à l'equilibre d'une masse fluide." Now all this is quite beside the purpose. There is no question about inverse propositions. The theorem as I have laid it down is undoubtedly true; and it may therefore be legitimately used in deducing from the existence of the equilibrium the essential properties that belong to it, and without which it cannot subsist. M. Poisson, not adverting that my reasoning is as much an analysis as if it had been expressed by algebraic equations, goes on to say, that the new condition is not necessary to the equilibrium. Now I have here proved that the equilibrium cannot take place without it, and that the usual condition is by itself insufficient.

The argument in p. 235 is equally inapplicable. The case mentioned is one in which the accelerating forces urging the particles is independent of the figure of the fluid: it is one in which the forces are explicitly given, and is therefore attended with no difficulty.

By means of such loose argumentation any point may be proved or disproved at pleasure.

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The foregoing propositions, which have been deduced by analytical reasoning from the single hypothesis that there is an equilibrium, contain essential properties, without which the equilibrium cannot subsist in any possible figure of the fluid mass. Another property is now to be added, no less essential, no less independent of all considerations relative to a particular figure, and equivalent to the new condition employed for finding the figure of a homogeneous planet, to which M. Poisson has objected.

Prop. 4.—Suppose that a homogeneous mass of fluid, revolving about an axis which passes through the centre of gravity, is *in equilibrio* by the attraction of its particles in the inverse proportion of the square of the distance; take any point in the interior of the fluid, the distance of which from the centre of gravity is equal to r, and let $V(r)$ denote the sum of all the molecules of the whole mass divided by their respective distances from the assumed point, and $V'(r)$ the like sum extending only to all the molecules within the level surface passing through the same point: then is $V(r) - V'(r)$ a constant quantity for all points of the same level surface.

Let ϕ denote the centrifugal force at the distance $'$ from the axis of rotation, and θ the angle which r makes with the same axis: then, as is well known to all geometers, thee equation of the interior level surface will be,

$$V(r) + \phi \times \frac{r^2}{2} \sin^2 \theta = C, \qquad (1)$$

C being constant for all points of the same level surface, and varying from one level surface to another. But, according to the second and third propositions, the portion of fluid bounded by the interior level surface would be *in equilibrio* without any change of its figure, if it revolved by itself; in which case the equation of the surface would be,

$$V'(r) + \phi \times \frac{r^2}{2} \sin^2 \theta = C'. \qquad (2)$$

Wherefore, by subtracting the two equations, we get,

$$V(r) - V'(r) = C - C', \qquad (3)$$

which is the property to be proved, because C and C$'$ are the same for all points of the same level surface.

The property just investigated and the equation of the outer surface of the fluid are together sufficient for determining the figure of a homogeneous planet. But without the property mentioned, the equilibrium would not take place. For nothing follows from the equation of the outer surface of the fluid alone, except that the equation (2) would be true of all the interior surfaces similar to the outer one and similarly posited about the

the centre of gravity. Now these surfaces are independent of all the matter exterior to them; and therefore they are not level surfaces, and the mass of fluid is not *in equilibrio*. But when the equations (2) and (3) exist at the same time, we thence derive the equation (1), which is the true equation of the level surfaces, and establishes the equilibrium.

It appears that $V(r) - V'(r)$ is the pressure upon the assumed point in the interior of the fluid. The pressure therefore is wholly determined by the figure of equilibrium, and has no immediate dependence upon the centrifugal force; and it is easy to see that this must be the case. For there is no centrifugal force at the points where the axis of rotation traverses the several level surfaces: at these points, therefore, the centrifugal force has no influence on the pressure; and, as the pressure is the same over the whole of every level surface, it follows that it is independent of the centrifugal force in all the interior of the fluid.

The similarity of the interior surfaces and the property contained in the fourth proposition, determine the figures with which the equilibrium is possible; and it is found that they can only be ellipsoids: the equation of the outer surface ascertains the rotatory velocity that must take place in every particular figure. We are therefore entitled, upon undoubted evidence, to establish the following proposition:

Prop. 5.—A homogeneous mass of fluid consisting of particles that attract one another inversely as the square of the distance, and revolving upon an axis passing through the centre of gravity, cannot be *in equilibrio* nor maintain a permanent figure, unless it have the figure of an ellipsoid.

The whole of the preceding reasoning may be brought within a narrow compass. If a homogeneous planet in a fluid state be *in equilibrio*, all the level surfaces must be similar to one another: but the ellipsoid is the only figure in which the level surfaces are all similar; wherefore the planet must have the figure of an ellipsoid. All this, it is presumed, is here proved by exact reasoning. On the other hand, the usual theory is grounded on an assumption without proof. If the outer surface of a homogeneous mass of fluid be a level surface, it is assumed, for no other reason than that there is no distinction of density, that all the level surfaces in the interior do necessarily exist and produce an equilibrium. But, if there be no distinction of density, there is a general equation belonging to the interior level surfaces; and, in order to prove the existence of these surfaces, on which the equilibrium depends, it should be shown that their equation is a necessary consequence of that of the outer surface. But this has not been

been done, except in the case when the accelerating forces urging the particles are explicitly given.

The foregoing solution of the problem is essentially the same with that contained in my paper, Phil. Trans. 1824. M. Poisson has objected to my conditions of equilibrium in an article printed in the *Annales de Physique et de Chimie*, tom. xxvii. p. 225, which I have not examined till very lately, since the publication of his remarks in this Journal for July last. If his objections have not been noticed here, care has been taken to place the subject in such a point of view as to avoid their force. Of his arguments, when they are not chargeable with insufficient reasoning,—some do not apply to my theory, and some are not inconsistent with it. But the length of what I have written obliges me to postpone my further remarks on this subject to a future occasion.

Aug. 6, 1827. JAMES IVORY.

[To be continued.]

XXXI. *Collections in Foreign Geology.*—[No. III.] *By* H. T. DE LA BECHE, *Esq. F.R., L., and G.S. &c. &c.*

[Continued from page 109.]

9. *Introduction to the Mineral Geography of Sweden; by* M. Hisinger*.

THE Scandinavian Peninsula, extending from S.S.W. to N.N.E. from the southern point of Scania to Cape North, is cut into a multitude of gulfs (*Fiordar*) on its western side, and traversed by a long chain of mountains (*Fiellrygg*) from Lindesnæs in Norway to the Frozen Ocean, approaching nearer to the North Sea than the Baltic; so that its western side is very steep, whilst towards Sweden the land falls in a very gradual manner. The southern part of this chain is named Langfield; the middle portion Dovrefield. The latter obliquely cuts the Peninsula, and is prolonged to above lake Oresund, at the point where the Herjedal and Jemtland abut on Norway. The Scandinavian Alps may thus be divided into three or four portions, which are united in such a man-

* From a notice in Baron de Férussac's *Bulletin des Sciences*, for March 1826, of a German translation in Leonhard's *Zeitschrift für Mineralogie*.

For excellent descriptions of the primitive rocks of the northern part of our island, which (with similar rocks in Ireland) may be considered as the geological prolongation of the Scandinavian Peninsula, consult Dr. Macculloch's Western Islands, Classification of Rocks, and papers in the Geological Transactions. For those of the E. of Ireland, the memoir of Mr. Weaver, in the Geol. Transactions.—*Trans.*

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ner that each is prolonged below the point where it joins the other. The great breadth of the southern part of this chain is remarkable, extending from 8 to 10 leagues, the top of which is nearly flat. The passes rise for the greater part from 2000 to 3000 feet above the level of the sea. M. Hisinger mentions the exact height of many of these places, as well as of the summits which rise above this vast chain, the highest of which is 7100 feet. He afterwards enters into details relative to the position of the lakes, the direction of the rivers, and all that respects the exterior configuration and natural divisions of the country, and then proceeds to examine its geology.

Primitive Rocks.—The principal rocks, which as it were form the base of the Scandinavian mountains, are gneiss and granite. The former, that most commonly met with, is more solid than slaty; it is often granitoïdal, here and there passing into common gneiss and granite. That these two rocks are of contemporaneous formation in the North, is clearly seen by their alternation, and the frequent passages of one into the other, though gneiss occupies by far the greatest extent of country. Granite (properly so called) is nevertheless found in considerable abundance between Kautokeino in northern Lappmark, and the frontiers of Sweden, near the river Muonio, in the parishes of Nas, Jarna, Aeppelbo, and Yttamalung, in Dalecarlia; in some districts of Upland, Westmania, and Ostrogothia; on the coast of the Baltic, N. of Calmar, and on the road from Alhem to Wimerby and Ingatorp; and between Saby and Grenna, in the government of Jonkoping. Granite is seen, but much less developed, in the great chain of Scandinavian Alps, in the valley of Nea; below Sylfiell and Eckordorr; near Skarfan, in the pass of Skarfdorr; at the southern extremity of lake Wiggelg, on the confines of Sweden and the Herjedal. The gneiss often occurs with a well-characterized slaty structure, as in the environs of Stockholm and Trollhætta, but most frequently with a granitic structure, and then forms low *plateaux*, and low hills in the plains of Upland, Westmania, Ostrogothia, and Westrogothia. Among the minerals found in the gneiss, dark-green hornblende and garnet are the most commonly met with. A granatiferous gneiss of great beauty occurs near the town of Huddikswall. Small veins of quartz, felspar, granite, and compact trap, are occasionally observable in this formation. Black tourmaline and many other rare minerals are sometimes found in the granitic veins, as in the environs of Finbo, Fahlun, Ytterby, and Waxholm. Gadolinite and sphene are often disseminated in the gneiss.

The great variety of rocks subordinate to this formation in the northern countries is remarkable. Besides mica slate, granular limestone, compact felspar, talcose and hornblende rocks, the gneiss contains the greater portion of the Swedish iron, copper, and argentiferous lead mines. It often passes into mica slate in the vicinity of these beds, the felspar suddenly ceasing, and being replaced by a greater quantity of mica, chlorite, hornblende, &c. The ores often occur in masses of considerable bulk.

Mica slate never occurs in the plains or low hills, except in beds subordinate to the gneiss; it commonly forms the most elevated summits of the great chain of Scandinavian Alps, and contains beds of metalliferous substances, limestone, &c. The other rocks, such as clayslate, limestone, talcose slate, compact felspar, and diorite, scarcely form any thing but subordinate beds in the gneiss or mica slate. Diorite and compact felspar alternate with gneiss in Smoland; talcose slate with micaslate in Jemtland; and with gneiss in the parishes of Hallefors and Grythytta, in Westmania. Dark-gray compact limestone is found in subordinate beds in the clayslate of Jemtland. On some of the Smoland heights, near the church of Saaby, and in the environs of Villkjol, a compact felspar occurs, containing small crystals of the same substance, thus forming a porphyry; it is however of inconsiderable extent.

Granular limestone is the most common and abundant rock of all those subordinate to the gneiss. As in the vicinity of the metalliferous situations, the gneiss sometimes passes into mica slate in those parts which approach the limestone; the latter is sometimes a pure carbonate of lime, at others it is mixed with the carbonates of manganese, iron, and magnesia. Among the numerous minerals disseminated in it, we may particularly distinguish spinelle in some beds in Sudermania; hornblende, sahlite, garnet and compact felspar are the substances which most commonly accompany the limestone. Galena, copper, and iron are sometimes found in it; as in the environs of Sahla, Tunaberg, Haakansboda, Langbanshytta, &c.

As far as respects the interior structure of the mountains, or the direction of the beds composing them, it may be stated in general terms that the direction of the beds is nearly parallel to the direction of the most elevated part of the great chain; thus in Sweden it is from N.N.E. to S.S.W. The dip of the beds is variable, and it is difficult to establish a rule in this respect; yet it would appear, that on the back of the mountains to the N. of Roras, the inclination is commonly towards the W. The angles of the dip vary somewhat less; and observation shows that the most horizontal beds generally occur on the highest summits, and those highest inclined, on the lowest

lowest parts of the mountains, and in the plains *. This nevertheless is subject to many exceptions.

Transition Rocks.—The transition rocks of Sweden are distinguished by their extent in an horizontal direction compared with their thickness, their position, and the nature of the fossils they contain. The rocks composing them are the following: graywacke, conglomerate, and quartzose sandstone; hornstone porphyry, and flinty slate; diorite both compact and porphyritic; a fine-grained and sometimes red-grayish sandstone, nearly without organic remains; aluminous slate; compact limestone, containing orthoceratites; and clayslate, containing fossils of the same genus, but smaller, named graptolites. A thick bed of greenstone occurs on the elevated summit of Mount Westgotha, above the four latter rocks; it is difficult to determine whether it belongs to the transition epoch, or is of volcanic origin.

The author afterwards describes with much care the different districts of Sweden in which transition rocks occur; viz. Dalecarlia, Jemtland, Nericia, Ostrogothia, Westrogothia, Kinnekulle, the Hunneberg and Halleberg, Mount Westgotha, the isle of Oeland, Scania, and the isle of Gottland.

Secondary Rocks.—The formations belonging to the secondary epoch are confined within the districts of Schonen and Scania. They are divided into two portions: sandstone and limestone. 1. Sandstone, containing beds of coal, bituminous slate, and schistose clay, occurs along the Sund, for the length of from three to four leagues, on the south of Kullaberg; its greatest breadth between the Sund and Süderas is more than a league and a half. The large-grained sandstone in the environs of Hoor, near the northern shore of lake Ringsjo, appears also to belong to the coal formation. 2. The limestones are of different kinds: Muschelkalk forms thick beds near the church of Ignaberga, at the foot of the Balsberg, near Christianstad, and in the environs of Carlshamn.

The limestone mixed with sand (greensand †) is developed in the countries of Svenstorp, Kopinge, Glamming, and Ingelstorp, in the districts of Ingelsta and Herresta, and to the E. of Istad. Beds of chalk, with nodules of flint alternating with a compact white limestone, are seen near Linhamn, on the Sund, to the S. of Malmo.

Basaltic Rocks.—These occur in Scania, at Holmestrand, in the southern part of Norway; at Mount Anneklef, near Hoors; at the Gjelleberg, near the church of Rostanga. An amyg-

* This is a position of strata which deserves attention.—*Trans.*

† So it stands in the French text.—*Trans.*

daloid with a brown base, partly resembling that of Holmestrand, has been found in isolated fragments near the Sund, and it will probably be met with in place. There are greenstone dykes in Scania, and the country of Christiana in Norway. In the latter country they traverse transition limestone and clay slate; in Scania, they occur in the slate, and also in the sandstone and gneiss. Trap dykes are also seen in the environs of Rostanga, Konga, and Andrarum.

Alluvion Formations.—The base of all these in Sweden is composed of the remains of primitive rocks; and it is seen, from the nature and disposition of the rolled pebbles, that the cause which has given birth to these extensive formations acted from north to south. The presence of stones on the coast of Northern Germany, derived from Swedish rocks, yet more clearly proves this fact. The great catastrophe which has produced these numerous alluvions is the last which the Scandinavian peninsula has experienced*.

Bog iron-ore is met with in Smoland, Dalecarlia, and Jemtland; where it occurs in sufficient quantity to supply the wants of the principal forges of those countries.

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10. *On the Fresh-water Formation of the Environs of Rome; by* M. Alex. Brongniart†.

M. Omalius d'Halloy was the first geologist who referred the calcareous rocks so well known at Rome and Sienna, under the name of *travertin*, to the fresh-water formation; and who showed that, with the exception of fresh-water shells, which he did not find in the travertines of Tivoli, this limestone presents all the characters of minute structure, position, and mode of occurrence on the great scale, which belongs to the fresh-water formations, such as we have characterized them. They more particularly present those singular tortuous canals so constant in the fresh-water limestones of all countries, tubular cavities which had not escaped so excellent an observer as M. Von Buch, who had described them with perfect accuracy before he knew their importance as a general character of these formations.

The extent of this fresh-water formation of Southern Italy, its importance both as it relates to geology and the arts, authorizes me in entering into some details respecting its formation and position relatively to other rocks.

* This formation appears evidently to be the same with the Diluvium of British geologists.—*Trans.*

† Extracted from the *Desc. Géol. des Env. de Paris,* by MM. Cuvier and Brongniart.

M. Omalius

M. Omalius d'Halloy recognized these rocks at the entrance of the Pontine marshes near Cisterna, at the foot of the volcanic hills of Velletri, in a low plain. The limestone is white, compact, and solid, pierced by numerous tubular cavities, and contains *Limneæ* and globular *Helices;* he presumes that it is in many points covered, as in Auvergne, by volcanic breccias. It would appear that this limestone also occurs to the south towards Calabria; for it is stated that the temples of Pæstum, in the gulf of Salerno, are constructed with a concretionary stone, which is very certainly a travertine.

This formation appears at first as a trace at Monte Verde, S. of Rome, is afterwards found well-characterized in Rome itself, and then in considerable extent and thickness, at some distance from this city, towards the E. in the direction of Tivoli, and on the N.W. in the direction of Civita Vecchia: it was while examining these different points that I became acquainted with the various circumstances attending its position.

At Monte Verde it occurs only as a thin interrupted bed, deprived of its essential characters; it rests on a siliceous sand, mixed with some augite, which covers an earthy and very homogeneous volcanic tufa.

At Rome, M. Brocchi, with whom I had the great advantage of visiting these places, pointed out to me the fresh-water limestone at the eastern foot of the Aventin, on the banks of the Tiber, at the spot named the Cavern of Cacus; it is compact, contains some fresh-water shells, and rests on a red and earthy volcanic breccia: it is not covered by any rock.

The plain which extends from Rome to the mountains where Tivoli is situated, is in a great measure covered by a thick deposit of travertine, commencing at Martellone on the route from Rome to Tivoli, and being continued to the foot of the mountains of the latter place. This plain, in which the quarries of Ponte Lucano are situated, which furnish the travertine employed in building, may be considered, as M. Omalius d'Halloy has observed, as the bottom of a great lake, at present traversed by the Teverone, bordered by a volcanic breccia, and filled up and rendered nearly dry by calcareous deposits; for it is not entirely dried, and we may, with M. Omalius d'Halloy, consider the small lakes of Tartari, the Solfatara, &c. as the remains of this vast mass of water.

Geologists who have examined this formation, and especially MM. Von Buch and Omalius d'Halloy, have remarked; 1. That the lower and ancient travertine, the formation of which does not now take place, was that used for buildings, as alone presenting sufficient compactness and solidity; that at present formed

formed by the lakes of the Solfatara and Teverone not being sufficiently dense. 2. That fresh-water shells are very rare in it; for not only does M. Omalius d'Halloy mention that he had not seen any, but he considers that their absence is to be accounted for by sulphuretted hydrogen gas dissolved in these waters, and which prevents any fresh-water Mollusca from living in them. The very different state of the lakes Tartari and of the Solfatara accord very well with this theory.

The first contains limpid water, its banks are covered with calcareous incrustations possessing a crystalline structure; these are seen to be ancient, and it appeared to me that the present waters do not possess the property of depositing any: the bottom of this lake is covered with vegetables of various kinds, with Batrachian reptiles, insects, &c.

The lake of the Solfatara, situated nearer the foot of the hills, is altogether different: it is formed of a considerable mass of whitish water, perpetually disengaging bubbles of air, and a marked odour of sulphuretted hydrogen gas; the water deposits a thick bed of white compact limestone, a true travertine, on the vegetables which grow on its banks, and in the channel at which it escapes. When the bottom of this lake is agitated, a considerable disengagement of gas is produced; in the line through which the gas passes, the water acquires a limpidity, owing without doubt to the dissolution of the limestone by the carbonic acid disengaged. There is no living animal either on the shores of this lake or in its waters; at least we have never observed any.

The differences presented by these two lakes seem to be in relation to the differences often seen in the lower and upper parts of fresh-water formations; that of the Solfatara shows the formation of the limestone in activity. The waters are too highly charged with carbonic acid and earthy matters to allow animals to exist in it; consequently the first calcareous deposits should not contain any of their remains, at least in the vicinity of the spring: but in proportion as the mass of mineral matters diminishes, or in proportion as the channels are obstructed by these deposits, the waters become less charged with gas and lime, the deposit less rapid and less crystalline, the animals begin to appear, and this deposit charged with their remains would be above the first. This is probably the present state of the Lago de Tartari; and this relative position of the fresh-water formation without shells, and that with shells, is precisely that observable in all those places where these two rocks have been observed: thus, in the Paris basin, the siliceous limestone without shells occurs beneath the shelly millstone, &c.

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These considerations have appeared to us of sufficient importance to arrest our attention for a short time, as they contribute to complete the history of a formation first recognized in the environs of Paris.

The travertine or fresh-water limestone of the plains of Tivoli is not, however, entirely without the remains of shells. I have observed them at Villa Adriana, at the foot of the hill, in a limestone bed, the position of which, relatively to the other rocks, is very clearly shown.

The fundamental rock of the Tivoli hills is a fine compact limestone, containing interrupted beds or nodules of chert, and which appeared to me to bear the greatest resemblance to the Jura limestone*; the fresh-water limestone sometimes rests immediately on this older rock, at others it occurs on a volcanic brecciola which rests on this compact limestone. The place last mentioned shows this superposition in the most evident manner. Proceeding from the surface downwards, we see: 1. a compact travertine, with tubular sinuosities and a few shells; 2. a mixture of friable travertine, and the debris of volcanic brecciola; 3. a thick bed of this brecciola.

The facts observed by M. Brocchi, which I have also verified with him, and those which I have myself noticed, establish the relative position of the rocks analogous to those of Paris, as well at Rome as in its environs; the following order of succession proceeding from the lowest to the highest.

1. A compact limestone, analogous to the Jura limestone*, or even, perhaps, to chalk. The fossils alone, when found, and they are very rare, can remove these doubts.

2. *Calcaire grossier*, composed at its base of blue, shelly, and argillaceous marl; and at its upper part, of reddish sandy limestone, and sometimes even of marine sandstone, as is clearly seen at Rome, at the foot of Monte della Grita.

3. Volcanic brecciola in all its modifications, covering the latter rock, as is well seen at Mount Marius.

4. The fresh-water formation. It would therefore be here in a different position from that which I have observed in the Cantal, in the department of the Puy de Dôme, and in that of the Allier. These may be referred to the middle or gypseous fresh-water formation, and those of the Roman states to the upper fresh-water formation, above the second marine formation; and this agrees still more perfectly with the position M. Prévost has assigned to the *calcaire grossier* of the Apennines.

I have already mentioned that there was a considerable

* Equivalent to the Oolite formation of the English series.—*Trans.*

quantity

quantity of fresh-water limestone or travertine on the side of Civita Vecchia. It is first seen constituting large platforms near Mala Grotta and Guido; it then ceases: but after having passed the Pulidoro and the hamlet of the same name, considerable masses of fresh-water limestone are traversed, forming salient portions, and appearing to advance towards the sea in the manner of a lava. It is thickest and most abundant at Monterone, where it rests on a rock having all the characters of a transition formation.

The celebrated cascades of Tivoli are not due to escarpments of the compact limestone, forming the mass of these hills, but to a stoppage of the valley produced by deposits from the waters which flow from it, and which were much more charged formerly than at present with carbonate of lime. The agitation of the waters gives rise to undulations in this deposit, not observable in the plain; and the less abundant precipitation allows the limestone to acquire a texture and crystalline aspect, removing it from travertine and rendering it more like alabaster. The same facts, owing to the same causes, are observable at the beautiful cascades of Terni. Compact fresh-water limestone or travertine is first met with in the environs and lower parts; and afterwards at Rieti, at the confluence of the Velino and the Nera, this little river precipitates itself over a bar of crystalline concretionary limestone, formed in the same way and on the same fundamental compact limestone as at Tivoli. M. d'Halloy has observed fresh-water shells in the concretionary limestone. H. T. D. B.

[To be continued.]

XXXII. *Reply to* Mr. Henderson's *Remarks on* Captain Sabine's *Pendulum Observations. By Capt.* E. Sabine, *R.A. F.R.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN reply to Mr. Henderson's communication in your last Number, I beg to acquaint him that a detailed account of the corrections of my pendulum experiments was presented to the Royal Society the day after my return from the continent, and read the same week, being in the commencement of last June *; and that I expect it will be printed at the close of a paper containing a continuation of the same experiments connecting Paris and London, in which I have been lately engaged.

Mr. Henderson has justly characterized the correction of the

* See our last Number, p. 143.—Edit.

length

length of the pendulum at New York as trifling: so also are the corrections at the four other stations, where the rate of the clock was obtained with the repeating circle. The observations recalculated show the clock to have been gaining at Jamaica $4^s,07$ daily, instead of $4^s,14$; at Trinidad $3^s,23$ instead of $3^s,19$; at Maranham $2^s,69$ instead of $2^s,70$; and at Bahia $2^s,76$ instead of $2^s,68$. No correction amounts to one-tenth of a second *per diem*, and the joint effect (in the comparison between the tropical stations and those of the middle and northern latitudes) does not exceed one hundredth of a second *per diem*; a quantity far too small to have any influence on the deductions.

I avail myself of the opportunity to subjoin a table of the corrected lengths of the pendulum, for the convenience of any person who may have occasion to employ them. The latitudes are given in the table to the nearest half minute, being sufficient for the required purpose. The observations of the southern stars have been recalculated with Mr. Fallows's right ascensions and declinations. The only alteration produced thereby worthy of notice is in the latitude of Ascension, from $7° \ 55' \ 48''$ to $7° \ 55' \ 10''$, by employing the more correct apparent declination of α *Centauri*: the alteration, however, has no influence beyond the mere correction; as $39{\cdot}0152 + 0{\cdot}20227 \sin^2 7° \ 55' \ 48''$, and $39{\cdot}0152 + 0{\cdot}20227 \sin^2 7° \ 55' \ 10''$ give the same theoretical pendulum for Ascension. In no other instance do the corrections exceed a very few seconds.

Stations.	Latitudes.	Pendulums.
Spitzbergen	79° 50′ N.	39·21469
Greenland	74 32·5	39·20335
Hammerfest	70 40	39·19475
Drontheim	63 26	39·17456
London	51 31	39·13929
New York	40 42·5	39·10120
Jamaica	17 56	39·03503
Bahia	12 59·5 S.	39·02433
Trinidad	10 39 N.	39·01888
Sierra Leone . . .	8 29·5	39·01997
Ascension	7 55 S.	39·02410
Maranham	2 31·5 S.	39·01213
St. Thomas	0 24·5 N.	39·02074

London, Aug. 13, 1827. EDWARD SABINE.

XXXIII. *Notice on the Larvæ of* Diptera. *By* William Sharp MacLeay, *Esq., F.L.S.*

To Richard Taylor, Esq.

No order of insects affords greater proof of the necessity of generalizing, than that of *Diptera*. We often hear of the last joint of the antennæ in this order terminating in a bristle, or being furnished with a lateral one; whereas this bristle is essentially part of the antennæ, being in fact articulated, and composed of as many joints as with the thicker ones will make up the proper number that characterizes the family. The true description therefore of the antennæ in *Musca vomitoria* is, that the last three joints compose a seta or bristle inserted laterally at the extremity of the third joint.

Owing moreover to the rarity of generalization in this science, I find on looking over the various entomological works that have been hitherto published, that whenever the larvæ of *Diptera* fall under consideration, they are altogether erroneously described. The head of each species when fully exserted is not of a variable but of a constant form, and like that of other insects is provided with two articulated antennæ. These antennæ are simple and triarticulate in the larvæ of the *Muscidæ*, and under a high power are to be seen situated on that bimammillary frons which was known to Reaumur, but owing probably to the minuteness of the object has been always badly figured, and was not at all understood by him. (See *Mémoires pour Hist. des Ins.* vol. iv. pl. 34. fig. 3, *dd*.) Degeer has represented them as minute tubercles (vol. vi. pl. 3. fig. 12); but either from not accurately investigating them or from not using a high power, and above all from not generalizing, he also remained ignorant of their articulated structure and of their being true antennæ. This is the more extraordinary, as the antennæ of those larvæ of *Diptera*, such as the *Culicidæ*, &c. which have not a retractile head have been long known and figured, and in some cases (as Degeer, vol. vi. pl. 18. p. 8), are so like the antennæ in the larvæ of *Muscidæ*, that it surprises one not a little that these last should have been so long imagined to be destitute of antennæ.

What have been by some entomologists termed the singular anterior prolegs of *Tanypus maculatus*, will be found on accurate examination to be the two anterior pedunculated spiracula, which, from the insect being aquatic, necessarily take a branchial form. The posterior "*prolegs*" are also pedunculated branchial spiracula of the same kind. All those organs, whether retractile or not, which are called *anterior prolegs*, and *tentacula* in *Chironomus*, *Tanypus*, &c. are the anterior

terior spiracula. So far as the word *proleg* may signify a process of the body in *Annulosa*, it may be an admissible term; but the utility of the expression may be questioned, as it scarcely ever adds to our knowledge of the physiology and real use of the organ. In dipterous larvæ generally, whether terrestrial or aquatic, it is extremely common to see the stigmata supported on peduncles.

It is worth remarking that although, in these larvæ, until we arrive at the extremes of the order, there are no stigmata along the sides, except the first or humeral pair; yet on dissecting a common flesh-maggot,—in which, by the bye, the tracheæ form a most beautiful microscopical object,—we may observe that the longitudinal trunks of these send off at equal distances lateral branches just as if there were spiracula to correspond with them. Nay more, in several species of dipterous larvæ we may observe the place of the deficient stigmata marked out as tubercles along the sides of the body, and they become very distinct in the pupa. The prolegs, as they have been termed, of the larva of *Elophilus pendulus* give a curious instance of these abortive stigmata in a high state of development, and what have been described in this insect as the anterior pair of feet, are no other than the usual palmated stigmata which occur on the humerus of the larvæ of *Muscidæ*, only they are here somewhat pedunculated.

Reaumur and Degeer afford us mines of information; but in the present state of the science, these most valuable authors must be read with the insects before us, else they will only serve to lead into error.

Having thrown out these few hints, in order to show the value of generalization in natural history, I conclude, Sir, with stating myself, Your very obedient servant,

Havana, June 20, 1827. W. S. MacLeay.

XXXIV. *Reply to* Mr. W. Phillips's *Remarks on the Crystalline Form of the Hyalosiderite. By Dr.* Walchner, *Professor of Chemistry, Carlsruhe.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE Philosophical Magazine and Annals of Philosophy, Number 3, contains some observations of Mr. W. Phillips, on the crystalline form of the hyalosiderite, which I described*, four years ago, as a new mineral substance. As I

* *Disquisitio Mineralogico-chemica de Hyalosiderite.* Friburgi 1822; and Schweigger's *Neues Journal* 1823.—[See also Phil. Mag. vol. lxiii. p. 181.—Edit.]

am reproached by the author, as not being acquainted with the practical use of the reflective goniometer, I feel myself tempted to reply in a few lines, which you will greatly oblige me by inserting in your valuable Journal.

In determining the inclinations of the planes of the hyalosiderite, it is true, I have employed the common goniometer; but it was because I had no reflective goniometer at my disposal: yet being convinced that any measurement of small crystals afforded by the common goniometer will remain still imperfect, I thought to have prevented any reproach on that account, by further observing that my determinations cannot boast of very great accuracy. Some time after the publication of my measurements, Dr. Gustavus Rose * found by means of the reflective goniometer the inclination $d - a$ 139° 16′, and observed the plane M in Mr. W. Phillips's figure, which the crystals I possessed had not. Therefore the observations of Mr. W. Phillips cannot, it is obvious, pretend to be altogether new.

Mr. W. Phillips allows himself to communicate to the public the unfavourable opinion he entertains of the mineralogists of the continent, whom he pleases to mention as prejudiced against the reflective goniometer. It is indeed singular and astonishing to meet with such a reproach at a time when all measurements are made by means of the instrument of the ingenious Dr. Wollaston; and it is still more so, considering the many valuable treatises on the improvements of this instrument, published some years ago in the German journals.

It may easily be presumed that Mr. W. Phillips, when giving his judgement on foreign mineralogists, was not acquainted with those treatises. As they contain the most unquestionable proof of the high esteem the invention of Dr. Wollaston has met with in Germany, I think it incumbent on me, in vindicating the naturalists of my country, to note the authors of some of the most valuable of them: Muncke, *Taschenbuch für Mineralogie*, xiii. 438; Studer, *Gilbert's Annalen*, xvi.; Baumgartner, *Gilb. Ann.* lxxi.; Reysser, *über genaue Messung der Winkel annalen Berlin* 1826; Rudberg, *Vetenskaps Acad. Handlingar* 1826.

I remain, Gentlemen, yours truly,

Dr. Walchner,

Carlsruhe, July 16, 1827. Professor of Chemistry.

* Poggendorff's *Ann.* iv. 1825.

XXXV. *On the Crystalline Forms of the Natural and Artificial Sulphuret of Bismuth.* By William Phillips, *F.L.S. F.G.S. &c.**

A SPECIMEN was lately received from Cornwall by my friend J. T. Cooper, Esq. for analysis; and having ascertained it to be sulphuret of bismuth, he was induced, by the rareness of this substance in well-defined prisms, to permit me to take several of them, in the hope of my being able to acquire some information respecting their form by means of the reflective goniometer, and of any cleavages they might afford. I also received from the above-named gentleman some of the same substance artificially melted and crystallized. The specimen from Cornwall I recognized as having been brought from Fowey Consols and Lanescot mines, (which are situated about five miles to the east of St. Austel,) from having heretofore seen a specimen of the same kind at those mines, but of which the crystals were extremely minute and imperfect.

The prisms have the ordinary character of sulphuret of bismuth, in melting immediately when placed in the flame of a candle, and they are bright externally and extremely flexible; accompanying them there are other prisms of the same general form, which are dull and rough externally, and brittle; and the surfaces produced by fracturing them in any direction do not evince any regular structure, but on the contrary are granular, and the particles of which they seem to be composed are whiter than sulphuret of bismuth, and occasionally exhibit a tinge of red. These prisms melt partially when placed in the flame of a candle, throwing off, while melting, numerous brilliant scintillations, and may therefore perhaps be considered as sulphuret of bismuth, including a mechanical admixture of some other substance: and it is probably owing to this impurity that Mr. Cooper has not yet been able to complete his analysis; the small quantity received from Cornwall not having sufficed to enable him to detect its nature.

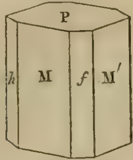
The artificially crystallized sulphuret of bismuth afforded only crystals so very slender as scarcely to exceed a human hair in thickness, but extremely bright. I found them to consist of rhombic prisms of 91° and 89°, which one of them afforded alternately around the crystal. Their form is given by the first and simplest of the succeeding figures, which shows them to be modified by the planes *h* and *f*, the incidence of M on M′ being 91° 00′, and of M on *h* 134° 30′. It is obvious from the mass inclosing these crystals, that they have at least

* Communicated by the Author.

one

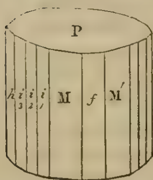
one principal cleavage, of which however I could not ascertain the position.

Fig. 1.



M on M'	.....	91° 00'
M on *h*	......	134 30

Fig. 2.



M on M'	.....	91° 30'
M on *h*	.....	134 30
h on *i*1	.....	137 40
—— *i*2	.....	156 35
—— *i*3	.....	162 25

The second figure represents some crystals from the Cornish specimen, and the measurements obtained from them are annexed. The near coincidence between the natural and artificial crystals is remarkable; and perhaps the small difference of 30', occasioned by the artificial affording M on M' 91°, while the natural gave 91° 30', may be accounted for by the extreme difficulty of separating the natural crystals from the matrix without bending them in some degree, since almost every one was attached to it at both terminations; and I am inclined to believe 91° to be the more correct measurement, since that of M upon *h* agrees in both cases: in both, the plane *f* is too imperfect for measurement.

The only bright cleavage of the native sulphuret is parallel to the plane *h*; but there does exist a cleavage at right angles to it, though difficult of attainment and far from bright: in one instance I obtained an incidence of 90° by the reflective goniometer. Cleavages also appear to exist parallel to the planes M M', but I did not succeed in deciding the fact by the goniometer. The prisms allow a cleavage sufficiently bright for its use in a direction at right angles to the axis, affording P on M or *h*, 90°.

From the preceding circumstances I am induced to consider the primary form to be a right rhombic prism of 91° and 89°.

XXXVI. *Outlines*

XXXVI. *Outlines of a Philosophical Inquiry into the Nature and Properties of the Blood; being the Substance of three Lectures on that Subject delivered at the Gresham Institution during Michaelmas Term* 1826. *By* John Spurgin, *M.D. Fellow of the Royal College of Physicians of London, and of the Cambridge Philosophical Society.*

[Continued from vol. i. p. 426.]

FLUIDITY is a property of the blood which is next to vitality in importance; and indeed it may be regarded as the second essential of its nature, fitting or adapting it to fulfil the purposes of its first essential,—Vitality. From all that we have advanced upon the composition of the blood, it may be seen that this property is modified or affected by manifold circumstances; by none, however, more than by withdrawing it from its native vessel and keeping it at rest; when it soon enters upon another kind of existence, intermediate, as it were, between fluidity and solidity, assuming the form and state of a soft solid by accomplishing its tendency to coagulation. But in order to comprehend the great importance of this property of the blood, (viz. its fluidity,) we must first express the notion we entertain respecting the term Vitality. The coagulation of the blood has been regarded as the last effect of its vitality, and as the best proof of its possessing life, inasmuch as in this respect it approximates to the condition of the solids of the body, which are considered to be undeniably vital, by reason of their exhibiting the undoubted marks of life—*Contractility* and *Sensibility*. But if these be the only legitimate marks and signs of the presence of life,—the only phænomena that are essentially vital,—such a proof as the above is extremely defective, if not highly fallacious; for whilst the coagulating blood exhibits no marks whatever of sensibility, its properties agree in nothing with contractility. Coagulation and contraction are terms applicable to two different kinds of matter. Coagulation applies to the solidifying of fluids, Contraction to the dimininished volume of solids: the coagulation of the blood, therefore, has nothing to do with the contraction of a muscle; nor the fibrillary structure of coagulated blood with muscular structure: in the former, moreover, the arrangement of the fibres is reticular or plexiform; in the latter, longitudinal only, and this most distinctly so; each fibre being kept separate from its neighbour by an investing sheath.

What are we to understand by the term Vitality? Is it a property of matter? of the blood for example; or is it something superadded to matter? to the blood, for example; or is it a term signifying

signifying nothing? or at best signifying but an imaginary existence? How many volumes have been written! how many hours have been spent! how many brains have been at work, to defend or to maintain each of these questions affirmatively, to the negation of the other! And, now, the task devolves upon us to express our assent to, or our dissent from, one or other of these sentiments; and we should unquestionably stand in a ridiculous dilemma indeed, were we to adhere to one of those views exclusively. We assent to the theory of life entertained by Hunter and many others, so far as it regards life as a distinct thing altogether from matter, a distinct somewhat; but we dissent therefrom, so far as it regards life as a mere principle of so abstract a nature and so indefinite a shape as to admit of being resolved into an imaginary somewhat, an ideal, inexplicable, and unintelligible phantom—a *materies vitæ diffusa!* We assent to the theory that life is a property of matter, so far as it regards it as dependent upon matter for its manifestation in this material world; but we dissent therefrom, so far as it regards life as a product from the combinations of matter; because if it be merely a result of certain combinations of matter, we are bound to ask what brought about those combinations? combinations, indeed, that are at variance with the known natural affinities of matter, and which would prove matter to be inconsistent with itself, and would render death a strange anomaly; nay, an absolute impossibility. To say that these effects are owing to the combined actions, the healthful operations of parts and organs having a mutual and indefeasible connection one among another, the destruction or disturbance of which is followed by the phænomenon of death, is begging the question with a witness! The theory assumes what is absolutely denied and disproved by the conclusion. The theory assumes the necessity of combined actions, of healthful organic operations, for manifesting the various effects called vital, and yet concludes that these actions and these operations require nothing to combine them. The combination is a mere chance—the healthy actions and operations are a mere chance—the life, as the result of these, is a greater chance still! in short, it comes to be, according to this theory, as well as according to the preceding, an imaginary somewhat, a shadowy nothing. But in expressing our opinion of the views that have been entertained respecting life, it might at first sight appear as if we were desirous of detracting from the merits of those illustrious men who have espoused one or other of the theories just now adverted to, and had thence deprived ourselves of the authority and excellent reasoning which they afford. So far, however, from this being the case, we are supported both by their authority and

and by their reasoning; and above all, by their experimental labours, in maintaining that life is distinct from matter, but nevertheless dependent upon it for the manifestation of vital phænomena in this material world: consequently, when we assert that certain conditions are indispensable on the part of matter for the manifestation of vital phænomena, and that other laws are in operation for the bringing about, and preserving it in, such a condition; such laws, for instance, as by their operation evince a perfect knowledge of, and full power over, every law that pertains to material things, we are supported by both classes in employing the term, *Vitality*,—to express *that condition of matter which fits it for accomplishing and fulfilling some specific living purpose, some particular living design.*

But still the proof may be demanded of us of the blood being a vital fluid, even under the view which we have taken of vitality; such a proof for instance, as whilst it exhibits some peculiarity that is connected with the blood, and identifies itself at the same time with some striking peculiarity in the solids, will stand in the place of that which has been adduced by Mr. Hunter, viz. its spontaneous coagulation. We confess we can advance no other, than their undergoing the same process after death, *i. e.* Putrefaction. For we might as well look for signs of vitality in a dead body, as for the same signs in a fluid that is in an altered condition almost the instant it is withdrawn from its vessel; not to mention the mortal influence of venous blood itself, if injected, as such, into the arteries, without first undergoing aëration in the lungs.

Putrefaction, or the return of animal matter to its original state and natural combinations or affinities, is the inevitable doom of flesh and blood; and in this common peculiarity, the most inveterate reasoner against the blood's vitality is furnished with a no less sensible than suitable mark to turn him from his headstrong scepticism. For the proofs of vitality, we would go to the seat of life, the living body; but for the changes which have been wrought upon matter in order to its being rendered vital and living, we must avail ourselves of the researches of the chemical philosopher. For the manifestations of life, we must behold and admire the actions of the bodily parts severally, and the movements of the universally circulating fluid, the blood, generally.

Mr. Hunter's experiments on the effects of cold upon the blood and upon the fluids of the egg, most satisfactorily confirmed the presence in, and influence of some principle upon them, which enabled them to resist congelation for a considerable time; but whether he could show with equal satisfaction that that principle was identical with life, or entitled to that appellation, without at the same time furnishing us with a definition of his term, must remain questionable, so long as science forbids indefinite terms to form a part of her vocabulary, or disowns indefinable notions as portions of her system.

To say that the blood enjoys vitality, or has a vital principle within it, in the same manner as the solids,—without defining what is meant by a vital principle,—is to leave the subject of the blood's vitality involved in as much mystery as if any other term, no matter how unintelligible, had been substituted in the place of vital principle, or vitality! Indeed, we cannot avoid repeating a remark which we made in our introductory lecture; that "*whenever a subject is defined and determined by occult qualities, it remains as obscure and unintelligible as if no definition or description had ever been given.*"

Not to incur the charge, therefore, of employing unknown principles as the basis of all our reasoning, we have attempted a definition of the term Vitality, which we hope one day to witness as the occasion of some discussion in the scientific world; and this with a coolness of intellect and dispassionate unprejudiced state of mind which is best befitting so important a subject,—a definition which we well know to be quite novel in science, and which, therefore, we would not put forward as a theory, but rather as a question for examination.

We have endeavoured to express ourselves in as concise a manner as possible on the blood's vitality, and to define in what sense it may be regarded as a vital fluid. But as in considering its vitality we find that property inseparable from its fluidity, and thence connected with that condition which fits it for traversing every part of the frame, we must follow the course of this fluid, or accompany it to those its destinations where its life becomes more and more apparent, or where its vitality becomes more and more manifest. Contemplating then the blood as circulating through every part of the body; as permeating the vessels from their largest calibre to their minutest capilli; and as flowing, in short, through a whole body composed almost entirely of such minute capilli,—we shall immediately discern the ground and reason of the blood's fluidity; this its property being, as we have before said, an essential in its nature secondary only in importance to its first essential, vitality. Much has been said on the cause of its fluidity, and many experiments have been instituted with the view to discover it: warmth or heat alone will not preserve it fluid; for it coagulates when effused into any cavity or part of the warm

and

and living body*: in fact, no extraneous circumstance whatever can be regarded as a *cause* of this phænomenon, however greatly its tendency to coagulation may be retarded or influenced by any one extrinsic agent. The fluidity of water and other liquids is ascribed to the presence of caloric, and these may be rendered volatile by an increase of the heat: not so the blood; this increase would only tend to destroy it altogether as blood.

Can we say it is a vital heat or the animal heat which keeps it fluid? We think not; because the cold-blooded animals as they are called, have their blood equally fluid. Is the motion to which it is subjected; the constant agitation by the heart; the incessant straining as it were which it undergoes through the minute capilli,—sufficient to preserve it in a fluid state? or more, to cause it to be so? The action of the heart may be referred to several causes, so may the reaction of the capillary vessels upon the blood, and both ultimately to the presence of life in the body: but blood or living fluids existed before the heart that moves them, as in the first days of conception; they existed before any vessel was formed for them to circulate through: wherefore the first cause of the blood's fluidity must be sought for independently of the secondary or accessory causes just adverted to. And we may venture to assert, because our assertion is founded in fact, that the essential and primary cause of the blood's fluidity is its vitality: and this is consonant to that rule in philosophy, which teaches "that the cause of the cause is also the cause of the effect,"—*causa causans est causa effectus*†.

The fluidity of the blood must be regarded as a very extraordinary property indeed, and one which is eminently characteristic of some more active and energetic agent existing within it, which withholds it from yielding to its own natural tendency to coagulation. This agent some would identify with a vital principle at once, without considering the possibility that material elements may admit of such states or combinations as to render them highly active in their nature, extremely subtile in their form, and capable of imparting their own activity

* If we take a fish out of the sea, the heat of its body perhaps about 60°, and bring it into an atmosphere of 70°, the blood on being let out of the vessels will immediately coagulate. This was ascertained on board of a ship lying off Bellisle, in the summer of 1761: for immediately upon a fish being caught, I ascertained its heat; and letting out part of its blood, it immediately coagulated, although the blood discharged was become warmer than that remaining in the vessels of the fish; which, however, still continued fluid.—*Treatise on the Blood, by J. Hunter*, vol. i. p. 35.

† The blood, says Mr. Hunter, has the power of preserving its fluidity; or in other words, the living principle in the body has the power of preserving it in this state.—*Treatise on the Blood*, vol. i. p. 148.

and susceptibility to motion; or in other words, their own fluidity to crasser or grosser combinations of matter,—as in the case of the blood more especially; for we regard it as an axiom, that for every natural effect there must be a natural efficient cause.

Seeing then that in order to the blood's fulfilling certain purposes and designs,—for we fear not to say that the best purposes and the wisest designs are discernible in the formation and mode of existence of every living thing,—it must enjoy fluidity in a very eminent degree; it must admit of so subtile a division without a destruction of its nature, as will enable it to penetrate into those minute channels and secret recesses where the human eye-sight is eluded, and where those delicate manipulations and chemical operations are performed which no human skill can imitate nor art accomplish. Still the pathways to these recesses are open to our search; still may we trace the blood thither: in other words, its vessels are continued thither. And though we do not possess the means to accomplish, nor enjoy the skill to imitate, the various and wonderful works therein proceeding every the smallest moment, and though we may fall very short of ever arriving at the modes of operation therein resorted to;—yet it is all open to investigation; all prepared to stand the scrutinizing ingenuity and research of man; all is calculated to augment the store of his knowledge, to perfect him in reason, and to advance him in intelligence and wisdom.

It will be to our purpose if we avail ourselves of the testimony of one or more of the skilful and patient microscopic observers of the blood's circulation; for although they were constrained to visit the reptile race of the animal kingdom, as frogs and the like,—nay, a lower race still, as worms; yet from this quarter even have they raised up rich treasures of knowledge for our use and benefit. The celebrated Leeuwenhoeck was among the first to employ that potent instrument the microscope in facilitating our descent, or rather our ascent, into Nature's secrets; and his details of the wonders of the minute creation are interesting in the highest degree. But as we may have occasion to advert to this celebrated authority occasionally in our way; and as we are desirous of giving familiar illustrations as we proceed, we will now adduce some interesting testimony from the observations of Mr. Baker, who was a Fellow of the Royal Society of London, and who devoted much of his time to this kind of research.

On the subject of the current and circulation of the blood, he says, "The tail of a newt or water-lizard applied in a glass tube in the manner directed for the eel, affords an entertaining prospect

prospect of the circulation through numbers of small vessels. But nothing can show it finer than an exceeding small newt of this water-kind which sometimes may be found less than an inch long, and so transparent that the blood may be seen running in all directions, not only through the vessels of the tail, but throughout the whole body. In the fin-like processes situated just below the head the blood is seen coming along an artery to the extremity, and then immediately returning towards the heart again, through a vein that lies close and parallel thereto, and with which its communication is very apparent. This affords a charming sight, and may be viewed by the third or fourth magnifier; for the globules of the blood in *newts* are larger than in any other creature I have examined, and are fewer in proportion to the serum or water they float along in." To which he also adds, that the figure of them, as they are carried along the vessels, changes in a most surprising manner.

The recently produced tadpole being very transparent, is a good object for discerning the pulsations of the heart together with the circulation of the blood in every part of the body; the skin and transparent parts of muscles; the furthest joints of the hinder legs of little crabs; the legs and tails of shrimps; the transparent legs and head of several small spiders, and the current of the blood may be clearly discerned both in the veins and arteries. Mr. Leeuwenhoeck found the globules in various kinds of fish,—as the perch, trout, and salmon,—to be flat and of an oval figure, as also in some birds. The serous part of the blood in fish and aquatic animals is in greater proportion to the red particles than in animals or man, whilst the particles themselves are larger.

In viewing several of the above objects the blood may often be observed passing through vessels so minute, that its globules cannot glide along otherwise than single, and squeezed into oblong forms: yet a hundred of such globules, if placed close to one another in a row, would not equal the length of the diameter of a large grain of sand. Some experiments which were made by Mr. Baker, and his friend Dr. Alexander Stuart, physician to the queen consort of George the Second, with a solar microscope of a peculiar construction, we cannot omit to mention; though for accurate information we allow such a method as they adopted to be in many respects deficient; but as this deficiency does not affect the point under consideration, their experience may be regarded as extremely satisfactory. "Our object," says Mr. Baker, "was a frog, whose limbs being extended and fastened on the frame, we opened the skin of the belly from near the anus to the throat; then

giving

giving a little snip sideways, both at the top and bottom, by sticking a fish-hook in each corner of the skin, it was easily stretched out before the microscope, and presented on the screen a most beautiful picture of the veins and arteries in the skin, with the blood circulating through them. In the arteries we could plainly perceive the blood stopping, and as it were receding a little at each dilatation of the heart, and then immediately running forwards again at each contraction; whilst in the veins it rolled on in a continual current with inexpressible rapidity." To this Mr. Baker appends a note, wherein he says, "When the arteries were magnified very much, by removing the screen to a considerable distance, the alternate expansion and contraction of their sides were very visible and remarkable. After considering this" he proceeds, "as long as we thought needful, we opened the abdomen, and extending the muscles before the microscope, by the same means as we had done the skin, we had the pleasure of viewing their structure, which we found to consist of bundles of transparent strings or fibres, lying parallel to one another, and joined together by a common membrane. These strings or fibres appeared through their whole length made up of minute roundish vesicles; or, in other words, seemed like rushes divided the long way. We could not be certain of any circulation through the muscles, though sometimes we imagined we saw a very slow motion of some transparent fluid: but the object growing dry and rigid, we proceeded to our last experiment, which was to draw out gently a part of the frog's intestine, in order to apply the mesentery to the microscope: and herein we succeeded so happily, that I believe the circulation of the blood was never before seen in so distinct and fine a manner. No words can describe the wonderful scene that was presented before our eyes! We beheld the blood passing through numberless vessels at one and the same instant, in some one way, in others the direct contrary. Several of the vessels were magnified to above an inch in diameter, and the globules of the blood rolling through them seemed near as large as peppercorns; whilst in many of the minutest vessels only single globules were able to find a passage, and that too not without changing their figure into that of oblong spheroids. We saw likewise much better than we had done before, the pulsation and acceleration of the blood in the arteries, in the manner before described, and could clearly distinguish two or three vessels lying over one another, with currents running different ways. In short, it appeared like a beauteous landscape, where rivers, streams and rills of running water are every where dispersed.

During

" During this examination we took notice of a vessel extremely minute, issuing from the side of a larger, and turning backwards from it in a curve line. We perceived at unequal intervals sometimes one, sometimes two, and sometimes three colourless globules, dropt or squeezed out of the larger vessels into this minute one, and gliding through it singly and very slowly; which made the *Doctor* imagine it might be a *secretory duct.* We observed likewise, that as the animal grew languid, and near expiring, the blood in the arteries would stop on a sudden, seem as it were coagulating, and then run backwards for some time; after which it would again recover its natural course, with a great deal of rapidity."

Mr. Leeuwenhoeck informs us that he saw, with great admiration, in the furthest extremities of a very minute fish's tail, how the larger arteries were there divided into the most fine and evanescent ones; and that many of the smallest veins returning from the said extremities, met together at last in some larger vein. There appeared also in some vessels such an agitation of that blood (which was protruded from the larger arteries towards the evanescent ones at the very extremity of the tail, and returned afterwards through many minute veins into a large one) as hardly can be conceived. In the larger arteries he could perceive a continual new protrusion or acceleration of the blood's course received from the heart; but in the smaller arteries the motion seemed equable without any such repeated propulsion: and though in the minutest vessels there appeared no colour, yet in the larger vein or artery, though near the end of the tail, the blood was plainly red. The communications of the arteries are sometimes direct with the veins, without their terminations becoming previously evanescent, and not to be traced. For the same curious observer tells us, that on each side of the little gristles, which give a stiffness to the tail of the fish, he could see a very open communication of the veins and arteries; the blood running towards the extremities through arteries, and returning back again through veins that were evidently a continuation of those arteries, and of the same diameter with them: and this he saw in thirty-four different places, in as many arteries and as many veins. The length of the whole fish was not half an inch; yet in the tail the circulation was notwithstanding visible in thirty-four places, and the current of it in sixty-eight vessels; and yet these vessels were very far from being the most minute of all. How inconceivable then must be the number of circulations in the human body! nor need we wonder to behold it issuing forth at every puncture of a pin or needle.

[To be continued.]

XXXVII. *Reply*

XXXVII. *Reply to* Mr. R. C. Taylor's *Remarks on the Hypothesis of* Mr. Robberds *on the former Level of the German Ocean.* *By* J. W. Robberds, *Esq. Jun.**

In originating the inquiry respecting the Eastern Valleys of Norfolk, my avowed object was, not to contend for victory, but to elicit truth. As this end can only be attained by fair and liberal discussion, it is highly satisfactory to me that my observations have called forth the remarks of Mr. R. C. Taylor. Willingly and with pleasure I acknowledge the candour by which the comments of my "fellow-traveller" have been directed; nor do I believe that he intentionally withdrew from the guidance of the same just and honourable feeling, when at the conclusion of his strictures he condemned me for attempting to deduce general principles from "*assumptions founded on the limited considerations of local operations.*" This charge can only have arisen from a misconception of my line of argument; for, not only did I in my introductory statement point out, as one of the laws of investigation which I prescribed to myself, that local circumstances could only be of importance as far as they belonged to a chain of connected evidence, and that they must be "included in a wider survey of correlative cases," in order to "establish a leading general principle in some branch of physical science,"—but at the close of my book I again distinctly asserted, that "no general principle can be established by a solitary isolated fact;" and that I should, therefore, according to the second law of investigation which I had laid down, proceed hereafter to compare and connect the changes in the Eastern Valleys of Norfolk with numerous observations made on the shores of every ocean. These passages must have been overlooked by Mr. Taylor, or I am sure that he would have refrained from preferring against me an accusation, which I had shown so much anxiety to avert. My observations were undoubtedly confined to a particular district; but my inference was equally restricted within the same limits. The general principle I left to be deduced from a general survey of corresponding cases; and I must still consider this to be the safest, the most satisfactory, and the most conclusive train of reasoning:—first, to select some local fact; to examine it in all its bearings, and submit it to the severe scrutiny of modern science; and then to ascertain how it is adapted to that universal frame of which it is an essential and integral part.

Such is the course of inquiry which I had marked out, and in which the short treatise that I published last year is only

* Communicated by the Author.

the first step. It surely indicates any thing but a disposition to decide precipitately, or an eagerness to draw extensive conclusions from narrow and insufficient data. My desire, on the contrary, is to make good my ground as I proceed. I therefore wished that the first in the long series of facts which I have collected, should pass through the ordeal of strict examination, and be tried by its own merits; and I am thankful to Mr. Taylor for having so effectually seconded my wishes on that point. There is no individual who is more practically conversant with the district, to which I referred, nor who has taken a more scientific view of its geological features. His professional pursuits have afforded him repeated opportunities of minutely investigating objects, which I have been able only to survey casually, in hours of leisure, abstracted from widely different occupations. I was well aware, both from his recorded statements and from colloquial communications, that we entertained opposite opinions on this subject; and I was solicitous that the grounds on which our respective judgements had been formed, should be distinctly set forth, and candidly compared. They are now before the public, and sooner or later the knowledge of truth will be the result. I appretiate duly the accuracy of Mr. Taylor's observations, and the importance of the phænomena which he has described. But his deductions from them ought to be received with caution. Mr. Taylor is the disciple of a school. I hope that I shall not be suspected of using the term invidiously; but the geologists of the present day have formed a school which, while it professes only to collect facts, for the information and use of an indefinitely remote posterity, has still a system of its own; while it deprecates hasty and premature decision in others, it has itself decided prematurely and hastily; and that too, upon the very points on which it is the most deficient in evidence, and the most inconclusive in argument. One of these is the invariability of the level of our present seas, which has insensibly become a canon of this school of science, without any satisfactory proofs whatever. As a member of this body, Mr. Taylor naturally upholds its doctrines, not indeed against his better judgement and honest conviction; but by the influence of great names,—by his respect for his teachers, by the supreme authority of his Cuviers and Bucklands,—he is predisposed to adopt their sentiments, and to read the volume of Nature in conformity with their expositions. I am grateful to these eminent men for the services, the invaluable services, which they have rendered to the cause of science; but it is to the facts which they have attested, not to the opinions which they have put forth, that I bow with the deference of accorded faith.

The operation of the bias thus given to Mr. Taylor's mind is very apparent in the whole of his reasonings upon the Valleys of Norfolk. He has admitted at once my historical proofs, and assented to the first position established by them; viz. that these valleys were formerly æstuaries, filled by the waters of the adjacent sea. But Mr. Taylor denies that the retreat of those waters affords any evidence of a depression in their general level; and he maintains that the conversion of the greater part of their early channels into dry land has been effected "*by the gradual precipitation from waters charged with alluvial mud, and the consequent exclusion of the tide from its ancient receptacles.*" In addition to this ground of difference between us, he also argues, that the marine deposits left on the sides of these valleys do not attest the former elevation of the floods by which they were occupied, but that they belong to the *crag stratum*, and are part of a continuous bed or layer which covers the substratum of chalk through the whole district between Harwich and Cromer. On these two points, in which Mr. Taylor strictly conforms to the orthodox geological creed of the present day, I shall now offer a few remarks, in defence of my peculiar and daring heresy.

The terms in which Mr. Taylor has signified his assent, leave some degree of uncertainty as to the extent of his concession: he admits, "*that at an early period of what may be termed in geological phrase, the existing state of our globe, the sea entered the mouths of these æstuaries, and rolled its tides far up into the interior.*" By "*the existing state of our globe,*" I conclude is here meant the form in which its continents were left at the *supposed* æra of the deluge, or 2348 years B. C. But I cannot so clearly perceive what is to be understood as an "*early period*" of that state. If the facts which I adduced prove any thing, it is evident from them that at the time of the conquest, or 3434 years after the above-mentioned epoch, the tides of the ocean still covered these valleys. I have already shown that there were salt-works as "far up into the interior" as Halvergate and South Walsham. A more careful examination of the Domesday Book has subsequently enabled me not only to discover an additional number of these works in the Flegg hundreds, but also that others were found at Tunstall, the adjoining village to Halvergate; at Fritton, on the western edge of Lothingland, near Herringfleet; and even at Cantley, on the Norwich branch of the valley of the Yare, which by the nearest possible water-course is full twelve miles distant from the present coast. This single fact, certified by a testimony so authentic and incontrovertible, would outweigh in a question like the present a hundred thousand inferences from

from the super-position and juxta-position of strata; it is indeed of such importance in my estimation, that I should not fear to rest the fate of my theory upon it, even if it stood alone and unsupported by that mass of collateral evidence by whch it is attended. Mr. Taylor seems willing to insinuate that these works were of little consequence; for he has inserted a note stating that the value of one of them was estimated at *seven pence*. In the magnificent rent-rolls of the present day this would indeed be a contemptible figure; but our ideas will soon be corrected if we compare it with the income derived from other species of property at that period, and consider also the change which has taken place, both in the positive denomination and relative value of money. Domesday Book affords numerous instances, in which the annual value of the best land in Norfolk appears to have been then *one penny* per acre. In whatever form this was reckoned, the worth of a salina must be computed by the same standard; and it will then be found to have been equal to that of seven acres of land. The same will be the result if we try it by the change in the currency. Hume calculated sixty years ago, that any sum at the Conquest must be conceived "as if it were multiplied more than a hundred-fold above a sum of the same denomination" in his days*. Since that time a further change has taken place, and the price of land especially has been more than doubled; so that we may fairly reckon the relative proportions to be as 1 to 240, or that seven *pence* in the year 1086 were equivalent to seven *pounds* now. Hence then it appears, that computing this to have been the average value of a salina, the works of this description round the æstuary of the Yare (amounting in number to 136) must have produced to the owners of the land an annual rental equal to 952 pounds of our present money†.

The value of these establishments has, however, but little connection with the question. We know that they existed, and that this would not have been the case if they had not been useful and profitable: it is also clear, that their use and profit must have been dependent upon the access of the salt tides,

* History of England, vol. i. p. 228.

† Mr. Ellis in his General Introduction to the Domesday Book, prefixed to the indices, (printed by order of the Record Commissioners,) says at page xl. "285 salinæ in Sussex averaged at 2*s*. 5½*d*. At Stollant in Dorsetshire 32 salt-pans yielded only 40*s*. In Devonshire at Flueta 11 salinæ yielded but 11 pence per annum. At Ermentone in the latter county, however, we have a salina yielding 13 pounds 10*s*. ad pensā & arsurā." This last sum, "of standard weight and fineness," was so enormous for those days, that I must acknowledge on consulting the original passage, it appears to me to refer to the produce of the whole property, of which the salina was only a part.

supplying their brine in all its strength and purity, unimpaired by the mixture of fresh-water. It is very immaterial whether these manufactories of salt were, or were not, equally distributed over all the branches of these valleys: their existence in one quarter and their absence in another must have depended upon many local circumstances, into which it is unnecessary to carry our inquiries. It is sufficient for us to have found such works so far up into the interior of the country, as at South Walsham, Halvergate, Fritton and Cantley; for they prove that the sea actually flowed up to those points not more than seven centuries ago, and consequently that the æstuary must have remained during at least *five-sixths* of what Mr. Taylor calls *the existing state of our globe.* We perceive that by some operation of nature a great change has been effected; and according to credible records, that change was so far completed in the year 1347 that the floods were then sensibly withdrawn from a large portion of that space which they had previously covered. The theory which has been advanced in opposition to mine, supposes the whole of this revolution to have occurred in the interval of 261 years, between 1086 and 1347; for we have evidence that at the former of these dates, the waters filled the valley to as high a level as Mr. Taylor will allow them ever to have reached, and that at the latter period the sand-bank by which he supposes them to have been excluded, was finally stretched across the original mouth of the æstuary. During thirty-four centuries and a half (I go on admitting for the sake of argument the *supposed* æra of the deluge), it is clear then that the sea continued to "roll its tides" to as great an elevation in the Valley of the Yare as, according to Mr. Taylor's ideas, they ever extended; and, consequently, during the whole of this immense term of years,—during a space five times as long as that which has elapsed since,—the law of nature, which was at last brought into operation to shut out these waves, was either suspended, or if not suspended, was working to no effect! I will not dwell upon the inconsistency of such a notion, but proceed to those more powerful and conclusive arguments which will demonstrate the utter incompetency of the cause itself to produce the effects that have been ascribed to its agency.

Mr. Taylor's first position is, that a ridge of pebbles, shingle and sand, being thrown up across the opening of the former æstuary, by degrees excluded the sea; and that the quantity of water subsequently admitted was thus so much reduced as to be not more than sufficient to fill the channels of the existing rivers. But Mr. Taylor forgets that this argument tells both ways. If these banks shut *out* the waters of the sea, they

they ought also to have shut *in* those which covered the valleys at the time when the barrier was raised. Supposing the level of the German Ocean to have remained invariably the same, the reflux at Yarmouth haven during the ebb, cannot have been greater than the influx during the flow, of the tide; no more facility can have been afforded for egress than for ingress; where the rise and fall have been uniformly equal, the volume of water passing and repassing through the same aperture and in the same space of time, must also have been equal. These are self-evident propositions; and from them it is most manifest that the æstuary of the Garienis might have been changed into a lake, but could never have been converted into dry ground by such a process as Mr. Taylor has described. Lake Lothing and Oulton Broad have not been emptied of their waters, in consequence of Kirkley Road having been blocked up; neither have their basins been filled by alluvial deposits or decayed vegetables. The great depth of those basins has of course prevented their being drained; but all the evidence given on this subject proves that the waters which they hold maintain their surface at the average elevation of the adjacent sea. This must equally have been the case in the Valley of the Yare. Yet if there be one fact more certain than another in this question, it is that the quantity of water existing within the compass of this district has been greatly diminished, and is still constantly decreasing. The salt tides, which formerly penetrated to points so far inland, have all been withdrawn; the floods which deposited the soil of the present marshes and meadows have all disappeared; the showers which have descended from the clouds of heaven have all been carried away; the streams, supplied by the land-springs of an area of 1420 square miles, have been drained off: not only have all these passed away, but the extent of our Broads has been contracted, and the surface of our river visibly depressed. It is impossible to survey the banks of the latter, without observing that foundations of old buildings, which were clearly laid below the usual level of the water, are now above its ordinary range; and in the upper part of its course through the city of Norwich, its bed has in some places become so shallow as often to impede the navigation. This fact is still further confirmed by the alteration which, as I have already pointed out, was found to be required at the public staithes; and Mr. Taylor has himself admitted, that there are "conclusive reasons" for assigning to the waters of these valleys, within the period of which we possess historical records, a level of as much as "four or five feet above the mean height of the existing rivers." It is clear that this change has not been

been produced by any deficiency in the supply of water, either from the main source, or tributary springs of the Wensum; for all the mills situated upon it have continued their usual operations, nor has there been any perceptible abatement in the strength of the current by which their machinery is set in motion. These facts evince that a progressive reduction in the amount of the floods of this valley has been going on from the date of the earliest records up to the present time. How then has the former excess above the quantity of water now existing been carried away? The mouth of the haven at Yarmouth is the only passage at which it can have flowed out; and in order to effect this, the average outfall of the ebb at that point during the last seven centuries must have been greater than the influx of the rising tide. But I have already shown that this could not have been the case, if the level of the German Ocean remained unchanged; therefore, as it is an indisputable fact that the receding tide has carried away a greater body of water than it brought in during its access, it necessarily follows that the floods which have left the valleys must have been drawn off by the depression of the reservoir in which they have been absorbed.

But Mr. Taylor maintains also that these floods have been driven out by the growth of solid land occupying their place; that the tide has been excluded "from its ancient receptacles by the gradual precipitation from waters charged with alluvial mud." This argument rests upon an assumption, which is physically impossible and logically absurd; it supposes water to have the power of forming deposits, not only even with, but actually above its own surface; which includes the proposition that a part is not only equal to, but greater than a whole. The average of the specific gravities of all mineral bodies is 2·5 *; therefore two cubic inches of earth are equal in weight to five of water; and supposing a body of water to be "charged with alluvial mud" to the amount of one-fourth part of its own weight, (which I consider to be a large allowance,) the volume of the former would be to that of the latter as 5×1 to $2 \div 4$, or as 10 to 1. Admitting still further, that these waters would clear themselves of all their impurities at one point, their deposit must settle beneath a superincumbent fluid at least ten times greater than itself. Each successive addition to a bed of new land must be a residuum of this kind, and consequently at the last stage of its completion the water which brought together its materials must still have flowed above its surface. To expel that water, therefore, by the precipitation of its own mud, would be part of a progression which, if we can suppose

* Playfair's Illustrations of the Huttonian Theory, p. 492.

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it to be carried to its utmost extent and with uniform results, could only terminate in the conversion of our whole globe into dry land, and the exclusion of water from the earth: this is an impossibility, and can only be credited by those who adopt St. Augustine's standard of belief.

I will now try this reasoning by the test of facts as exhibited in these valleys. Mr. Taylor says, that "four distinct processes contributed to the formation of the ground on which the town of Yarmouth now stands. First, the accumulation of heavy materials rolled by the sea; second, the deposit of oozy sediment from muddy waters; third, the external covering of sand by the operation of the winds; and lastly, the rise and decay of vegetable substances." This narrow strip of land is evidently the work of the ocean on which it abuts; yet it lies at this time so far above the highest range of those waters by which it was formed, that it is never overflowed by them; the fullest tide and most furious storm acting in conjunction, are insufficient to carry them over the effectual and insurmountable barrier which they themselves have raised. Here then is a manifest and decisive proof that they formerly exerted a power which they no longer possess; the only natural cause from which this power can have proceeded, is their acting at a higher level than that which they now occupy; the loss of this power establishes, therefore, the loss of that elevation from which it was derived. To overcome this difficulty, Mr. Taylor has called in the assistance of two minor agents—wind and vegetation; the basis only of this bank he considers to be alluvial, and the superstructure to be composed of mounds of drifted sand and accumulations of decayed vegetables. Let us inquire how these agents have performed the respective parts assigned to them.

My mode of studying geology has been to collect from the best sources all the most positive facts that I could ascertain, relating to the structure of our globe. In the course of these researches I have not failed to observe the operation to which Mr. Taylor has claimed my attention. The spread of what is called the sand-flood in Egypt, and that of the landes in the South of France, are phænomena which may undoubtedly be repeated in all similar situations; wherever extensive tracts of sand exist, their surface may be carried forward by the winds and heaped upon the adjoining land. But in these cases the storm does not bear upon its wings the spoils of distant climes to deposit them when and where it lists; it is from the neighbouring desert that it sweeps the dry and sterile materials, with which it deluges the cultivated vale. How then does this process apply to the district round Yarmouth?

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From what beds of sand in its vicinity have the grains been wafted that are strewed over its whole plain? Till within the last eight centuries it was confessedly at first a shoal in the æstuary, and then an island between the two entrances into the river; it is now a peninsula, about four miles long and half a mile in breadth. On the east it is bounded by the sea; the haven divides it on the south from the inhabited tracts of Gorleston and Southtown; and on the west from the meadows and marshes that extend far into the interior; on the north, the buildings and inclosures of Caistor intervene between this level and the higher grounds, except for a short space, where it reaches without interruption to the foot of the cliff, which stretches thence along the coast to the northward. This is the only point from which the wind can have conveyed any external drifts of sand into the quarter which we are investigating. I will, therefore, ask in the first place, is it possible that the limited supply derived from this single spot can have produced an average elevation even of one inch, when scattered over an area of two square miles? and in the second place, must not the winds, blowing upon this tract from every other point of the compass, have swept away into the surrounding waters at least as much of the loose, shifting soil as can have been brought in by all the gales that have come from N.N.E.? The nature of sand and dust cannot be changed by the atmosphere of Yarmouth; they can acquire there no additional weight or consistency to make them more tenacious of their position, nor can the old proverb of "*Lightly come and lightly go*," be less exemplified in their departure than in their approach.

From these considerations it is obvious that the ground which we are exploring can have been little, if at all, raised by any "external covering of sand," brought there by the winds; and that the superficial coat of that material cannot at this time be very different from what it was when the bank first emerged from the waves. Its component particles may have been more or less removed from one side, and accumulated into hillocks on another; but such transfers evince no sensible addition to the aggregate amount of the whole mass. "The conflict of meeting tides and the fury of our easterly gales," to which I ascribed the original construction of this work, are certainly adequate to such an operation in the mouth of a wide æstuary; but the bank when so formed can have been left dry by no other process than the depression of the waters beneath which it was collected.

Supposing, however, that Mr. Taylor had been able to point out any quarter whence the wind could have brought these

these sands, it would still have been incumbent upon him to provide a foundation to receive them, constantly above the highest level of the sea; for by no means could they have permanently accumulated, while exposed to the action of the waves. A striking proof of this has been already adduced in the dispersion of the Scrotley Bank in the year 1582. But a more familiar and more frequently recurring illustration of my argument may be seen on flat beaches, where, when the tide is out, a strong wind blowing along the shore drives the sand before it, and forms numerous mounds and ridges; but the first ripple of the returning flood sweeps them all away, and "the baseless fabrics—leave not a wreck behind." Mr. Taylor's own theory, therefore, presupposes that his "heavy materials rolled by the sea," and his "oozy sediment from muddy waters," must have been raised above the access of the swelling tide and of the storm-driven surge, before his "external covering of sand by the operation of the winds" can possibly have found a resting-place upon them. Nature, ever uniform in all her laws and in all her operations, makes this an indispensable preliminary to the commencement of such a process. How then does Mr. Taylor elevate his marigenous foundation to a secure altitude above the reach of the only agent which he employs in its construction? With his ideas, he cannot possibly explain this point: the instances to which he has referred, of shores that are guarded by "defences of sand accumulated by the waves," absolutely prove nothing; there is not one of them which contradicts my theory, or rather, by which it is not substantiated; for they all oppose to his inference the same insuperable difficulty, by requiring him to point out, how the alluvial base on which his ramparts have been erected, first emerged from the waves which they now repel. Thus the very agency to which Mr. Taylor has had recourse assists to demonstrate, that in the natural course of things, no beds of earth, brought together by the sea, can rise above its level, but by the subsidence of the waters beneath which they were formed *.

To show that I have estimated correctly the effect of the winds in this instance, I might appeal to no less an authority than that of Mr. Taylor himself, who concludes this part of the subject by owning "*the comparatively insignificant height to which the sand has hitherto been drifted on Yarmouth Denes.*"

* I except of course the effects of volcanic expansion, the growth of coral rocks, and the mechanical contrivances of human skill, none of which are locally applicable in this inquiry.

If I had been disposed to avail myself of an *argumentum ad hominem*, instead of relying upon fact and reason, I might have referred at once to this passage: but I do not like to take advantage of the admissions of a candid adversary; nor should I think my opinions worth maintaining, if they required the support of a mere quibble or trick of debate. I cannot refrain, however, from expressing some astonishment at Mr. Taylor's having charged me, not indeed in direct terms, but certainly by implication, with ignorance of my subject, or at least with inattention, to a most important feature of it, because I "overlooked altogether" a circumstance, which, after all, he himself admits to be "*insignificant*." It is surely no proof of defective vision, that I was unable to discover a *mountain*, where he acknowledges that there is scarcely a *mole-hill* to be found.

Having now shown that the principal of Mr. Taylor's subordinate agents, viz. the wind, has not contributed materially to raise the ground upon which the town of Yarmouth stands, and indeed that it could not have acted in the manner which he has described, unless the sea had previously retired; it is not necessary for me to investigate very minutely the extent to which his other auxiliary, viz. vegetation, has been employed in the same work. Where the accretion of drifted sand is, confessedly, of so little account, neither the "*rise of vegetable substances*" to bind the flitting mass, nor their "*decay*," to increase its bulk, can be worthy of much consideration. It is, however, only by the first of these processes that the most ardent imagination can for a moment suppose vegetation to have aided in forming any part of the Yarmouth district; for, as to vegetable mould, except where it has been artificially produced in the immediate vicinity of the town, there is about as much of it in the composition of the surrounding dunes, as there is in that of the great African Desert. The bare idea of vegetation presumes also the previous existence of a compactly settled and undisturbed bed, wherein plants may take root and flourish; neither the *Arundo arenaria*, nor any of the grasses which bind the sand on our shores, will grow in the sea, or even below high-water mark; consequently the ridges now covered with them, must have been permanently abandoned by the waves, before these plants could begin to shoot their fibres through them: and hence it follows, that even if the effects of vegetation had been so decidedly and extensively manifest, as to confirm Mr. Taylor's representations of their importance,—still they could only have been regarded as the work of a secondary agent and of a subsequent period, and they must have been left entirely out of

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the question, in an inquiry as to the *origin* of these lands, and the nature of that operation, by which they were *first* raised above the level of the sea.

With respect to the flat sandy tract between Caistor and Gorleston, I have thus demonstrated,—

First, That the wind and vegetation—the two "powerful agents," which Mr. Taylor condemns me for having altogether overlooked—have in fact contributed in a very "insignificant" degree to its formation; and

Secondly, That from their very nature, they could not have begun to act in the manner described, unless the foundation for their superstructure had been previously elevated above the highest range of the tide.

Mr. Taylor's arguments, therefore, have left unshaken all my positions; and until a stronger case can be made out against them, I am still authorized to maintain,

1. That the shore which stretches across the opening of this valley, was originally a bank thrown up by the sea in the mouth of the ancient æstuary.

2. That supposing the German Ocean to have retained invariably the same level, "its waves must always be capable of sweeping away at one time what they may have washed up at another, and that they must at least occasionally overflow the mounds which they raised." And

3. That as this portion of our coast, although "compacted and depressed by the weight of massy buildings and the traffic of a numerous and busy population," has never for many centuries been surmounted by the assailing waters, it is evident that these no longer rise to that height, to which they must have flowed at the period of its formation.

Here then we have a decided flood-guage, to which we may safely appeal, if any doubts should arise, as to the character of the phænomena of the interior valleys with which it is connected. That natural principle which I have shown to have operated in the construction of the one, must be equally applicable through the whole range of the others, and will be found to accord perfectly with all the facts that they exhibit. Alluvial ground, lying above the waters that formed it, proves that the surface of those waters has fallen: they may have been raised above their ordinary level by transient floods; but at all events they have sunk below the elevation at which they stood, when the mud was deposited, which by their subsidence has been converted into dry land. The main question, therefore, with respect to these valleys, is—has their soil been brought into them by such extraordinary inundations? or or has it been gradually spread over them, below the usual level of the waters that anciently covered their whole extent?

Mr. Taylor has appealed to the evidence given before the Committee of the House of Commons to prove, "that the chief portion of the eastern marshes is even now eighteen inches to two feet below the surface of the rivers which pass through them, and that the water is artificially kept out by embankments and draining mills." Every one acquainted with this district must be aware, that this is far too comprehensive an assertion; and I must regard it as a general inference, hastily drawn from local and limited facts. That the circumstances here described do partially exist along the course of the rivers, particularly where they approach towards the sea, is not denied; yet the slight banks which have been thrown up, must be looked upon in many instances rather as memorials of past, than as proofs of existing danger; and seem to have been more designed to guard the lands from the effects of unusually high tides, than to rescue extensive tracts, which without the protection of such barriers would be constantly overflowed, to the depth of eighteen inches or two feet. But it is certain, that for the space of several miles below the city of Norwich, there is neither embankment nor draining mill, and that the surface of the meadows is decidedly above the level of the river. Within the memory of those now living, they have advanced considerably in solidity, and the waters which in rainy seasons used to stagnate upon them, are now quickly carried away, without the assistance of artificial draining. The lowest bed of their soil is sand, in which marine shells are found; above this, the sand is mixed with black mud, and the shells that are distributed in it are both fluvial and marine: the external covering consists entirely of black mud, full of the remains of aquatic plants, and abounding in freshwater shells. In this series we may trace the successive operations by which the bottom of the valley was filled: first, the sea exclusively occupying the whole basin; then, as its tides were gradually withdrawn, the waters from inland springs, at first mingling with them, next supplying their place, and finally, settling into the contracted channel of the present river. These are proofs, not of transient and irregular inundations, but of the long residence and gradual retreat of deep waters. It is far however from my object to contend, that no portion whatever of the exterior surface of these valleys was produced by the casual and temporary floods, by which they are known to have been overspread even at no distant dates; on the contrary, I shall show that these floods have been a natural and necessary

necessary part of the regularly progressive change which is here displayed. But neither have they expelled themselves by their deposits, nor have they been of sufficient frequency and duration, to have been the only agents in raising the land of these districts above the ordinary level of their rivers *.

It is evident that the sea anciently flowed so copiously up the æstuary, as to exclude from it the animal tribes which live only in fresh waters. I am not appealing, in proof of this, to the marine exuviæ on the sides of the hills; my observations are for the present confined to those which are below the alluvial mud of the valley, where the beds of sand and the fossil spoils which they contain, attest the early and absolute dominion of the sea. How then have its tides been driven out and compelled to yield this part of their empire to the fresh-waters? Not by the increased quantity and force of the latter; and it has been already shown that they could not expel themselves. Here then again, in another stage of the same operation of Nature, no competent and satisfactory cause can be assigned for a visible and unquestionable effect, but the falling level of the sea. Those who deny the intervention of this principle, supply its place by a confused and contradictory train of causes; while by admitting its cooperation, the whole course of events, from first to last, is made clear, intelligible, and consistent. Its influence, in forming the belt of sand which crosses the mouth of the valley, has been already established by facts and inferences which no theory can overcome; and it is manifest, that the waves which surmounted that bank must have flowed up high into the interior of the country. Allowing their level to have been gradually declining, while by their successive deposits the surface of the lower beds of sand was progressively rising, the combination of these two causes must necessarily have diminished the amount of the tides that made their way into the inner valley, and will account for their having been finally altogether withdrawn. By this process, room was made for the fresh water to descend from the inland fountains; it then first brought down its mud and the spoils of its vegetable and animal tribes to mingle with the sand and shells of the saline floods; and

* Sir William Drummond states in his *Origines* (vol. ii. p. 18.), that the stratum of black earth deposited by the Nile in the Valley of Egypt, "rarely exceeds two or three feet in depth." This appears to be the whole amount of the mud produced in the course of more than 2500 years by the regular annual overflow of that vast and fertilizing stream. Therefore the average increase must have been less than an inch per century. How little then can have been added to the surface of the Norfolk valleys in the space of about 700 years, by the accidental and uncertain inundations to which they have been subject!

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at last occupied the space abandoned by the retiring sea. In this stage the superficial bed of alluvial mud began to accumulate; aquatic plants took root in it; and it was thickly inhabited by the various races of river molluscæ, that succeeded the testaceous progeny of the ocean. While the surface of this growing mass was slowly raised, that of the waters above it was gradually lowered by the falling level of the basin into which they flowed; and when by this drain the attrition of the current was brought to work upon the uncompacted body of soft mud, the streams cut out for themselves the channels of the existing rivers. Into these channels the waters settled by degrees; and as they continued to be drawn off by the depression of the sea, in time they left uncovered the level of the deposit which they had formed. But when in rainy seasons, or after a rapid thaw, they came down more copiously than usual, and in conjunction with this circumstance an extraordinary elevation of the tide offered a temporary obstacle to their passage, their outfall being thus stopped, they were turned back in their course, and necessarily rising above their common height, they spread themselves over the face of the new lands. This was undoubtedly the cause of the inundations that were so often experienced here in former times: and on the same principle the yearly overflow of the Nile has been most rationally accounted for, by ascribing it to the joint action of the torrents from the mountains of Abyssinia, and the etesian or periodical winds, the latter of which, blowing at that season invariably from the north, raise the level of the Mediterranean on the coast of Egypt, and for a time check the outflow of the swoln river. In the more consolidated and higher grounds of the Norfolk valleys, such floods have now been for many years entirely unknown; and in the lower districts they have become less frequent, less extensive, and of shorter duration. Yet while this change has been in progress, greater apparent impediments have been opposed to the departure of any superabundant waters, by the acknowledged contraction of the channel of the river, and the accretion of the bar at the mouth of the haven. The increased facility with which they have nevertheless been carried off, and the evident depression of the surface of the river under such circumstances, imply the existence of some latent cause, perceptible only in its effects, and sufficiently powerful to more than counteract difficulties, from which such opposite consequences must otherwise have resulted. That cause cannot be found in the mere unassisted "precipitation from waters charged with alluvial mud;" superadded to this, there must have been a permanent subsidence of the waters themselves, and that

subsidence

subsidence must have been connected with a corresponding fall of the neighbouring ocean. In no other way can that great reservoir have drawn off the saline, the mixed, and the fresh waters, which in succession have covered the bottom of our valleys; nor without such an internal change in its own relative state, could it have effected those revolutions, which have altered the face of an extensive district, and the progress of which may be traced alike in the memorials of nature and the records of man.

XXXVIII. *A Description of a new Genus and some new Species of Saurian Reptiles; with a Revision of the Species of Chameleons.* *By* J. E. Gray, *F.G.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

FOLLOWING the maxim of the great Linné, "Nulla dies sine lineâ," I have sent you the descriptions of a few new reptiles which are in the British Museum, along with a revision of the Chameleons, which I had found very much embroiled by the compilations of former naturalists, when I was lately engaged in naming the species in the above collection, along with some corrections of my former paper.

J. E. Gray.

Fam. IGUANIDÆ.

Leiocephalus, *n. g.*

Caput scutatum; corpus et cauda æqualiter squamosa, pori femorales nulli; digiti inæquales simplices; dentes denticulati, palatini perparvi.

Head and eyebrows shielded; body and tail uniformly scaly; femoral pores none; toes unequal, simple; teeth denticulated, palatine teeth very small.

This genus has very much the external appearance of the *Agamæ;* indeed so much, that before I was able to examine its teeth, I was led to consider it as forming a section of that genus; but the teeth, which may be considered as one of the best characters for determining the natural relations of this kind of animals, place it with the *Iguanæ.*

In this family it is most nearly allied to the second section of the genus *Lophyrus,* which appears to be the intermediate link between the true *Lophyri* and the *Leiocephali;* but it is immediately distinguished from the latter, by the head being regularly shielded, and not provided with a large single occipital perforated shield. The shields of the head are peculiar: they

they may be thus described, after the nomenclature used by the German zoologists: One rostral shield; then a group of 6 or 7 small nasal scales on each side; 2 anterior frontals; 3 posterior frontals in a cross band; 4 vertebral scales; 6 superciliary band-like scales over each eye; 10 occipital shields; the first 5 small, forming a narrow, and the hinder 5 larger and longer, forming a broad band; so that there are 8 large scales on each side, and 3 odd or medial scales, without the superciliary ones.

L. carinatus, n.

Caudâ corpore longiore; capite glabro; squamis latis lanceolatis, dorsi carinatis aculeatis, abdominis glabris vix carinatis, dorso obliquè multùm carinato.

Inhab. ———? *Mus. Britan.*

Length 8 inches; body $3\frac{1}{2}$, tail $4\frac{1}{2}$.

Tail rather longer than the body; head smooth. Scales broad lanceolate, those nearer the head the smallest, those of the back, upper part of limbs and tail strongly midribbed and ending in sharp tips, so as to form raised longitudinal lines, tending obliquely towards the tail; the dorsal line the largest. The scales of the lower parts smooth, scarcely midribbed, green marbled with brown, beneath the epidermis verditer-green.

Lophyrus, *n.*

This genus was first called *Uranascodon* by Caup, and altered to *Uraniscodon* by Boïe, who has since changed it to *Ophryesa*. It may be divided into two sections, which will probably form two genera, thus:

1. *Head and eyebrows uniformly scaly: palatine teeth large and distinct.*

This nearly agrees with the *Lophyrus* of Spix, and contains 4 or 5 species figured by Spix, in t. 10, 12, 13 and 13 *a*.

2. *Head scaly with a large occipital subperforated scale; the eyebrows shielded; the palatine teeth very small* (or none).

This agrees with the *Agamæ* of Spix. It contains the *Agama hispida*, *A. tuberculata*, *A. nigricollis*, and *A. Cyclurus* of Spix, t. 15, 16, 17, said to be different ages and sexes of the same species; and *Lophyrus ochro-collaris*, Spix, t. 12. f. 2.

This section appears by the superciliary scales to be intermediate between the true *Lophyri* and the *Leiocephali* which I have just described; and should it hereafter be found to be a genus, Boïe's name of *Ophryesa* may be used to designate it.

The following apparently new species belongs to this section. It is very nearly allied to the *Agamæ* in external appearance: I therefore propose to call it

L. (*Ophryesa.*) *Agamoides*, n.

Dorso

Dorso vix cristato, colli lateribus pone aures fasciculis quatuor spinarum trihedrarum utrinque; squamis capitis convexis, supra aures acuminatis, dorsi parvis carinatis aculeatis, membrorum caudæque paulò majoribus, abdominis lævibus; superciliis carinatis.

Inhab. ——— ? *Mus. Britan.*

Length of body 5 inches; tail —, injured.

Back slightly crested; sides of the neck and behind the ears with four tufts of trihedral spines on each side; scales of the head convex, those over the ears pointed; the scales keeled, sharp tipt; those of the back, small; of the limbs and tail, rather larger; those of the belly and beneath, armless; eyebrows keeled, with four superciliary scales on each side.

Zonurus.

From a re-examination of a specimen in spirits, I find that the teeth of this genus are placed on the inner side of the jaws, and therefore it ought to be referred to this family. I was misled by seeing only a dried specimen, and by Cuvier's expressly describing the teeth as in *Agama*. I may observe, that the best way to examine the structure of. the teeth is to take a needle and turn down the inner gums from the edge of the teeth; when if they are placed on the inner edge of the jaw, their roots will be distinctly seen; while if they are only on the edge, the bone of the jaw will be smooth and of a uniform colour.

The genus should be arranged after *Cyclura*, having no palatine teeth and being provided with femoral pores: but it may be easily separated from it, by the large size and peculiar form of the shields of the head.

Fam. IV. CHAMÆLEONIDÆ.

Gen. Chamæleon.

Chamæleon vulgaris, Daud.

Superciliis cristatis, occipite cristato, squamis parvis uniformibus, capitis lineæ dorsalis et ventralis a mento usque ad anum majoribus, lineâ ventrali albâ.

Lacerta chamæleon, *Linn. Syst. Nat.*

Chamæleon vulgaris, *Daudin Rept.* iv. 181.

——— mutabilis, *Meyer Syn. Rept.* 27. n.

Egyptian Camelion, *Walcot Exot. Anim.*

Icon *Müller Natur. Syst.* iii. t. 12. f. 4. *Leske Naturgesche*, t. 7. f. 3. *Borowsky Theire* iv. t. 4. *Meyer Thiere*, t. 57. *Knorr, Del. Nat.* ii. t. 55. f. 2. *Lacepede, Hist. Quad. O.* ii. t. 3. *Walcot Exot. Anim.* t. *Prosper Alpin. Hist. Egypt.* t. 9. f. 2, bad. *Bosman, Guinea*, t. at p. 252. f. 6, 7?

 1. Cha-

1. Chameleon. *Perrault, Mem. Acad. Par.* 1699. i. t. 25. cop. *Prosper Alp. Hist. Egypt.* t. 10.
   Chameleo Parisiensis *Laur.*

2. *Seba Thesaurus* t. 82. f. 1. from which is described
   Chamæleon Mexicanus *Laur. Rept.* 45.
   Lacerta Chamæleon β. *Gmel. Syst. Nat.* i. 1069.
   Chamæleon calcaratus, part. *Merrem Rept.* l. c.

3. *Seba Thesaurus* t. 82. f. 2. cop. in *Ency. Meth.* t. f. 2. from which is described, perhaps altered from *Perrault,*
   Chamæleon carinatus. *Merrem Rept.* 162.

4. *Seba Thes.* i. t. 82. f. 3. from which is described
   Chameleon Zeylanicus. *Laur. Rept.* 46.
   ———— Senegalensis var. 2. *Daud. Rept.* 201.
   ———— subcroceus. *Merrem Rept.* 162.

5. *Seba Thes.* i. t. 83. f. 4. from which is established
   ? Chameleon vulgaris var. 1. *Daud. Rept.*
   ? ———— Africanus, *Laur. Rept.* 46.
   ? Lacerta Africana, *Gmel. Syst.* i. 1069.
   ? Chameleon calcaratus, part. *Merrem.* l. c.

6. Chamæleon candidus, *Laur. Syn.* 46.
   Lacerta Chameleon, γ. *Gmel.* l. c. 1069.

? 7. Chameleon trapu, *Geoff. Rept. d'Egypt.*

8. Anatomy. 1. *Perrault Mem. Acad. Sci. Par.* iii. t. Trans. *Pitfield Nat. Hist.* p. 16, and *Blasius Anat. Anim.* t. 14; skeleton copied.

Seba Thesaurus, i. t. 82. f. 6. copied in *Daudin Reptiles,* i. t. 2. f. 1.

Cuvier Oss. Foss. v. ii.

Inhab. Africa and India.

Length 22 inches; body 11, tail 11.

Head over the eyes keeled; occiput crested; crest reaching to the middle of the space between the eyes; scales small, uniform with those of the head; the dorsal and medial ventral line from the chin to the vent larger, naked; the parts denticulated, the ventral line white, back of the head furnished with a slight lobe covered with larger scales, not always evident in the dried specimens; but never so distinct as in *C. dilepis.*

Most of the species have been described from Seba's figures, which appear all to be referable to this species, although he gave them as coming from various parts of the world: but no reliance can be placed in his habitats. *Seba,* i. t. 82. f. 2. and 83. f. 4. has figured and added a sixth toe, which caused Merrem to place them together under the name of *C. calcaratus;* but this appears to be only a character of what is often found

found in specimens preserved in spirits, that the sole is slightly produced behind. *Seba*, i. t. 82. f. 1. and f. 3. appear to have been drawn from specimens in spirits, while f. 2. is evidently the same kind of animal which had been dried. The figure of Seba, i. t. 83. f. 4. is the most doubtful; but the figure is difficult to understand, and it certainly more resembles this species than any that I am acquainted with. The *Chameleon candidus* of Laurenti, is only a bleached specimen. The *Chameleon trapu* of Geoffroy I only know by the figure, which differs from my specimens in some particulars, and resembles *C. dilepis*, but the scales of the head appear rough, and the lobes on the side of the face are not large enough for that animal. None of these figures give any idea of the animal in its living and healthy state, when all the angles of the ridges appear by the drawing which I have seen of them to be abolished.

Chamæleon pumilus Laur.

Superciliis cristatis; occipite carinato; squamis parvis; dorso utrinque scutis ovatis sparsis; capite tuberculato, areis duobus lævibus ovalibus utrinque supra dorsum; caudæ basi, mentoque cristatis.

Chamæleon pumilus, *Laur. Rept.* 46.

——— Bonæ Spei, *Laur.*

Lacerta pumila, *Gmel. Syst. Nat.*

Chamæleon margaritaceus, *Merrem Syn. Rept.*

Icon *Seba*, *Thesaurus*, i. *t.* 82. *f.* 4, 5. bad. *Daud. Rept.* iv. *t.* 54. good.

Inhab. Cape of Good Hope.

Length 5 inches; body $2\frac{1}{2}$, tail $2\frac{1}{2}$.

Eyebrows crested, occiput keeled, scales square with scattered ovate shields on each side of the back. Head tubercular, with two oval smooth places on each side; back part of the tail and chin denticulated.

Cham. dilepis, Leach.

Superciliis cristatis, occipite depresso, lobo lato pyramidali utrinque posticè instructo, squamis magnis rugosis, capitis dorsi gulæ ventrisque carinis magis denticulatis, occipitis loborumque squamis latis hexagonis planis lævibus.

Chamæleon dilepis, *Leach*, *Bowdich's Ashantee*, App. 493 (1819).

——— planiceps, *Merrem Rept.* 162. not Synom. (1820).

——— bilobus, *Kuhl Beitr. Zool.* 104.

Inhab. Africa. Fantee, *Capt. Maryatt*, R.N., Gamboon, *T. E. Bowdich.*

Length 13 inches; body $6\frac{1}{2}$, tail $6\frac{1}{2}$.

Eyebrows crested, occiput depressed pyramidical with a broad lobe on each side behind; scales large, rugose, ridges on the head, back, throat and belly larger, denticulated; the scales on the top of the head and the occipital side-flaps broad, hexagonal, flat and smooth.

In the Museum is another specimen of a smaller size, which agrees with the above, but the flaps on the side of the head are small. It may be the other sex, or the young; it is 8 inches long.

Kuhl in his *Beitrage*, and Caup, in his paper in the *Isis*, speaks of a Chameleon under the name of *C. bilobus* of Leach. The only one which I am aware that Dr. Leach has described is the above, which certainly agrees with Merrem's description, but not with his references.

Chameleon Senegalensis.

Superciliis cristatis denticulatis; occipite plano, posticè convexiusculo; squamis parvis uniformibus granulatis, capitis paulo majoribus ovatis planis; nuchâ, gulâ, abdomineque denticulatis.

Lacerta Chameleon, *Shaw. Zool.* iii. 253.
Chamæleon Senegalensis, var. 1. *Daud. Rept.* iv. 209.
——— Gymnocephalus, *Caup. Isis*, 1825.
Chameleon de Senegal, *Cuv. R.A.* ii. 52.
Icon. *Shaw. Miller. Cy. Phys.* t. cop. *Shaw. Zool.* iii. *t.* 76. *Seba*, i. *t.* 83. *f.* 5? *Skeleton*, Ency. Method. *t.* *f.* 1.

Inhab. ———?

Length 8 inches; body 4, tail 4.

Eyebrows crested, denticulated; occiput flat, behind rather convex, not denticulated, bony; scales small, uniform, granular; scales of the head rather larger, ovate, flat; back of the neck, throat and belly, denticulated.

The three bony processes of the occiput converge, but they do not unite together at their tips, as may be seen by the larger scales with which they are covered.

Chameleon bifurcus, Brong.

Superciliis arcuatis, occipite lunato obliquè cristato, facie ante oculos in lobos duos lanceolatos producto; squamis planis, quadrangularibus capitis majoribus sexangularibus superciliis, crista occipitali nuchâque denticulatis.

C. bifurcus, *Brongniart, Bul. Soc. Philom.* iv.
C. bifidus, *Daud. Rept.* iv. 217.
Icon. *Brongniart, Bul. Soc. Phil. t. f. Daudin. Rept.* iv. *t.* 54. very good. Head, *Cuvier, Oss. Foss.* v. f. 32, 33.

Inhab. Java. *Mus. College of Surgeons, Mus. Britan.*

Length 10 inches.

Eye-

Eyebrows arched; occiput lunate, obliquely crested; the face between the front of the eyes and the tip of the nose extended into two long lanceolate lobes; scales flat, quadrangular, rather large; scales of the head, especially those of the forehead, lobes and occiput, larger, hexangular; edge of the eyebrows, occipital crest, and back of the neck denticulated; the scales on the side of the belly united into a group so as to form an irregular whitish band on each side.

Chamæleon Parsonii, Cuvier.

Superciliis arcuatis; occipite obliquè maximè cristato, facie ante oculos in lobos duos breves compressos serratos producto; squamis parvis quadrangularis, congregatis, capitis majoribus sexangularibus.

Cameleonis rarissima, &c. *Parsons, Phil. Trans.* lviii. 195. *Naturforscher. Ic.* v.

Chamæleon Parsonii, *Cuv. Oss. Foss.* v.

Lacerta Chamæleon, δ. *Gmel. Syst. Nat.* i. 1069.

Icon. Parsons, Phil. Trans. lviii. *t.* 8. *f.* 1. 2. cop. *Naturforsch.* v. 184. *t.* Head. *Cuvier, Oss. Foss.* v. *t.* 16. *f.* 30, 31.

Inhabits Africa. *Mus. Coll. of Surgeons.*

Length 12 inches.

Eyebrows arched; the occiput obliquely expanded over the back of the neck, above flat; the front of the head over the nostrils expanded into two short compressed serrated lobes; scales of the head large and hexangular, smooth, those of the body small quadrangular, separated by the wrinkles into ovate groups, back not crested with a central row of larger scales.

This species is easily known from the former by the large size of the occipital crest, and by the dentated nasal processes.

My late friend Dr. Kuhl, in his *Beiträge zur Zoologie* has described two other species, which I have only casually seen in the French Museum.

1. *Chamæleo Tigris.*

Obscurus, rufescens, totus maculis nigris adspersus, labiis albescentibus; corpus gracile, capite parvo, lineis tuberculorum duabus pone oculos in unam medianam parum elevatam confluentibus; frontali utrinque cum occipitali confluentibus.

Chamæleo Tigris, *Cuvier Mss.* Kuhl *Zool. Beitr.* 105.

Icon ———— ?

Inhab. ———— ? Mus. Paris.

2. *Chamæleo Seychellensis.*

Rufescens, crista dorsali et abdominali nulla; collo anticè serie longitudinali mediana appendicum ad duodecim, quarum

anterior

anterior major serrata lobiformis; capite cristis osseis quatuor tuberculatis, oculi margine superiori posteriori et inferiori semicirculo tuberculorum osseorum cinctâ.

Chamæleo Seychellensis, *Peron Mss.* Kuhl *Zool. Beitr.* 105.

Icon. ———.

Inhab. Seychells Islands. *Peron Mss.* Paris.

Fam. AGAMIDÆ.

Gen. Agama.

§ 1. *Scales keeled; those of the head similar. Tail uniformly scaly.*

A. *subspinosa*,

Cauda corpore ferè duplo longiore; nucha carinis spinosis; squamis parvis, dorsi scabris, occipitis magnis ovatis imbricatis glabris, membrorum externè caudæque præsertim majoribus carinatis aculeatis.

Inhab. ——— ? *Mus. Britan.*

Length 8 inches; body 3, tail 5.

Tail nearly twice as long as the body; neck with spinous ridges, scales small, those of the upper part of the body rough, upper part of the head large ovate imbricate smooth; those of the outer sides of the members, especially of the tail, larger, keeled and sharp tipt.

Most like A——— *n.* but the tail is shorter, and the scales of the head are smooth and white, while in the latter they are tubercular, and the legs are more truly spinous.

§ 2. *Scales keeled, the head with a large occipital scale; tail uniformly scaly.*

A. occipitalis.

Cauda corpore duplo longiore; nucha spinosa; squamis latis ovato-lanceolatis; aculeatis, caudæ membrorumque majoribus, abdominis parvis quadrangularibus glabris, capitis plerumque glabris.

Inhab. ——— ? *Mus. Britan.*

Length 10 inches; body $3\frac{1}{2}$, tail $6\frac{1}{2}$.

Tail twice as long as the body; neck with scattered groups of spiny scales; scales broad, ovate-lanceolate, sharp tipt, those of the tail and outer side of the limbs larger and more prickly; those of the belly small, smooth, quadrangular; those of the head mostly smooth, the largest over the eyes, with a long central vertebral scale between the nostrils and another over the occiput. The scales of the two central series beneath the tail smallest and unarmed.

British Museum, July 1, 1827.

XXXIX. *No-*

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 21. THOMAS TELFORD, Esq. was admitted a Fellow of the Society; and the reading of a paper "On the theory of the diurnal variation of the needle," by S. H. Christie, Esq. F.R.S. commenced on June 14, was resumed and concluded.

Mr. Christie having been led to doubt the validity of the moving easterly variation adopted by Canton, but at the same time having observed that the changes in direction and intensity appear always to have reference to the position of the sun with regard to the magnetic meridian, was led to connect these phænomena with Professor Seebeck's discovery of thermo-magnetism, and Professor Cumming's subsequent experiments; and to refer the phænomena of diurnal variation to the effect of partial heating modified, perhaps, by that of rotation and by peculiar influence in the sun's rays.

In support of this opinion he cites passages from papers by Professor Cumming and Dr. Traill, whom a similar idea appears also to have impressed. But in place of looking to the stony strata of which the earth's surface consists as the elements of the thermo-magnetic apparatus which this doctrine requires, the author regards them as rather consisting of the atmosphere and the surfaces of land and water with which it is in contact. Thermo-magnetic phænomena, he remarks, have hitherto only been observed in metallic combinations; but this may be owing merely to the small scale on which our experiments are conducted.

To put to the test of experiment whether thermo-magnetism could be excited when the surfaces of two metals instead of touching at one point were in symmetrical contact throughout, the author first employed a compound ring of bismuth and copper, the copper outwards; and he found that to whatever point heat was applied, magnetic powers were developed; a needle being affected differently according to the different positions in which the ring was placed with regard to it. After a lapse of two years from this first experiment, the author resumed the inquiry with an apparatus consisting of a flat ring of copper having its inner circumferences grooved and united firmly by soldering and fusion to a plate of bismuth cast within it; the whole forming a circular plate twelve inches in diameter, weighing 119 ounces Troy, which was made to revolve in its own plane.

Heat was applied by a lamp to a given point in the circumference of this plate, and a delicately suspended needle partly neutralized, was placed near it, and the deviations observed in all positions of the heated point, which was made to revolve; the lamp being withdrawn. These experiments led him to conclude that the effect of so heating a portion of the circumference, was to create a temporary polarity in the plate, the law of which he explains. He

He then details a set of experiments by which he assured himself that a uniformity of action obtained wherever in the circumference the heat was applied. He next instituted a series of observations for determining the laws which govern the magnetic phænomena resulting from the application of heat as above described; the results of which are stated in the form of tables.

Four poles appear to be produced, two north and two south, the north both lying in one semicircle, and the south both in the other, and not in alternate quadrants, and all the poles lying rather nearer to the centre than the line of junction of the two metals. The experiments were pursued in a variety of positions of the plate with respect to the meridian and horizon, and with a similar general result.

From these experiments the author concludes that uniformity of junction of the two surfaces of a thermo-magnetic combination is no obstacle to the development of transient polarity. Regarding the earth and its atmosphere as such a combination, and limiting our views to the intertropical zone alone, we should have two magnetic poles produced on the northern, and two on the southern sides of the equator, the poles of opposite names being diametrically opposite to each other.

To apply this to the earth, it is necessary to know the times of greatest heat in the twenty-four hours: this may be assumed at three o'clock in the afternoon. The apparatus used by the author not affording, when adjusted to the latitude of the place, sufficient magnetic power to render the effects distinct, he substituted for it an artificial imitation consisting of two magnets, six inches long, so placed with respect to a revolving axis parallel to the axis of the earth, as to imitate the position of the poles produced by thermo-magnetism in his plate, and making the apparatus revolve round this axis, he noticed the deviations produced thereby on a compass, placed horizontally over it. These deviations he then compares at length, with those actually observed first by Lieut. Hood, in 1821, at Fort Enterprize, lat. 64° 28′ N.; 2dly, by Canton, in London, in 1759; 3dly, by Lieut. Foster, at Port Bowen, in 1825; 4thly, by Col. Beaufoy, on Bushy Heath, in 1820. The results of this comparison are on the whole, generally such as to indicate a conformity between the hypothesis and fact, with the exception of some deviations from the exact times of maximum and minimum variation which could not but be expected.

The author then considers the manner in which the distribution of land and sea over the globe modifies the point of greatest heat, and in consequence the place of the diurnal poles. He next observes, that at the commencement of the experiments, he had no idea of being able to reduce the deviations of the needle to so simple a law as that resulting from a polarity in a particular direction communicated to the plate; but that he considered it of the greatest consequence, to ascertain whether the deviations on the outer edge of his plate, had the same general character with those within, at the time of junction of the metals; since these situations of the needle

needle would correspond to great elevations in the atmosphere, and points near the earth's surface, respectively, the character of the deviations turns out to be the same in both cases, so that in this respect the hypothesis, so far as is known, agrees with observation.

One general effect of some experiments with a hollow copper shell filled with bismuth, afforded a striking correspondence with nature. The whole equator being heated, and one part more than the rest, he uniformly found that the elevated pole being towards the north, the north end of the needle deviated when the place of heat was on the meridian above the horizon, and south when below, which is precisely the character of the diurnal variation in north latitudes.

A paper "On the variation of the needle," by Capt. Sabine, F.R.S., and another "On a new vegetable principle," by Mr. Frost, F.L.S., having been read, the Society adjourned over the long vacation, to meet again on the second Thursday in November next.

GEOLOGICAL SOCIETY.

May 18.—G. J. Roupell, Esq. M.D. of Caroline Street, Bedford Square; and Isaac Lyon Goldsmid, Esq., of Dulwich Hill House, Camberwell,—were elected Fellows of the Society.

A notice was read "On a Whin dyke in Cooper Colliery, near Blythe, Northumberland," drawn up from the information of Mr. Bryham, agent at the Cooper Coal Works, by W. C. Trevelyan, Esq. F.G.S. &c.

The total length to which this dyke has been traced is 1577 yards. It increases in breadth from S. to N.; being $4\frac{3}{4}$ yards wide near the most southern point, where it has been cut through, and $21\frac{1}{2}$ yards wide at the most northern spot hitherto observed. It is formed of two walls of greenstone, each from two to four feet in thickness; and these walls contain between them a breccia, composed of fragments of shale and whin, cemented by calcareous and argillaceous matter. Carburetted hydrogen and pure water issue from a narrow fissure in the broadest part of the dyke. The coal of the beds through which the dyke passes is charred, and deteriorated in quality, to the distance of about forty yards on each side.

The reading of a paper was begun, "On the fixed rocks of the Valley of the St. Lawrence, in North America," by John J. Bigsby, Esq. M.D. F.G.S. &c.

June 1.—Henry Campbell White, Esq. of Comer-Hall, Hemel Hempstead; and Samuel Sharpe, Esq. of New Ormond Street, London,—were elected Fellows of the Society.

The reading of Dr. Bigsby's paper, begun at the last meeting, was concluded.

The observations of the author in person were made principally in the Canadas, and on the northern shores of the great Lakes; and he connects with them a sketch from various authorities, of the regions which border the Valley of the St. Lawrence upon the S.W. and the lakes on the south and west of Upper Canada.

The north-western side of the St. Lawrence Valley consists

 princi-

principally of an arm of the primitive ranges which extend from Labrador and Hudson's Bay to the sources of the Mississippi: and from this, at the outlet of Lake Ontario, a band is sent out across the Valley of the St. Lawrence to join the primitive formations of the United States. Numerous boulders of a limestone resembling the mountain limestone of England, are found on the north shore of Lake Erie; and this with other rocks in horizontal strata appears *in situ* at Lake Huron: the line of junction with the primitive rocks extending from Penitanguishene to Kingston, thence up the Ottawa to the Falls of the Chat, and the Longsault Rapids, from whence it stretches north-easterly to Cape Tourment in the north bank of the St. Lawrence 30 miles below Quebec.

The strata which overlie the transition rocks, in the St. Lawrence Valley, are, in a descending order, the following:—

1. Dark shale resting upon limestone, and containing terebratulæ, favosites, turbinolia, milleporites, trilobites, &c.; this extends for many miles, along the south of Lake Ontario and the south-eastern shore of Lake Erie.

2. Cherty limestone, beneath which is blue limestone with copper pyrites, and foliated strontian;—this last containing producti, and corallines, in addition to the fossils above enumerated. The brown limestone of Niagara contains cellular madrepores, pentameræ, trochi, trilobites, &c.; and the junction of this limestone with the shale is well seen beneath the table rock of the Niagara Falls. The shale on the south of Lake Ontario is from 120 to 250 feet in thickness. Its place is superior to that of the muriatiferous sandstone: and in this respect the author conceives the order of stratification here to be distinguished from that which obtains in Europe; since the same fossil remains have not yet been found in Europe above the saliferous sandstone.

3. Arenaceous rocks, in the lower beds of which are brine springs. The stratum which forms the floor of the salt springs on the south borders of Ontario, varies from a red or greenish sandstone to a greenish or red clayey slate; and is occasionally 80 feet in thickness.

4. Another group consists of a quartzose aggregate, from 40 to 60 feet thick, resting on grauwacke, either fine-grained or slaty; the finer varieties containing the asaphus latocaudatus, bellerophon, and a bivalve resembling a sanguinolaria. No coal has been found in this vicinity.

5. Another portion of the stratified rocks is ranked by the author with the intermediary limestone of Daubuisson; the higher beds containing organic remains resembling those of the transition limestone of Germany and Wales; while none of the organic remains of the superior deposits are found in it. It occurs in many parts of Lower Canada, on the northern shore of Lake Ontario, Lake Simcoe, Lake Huron, and Lake Superior.

Near the outlet of Lake Ontario, cliffs upwards of 100 feet in height are formed of sandstone, grauwacke, and conglomerate; and for many miles down the St. Lawrence these rocks underlie the intermediary limestone. At the Falls of Montmorency near Quebec, the conglomerate

conglomerate rests upon gneiss and other primitive rocks; but at Malbay it is interstratified with brown limestone, and contains spiral univalves and various bivalves*. The author is inclined to refer the formation to the old red sandstone.

The porphyries of Gros Cap and Nipigeon on Lake Superior, contain agate, chalcedony, fluor, green earth, and vitreous felspar: they are unstratified, and form serrated precipices. Near Gravel Point they much resemble some of the porphyries of Arran in Scotland.

The rocks of the St. Lawrence Valley, beneath the series above mentioned, consist, in a descending order, of grauwacke, intermediary limestone, quartz-rock, primitive limestone, and various slaty rocks,—including gneiss, mica slate, actinolite slate, with syenite, greenstone, and ophicalcic rock. The prevailing direction of the strata, for more than 1000 miles, from the River Saguenai, on the north of the St. Lawrence, to the northern shore of Lake Huron, is to the N.E.; and the rocks are regarded by the author as the most recent of the primitive class. Quartz-rock prevails on the north of Lake Huron for more than 70 miles; and the islands in that part of the lake consist of fine-grained red and gray granite, with quartz-rock and trap: and vast masses of granite alternate with greenstone for a space of 300 miles on the north shore of Lake Superior. Of the slaty primitive rocks, the most abundant is gneiss; which constitutes some of the principal heights, and forms the mountains N.E. of Quebec, and lines the northern shore of the St. Lawrence. Cape Tourment, 1800 feet in height, consists of this rock; so also the outlet of Lake Ontario, and it skirts the north shore of Lakes Simcoe and Huron, and occupies a considerable tract on the north of Lake Nipissing, and at the upper part of the river Ottawa.

The author supposes that the numerous boulders of Labrador felspar on the shores of Lake Huron, on the S.W. of Lake Simcoe, and even so far eastward as the outlet of Lake Ontario, have been derived from a tract about 60 miles west of Penetanegeneshene, where the gneiss passes into Labrador felspar, traversed by veins of pyroxene and garnet; and this he supposes to be the southern verge of a vast tract of the same composition. Magnetic iron ore is associated with syenite on the north of Ontario. Greenstone occurs in veins in Lower Canada: near Lake Huron it supports intermediary limestone; and it is found at Gros Cap in Lake Superior, and forms numerous dykes of great size in the north shore of that lake. A mass composed of a mixture of augite and hornblende occurs near Montreal, constituting Montreal Hill, 650 feet high, from which numerous dykes cut through the shelly deposits at the base of the hill.

The primitive limestone appears in every part of the St. Lawrence Valley to belong to one and the same epoch, and occupies a considerable space on the south-western frontier of Lower Canada, near Lake Champlain. In Upper Canada, the upper part of the river Ottawa has its course through this rock, and considerable masses of it occur in Crew Lake: the same white marble is seen at Lake Chat, and

* Some of the fossils of this formation have been figured in the Geological Transactions, 2nd Series, Vol. I.

on the left of Lake Chauclière, on the river Calumet and on the river Gauanoque, about 18 miles below Kingston; it is blended with serpentine.

June 15.—The Hon. William Francis Spencer Ponsonby, of St. James's Square, London; William Terry, Esq. of High Wycombe, and Duke Street, St. James's Square; the Rev. Richard Gwatkin, B.D. Fellow and Tutor of St. John's College, Cambridge; the Rev. George Peacock, M.A. F.R.S. Fellow and Tutor of Trinity College, Cambridge; the Rev. Julius Charles Hare, M.A. Fellow of Trinity College, Cambridge; the Rev. John Hutton Fisher, M.A. Fellow of Trinity College, Cambridge; the Rev. Richard Sheepshanks, M.A. Member of the Astronomical Society, and Fellow of Trinity College, Cambridge; and Major General Sir John Malcolm, G.C.B. F.R.S. &c.,—were elected Fellows of the Society.

A notice was read, "On some fossil bones of the elephant and other animals, found near Salisbury:"—by Charles Lyell, Esq. F.R.S. F.G.S. &c.

Bones and teeth of the elephant, rhinoceros, and ox, have been found for many years past in the brick-earth at the village of Fisherton Anger, at the distance of about $\frac{3}{4}$ of a mile from Salisbury Cathedral. Several pits sunk in this brick-earth show that it varies in thickness in different places from about 10 to 20 feet. It bears every mark of a tranquil sedimentary deposit from water; but the laminæ are sometimes divided by thin layers of fine sand, or occasionally, but rarely, by a layer of small flint pebbles. There are no marine remains; but land-shells are said to occur sometimes in this deposit. The brick-earth rests upon a bed of chalk flints, the greater part of which are not water-worn: and beneath these is chalk, which is loose and rubbly in the upper part.

This brick-earth is not connected with the alluvial soil of the present valley, but appears to have been deposited when the valley was at a higher level; for it forms a low terrace, along the side of the river Wily, between Salisbury and Wilton, rising 30 or 40 feet above the present water-meadows. It is necessary at least to suppose that when these beds were accumulated, the water rose much higher than it now does.

The bones are in a very decomposed state, but have no appearance of having been rolled; they are found in the lower part of the brick-earth, and not in the subjacent flint gravel. And in one spot there is reason to believe that the remains of an entire skeleton of an elephant might have been procured.

A paper was read, entitled "Remarks on some of the strata between the chalk and the Kimmeridge clay, in the south-east of England:"—in a letter to Charles Lyell, Esq., from Wm. Henry Fitton, M.D. P.G.S. &c.—The objects of the author were; first, to ascertain in the interior, the existence of that remarkable group of strata, which on the coast has been found to include the remains of organized bodies supposed to belong to freshwater; and secondly, to trace along the western boundary of the chalk the strata which immediately succeed it. For the latter purpose, he gives a series of sections at right angles to the outcrop of the chalk, on the boundary of that formation

formation passing from the coast of Dorsetshire, round the Blackdown hills in Devonshire, and thence by the vales of Wardour, Warminster, and Pewsey, through Oxfordshire, Buckinghamshire, Bedfordshire, &c. to Hunstanton Cliff on the coast of Norfolk, where the course of the chalk range is interrupted by the sea. These sections prove that the order of the strata is throughout the same as in the Isle of Wight, and in Kent, Surrey and Sussex;—and the paper describes the principal variations in the proportions and characters of the beds, at the site of the several sections.

In proceeding westward from the Isle of Wight, the beds which intervene between the chalk and the Purbeck limestone appear to run together; and cannot well be distinguished further west than Lulworth Cove. Beyond that point no trace has yet been detected of any of the freshwater beds beneath the lower green-sand; nor is the separation of the upper from the lower of these sands by a stratum of clay (Gault) any longer discernible. Some fossils, however, of the gault occur in the *sands* on the coast near Lyme Regis, and at the well-known quarries of Blackdown; and the presence of the gault itself beneath the upper green-sand is again distinct in the Vale of Wardour, and throughout the entire range from thence to Norfolk.

The only places in which the author has detected the presence of the freshwater beds succeeding the lower green-sand, are in the Vale of Wardour, and in the vicinity of Aylesbury: and it would appear that the great extent of the sands immediately beneath the chalk, shooting out beyond the subjacent strata, and concealing their outcrop, may be one cause why the group next in succession is but rarely visible in the interior;—though it is also probable that strata produced at the bottom of freshwater-lakes, or of æstuaries, were originally deposited in detached portions, comparatively of no great extent.

In the Vale of Wardour, the series consists of,—1. Chalk; 2. Upper green-sand; 3. Gault; 4. Traces of the lower green-sand (Shanklin sands); 5. Traces of the Hastings sands; 6. the Purbeck strata,—containing in great abundance freshwater shells, principally of the genus Cyclas, and in the upper part the Cypris faba: which remarkable fossil therefore pervades the whole group between the lower green-sand and the Portland stone; 7. Calcareous strata, containing the fossils of the Portland stone, and of the same mineralogical character with the beds of that formation in the Isle of Purbeck; 8. Clay, like that of Kimmeridge, &c.

The succession in the vicinity of Aylesbury is nearly the same with that of the Vale of Wardour; the Portland stone being covered at Whitchurch by beds of whiteish fissile limestone, containing freshwater shells, among which are Cyclades, and a species of Cypris. The Portland strata occur also at Brill Hill in Buckinghamshire, and at Garsington in Oxfordshire; and the remarkable nodules of Shotover-hill, though differing considerably in appearance from the limestone of Portland itself, must probably be referred either to that formation, or to a group of strata which, from their abounding in green particles, might be confounded with some of the calcareous beds of the lower green-sand, but which, both in Buckinghamshire and

and on the coast of the Lower Boulonnois, occur beneath the equivalent of the Portland stone.

At the close of this meeting, which terminated the session, the Society adjourned till Friday evening, the 2nd of November.

ASTRONOMICAL SOCIETY.

June 8.—There was read a paper by F. Baily, Esq. "Remarks on the Astronomical Observations of Flamsteed." The object of this communication is to draw the attention of astronomers to the observations of that eminent observer, which the author considers, from their great number, from the care bestowed upon them, and for the very long period elapsed since they were made (nearly 140 years), to be deserving of more strict examination than they appear yet to have received. The imperfection of the instruments used by Flamsteed appears to have been one chief cause of this neglect, being in fact only a mural arc of 120° with cross wires in the focus of the telescope fixed in the plane of the meridian, and an uncompensated clock. The arc, though of 80 inches radius, could be read off only to 10″, or at most by an estimation of proportional parts to 5″. With these however, imperfect as they necessarily were, he succeeded in constructing a catalogue of the mean right ascensions and declinations of about 3000 stars, which has served as a basis for the observations of all subsequent astronomers.

This catalogue was of course very inadequately reduced. Aberration and nutation were unknown in Flamsteed's time, and refraction very imperfectly; so that the catalogue, as it stands, cannot be considered as at all fairly representing the observations. It was, however, for many years the only catalogue in the hands of astronomers, till it gave way to Mayer's, which being deduced from observations made with better instruments, and by an astronomer of great ability, was held more worthy of confidence. But, Mr. Baily observes, the observations of Mayer were made in precisely the same way as Flamsteed's, the right ascensions being determined by the time of the stars passing the vertical wire of the telescope of a mural quadrant. His clock, however, was compensated, and his arc better divided, and no doubt better adjusted. More confidence therefore is to be placed in any *one* observation of Mayer's than in any corresponding *one* of Flamsteed's. But the number of Mayer's stars is not above one-third of Flamsteed's; the greater part were observed only *once*, and not above fifty so often as six or seven times. Whereas Flamsteed observed most of his stars several times; above a tenth part more than ten times; many of them more than fifty, and some even more than 100: thus *ν Geminorum* was observed 103 times; *Spica* 108; *γ Geminorum* 124; *Aldebaran* 125; *Regulus* 133; *η Geminorum* 163, and *μ Geminorum* 193 times. It must be evident then, that much that is defective in an instrumental point of view, must be compensated and rectified by the frequency of the observations; and, although eclipsed by the catalogues of Bradley and Piazzi, the author, considering the great age of Flamsteed's, regards it as possessing a degree of interest, in common with all ancient and authentic records, which ought never to be lost sight of.

He

He therefore conceives that it would be an object highly worthy the attention of astronomers to re-examine and re-reduce the original observations of Flamsteed, with the aid of all the accessions of accuracy which modern improvements have introduced, in the determination of the corrections. The chief difficulty would lie in the corrections for the barometer and thermometer, which Flamsteed has not recorded. But he remarks that, should no registers of these instruments at that period and in that district be found elsewhere, no great error would arise from employing a mean state of the barometer, and adopting a scale of temperature deduced for each day in the year from the meteorological journal of the Royal Society, from a mean of several years by night as well as by day.

It is not known at what precise period Flamsteed's catalogue was constructed; his observations were made between 1689 and 1719, and the reductions were performed between 1696 and 1708. But, besides the observations reduced and published in his catalogue, there exist in his journal a great many which have never yet been reduced at all. Miss Caroline Herschel has discovered no less than 560 of this kind, and these are probably not all. These ought to be included in any re-computation which may be set on foot.

One peculiarity in Flamsteed's mode of observation is to be considered as fortunate, not only as affording facilities for reduction, but as producing a probable exemption from many instrumental errors; viz. that it appears to have been his practice to observe in zones. Still the task of reduction could not but be considerable. Mr. Baily, however, conceives that great benefit could not but result were it only partially executed; reducing 200 or 300 of the principal stars from the *total* observations of each star. If this were done, it would authorize a fair judgment of the value of the whole mass; and were such partial reduction attended with satisfactory results, he thinks no doubt could exist that public spirit enough would be found to support and carry through the bold and difficult undertaking he suggests. To any one who may be inclined to make the preliminary trial, he recommends as a useful aid Miss Herschel's "Catalogue of Stars taken from Mr. Flamsteed's Observations," published by the Royal Society in 1798, which contains a list of all the stars observed by him, but not in his catalogue, as well as an index to every observation of every star, and a copious list of errata.

Mr. Baily concludes by expressing his hope that these remarks may attract the attention of some astronomer who has both talent and leisure for the pursuit of an inquiry of this kind, and who may thus become the means of restoring Flamsteed's observations to the high rank they once held, and to which it is hoped they will still be found justly entitled.

Mr. Thomas Taylor, jun., of the Royal Observatory at Greenwich, transmitted the following ephemeris of the positions of the four new planets, at their ensuing oppositions, computed by himself; at the same time remarking that the places of Ceres and Vesta differed materially from the places as given in Bode's *Astronomische Jahrbuch.*

The computations are for the *noon* of each day.

PALLAS.						CERES.					
1827.	AR			Dec. N.		1827.	AR			Dec. S.	
	h	m	s	°	′		h	m	s	°	′
Aug. 7	22	10	46	9	56	Sept. 6	0	57	23	14	23
8		10	5	9	48½	7		56	50	14	30
9		9	24	9	40½	8		56	15	14	37
10		8	42	9	32	9		55	39	14	44
11		7	59	9	24	10		55	2	14	51
12		7	16	9	15½	11		54	23	14	58
13		6	33	9	7	12		53	43	15	4
14		5	49	8	58½	13		53	2	15	10
15		5	4	8	49½	14		52	20	15	16
16		4	19	8	40	15		51	37	15	22
17		3	34	8	30½	16		50	53	15	28
18		2	49	8	21	17		50	7	15	34
19		2	4	8	11	18		49	20	15	40
20		1	19	8	1	19		48	32	15	46
21	22	0	34	7	51	20		47	44	15	51½
22	21	59	48	7	40½	21		46	56	15	57
23		59	2	7	30	22		46	7	16	2½
24		58	16	7	19	23		45	17	16	8
25		57	30	7	8	24		44	26	16	13
26		56	44	6	56½	25		43	35	16	17½
☍ 27		55	58	6	45½	☍ 26		42	44	16	22
28		55	12	6	34	27		41	52	16	26½
29		54	26	6	22½	28		41	1	16	31
30		53	41	6	11	29		40	9	16	35½
31		52	56	5	59½	30		39	17	16	40
Sept. 1		52	12	5	47½	Oct. 1		38	25	16	44
2		51	28	5	35½	2		37	32	16	48
3		50	45	5	23½	3		36	40	16	51½
4		50	2	5	11½	4		35	48	16	55
5		49	20	4	59	5		34	56	16	58
6		48	39	4	47	6		34	4	17	1
7		47	58	4	34½	7		33	12	17	4
8		47	18	4	22	8		32	21	17	7
9		46	39	4	9½	9		31	30	17	9½
10		46	1	3	57	10		30	39	17	11½
11		45	24	3	44½	11		29	48	17	13½
12		44	47	3	32	12		28	58	17	15
13		44	11	3	19½	13		28	8	17	16
14		43	35	3	7	14		27	19	17	17
15		43	1	2	54	15		26	30	17	17½

VESTA.			JUNO.		
1827.	Æ	Dec. N.	1828.	Æ	Dec.
	h m s	° ′		h m s	° ′
Nov. 25	5 50 58	18 43	March 5	12 42 35	−0 38½
26	50 10	18 44½	6	41 57	−0 31
27	49 20	18 46	7	41 18	−0 23½
28	48 28	18 47½	8	40 38	−0 15½
29	47 34	18 49	9	39 58	−0 7½
30	46 39	18 50½	10	39 18	+0 0½
Dec. 1	45 42	18 52	11	38 37	+0 9
2	44 44	18 53½	12	37 55	+0 18
3	43 44	18 55	13	37 12	+0 27
4	42 43	18 56½	14	36 28	+0 36½
5	41 31	18 58	15	35 42	+0 45½
6	40 38	18 59½	16	34 56	+0 54½
7	39 34	19 1½	17	34 10	+1 3½
8	38 29	19 3	18	33 23	+1 12½
9	37 23	19 4½	19	32 36	+1 21½
10	36 17	19 6	20	31 49	+1 30½
11	35 10	19 8	21	31 2	+1 39½
12	34 3	19 9½	22	30 14	+1 48½
13	32 56	19 11½	23	29 26	+1 57
14	31 48	19 13	24	28 39	+2 6
☍ 15	30 40	19 15	☍ 25	27 52	+2 15
16	29 32	19 17	26	27 4	+2 23½
17	28 24	19 18½	27	26 16	+2 32½
18	27 17	19 20½	28	25 28	+2 41
19	26 10	19 22	29	24 41	+2 49½
20	25 3	19 24	30	23 55	+2 58
21	23 57	19 26	31	23 8	+3 6½
22	22 51	19 27½	April 1	22 21	+3 14½
23	21 46	19 29½	2	21 36	+3 23
24	21 42	19 31	3	20 50	+3 31
25	19 39	19 33	4	20 5	+3 39
26	18 36	19 35	5	19 20	+3 47
27	17 34	19 37	6	18 36	+3 54½
28	16 31	19 39	7	17 52	+4 2
29	15 29	19 41	8	17 9	+4 10
30	14 28	19 43	9	16 27	+4 17½
31	13 27	19 45½	10	15 46	+4 25
1828.			11	15 5	+4 32½
Jan. 1	12 27	19 47½	12	14 25	+4 39
2	11 28	19 49½	13	13 44	+4 46
3	10 30	19 52			

There

There was next read a paper by Mr. Utting, "On a *new period of eclipses.*" In this communication, Mr. Utting adverts to the well-known Chaldean periods of 223 lunations and 6890 lunations: but, besides these, there are others that have been brought in aid of computations relative to eclipses. The new period, which Mr. Utting proposes, consists of 3803 lunations, or about 307 Julian years and 174 days; at the end of which time he says the sun and moon will be in conjunction at the opposite node to that from which they started: and at the end of 7606 lunations they will be in conjunction at the same node. Mr. Utting gives the secular motions of the sun's longitude and anomaly, and of the moon's longitude, anomaly and node, from the most recent tables: from which he deduces the correct period of a mean lunation; and thence the other necessary quantities for the computation of the time of the mean conjunctions.

The next papers were a series of observations by Major Hodgson: 1°. On the transit of Mercury over the sun's disc, on Nov. 4, 1822. 2°. Occultations of stars by the moon; particularly of the *Pleiades* on March 17, 1823. 3°. A set of equal altitudes for determining the time at Futty Ghur. 4°. Transits of Moon and moon-culminating stars at the same place.

Mr. Baily read the following extract of a letter from Professor Harding, of Göttingen.—"You will be pleased to hear of a small *variable* star, which I have discovered since June 9th, in last year. It is situated in *Serpens;* and my observations (which however are not very correct) give its position for 1800, as follows:

$$Æ = 235° 22' 3'' \qquad D = +15° 44' 48''$$

"It has, at this time, its greatest light; and will soon begin to diminish. As far as I have yet observed, its period seems to be about eleven months. This star is neither in the *Histoire Céleste*, nor in Bessel's *Zones:* nor have I observed it before the year 1826. When smallest, it is *entirely invisible.*"

A letter was read from Mr. George Innes, of Aberdeen, giving the results of his computations relative to the solar eclipse of the 28th of November last. He states that in calculating the parallaxes in longitude and latitude, he used the method of the Nonagesimal, and went over the process for each of the places, both according to the method used by Dr. Brinkley, and to that of Delambre. The difference in the results did not, in any case, exceed three-tenths of a second. For the reduction of the latitude, and equatorial parallax, the compression $\frac{1}{305}$ was used. No corrections were applied to the semidiameters of the sun or moon, for irradiation or inflexion. The augmentation of the moon's semidiameter for each of the places was obtained from Delambre's formulæ *only.* The following are the observations used in the computations, which are all expressed in *mean time.* In computing the Greenwich observation, he adopted that which was made with the five-feet equatorial: but it appears from the observations made at other places (except Dublin) that some of the others are nearer the truth.

Bushey

Bushey Heath	=	28d 21h 46m 4s,0 beginning.
		28 23 58 19,0 end.
Epping	=	29 0 0 54,0 end.
Greenwich ..	=	29 0 0 11,5 with 25 inch achrom. end.
		.. .. .. 12,4 with 46 inch do. end.
		.. .. .. 13,4 with 5 feet equat. end.
		.. .. .. 16,5 with 30 inch achrom. end.
		.. .. .. 18,0 with 7 feet Newt. end.
Dublin	=	28 23 25 30,0 end.
Armagh....	=	28 23 24 10,8 end.
Aberdeen ..	=	28 23 49 45,1 end.

From these Observations Mr. Innes obtained the following results:

	Conjunction Nov. 28. *Apparent* time.	Longitude.
Bushey Heath *b*	23h 35m 50s,66	
Bushey Heath *b & e*	35 50,19	
Bushey Heath *e*	35 47,64	1m 25s,57 W.
Epping	37 38,08	0 24,87 E.
Greenwich	37 13,21	
Dublin.	11 51,40	25 21,81 W.
Armagh	10 34,23	26 38,98 W.
Aberdeen	28 48,30	8 24,91

The results deduced from the observations at Bushey Heath marked *b*, *b* and *e*, and *e*, are obtained from the *beginning* of the eclipse, from the *beginning and end* conjointly, and from the *end*. And from *these* Observations Mr. Innes obtains + 9″,68 for the error of the tables in longitude, and − 2″,44 for the error in latitude. The lunar tables made use of were the recent ones of M. Damoiseau: and Mr. Innes has subjoined to his paper the whole of the elements employed in the calculations.

A description of an instrument called A *Tangent Sextant*, intended to determine the distances of objects from an observer, when their distances from each other are known, was communicated to the Society by Captain John Ross, R. N. who states that it was invented by an eminent land-surveyor, who lived to finish but two of them; and Captain Ross has since made some improvements in the instrument.

The index-mirror is placed diagonally across the bar of the index, having the centre in a line with the front edge of the index; and, in observing, the telescope is so placed that the centre of the instrument is towards the observer, and the limb directed towards the objects, which are brought in apparent contact, as in other reflecting instruments. The edge of the revolving index intersects another fixed and graduated radius, passing through the zero of the limb; and the figures at the point of intersection indicate the number

ber of times which the distance of the objects from each other is contained in the distance of the observer from the nearer of them, *when the angle at the nearer object is a right angle*, the only case of the problem which, in this application of his instrument, the inventor appears to have had in contemplation.

A method of making the necessary computations for deducing the longitude from an occultation of the moon, by Lieut. C. R. Drinkwater, R. N. was read. In the method proposed by Mr. Drinkwater, the sun's right ascension, and the moon's declination, horizontal parallax and semidiameter, are taken from the Nautical Almanac for the approximate Greenwich time; and the star's meridian distance being found, the latitude and parallax are corrected for the earth's ellipticity.

Then, calling the reduced horizontal parallax P, the star's meridian distance M, its polar distance p and the latitude l; he computes arc A, from $\tan A = \cos M \, . \cot l$; arc B, from $B = p \pm A$; C, the principal effect of parallax in polar distance from $C = P \, . \sin l . \sin B \, . \operatorname{sect} A$, ($-$ when A is less than p, otherwise $+$, unless M exceeds 6^h, when it is always $-$); D the parallax in right ascension from $D = P \, . \cos l \, . \sin M \, . \operatorname{cosect} p$ ($-$ when the star is east of the meridian, otherwise $+$); and E, the final correction of declination from $E = \frac{D^2 . \sin 2p}{\sin 1''}$ always $-$. And having thus found the declination of the point of occultation, the difference between it and that of the moon's centre is known; and from this difference, the moon's semidiameter, and the declinations of both objects. Mr. D. computes in the usual way the difference between the right ascension of the planet; and hence finds the right ascension of the moon's centre: from which, by interpolating in the Nautical Almanac, he finds the Greenwich time.

The business of the evening being concluded, Professor Amici (one of the Associates of the Society), at the request of the President, obligingly permitted the inspection of several instruments of his invention and workmanship. The principal were, a new compound achromatic microscope, a reflecting circle on a new construction, and a prismatic reflecting sextant. The microscope is formed with three achromatic double object-glasses, applied separately, or in combination; their common axis is directed vertically downwards, and the refracted pencil is turned into a horizontal position by internal reflexion at the base of a right-angled prism, for the convenience of observation, and after passing along the axis of a tube about seven inches long, received on the eye-piece. The object is powerfully illuminated from below by the reflected light of an Argand lamp, or daylight, collected in the focus of a large concave mirror. Several objects were viewed, such as the feathers on the wing of a butterfly; the pollen of the mallow, &c., which were shown with the utmost distinctness and beauty with powers of 700 and 1500* (the

* These powers, however extraordinary they may seem, are not exaggerated. The writer of this notice has since, more than once, witnessed, and

(the latter as stated by Mr. A. to have been determined by himself in conjunction with M. Arago in Paris).

The reflecting circle consists of two circles, of about six inches diameter, revolving one within the other, the outer of which carries the verniers, on which the divisions of the inner are read off to 20″. To each of these circles, in a plane perpendicular to its own and intersecting it in an exact diameter, is attached a long narrow plane reflector, in breadth equal to half the object-glass of the telescope, and in length, nearly to a diameter of the circles, and so contrived as to lie wholly below the plane of the limb, or at the backs of the circles, and to have their edges almost in contact, and intersecting exactly in the prolongation of the common axis of both circles; by which means parallax is avoided, and the necessity of a reduction to the centre, when the angles between near objects is taken, obviated. The attachment of these reflectors to the circles, so as to preserve the requisite freedom of motion and stability of the centre-work, is performed by a frame-work easier to imagine than to describe without a figure. The telescope is attached to the limb of the vernier circle (which is either held in the hand by a proper handle, or supported on a pillar and counterbalanced). Its object-glass is covered half by one of the reflectors and half by the other; and the images of two objects seen by reflexion in each, being brought to coincide, it is manifest that the angle between the reflectors (which is equal to half the angle subtended by the objects at the centre of the instrument) will be read off on the divided limb. The principal advantages offered by this construction are, the avoiding of parallax, and a power of measuring the same angle on four different parts of the limb (independent of the four readings of the verniers).

The prismatic reflecting sextant is nearly on the same principle, only that the internal reflexion at the bases of right-angled glass prisms is here employed in the place of metallic or silvered glass mirrors. It has, we believe, been described in the *Correspondence Astronomique* of Baron Zach, for which reason it is unnecessary to give any further account of it here.

XL. *Intelligence and Miscellaneous Articles.*

AMMONIA IN NATIVE OXIDE OF IRON.

THE formation of ammonia by the mutual action of iron, atmospheric air and water, was long since shown by Austin. M. Chevallier exposed various specimens of native oxide of iron to heat,

and taken part in, the determination of several of the powers of the microscope in question, under various combinations, in the presence of M. Amici himself, and several gentlemen of the greatest experience in such trials; when the results for four combinations tried were respectively 300, 700, 1120, and 3159 (all for *linear* measure). With the last enormous power, vision was still *tolerably* distinct, though necessarily very obscure: with all the others, admirably perfect.

and

and in every instance procured ammonia from them, even after the precaution of washing the finely-powdered ore with boiling water. 150 parts of hæmatites iron ore from Spain yielded 2 parts of muriate of ammonia.—*Annales de Chimie et de Physique.*

DETECTION OF HYDROCYANIC ACID.

MM. Lassaigne and Leuret have made several experiments upon the use of persulphate of iron and of copper, in detecting the presence of hydrocyanic acid in the contents of the stomachs of animals which have been killed by a dose of from two to five or six drops of the pure acid. They found that hydrocyanic acid cannot be discovered in animals which have been killed by small doses of it, if the bodies are previously exposed to the air for two or three days; that after a longer time than this, the disappearance of the poison is due to its decomposition, which is favoured by the presence of putrescent animal matter: and they state that when it is requisite to examine a body to ascertain the presence of this poison, it should be done as soon as possible after death.—*Journal de Chem. Med.*

TRANSFERENCE OF HEAT BY CHANGE OF CAPACITY IN GAS.

Many of the copper vessels in which gas is compressed at the Portable Gas-works are cylinders, from two to three feet in length, terminated by hemispherical ends. These are attached at one end to the system of pipes by which the gas is thrown in, and being so fixed the communication is opened: it frequently happens that gas, previously at the pressure of thirty atmospheres in the pipes and attached recipients, is suddenly allowed to enter these long gas vessels, at which time a curious effect is observed. That end of the cylinder at which the gas enters, becomes very much cooled, whilst, on the contrary, the other end acquires a considerable rise of temperature. This effect is produced by change of capacity in the gas: for as it enters the vessel from the parts in which it was previously confined, at a pressure of thirty atmospheres, it suddenly expands, as its capacity for heat increases, falls in temperature, and consequently cools that part of the vessel with which it first comes in contact; but the part which has thus taken heat, from the vessel being thrust forward to the further extremity of the cylinder by the successive portions which enter, is there compressed by them, has its capacity diminished, and now gives out that heat, or a part of it, which it had the moment before absorbed: this it communicates to the metal of that part of the gas vessel in which it is so compressed, and raises its temperature. Thus the heat of temperature is actually taken up by the gas from one end of the cylinder and conveyed to the other, occasioning the difference of temperature observed. The effect is best observed when, as before stated, the gas at a pressure of thirty atmospheres is suddenly let into the vessels; the capacity for the parts is such, that the pressure usually sinks to about ten atmospheres.—*Royal Institution Journal, N.S.* July.

ANALYSIS

ANALYSIS OF SPATHOSE IRON.

M. Lassaigne has analysed a variety of this ore from Tenzen in the Grisons; the results are:

Carbonate of lime	47·46
——— magnesia	19·33
Protocarbonate of iron	11·08
Water	22·13
	100·00

M. Lassaigne considers this as equivalent to 1 atom of protocarbonate of iron, 3 atoms of carbonate of magnesia, 5 of carbonate of lime, and 13 atoms of water: but adopting the atomic numbers employed in England, we must substitute 5 atoms for 3 of carbonate of magnesia.

This mineral is white, with a shade of yellow; it is crystallized in rhomboids. When heated by the blowpipe it decrepitates, becomes more opake, yellow, and afterwards colourless: if the calcination be performed in a glass tube closed at one end, the upper part of it is moistened with a large quantity of water. The specific gravity of this mineral is 2·927; it therefore differs in this respect from carbonate of iron, the specific gravity of which, according to Kirwan, is 3·640 to 3·810.—*Annales de Chimie et de Physique.*

SALT OBTAINED FROM OPIUM.

M. Dupuis lately presented to the *Société Philomathique* a salt obtained from opium, which he considers as a natural compound of morphia, and such as it exists in the vegetable;—analysis induces him to believe that it is sulphate of morphia. The salt in question is obtained in the following manner: He exposes extract of opium, nearly of the consistence of thin honey, to the air for five or six months; this extract gradually becomes a crystalline mass: he mixes it with a small quantity of water, and the crystals precipitate; he decants the fluid, and thus separates a part of the crystals, which he purifies by washing them with a small quantity of water and then with cold alcohol. . . . . .

M. Dupuis concludes from his experiments that opium contains narcotine, and more of the sulphates of morphia, lime and potash. —*Journal de Pharmacie*, June 1827.

ARSENIC AND CADMIUM.

M. Bischoff proposes a simple method of distinguishing arsenic and cadmium from each other. It is well known that both are precipitated of a yellowish colour by sulphuretted hydrogen; but according to M. Bischoff, hydrosulphuret of ammonia (*hydrosulfate d'ammoniaque*) gives no precipitate with arsenious acid, but forms with cadmium.—*Ibid.* July.

QUANTITY OF CHARCOAL OBTAINED FROM DIFFERENT KINDS OF WOOD.

M. Karsten has made numerous experiments on this subject. One hundred parts of the wood were reduced to the state of shavings and

and dried by exposure to the air: the *rapid* carbonization commenced at a high temperature, and the *slow* at a low one; the quantity of ashes was determined by incinerating the charcoal.

	Rapid. Charcoal.	Ashes.		*Slow.* Charcoal.	Ashes.
Young oak	16·39	0·15	—	25·45	0·15
Old oak	15·80	0·11	—	25·60	0·11
Young beech	14·50	0·375	—	25·50	0·375
Old beech	13·75	0·40	—	25·75	0·40
Young hornbeam	12·80	0.32	—	24·90	0·32
Old hornbeam	13·30	0·35	—	26·10	0·35
Young alder	14·10	0·35	—	25·30	0·35
Old alder	14·90	0·40	—	25·25	0·40
Young birch	12·80	0·25	—	24·80	0·25
Old birch	11·90	0 30	—	24·40	0·30
Young pine	14·10	0·15	—	25·10	0·15
Old pine	13·90	0·15	—	24·85	0·15
Young Norwegian pine	16·00	0·225	—	27·50	0·225
Old Norwegian pine	15·10	0·25	—	24·50	0·25
Young Scotch fir	15·40	0·12	—	25·95	0·12
Old fir	13·60	0·15	—	25·80	0·15
Lime	12·90	0·40	—	24·20	0·40
Rye-straw	13·10	0·30	—	24·30	0·30
Fern	14·25	2·75	—	25·20	2·75
Reed	12·95	1·70	—	24·75	1·70

Jameson's Journal.

WATER OF THE DEAD SEA.

Mons. C. G. Gmelin gives the following as the results of his analysis of this water:

Chloride of calcium	3·2141
———— magnesium	11·7734
Bromide of magnesium	0·4393
Chloride of sodium	7·0777
———— potassium	1·6738
———— aluminum	0·0896
———— manganese	0·2117
Muriate of ammonia	0·0075
Sulphate of lime	0·0527
	24·5398
Water	75·4602
	100·0000

Annales de Chimie et de Physique.

NEW ACID IN STAVESACRE.

M. Hofschlaeger of Bremen has discovered in the seeds of Stavesacre a new acid. It is white, crystalline, volatile at a low temperature; and a small quantity of it excites violent vomiting.—*Journal de Pharmacie*, July 1827.

A METHOD

METHOD OF DETECTING MINUTE QUANTITIES OF OPIUM, IN SOLUTION: BY R. HARE, M.D.

Through the discoveries of Sertuerner, it is now well known that opium contains an alkaline substance called morphia, to which it owes its efficacy in promoting sleep and relieving pain: also, that this alkali is naturally in union with an acid called meconic, which produces a striking red colour with solutions of red oxide of iron. Nevertheless, this property has not been proposed as a means of detecting opium; which has probably arisen from the circumstance that the meconate of iron does not precipitate. I have, however, contrived a method by which a quantity of opium not exceeding that contained in ten drops of laudanum may be detected in a half-gallon of water.

My process is founded on the property which meconic acid has of precipitating with lead. Hence, by adding a few drops of acetate of lead to any infusion, containing any quantity of the drug in question, not more minute than the proportion above mentioned, an observable quantity of the meconate of lead falls down. The precipitation, where the quantity is small, may require from six to twelve hours, and may be facilitated by a very gentle stirring with a glass rod to detach the flocks from the sides of the recipient, which should be conical, so as to concentrate them during their descent. The meconate being thus collected at the bottom of the vessel, let about thirty drops of sulphuric acid be poured down on it by means of a glass tube. Let this be followed by as much of the red sulphate of iron. The sulphuric acid liberates the meconic acid, and thus enables it to produce, with the iron, the appropriate colour which demonstrates the presence of that acid, and consequently of opium.

EASY MODE OF OBTAINING MECONIC ACID: BY R. HARE, M.D.

If to an aqueous infusion of opium we add subacetate of lead, a copious precipitation of meconate of lead ensues. This being collected by a filter, and exposed to sulphuretted hydrogen, meconic acid is liberated. The solution is of a reddish amber colour, and furnishes by evaporation crystals of the same hue. A very small quantity produces a very striking effect in reddening solutions of peroxide of iron.

Instead of sulphuretted hydrogen, sulphuric acid may be used to liberate the meconic acid. The presence of the former, in excess, does not seem to interfere with the power of reddening ferruginous solutions; but any excess of sulphuric acid may be removed by whiting, which is not acted upon sensibly by the meconic acid. Yet the acid procured in this way did not crystallize so handsomely, or with so much facility, as that obtained by sulphuretted hydrogen.

METHOD OF PREPARING DENARCOTIZED LAUDANUM: BY R. HARE, M.D.

Agreeably to the observations of the French chemists and physicians, the unpleasant effects of opium reside in a principle called narco-

narcotine; and Robiquet has informed us, that by digestion in æther, the drug may be depurated of that noxious principle. It struck me, as soon as I became acquainted with the statement of Robiquet, that it was of the utmost importance to humanity to have it tested, and the result made known to my countrymen, if favourable.

Some opium, shaved by rubbing it on the face of a jack-plane, was subjected four times successively to as much æther of the specific gravity of ·735 as would cover it, allowing each portion to act upon it for about twenty-four hours.

The opium was afterwards subjected to as much duly diluted alcohol as would have been adequate to convert it into laudanum, of the common kind, had it not been subjected to the æther. In the æther which had been digested on the opium, a deposition of crystalline matter soon commenced. The stopple being removed, and the mouth of the containing vessel, (in this case a common French tincture bottle,) being covered with blotting paper, in a few days nearly the whole of the liquid evaporated spontaneously, leaving much crystalline matter mixed with colouring matter. The former is, no doubt, the principle distinguished by Robiquet, since called narcotine.

The digestion of the opium with the æther is conveniently performed in the Papin's digesters, which are sold at some of the hardware stores in this city.

The æther should be kept near the temperature of ebullition.

The first use which was made of the denarcotized laudanum, was by way of an enema of thirty drops, in the case of a child tortured by ascarides, to whom it gave early relief, inducing a comfortable, and apparently natural sleep, and causing subsequently no unpleasant symptoms. The second instance was a case of severe headache, which was relieved in about thirty minutes, by ten drops taken into the stomach. A refreshing slumber succeeded, which was not followed by any of the distressing sensations to which the patient has always been subjected, after taking common laudanum.

Dr. Hare then subjoins some cases of the successful exhibition of denarcotized laudanum, by a medical friend.

ON THE OCCURRENCE OF GALENA IN THE INFERIOR OOLITE: BY MR. W. LONSDALE.

In widening the road between Frome and Buckland Denham in the autumn of last year, the workmen laid open a section about fourteen feet deep in the inferior oolite. The stone was broken *in situ* into irregular fragments, which in some instances were parted by a mere fissure, and in others were separated to the distance of six or eight inches. Near the centre of the section occurred a perpendicular vein six feet wide, of a stiff blue clay. From some distance on each side of this vein, small strings of galena appeared ramifying through the interstices of the stone, occasionally filling them entirely, and then exhibiting the appearance of a breccia of oolite cemented by lead; but most commonly crystallized carbonate of lime supplied the place of the ore. Specks of galena were likewise disseminated

disseminated through those portions of the stone which were adjacent to the fissures occupied by the metallic mineral. Towards the top of the section cakes of the sulphuret of lead were found in a loose state, and exactly filling the irregular cavities in which they were deposited.

The above description I received from one of the overseers and from the men employed upon the road. As I was desirous of ascertaining as far as possible the truth of the account, T. Bunn, Esq. of Frome, to whose love of science we are indebted for a knowledge of this interesting phænomenon, had the road opened for my inspection. The result verified the previous details.

I presume that this is the first instance in which galena has been found in so comparatively recent a formation; but the connection of the inferior oolite with the carboniferous limestone in that neighbourhood, apparently affords data for attempting an explanation of its occurrence. The latter rock is well known to abound with lead. At Shipham near Axbridge, the dolomitic conglomerate, which occupies a position analogous to that of the inferior oolite of Frome, also contains numerous irregular veins which yield galena. In both the newer deposits the situation of the metal accords with the character of the stone. The inferior oolite possesses a natural tendency to split into angular fragments, and in consequence of its great softness would divide with facility in their direction to every disruptive force. The dolomitic conglomerate has no natural cleavage; and at Shipham is a rock of great hardness: it would therefore yield only to a powerful agent, and then not in narrow fissures, but in chasms of great diversity of form.

When these circumstances are considered,—the metalliferous nature of the mountain limestone, the total absence of galena in the oolite, when other formations are interposed between it and the older rock,—we may surely be permitted to conclude, that whatever power, whether it were infiltration, infusion in a molten state, or sublimation, by which the ore was deposited in the carboniferous limestone, the same power acting cotemporaneously deposited it in the dolomitic conglomerate and inferior oolite. From these partial data we may likewise be permitted to draw the general conclusions, that the age which has been assigned to metallic veins is too remote; and that we may expect to find them, when our acquaintance with the unconformable superposition of rocks shall be greater, among the more recent of our secondary formations.

ECONOMY OF TEREDO NAVALIS, &c.

In the Report for the year 1826-7, of the Portsmouth and Portsea Philosophical Society, which has just been transmitted to us, appears the following report of a lecture, by Mr. C. Willcox, on the boring and lithophagous marine animals; containing some remarks on the œconomy of *Teredo navalis*, which seem to be important:—

The habits and œconomy of the *Teredo navalis*, the most destructive of the testacea, were described, and the irregular shape of the shell, description of the head, and formation of the

 hinge

hinge and valves noticed. A fine specimen was exhibited, and the statement of authors who affirm that it extends the whole length of the tube proved to be erroneous; since these tubes, which are formed by a peculiar secretion from the body of the animal, are often many feet in length and circuitous in their course. This was shown to be the fact, by a large piece of wood pierced in all directions. The manner in which it effects its passage and the appearance of the interior of the tubes were described. The assertion that the *Teredo* does not attack teak timber was shown to be incorrect, and its destructive ravages on the bottom of ships exemplified, by a relation of the providential escape of H. M. S. Sceptre, which having lost some copper from off her bows, the timbers were pierced through to such an extent, as to render her incapable of pursuing her voyage without repair.—The lecturer then exhibited the formation of the tubes (through an extent of several feet) in a plank of African timber. Hence the opinion, that the *Teredo* is attached to one end of the shelly tube, was considered to be erroneous, as in this case it would have exceeded seven feet in length. The opinion that the animal revolved was also presumed to be unfounded, for then only one valve (from the peculiar construction of the head) would be effective; whereas by a semi-volution, both are called into action; while from its very tender state, and the contorted direction of the tubes, it must of necessity be twisted up, if such revolution took place.—Mr. Willcox then noticed the habits and œconomy of the *Pholades*, exhibiting some specimens both in the living and dead state. Their manner of boring was explained, their phosphorescence shown, and their ravages described.—The lecturer next adverted to an insect called the *Lepisma*, and concluded by stating, that these minute depredators were frequently so numerous, that 300 will occasionally be found in the space of two inches square, and their attack commences the moment the timber is in the water, more particularly in the eastern part of the globe.

An interesting debate followed, as to the mode of action of the valves of the *Teredo;* in the course of which Mr. Willcox observed, that he thought Dr. Turton, in his observations relative to Sir E. Home's opinion on the *modus operandi* of this animal, had mistaken the action of the double-nosed auger, and had described it as a centre-bit; but that their opinions of the method of boring admitted of being reconciled; the mode appearing to be, that by a secretion from its body, the decomposition of the material is effected and reduced to a species of soft mud or pulp, which is mechanically removed, and a fine polished surface left. This idea, whilst it agrees with many of the statements, does not invalidate the experiments made on the charcoal produced by burning the excrementitious matter, and meets the difficulty which at first appears, as to the possibility of such fragile animals piercing materials of so hard a texture.

At the conclusion of this lecture, the Curator of the Museum, drew a comparison between the recent *Teredines* and the fossil remains of that animal, as exhibited in specimens of wood, from the London clay of Sheppy; showing that though the precise identity of of the species could not now be ascertained, yet such identity was very probable, as their operation and effects appear to have been the same. He also produced specimens of the *Mytilus lithophagus*, *M. Pholadis*, and *M. rugosus*, the two former penetrating madrepores, the latter limestone. Having pointed out the structure of the animals, and the formation of the valves, he concluded, that possessing no apparent means of producing their perforations by a mechanical operation, this tribe, at least, might be considered as producing that effect by a chemical solvent, which, as the substances acted upon were calcareous, was probably an acid; although he admitted that the action of tests had not made the presence of such secretion satisfactorily apparent,—recommending a prosecution of observations and experiments, as the subject had excited much attention.

NEW PATENTS.

To William Church, of Birmingham, for improvements in apparatus for spinning.—Dated the 18th of July 1827.—6 months allowed to enrol specification.

To George Anthony Sharp, of Putney, for an improved table urn.—18th of July.—6 months.

To Robert More, of Underwood, Stirlingshire, Scotland, distiller, for improvements, communicated from abroad, in the process of preparing and cooling worts or wash from vegetable substances, for the production of spirits.—18th of July.—6 months.

To Robert More, of Underwood, Stirlingshire, Scotland, distiller, for processes, communicated from abroad, for rendering distillery refuse productive of spirits.—18th of July.—6 months.

To Edward Dodd, of No. 62, Berwick-street, for improvements on piano-fortes.—25th of July.—6 months.

To Thomas Peek, of St. John-street, Clerkenwell, for a revolving steam-engine.—1st of August.—6 months.

To William Parkinson, of Barton-upon-Humber, and Samuel Crosley, of Cottage-lane, City-road, Middlesex, gas-apparatus manufacturer, for an improved method of constructing and working an engine, or producing power and motion.—1st August.—6 mon.

To Joseph Maudslay, of Lambeth, for an improvement on steam-engines.—1st of August.—4 months.

To Lionel Lukin, esquire, of Lewisham, Kent, for improvements, partly communicated from abroad, in collars and saddles for draught and carriage horses.—1st of August.—6 months.

To Eugene du Mesnil, esquire, of Soho-square, for an improvement in stringed musical instruments.—1st of August.—6 months.

To Anthony Scott, of Southwark Pottery, Durham, for an apparatus for preventing the boilers of steam-engines, &c. becoming foul, and for cleaning the same.—4th of August.—2 months.

To Peter Burt, of Waterloo-place, Limehouse, mathematical-instrument-maker, for an improved steam-engine. Communicated from abroad.—4th of August.—6 months.

To John Underhill, of Parkfield Iron-Works, Wolverhampton, iron-master, for improvements in machinery for passing boats, &c. from a higher to a lower, or a lower to a higher level, with little or no no loss of water; also applicable to the raising or lowering of weights on land.—13th of August.—6 months.

To William Dickinson, of Bridge-street, Southwark, for an improved buoyant bed or mattress.—13th of August.—6 months.

To Thomas Breidenback, of Birmingham, for improvements on bedsteads, and in making articles to be used in various ways with bedsteads from a material hitherto unused for such purposes.—13th of August.—6 months.

To W. Alexis Jarrin, of Bond-street, confectioner, for improvements in apparatus for cooling liquids.—13th of August.—2 months.

To William Chapman, of Newcastle-upon-Tyne, for improvements in waggons for rail-ways.—14th of August.—2 months.

To Henry Pinkins, of Philadelphia, North America, and of the Quadrant Hotel, Regent-street, for an improved apparatus for generating gas to be applied to lights and other purposes.—15th of August.—6 months.

To William Spong, of Aylesford, Kent, for an invention for diminishing friction in wheel-carriages, water-wheels, and other rotary parts of machinery.—15th of August.—6 months.

METEOROLOGICAL OBSERVATIONS FOR JULY 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·46 July 6. Wind E.—Min. 29·76 July 1 and 20.
Range of the mercury 0·70.
Mean barometrical pressure for the month . . . . 30·103
Spaces described by the rising and falling of the mercury . . . 4·320
Greatest variation in 24 hours 0·400.—Number of changes 28.

Therm. Max. 80° July 7th and 8th. Wind N.—Min. 53° on several nights.
Range 27°.—Mean temp. of exter. air 65°·03. For 31 days with ☉ in ♋ 66·76
Max. var. in 24 hours 25°·00—Mean temp. of spring water at 8 A.M. 52°·87

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 23rd . . . . 94°
Greatest dryness of the air in the afternoon of the 18th 37
Range of the index 57
Mean at 2 P.M. 50°·7—Mean at 8 A.M. 58°·8—Mean at 8 P.M. 65·6
—— of three observations each day at 8, 2, and 8 o'clock . . 58·4
Evaporation for the month 3·70 inch.
Rain near ground 1·115 inch.—Rain 23 feet high 1·025 inch.
Prevailing Wind S.W.

Summary of the Weather.

A clear sky, 6; fine, with various modifications of clouds, 15; an overcast sky without rain, 6; foggy, ½; rain, 3½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
20	16	23	1	28	22	10

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2½	2	2	2	3	11	6	2½	31

General

General Observations.—This month has been remarkably fine, dry, and generally calm, but not hot for the season, except on the 7th and 8th, on which days the thermometer in the shade rose to 80 degrees. No measurable rain fell here from the 4th to the 19th, and the refreshing showers on two or three days afterwards were found very beneficial to the herbage, fruit, and corn crops. From the uniformly fair weather the mean barometrical pressure is high for this period; and on many serene evenings the sky round the horizon at sunset was tinged with light blue, surmounted by light red and yellow, the brightest colours being opposite to the sun; but the evening twilight reflected richer tints upon the surrounding water, and the setting sun was frequently followed by small orange-coloured arcs, which indicated a continuance of fair weather.

On the 29th, sheet lightning appeared soon after sunset, between the N.W. and S.E. points, and emanated in quick succession from low cirro-strative clouds through the night. These clouds appeared at noon in the character of descending *cirri*: they afterwards transformed into *cirrostrati*, and were wafted in the evening to the western horizon by a brisk easterly wind under very hot sunshine, by which means they acquired much electrical matter.

The mean temperature of the external air this month is nearly one degree higher than the mean of July for the last eleven years.

The wheat harvest has just commenced in this neighbourhood, (August the 2nd) and from appearances here, and reports from all parts of the country, the crops will be both abundant and of good quality.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one lunar and two solar halos, six meteors, lightning and thunder in the night of the 29th; and four gales of wind, or days on which they have prevailed, namely, two from the S., one from S.W., and one from the West.

REMARKS.

London.—July 1. Cloudy. 2. Rainy morning. 3. Fine. 4. Showers. 5, 6, Fine. 7. Fine: clear. 8—15. Fine. 16. Cloudy and fine. 17. Fine. 18. Cloudy. 19. Cloudy: showery. 20. Cloudy: fine. 21. Cloudy. 22. Rainy. 23, 24. Cloudy. 25, 26. Cloudy and fine. 27. Cloudy. 28. Fine. 29. Sultry. 30. A thunder storm at 4 A.M.; windy day. 31. Cloudy.

Boston.—July 1. Rainy. 2. Cloudy: rain A.M. and P.M. 3—5. Fine. 6. Fine: thermom. 78° 4 P.M. 7. Cloudy. 8, 9. Fine. 10. Fine: therm. 79° ½ past 2 P.M. 11—17. Fine. 18. Cloudy. 19. Fine. 20. Cloudy: rain A.M. 21. Cloudy. 22. Fine. 23. Cloudy. 24. Fine. 25, 26. Cloudy: rain P.M. 27. Fine. 28. Fine: thermom. 81·5 3 P.M. 29. Fine. 30. Fine: thunder and lightning from 3 to 7 A.M. with rain, stormy, P.M. 31. Fine.

Penzance.—July 1. Heavy showers. 2. Rain: fair. 3. Clear. 4. Misty rain. 8. Clear. 9—11. Fair. 12—18. Clear. 19. Rain. 20. Fair: showers. 21. Fair: clear. 22. Rain. 23. Showery. 24. Misty. 25. Showers. 26. Misty rain. 27. Rain. 28. Fair. 29. Heavy clouds, thunder and lightning. 30. Clear. 31. Fair.—Rain guage ground level.

RESULTS.

London.—Barometer: Mean of the month	30·222 inch.
Thermometer: Mean of the month	65·950°
Evaporation	4·70 inch.
Rain	1·37.

Meteoro-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.										
July 1	29·95	29·92	29·52	29·50	29·80	29·76	29·28	78	55	65	56	70	57	58	SW.	W.	S.	NW.	...	...	0·20	0·425	0·175	0·23
☽ 2	29·98	29·95	29·60	29·54	29·82	29·82	29·37	74	53	68	55	73	54	60·5	SW.	NW.	SE.	W.	...	...	·13	0·150	...	...
3	30·32	29·98	29·80	29·70	30·06	29·87	29·30	73	50	66	55	70	54	63	NW.	NW.	W.	SW.	...	0·30	...	...	...	·10
4	30·37	30·32	29·92	29·90	30·23	30·20	29·64	78	59	64	58	73	61	61·5	W.	W.	SW.	W.	...	...	·09	...	·020	...
5	30·57	30·37	30·06	29·98	30·38	30·20	29·75	75	44	67	57	72	53	62	NE.	N.	N.	NE.	...	...	...	...	...	...
6	30·57	30·51	30·12	30·10	30·46	30·40	29·90	88	50	70	55	69	55	62·5	SW.	NE.	E.	NW.	...	·40	...	...	...	...
7	30·51	30·49	30·18	30·18	30·42	30·40	29·77	85	56	72	58	80	59	69	NNW.	NE.	N.	NW.	0·95	...	...	...	...	...
○ 8	30·49	30·41	30·18	30·15	30·43	30·38	29·76	87	55	73	59	80	59	69	NNW.	NE.	N.	calm	...	...	...	...	...	...
9	30·41	30·25	30·12	30·10	30·34	30·26	29·40	84	55	68	58	75	56	65·5	NW.	NW.	NW.	W.	...	·30	...	...	...	...
10	30·25	30·19	30·04	30·00	30·22	30·10	29·50	81	56	68	59	72	58	65	W.	W.	W.	W.	...	...	...	...	...	...
11	30·30	30·25	29·90	29·90	30·15	30·10	29·50	84	46	68	58	76	54	63	NW.	NW.	NW.	NE.	...	...	...	...	...	...
12	30·33	30·30	29·90	29·90	30 20	30·18	29·65	84	48	67	52	69	55	63	N.	S.	NE.	E.	·98	·30	...	...	...	...
13	30·31	30·30	29·88	29·88	30·20	30·18	29·70	84	43	70	54	69	53	64	E.	SE.	SE.	calm	...	...	...	...	...	...
14	30·31	30·20	29·90	29·88	30·16	30·10	29·65	77	43	69	56	69	54	65	SE.	NE.	NE.	calm	...	...	...	...	...	...
☾ 15	30·20	30·18	29·81	29·80	30·04	30·03	29·55	80	52	70	56	75	54	63·5	SE.	SE.	E.	E.	...	·55	...	...	...	...
16	30·19	30·18	29·82	29·80	30·05	30·04	29·53	80	46	73	55	71	55	65	SE.	NW.	SW.	E.	...	...	...	...	...	...
17	30·18	30·14	29 80	29·78	30·09	30·06	29·46	83	57	71	56	73	60	67	W.	NW.	SW.	W.	·94	...	...	...	...	...
18	30·15	30·14	29·82	29·80	30·08	30·01	29·33	78	54	68	57	75	58	66	SW.	NW.	SW.	NW.	...	·50	...	...	...	...
19	30·15	29·91	29·70	29·66	30·04	29·88	29·38	70	60	68	57	68	60	64	SW.	SW.	W.	W.	...	...	·45	0·255	·360	...
20	30·07	29·92	29·70	29·66	29·85	29·76	29·14	72	50	68	57	70	54	62	NW.	W.	NW.	W.	...	...	...	0·150	...	·17
21	30·11	30·07	29·80	29·79	30·04	29·96	29·36	73	48	66	56	71	53	59·5	NW.	NW.	NE.	W.	...	·20	...	...	...	...
22	30·14	30·11	29·70	29·70	30·03	29·99	29·50	67	57	67	56	64	59	62·5	SW.	SW.	S.	W.	...	...	·15	0·700	·490	...
23	30·20	30·14	29·87	29·80	29·98	29·96	29·48	78	60	67	58	73	60	62·5	SW.	W.	SW.	calm	...	...	...	0·350	...	...
● 24	30·20	30·11	29·86	29·80	30·10	30·06	29·50	76	58	72	60	75	62	68	NW.	W.	SW.	calm	...	·15	...	...	·020	...
25	30·22	30·11	29·84	29·78	30·05	29·99	29·34	77	49	67	59	76	54	67	SW.	NW.	NW.	calm	...	...	...	...	...	...
26	30·26	30·22	29·84	29·76	30·16	29·96	29·53	77	53	67	56	73	61	63	W.	SW.	SW.	calm	·97	...	·33	0·100	·050	·20
27	30·31	30·26	29·89	29·88	30·22	30·15	29·60	79	66	69	61	78	65	60	NW.	SW.	NW.	NW.	...	·30	...	0·120	...	·48
28	30·31	30·31	29·98	29·96	30·25	30·23	29·50	82	54	72	62	78	59	71·5	NW.	W.	SW.	NW.	...	...	...	...	...	...
29	30·31	29·93	29·86	29·68	30·21	29·92	29·52	87	64	72	58	74	66	72	E.	SE.	SE.	calm	...	...	·02	...	...	...
30	30·35	29·95	29·88	29·70	30·04	29·87	29·13	80	52	71	58	73	55	65	W.	NW.	SW.	calm	...	...	...	...	...	·08
31	30·35	30·28	30·00	30·00	30·27	30·22	29·60	75	50	71	53	75	56	65	var.	W.	W.	calm	·86	·70	...	...	...	...
Aver. :			30·18	29·50	30·46	29·76	29·50			73	52	80	53	64·3					4·70	3·70	1·37	2·250	1·115	1·26

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

OCTOBER 1827.

XLI. *On the Figure of Equilibrium of a Homogeneous Planet in a Fluid State; in reply to the Observations of* M. Poisson, *published in this Journal for July last. By* J. Ivory, *Esq. M.A. F.R.S.*

[Continued from p. 168.]

IN the last Number of this Journal I have investigated the figure of a homogeneous planet entirely fluid. By a process of analytical reasoning founded on the homogeneity of the fluid, and the nature of the forces urging the particles, I have shown, what is contrary to the usual theory, that two conditions are necessary to the equilibrium of the whole mass. I have next proved that, when both conditions are fulfilled, the planet will have a permanent figure, and that this will not be the case, if either of them be wanting. This solution is not different from the usual theory, except in one of the conditions, to which objections have been made; and, as we shall chiefly have occasion to speak of this condition, it may be proper to recall what it consists in to the recollection of the reader.

Conceive a level surface in the interior of the planet, and let V denote the sum of all molecules contained within the level surface divided by their respective distances from any point assumed in that surface, and $\Delta.V$, the like sum for all the molecules exterior to the same surface; then the new property of the equilibrium consists in this, that $\Delta.V$ must have a constant value for all the points in the same level surface. The invariability of $\Delta.V$ leads to some curious consequences. In the first place, $\Delta.V$ merely augments the pressure in the equation of the level surface, thereby showing that the action of the exterior stratum upon the interior fluid has no other effect than to cause a pressure that is equable on all parts of the

the level surface*. Again, because the expression of $\Delta . V$ is independent of the coordinates of all the points in, or within, the level surface, it follows, from the nature of that function, that the attraction of the stratum upon all the interior particles, will be equal to zero, or will be equal in all opposite directions. If a stratum laid upon any level surface possess either of the two properties; namely, equality of pressure or nullity of attraction upon all the interior particles, it will necessarily be possessed of the other.

We are now prepared to estimate the force of M. Poisson's objections. In the article written by him, and inserted in the *Annales de Chimie et de Physique*, tom. xxvii. p. 225, he sets out with giving a succinct and clear exposition of the principles of the usual theory. In the case of a homogeneous fluid these principles are only two, which relate, the one to the algebraic expressions of the accelerating forces, and the other to the perpendicularity of gravity to the outer surface. The necessity and the sufficiency of these two conditions in all cases whatever in which a homogeneous fluid is considered, are conveyed in these imperative words, *Il faut et il suffit*. There is no objection to this theory when the accelerating forces are explicitly given. But when the particles of the fluid attract one another, the accelerating forces at any point in the interior vary according to the situation of the point with regard to the attracting matter; on this account the equation of the level surfaces is extremely complicated; and it by no means appears that, in this case as in the more simple one when the accelerating forces are explicitly given, we can infer the existence of the interior level surfaces, which is necessary to the equilibrium, from the equation of the outer surface alone. I have therefore departed from the usual theory, and have sought to deduce the conditions of the equilibrium of a homogeneous planet entirely fluid, by a different procedure. The conclusions I have obtained rest on their own evidence. All attempts to solve completely the equation of the outer surface of a homogeneous mass of fluid *in equilibrio*, have hitherto failed, and it is not known what figures come under it. If it comprehend ellipsoids exclusively of all other figures, the usual theory will agree with my investigation; if it extends to other figures, it is certain, from what I have proved, that these are not figures of equilibrium.

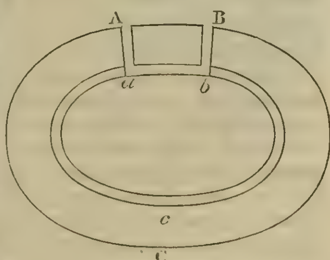
* In a short paragraph, p. 167 of this Journal, for last month, $V(r)—V'(r)$, or $\Delta . V$, is said to be the pressure in the interior of the fluid. It is not, however, the whole pressure, but only that part of it produced by the attraction of the exterior stratum. The whole paragraph is not much to the purpose, and had better have been left out.

M. Poisson's

M. Poisson's first objection to my investigation, is one of his own imagining. In a homogeneous planet the surfaces that possess the new property are all similar to the outer surface, and similarly posited about the centre of gravity; and therefore they cannot cross one another as M. Poisson supposes. And in that part of my paper, where I have spoken of the equilibrium of a homogeneous mass of fluid consisting of particles that attract according to any law, the indefinitely thin strata are constructed in succession by the principle in § xxi. *prem. part.* of Clairaut's work on the Theory of the Earth, that is, so that the thickness of a stratum, at any point, is reciprocally proportional to the gravitation at the surface on which the stratum is laid. The surfaces in question have no reference to any points given in space; but their relative situation with respect to one another is determined; they are necessarily contained, one within another, which exempts them from M. Poisson's argument.

M. Poisson next (pp. 230, 231, 232) undertakes to examine in what manner the equilibrium of a mass of fluid consisting of attracting particles may remain undisturbed by the addition of a stratum of the like attracting particles. His intention is to prove that there may be an equilibrium in both cases, at the same time that the stratum exerts unequal pressures upon the surface on which it is laid. This would be directly contrary to my third proposition, and would destroy the whole of my theory. It is therefore necessary to consider this part of M. Poisson's article with particular attention.

Let *a b c* be a homogeneous mass of fluid *in equilibrio* by the attraction of its particles and a centrifugal force; and suppose that the equilibrium still continues to take place in the larger body ABC, formed by laying a stratum of the fluid upon the first. Let A*ab*B be a canal of which the branch *ab* is within the interior body, and lies along its surface; and the other two branches A*a* and B*b*, perpendicular to the same surface, traverse the exterior stratum and end in the outer surface. Because the whole body ABC is *in equilibrio*, the fluid in the canal will be so too, and will have no tendency to run out at either of the orifices A, B. We must next estimate the forces that urge the particles in the canal. Resolve



all the forces which act upon the molecules in the branch A a, and multiply every molecule by all the resolved forces which urge it in the direction of the canal from A to a, and call the sum of the products p. Let q denote the like sum of all the molecules in the canal B b respectively multiplied by the forces which urge them in the direction of the canal from B to C. With respect to the branch $a b$, M. Poisson observes that we may neglect the centrifugal force of the particles contained in it, and the attraction of the interior body $a b c$ upon them; because these forces acting perpendicularly to the surface $a b c$, which is *in equilibrio* separately, have no tendency to produce motion in the canal. The attraction of the exterior stratum is the only remaining force that acts upon the fluid in the canal $a b$, and we shall put δ for the sum of the products of all the molecules multiplied respectively by the attraction of the stratum resolved in the direction of the canal from a to b. We now obtain this equation,

$$p + \delta - q = 0$$

which expresses that the fluid has no tendency to run out at the orifice B. This equation coincides with what M. Poisson has obtained, p. 232*, and of which he remarks p. 233, "C'est de cette manière que l'equilibre de la masse interieure n'est pas troublé par les pressions inegales exercées sur la surface $a b c$ par la couche fluide qui l'enveloppe de toutes parts." Now this supposes that δ stands for some determinate value in the foregoing equation. But innumerable canals may be made to pass from a to b along the surface of the interior body; and, as the same reasoning will apply to all, δ will stand for the sum of the products of the molecules in any one respectively multiplied by the attraction of the exterior stratum reduced in the direction of the canal. Not only so, but δ will stand for the sum of the like products in innumerable other canals passing from a to b through the fluid mass $a b c$; for the reasoning requires nothing more than that the canal have its ends at a and b in the surface of $a b c$, and that it be wholly contained within that surface. It is plain therefore that δ can have no determinate value, and that the foregoing equation cannot be true, unless $\delta = 0$, and $p = q$. Thus the pressures of the exterior stratum upon the surface on which it lies, are equal, and not unequal as M. Poisson alleges. We may arrive at the same conclusion more readily by continuing the canal $a b$ all round the surface of $a b c$, so as to return into tself: for then δ must have the same value in the two parts of the canal going from a to b either way; and as these parts

* *Ann. de Chimie et de Physique*, tom. xxvii.

are

are entirely arbitrary, it manifestly follows that δ can have no other value but zero. In reality the reasoning of M. Poisson, when pushed to the proper conclusion, turns out to be a demonstration of my third proposition.

But it is unnecessary to consider particularly the manner in which the stratum acts upon the interior fluid body $a\,b\,c$. For the exterior matter can produce no action on the interior fluid, except through the intervention of pressures on the surface, and the continuance of the equilibrium requires that these pressures be equal.

I have already sufficiently replied to what is contained in pp. 234, 235, of M. Poisson's article in a note, p. 165, of the last Number of this Journal.

It does not appear that M. Poisson has refuted my theory of the equilibrium of a homogeneous planet; and he never will be able to refute it.

There is still one point relating to this subject that seems to require some elucidation. It is the case of a planet very little different from a sphere. This case is the more deserving of attention, because the solution of it seems to be deduced solely from the equation of the outer surface of the fluid. The discussion of this matter would make too great an addition to what I have written; but I will enter upon it at a future occasion, and I will show that the equilibrium can no more be inferred from the equation of the surface in this case than in any other, and that in reality there are other principles, besides that equation, concerned in the investigation.

I shall conclude at present, with noticing the postscript to M. Poisson's observations inserted in this Journal for July last. It relates to the heat absorbed or extricated, or, which is the same thing, to the variation of temperature, when a given mass of air changes its volume and at the same time retains the whole of its heat. It is not, however, to the formula (6) of his Memoir in the *Conn. des Tems* 1826, that I object, but to the use that is made of it, and to the integrals (7) and (8) derived from it.

In order to elucidate this matter, I shall take the formula (6), *Conn. des Tems*, 1826, p. 263, viz.

$$\omega = (k - 1)\,(1 + \alpha\theta).\,\frac{\gamma}{\alpha}:$$

here θ is the original temperature of the given mass of air; α is the dilatation of gas for one degree; ω is the increase of temperature, or the heat extricated, when the air suffers the small condensation γ; and $k - 1$ is a number deduced from the experiment of MM. Clement and Desormes. Let ρ denote the

the density of the air, and put $d\,i = \omega$; then we have $\gamma = \frac{d\varrho}{\varrho}$, and the foregoing equation will become,

$$\frac{\alpha\, d\, i}{1 + \alpha\,\theta} = (k-1)\ \frac{d\,\varrho}{\varrho}. \qquad (1)$$

It must be observed, that this equation expresses the relation of the differentials only for a small initial variation of the mass of air, or when $i = 0$. What may be their general relation, when i has acquired any finite value, cannot possibly be duduced from this equation alone. M. Poisson derives this integral from the expression (1), viz.

$$\frac{1 + \alpha\,\theta + \alpha\, i}{1 + \alpha\,\theta} = \left(\frac{\varrho}{\varrho'}\right)^{k-1},$$

ϱ' being the density at the initial temperature θ. There is no doubt that the formula (1) is thus satisfied: for if we differentiate the integral, and put $i = 0$ in the differential, we shall arrive at the formula in question. But there are innumerable other integrals that will equally produce the same result in the like circumstance: for instance,

$$\frac{1 + \alpha\,\theta + \beta\, i}{1 + \alpha\,\theta} = \left(\frac{\varrho}{\varrho'}\right)^{\frac{\beta\,(k-1)}{\alpha}},$$

β being any arbitrary number. It appears therefore that M. Poisson's integral is not the only one that will fulfil the required conditions; there is no evidence that it contains the law of nature more than any other; it is dependent on a hypothesis, or an arbitrary assumption.

We shall pursue this investigation on less exceptionable principles in the manner following. Suppose that the air is condensed by the diminution of its temperature, the pressure remaining constant: then, τ being the decrease of temperature when the density varies from ϱ' to ϱ, we shall have, by the usual principles,

$$\frac{\varrho}{\varrho'} = \frac{1 + \alpha\,\theta}{1 + \alpha\,\theta - \alpha\,\tau}, \qquad (2)$$

Differentiate this equation, and put $\tau = 0$ in the differential; then,

$$\frac{\alpha\, d\,\tau}{1 + \alpha\,\theta} = \frac{d\,\varrho}{\varrho}.$$

This equation expresses the relation of the small initial variations of temperature and density. If we combine it with the formula (1), and put $k - 1 = \frac{\alpha}{\beta}$, we shall get,

$$\frac{d\,i}{d\,\tau} = \frac{\alpha}{\beta}.$$

The next question is, whether $k - 1$, or $\frac{\alpha}{\beta}$, is a constant

or

or variable ratio. The experiments of Gay-Lussac appear to make it constant in a great range of temperature and pressure; and I have proved from the admitted properties of air, that, when the pressure is constant, the variations of latent heat and temperature, i and τ, preserve the same constant proportion, as far as our experience extends. Thus we have $\beta i = \alpha \tau$; and, by substituting in the formula (2), we get,

$$\frac{\varrho}{\varrho'} = \frac{1 + \alpha \theta}{1 + \alpha \theta - \beta i}.$$

This formula is not different from that in p. 252 of this Journal for April last, except in the sign of βi; and the reason is, that i here stands for the change of temperature produced by the latent heat which is set free, and in the other formula i denotes the variation in the quantity of the latent heat contained in the given mass of air.

What has now been said fully explains the remark I made on the formula of M. Poisson, published in the *Conn. des Tems* 1826, and proves the justness of that remark.

Sept. 10, 1827. JAMES IVORY.

[To be continued.]

XLII. *Outlines of a Philosophical Inquiry into the Nature and Properties of the Blood; being the Substance of three Lectures on that Subject delivered at the Gresham Institution during Michaelmas Term* 1826. *By* JOHN SPURGIN, *M.D. Fellow of the Royal College of Physicians of London, and of the Cambridge Philosophical Society.*

[Concluded from p. 191.]

HAVING drawn your attention to this leading property of the blood—its fluidity, and illustrated the nature of this property in one respect, that is to say, in its being fitted thereby to traverse every angle and corner of the body; and having but slightly adverted to, and only indirectly demonstrated, the cause of this its fluidity: we must crave your indulgence to permit us to employ the expressions *purpose* and *design* as forming a part in our reasoning, in the same manner as the algebraist would employ the characters x, y, z, to form a part of his analysis, in order that by means of the known values of the other characters he may arrive at the values of those that are unknown. And we crave this indulgence the more confidently, because we do not, as others have done, avail ourselves of the method to discover a proportion between life and matter, seeing that there is no proportion between them; but to to demonstrate that matter relatively to itself alone enjoys higher and lower degrees of vitality, or manifests vital phænomena in greater and less perfection, according to the condition in which it stands, or according to the state into which it may be brought. No one can deny that organization is a condition of matter suited to the manifestation of vital phænomena! No one can deny that the higher or the more perfect the organization, the more complete and perfect is the manifestation of the vital phænomena:—thus for the sake of illustration, the results of the cerebral organization which, doubtless, are motion, feeling, memory, thought, judgement, and the like, are superior to those of muscular structure or organization, to wit,—contraction and relaxation or irritability! But again, no one can deny that each of these parts or organs is momentarily dependent upon the presence of the blood to enable it to manifest its peculiar vital phænomena. Let me, therefore, intreat your attention to this point one moment; let me ask you, in what other relationship do the vessels and organs stand to the blood which circulates through them, than this, (viz.) that *by virtue of the blood they live, and by living, act.*

Now, in speaking of a purpose and design, how well do we see them accomplished or fulfilled by the fluidity of the blood! the very building up and preservation of the body is accomplished and fulfilled by this fluid, for it possesses in its composition those elements of which the body itself is compounded, according to what we remarked in our introduction, "that nothing exists in the body which did not first exist in the blood." We have observed, that between life and matter there can be no proportion; but we have observed likewise, that there is a proportion between the various conditions of matter;—that a proportion may be instituted between one condition of matter and another, even in regard to organization as being more and less perfect and excellent. But are we prepared to show that in regard to the blood there are in the same manner divers degrees of perfection, not only in respect to its quality whether good or bad, but also in respect to perfection as being less compound, more active, more fluid, and thence better enabled still to penetrate into the almost invisible recesses and delicate textures where the red and more compound, and thence less perfect blood can never reach, without threatening destruction to the whole viscus, as happens in inflammation? In short, can we show that there is a fluid, which by virtue of the perfection of its nature is better enabled still to accomplish and to fulfil certain specific purposes and designs,—a fluid which in relation to life is as nothing, but in relation to the blood is as the life thereof; and thence the cause of its vitality and fluidity.

fluidity. This more direct, more immediate, and proximate cause of the blood's fluidity, it may seem at first sight difficult to demonstrate, as having apparently no real existence in animal nature; but if the reality of things diminishes with their invisibility to our eye-sight, or in proportion as they evade our senses, it would be a fruitless effort to search the finer textures and organizations of our bodies,—for in so doing we should wander from the substance to the shadow: but if on the contrary the reality of things increases with their imperceptibility to our senses, as is in truth the case; if the finer textures and organizations of our bodies astonish us by the beauty, the simplicity, and the perfection of their forms, as in truth they do; we are labouring to a good purpose when we are investigating them, we are tracing the direct path from effects to causes, and proceeding by the method so insisted on at the present day—of induction.

Now, as the blood possesses all those elements which are subservient to the building up the different textures of the body from the cerebral to the osseous on the one hand, and enjoys those properties and essentials which enable it to maintain its presence in and circulation through them on the other, we are compelled to assent to the doctrine which teaches, or which supposes, the permeability of the cerebral and nervous textures, and their permeation by a subtile invisible fluid; a fluid not the less real for being invisible, nor the less material for the want of tangibility; a fluid that fulfils and accomplishes certain specific purposes and designs pre-eminently,—which in relation to the solids and fluids is alone vital, which builds up and compounds from its own elements the entire cerebral and nervous textures, in the same manner as the red blood furnishes the elements for the building up and compounding the grosser vascular, fleshy, and bony textures: in fine, a fluid which is every where present in the body, requiring brain and nerves for its distribution, and red blood for its seat and habitation*, imparting life at the same time to the cerebral

* We hope to have it in our power to consider of the question of the existence of a nervous fluid properly so called, and to discuss the grounds for the objections which have been urged against the necessity or possibility of its existence. We believe it would be just as rational to suppose the phænomena of electricity could be presented in mundane nature without the medium of an appropriate mundane agent, as that vital phænomena could be presented in animal nature without the medium of their appropriate animal agent. All qualities, attributes, forces, powers and modes, must in the nature of things have their subjects in which they reside, or on which they depend: as for example, sound is a mode of motion of the air, and is dependent on air for its existence: the sensation it excites in the cor-

cerebral organizations which thus by living, act. Should you say, can it be possible for there to be such a material fluid? we request you to contemplate the powers of the seminal fluid in all animals, which has the wonderful power of accomplishing and fulfilling certain specific purposes and designs to perfection; in that it can serve as the commencing point for a new being, the exact type of the prototype parent; nay, can from one vile reptile and insect, from one individual parent, produce ye a thousand offspring,—each typical of the same parent, but not the same type. This is fluidity, this is plasticity, this is adaptability to certain living purposes and designs which none can question.

Physiologists and philosophers of every age have entertained some notion concerning a vital principle, and have endeavoured to express their ideas by some peculiar term; whence we read of as many terms as there have been distinct notions maintained by different sects of philosophers. Thus we hear

corresponding organ of hearing, belongs to living or sentient beings, and therefore requires a medium for its existence as such: the organ alone is not that medium, the nerves alone are not that medium; for these exist during fainting, and after death for a time; and yet the sensation perishes, or the susceptibility of being affected by sound on the part of the body is lost: it is the same with the other animal senses. The only change of state in the body at such a time is the cessation of the circulation of a fluid;—in respect to the blood-vessels, we know that the circulation of their blood has ceased; but in respect to the nerves, we conjecture that the circulation of their appropriate fluid or blood has stopped also; and we are not allowed to do more than form the conjecture by certain physiologists, who at the same time are well aware that the simple division of, or pressure upon a nerve, is sufficient to destroy the communication of external impression to the brain, even while red blood is circulating in a living body. We do not suppose that the nervous fluid alone is the medium of sensation, any more than that the nerve alone is; but we believe that, for the existence of sensation, or for our being rendered sensible of some outward impression, both a nerve and a nervous fluid are indispensably requisite; the fluid standing in the same relationship to the nerve as the blood does to the blood-vessel; or as what is active, stands to what is passive; the latter deriving all its reaction from the former, and giving the appearance of *its* being the sole mover. We would request all physiologists to consider the source of the seminal fluid, and the *size* of the vessels through which it passes, and then to reflect upon the seminal tubes of the insect race conjointly with the nature of the fluid itself; how that it must be essentially as intrinsically diverse in every genus and species of that race, howsoever similar the outward appearance may be, as are the genera and species themselves. If they have any power of reflection or correct judgement, such considerations will ever strike them with astonishment, and cause them to be very backward in expressing their disbelief of, or in their pronouncing as ridiculous. the notion of an animal fluid, not electrical or galvanic, circulating through the nerves, and existing in, because capable of being eliminated from, the blood, and imparting to the blood, so long as it itself is vital and fluid, both vitality and fluidity.

of

of the Vital Principle, the *Anima* of Stahl, the Animal Spirits of Des-Cartes, the Nervous Fluid, the Galvanic Fluid of some late writers, the *Vis Medicatrix Naturæ* of Cullen, the *Materies Vitæ diffusa* of Hunter, the Principle of Irritability. All these notions, however, have failed to satisfy the mind, which is averse from conjecture, and which exults at beholding truth in its own light! When we observe, therefore, that certain specific purposes and designs are fulfilled and accomplished by the organic structures that are displayed before us in all the animal and vegetable creation in such an endless variety,—when we observe that these structures depend momentarily upon the presence of a fluid answering to blood, by virtue of which alone they live, and thence by living, act,—when we observe that there are gradations of excellence in the condition of the blood, and that the higher or the more exalted the condition, the more wonderful is the organization which is produced from and preserved by it;—we admire the gradation of existence, the subordinate dependence of one thing upon another: we behold the perpetual relationship and distinction between cause and effect maintained and adhered to; the order of subsistence to be that of existence: in short, we discern the rules of true philosophy scrupulously observed, and her axioms supported and substantiated by all our experience.

With these principles in mind, we have followed the successive gradations of development as represented in the formation of the chick in the egg, and as described by several patient observers of this process from first to last. A gelatinous molecule, as it is called by Sir E. Home, from which the future embryo is to be formed, would appear to the naked eye as a most confused, indeterminate fluid; but is found by the microscope to have its centre made up of globules $\frac{1}{2800}$th part of an inch in diameter, surrounded by circles of a mixed substance, made up also of small globules and of some that are larger and oval-shaped.

From this, as from a commencing point, the work of formation proceeds; the albumen or white and yolk of the egg are nothing else than materials suited exactly to the purposes and objects intended by this molecule or formative substance, and derived, like the elements of the molecule itself, from Nature's stores. The white and the yolk of the egg respectively to the molecule, are passive in the first stage of formation; but as these stages proceed, the elements of which *they* consisted are elevated into a new condition, and in this condition form a part of the thence increasing and developing animal; they thus become relatively to the remaining portion of the white and yolk of the egg, active and living; and thence, by virtue of *their* new condition, are instrumental in developing the purposes and designs of the living point or molecule still further; so that each distinct stage is a medium and instrument, bearing the energy of the antecedent stage fully and perfectly to the subsequent one; each stage is thus passive to the influences or designs and purposes of the antecedent one, and thence active in influencing the stage subordinate or secondary to it. Thus, and thus alone, are we enabled to see how the purposes and designs of the primitively living molecule, or how its living energies and forces, can flow determinately and designedly through all the dependent stages, until the work intended is accomplished, or at its end—the adult being!! from whence as from a new beginning the work can proceed *de novo*, and a multiplication of the species emulous of an infinity be effected, and by the multiplication, a succession thereof emulous of an eternity. And herein we have another rule of true philosophy confirmed in nature; viz. "that the end rules the cause, and the cause the effect;" or, "the end is the all of the cause, and the cause the all of the effect."

In declaring these principles we know that we are liable to be misapprehended by reason of the defect of terms, or of our defective application of the terms in common use: but if there is any meaning in language, we do not employ any of its terms to signify nothing; and consequently in making mention of *life*, we only use the term to designate the relationship in which one condition of organization or of animal matter, whether fluid or solid, stands to another condition in the same being. Thus, as we have stated above, the blood in relation to the organ it permeates is as its life; but in relation to the fluid which causes it to be what it is, and which in the order of formation was prior thereto, the blood is *only vital in a secondary or inferior sense!*—Of all material fluids, the seminal fluid is most vital; but yet is not life, nor can it bear any proportion to life, because its product is merely one limited thing, differing from all other things and enjoying only limited capabilities: whereas what is unlimited, possessing all things infinitely, and thence enjoying infinite capabilities, being above and beyond the nature of matter and all finite things, cannot be represented nor defined by natural language; for language is derived from the limited things of nature, and the definite forms thereof.

To make the application.—The operations, the changes, the phænomena which are said to result from a principle called Life (from a principle that has never been defined), do all convey to us, as intelligent beings, some idea of intelligence and design: but we do not on that account confound the intelli-

gence

gence and design with the works so wisely designed. Consequently, we do not require, nor ought we to assume, the existence of a principle of life, in addition to the all-intelligent, all-designing, and only living hand. Such an assumption bewilders the mind, by turning it out of its natural and proper course, and causes Science, the golden fruit of its labours ! ! ! to scatter its seeds over the barren fields of mysticism and incredulity.

XLIII. *Collections in Foreign Geology.*—[No. IV.] *By* H. T. De la Beche, *Esq. F.R., L., and G.S. &c. &c.*

[Continued from page 176.]

11. *On the Chalk of the Cotentin; by* M. J. Desnoyers*.

Chalk Formation or Baculite limestone (*Calcaire à Baculites*).

Synonymes. *Banc des Baculites.* (De Gerville.) *Baculite limestone, Calcaire à Baculites.* (C. Prévost, Defrance, De la Beche, de Caumont.) *Variety of Falun or Marl.* (De Gerville.)

THE common disposition to attribute characters to chalk, even though geologically considered, which in many places only belong to a portion of its upper or middle divisions, such as whiteness, earthy fracture, loose and tufaceous texture, the presence of silicate of iron, or siliceous concretions, feeble traces of which characters are alone visible in the baculite limestone (of the Cotentin), has, doubtless, as yet prevented this rock from being recognized as a complete equivalent of this important formation.

* Extracted from the author's *Mémoire sur la Craie et sur les Terrains Tertiaires du Cotentin*, inserted in the *Mémoires de la Société d'Histoire Naturelle de Paris*, vol. ii.

The whole of the above memoir is interesting, more particularly that part of it here extracted, as it shows the melting together, if I may so express myself, of the chalk and greensands; that is to say, there is a rock here described which contains the fossils found in both. This fact is of importance, as it shows that we should guard against attaching too much importance to some of those divisions made in this class of rocks, derived from their difference in mineralogical structure. M. Desnoyers, in common with some of the continental geologists, seems to include under the head of "Chalk formation," the Upper and Lower Chalk, the Upper Greensand, the Gault, and the Inferior Greensand of our English series; there is however, occasionally, some little uncertainty as to the latter, which appears sometimes to be separated from, at others included in, the Chalk formation. Be this as it may, the Baculite limestone of this part of Normandy is a good example of the great changes that take place in the mineralogical character of the more modern rocks in horizontal distances.

I have been obliged in some places to condense this extract, in order to accommodate it to the necessary limits; but have, I believe, omitted nothing material.—*Trans.*

Reduced

Reduced to the beds which alone belong to the baculite limestone, this rock appears to me to be as easily determinable by its mineral structure and organic remains as any known formation. Always, however, rejecting the solid calcareous beds, containing milliolites, and those with pisolite concretions, which under the name of *fahlun* have been confounded with the friable beds of the *calcaire à Cerites*, and perhaps a chloritic limestone with and without nummulites.

Thus limited, this formation is composed of a calcareous rock, (the true baculite limestone,) commonly white or yellow, solid, heavy, homogeneous in appearance, and nearly as compact as the hardest of the Jura limestones *: it often presents spathose laminæ, and its texture is sometimes slightly granular. When the spathose solution has not uniformly penetrated the mass, which most frequently occurs, the cause of this granular and false oolitic appearance is discovered in numerous rounded fragments of corals and shells, which sometimes form irregular nests, the destruction of which, joined to the absence of the spathose cement, often produces cellular cavities in the midst of the most compact beds. This first and most common state completely represents the coarse chalk of Saintonge and Perigord. A penetration of siliceous matter often renders this structure complicated, by producing a complete mixture with the calcareous paste, a sort of siliceo-limestone; or forms in the upper part, as at Fréville, isolated nodules of pale chert covered by an earthy white calcareous substance, resembling those of Maëstricht, and the ancient chalk. Traces of this double precipitate may be observed in the thin and irregular veins of these two substances, which cross and vertically traverse the strata.

When there is a mixture of the debris of more ancient rocks, especially of sandstone and quartz, the beds take an arenaceous structure, become a kind of sandstone, always, however, hardened by the same calcareous cement: this is to be observed at Bonne Ville and Orglandes in the upper beds. When on the contrary there is no mixture of the paste or fragments of foreign substances, beds alone formed of very attenuated marine bodies, or small calcareous fragments, sometimes white and staining, (*fahlun crayeux* of Nehou,) commonly united, varying in size from the finest powder to that of a nut, then constitute incoherent marls, worked for agricultural purposes, as are those of the *calcaire grossier*, which have been confounded with them. The latter can however be distinguished from them by the presence of *Miliolites*, *Rotalites*, and other mi-

* Equivalents of the Oolites.—*Trans.*

croscopical

croscopical *Cephalopodes*, which I have never seen in the marls of the chalk, where they are replaced by small foraminous corals, *Craniæ* and *Thecideæ*. These latter deposits much resemble, as to fossils and incoherence, the beds, also subordinate to chalk, of Mirambeau, in the Charente Inferieure, and that of Maëstricht.

The different kinds of baculite limestone, such as have been above enumerated, present either, as at Orglandes, unconnected and unstratified masses of compact limestone in the midst of calcareous gravels containing the same fossils, and subordinate to the same formation; or, as at Bonne Ville, very extended thin beds, uniform tables, for the length of many yards; or, as at Néhou, isolated marls without solid beds; or lastly, as at Fréville, the entire system, composed of many nearly horizontal strata, alternately coherent and friable, compact and marly; incontestably proving the relations of the beds, elsewhere found isolated. The whole, however, as far as can be judged from the few known places where workings are carried on, of infinitely less thickness than the chalk of the great basins of France and England.

I am only able to afford a very imperfect idea as to their topographical extent, and shall add nothing to the notices collected by M. de Gerville, who can best assign them their true limits. This rock, more particularly developed to the S. of Valognes, in a space comprised between Sainte Mère Eglise, Montebourg, and Pont l'Abbé, would appear to have a direction from E. to W., and to form one system, at present interrupted by several small rivers. I have only studied it in the communes of Fréville, Cauquigny, Bonne Ville, Orglandes, and Golleville, to which list M. de Gerville adds Gourbeville, Hauteville, Néhou, Rauville, and Sainte Colombe. I cannot however assert that all these localities would afford the true compact limestone, equivalent to chalk.

I have no where very evidently seen the immediate superposition of this rock on those more ancient, though many circumstances connected with relative position and dip lead me to conclude that it rests on the oolite limestone, named *Calcaire de Valognes*, at Orglandes and Picauville; on *Calcaire avec Gryphées arquées** at Cauquigny and Fréville, and on transition rocks near Néhou; but I have no where observed the slightest fact which might lead us to doubt the posteriority of this formation to all of them. I can with certainty affirm

* Lias: I conceived the Fréville baculite limestone as resting upon this rock in the Geol. Trans. new series, vol. i. p. 88. 1822.—*Trans.*

that

that it existed prior to the *calcaire grossier* and its marls. Every where, in fact, where the baculite limestone is covered, at least to my knowledge, it is evidently so by one of the beds of this latter formation, especially by the concretionary limestone.

When, as with certain marls, the contact with the *calcaire grossier* occurs as a leaning against rather than as a real superposition (as at Fréville, Orglandes, and Bonne Ville), no traces can be observed of its passage beneath the beds of the compact chalk; while strong proofs of the contrary are found in the real position of one of these marls above the baculite limestone of Sainte Colombe, in the absence of any kind of bed above the *calcaire grossier* which might represent the baculite limestone, and in the absence in the midst of the latter of every organic or mineral remain which could have belonged to the former. A still stronger proof would be afforded by the dip of the baculite limestone beneath the *fahluns* of the *calcaire grossier*, which I consider I have observed at Orglandes; yet however, too imperfectly to be given as a positive fact.

I shall not further insist on these mutual relations, which examples of real stratification will make more clear, by showing the direct position of the chalk beneath the *calcaire grossier*.

1. *Section of the principal Baculite Limestone Quarry at Fréville.*

Traces of *Calcaire grossier*, 6 to 8 feet.	1. Vegetable earth. 2. Rolled debris of quartz, white sandstone, and chert; fragments of corroded and worn milliolite limestone; enveloped in a brown sandy clay. 3. Concretionary pisolite limestone, either imbedding these debris, or more homogeneous, divided into plates of three or four inches.
Chalk, 15 to 20 feet.	4. Pale chert, surrounded by a chalky substance. 5. Incoherent limestone, slightly marly, containing many small corals, thecideæ, craniæ, and nucleolites. (1st *fahlun*). 6. Hard and compact baculite limestone. 7. Calcareous marl, nearly friable, (*fahlun*). 8. A slightly cellular bed of compact limestone. 9. Nearly powdery and incoherent bed. 10. An irregularly indurated bed. 11. Very continuous bed, the most compact of the whole quarry. The same fossils in all the beds.

2. *Quarry*

2. *Quarry named the Fosse de la Bonne Ville.*

1. Vegetable soil.
2. Greenish clay, or bad fullers-earth; of considerable thickness in the vicinity.

Calcaire grossier.

3. Very hard nodules of greenish limestone containing millionites, and of concretionary limestone; 1 to 2 feet.

Chalk.

4. Compact limestone, mixed with small quartzose pebbles, constituting a single very hard horizontal bed, a kind of pavement for more than 600 paces. *Craniæ*, *Ammonites*, *Baculites*, &c.; $2\frac{1}{2}$ feet to 3 feet.
5. Less coherent limestone, and more gravelly, with the same shells.

3. *Quarries at Orglandes.*

1. Vegetable soil.
2. Traces of brown clay.

Calcaire grossier.

3. Pisolitic concretionary limestone, with impressions of *Calcaire grossier* shells. (*Cerithium Cornucopiæ*, *Hipponyx*, *Clypeaster*, &c.)

Chalk.

4. Gravelly limestone bed, with chalk fossils and small sandstone pebbles; 2 feet.
5. A very irregular mass or bed, compact, sublamellar, or mixed with silex; 3 to 4 feet worked.

Organic remains of the Baculite limestone, found for the most part at Orglandes, Fréville, Golleville, Néhou, and preserved in the collections of MM. de Gerville, Defrance, and C. Prévost, and in my own cabinet. Far from pretending to present a complete enumeration of the species in this rock, I shall only notice those incontestably found with the *Baculites* in the compact limestone and subordinate marls*.

Shells.		Other Localities.
Nautilus	(2 species at least)	
Belemnites mucronatus . .	Schlott	In the upper white chalk at Meudon, Bougival, and Mantes near Paris; marly chalk of Ilseburg in Prussia?

* I have been obliged, from the necessary length to which these communications must be confined, to omit many useful observations which accompany the names of most of the species, and to form the table upon a somewhat altered plan.—*Trans.*

Shells.		Other Localities.
Baculites vertebralis . . .	Lamarck	Lower chalk at Maëstricht, Aix-la-Chapelle, Dantzick, Tours, Rouen, Nogent le Rotrou, &c.
(2 other species . . .	De Gerv.)	
Hamites cylindricus . . .	Defrance.	
Scaphites (2 species)		
Ammonites rhotomagensis	Defrance	These four species are found in the lower chalk (firestone) of Rouen, Havre, Mortaigne, and Sussex.
——— *Gentoni* . . .	Defr. .	
——— *varians*	Sowerby	
——— *constrictus* . .	Sow. . .	
Gryphæa (small species)		Very common in the lower chalk of the departments of La Sarthe, Eure et Lior, and Orne.
Ostræa . . . (var. of *O. vesicularis* named *incerta* by Defr.) . . .		More resembles those of the tufaceous chalk of Tours, Périgueux, Mirambeau, and Maëstricht, than those of the upper chalk of the Paris basin.
Pecten quinquecosta . .	Sow.	Common in lower chalk.
——— *versicostatus* . . .	Defr.	
——— *quadricosta* . . .	Sow. . .	Greensand near Exeter, and of Wiltshire; Maëstricht.
——— *intextus*	Br. . . .	Havre.
Trigonia scabra ?	Lam. . .	Comm. in the lower chalk and greensand of Rouen, Regmalard, Tours, Perte du Rhone, Mirambeau, and Blackdown*.
Inoceramus	Park.	
Gervillia solenoides . . .	Defr. . .	Hard chalk of the Isle d'Aix; sandstone subordinate to the lower chalk of Longy (Orne).*
Crania antiqua	Defr.	
——— *striata*	Defr.	
——— *stellata*	Defr.	

* Also in the inferior greensand of Lyme Regis, Dorset.—*Trans.*

Thecidea

Shells.		Other Localities.
Thecidea radians	Defr. . .	Maëstricht.
——— *recurvirostra*	Defr. . .	Maëstricht.
Terebratula subplicata .	Mantell	Upper chalk of Lewes, Maëstricht, Tours, Beauvais, &c.
——— *pectita* . . .	Sow. . .	Havre.
——— *recurva* . . .	Def. . . .	Maëstricht.
——— *gallina* . . .	Br. . . .	Common in the older chalk.
——— (*alata* . . .	Lam.) . .	

Radiated Animals.

Asterias		Chalk of Paris, Kent, Sussex, &c.
Apiocrinites ellipticus . .	Miller	Lower chalk of Touraine, Wiltshire, Kent, and Sussex.

Echinites.

Nucleolites (approaching *N. asterostoma* . .	Desm.)	Nucleolites are common in the lower chalk of Tours, Rouen, and Verona *.
——— (approaching *N. columbaria* . .	Lam.)	
Spatangus prunella . . .	Lam.	Chalk of Meudon, Havre, and Maëstricht.
——— *cor anguinum*	Lam.	Characteristic of the lower chalk in many places.
or *suborbicularis*	Defr.	
Echinus		Small species approaching *E. petaliferus*, and *E. Menardi*, Desm. found in the greensand of Cap la Hêve and Mans.
Ananchites ovata	Cuv. & Br.	Upper chalk of Meudon and the environs of Paris.
Clypeaster oviformis. . .	Lam. . . .	Chalk of Verona.

Corals, &c.

Flustra reticulata	Desm. & Les.	
——— *flabelliformis* . .	Lamouroux.	
Lichenopora cretacea . .	Defr.	Maëstricht, Meudon.

* Found also in the chalk with quartz grains (the lowest part of the chalk) in the vicinity of Lyme Regis, Dorset.—*Trans.*

Lunulites cretacea . . .	Defr.	Maëst., Tours, & Lewes.
Millepora (approaching *antiqua* Defr.)	Mirambeau.	
Pagrus proteus	Defr.	Meudon, Tours.
Alveolites	Lam.	Picardy, Havre.
Caryophyllia	Mantell . . .	Sussex, and Kent.
Alcyonium globulosum .	Defr.	Sussex.

This table, though necessarily short and incomplete, nevertheless presents us with many important results. We in the first place remark more than thirty species recognised elsewhere as alone belonging to the chalk formation, some to the upper chalk, (*Belemnites mucronatus*, *Ananchites ovatus*, *Crania*); others, and these form the greater number, to the middle chalk (*craie moyenne*); and some, though peculiar to the baculite limestone, more nearly approach the fossils of the chalk, than those of any other formation.

A second and equally essential observation is, that while determining the species in the most careful mode possible, whose *analogues* I have pointed out in equivalent rocks, slight differences have presented themselves, which were at least sufficient to form varieties.

Another fact worthy attention, is the absence of many genera and species alone found in this formation in the neighbouring basins. Such are the *Turrilites*, *Gryphæa colomba*, *G. striata*, *Ostræa carinata*, &c. &c., and especially the zoophytes, of the genera *Chenendopora*, and *Halliroa* (Lam^{x}.), *Ventriculite* and *Spongus* (Mantell). This negative fact is another point of resemblance between the baculite limestone and the Maëstricht chalk.

After such strong arguments as those deduced from relative position and organic remains, which have so perfectly agreed in proving the geological identity of the baculite limestone and the chalk, it may perhaps appear superfluous again to notice so secondary and accidental a circumstance as that of the nature of the rock itself, especially in such a rock as the chalk, in which is found such a variety of composition, texture, colours, and elevations above the sea.

The chalk has generally been considered exempt from the compactness common to so many other secondary rocks, or at least as having presented but few examples of it; and yet it will perhaps be seen that it is not a greater rarity than in the *Calcaire grossier* and the Jura limestone. This modification of the chalk occurs under various circumstances, and equally in all its divisions. Thus we see two isolated deposits of white chalk

chalk with flint acquire, in two countries distant from each other, (the N.E. of Ireland and the Vicentin,) a compactness with conchoidal fracture, more perfect, more homogeneous perhaps, than any of the Jura limestone beds *, even passing into a granular and spathose structure; and we are enabled, with the effect, to discover the cause of this alteration from a previous state, in the influence of the igneous action of the basalts common to these two countries.

The uplifting of the chalk at Corfu Castle appears to coincide with its hardness; as Mr. Webster informs us that this chalk, though exceedingly hard when vertical and contorted, is soft and tender when horizontal.

Another circumstance attending the compactness of chalk is its connection with more ancient calcareous deposits, joined to very considerable elevation. Such appear to be those singular deposits of the Alps of Savoy and Bavaria, which have been referred to chalk and greensand, which, notwithstanding their great hardness, their sublamellar and compact texture, their black colour, and their elevation above the sea, MM. Brongniart, Buckland, Beudant, Boué, and Deluc agree in referring to this great formation. It was when on the subject of these deposits, that M. Brongniart has shown the little importance of external characters in geology, compared to the value of zoological characters, and relative position.

The middle beds of the chalk, (*glauconic crayeuse*, *tufau*, chalk-marl, &c.) commonly tender and marly in France and England, are there in some situations of considerable hardness; and in Prussia, near Quedlingburg, Halberstadt, Goslar, as well as in some other parts of Germany, they approach, as regards this character, the Jura limestone, with which M. Keferstein has sometimes confounded them. The Planerkalk, which may certainly be referred to the chalk formation, presents the same compactness. Some beds of this formation in the departments of the Basses Alpes, and Alpes Maritimes, commonly possess a no less remarkable hardness.

The immediate contact of fresh-water limestone on chalk appears also under some circumstances to have hardened it by cementation; at least I thought I observed this in the environs of Tours and Nogent le Rotrou. An analogous spathose penetration, most probably, however, independent of fresh-water deposits, appears to have rendered the upper beds of the chalk very compact, and even completely crystalline. MM. Cor-

* The Jura limestones are in general much more compact than their British equivalent the Oolite formation.—*Trans.*

dier,

dier, Omalius d'Halloy, Constant Prévost, de Bonnard, and Brongniart, have observed this hardening of the chalk in many parts of France, either in entire beds or in nodules. M. Elée de Beaumont has observed many remarkable facts in the departments of the Seine Inferieure, and Eure, at St. Etienne de Rouvray, Louviers, Caumont, Vernon, Rolleboise, in the peninsula formed by the Seine from Elbeuf to la Bouille: these are, the occurrence, in the midst of loose beds, of unconnected beds or masses, sufficiently compact, homogeneous, and sublamellar to be worked as marble, the connection of which with the upper chalk was observed by M. de Beaumont. Similar deposits appeared to me to occupy the same situation at Châteaudun (Eure et Loire).

MM. Conybeare, Webster, Parkinson, Phillips, Mantell, and Winch, have given numerous examples of this modification of the chalk of the S.E. of England and Yorkshire, in their memoirs.

But to return to the Valognes chalk:—No other bears so great a resemblance to it in texture and composition as the spherulite coarse chalk (*craie grossière à spherulites*) of Sainonge, Périgord, and Gascony. We have, in fact, the same union of a compact and crystalline cement, penetrating a mass of the remains of shells and corals; the same alternation of friable and solid, compact and granular beds; in a word, it appears in these cases to be the calcareous sand of Maëstricht connected by a spathose paste, the effect of a chemical solution posterior to a mechanical deposit.

Can these analogies, joined to the observed place of the coarse chalk of Saintonge and Périgord, between the greensand (*sable vert*) and chalk with flints, suffice for supposing the Valognes chalk to have occupied a precisely identical situation in the midst of the great chalk formation? We have seen that, in general, compactness is but a slight kind of approximation; and that from the assemblage of the other characters, the baculite limestone does not completely resemble any one of the various divisions, elsewhere comprised between ferruginous or iron sand (*sable ferrugineux*) and the tertiary rocks. May we not presume that it is not identical with any one of them, but that, contemporaneous with them, it represents the whole in geological order, with variations produced by the different circumstances of the deposits? It will be observed that these modifying circumstances must have been the same in two other localities, above cited as approaching the baculite limestone, Maëstricht and La Charente. Now, these three chalk rocks are remarkably analogous in being all deposited

posited near transition rocks: the chalk of Maëstricht, at the foot of the Ardennes; that of Saintonge, not far from the granites of La Vendée; and that of Valognes, near the old rocks of the Cotentin. The absence in the latter country of the ferruginous sand (*sable ferrugineux*), the still uncertain place of the greensand (*sable vert*), and of every rock which might exclusively represent the upper chalk; the mixture of the fossils of the two chalks, and the isolated geographical situation of this little basin,—are circumstances sufficiently powerful to afford colour for this supposition. H. T. D. B.

XLIV. *On Caustics. By Mr.* H. Moseley, *A.B. St. John's College, Cambridge.**

THE term Caustic is throughout the following paper to be understood as applying itself to the locus of the ultimate intersections of consecutive rays of light after their reflexion or refraction at a curvilinear surface.

On Caustics by Reflexion.

To determine the caustic formed by rays incident from a given point S on a given reflecting curve; let us imagine an ellipse, having its focus in S, to be so described through any point P of the reflecting curve, as to have a contact of the second order with the curve in that point,—conditions which it may evidently be made to satisfy, since it involves five arbitrary constants in its equation, of which two may be made to fix the position of its focus, and the remaining three will give it the contact required.

Now, since the curvature of the osculating ellipse and that of the given curve are the same in their point of contact, rays incident at that point will be similarly reflected by the two curves. But in the ellipse, rays incident from S will be reflected so as to intersect in (the further focus) H. The intersection of rays reflected by the given curve will, therefore, also be in H. And the question reduces itself to the determination of the curve described by the focus H of that osculating ellipse; whose focus S is in the radiating point.

Let $\alpha\,\beta$ be the axes of the ellipse thus taken at the point P; p a perpendicular to the common tangent of the two curves at that point, from S; r the length of the incident ray, SP; ϱ that portion of the reflected ray which is intercepted between the reflecting curve and the caustic, or HP.

* Communicated by the Author.

Then

Then we have by a property of the ellipse

$$p^2 = \frac{\beta^2 r}{2\alpha - r}$$

$$\therefore \therefore \log. \frac{p^2}{r} = \log. \beta^2 - \log. (2\alpha - r)$$

$$\therefore \quad \frac{d \log. \frac{p^2}{r}}{d r} = \frac{1}{2\alpha - r} = \frac{1}{\varrho}$$

$$\therefore \quad \varrho = \frac{d r}{d \log. \frac{p^2}{r}}.$$

Now p and r are the same in the reflecting curve and the ellipse; and p, being in fact a differential expression in x and y of the first order, $\frac{dp}{dr}$ is one of the second; it follows therefore, that since the differential coefficients of the first and second orders are the same in the two curves, r, p, and $\frac{dp}{dr}$ are the same, and therefore the expression $\frac{dr}{d \log. \frac{p^2}{r}}$ is the same. Taken therefore in the reflecting curve it will determine the length of the reflected ray.

The above equation is given (deduced on other principles) in Mr. Coddington's Optics. The reader is referred to that ingenious work for a method of determining from it the *equation* to the caustic in terms of r and p.

In that particular case of the caustic by reflexion, in which the radiating point is situated at an infinite distance, and the incident rays are therefore parallel to one another. The equation to the caustic may be determined immediately in terms of its rectangular coordinates, by considering it as traced out by the focus of an osculating parabola, having its axis parallel to the direction of incidence.

Calling x, y the coordinates of any point P of the reflecting curve, X, Y the coordinates of the corresponding point S of the caustic (or of the focus of the parabola), and 4α the parameter of the parabola; we have,

$$\left.\begin{aligned} (x - \mathrm{X})^2 &= 4\alpha \{y - \mathrm{Y} + \alpha\} \\ \therefore x - \mathrm{X} &= 2\alpha \left(\frac{dy}{dx}\right) \\ 1 &= 2\alpha \left(\frac{d^2y}{dx^2}\right) \end{aligned}\right\}$$

$$\therefore \quad x - \mathrm{X} = \frac{\left(\frac{dy}{dx}\right)}{\left(\frac{d^2y}{dx^2}\right)} \quad \ldots\ldots\ldots\ldots \quad (1)$$

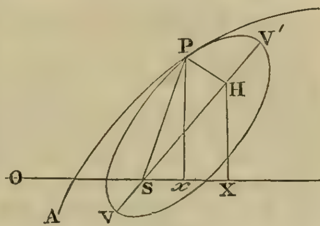
$y - \mathrm{Y} =$

$$y - Y = \frac{\left(\frac{dy}{dx}\right)^2 - 1}{2\left(\frac{d^2y}{dx^2}\right)} \quad \ldots\ldots\ldots \quad (2)$$

As before, the equations (1), (2) here proved with regard to the parabola only may be shown to hold also in the reflecting curve. By eliminating x and y between them and the given equation in x and y, the required equation in X and Y is readily determined.

To investigate the equation to a caustic formed by rays diverging from a point at a finite distance in terms of its rectangular coordinates.

Let S be the radiating point, AP the reflecting curve, VPV′ an ellipse having a contact of the second order with the reflecting curve in P, and having its focus in S. The given position of the focus determining two of the arbitrary constants, and the contact, the remaining three. The locus of H is the caustic required.



X, Y coordinates of H } on any axis OX passing
x, y ———————— P } through S,
$VV' = 2\alpha$.

$$\therefore \left\{(X - x)^2 + (Y - y)^2\right\}^{\frac{1}{2}} + \left\{x^2 + y^2\right\}^{\frac{1}{2}} = 2\alpha.$$

Now by supposition, y' and y'' or $\frac{dy}{dx}$, $\frac{d^2y}{dx^2}$, and also x and y are the same in the reflecting curve and in the ellipse $\therefore$ differentiating twice with respect to x and y, the resulting equations will hold in both curves

$$\therefore \frac{(X - x) + (Y - y)y'}{\left\{(X - x)^2 + (Y - y)^2\right\}^{\frac{1}{2}}} = \frac{yy' + x}{(y^2 + x^2)^{\frac{1}{2}}} \quad \ldots\ldots \quad (2)$$

Hence, taking the h. logs. and differentiating again,

$$\left.\begin{matrix} \dfrac{-1 - y'^2 + (Y - y)y''}{(X - x) + (Y - y)} + \\ + \dfrac{(X - x) + (Y - y)y'}{(X - x)^2 + (Y - y)^2} \end{matrix}\right\} = \left\{\begin{matrix} \dfrac{1 + y'^2 + yy''}{yy' + x} \\ - \dfrac{x + yy'}{x^2 + y^2} \end{matrix}\right.$$

whence by elimination in equation (2) and by reduction, we get,
$$\frac{Yy''}{X + Yy'} = \frac{1 + y'^2 + yy''}{x + yy'} - \frac{x + yy'}{x^2 + y^2}$$

$$\therefore y''. \left\{\frac{Yx - Xy}{X + Yy'}\right\} = \frac{(y - xy')^2}{(x^2 + y^2)} \quad \ldots\ldots \quad (3)$$

 Elimi-

Eliminating x and y between equations (2) and (3) and the given equation to the reflecting curve, we have the equation in X and Y to the caustic.

On Caustics by Refraction.

The oval whose equation is

$$(x^2 + y^2)^{\frac{1}{2}} + m\{(a - x)^2 + y^2\}^{\frac{1}{2}} = \mathrm{A}$$

will refract rays accurately from the origin to a point in the axis at distance (a) from it *. Let the coordinates x, y be transferred to an axis inclined at an angle (θ) to the former; then

$$\left.\begin{array}{l}\{(x \cos\theta - y \sin\theta)^2 + (x \sin\theta + y \cos\theta)^2\}^{\frac{1}{2}} \\ + m\{a + y \sin\theta - x \cos\theta)^2 + (x \sin\theta + y \cos\theta)^2\}^{\frac{1}{2}}\end{array}\right\} = \mathrm{A}.$$

Which equation involves four arbitrary constants or if (m) be given three; hence the curve may be made to have from a given origin a contact of the second order with any given refracting curve, and the medium being the same, both curves will refract a small pencil of rays to the same point; the locus of this point will therefore be the caustic required. Let X and Y be the coordinates of this point; then we have,

$$\{x^2 + y^2\}^{\frac{1}{2}} + m\{(\mathrm{X} - x)^2 + (\mathrm{Y} - y)^2\}^{\frac{1}{2}} = \mathrm{A}$$

And proceeding as in the last case, we may obtain equations determining the caustic.

In the case in which *parallel* rays are incident on a refracting curve: let there be taken an ellipse having its axis parallel to the direction of incidence, and its ellipticity, or the ratio of its semiaxis major to its eccentricity, equal to the ratio of the sine of incidence to the sine of refraction; and let it be made to have a contact of the second order with the refracting curve in any point P. Then it is evident that a small pencil of rays incident at P, will be made to intersect in the further focus.

Calling (X, Y) the coordinates of this focus, (x, y) those of the point P, and (a) the semiaxis major of the ellipse; we have

$$\left.\begin{array}{l}\dfrac{(\mathrm{X} - x)^2}{1 - m^{-2}} + \left\{y - \mathrm{Y} - a m^{-1}\right\}^2 = a^2 \ \ldots\ldots\ (1) \\ -\dfrac{\mathrm{X} - x}{1 - m^{-2}} + y' \left\{y - \mathrm{Y} - a m^{-1}\right\} = 0 \ \ldots\ldots\ (2) \\ \dfrac{1}{1 - m^{-2}} + y'^2 + y''\left\{y - \mathrm{Y} - a m^{-1}\right\} = 0 \ \ldots\ldots\ (3)\end{array}\right\}$$

$\therefore$ by equa. (3) $\quad y - \mathrm{Y} - a m^{-1} = -\dfrac{1 + (1 - m^{-2}) y'^2}{y''(1 - m^{-2})}$

$\therefore$ by $\quad$ (2) $\quad \dfrac{x - \mathrm{X}}{1 - m^{-2}} - \dfrac{y'\{1 + (1 - m^{-2}) y'^2\}}{y''(1 - m^{-2})} = 0$

* Coddington's Optics.

$\therefore\ x - \mathrm{X}$

$$\therefore x - X = \frac{y'}{y''}\left\{1 + (1 - m^{-2})y'^2\right\} \text{..................} (\alpha)$$

$$\text{by (1)} \quad \therefore \frac{y'^2\{1+(1-m^{-2})y'^2\}^2}{y''^2(1-m^{-2})} + \frac{\{1+(1-m^{-2})y'^2\}^2}{y''^2(1-m^{-2})^2} = a^2$$

$$\therefore a^2 = \frac{1+(1-m^{-2})y'^2}{y''^2(1-m^{-2})^2}\left\{1 + (1 - m^{-2})y'^2\right\}$$

$$\therefore a = \pm \frac{\{1+(1-m^{-2})y'^2\}^{\frac{3}{2}}}{y''(1-m^{-2})} *$$

$$\therefore y - Y = -\frac{1+(1-m^{-2})y'^2}{y''(1-m^{-2})} \pm \frac{\{1+(1-m^{-2})y'^2\}^{\frac{3}{2}}}{y''(1-m^{-2})} \cdot m^{-1}$$

$$y - Y = -\frac{1+(1-m^{-2})y'^2}{y''(1-m^{-2})}\left\{\mp\left\{1+(1-m^{-2})y'^2\right\}^{\frac{1}{2}} + 1\right\} \text{...} (\beta)$$

$$\text{also, } x - X = \frac{y'}{y''}\left\{1 + (1 - m^{-2})y'^2\right\} \text{......................} (\alpha)$$

Eliminating x and y between the equations (α) and (β) and the given equation to the refracting curve, we obtain an equation in X and Y to the caustic.

If $m = 1$, and the sine of incidence becoming equal to the sine of refraction, we pass from a refracting to a reflecting medium. Calling $4p$ the parameter of the parabola into which our ellipse will have resolved itself, we have since $1 - m^{-2} = \frac{a^2 - a^2 m^{-2}}{a^2} = \frac{2p}{a}$

$$y - Y = -\frac{1 + \frac{2p}{a}y'^2}{\frac{2p}{a}y''}\left\{-\left(1+\frac{2p}{a}y'^2\right)^{\frac{1}{2}}\left(1-\frac{p}{a}\right)+1\right\}$$

$$y - Y = -\frac{1 + \frac{2p}{a}y'^2}{\frac{2p}{a}y''}\left\{-\left(1+\frac{p}{a}y'^2\right)\left(1+\frac{p}{a}\right)+1\right\}$$

neglecting powers of $\frac{p}{a}$ above the first

$$y - Y = -\frac{1}{\frac{2p}{a}y''}\left\{-\frac{p}{a} - \frac{p}{a}y'^2\right\}$$

$$\left.\begin{aligned} y - Y &= -\frac{1 - y'^2}{2y''} \\ \text{also, } x - X &= \frac{y'}{y''} \text{ by equ}^{\text{n}}. (\alpha) \end{aligned}\right\}$$

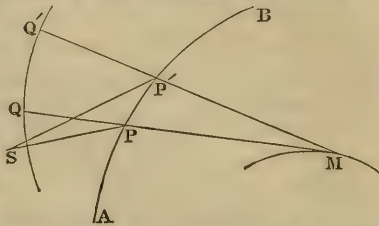
The above equations coincide with those already determined for

* This expression for the semiaxis major of the osculating ellipse evidently reduces itself, as it ought, to the known expression for the radius of curvature, when $m^{-1} = 0$.

for the equation to the caustic by reflexion in the case of parallel rays.

On the Involutes of Caustics by Reflection and Refraction.

Let the consecutive rays SP and SP′ incident from S on the refracting curve AB, be made after refraction to intersect in M.



Now a curve, such that for every point of it $SP + m PM =$ constant, may be taken so as to have a contact of the second order with the curve AB in P, and therefore so as to coincide with it in the consecutive points P and P′.

Also in this curve, and therefore in AB

$$SP + mPM = SP' + mP'M$$

$$\therefore \quad \frac{SP}{m} + PM = \frac{SP'}{m} = P'M.$$

Produce MP and MP′ to Q and Q′, so that $PQ = \frac{SP}{m}$ $P'Q' = \frac{SP'}{m}$,

then $$PQ + PM = P'Q' + PM,$$

or, $$QM = Q'M;$$

therefore the locus of Q is the involute to the locus of M, *i.e.* it is the involute to the caustic.

Let (X, Y) be coordinates of Q, (x, y) of P,

then $$\left\{ x - X \right\}^2 + \left\{ Y - y \right\}^2 = \frac{x^2 + y^2}{m^2} \quad \ldots\ldots \; (1)$$

Now I have demonstrated in the Annals of Philosophy for July 1826, that if there be two curves such that the position of the tangent to any point in the one curve is given in terms of the coordinates to a corresponding point in the other curve, then establishing any relation whatever between the coordinates of these two points, we may differentiate the equation expressing this relation with regard to the coordinates of the latter point, considering those of the former as constant. This principle is manifestly applicable to the case we are investigating, since the position of QM and the distance PQ are given in terms of the coordinates of P, the quantity m, and the position of S; also the position of the normal QM and of the point Q determine the position of the tangent at Q.

Therefore

Therefore differentiating the equation (1) with regard to x and y.

$$(x - X) - y'(y - Y) = \frac{x + yy'}{m^2}$$

$$\frac{m^2 - 1}{m^2}(x + yy') - (X + Yy') = 0 \ldots\ldots (2)$$

In the case of parallel rays, the equations (1) and (2) become

$$(x - X)^2 + (Y - y)^2 = \frac{x^2}{m^2}$$

$$\frac{m^2 - 1}{m^2} x + yy' - (X + Yy') = 0.$$

The equations (1) and (2) together with the given equation $y = fx$ of the refracting curve determine by the elimination of x and y, the involute to the caustic.

Thus when the refracting curve is a straight line taking the axis of (x) perpendicular to it, we have

$$x = \text{constant} = a \quad y' = x^{ty}$$

hence

$$Yy' - yy'\frac{m^2 - 1}{m^2} = 0$$

$$\therefore Y = y \cdot \frac{m^2 - 1}{m^2}$$

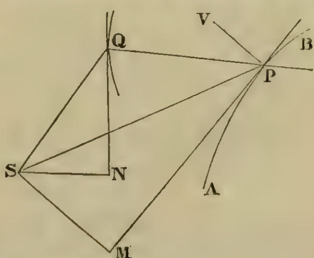
$$\therefore \left\{X - a\right\}^2 + \frac{y^2}{m^4} = \frac{a^2 + y^2}{m^2}$$

hence

$$Y^2 = (m^2 - 1)\left\{(X - a)^2 - \left(\frac{a}{m}\right)^2\right\}.$$

The equation to an ellipse or hyperbola according as (m) is less or greater than 1. According to this condition, therefore, the caustic is the evolute to an ellipse or an hyperbola.

We may establish geometrically a relation between SP and SQ, and the perpendiculars from S on the tangents at P and Q by means of which the involute to the caustic may be readily determined in terms of the radius vector, and the perpendicular on the tangent. For let PM and QN be tangents at P and Q, SM and SN perpendiculars upon them from S.



Let SP $= r$, SQ $=$ R
SM $= p$, SM $=$ P

Draw PV perpendicular to PM

$$\text{Now} \qquad \frac{\sin PQS}{\sin PSQ} = \frac{PS}{PQ} = m.$$

Also PQ is perpendicular to QN, and $\therefore$ parallel to SN

$$\therefore \sin PQS = \sin QSN$$

and
$$PSM = SPV = \angle \text{ of inc}^{cc}.$$
$$NSM = VPQ = \angle \text{ of refr}^{n}.$$

$\therefore$

$$\therefore \frac{\sin \text{QSN}}{\sin \text{PSQ}} = m = \frac{\sin \text{PSM}}{\sin \text{NSM}}$$

$$\therefore \frac{\sin(\text{PSQ} + \text{PSN})}{\sin \text{PSQ}} = \frac{\sin(\text{PSN} + \text{NSM})}{\sin \text{NSM}}$$

$$\therefore \cos \text{PSN} + \sin \text{PSN} \cot \text{PSQ} = \cos \text{PSN} + \sin \text{PSN} \cot \text{NSM}$$

$$\therefore \cot \text{PSQ} = \cot \text{NSM}$$

$$\text{PSQ} = \text{NSM}$$

$$\therefore \text{QSN} = \text{PSM},$$

$$\therefore \frac{\text{R}}{\text{P}} = \frac{r}{p} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

Also, $$\text{R}^2 = r^2 + \frac{r^2}{m^2} - \frac{2r^2}{m} \cos \text{QPS}$$

$$\text{Cos QPS} = \cos \{ \angle \text{ inc.} - \angle \text{ refr.} \}$$

$$\text{Cos} \angle \text{ inc.} = \frac{p}{r}$$

$$\text{Sin} \angle \text{ inc.} = \pm \frac{(r^2 - p^2)^{\frac{1}{2}}}{r}$$

$$\text{Sin} \angle \text{ refr.} = \mp \frac{(r^2 - p^2)^{\frac{1}{2}}}{mr}$$

$$\text{Cos} \angle \text{refr.} = \left\{ 1 - \frac{r^2 - p^2}{m^2 r^2} \right\}^{\frac{1}{2}}$$

$$\therefore m r^2 \cos \text{QPS} = \pm p \sqrt{(m^2 - 1) r^2 + p^2} - r^2 + p^2$$

$$\therefore \text{R}^2 = r^2.\left(1 + \frac{1}{m^2}\right) - \frac{2}{m^2} \left\{ \pm p \sqrt{(m^2 - 1) r^2 + p^2} + r^2 - p^2 \right\}$$

$$\therefore m^2 \left(\frac{\text{R}}{p}\right)^2 = (m^4 + 1)\left(\frac{r}{p}\right)^2 - 2 \left\{ \pm \sqrt{(m^2 - 1)\left(\frac{r}{p}\right)^2 + 1} - \left(\frac{r}{p}\right)^2 + 1 \right\}$$

$$\therefore m^2 \left(\frac{\text{R}}{p}\right)^2 = (m^2 + 1)\left(\frac{\text{R}}{\text{P}}\right)^2 - 2 \left\{ \pm \sqrt{(m^2 - 1)\left(\frac{\text{R}}{\text{P}}\right)^2 + 1} - \left(\frac{\text{R}}{\text{P}}\right)^2 + 1 \right\} \quad \ldots\ldots\ldots\ldots \quad (2)$$

Eliminating (r) and (p) between the equations (1) and (2), and the given equation $p = fr$ of the refracting curve we shall obtain the required equation in R and P.

Thus in the logarithmic spiral, since $\frac{r}{p} = a$, we have by equation (1) $\frac{\text{R}}{\text{P}} = a$, $\therefore$ the involute to the caustic is also a logarithmic spiral similar to the refracting curve. Also the evolute is in this case similar to its involute; therefore the

the caustic is a logarithmic spiral similar to the refracting curve.

If in the equations we have thus deduced for the involute to the caustic by refraction, (m) be made equal to unity, we shall at once determine the involute to the caustic by reflexion. Thus for rectangular coordinates, we have

$$X^2 + Y^2 - 2Xx - 2Yy = 0 \quad \ldots\ldots\ldots \quad (\alpha)$$
$$Yy' + X = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (\beta)$$

Or, to determine the curve in terms of R and P, we have

$$\frac{R}{P} = \frac{r}{p} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (\alpha)'$$
$$R = 2P \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (\beta)'$$

From these last equations it will be easily seen that the involute to the caustic by reflexion, is in fact the curve which would be traced out by a point similarly situated with the radiating point in a curve similar and equal to the reflecting curve made to roll on its circumference, similar points having first been in contact, and both curves being in the same plane.

XLV. *Reply to* Mr. R. C. Taylor's *Remarks on the Hypothesis of* Mr. Robberds *on the former Level of the German Ocean. By* J. W. Robberds, *Esq. Jun.*

[Continued from p. 207.]

AFTER admitting the general validity of the evidence, which I have adduced from historical facts, and from the names of several places on the verge of these valleys, Mr. Taylor has objected to the accuracy of some minor points, which do not materially affect the main argument. I acknowledge readily the error into which I was betrayed, respecting Waybridge. As I could find no village bearing that name on either of the three rivers, I too incautiously adopted Gillingwater's version of it, who, in his History of Lowestoff, p. 27, rendered it Weybread, without even mentioning the real name given by Swinden. Mr. Taylor calls mine a "*mistaken quotation.*" This is not correct; for I *quoted* the word right, but *explained* it wrong, having cited it as "Waybridge (the present Weybread)." The bridge at Acle, so designated, may have existed in the eleventh century, without affording any conclusive proof of the fallacy of my inference from more general and unquestionable facts. I much wish that Mr. Taylor had given his authority for that statement. In the earliest account of the priory founded there in the reign of Edward I., it it is described as *St. Mary of Weyburgh**. But I will not hazard even a conjecture upon so insecure a foundation as the unsettled orthography of that period.

I cannot understand the grounds upon which Mr. Taylor objects to my etymology of Herringby and Herringfleet. His quotation from Domesday Book of *Haringbei*, is decidedly in favour of my opinion. The Anglo-Saxon for *herring* was hæpinᵹ; and at this time it is in Dutch *haring*, in German *häring*, and in French *hareng*. The derivation is so obvious, that no other either has been, or can be, offered;—how then can it be said, that my explanation of it is strained, in order "*to favour a given theory?*" With respect to Herringfleet, the existing form of the name is a far better authority to ascertain its origin, than that which was employed by the Norman surveyors. Their register has but few instances of the same place having been twice entered precisely in the same manner; and the variations of the spelling are such, that nothing can be inferred from the introduction or omission of merely a single letter in any name. In cases of doubtful etymology therefore, truth must be sought after by comparing the ancient and modern appellations with those local or historical contingencies from which the designation was most probably taken. If we apply this rule to Herringfleet, we shall find, that the early entry of *Herlyngflete* proves nothing in opposition to that interpretation, which the present name so obviously suggests. There can be no doubt with respect to the meaning of the last syllable. It is evidently the Anglo-Saxon Fleoꞇ, the signification of which, according to Somner, was "*Æstuarium—an arme of the sea; a place where the sea ebbeth and floweth.*" Junius gives it the same meaning—"*Sinus maris, Æstuarium.*" Under the Latinized form of *Fleta* or *Fletum*, Sir Henry Spelman explains it as "*Æstuarium, fluentum, seu canalis quem aqua fluens et refluens occupat;*" which Dufresne, in his Glossary, has confirmed, by repeating this passage *verbatim* after our Norfolk antiquary. Mr. Taylor's scepticism has made me particular in citing these authorities; for, after all, this syllable is the most important part of the name to which it belongs, and must decide its character. It denotes the situation of the place in question to have been upon an æstuary, or arm of the sea; and this will be found to be invariably the case, in every instance in which the term occurs throughout our maritime districts†. This is the only material circumstance

* Blomefield and Parkins, 8vo. vol. ii. p. 92.

† *Fleet*, at the mouth of the Arm, in Devonshire; *Fleet*, near Portland Isle, in Dorsetshire; *Northfleet* and *Southfleet*, near Gravesend; *Ebbsfleet*, in

circumstance required to substantiate my theory; for whether the adjunct be *Herlyng* or *Heryng*, it cannot alter the fact that Herringfleet, in the Anglo-Saxon times, was situated upon an "æstuary or arm of the sea." This point being settled, the import of the prefixed term must be gathered from probability and consistency, while authority and fact are either silent or discordant. Which then is the most probable and consistent opinion—that a village, denominated in part from its situation on an arm of the sea, should have taken the rest of its name from a fish in which that sea abounds, or that it should have combined with such an appellation the name of another place, in a remote and inland part of the country, with which it does not appear ever to have had the slightest connection? To prefer the latter conclusion would indeed be to strain etymology, for the purpose of "*favouring a given theory*." With equal, if not with greater reason I might have alleged, that the principal canal in the town of Rotterdam is actually called the *Haringvliet;* for it is surely more rational to suppose, that the fishermen, who inhabited Lothingland, kept up an intercourse with the shores of the opposite continent, than to believe, out of mere respect for the orthography of Domesday Book, that they travelled forty miles up the country, to borrow from Harling (a market town of Guiltcross, not Shropham, hundred) a name for one of their early settlements. Nor is Mr. Taylor more fortunate in his objection, that the names of Herringby and Herringfleet can have "no reference to fish, whose habits lead them to avoid *shallow muddy rivers*." This argument sets out with a most flagrant *petitio principii*—actually first assuming as a fact, the very point which is to be demonstrated. Let it be proved that, in the Anglo-Saxon times, there were no waters but "*shallow muddy rivers*" in these valleys, and I will then admit the force of the objection. Let it be proved, that Fleoꞇ denotes—not "an æstuary or arme of the sea,"—but "*a shallow muddy river;*"—that the brine, which five centuries afterwards supplied the salt-pans at Herringby and Fritton, was conveyed to them by "*shallow muddy rivers:*"—I will then allow, that the etymology, which I have suggested, affords no collateral evidence in support of my argument. But until the proofs which I have required can be adduced, I must still believe, that Herringby and Herringfleet were fishing establishments of the early Saxon colo-

in the Isle of Thanet; *Bemfleet*, in Essex, near the Isle of Canvey; *Fleet, Surfleet, Wainfleet*, and *Saltfleet*, on the coast of Lincolnshire;—all these places were evidently so denominated from the course of the tidal waters, on the banks of which they were fixed.

nists, and that they form a part of that connected chain of facts, which attests the former residence of the sea in these valleys, and illustrates the cause of its gradual retreat.

I have thus far restricted my observations to those points, on which Mr. Taylor has touched, within the range of historical evidence; there are some other comments of minor importance, which I have omitted to notice, thinking it unnecessary to show how far they are from having in the least degree invalidated my statements. I shall now proceed to the consideration of his remarks upon the earlier phænomena which this district presents. Mr. Taylor's object is to make it appear, that these "belong to an antediluvian formation; and *therefore* cannot be admitted as evidence of supposed changes or of events that have occurred subsequently to the deluge." Here again a favourite dogma of the modern geological school is advanced as certain and infallible. It is not necessary that I should stop now to explain the grounds of my dissent. In a future stage of this inquiry, I shall not shrink from avowing and defending my own opinions, or from pointing out the errors of those who, under the mistaken idea of serving the cause of religion, are so anxious to square the œconomy of nature with the early traditions of the East and the imperfect philosophy of an ignorant age. The beds of sand and gravel, which Mr. Taylor's orthodox creed calls *diluvium*, belong of course to an older formation than the alluvial deposits of the valleys. I wish to avoid all idle disputes about mere words; and therefore admit at once, that if the upper layers of marine shells on the sides of the hills can be proved either to have preceded, or to be part of the former, instead of being, as I regard them, the first and earliest portion of the latter, they would in that case afford no evidence of the height to which the waters of the ancient æstuary once flowed. But I deny that Mr. Taylor has adduced a single satisfactory proof in support of his position,—that these shells belong to the *crag stratum*, or that early formation which he calls antediluvian. According to his hypothesis, as explained by his Section, No. 3, a bed of crag, surmounted by *diluvium*, is uniformly spread over the chalk through the whole of this district; and the Valley of the Yare is an excavation produced by some powerful stream, which, by acting upon the upper beds after they were complete, intersected them in such a manner, as to display their series in the escarpment on either side. In opposition to this theory I have already shown,

First, that although the chalk rises on each side considerably above the surface of this valley; yet in no instance has it ever

ever exhibited a natural cliff, or precipitous bank, capped with sand and gravel.

Secondly, that beds of recent shells, similar to those which lie on the sides of the hills, are constantly discovered below the alluvial mud in the bottom of the valley; and

Thirdly, that these shells do not universally cover the chalk, and in fact, that no continuous layer of them exists out of the basin of this valley, to prove that they have any connection with the *Crag* of Harwich, Woodbridge, Lowestoff, and Cromer.

According to Mr. Taylor, "experienced well-sinkers do affirm, that on forming deep wells in various places round Norwich, at a distance from the river, they have occasionally encountered a stratum of shells overlying the chalk;" and he refers to *one* instance of this on Musholt heath. In consequence of this statement, I have again examined all the chalk-pits in this neighbourhood: and over an area of several miles in extent, where this universal sub-stratum has been laid open in a great variety of situations, I have not been able to meet with a single shell in the sand by which it is covered. I have investigated with particular care those at the foot of Mount Surrey and under St. Michael's hill, which are in the immediate vicinity of the well alluded to by Mr. Taylor, and could not perceive in them the least trace of what is called the *Crag stratum* or of any shelly deposit whatever.

It is not my wish to express the slightest doubt of any facts, which Mr. Taylor describes from personal observation; I only dispute his inference from them. There may be—I have indeed admitted that there are—in this district beds of fossils of marine origin, belonging to the superficial coat of sand and gravel; but, wherever they occur, they can only be regarded as local and casual vestiges of that ancient ocean, which once flowed over the highest ground in these parts; for I must insist, that the total absence of any such remains in all the chalk-pits, that are not within the circuit of the valley, proves most decidedly, that the supposed stratum is altogether an unestablished deduction from misconceived and unsound premises. Geologists have applied too hastily to the diversified and partial features of this upper formation, the principles by which they have traced the continuity of the lower rocks. The outcrop of solid masses at distant points may be sufficient to indicate the inclination of their beds and the direction in which they extend; and it is the habit of drawing these conclusions from such appearances, which has led to the prevalence of the false idea, that, because there are beds of shells in the cliff at Harwich,

Harwich, and others in that of Cromer, therefore the shells in the interior valleys of Norfolk, between those two positions, must necessarily belong to them, and demonstrate the existence of a connected layer, concealed by beds of sand and gravel, through the whole intermediate space. Mr. Taylor has overlooked the influence of this habit; he has made no allowance for the disposition which it produces to generalize inferences, and to measure the effects of the most opposite properties in nature, by one pre-conceived system and uniform scale; he has surrendered his judgement to authority—has endeavoured to reconcile with the promulgated opinions of others, all that has come under his own observation; and has thus permitted himself to appeal to a few fossils, taken from a single well, as "*absolute proof* of a continuous shelly bed," although the actual series of the strata, laid open to day-light for the space of several hundred yards within a short distance of that very spot, exhibit not the faintest trace of any "continuous bed" of the kind. The authority on which Mr. Taylor seems principally to rely, is that of Mr. W. Smith, who has stated, that "through Norfolk the Crag shells lie near to, or are in contact with, the top of the chalk." But whence are the facts taken, on which this statement rests?—From the vale of Aylsham and the Flegg hundreds, which are portions of the ancient æstuary itself. Proofs of the general diffusion of marine exuviæ through the whole extent of the valleys, multiply upon me daily; but from the higher grounds, whence alone the evidence of an universal and continuous *Crag stratum* could be drawn, nothing has yet been adduced, to support, in a satisfactory and decisive manner, the theory that has been advanced. On the contrary, wherever excavations have been made, that afford opportunities of exploring the nature of the masses of earth above the chalk, the non-existence of a connected shelly deposit has been actually and clearly ascertained. We have therefore no proof whatever, that the accumulations of shells within these valleys belong to the same æra and the same formation, as those which in some places are found so deeply buried beneath thick banks of sand and gravel. They may be—and many circumstances indicate that they certainly are—monuments of distinct natural operations, proceeding from separate causes, and occurring at various dates.

An attentive consideration of geological facts has led me to the conclusion, which, as I have since found, was also adopted by the unfortunate Lavoisier,—viz. that the series of stratified rocks and alluvial soils, which compose the surface of our globe, have been produced by alternate elevations and depressions

pressions of the sea. These changes I conceive to have been effected by successive transfers of the existing body of water from one hemisphere to the other; and the solution of the problem, which they exhibit, appears to be connected with some of the most interesting astronomical phænomena, displayed in the history of the heavens. I have been induced thus to anticipate the course of my inquiry, and to point out the nature of the general inferences which I shall hereafter draw from a long chain of evidence, in order to explain my view of the distinction between the *Crag stratum* and the shelly deposits of the Norfolk valleys. The chalk which constitutes so large and important a feature in the central basin of Europe, was formed in the bosom of a tranquil ocean, whose surface must have been more than 1000 feet above that of the sea, now existing in this quarter of the globe. Beneath the present level of those seas, we find the most decided traces of submerged forest-land, at points so distant from each other and over spaces so extensive, that the fact cannot possibly be accounted for by any subsidence or sinking of the ground on which these trees were produced. From the nature of their remains it is evident, that they can have flourished only on dry land; and as they grew in some of the lowest hollows of the chalk, it is equally clear, that the waters, in which that formation was consolidated, must have been withdrawn to such an extent as to leave uncovered considerable tracts, which at this time are constantly overflowed by the tides. During this retreat of the waters, their level appears to have been subject to repeated minor fluctuations, and the lower basins of the chalk were filled with beds of sand and clay, which in some districts exhibit the vestiges of fresh-water tribes, alternating with the marine exuviæ of an intermediate date. These beds seem scarcely to have extended into Norfolk, where the general range of the chalk was probably above their level; some portions of them may however rest in the deepest bottoms of the valleys, and on the declivities of the present coast; and it was perhaps on their surface that the forests arose and the animals lived, which a subsequent catastrophe overwhelmed*. At the period to which I am referring, there was no variety of climate over the whole face of our globe; the

* Mr. W. Smith, in his geological map of Norfolk, has indeed delineated Strumpshaw and Poringland hills, as detached outlying masses of the London blue clay; but I believe that Mr. Taylor will agree with me in regarding this as an error; for I am not aware that any organic remains have ever been found in these hills, to identify them with the formation to which Mr. Smith considers them to belong.

vegetable

vegetable and animal tribes, which are at present never found beyond the tropical regions, abounded then even in the highest northern latitudes, for their vestiges have been discovered there under such circumstances, as prove them to have been natives of the districts in which they perished. Their remains are either buried under, or associated with beds of sand and gravel, which rise to between 300 and 400 feet above the present level of the sea, and from which it is manifest, that the waters must have again invaded these tracts, rising above the height at which these memorials of their action still exist. By this elevation of the ocean, the forests which had grown up during its retreat, were largely overwhelmed,—many of the land animals, which in the interval had taken possession of the woods and pastures of the earth, were destroyed by the ascending flood,—and various deposits of marine bodies were mixed throughout the general mass of the detritus collected by the agitated waves. The earliest of these relics constitute what is commonly termed the *Crag stratum;* and in order "to favour a given theory," the name of *Diluvium* has been employed to designate the upper beds formed during the subsequent stages of this operation. From the nature and extent of the materials thus accumulated, it is clear that this last residence of the sea in these parts must have been of long duration; and it by no means follows, that the deposits of shells, which have been found intermingled with them, must necessarily be continuous and contemporary. The marked prevalence of particular genera at different points—their apparently gradual approximation towards a more perfect agreement with the testaceous tribes of the present day—their diversified characters and dissimilar positions,—all indicate, that these deposits must have been the unconnected results of those successive local changes which attended the access and departure of the waters.

Such is the course of events which I have traced in the geological records of this district. Mr. Taylor's theory and the doctrines of the school to which he belongs, in effect admit the outline of the series of operations here described; they allow that the ocean, in which the chalk was formed, had been withdrawn, and that this was followed by a rising and then by another falling of the waters; but instead of connecting the two last changes with the other natural revolutions manifested in the structure of our globe, they ascribe them to a transient, although universal inundation, effected by supernatural agency. It would be useless to seek explanations of physical difficulties from those who cut every knot by the aid of divine interposition.

position. But I am unable to perceive how they can reconcile with their views of the question, the positive fact, that considerable portions of what was dry land before the last irruption of the waters, are at this time actually covered by the sea. This is proved by the submarine forests, that have been found on every coast of our island. The lands on which they grew, must have been at that distant period far above the range of the waves; they must also have been submerged by that last catastrophe, which in the current phraseology of the day is called *diluvian.* It cannot then be denied, that before this event, there were sylvan tracts which are now buried beneath the floods; and if we compare those remains of them, which have been discovered on our own shores, with the corresponding traces that exist on the opposite coasts of the continent, it will be found by no means improbable, that they were originally connected, and that the whole bed of the intervening sea was at that time an extensive wooded valley.

From whatever cause then, this last great influx of waters over our quarter of the globe may have proceeded, it is admitted by all parties, that such an event did take place; neither can it be disputed, that in the course of their retreat to their present level, the last remaining portions of them, which left our district, must have flowed out through the valleys, which at this time open to the ocean. Consequently in the concluding stage of this revolution, there must have been a period, at which the retiring flood was confined within the limits of the valleys; or, in other words, when the valleys were arms of the sea or æstuaries. Here then is the point, at which the records of nature and the facts of history meet and coincide. Even the most conflicting theories acknowledge a certain series of changes, at the termination of which the eastern valleys of Norfolk must have been connected branches of an æstuary; it is also allowed on all sides, from the most positive and satisfactory evidence, that this was their actual condition within less than seven centuries. Are we then to believe, that these physical proofs and historical testimonies, while they agree in every essential particular of the point which they establish, still relate to two distinct facts and two separate periods? Is it not, on the contrary, more consistent with the harmony of nature, more agreeable to sound reason, more pertinent to an intelligible and unsophisticated philosophy, to refer them to one and the same state of things? Geological phænomena inform us, that at some uncertain date, an æstuary must have occupied these valleys; History announces, that at a definite æra such an æstuary did exist. By connecting

ing these circumstances, the only point which is left unsettled by the former, is decided by the latter, and the whole scheme becomes clear and uniform; but by persisting in keeping them separate, we are bewildered in a maze of inexplicable mysticism, and embarrassed amidst a chaos of imaginary strata and supposititious diluvian remains. So also by pursuing these principles, the subsequent conversion of this æstuary into a range of solid and productive land, exhibits the continued advance of the same operation; its progress displays the result of the same immutable laws; it marks another stage in the same consistent and uninterrupted process; it connects the changes of all past times with the recent creations of the present day.

It is then very evident that during this revolution, the retreating waters must for a season have been confined within the circuit of the valleys; and whatever deposits were formed at that time, cannot of course extend beyond the same space. Hence, if we find deposits that are actually restricted within those limits, the legitimate inference is, that they belong to that period and were collected under the circumstances just described. I have already shown the total want of the necessary proofs, to identify or connect the shelly beds of the Norfolk valleys with any others on higher grounds, or in positions that indicate an earlier origin; it now therefore only remains for me to make it appear manifest, that they do terminate on the sides of the hills which skirt the valleys. This point is in fact substantially admitted by Mr. Taylor; for he allows that "the chalk rises above the level of the highest crag deposits;" that these deposits "rarely rise to 80 feet above the present level of the sea, and *in general not more than half that elevation;*" and that after tracing them in the valley of the Yare, "*it is chiefly on descending again into the other valleys of this district, that fresh proofs present themselves.*" In considering these statements we must remember, that Mr. Taylor universally refers to the *Crag stratum* of other writers every layer of shells that comes under his own observation. If then we divest his accounts of the erroneous colouring given to them by this misconception, what terms can describe more explicitly the very facts to which I have appealed? They acknowledge that these beds of shells do not extend over the high grounds—that their general elevation is 40 feet above the level of the sea—and that they are confined to those valleys over which the ancient æstuary flowed. In more than one instance I have also ascertained the positive fact, that the upper stratum of these shells is nothing more than a narrow belt, superficially

superficially reposing on the slope of the hills. At page 14 of my Observations, I referred to one bed, a section of which, being laid open, showed "that it gradually shelved off to a point as it receded from the valley, and soon terminated." Since that time it has been completely cut away by the chalk-diggers, and not a vestige of it is now left. In another pit I noticed two layers; the lowest not much above the surface of the chalk, consisting of broken shells, mixed in an indurated mass of gravel and ferruginous sand; and the upper about ten feet higher, composed principally of the *Buccinum undatum** and *Turbo littoreus* loosely imbedded in a white quartzose sand. On my last visit to this spot I found the latter entirely exhausted for the space of about fifty yards, except a small portion not more than six inches deep and seven feet in length, which it was evident, that the next excavation would altogether remove. If to these decisive cases we add the striking fact, that no traces whatever of corresponding beds are discernible in any upland situations, where the strata have been actually explored, we have a right to infer from such circumstances, that these marine deposits are confined to the basins of the valleys†. It is far however from my object to contend, that they are all of the same age; I have on the contrary admitted, that the lowest are of a more ancient date, and appear to have some affinity to what is called the *Crag stratum;* it was from the upper and more recent beds that my conclusions were drawn. Not only is it evident that these are not buried beneath thick beds of sand and gravel; but we have the most decided proofs, that they actually repose on the surface of that mass which bears the name of *diluvium.* This I consider to be the essential characteristic of that subsequent adventitious formation, which has been with propriety termed *alluvial;* and every circumstance connected with these beds tends to corroborate this account of their origin. The uncompact and loose arrangement of their materials distinguishes them at once from the Crag, the structure of which, according to Mr. Taylor's description, is in some places so solid and rock-like, as to admit of its being hollowed into artificial caves and

* The *Murex striatus* of Mr. Taylor.

† The "*diluvial agency*" by which Mr. Taylor imagines them to have been displaced from those situations, where, according to his theory, they ought to be found, is a most convenient assistant, whenever a stubborn unaccommodating difficulty is to be swept away, or a reason assigned for the absence of material evidence. I should be almost disposed to regret having rejected its services, did I not remember, that they would be of no avail, since *that, which may be made to account for every thing, proves nothing.*

grottos,

grottos, and even used for building. So also on the other hand, the exuviæ which they contain bear so perfect a resemblance to the littoral shells which now abound in the neighbouring ocean, that they may be regarded as necessary links in the series of marine spoils—connecting those which attest the last previous operations of the sea with those which are daily left on its present shores by the ebbing tide.

In the list which I gave of these shells, I did not pretend to enter into minute scientific details; I only enumerated those, which are sufficiently abundant to render probable the actual abode of the living animals, where their remains are still to be seen. Single specimens of rare or extinct tribes occupy the most conspicuous and important places in the cabinets of the curious and the transactions of learned societies; but they are of no account in determining the character and antiquity of any formation, in which they may have been accidentally associated with multitudes of the commonest and most recent species. The microscopic eye of science often dwells so intently upon trifles, that it magnifies them into undue consequence; and their proportions thus unnaturally enlarged, engross the whole field of view, excluding all the ordinary facts and constantly recurring phænomena, from which alone just and comprehensive ideas of physical truths can be derived. Hence it is that a few extraneous substances, dislodged from an earlier stratum, and fortuitously washed down to the side of the valley of the Yare, are triumphantly appealed to in support of a theory, which is not in accordance with all the prevailing and most decidedly marked features of the district. The tooth of a Mastodon has been discovered at Whitlingham! Prodigious! This wonderful grinder therefore is an "absolute proof" that all the myriads upon myriads of *Buccina, Tellinæ, Cardia, Mytili, Turbines littorei, &c.* whose exuviæ line the basin of this valley, were coeval with the Mastodon, the Palæotherium, and all the lost races of quadrupeds, which perished in the last great convulsion experienced by the earth. It is far from my wish to treat with levity or ridicule the industrious researches of scientific men; but who can always suppress the rising smile at their complacent self-delusions, or never demonstrate, but with composed gravity, the *haud sequitur* of their inconclusive reasonings upon petty and misplaced facts? Nor would I be thought to under-rate the importance, either of our national, or of our provincial museums:—to those who use them intelligently and carefully, they are funds of valuable information; but I cannot too earnestly point out the necessity for great prudence in selecting, and still greater caution

caution in drawing conclusions from the specimens exhibited in them. None are more easily imposed upon than the virtuoso;—none look at Nature with a more contracted ken than the mere student of curiosities. Let us for instance suppose, that Mr. Taylor had presented to one of these repositories of wonders a nicely fractured stone, neatly labelled "*Granite, from the Black Meg Rock, near Beeston, on the coast of Norfolk.*" Some traveller in search of the scientific, espies this amongst the other treasures of the geognostically stored glass-case, and enters a minute description of it in his note-book. At the next conversazione he details to his listening coterie the newly discovered fact, that mountains of granite exist in Norfolk, and silences every sceptical inquiry, by appealing to this publicly displayed portion of Black Meg as an "absolute proof" of his assertion. I should wish to be informed how such an inference would be more rash, more unfounded, more preposterous, than the assumption, that the recent shells of the Norwich Valley must belong to the *Crag stratum*, because "the British Museum contains the tooth of a Mastodon," which was found there, and because a few other "travelled fragments" of a more ancient bed may perchance be dispersed amongst them. In the one instance Mr. Taylor immediately perceives that all the boulders and pebbles of primitive rocks, which lie on the shores of Norfolk, were brought from a distance by powerful currents:—let him view the interior of our valleys with the same good sense, unswayed by authority and unwarped by prejudice; he will then admit, that no sound notions with respect to their general character can be obtained from those stray scraps of earlier remains—those rare morsels of antiquity—to which, as far as regards this question, a too ardent enthusiasm has given an imaginary and deceptive weight.

I am not so vain as to expect that I can shake Mr. Taylor's faith, or that of any advocates of the Diluvian theory. I know how tenacious our nature is of the creed which has once been embraced, and how difficult it is to erase from the mind impressions once received. But I have so far confidence in the truth of my opinions, as to believe that they will in time make their way, where they have not to contend with the inveterate influence of a pre-adopted system; and it is in this hope, that I have ventured upon the present reply to my opponents. Those alone can enter upon this inquiry with an unbiassed judgement, who are yet free to decide upon the general questions with which it is connected; and to such I feel assured that I shall not address myself in vain. Let them first consider well the arguments employed to prove the "*antediluvian*"

vian" origin of the marine deposits in the Norfolk valleys. I will not ask whether these amount to a positive and irrefutable demonstration of the fact—it would be too much to require,—but do they afford any admissible even presumptive evidence in support of the peremptory conclusion that has been drawn? They have utterly failed in their attempts to establish a continuity of position and identity of character between these beds and any antecedent formation; and unless these two points can be clearly substantiated, all the rest is mere fable and conjecture.

On the other hand, let them observe the connection and harmony of the following train of inferences, deduced from the general phænomena of this district.

1. The masses of sand and gravel, here universally covering the chalk, denote by their elevation that the waters which formed them were raised between 300 and 400 feet above the range which the ocean now takes.

2. In the last stage of the retreat of these waters to their present level, they must have been drawn off through the valleys which open to the sea; and consequently there must have been a period, during which their streams entirely filled, and were confined to, these channels.

3. These valleys contain extensive beds of marine shells, which are not only spread over the bottom of them, but also at various points rise on each side to the height of about forty feet above the intervening alluvial formation. They terminate on the edge of the valleys, the uppermost of them resting on the surface of the sand, with a thin coat of vegetable earth above them, and consisting almost exclusively of the recent species which now inhabit the shores of the German Ocean.

4. At the earliest dawn of history upon this region, its valleys formed a connected æstuary; we have positive evidence, that at the close of the eleventh century the salt tides still covered a considerable portion of them, and from that time they have been gradually converted into pasture land.

In this series, the place of the marine remains (at least of the highest of them) is so distinctly pointed out and clearly determined, that it seems almost impossible to entertain a doubt upon the subject. They must have been deposited after the banks of gravel and sand were completed; for they rest upon them.

They must have been formed at that period, when the waters occupied only the troughs of the valleys; for they are confined within the same limits.

They must have been left by the retreat of those waters to their present level; for they are the latest relics of the sea: and

They

They prove the immediate connection between their native floods and the German Ocean, by their own close resemblance to the shelly tribes that now frequent the shores of the latter.

The successive stages of this revolution are here brought down by the records of nature to the period, from which historical documents attest its further progress. The change which is so incontrovertibly established by the latter, is at this point combined with, and, if I may so express myself, dovetailed into the operations displayed by the former ; there is no break in the train of events—no suspension of the laws which regulate the material universe—no interruption of the eunomy of creation ; but every link in the chain of evidence is so well adapted and so firmly riveted, that if Mr. Taylor could even succeed in identifying these remains with those of the Crag pits of Suffolk and the Cliffs of Cromer, he would demonstrate, not the fallacy of my conclusions with respect to the eastern valleys of Norfolk, but his own error in assigning to that stratum so high and obscure an antiquity. The littoral character of these shells, the drifted substances intermingled with them, and the arrangement of the whole in parallel lines on both sides of what is admitted to have been once an æstuary,—these circumstances all support my conclusion, that there formerly existed at this elevation a beach or strand, and consequently, that the sea was there stationary for a considerable space of time during its retreat. This opinion is powerfully confirmed by numerous coincident traces of the same fact, not only in various situations on the shores of our own island, but also in distant countries. I purposely abstain from pointing them out now, as I am engaged in preparing for the press a detailed account of them, with geological and historical observations on the change of level that has taken place in different seas. When the facts which I have collected in reference to this question are brought properly to bear upon it,—when it is seen that similar deposits of recent shells have been found in the beds and on the sides of other valleys, where no *Crag stratum* has ever been thought to exist, and even on the face of the oldest primitive rocks,—it may perhaps excite some surprise that so manifest and undeniable a truth should ever have been doubted. The stigma of heresy, which now attaches to my opinions, will, as I confidently anticipate, be then successfully wiped off; and it will be found that I have not been amusing the public with a fanciful and untenable proposition, but that I have been laying the corner-stone of a solid basis for future and more extended inquiry.

XLVI. *De-*

XLVI. *Description of two remarkable Ores of Copper from Cornwall.* *By* William Phillips, *F.L.S. F.G.S. &c.** *With an Analysis of the same, by* M. Faraday, *F.R.S. &c.*

A MASS consisting of a few hundred pounds weight of a very singular kind of copper ore, was lately found in a vein in Condurrow mine, which is situated in granite, and about half a mile south of the old and celebrated copper mine called Dolcoath, which is near to Camborne in the county of Cornwall.

In the aspect of the mass in question there is nothing indicating its being constituted for the most part of copper, for it bears no analogy in appearance to any of the known ores of that substance; its great weight, however, induced the trial, and some parts of it were found to contain $64\frac{1}{2}$ per cent of that metal, others yielded less copper, and were less heavy; some, having the same aspect, were quite light; the specific gravity of the portion analysed, as hereafter detailed, was 5·2045, as taken by my friend S. L. Kent, F.G.S.: the greater part of the mass was broken up and mingled with other ores of copper.

The general colour is brownish-black, sometimes presenting however a tinge of blue. A specimen in my possession, which was broken from the mass immediately on its discovery, presents a flat conchoidal fracture, and a highly polished black surface; but, having parted with much of the water it contained when first broken in the vein, owing to exposure, it is now cracked in various directions, as are all the specimens I have seen, and readily divides into irregular portions very much resembling those of starch: these portions are covered by a blackish-brown powder which soils the fingers; on its removal, the surface of the portion sometimes has a slightly bronzed appearance. It is hard, but not sufficiently so to scratch glass; is brittle; yields to the knife, which leaves a polished metallic-looking surface, nearly of a lead gray colour. When powdered it is soot-black. A fragment placed on a red hot coal when in the fire, shortly afforded a copious white vapour, leaving on the coal a metallic substance in a semi-fluid state, of a yellowish colour.

The mass of copper ore was found 65 fathoms under the surface of Condurrow mine, alone in the vein, or rather, unmixed with other metallic ores: beside it lay a mass of native copper weighing about 150 pounds; and about half a ton more

* Communicated by the Author.

was

was found in its immediate neighbourhood, and much yellow coppre ore at a distance from it on the east in the vein; about 8 fathoms above it was found a small quantity of gray copper ore, and 3 to 5 fathoms below it, the oxide of tin occurred in considerable quantity; but no other mass in any degree resembling this was found in any other part of the vein. It should be stated that the native copper lying beside it in the vein, was highly crystalline, and was coated by the substance under consideration.

To Francis Daniell, Esq. of Camborne I am indebted for the information, relative to the circumstances under which this singular ore of copper was found; and to Robert Bennett, Esq. of the same place, for some specimens of it, and also of a very remarkable specimen of a metallic mineral found nearly in contact with it. The greater part of this specimen, which is about the size of a walnut, consists of a tin white metallic substance, which is hard, but yields to the knife, and is extensible under the hammer. It is coated by the black ore above described, and is accompanied by native copper, which is even in some places intermingled with it, as is discoverable by the assistance of a glass, on viewing a surface produced by the knife: by exposure this surface acquires a tinge of yellow. This substance greatly resembles that which is left on the coal, as already detailed, after driving off a white vapour by heat, from a fragment of the black ore; and the experiments of my friend M. Faraday, Esq. detailed below by his permission, and which he kindly undertook at my request, render it probable that each is *arseniuret of copper*. The black ore contains occasional specks of a yellowish metallic-looking substance, which also I believe to be the same substance. The black mineral I propose to distinguish by the name of *Condurrite*, as having been observed only in Condurrow mine. It is doubtless a mere mechanical deposit, arising perhaps from the natural decomposition of other ores which abounded in copper and arsenic.

Examination of the Condurrite, *by M. Faraday, Esq.*

Royal Institution, July 18th, 1827.

When heated in a close tube, water first rises from it, then arsenious acid, which condenses in a pure and crystalline form; and a metallic mass remains, having nearly the colour and lustre of copper, but containing, besides copper, a little arsenic in the metallic state, a little sulphur, and a trace of iron. Very feeble indications of the presence of a little animal or vegetable matter are observable on the first impression of heat. If this substance

substance be heated in the open air on platina foil, nearly the same effects and appearances are produced, but the residue is black from the superficial oxidation of the copper. If the substance be heated upon carbonaceous matter, or by the reducing part of a blowpipe flame, then a reduction of part of the arsenious acid takes place, and a metallic residuum containing more arsenic than in the former case is produced, and which is consequently more fusible.

This substance dissolves entirely in nitric acid, the portions of metal, &c., which are not naturally in the state of oxide, being oxidized at the expense of the acid. When pulverized and acted upon by cold muriatic acid, arsenite and muriate of copper are found in solution, apparently without the evolution of hydrogen, and metallic arsenic remains undissolved.

I have little doubt that the substance is a mechanical deposit, and I find it to vary somewhat in composition. 34·5 grains being heated carefully in a tube, the loss of water and of arsenious acid was ascertained, and then the metallic residuum, weighing 22·45 grains, was dissolved in nitromuriatic acid; the sulphuric acid formed was separated by muriate of baryta, and the sulphur in it was ascertained; the excess of baryta was then removed, and afterwards the oxide of copper thrown down by caustic potash, and ultimately the arsenic acid formed thrown down by nitrate of lead. The proportions were as beneath:

		34·5 parts consist of	100 parts consist of
	Water	3·1	8·987
	Arsenious acid	8·95	25·944
Alloy	Copper	20·87	60·498
	Sulphur	1·057	3·064
	Arsenic	0·52	1·507
	Trace of iron.		
		34·497	100·000

The manner in which these substances are arranged in the mineral is uncertain, and may be put several ways. It is probably a mixture of metallic arsenic, arsenite of copper, oxide of copper, a little copper pyrites; one or more of these substances being in combination with water.

Examination of the Arseniuret of Copper, *by M. Faraday, Esq.*

Royal Institution, Sept. 10th, 1827.

The specimen you sent me is intermingled with portions of a substance like that before analysed: it also contains portions of copper nearly pure. Some of the purest and most uniform parts which I could select, when heated in a small

green

green glass tube, still gave out arsenious acid, which sublimed and crystallized as in the former case; the quantity was however small, and the rest of the mineral fuzed at a red heat into a substance which, when cold, was brittle, gray, and by examination proved to consist of copper and arsenic, in combination with a small quantity of sulphur, and a trace of iron. I have no doubt that the gray metallic hard substance is an arseniuret of copper, but the difficulty of separating it perfectly from the accompanying bodies will interfere with an accurate determination of its composition.

XLVII. *Remarks on* Col. Miller's *Plan for Mooring Ships in Roadsteads. By* Mr. J. P. De la Fons.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

BEING the patentee of a most material improvement in moorings, which invention is also applicable to other valuable purposes, I embrace as early an opportunity as occurs to me, for offering a few remarks upon a communication on the subject of moorings, which appeared in your Journal for August last.

That a mooring upon principles of perfect security is much wanted for the use of the merchant-service, no one acquainted with the subject can dispute. Whoever has witnessed the horrors of a shipwreck, which in the majority of cases originates in a defective anchorage must regret that nothing has yet been successfully attempted for the purpose of averting so frightful a calamity. In the hopes of attaining so desirable an object, various methods have been suggested, similar in principle to the one in question; some of which have been tried, but, as might be expected, they have failed at the very time when security was of the utmost importance. Nor can it be wondered at, when we consider that in lieu of the anchor, which, although it takes hold of the ground, is so liable to fail, a mass of iron depending upon its weight alone has been substituted, and with as probable a chance of success as if the aëronaut were to dispense with his grapples, and in the expectation of arresting its progress, attach his balloon to a weight that was inadequate to the resistance of so large a body driving before the wind.

Your correspondent's plan so far differs, that he proposes in some cases to fasten it, with pegs driven around it, into the ground by means of the diving-bell, which (the bell not being large enough to contain powerful machinery) I conclude he purposes doing by manual labour: but I apprehend any attempt

tempt at securing it by such means must prove abortive, as nothing of sufficient strength could be driven with so limited a space to work in.

Mooring-blocks are likewise objectionable in shallows, as where a vessel draws much water she is liable to run foul of them, and thereby sustain considerable damage,—an evil that is avoided by making mooring-anchors with only one fluke.

The idea that the buoy would from its tendency upwards act as a spring upon the cable is incorrect, as the weight of the chain would reduce it to an equilibrium, unless it were made of an enormous size, so large as to endanger small craft that might come in contact with it.

There is an oversight in the method proposed for mooring the vessels, which, if secured to it in the way described (viz. by a strong hoop round the centre of the buoy), a very trifling strain, by elongating the hoop, would crush it nearly flat; but this error might easily be obviated by passing a bar through the buoy with the ring at the top for making fast to.

Having shown in the preceding observations that no anchorage can be relied on unless it take firm hold in the ground, it will be evident, and it has been admitted by every nautical man who has inspected it, that the safety mooring is constructed upon principles of perfect security, and that it could only fail by the breaking of the chain. The security of the mooring is effected by means of a pile strongly fastened to the mooring-chain, which pile with the chain attached to be driven perpendicularly into the bed of the waters, at any station where it may be required. The piles should be about five or six diameters in length, and may be driven to the depth of several feet into the earth, according as the nature of the ground may require: this can be done at any anchorable depth, and without the aid of a diving-bell, by means of a newly-invented apparatus, which cannot be described without the assistance of drawings. The pile should be of a porous kind of wood, as when swoln by the absorption of moisture it would require immense perpendicular action to raise it,—a description of force to which it never would be subjected;—and being driven *flush* with the bottom, its stability could not possibly be affected by a vessel riding as she usually does at an angle of about thirty degrees.

Every practical man that has been consulted upon the subject, agrees, that if these permanent moorings were laid down in the roads, numbers of lives, as well as property to an immense amount, would be annually saved; as when a large ship drives, not only is she in danger, but as she bears down upon the others, they also to avoid the impending danger are compelled

pelled to slip their cables, until in like manner the evil has increased to an alarming extent,—a calamity that might henceforth be avoided, at an expense scarcely *equal to the value* of *one richly freighted* vessel. How many valuable India-men have been lost in sight of home, after escaping the perils of a long voyage, merely for the want of a security that can be afforded them at a comparatively trifling expense!

Yours, &c.

George-street, Hanover-square. J. P. De la Fons.

XLVIII. *On the Adhesion of Screws. By* B. Bevan, *Esq., Civil Engineer.**

NEXT to that of nails, there are few things in more general use amongst artificers than *screws.* I do not recollect seeing in any publication the results of any experiments on the *force* necessary to draw screws of iron, commonly called wood screws, out of given depths of wood. Having heretofore published the force necessary to extract nails driven into wood, the following results of my experiments on the force required to extract screws, made some time ago, may be of some interest.

The screws I used were about two inches in length, $\frac{22}{100}$ diameter at the exterior of the threads, $\frac{15}{100}$ diameter at the bottom, the depth of the worm or thread being $\frac{35}{1000}$, and the number of threads in one inch $=12$. They were passed *through* pieces of wood, exactly half an inch in thickness, and drawn out by the weights specified in the following table:

Dry beach	460 lbs.
Ditto ditto	790
Dry sound ash	790
Dry oak	760
Dry mahogany	770
Dry elm	655
Dry sycamore	830

The weights were supported about two minutes before the screws were extracted.

I have also found the force required to draw similar screws out of deal and the softer wood about half the above.

From which we may infer, as a rule to estimate the *full* force of adhesion, in hard wood...$200{,}000\ d\,\delta\,t = f$,

and in soft wood...$100{,}000\ d\,\delta\,t = f$,

d being the diameter of the screw; δ the depth of the worm

* Communicated by the Author.

or thread; and t the thickness of the wood into which it is forced,—all in inches; f being the force in pounds to extract the same.

We may from the above experiments observe the approximation to perfection in the art of screw-making; for had the screw been greater in diameter, there would have been a waste of material; or had it been less, it would have been not sufficiently strong, which may be proved as follows:

The cohesion of wrought iron has been found from a number of experiments to be about 43,000 pounds per cylindrical inch; and as the smallest diameter of screw used in my experiments was ·15, it would have been torn asunder by a force of about 968 pounds; or if the hard wood had been 5-8ths of an inch thick, into which it had been screwed, the screw would have been broken, instead of forcing its passage out of the wood.

B. Bevan.

XLIX. "Dr. Price *and his Followers*." (See Phil. Trans. for 1826. Part iii. p. 297.)

I KNOW not, neither am I anxious to learn, for whom this civil appellation is intended. If it merely refers to those theorems of Dr. Price which have been the subject of Dr. Young's animadversions in his late communication to the Royal Society, I should think it impossible that any person acquainted with the subject, would have the least difficulty in determining which of the two Doctors he should prefer to follow on this occasion.

In the 66th volume of the Philosophical Transactions, Dr. Price gave sundry theorems for determining the values of annuities when the payments are made at shorter intervals than one year, and for that purpose proceeded on the same principles in investigating the values of the different payments with those universally adopted in regard to the values of such payments when they are made annually; for if 1*l.* increased by its interest for a year, or $\overline{1+r}$, be the amount of 1*l.* in a year, 1*l.* increased by its interest for a shorter term will be its amount in that term. Supposing therefore such term to be $\underline{1/a\text{th}}$ part of a year, $\frac{r}{a}$ will be the interest, and consequently $1+\frac{r}{a}$ will be the amount. The converse therefore of these expressions or $\frac{1}{1+r}$ and $\frac{1}{1+\frac{r}{a}}$ will be the present value of 1*l.* to be received at the end of a year, or at the end of $\frac{1}{a}$th part of a year.

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—The series $\frac{1}{1+r} + \frac{1}{\overline{1+r}|^2} + \frac{1}{\overline{1+r}|^3} \ldots\ldots \frac{1}{\overline{1+r}|^n}$ is known to express the value of an annuity of 1*l.* for n years $= \frac{1}{r} - \frac{1}{r \cdot \overline{1+r}|^n}$. For the same reason, if $\frac{1}{a}$th part of 1*l.* be paid a times in each year, the series $\frac{\frac{1}{a}}{1+\frac{r}{a}} + \frac{\frac{1}{a}}{\overline{1+\frac{r}{a}}|^2} + \frac{\frac{1}{a}}{\overline{1+\frac{r}{a}}|^3} \ldots$

$\ldots + \frac{\frac{1}{a}}{\overline{1+\frac{r}{a}}|^{na}} = \frac{1}{r} - \frac{1}{r \cdot \overline{1+\frac{r}{a}}|^{na}}$ will express the value of 1*l.* per annum payable every $\frac{1}{a}$th part of a year for n years.

In the Preface to Taylor's Logarithms, Dr. Maskelyne assumes r to be the interest of 1*l.* for one *time*, and supposes the payment of the annuities to be made so many *times.* This is in fact the same as Dr. Price, and I believe every other person since his time have done, who have had a due knowledge of the subject. It necessarily follows that temporary annuities payable at shorter intervals than a year must be more valuable than annuities payable yearly; and in consequence, Dr. Price states the value of an annuity of 1*l.* payable half-yearly for five years at 4 per cent, to be 4·4913, and its value payable yearly to be 4·4518; or in other words, the two half-yearly fractions in any year being greater than the single fraction in the corresponding year, that is $\frac{\frac{1}{2}}{1{\cdot}02} + \frac{\frac{1}{2}}{\overline{1{\cdot}02}|^2}$ being greater than $\frac{1}{1{\cdot}04} \ldots \frac{\frac{1}{2}}{\overline{1{\cdot}02}|^3} + \frac{\frac{1}{2}}{\overline{1{\cdot}02}|^4}$ than $\frac{1}{\overline{1{\cdot}04}|^2}$ &c. it follows that the sum of the former must be greater than the sum of the latter. But instead of comparing the *sum* of the two half-yearly terms with the corresponding yearly term, Dr. Young compares the second of each half-yearly term with *half* the corresponding yearly term, or $\frac{\frac{1}{2}}{\overline{1{\cdot}02}|^2}$ with $\frac{\frac{1}{2}}{1{\cdot}04} \ldots \frac{\frac{1}{2}}{\overline{1{\cdot}02}|^4}$ with $\frac{\frac{1}{2}}{\overline{1{\cdot}04}|^2}$ &c., and by this means finds the discount taken half-yearly to be *greater* than the discount taken yearly; which if true, would make the value of an annuity payable half-yearly to be *less* than its value payable yearly, which is self-evidently wrong*.—Dr. Price is said to have fallen into error by " adopting the legal restraints

* It may be easily proved that the value of the *second* half-yearly payment in each year is always less than the value of half the payment at the end of that year; and on the contrary that the value of the *first* half-yearly payment

restraints on usury as an essential step in his calculations." Where does this appear? or what connection have those restraints with any calculations of the kind? I am certain that no such idea ever entered into the mind of Dr. Price, or of any person acquainted either with the doctrine of annuities or with the laws of this country. In order, however, to remove all difficulties and correct all errors on the subject, we are favoured with a much simpler process. "An annuity of 1*l.* payable half-yearly (is said) to be equal to two annuities of $\frac{1}{2}$—the one beginning at the end of the year—the other anticipated by half a year; and the value of this portion is greater than the other by half of the payment, that is by $\frac{1}{4}$," if the annuity is payable quarterly, the excess will be $\frac{1}{4}$ the payment, or $\frac{1}{8}$; or in other words, the excess in the first case will be $\frac{1}{4}$ year's purchase of the annuity; and in the second case it will be $\frac{1}{8}$ year's purchase; nor does it appear to make any difference whether the annuity be for life or a term, or whether the term be 5 or 50 years—or whether the rate of interest be 4 or 40 per cent, so that in *all cases* an annuity of 100*l.* worth 1000*l.* payable yearly, will be worth 1025*l.* if paid half-yearly, and 1012*l.* 10*s.* if paid quarterly. This is really setting all the rules of arithmetic at defiance, and requires no answer. By this indiscriminate mode of making the same additions to all annuities, it is admitted that the calculations in some instances become in appearance a *little paradoxical;* for the value of a reversionary annuity during one life after another, by making the same addition to the single as to the joint lives, becomes the same whether the payments are made yearly, half-yearly, or at shorter intervals: but it is well known that the additions to single and joint lives are different, and therefore that the values of all annuities in which single and joint lives are concerned must vary as the payments are more or less frequent.

But I shall proceed no further with this subject, my motive in entering upon it having been to prove the truth of Dr. Price's solutions, rather than to expose the errors of those who do not understand them.

The public have lately been overwhelmed with tables of the decrements of human life, formed either by amalgamating all

payment is always greater: and it may be further proved, that the difference between the values in the former case is always less than it is in the latter. In other words, that in any given year (n) the excess of $\frac{\frac{1}{2}}{\overline{1+r}|^{n}}$ above $\frac{\frac{1}{2}}{\overline{1+\frac{r}{2}}|^{2n}}$ is less than the excess of $\frac{\frac{1}{2}}{\overline{1+\frac{r}{2}}|^{2n-1}}$ above $\frac{\frac{1}{2}}{\overline{1+r}|^{n}}$.

the

the old tables into one heterogeneous mass, and thus giving the true probabilities of life in no place whatever, or by interpolating some of the decrements in one table into those of another; for which purpose a vast variety has been given of complicated and useless formulæ. But little or no advance has been made in determining more correctly the probabilities and duration of human life. The tables published in the Report of the Committee of the House of Commons, are in general so incorrect, and some of them are even so absurd, as to be unfit for use; and serve only to encourage the popular delusion of the improved healthiness and greater longevity of the people of this kingdom.

F. R. S.

L. *On the Natural Embankments formed against the German Ocean, on the Norfolk and Suffolk Coast, and the Silting up of some of its Æstuaries. By* R. C. Taylor, *Esq. F.G.S.*

[With an Engraving.]

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE discussion between Mr. Robberds and myself respecting the natural embankment across the ancient æstuary of the Yare*, leads to a more general consideration of the process by which that and other similar barriers on this coast have been effected. The mode I suggested in a former communication has been thought defective †, on account of the difficulty, first, in pointing out a competent agent to rear the fabric, and next, of obtaining an adequate supply of materials. From the argument of the utter impossibility of the sea to throw up a permanent barrier against its own course, I must dissent, as the instances are so numerous to the contrary, that, fortunately for our island, the difficulty is chiefly to select cases in the affirmative. I presume the word "permanent" to be here applied in its limited sense: since we have seen, that the attacks of currents, tides, and impetuous waves, when aided by the winds, and uniting their tremendous forces in one direction, not even the solid mountain can permanently withstand.

Such is the tendency of the sea to throw up sand upon that part of the coast we have been describing, that during the last 480 years, as I have shown elsewhere, it has cost about a million and half of money, not merely to keep open a passage for shipping, but to preserve a sufficient channel for the discharge of the united waters of the rivers. It is obviously the opinion of

* See Phil. Mag. and Annals, N. S. vol. i. p. 351. † Ibid. vol. ii. p. 199.

of every engineer who has reported on this important subject, that nothing but a continuation of the same unremitting vigilance heretofore exercised, will prevent the closing of that opening for all useful purposes. That this evil is not hypothetical is certain; because it has occurred no less than eight times in the period above stated. A constant effort is going on, on the part of Nature, to stretch the barrier, she has previously constructed, entirely across the æstuary, so as either completely to shut out the low grounds from the sea, and to unite, by one continuous sand-bank, Caister with Gorleston, or to divert the current of the river along the base of the Gorleston cliffs. It is counteracted alone by artificial means; by the application of the back waters, and the employment of a forest of timber. An inspection of Plate II. fig. 1. will render further details of this locality inexpedient.

When referring to the agency of vegetation, in contributing to raise those natural mounds around our shores, it was not intended to convey the impression that such elevations were heightened by the accumulation of solid vegetable matter. By the agency employed in these cases, it is scarcely necessary to observe, is meant, that singular disposition of certain plants, the *Arundo arenaria* or Marram* in particular, to collect around them, to hold in a net-work composed of their fibres, stems and branches, the loose and shifting sands;—to bind in one comparatively compact mass, a substance which is apparently little adapted to the important office it is designed to fulfill.

The œconomy of Nature, in forming these sand-ridges, is by no means an uninteresting object of contemplation.

Perhaps by chance a small portion of the *Arundo* fixes itself upon a shingle bed, with scarcely sand sufficient to cover the first root. Rapidly it sends around its creeping stolones, and these serve to arrest some portion of the sands that are constantly moved by the winds. Occasionally the quantity is sufficient, for a time, to overwhelm the young plant: soon it rises with increased vigour to the surface; now appearing not as one, but as many plants. Again the sand accumulates: layer after layer succeeds; the *Arundo* spreads its shoots still further and higher, and always sends forth its creepers in search of the newly collected sand. By its agency a small hillock is formed, which in time, and by an extension of the same vegetative and accumulative process, becomes a ridge many feet in elevation; or a sand cliff on which the most boisterous wave is rarely capable of encroaching. I had an oppor-

* From the Gaelic *Muran*, the Sea Reed, or perhaps from the Dutch *Marren* 'to bind'?

opportunity of observing an interesting instance of the early part of this process, on the coast of Hampshire, within the last month. Upon a small, newly-formed hillock of sand, in a moist situation, a single plant of the *Arundo* genus had established itself. It had struck out lateral shoots so vigorously, as to induce me to take more accurate note. On measuring one of its stolones I found it to be twelve yards long; and several others, radiating from the original stem, were not less than ten yards each. When thus arranged, their extremities formed a circle, whose diameter was upwards of twenty yards. The joints of the shoots were from six to nine inches asunder. From each or most of these a root was directed downwards into the sand, and one or two young shoots upwards. Calculating them by the number of stolones, it appears that this plant, during a single year, had multiplied itself five-hundred-fold, or had produced what was equivalent, the rudiments of so many distinct plants, independent of the further power of production by seed.

I proceed to notice two or three other principal points along the eastern coast, where alluvial deposits have been made under circumstances very similar to those which produced the first we have described at Yarmouth.

At Lowestoft Ness, about seven miles to the southward, the sea has erected a complete series of natural embankments against itself, affording a fair illustration both of the formation of low tracts of land, and of the barriers by which they are defended, (fig. 2). The present extent of land thrown up by the sea, chiefly above its own level, and, excepting in a small portion, out of reach of its highest tides, is nearly three miles long, projecting from the base of the original cliff, to the distance of 660 yards, at the point of the Ness. This encroachment has been effected at distinct and distant intervals: its form is influenced by the direction of the currents in the channel or roadstead, and the position of the adjacent shoals; and the lines of growth are indicated by a series of concentric ridges or embankments, inclosing certain areas. Several of these ridges have been formed within the observation of persons now living, the process being precisely similar to that by which I attempted to account for the great bank across the æstuary of the Yare. 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 upon its summit. Sand is blown from the beach and fills the interstices. The *Arundo* and other marine 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 these external and incomplete mounds, but it is singular to observe how soon the breach is repaired. After a while, the areas inclosed by these concentric embankments become pasturage. The marine plants are succeeded by a better species of herbage, the quality of which improves in each successive area, as step by step we approach from the latest to the earliest formed, at the base of the ancient cliff. Become sufficiently firm to support buildings, they are now occupied by numerous dwellings and offices, where it is recollected the sea formerly flowed; the change being effected not through the depression of its level, but from the obvious operations I have traced.

About seven miles further south, another Ness is formed, at Covehithe Point, and is about half the extent of the last described, (fig. 3). Here are five principal concentric ridges, four of which are now occupied by the *Arundo arenaria.* This Ness has also materially increased in our own times, and presents the same characters as the last. It is stretched across an ancient inlet of the sea, resembling Lake Lothing, or the Yarmouth Æstuary, on a reduced scale, and forms a complete barrier to the entrance of the sea into a low valley which is partly occupied by a small fresh-water lake.

The drawings which illustrate this and Lowestoft Ness, are copied from my surveys of this coast in 1825-1826.

The other sketch (fig. 4.) contains a remarkable illustration of the power of the ocean to form alluvial barriers, and to block up the outlet of a considerable stream. It would appear on an examination of the site, as well as from a consideration of the map of the district, that at some remote period the river Alde entered the sea at Aldborough: that a repetition of the process which diverted the mouth of the Yare four miles to the south, effectually barred the ancient outlet of the Alde, and transferred it to a point no less than ten miles to the south-west; where, being reinforced by the waters of a second river, it enters the sea, in a line nearly parallel with the coast. The external ridge of sand forming Orford Ness, on which the lighthouses are erected, has a strong resemblance to those we have before described, and stretches round so as to form a complete bulwark to the low lands between Aldborough and Hollesley.

It is to be observed that the progress of the sand in covering the newly-formed ridges, and also of the plants which follow that operation, particularly in the three first instances, is from the

the north, towards the south. At Lowestoft, the sand hillocks gradually decline from the Corton extremity towards the Ness, from nearly twenty feet in height at first, to a few inches over the shingle. The same at Yarmouth, where the high sand-hills decrease from Caister southward, and cover (as the engraving exhibits) scarcely one half the entire bank. With regard, therefore, to the origin of one large portion of the blown sand, it is perfectly obvious that it has travelled southwards from the great depositories in the cliffs and hills, situated to the north of these points; other portions are continually added by that which is drifted by the wind from the beach, and is retained by the plants which appear to be designed to perform that particular office. It is also obvious that these embankments commence at their northern extremities.

The process which has been detailed, is not hypothetical, nor has it been coloured for the sake of harmonizing with a favourite theory; it is the result of some years attention to the circumstances which, requiring very little of the assistance of man, have formed those bulwarks around our coast that rival in stability many of his boasted works.

One word as to the source whence the materials are derived, which Mr. Robberds conceives inadequate "to produce an average elevation even of one inch," over the peninsula of Yarmouth.

It is impossible, when considering attentively the ever-shifting bed of the German Ocean; the enormous extent of its shoals; the multitude of its sand-banks; the powerful tendency of the north-east winds, and the great tidal current from the same quarter, towards this part of our coast, not to feel assured that ample means for the formation of other banks and alluvial headlands are readily at hand, wherever and whenever local causes give direction to powers of such prodigious magnitude. To elucidate this opinion more fully, we are enabled to refer to the estimates made by Mr. Stevenson, (a civil engineer of great eminence and accuracy,) from a vast number of observations and comparisons on the dimensions of the sandbanks in the German Ocean. The result of this computation is, that their average height is about 78 feet; and the aggregate cubical contents of these immense collections of debris is equal to about 14 feet in depth of the whole North Sea; or to 28 feet in elevation, over the entire area of Great Britain! Let it not be asked then, whence is the material to be derived sufficient to raise a bank of a couple of square miles in area, on an exposed part of the shore of the same ocean, half-a-dozen feet above its level?

In a former article I accounted for the gradual exclusion of marine waters from the æstuaries, and for the silting up of the greater portion of the basin which these waters once exclusively occupied. In this view, the assistance which the process received from the alluvium of the adjacent high grounds, the deposits by land-floods, and the final covering of peat, was estimated to the utmost probable extent of those contributing causes. It is agreed that a solid substance has been substituted for a fluid; the surface of which solid substance, including these accretions, is ascertained to be *below* the level both of the adjacent rivers and of the ocean. Those ancient receptacles, formerly filled with water, are now occupied, to the depth of from 10 to 20 feet, with that description of deposit common to all rivers which pass through a diluvial country, and which open upon a coast whose soft and moveable base is continually affected by the waves. The original arms of the sea are now contracted into streams, which receiving but little accession from the tides, are well adapted for conveying the upland waters, and for the passage of a valuable internal trade. Some of the small lakes have been drained, and partially converted to pasturage in our own days: the margins of the Broads are gradually contracting; even Oulton Broad, which has been thought an exception, is unquestionably diminishing through the formation of peat, which stretches by slow but perceptible degrees over its retired recesses. That Lake Lothing and other similarly circumstanced lakes are the last to silt up, is mainly attributable to their having now no currents loaded with muddy particles passing through them: they have thus been preserved from one of the most active agents in filling such reservoirs. From the earliest period there evidently existed a tendency to deposit oozy matter in the valleys, which tendency was materially increased, when, by the subsequent operations of nature, the outlets against the sea were narrowed. Notwithstanding the calculations on the relative specific gravities of earth and water, which, without a due consideration of all the circumstances, have been set in array against this hypothesis, I am satisfied that the bottoms of the valleys have been raised through such an agency. Against all the arguments which ingenuity can devise to the contrary, must be set the simple truth, that the process has gone on, to a considerable degree, from time immemorial, on various parts of this and other shores; and that it is still going on, and may be daily witnessed, to the fullest extent that my suggested system demands. This tendency of turbid waters, when in a state of comparative quiescence, to deposit the substances they hold suspended, is converted to the benefit of the land-owner in abundance of instances; some of them even on the coast of Norfolk; and, in the words of one of the parties, has proved a mine of wealth to those who have availed

themselves

themselves of it. An instance of undoubted authenticity may be mentioned, in the formation of 1600 acres of valuable land, within the last three years, by Colonel Cheyke, of Rawcliff, near Thorne, in Yorkshire. It was accomplished by the process of warping; that is, by inclosing a given space, upon which the turbid waters of the River Ouse precipitated their silt. The first portion of the land so inclosed consisted of 429 acres, "on the surface of which was deposited, in one year, a fine alluvial soil of the average depth of three feet*." This new land was let off at not less than 35 shillings per acre; and the gold medal of the Society for the Encouragement of Arts was awarded to the proprietor. Subsequently, above 1100 acres more, in two portions, have been formed; and in a letter from that gentleman, in 1825, it is stated that he obtains a deposit of *from three to four feet in one year*, upon that area.

That an average accumulation, to the extent even of a hundredth part, was required for the formation of the low marsh lands in the eastern valleys, it is unnecessary to conceive; but it is evident that, during the long lapse of ages, such a system was slowly performing by nature, under circumstances favourable to the operation.

In proportion as the entrance for the tidal waters was gradually restricted, and their escape was impeded, did the æstuary of the Yare remarkably resemble one of those large artificial inclosures which are embanked for the purpose of warping. Had an engineer, in remote times, projected the conversion of this once extensive waste of waters into a tract of valuable land, as it now exists, his principal operations would assimilate to those which Nature by degrees performed without his intervention. Were those natural barriers which have interrupted the entry of the great body of water, removed; were our æstuaries again emptied of the ooze and alluvium which have filled them,—the same circumstances, making reasonable allowances for the interposition of man, would be renewed; the waters would again flow up to the walls of Norwich, as high and as freely as they ever did; the same system of obstruction and silting would re-commence; and finally would re-conduct to the same phænomena as are at the present moment exhibited†.

There are strong reasons for considering that the other æstuaries (whose openings are sketched in the plate,) were also consolidated by the like influence.

* Trans. Society for the Encour. of Arts, vol. xliii.

† One fact, adverted to in agricultural reports, and not wholly to be overlooked in an inquiry like the present, is, that in the great Marshland district of West Norfolk, those parts which are situated the most remote from the sea, and were the earliest inclosed, are on a *lower* level than those which were reclaimed at a subsequent period.

The

The proofs, physical and historical, having been scrutinized with caution, and as Mr. Robberds is pleased to add, with candour, it would appear that the depression of the sea, to the extent assigned, is far from a necessary consequence deducible from that evidence. It has been shown * that the bed of marine shells, at an elevation of 40 feet, alleged to be similar to existing races in our seas, and marking out the line of an ancient beach, belong chiefly to non-existing species, and to the highest in the series of anti-diluvial formations †. The circumstances relating to these testacea, on which much stress was laid, and which probably suggested the first hints whereon to found the hypothesis, not being again adverted to, it is not unfair to infer are abandoned.

We descend, therefore, at one step to the level of the salinæ in the valleys, for there are no intermediate horizontal beds of shells and marine exuviæ, which, on the theory of the declining waters, must necessarily have occurred and be more or less visible, like the margins of those ancient lakes whose surfaces have progressively fallen by the wearing down of the barriers at their outfall. None of the marshes, at the points where the salinæ mentioned in Domesday are conceived to have been situated, are elevated so much above the level of the sea as to be inapplicable to their original purposes, were not the mouths of the æstuary, as I endeavoured to explain, filled up so as to exclude an adequate supply of salt water ‡. The levels, recently taken by various engineers, prove that the rivers in their progress thence to the ocean have little or no fall: it is well known the marshes for many miles from the sea are below the levels of those rivers; and in the evidence on this subject before the committee of the House of Commons, it was deposed, that on the occasion of the sea breaking through Lake Lothing, 35 years before, it covered this entire level of marshes to the depth of 3 to $3\frac{1}{2}$ feet with salt water.

Domesday-book, therefore, would seem to present us with

* Phil. Mag. and Annals, vol. i. N. Ser. p. 282.

† An appearance similar to the Bramerton shell bed is repeated on the banks of the Thames, in the great and well-known depository of testaceous remains, 60 feet above the level of that river at Woolwich. Here also observers have not been wanting, to speculate on the apparent subsidence of the waters: yet the same stratum sinks beneath the clay under London, and is intersected in the Tunnel at Rotherhithe, 40 feet *below* the surface of the Thames.

‡ It is not essential to a saltwork that any considerable depth of sea-water should be within command; the quantity required is uniform, but not large: I believe about 10 inches deep in the reservoirs, and $1\frac{1}{2}$ to 3 inches only in the pans. The writer has recently examined, with some interest, the salinæ on the Hampshire coast. They are more numerous than on any other part of our shores, and extend from Lymington, westward, three or four miles.

a col-a collateral proof of the permanent level of this sea; and the circumstances of the salinæ will not, on examination, be found to strengthen the contrary opinion so much as was anticipated. Without dwelling further on this head, it only remains to be noticed, that some caution is to be observed in determining their real localities; because the boundaries of several parishes, enumerated in that ancient document, not only frequently stretch to a remote distance from the villages to which they appertain, but it is of common occurrence in this district that detached portions belong to parishes that are situated many miles in the interior. The level tract of marshes between the Yare and the Waveney, opposite Reedham and extending to Breydon Water, contains no less than nine instances of this intermixture, some of which are detached eight miles from the main parts of their parishes. Local knowledge, in inquiries of this kind, is often desirable, to explain many circumstances otherwise obscure, and irreconcileable with probability.

Respecting the hypothesis of the general subsidence of the German Ocean, I offer no other opinions than may be inferred from the review of the facts which are intimately connected with this investigation; and from a previous remark that "if any alteration could be perceptible over so extensive an area, it would be an elevation, corresponding with the disintegration of the land." I was not then aware of a paper which was read before the Geological Society of London in 1816; before the Wernerian Society of Edinburgh in 1820, and published the same year in the Edin. Phil. Journ., entitled "Observations on the Bed of the German Ocean or North Sea, by Robert Stevenson, Esq. Civil Engineer*." This treatise is announced as the result of much personal observation, and considerable professional experience of the eastern coast of Great Britain. Its object is to prove "a great change" in the level of the German Ocean; not that it has fallen, conformably with the views of Mr. Robberds, but that it has *risen*, and continues slowly to rise. The cause is traced to the elevation of the bed of that ocean, by means of the vast increase of alluvium, the wearing away of cliffs and headlands, the reduction of high grounds, the debris of mountains, the decay of vegetable and other organic substances, and the mud of rivers. Mr. Stevenson has founded his reasoning on the consideration of a long series of observations; but it would be unfair to institute any comparison of his hypothesis with that of Mr. Robberds, as the data which have influenced the opinion of that gentleman are not before us. The circumstance is chiefly introduced to show to what opposite conclusions the ablest advocates of truth, sci-

* Edinb. Phil. Journ. vol. iii. art. V.

ence,

ence, and candid inquiry, will not unfrequently tend, and consequently how essential to geological speculations is the previous collection and strict examination of facts.

It will be remembered that the subject of debate was, not so much the level of the ocean, as the validity of the proofs contained within, and adduced from, the district under consideration, and an examination of those data which were urged as strong indications of the gradual depression of the waters. Possessing much local acquaintance with the field of inquiry, I entered into the discussion with some confidence; and the evidence being unsupported by facts drawn from a wider range of observation, the investigation appeared, as indeed it professed, restricted to this district peculiarly.

When in pursuance of the plan sketched by Mr. Robberds, the proofs selected from other shores and from more distant regions are arranged, we shall possess better and more abundant criteria by which to form our judgement: a more extended and interesting field will be opened to our contemplation, and the opinions of the author will be more satisfactorily developed.

I shall not weaken the interest which a consideration of the prominent features of this discussion has created, by combating minor points of difference; being unwilling to appear too tenacious in subordinate details. I am well content to leave it to the decision of those, whose judgement in matters of science we are accustomed to respect. Whatever that decision may be, we shall have had the mutual satisfaction of contributing, through the medium of an amicable controversy, and apart from its speculative portions, to a more accurate knowledge of the physical circumstances of this district.

[It is interesting to trace the origin of certain local terms, and to observe their connection with the Northern maritime nations. Thus the *Meals* or sand ridges on the north coast of Norfolk evidently have their name from the same source with the Teutonic and Islandic *mal*, Swedish *mölja*, Dutch *moeilje*, a pier or mole-head, Ang.-Sax. *mæl*, Germ. *mahl*, a boundary.

The etymology of *Marram* has been mentioned in a previous note.

The Rands or Ronds, described at p. 352, vol. i. as being the spaces between the rivers and their embankments in these valleys, derive their name from the Teutonic *Rand*, an edge, brink, or margin. Bishop Wilkins says that *rand* means flank, in which sense it is applied to a part of beef and to the welt of a shoe.

The appellation Denes for the sandy alluvial tracts next the sea on the Norfolk coast, corresponds with *Dunes, Duynen*, used by the French and Flemish for those on the opposite coast. Thus Duynkerke (Dunkirk) derives its name from the church originally founded on the Denes on the first establishment of that town.

The etymology of Breydon is more doubtful; perhaps from its contiguity to the Denes, like the Flemish Bredene near Ostend: or from the Saxon *Bradan* or *Brædene*, broad.]

Note.—The Maps in the Plate are on the following scales: Fig. 1. at 1 mile to an inch; fig. 2 & 3. ¾ mile to an inch; fig. 4. 2 miles to an inch.

LI. *Pro-*

LI. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

March 12. M. LATREILLE made a verbal report upon a work of M. Payradeau, entitled *Catalogue descriptif et méthodique des Annélides et des Mollusques de l'Ile de Corse.*—M. Cauchy read a memoir On the pressure or tension of solid bodies. —M. de Saint-Fargeau read a notice On hybrid productions of *Volucellæ.*—M. Navier communicated a memoir On the motion of an elastic fluid which escapes from a gasometer.—M. Meirieu read a manuscript, entitled *De la Lithomylie, ou Récherches sur la Destruction de la Pierre dans la Vessie par des Moyens mécaniques.*—The Academy appointed two Commissions, one constituted of MM. Gay-Lussac, Dulong, Arago, Fourier and Thenard, to determine the merits of Memoirs communicated on the compression of liquids;—the second, consisting of MM. Thenard, d'Arcet, Dulong, Gay-Lussac and Chevreul, for the purpose of adjudging the prize founded by M. Monthyon, for rendering an art or trade less unhealthy.

March 19.—The Minister of the Interior forwarded a memoir of MM. Pihorel and Desmoulins, On the examination of the body of Mr. Drake, who died at Rouen in consequence of the bite of a rattlesnake.—M. Henry wrote to remind the Academy, that it was he who gave the process at present adopted for the preparation of sulphate of quina, believing himself, on this account, intitled to one of the prizes instituted by Monthyon.—M. Latreille read a report upon a memoir of M. Vallot concerning the *Cecidomyes.*—M. Vallot was advised to continue his researches.—MM. Cuvier and Duméril reported very favourably respecting the anatomical researches which MM. Milne Edwards and Audouin had presented, On the circulation in *Crustacea.*—M. Biot read a memoir On the measurement of azimuths in geodesical operations.—M. Cuvier read a memoir On the genus of fish called *Pogonias.*—M. Geoffroy communicated on this subject some observations which he had made on certain *siluri* of the Nile:—these fish make a noise under water which is very sensible at the surface; they appear to employ the spines of their fins for this purpose.—M. Girard gave a verbal account of M. Lambardie's work, intitled *Observations sur le Projet de Barrage de la Seine.*

March 26.—Mr. Scoresby, recently named a corespondent, returned his thanks to the Academy.—M. Segalas deposited a sealed packet.—M. Felix Haise presented a new valve, which he considered as adapted to prevent the explosion of boilers.—M. Mathieu made a favourable report of a water clock, invented by M. Blanc of Grenoble.—M. du Petit-Thouars read a memoir On the history of coniferous trees.—MM. Raspail and Robineau Desvoidy communicated their researches into the natural history of the *Alcyonella stagnorum.*

April 2.—The Minister of the Interior communicated the report of the Prefect of Doubs upon the fossil bones which had been discovered by Dr. Buckland in the grottos of Osselles near Besançon.—M. Levret sent a notice On atmospheric refractions.—M. Latreille gave a favourable account of the work presented by M.

Le Pelletier de Saint-Fargeau, relating to hybrid productions.—M. Beudant made a favourable report on the memoir of MM. Delcros and Rozet, on the nature of the soils which compose the mountains of the south part of the marshes of Caronte and Berre, department des Bouches-du-Rhône. This memoir particularly describes some deposits of lignite of a prior formation to those which have hitherto been noticed.—M. Moreau de Jonnès communicated some statistical sketches On the civil life and domestic œconomy of the Romans at the commencement of the fourth century of the Empire.—M. Rozet read a geological notice on the environs of Aix in Provence.—The experiments of M. Giroux de Buzareingues, on the reproduction of domestic animals, were read.

April 9.—The Minister of the King's Household transmitted a memoir by M. Ratienville On several œconomical methods of giving woollens a deep blue colour without using indigo.—M. Bernay addressed to the Academy a new solution of the problem of longitudes; —MM. Griffon (father and son), a design for a new machine;—M. Losana, a collection of meteorological observations made at Lombriasco.—M. Gay-Lussac presented an instrument proposed by M. Collardeau, which indicates the number of atmospheres and portions of an atmosphere which correspond to the elastic force of vapour in steam-engines.—M. Sturm read a memoir On the application of the calculus to determine the course of reflected or refracted rays.—M. Damoiseau read a memoir On the perturbations of the comet of $6\frac{1}{2}$ years.—The Academy, by the confirming opinion of the Section of Geometry, decided that there is no occasion for proceeding to supply the place of M. Laplace at present.—M. Geoffroy Saint-Hilaire read a memoir upon a monstrous junction of the membranes and the yolk, and the effects which the adhesions produced in a newly hatched chicken.

April 16.—M. Cazenave requested to deposit a sealed packet containing a description of a new method of breaking a stone in the bladder.—M. Banque sent some observations on the employment of the ethereal tincture of the powdered leaves of the Belladonna, in a case of spontaneous hydrophobia and a violent colic.—M. Sérullas sent a letter to fix the date on the subject of some new observations which he had just made on the cyanurets.—M. Desgenettes became a candidate for the vacant place of Academician.—M. Geoffroy Saint-Hilaire announced, that it results from the recent observations of Dr. Barry, that the air is more compressed in the egg than in the atmosphere.—M. Cauchy read a memoir On the integration of linear equations with partial differences. M. Becquerel read a memoir On the electricity developed during chemical action, and upon the employment of very weak electrical currents as a mean of exciting the combination of a great number of bodies.—M. Richard read the *Monographie des Orchidées*.—M. Boullay read a memoir On double iodides. The Commission named by scrutiny, for presenting candidates for the vacant situation of free Academician, is composed of MM. Le Gendre, Fourier, Desfontaines, Thenard, Andreossy, and Maurice.

April 23.—M. Darnaud writes, that in a part of Greece which he has

has lately visited, hydrophobia is cured by making deep incisions under the tongue of the patient.—M. Vernière sent some experiments upon the means for arresting the action of poisons caused by the bite of venemous animals.—M. Arago communicated the results recently obtained by M. Despretz, relative to the law of Mariotte. M. Bouvard presented a memoir containing a new discussion relating to the meteorological observations made at the Royal Observatory at Paris.—M. Boullay finished the reading of his paper On the double iodides. M. Schlick (a Danish architect) read a memoir on the subterraneous passage now making under the Thames by M. Brunel.—M. Raspail read an analytical extract from his physiological researches on grains and oils.

April 30.—M Ratienville announced that a dye of certain indigenous plants is capable of dyeing woollens of a royal blue colour without the use of indigo.—M. Denaix presented an essay On methodical and comparative geography;—M. Ostrogradsky, a memoir On the propagation of heat in the interior of solid bodies;—M. Pihorel, Observations on the bite of rattle-snakes.—M. Arago communicated a notice respecting the sounds produced in M. Clement's experiment.—M. Morin, apothecary at Rouen, sent to the Academy an analysis of a concretion found in the brain of a man who died of acute gastritis. According to him, this concretion was composed of cholesterine, coagulated albumen, and phosphate and carbonate of soda. According to the report of M. Chevreul, the experiments of M. Morin are not sufficient to determine with certainty the existence of cholesterine in the concretion.—M. Labillardière reported respecting the observations of MM. Poiteau and Turpin relative to the peculiar directions which are assumed by the radicle and stalk of a plant moved circularly: the explanation of the two authors agrees with that which Mr. Knight had previously given.—M. Cauchy gave an account of a memoir by M. Roche, On the rotary motion of solid bodies.—M. Poisson read a memoir On the rotary motion of the earth.—The Commission directed to present the candidates for the place of free Academician now vacant, made its report;—the candidates are: M. le comte Dau, MM. Cassini, Desgenettes, Lamandé and General Rogniat.

May 7.—The Academy received a memoir from M. Tabareau On the explosion of steam-boilers.—A paper, intitled *Examen de l'Ouvrage de M. Dutrochet sur l'Agent immédiat du Mouvement vital;*—M. Freycinet read a letter from the naturalists attached to the expedition of M. Durville: it was dated 'Port Jackson.'—M. Arago communicated a memoir which he had received from M. Boussingault, upon the composition of native argentiferous gold.—M. Moreau de Jonnès read a dissertation on the bite of rattle-snakes.—A free Academician was elected: M. Cassini had 31 votes, and M. Daru 30.—MM. Arago and Dupin gave a very favourable account of M. Poncelet's course of mechanics applied to machines.—M. Heurteloup read a memoir on *lithotritie*, in which he related several new cases of cure.

May 14.—M. Cordier announced the loss which the Academy had sustained by the death of M. Ramond.—M. Tabareau addressed

 a second

a second memoir on the explosion of steam-boilers; M. Van-Hoorick announced that he had discovered a new construction of the under carriage applicable to all sorts of carriages.—M. Marcel de Serres sent a memoir on Geology.—M. Arago read a letter from M. Despretz, in which he gave an account of some experiments intended to render the heat sensible which is disengaged during the compression of liquids.—M. Cléver of Maldigny (army surgeon) read a Memoir,—after having been seven times cut for the stone, it was reproduced an eighth time. M. Cléver determined to submit to the operation of *lithotritie;* M. Civiale succeeded perfectly; the pain occasioned by *lithotritie* is nothing, when compared with that of lithotomy.—MM. Duméril and Fréd. Cuvier presented the report required by the Minister on the death of Mr. Drake. The conclusions are, that the only measure of the Police which could be efficacious, would be that of interdicting the introduction of living venemous serpents into France, always excepting those which are connected with the interests of commerce, or for the advantages of science by persons of reputation.—A Commission consisting of MM. Le Gendre, Poisson, Lacroix, Fourier and Poinsot, will propose a mathematical prize question to be adjudged in 1829.

May 21.—The Keeper of the Seals forwarded a sample of M. Delattre's indelible ink.—M. Mangin presented a memoir, entitled *Moyen unique d'obtenir de grands Avantages de l'Emploi des Paratonnerres.*—M. Merieu proposed that comparative experiments should be made upon the efficacy of all instruments used in lithotomy hitherto invented.—M. Tabareau sent a third memoir On the bursting of steam-engine boilers.—M. Arago presented a detailed analysis of a memoir, which M. Kupffer had sent him from Kasan, On the diurnal variations of direction and intensity in terrestrial magnetism.—M. Girard, in the name of the Commission, gave a favourable account of the model of a carriage with four wheels, which M. Van-Hoorick had presented.—MM. Arago, Bouvard, Mathieu, Damoiseau and Le Gendre were appointed by ballot as a Commission to examine for the present year such pieces as were likely to obtain the medal instituted by Lalande.—M. Arago presented from the author M. Leopold Nobili de Reggio, a memoir, entitled *Projet d'un Instrument comparable propre à mésurer les Courans électriques.*—M. Faure read a memoir on the iris and on artificial pupils.—M. Giroux of Buzareingues communicated some observations and experiments which he had made on the reproduction of domestic animals.—M. Benjamin Delessert read for M. Delelle, professor at Montpellier, a description of a new genus of the family of the Cucurbitaceæ.

June 4.—The election of M. Cassini, jun. was confirmed by the King.—A notice was received from M. Girard On hydraulic cements.—M. Cordier read a paper On the temperature of the interior of the globe; and M. Bormond a memoir On the regularity of the geognostic phænomena of Arkose, in the east of France.

June 11.—At this sitting were read, éloges by M. Cuvier on M. Halle and M. Corvisart; and M. Dupin's researches on the canals of France.

LII. *In-*

LII. *Intelligence and Miscellaneous Articles.*

DUTY OF STEAM-ENGINES.

A REMARKABLE improvement in the duty of steam-engines has of late taken place in two instances on mines in Cornwall, which appears from the printed accounts which record the work performed monthly by the engines at all the principal concerns. Hitherto the best have reached only to 40 millions of pounds of water lifted one foot high by each bushel of coal consumed; some have indeed occasionally done more, but not to any extent or continuance.

The improvement which we now notice is in two engines erected by Capt. Samuel Grose, and the only ones as we believe which he has yet constructed. We are not acquainted with the exact nature of the difference between these and other engines usually employed; but we believe there is nothing altered of importance in the general principle, but rather in smaller details in matters which have been hitherto overlooked.

Whatever it may be, however, it is not less creditable to the engineer, and is particularly worthy of praise, as not having been attended, as we are told, with additional complication or expense.

The first engine in which this improvement appeared was one erected at Wheal Hope, of 60-inch cylinder, working single as usual.

The duty reported, is in	April	42,101,739 lbs.
	May	42,241,650
	June	54,725,716
	July	55,012,292
	August	50,979,084

These results are, however, much exceeded by the engine afterwards built by Capt. Grose, at Wheal Towan, of 80-inch cylinder, where the duty done has been

April	61,877,545
May	60,632,179
June	61,762,210
July	62,220,820
August	61,764,166

Thus exceeding by nearly 50 per cent what had hitherto been attained.—We should be very glad to be able to lay before our readers some good account of the particulars of this great improvement, which we hear is in progress of being applied to other large engines in the same district.

ON THE USE OF CHLORINE IN DESTROYING FIRE-DAMP.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen, West Bromwich, Sept. 20, 1827.

In your Number for August, (which I have only received within a few days,) I observe a notice of a paper (communicated by Mr. Children)

Children) to the Royal Society, " On destroying the fire-damp in mines by chloride of lime, by F. Fincham, Esq."

It is a somewhat remarkable coincidence that I had recommended the use of chlorine for the same purpose in January last; and although I have some doubt as to its practicability, I am now pursuing a series of experiments in a colliery at West Bromwich, in the hope of removing the objections against the use of chlorine, which have hitherto prevented its adoption.

If my experiments prove successful, I will take an early opportunity of forwarding you an account of them.

I am, &c. JOHN WOOLRICH.

MINERAL WATERS OF BILIN.

The following are the contents of these springs, according to Professor Steinmann:

	Joseph's-Well.	*Caroline's-Well.*
	In 10,000 parts.	
Sulphate of lime	2·462	2·127
Ditto of soda	7·212	6·943
Muriate of soda	3·811	3·303
Carbonate of soda	31·182	23·411
Ditto of lithia	0·114	0·105
Ditto of strontia	0·018	0·018
Ditto of lime	3·658	3·801
Ditto of magnesia	2·573	2·010
Ditto of protoxide of iron	0·064	0·071
Ditto of protoxide of manganese	0·015	
Subphosphate of alumina	0·019	
Phosphate of lime	0·007	
Silica	0·505	0·549
Solid contents	51·040	42·338
Carbonic acid gas	23·620	22·317
Atmospheric air	0·153	0·108
	74·813	64·763

Schweigger's Jahrbuch der Chemie, &c. N.R. Band xviii. *p.* 184.

ANALYSIS OF ALLOPHANE.

Dr. Walchner has recently analysed this mineral, from Gersbach, in Schwarzwalde, having a specific gravity of 1·8, with the following results:

Silica	24·109
Alumina	38·763
Oxide of copper	2·328
Water	35·754
	100·954

Ib. Band xix. *p.* 154.

INDIGO

INDIGO COPPER-ORE.

Demselben has obtained from a specimen of this mineral from Hansbaden, specific gravity 3·8.

Copper	64·773
Sulphur	32·640
Lead	1·046
Iron	0·462
	98·921

Ib. p. 158.

SPECIFIC HEAT OF GASES.

MM. Aug. de la Rive and F. Marcet having made numerous experiments on the above subject, have arrived at the following conclusions:—

1st, That equal volumes of all gases under the same constant pressure, have the same specific heat.

2dly, That all other circumstances being equal, the specific heat of all gases diminishes with diminished pressure, in a slightly converging series, and in a much less degree than that of the pressures.

3dly, That each gas has a different conducting power; that is to say, all gases have not the same power of communicating heat.—*Ann. de Chim. et de Phys.*

COMPOSITION OF APATITE.

Mons. G. Rose remarks, that Berzelius having observed that the analyses of apatite by Klaproth and Vauquelin gave results which agreed with each other and also with definite proportions, he did not think it necessary to analyse it again. M. Rose, however, finds that 100 parts of apatite from the following places, contained the annexed proportions of chloride and fluoride of calcium,—the remainder being phosphate of lime, with a small quantity of iron and magnesia in some cases, but quite accidental and unimportant.

	sp. gr.	Chloride of Calcium	Fluoride of Calcium
Apatite from Snarum in Norway	3·174	4·280	4·590
——— Cabo de Gata in Spain	3·235	0·885	7·049
——— Arendal	3·194	0·801	7·010
——— Greiner in the Tyrol	3·175	0·150	7·690
——— Faldigl in the Tyrol	3·166	0·100	7·620
——— Saint Gothard	3·197	a trace	7·690
——— Ehrenfriedersdorf	3·211	ditto.	7·690

Ibid.

NOTICE OF FOSSIL TREES, NEAR GALLIPOLIS, OHIO; BY DR. S. P. HILDRETH.

About two miles above Gallipolis, and half a mile from the Ohio river, is the location of several petrified trees. They are found near the base of a mural precipice of sandstone rock, 50 feet in height, and crowned with earth and trees to an elevation of 70 feet.

From

From the foot of the rock, the ground gradually descends 30 or 40 feet to the Ohio bottom, which is low and swampy near the hill. This descent is probably made by the debris and earth rolling down from the hill, and gradually accumulating for ages, so as to cover a larger portion of the sandstone rock below the surface, than now appears above. The Ohio river no doubt once washed the base of the rock, but has gradually changed its channel to its present bed.

The rock in which the trees are imbedded, is a coarse sandstone, and they appear in the face of the rock at different elevations, some near the present surface of the ground, and others 4 or 5 feet above. They are 7 in number, and scattered through a space 80 rods in length;—some appear to have fallen, or been deposited with their tops, or branches, towards the river, and others in the opposite direction;—some came out of the rock obliquely, and others at right angles: they vary in diameter from 8 to 18 inches. I am not satisfied as to what family of trees they belong, but some of them look like elm. They are readily distinguished from the rock in which they are imbedded, by their different colour and composition; their colour being much darker, and texture much harder; having a reddish-brown cast, like iron-ore, and so hard as to scintillate briskly, when struck with a hammer or head of an axe, affording evidence of their siliceous composition. The interstices of the laminæ are in some places filled with small crystals of quartz; and in others, with thin layers of stone coal. There is evidently a considerable quantity of iron in its composition, as the surface becomes quite red after being heated in the fire. The cortical part seems to have been more difficult to petrify than the ligneous portions, as it is in most of the trees readily separated from the wood and from the surrounding rock; being also easily broken, and resembling iron-rust in colour and appearance on some of the trees; and on others, like black sand or emery. The trees do not project much beyond the face of the rock, but appear to have been broken off at the same time when the rock was rent in which they are imbedded. Sandstone is the principal rock formation throughout this part of the state of Ohio, forming mural precipices from 50 to 100 feet high, and in some places for half a mile, or a mile in extent, on the margins of the Ohio bottoms on both sides of the river, and underlying the river hills and country adjacent for a great distance; appearing near the beds of creeks and ravines, where the superincumbent earth has been washed away by the streams; but is seen no where to better advantage, than near the Ohio river. It is of various qualities; micaceous, argillaceous and quartzose or siliceous; some so hard and compact as to make good mill-stones, and nearly resembling granite in colour and texture; and some so fine and close-grained as to bear the chisel of the sculptor nearly as well as marble. From the position of these fossil remains, I am led to conclude that the trees were brought to this spot by the water, at that remote period when the valley of the Ohio was an ocean, and covered in a vast bed of sand by some great convulsion of Nature. The sand in time became cemented into rock, and the spaces occupied

cupied by the ligneous parts of the trees were, by infiltration, filled up with siliceous particles and iron, with some partial attempts at carbonization. Had there been a large pile of trees in a body, they would probably have formed stone coal, as is the case in the sand rock a few miles above; but this is only conjecture. There is a bed of stone coal in the same hill, not far from the trees. Native alum and copperas are also found in this vicinity.—*Silliman's Journal, June* 1827.

ON THE FASCINATION OF SNAKES; BY MR. NASH.

I have often heard stories about the power that snakes have to charm birds and animals, which, to say the least, I always treated with the coldness of scepticism, nor could I believe them until convinced by ocular demonstration. A case occurred in Williamsburgh, (Mass.) one mile south of the house of public worship, by the way side, in July last. As I was walking in the road at noon-day, my attention was drawn to the fence by the fluttering and hopping of a robin red-breast, and of a cat-bird, which upon my approach flew up, and perched on a sapling two or three rods distant; at this instant a large black snake reared his head from the ground near the fence. I immediately stepped back a little, and sat down upon an eminence; the snake in a few moments slunk again to the earth, with a calm placid appearance, and the birds soon after returned and lighted upon the ground near the snake: first stretching their wings upon the ground, and spreading their tails, they commenced fluttering around the snake, drawing nearer at almost every step, until they stepped near or across the snake, which would often move a little or throw himself into a different posture, apparently to seize his prey, which movements, I noticed, seemed to frighten the birds, and they would veer off a few feet, but return again as soon as the snake was motionless. All that was wanting for the snake to secure the victims seemed to be, that the birds should pass near his head, which they would probably have soon done, but at this moment a waggon drove up and stopped. This frightened the snake, and it crawled across the fence into the grass; notwithstanding, the birds flew over the fence into the grass also, and appeared to be bewitched to flutter around their charmer, and it was not until an attempt was made to kill the snake that the birds would avail themselves of their wings and fly to a forest one hundred rods distant.

The movements of the birds while around the snake seemed to be voluntary, and without the least constraint; nor did they utter any distressing cries, or appear enraged, as I often have seen them when squirrels, hawks, and mischievous boys attempted to rob their nests or to catch their young ones; but they seemed to be drawn by some allurement or enticement, (and not by any constraining or provoking power;) indeed, I thoroughly searched all the fences and trees in the vicinity to find some nest or young birds, but could find none.

What this fascinating power is, whether it be the look, or effluvium, or the singing by the vibrations of the tail of the snake, or any thing else, I will not attempt to determine; possibly this power may be owing to different causes in different kinds of snakes. But so far as the black snake is concerned, it seems to be nothing more than an enticement or allurement with which the snake is endowed to procure his food.

In the month of June, 1823, in company with a friend, I had just crossed the Hudson river, from the town of Catskill, and was proceeding in a carriage, by the river, along the road, which is here very narrow, with the water on one side and a steep bank covered with bushes on the other. Our attention was in this place arrested by a number of small birds, of different species, flying across the road and then back again, and turning and wheeling in manifold gyrations, and with much chirping, yet making no progress from the particular place over which they fluttered. We were not left long in doubt, when we observed a black snake of considerable size, partly coiled and partly erect from the ground, with the appearance of great animation, his eyes brilliant, and his tongue rapidly and incessantly brandished. This reptile we perceived to be the cause and the centre of the wild motions of the birds, which ceased, as soon as the snake, alarmed by the approach of the carriage, retired into the bushes; the birds, however, alighted upon the neighbouring branches, probably awaiting the re-appearance of their tormentor and enemy. Our engagements did not permit us to wait to see the issue of this affair, which seems to have been similar to that observed by Mr. Nash.—American Editor.—*Silliman's Journal, June* 1827.

FOSSIL REMAINS OF THE MASTODON LATELY FOUND IN ONTARIO COUNTY, NEW-YORK.

In repairing and cleansing the village spring, and the ditches connected with it, which are dug in marl that extends two feet below the surface, it was deemed proper to deepen them; and in doing this the bones were found—about half a mile east of the court-house at Genesseo, in a small marsh, that has some elevation above the surrounding country.

The tusks were first seen, and then the head, but these, as indeed the whole skeleton, were in such a state of almost total decomposition, as to defy all attempts at preservation. The skeleton lay in the direction so frequently observed in the remains of this animal, South West and North East. The head rested upon the lower jaw. The tusks were much decayed; their points were five feet apart, and curved at least a foot from the centre. They were four feet and two inches in length; the largest diameter could not be ascertained on account of their decay—but it was preserved a considerable distance and then gradually diminished, so that at five inches from the point the diameter was three inches. The laminated structure of the tusk was rendered evident by decomposition, which had in a measure separated the laminæ, and the whole was supposed to be phosphate of lime.

Of the two (superior) incisors, no trace could be discovered, but the eight molars were *in situ*. The length of the largest tooth was

was six and a quarter inches; of the smallest three and a half: the crown of the tooth was two and a half; and the breadth of the enamel from $\frac{1}{8}$ to $\frac{3}{8}$ of an inch, as was rendered visible by wearing away of the surface. The roots were all broken and decayed. The animal could not have been old, as eight molar teeth were found; old animals have only one molar on either side of each jaw.

The pelvis was twenty-two inches in its transverse diameter, between the acetabula at the inferior opening. The epiphyses of the large bones and the patellæ were found nearly perfect, not having suffered from decay.—*Silliman's Journal, June* 1827.

SULPHOCYANURET OF POTASSIUM IN SALIVA.

MM. Tiedmann and Gmelin evaporated the saliva of a young man secreted while he was smoking, and obtained 1·14 per cent of residuum, which was treated with hot alcohol. During cooling it deposited a pale-brown substance, and the alcohol was evaporated and left a residuum, which was treated with water. Large flocks of a brownish-white colour were separated, and the water contained sulphocyanuret of potassium. It reddened tincture of litmus, owing to the change suffered by the saliva during evaporation, effervesced when cold on the addition of nitric acid, gave no precipitate with chlorine, muriatic acid, alum, permuriate of mercury or potash; but it gave a precipitate with muriate of tin, acetate of lead, sulphate of copper, protonitrate of mercury, nitrate of silver, and tincture of galls; and with permuriate of iron it gave no precipitate, but became of a deep blood-red colour. This colour, which Treviranus has already observed, can be derived only from sulphocyanic acid; for the saliva distilled with phosphuric acid gave a limpid product, which had no acid smell, although it sensibly reddened litmus, and gave with permuriate of iron a very deep red colour. This product gave abundant white precipitates with the nitrates of silver and mercury; and after it was heated with chlorate of potash and muriatic acid, it gave sulphate and muriate of barytes. Lastly, with the sulphates of iron and copper a white precipitate was obtained, which when well washed, imparted to potash the property of reddening the permuriate of iron.—It is proved readily by examining the ashes of the saliva, that it is potash which exists in combination with the sulphocyanic acid. The soluble portion of these ashes consists of much carbonate of potash, of phosphate of potash and chloride of potassium, and of a small quantity of sulphate of potash; the insoluble portion was phosphate of lime, and a small quantity of carbonate of lime and of magnesia. Sulphocyanic acid was also discovered in the saliva of another young man, secreted without smoking.

The saliva of a dog and sheep was also examined. The following are the results obtained from human saliva, and that of these animals:

Human saliva contains only 1 to 2·5 per cent of solid matter; that of the dog contains more. The various kinds of saliva consist of, 1st, the principle of the saliva (*speichelstoff*); 2dly, osmazome; 3dly, 3dly, mucous matter, which appears to be held in solution by the carbonate of potash; 4thly, probably a little albumine; 5thly, in the human saliva, a fatty matter containing phosphorus; 6thly, soluble salts, consisting of acetate of potash, the presence of which, however, cannot be ascertained by any direct means, but only by incineration; carbonate of potash, which in the sheep occasions the saliva to effervesce with acids; it is probably in the state of bicarbonate. The saliva of the sheep contains it in the greatest quantity; then that of the dog, and lastly, human saliva; phosphate of potash, in the greatest quantity in man and the sheep, and least in the dog; sulphate of potash, which exists in the three kinds of saliva in very small quantity; muriate of potash, an alkaline sulphocyanuret, there is most of it in human saliva, a very small portion in that of the sheep and dog's saliva is probably free from it; the muriate in man is almost entirely that of potash; in the dog and sheep it is muriate of soda, with very little muriate of potash; the contents of the saliva insoluble in water are phosphate of lime, less carbonate of lime, and very little carbonate of magnesia.—*Ann. de Chim. et de Phys.* tom. xxxv. p. 266.

SCIENTIFIC BOOKS.

Preparing for Publication.

Cases and Observations on the successful Treatment of Disorders of the Digestive Organs, Asthma, Deafness, Blindness, Lameness, &c. by Galvanism, &c. 2d edit. By M. La Beaumé, F.L.S. &c.

Outlines of Modern Midwifery; and early in the Spring, a work on the Diseases of Women and Children. By Dr. Conquest.

NEW PATENTS.

To Lemuel Wellman Wright, of Mansfield-street, Borough Road, Surrey, for improvements in the construction of cranes.—Dated the 17th of August 1827.—6 months allowed to enrol specification.

To Lemuel Wellman Wright, of Mansfield-street, Borough Road, Surrey, for improvements in machinery for cutting tobacco.—21st of August.—6 months.

To Gabriel de Seras of Leicester-square, Stacey Wise, and Charles Wise, of Maidstone, paper-makers, for certain improvements communicated from abroad, in sizing, glazing, or beautifying the materials employed in the manufacturing of paper, pasteboard, Bristol boards, &c.—21st of August.—6 months.

To John Hague, of Cable-street, Wellclose-square, for a new method of working cranes, or tilt hammers.—30th of August.—2 months.

To B. M. Combs, of Birmingham, for certain improvements on or additions to a pulley machinery and apparatus used for securing, fixing, and moving curtains, and roller and other blinds.—30th of August.—2 months.

To William Deltmer, of Upper Mary-le-bone-street, Fitzroy-square, piano-forte maker, for improvements on piano-fortes.—30th of August.—6 months.

To

To William J. Ford, of Mildenhall, Suffolk, farrier, for improvements in the make, use, and application of bridle-bits.—6th of September.—2 months.

To George Clymer, of Finsbury-street, for an improvement in typographic printing between plain or flat surfaces.—6th September.—6 months.

Mr. Moyle's *Meteorological Register for* 1826, *kept at Helston, in Cornwall.*

	Barometer (corrected to 32° of Fahrenheit).				
	Max.	Day.	Min.	Day.	Monthly mean.
January	30·4560	17	29·4000	6	29·9367
February . . .	30·4500	26	29·3000	16	29·9086
March	30·3500	31	29·3800	24	30·2793
April	30·4000	14	29·4660	12	29·8467
May	30·3200	1	29·6700	19	30·0232
June	30·4000	17	29·8800	27	30·1673
July	30·2100	25	29·3860	8	29·9303
August	30·2400	18	29·5200	25	29·9029
September . .	30·2440	11	29·3280	5	29·8545
October	30·2800	28	29·4400	25	29·8115
November . .	30·4300	21	29·0500	13	29·8800
December . .	30·6200	28	29·2600	1	29·9271
Annual mean	30·3691		29·4233		29·9631

	Thermometer.				Rain in 100dths of an inch in 100 feet.
	Registery in shade.			Black Ball in Sun.	
	Max.	Min.	Monthly mean.	Monthly mean.	
January	51°	24°	39·92	60·2	2·98
February . . .	53	36	46·90	67·2	4·89
March	60	40	46·00	73·1	2·73
April	60	36	49·40	77·8	0·77
May	71	38	49·70	90·0	0·37
June	78	48	69·70	100·8	0·30
July	80	53	64·80	120·2	0·68
August	76	52	64·00	113·0	1·76
September . .	68	45	60·20	103·0	4·65
October	65	42	55·70	91·0	1·67
November . .	56	31	44·80	70·5	3·91
December . .	55	34	46·77	61·0	3·66
Annual mean	64·4	39·9	53·15	85·65	28·37

	Winds.										
	E.	NE.	N.	NW.	W.	SW.	S.	SE.	Prevailing Winds.	Wet Days.	Dry Days.
January....	9	6	2	2	0	5	2	5	E.	10	21
February...	1	0	0	3	3	16	2	3	SW.	14	14
March.....	8	2	6	2	3	8	1	1	SW.	6	25
April......	0	0	7	4	4	9	4	2	SW.	5	25
May.......	5	7	10	4	0	4	0	1	N.	6	25
June......	6	5	7	8	1	2	1	0	NW.	1	29
July.......	4	0	5	5	6	10	1	0	SW.	7	24
August....	2	0	8	3	4	9	4	1	SW.	12	19
September..	8	1	3	3	3	11	1	0	SW.	11	19
October....	3	0	1	10	6	6	4	1	NW.	8	23
November..	1	8	6	8	2	4	1	0	NW.	14	16
December..	4	4	4	7	1	9	3	1	SW.	14	17
	51	33	59	59	33	91	24	15	SW.	108	257

Barometer.—Highest 30·6200. Dec. 28. Wind N.E.
Lowest 29·0500. Nov. 13. Wind S.W.
Com. Thermometer.—Highest 80°. July 30. Wind S.W.
Lowest 26°. Jan. 9. Wind N.E.
Registry in Shade.—Highest 80°. July 30. Wind S.W.
Lowest 24°. Jan. 9. Wind N.E.
Registry in Sun. Black Ball.—Highest 140°. Aug. 19. Wind S.E.
Lowest 24°. Jan. 9. Wind N.E.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·41 Aug. 29. Wind NE.—Min. 29·38 Aug. 15. Wind SW.
Range of the mercury 1·03.
Mean barometrical pressure for the month . . . . 30·009
Spaces described by the rising and falling of the mercury . . . 4·620
Greatest variation in 24 hours 0·360.—Number of changes 21.
Therm. Max. 77° Aug. 2. Wind SE.—Min. 48° Aug. 26. Wind N.
Range 29°.—Mean temp. of exter. air 62°·58. For 31 days with ☉ in ♌ 64·43
Max. var. in 24 hours 23°·00 —Mean temp. of spring water at 8 A.M. 54°·35

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 16th . . . . 84°
Greatest dryness of the air in the afternoon of the 5th 38
Range of the index 46
Mean at 2 P.M. 49°·6—Mean at 8 A.M. 57°·4—Mean at 8 P.M. 61·5
—— of three observations each day at 8, 2, and 8 o'clock . . 56·2
Evaporation for the month 3·85 inch.
Rain near ground 2·060 inch.—Rain 23 feet high 1·900 inch.
Prevailing Wind N.

Summary of the Weather.

A clear sky, 5½; fine, with various modifications of clouds, 15½; an overcast sky without rain, 6; rain, 4.—Total 31 days.

Clouds.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
21	12	29	0	25	25	13

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
8	7½	1	1	3	4½	2	4	31

General Observations.—The month has been generally fine and dry, with occasional gales of wind; and the quicksilver in the barometer has been pretty uniform in its elevations and depressions.

In the evening of the 9th instant, lunar lights and shades were observed here in front of a dark cirrostrative cloud near the horizon; they were produced by the moon's rays passing through several apertures in the cloud, and were almost as conspicuous as the solar lights and shades that are sometimes produced by the same means.

On the 15th upwards of an inch of rain fell, mostly in the night, which was brought on by the inosculation of two winds, the lower one from the S.W., and the upper one from the North, and for three or four days in the middle of the month it also rained; but since that time none measurable has fallen, consequently an abundant corn-harvest has been got-in in this county to the satisfaction of the farmers in general.

Although the mean temperature of the external air this month is one degree and one-fifth lower than the mean of August for the last eleven years, arising from the cold nights; yet the uniform dryness of the air and hot sunshine have been very favourable for ripening the corn and fruits, with very little loss sustained by the few wet days.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are fourteen meteors, frequent thunder on the 16th and 17th in the day-time, sheet-lightning in the evening of the 23rd; and six gales of wind, or days on which they have prevailed, namely, three from the N., one from N.E., and two from the S.W.

REMARKS.

London.—Aug. 1, 2. Sultry. 3. Rain during the night. 4. Showers. 5—9. Fine. 10. A thunder storm at noon with heavy showers of rain. 11. Showery, with thunder. 12—15. Cloudy. 16. Showery. 17. A thunder storm at 3 P.M.; showery. 18—20. Fine. 21. Cloudy: with showers. 22. Cloudy. 23. Fine. 24. Cloudy. 25. Showers. 26. Showery. 27. Cloudy and fine. 28, 29. Fine. 30, 31. Cloudy.

Penzance.—Aug. 1. Clear. 2, 3. Fair showers. 4—6. Fair. 7, 8. Clear. 9. Clear: rain at night. 10. Clear. 11. Showers. 12. Clear. 13. Showers 14. Rain: fair. 15, 16. Fair: rain. 17. Rain. 18. Fair: showers. 19. Clear. 20. Fair: light showers. 21. Showers: fair. 22—26. Clear. 27, 28. Fair. 29—31. Clear.—Rain guage ground level.

Boston.—Aug. 1—3. Fine. 4—6. Cloudy. 7—9. Fine. 10. Rain: with thunder and lightning, 1 P.M. 11. Fine: rain, P.M. 12, 13. Fine. 14. Cloudy: rain A.M. 15. Cloudy: rain, A.M. and P.M. 16. Fine: rain, P.M. 17. Cloudy. 18. Cloudy: rain, A.M. 19. Cloudy. 20. Rain. 21, 22. Cloudy. 23. Fine. 24. Cloudy: rain at night. 25, 26. Cloudy. 27. Fine. 28. Cloudy. 29. Fine. 30. Fine: rain, P.M. 31. Cloudy.

RESULTS.

Winds, NE. 7: E. 1: SE. 4: SW. 6: NW. 13.

London.—Barometer: Mean of the month........................ 30·147 inch.
Thermometer: Mean of the month .... 61·419°
Evaporation .... 3·93 inch.
Rain .... 1·99.

Meteoro-

Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1827.	Barometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Gosport. Max.	Gosport. Min.	Boston 8½ A.M.	Thermometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Gosport. Max.	Gosport. Min.	Bost. 8½ A.M.	Wind. Lond.	Penz.	Gosp.	Bost.	Evapor. Lond.	Gosp.	Rain. Lond.	Penz.	Gosp.	Bost.
Aug. 1	30·28	30·06	30·00	29·90	30·17	30·07	29·53	81	45	69	54	71	54	65·5	SW.	W.	NE.	W.	...	...	...	...	...	...
2	30·06	29·92	29·74	29·68	29·94	29·82	29·61	89	61	66	54	77	61	68·5	SE.	SW.	SE.	NW.	...	...	...	...	0·010	...
3	29·92	29·83	29·60	29·50	29·80	29·74	29·10	81	59	67	58	76	61	69	SW.	SW.	SW.	W.	...	0·55	0·21	...	·020	...
4	30·19	29·92	29·78	29·60	29·87	29·68	29·02	78	54	66	58	73	59	66	SW.	NW.	SW.	W.	...	...	...	0·205	·025	...
5	30·42	30·19	29·98	29·94	30·18	30·04	29·50	74	56	66	59	73	58	61	NE.	NE.	N.	NE.	...	...	·14	...	...	...
6	30·44	30·42	30·08	30·06	30·30	30·24	29·82	71	46	67	59	71	53	61	E.	NW.	NE.	calm	0·89	·40	...	...	·020	...
○ 7	30·44	30·28	30·04	30·00	30·27	30·17	29·83	77	41	68	54	73	53	60	SE.	E.	SE.	NW.	...	...	...	...	...	...
8	30·28	30·16	29·94	29·90	30·14	30·05	29·65	81	44	69	56	69	55	61·5	SE.	SE.	S.	NE.	...	...	...	...	...	...
9	30·16	29·84	29·90	29·86	30·00	29·90	29·10	78	56	69	57	72	60	61	NE.	W.	S.	W.	...	·45	...	...	...	...
10	29·84	29·77	29·70	29·68	29·72	29·68	29·06	76	52	67	57	73	54	60·5	SW.	NW.	W.	W.	...	...	·14	0·150	·020	0·16
11	29·84	29·74	29·64	29·60	29·65	29·64	29·06	72	50	65	53	69	54	60	NW.	W.	W.	W.	·90	...	·48	0·230	·050	·16
12	30·04	29·84	29·84	29·74	29·90	29·72	29·15	67	46	64	54	69	51	54	NW.	NW.	NW.	NW.	...	·30	...	0·175	...	·10
13	30·08	29·85	29·85	29·70	29·95	29·86	29·40	68	49	65	54	73	61	58	NW.	W.	SW.	NW.	...	...	·21	...	·090	...
☾ 14	29·85	29·52	29·60	29·35	29·73	29·52	29·50	72	46	68	54	72	61	66	SW.	SW.	SW.	NW.	...	...	·04	0·805	·140	·23
15	29·58	29·52	29·30	29·28	29·43	29·38	28·77	74	54	65	54	71	57	65	SW.	W.	SW.	NW.	...	·25	...	0·350	1·085	·46
16	29·89	29·58	29·50	29·38	29·56	29·39	28·96	71	51	62	54	66	58	63·5	SE.	NW.	N.	E.	...	...	·20	0·435	·265	·07
17	30·13	29·89	29·70	29·62	29·87	29·70	29·27	80	54	62	53	68	55	66	NE.	N.	NE.	E.	...	...	·45	0·200	·335	·03
18	30·15	30·13	29·82	29·80	29·98	29·97	29·54	76	52	64	54	69	55	58	NE.	NE.	NE.	NE.	...	·25	...	0·235	...	1·08
19	30·18	30·15	29·84	29·84	29·99	29·95	29·55	73	50	66	55	69	56	58·5	NE.	NE.	NE.	calm	·94	...	...	...	...	...
20	30·19	30·18	29·90	29·90	30·04	30·01	29·55	72	50	63	55	70	55	55	NW.	N.	N.	NW.	...	...	...	...	...	·12
21	30·30	30·19	29·92	29·90	30·08	30·05	29·60	76	50	66	54	71	54	62	NE.	NE.	NE.	E.	...	·45	...	...	...	·14
● 22	30·46	30·30	30·04	30·00	30·22	30·15	29·72	67	52	64	56	65	54	58	NW.	NE.	NE.	NE.	...	...	...	0·080	...	...
23	30·46	30·38	30·22	30·20	30·34	30·33	29·85	74	52	65	51	71	55	58·5	NW.	NE.	N.	NW.	...	...	...	...	...	...
24	30·38	30·25	30·20	30·18	30·29	30·16	29·75	73	48	66	56	74	53	60	NW.	NE.	N.	N.	...	·40	...	...	...	...
25	30·30	30·25	30·10	30·10	30·20	30·17	29·64	64	47	61	54	66	50	54	NW.	NE.	N.	NW.	...	...	·07	...	...	·08
26	30·43	30·30	30·12	30·10	30·20	30·18	29·70	64	44	61	53	63	48	54	NW.	NE.	N.	NW.	...	...	·04	...	...	...
27	30·43	30·42	30·15	30·12	30·33	30·30	29·77	64	53	64	53	68	54	54·5	NNW.	NE.	N.	NW.	...	·35	...	...	...	...
28	30·53	30·43	30·23	30·22	30·33	30·31	29·71	72	41	64	55	68	52	59	NW.	NE.	N.	NW.	·90	...	...	...	...	...
☽ 29	30·53	30·42	30·26	30·20	30·41	30·37	29·87	72	47	65	54	68	51	53·5	NW.	NE.	N.	N.	...	...	...	...	...	...
30	30·44	30·34	30·26	30·20	30·33	30·20	29·70	65	42	65	53	67	53	60	NW.	NE.	NW.	NW.	...	...	...	...	...	...
31	30·50	30·44	30·14	30·14	30·32	30·26	29·76	73	41	66	56	67	53	56·5	NE.	NE.	NE.	NE.	·30	·45	·01	...	...	·11
Aver. :	30·53	29·52	30·26	29·28	30·41	29·38	29·48	89	41	69	51	77	48	60·2					3·93	3·85	1·99	2·865	2·060	2·74

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

NOVEMBER 1827.

LIII. *On the Figure of Equilibrium of a Homogeneous Planet in a Fluid State; in reply to the Observations of* M. Poisson, *published in this Journal for July last. By* J. Ivory, *Esq. M.A. F.R.S.*

[Concluded from p. 247.]

IT follows from what has already been proved, that a homogeneous planet entirely fluid cannot have a permanent figure unless all the level surfaces, or surfaces of equal pressure, be similar to one another and similarly posited about the centre of gravity. But there is no figure possessed of this property except the elliptical spheroid oblate at the poles and protuberant at the equator. Such, therefore, and no other, would be the figure of every planet, could we suppose it entirely fluid and homogeneous. But the consequences we have mentioned cannot be deduced from the single principle, that gravity is every where perpendicular to the surface of the planet. Another condition, no less essential to insure the equilibrium of the fluid mass, is expressed by the algebraic formula already employed, viz.

$$\Delta \, . \, V = C.$$

This formula contains two very remarkable properties of a homogeneous planet *in equilibrio:* First, the attraction of all the matter of a stratum contained between two level surfaces, upon all the interior particles, has no other effect than to cause an equable pressure over all the surface of the fluid upon which the stratum lies: Secondly, the attraction of the stratum upon any particle in the inside, is equal in all opposite directions. These two properties are necessarily connected, so that one cannot take place without the other. According to this theory, it is the foregoing formula, or the properties of

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which it is the expression, that determines the figure of a homogeneous planet *in equilibrio,* to be an elliptical spheroid exclusively of all other figures; and the equation of the outer surface ascertains the rotatory velocity requisite to maintain a given oblateness of the planet.

In the mode of investigation we have followed, every point has been strictly demonstrated from principles universally admitted, and to which no objection can be made. The conclusions at which we have arrived, rest on their own evidence, and are independent of what it is usual to lay down systematically respecting the equilibrium of a homogeneous fluid. We are under no necessity of entering upon any discussion of the received doctrine for the purpose of removing difficulties, or of seeking new arguments in favour of what has been rigorously proved: but it will be satisfactory, and it cannot but contribute to remove some erroneous notions prevalent upon a difficult subject, which has been treated by all authors in a manner too general and devoid of precision, if we show, by a careful examination, that the theory which has prevailed unquestioned from the time of Clairaut and Euler, affords no solid ground for the objections which have been urged against the solution we have proposed.

The equilibrium of a homogeneous planet in a fluid state, to which we confine our attention for the present, requires nothing more, according to the usual theory, than that the force of gravity be every where perpendicular to its surface. When this single condition has been deduced from the laws of hydrostatics, the problem, it is said, is brought within the dominion of the mathematics, and has no further dependence on physical considerations for its solution. But if we inquire into the grounds on which all this is asserted, we shall find that they are not altogether sure and satisfactory. No clear demonstration has ever been given that the equilibrium is a necessary consequence of the perpendicularity of gravity to the surface of the planet. This is no doubt always an essential condition; and, when the accelerating forces urging the particles of the fluid are explicitly given, it is sufficient alone for investigating all the circumstances of the equilibrium. But the mutual attraction of the particles introduces new forces, which vary with the figure assumed by the mass of fluid, and modify the solution of the problem. The hydrostatical theory as applied to a homogeneous planet is defective in the evidence of its fundamental principles; and it has remained steril and ineffective. The only application that has been made of the hydrostatical theory is when the planet *in equilibrio* is very little different

ferent from a sphere. If the centrifugal force be only a very small part of the attraction, which is true with respect to all planets, the problem is simplified extremely, as we are permitted to reject the square and other powers of the fraction expressing the proportion of the two forces. By taking advantage of this simplification Legendre proved, in the *Mém. de l'Acad. des Sciences* 1784, that the elliptical spheroid is the only figure deducible from the equation of the outer surface of a planet entirely fluid. The process of Legendre has been practically executed only as far as the first power of the centrifugal force: but it is shown that the calculation may be continued to any required degree of exactness; that there is only one series expressing the radius of the spheroid; and, consequently, only one figure which will answer the conditions of the problem, although the nature of that figure is not precisely ascertained. M. Poisson has lately remarked that the figure in question must be an elliptical spheroid, the radius being expanded in a series; because, when the centrifugal force is very small, it is known that the conditions of equilibrium can be fulfilled by an elliptical spheroid, and by one such figure only, which must therefore coincide with the single figure found by the other process. But there is some reason to demur with respect to M. Poisson's argument, inasmuch as the equilibrium is not equally sure in the two cases. In the elliptical spheroid, the equilibrium is fully ascertained, as all will allow; but it is not so certain in the other case, since it is inferred from the single condition of the perpendicularity of gravity to the surface of the planet.

In order to throw some light on Legendre's method of investigation, it is requisite to examine it more particularly. The attractive force at the surface of a sphere, of which the radius and the density are both unit, being $\frac{4\pi}{3}$, we shall put $\frac{4\pi}{3} \times \alpha$ for the centrifugal force at the distance 1 from the axis of rotation, and α will be the proportion of the latter force to the former. Let r be a radius of the spheroid drawn from the centre of gravity to a point in the surface of the fluid; θ the angle which r makes with the axis of rotation; and V the sum of all the molecules of the spheroid divided by their respective distances from the same point of the surface: then, the equation of the fluid's surface, which expresses the perpendicularity of gravity, will be,

$$V + \frac{\alpha}{2} r^2 \sin^2 \theta = C \quad \ldots\ldots\ldots \quad (1)$$

C being a constant quantity. If we suppose $\alpha = 0$; that is, if

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there be no centrifugal force, the equation will belong to a sphere; and the question is to determine what the figure will be, when α is a very small fraction. Let a denote the radius of a sphere equal in volume to the whole fluid mass, or the radius of the sphere which would be the figure of the fluid in the absence of all centrifugal force; then we may assume with Legendre, that the radius of the oblate spheroid is expressed by this formula,

$$r = a(1 + \alpha y) \quad \ldots\ldots\ldots\ldots (2)$$

y being a quantity depending upon the position of r, negative at the poles where the spheroid falls within the sphere, and positive at the equator where the spheroid rises above the sphere. It will be sufficient for our present purpose if we admit, in order to fix our ideas, that the spheroid is a figure of revolution; and then y will be a function of the arc θ alone. What we have now to do is to determine the expression of y by means of the equation (1); and we shall suppose that this problem is solved, and that we have found y in a series of terms proceeding by the powers of α. Conceive now that the whole sphere having its radius equal to a, is divided into innumerable concentric spheres; put a' for the radius of one of the interior spheres, and form this expression,

$$r'' = a'(1 + \alpha y).$$

It is manifest that the equation (1) will be equally true at the surfaces of the two spheroids of which r and r' are the radii; for y is the same in both these quantities, and a and a', in which consists their only difference, are entirely arbitrary. It follows, therefore, that the whole fluid sphere, and all the concentric spheres, are changed by the centrifugal force into spheroids similar to one another and similarly posited about the common centre. If any one of these spheroids taken separately from the rest be *in equilibrio*, they will all be *in equilibrio* independently of one another; for the equation (1), which is made the only condition of equilibrium, applies alike to all. It thus appears that the analytical assumptions in the process of Legendre bring us necessarily to the same conclusions already obtained in the first and second propositions. But there is no proof that any one of the spheroids is *in equilibrio.* On the contrary, we learn from the third proposition, that there can be no equilibrium unless a new condition be superadded to the equation of the fluid's surface. It is necessary that all the interior similar surfaces be surfaces of equal pressure, which cannot happen unless the fluid mass be an elliptical spheroid exclusively of all other figures.

It has now been shown, that the analytical process of Legendre

gendre is insufficient to prove the equilibrium of a homogeneous planet. The only question that remains to be considered, in order to enable us to form a correct judgement of his investigation, is to inquire whether the figure determined by the equations (1) and (2) be necessarily an elliptical spheroid. If this shall be found to be the case, it must be occasioned by the influence of some principle that has escaped notice.

A little reflection will soon convince us that the value of y deduced from the equations (1) and (2) can possibly be no other than the radius of an elliptical spheroid expanded in a series. This is in reality a mathematical consequence of the discovery of Maclaurin. An elliptical spheroid of any oblateness will be *in equilibrio*, as was proved by that geometer, when it revolves with a proper rotatory velocity. The same mass of fluid may, therefore, assume every variety of the form mentioned, from the sphere to the most extreme oblateness, and still be *in equilibrio* in every case, provided the rotation be adapted to the figure. Now the equation (1) is common to all the spheroids *in equilibrio*, expressing generally the relation between the oblateness and the centrifugal force. It follows, therefore, from the received principles of analysis, that if we set out from any one of the spheroids in question, we may thence find the whole suit of related figures by means of their common equation.

The investigation of Legendre falls exactly under the description just given. The calculation begins at the sphere, which is one of the figures that fulfill all the conditions of the equilibrium of a homogeneous fluid. It is no doubt an extreme case, the oblateness and centrifugal force being both evanescent; but, for this very reason, it is the most favourable point to set out from. In the sphere we have $\alpha = 0$; and if we suppose that α is a very small fraction, the square of which may be neglected, the resulting figure is found to be an elliptical spheroid. If we now push the calculation to include the powers of α, it is proved at least that the series for y has only one form, and consequently that there is only one solution of the problem. But we know, from what Maclaurin has proved, that, when α is very small, there is one elliptical spheroid, and only one, depending upon the quantities a and α, which will satisfy the equation (1); whence it follows that the series for y can be no other than the radius of the spheroid expanded in a series. We have here made use of M. Poisson's argument, but we have placed it on its proper foundation. It is not because there must be an equilibrium on account of the equation (1), that the two figures must coincide; but because

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it is impossible by means of that equation to deduce from the sphere any other figure beside the elliptical spheroid. If we set out from any other initial figure but the sphere, or, more generally, from any other figure but one of the elliptical spheroids into which the same mass of fluid may be moulded, it will be impossible to obtain any solution of the problem of a homogeneous planet by means of the equation (1) alone.

The *rationále* of the analytical process of Legendre is undoubtedly such as we have just explained. It is founded on the connection that necessarily subsists between all the elliptical spheroids with which the equilibrium of the same homogeneous fluid mass is possible; a connection which enables the analyst to deduce all the related figures from any one by means of their common equation (1). The success of the investigation depends upon a mathematical relation of the figures, and not upon the conditions of equilibrium which are inherent in the figures themselves. No just objection can, therefore, be urged against the theory we have laid down, from the process of Legendre, when the principles of it are rightly understood; besides, the result obtained by it is nowise inconsistent with the conclusions at which we have arrived. But the method of Legendre is very imperfect. It necessarily supposes that α is a small fraction; because α is contained within a certain limit in all the related spheroids. Although it is proved that the analytical operations are possible, yet it is found that they cannot be practically executed after the first step. It has no pretensions to be called a solution *à priori* deduced solely from the hydrostatical laws of equilibrium. In all these respects it is inferior to the view we have taken of the problem; which ascending to the first principles of the research, derives, by a general analysis, the absolute conditions which are necessary and sufficient to the solution; and by this means reduces the mathematical determination of the figure to the utmost simplicity of which it is capable.

On another occasion I will show that the equilibrium of a homogeneous mass of fluid furnishes the only key to the general solution of the problem of the figure of the planets in a fluid state, and supposed to consist of heterogeneous strata; a problem which has not yet been touched upon, at least with success, except by Clairaut, whose investigation includes only the first power of the oblateness.

Oct. 11, 1827. JAMES IVORY.

LIV. *On*

LIV. *On the Geological Features of the Eastern Coast of England; and concluding Remarks on* Mr. Robberds's *Hypothesis. By* R. C. Taylor, *Esq. F.G.S.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

CONCLUDING that the observations of Mr. Robberds had terminated in the preceding Number of the Phil. Mag., I annexed some comments thereon in my communication of last month. Without extending the discussion by the introduction of new matter, I will now, with your permission, limit myself to a few remarks necessarily suggested by Mr. Robberds's second paper.

By adopting in some instances a nomenclature which appears applicable to the case, I am certainly open to the charge of adhesion to the modern school of geology. Most English geologists have, for the sake of convenience, but without thereby pledging themselves to individual speculations, agreed on the principal terms by which to distinguish certain formations; and amongst others, I am well content to employ these terms in the sense in which they are generally applied.

But still, in the investigation into which he has entered, "Mr. Taylor has surrendered his judgement to authority," p. 276. Here I acknowledge no authority but that of my own personal observation. The ground was probably not untrod by geological observers; but their views, whatever they might be, were unrecorded*. Unshackled by precedent or by conflicting systems, I have demonstrated the general continuity and extension of the crag formation, beyond the possibility of doubt in my own mind, or, I think, in that of those who may follow up the investigation. I have not, "because there are beds of shells at Harwich and others at Cromer," decided from that circumstance merely, that these must be extremities of the same strata; but I have, step by step, with no little labour, during the last seventeen years, examined and identified nearly the whole intermediate area; and, in reviewing the data by which I have been governed, have no more distrust of the fact than of the prevalence of clay in Essex, or of sand on Bagshot-heath.

* In justice I ought to except the name of William Smith, the first to depict the strata with which the surface of this island is diversified, and the first to discover and apply that remarkable rule by which their identity is established.

But

But I have nowhere asserted that the exuviæ are uniformly continuous, or even that the beds which contain them are wholly so. On the contrary, those inferences are expressly guarded against, when remarking that a leading characteristic of the shells is, "that they are by no means diffused in equal numbers and proportions, but occur, at intervals, in groups and genera;" and that wherever the chalk or earlier formations rise above the general level of the crag, that deposit would then be absent,—circumstances which find a parallel in all the soft strata above the chalk, from the plastic clay upwards. It was further observed, that the upper marine deposits in some cases appear to surround the bases of chalk eminences, which then "present the appearances of tongues or promontories, protruding into the crag" district, and "account for the occasionally apparent absence of that formation," and for the sinuosities of its outline. This is also of common occurrence in the later deposits, whose boundaries are much, but not entirely, influenced by the configuration of their chalk basis.

That the organic remains, peculiar to certain strata, are sometimes unusually abundant and sometimes absent from their matrices for considerable intervals, we might appeal to the experience of every practical geologist, who has traced in detail any of our superior formations, such as the plastic and London clays, and indeed, some of the indurated strata, such as the lias, (lyers?) the oolites, and the calcareous sandstones. Yet the identity of these strata is in most cases satisfactorily established. So also with the crag beds, which may be frequently seen in the open face of a cliff or excavation, in parts abounding with fossil shells, in others devoid of them: sometimes suddenly ceasing; sometimes gradually thinning off; but the continuity of the matrix is still apparent and unquestionable. Perhaps there is no formation in the entire range of superior and supermedial strata, whose organic accompaniments are not unequally distributed, and at times wholly interrupted.

I am perfectly aware, from personal experience, how well this observation applies to the deposits in East Norfolk, and how often the sand and gravel beds are either without fossils, or contain only occasional indications of their existence. But enough has been seen to prove that the lower beds on which the diluvium and "the ante-human gravel" repose, may be classed with the highest known marine formations.

In most of the instances where the crag is exposed in the Norwich valley, the chalk hills rise higher than the shelly beds, and,

and, of course, the latter never penetrate into the older formation. Consequently, the extent of these beds, internally, would be governed by the distance at which the plane of the one is intersected by the slope of the other. The principal excavation is that at Whitlingham, which I understand has been worked from time immemorial, and the face of the chalk hill has been removed at least 100 yards. Yet I have collected, and now possess, from this perpendicular face in the beds overlying the chalk, numerous specimens of testacea. In some parts of this excavation, intermediate between the chalk and sand, was a bed, about a foot thick, of coarse angular flints, much resembling those which interpose almost uninterruptedly between the plastic clay and the chalk, and containing shells. The deposits above the chalk at this place have also produced remains of the elephant, mastodon, deer, whale, and small fish*.

Mr. R. ascribes the presence of extinct species of shells to their accidental admission; and with equal facility the animal remains, of which I enumerated four or five genera from a variety of localities (although I am somewhat humorously made to announce a solitary, but "prodigious" specimen,) are likewise fortuitously introduced from some "earlier" stratum.

From the observations I have been enabled to make, both in the Norwich beds and in distant parts of the formation, the animal remains are commonly blended with the mass, or in the detritus immediately overlying; by which circumstance we may conjecture that they were partly of contemporaneous and partly of subsequent origin, with the testacea.

The area occupied by the crag extends, with few interruptions, from the borders of Essex, along the eastern side of Suffolk, over an average width of about seven miles, contracting toward the north, and partially concealed there by diluvial clay. In this southern or Suffolk portion the testaceous exuviæ are astonishingly abundant, and are applied as a valuable manure. Further northward we distinctly resume the traces of this deposit: its beds then become thinner; its productions less abundant and more covered with debris; until, at the distance of 100 miles from its commencement, it wholly ceases, and the chalk attains a superior elevation. Its south-

* I have the authority of an excellent comparative anatomist for stating, that to my list of the animal remains discovered in the diluvial clay of the Waveney Valley, may be added the crocodile, the plesiosaurus, ichthyosaurus, squalus, and balistes.

west boundary, in both counties, is irregularly defined, amidst the diluvial clays and the interruptions of older formations.

Viewing all the circumstances, the general agreement of the organic remains, the evident connection of the main Suffolk portion with that on the Norfolk coast, and in various parts in the interior, I am unable to draw any other inference than that they are parts of one contemporaneous formation, from whence there appears no adequate reason for excepting the Norwich extremity.

We have seen that, while constantly retaining some characteristic features, this formation exhibits rapid and frequent fluctuations; that no neighbouring portions are precisely alike; that at every hundred yards it presents novelty of arrangement; in structure varying from fine sand to coarse gravel or ferruginous conglomerates; in thickness, from a few inches to fifty feet; in the presence or absence of fossil substances, and in the nature of those accompaniments. In the eastern valleys the proofs do certainly appear in sufficient abundance to establish their geological conformity with more distant portions of the district.

With this conviction, can I hesitate between admitting a system opposed to these facts, and apparently derived from the inspection of a few excavations near Norwich, or following the guidance of a judgement formed from an investigation extending over five hundred square miles?

With regard to the orthography of Heringfleet, I would only add that I find it Herlingfleet more than thirty times in various ancient records besides Domesday Book: and that many inland towns have their names compounded of Hering, Haring, and Herling: also that the word Fleet signifies a river or watercourse (which is its present use in the Norfolk marshes) as well as an æstuary.

A recent opportunity of examination has strengthened the opinion that the large bouldered fragments of primitive rocks on the coast are derived from the Norfolk diluvium. They are not unfrequently seen in pits or upon the surface many miles from the sea, particularly in the neighbourhood of North Walsham and Aylsham. The circumstance is referred to chiefly, because I am quoted, perhaps inadvertently, as conceiving these rounded masses to be drifted to our shores by currents.

Mr. Robberds's unabating adherence to his views of the marine exuviæ have placed him in a dilemma. In arranging his proofs of the former altitude of the sea, the most prominent, the

the most irresistible corroborative fact insisted upon, was the complete identity of these exuviæ with "those of the testaceous molluscæ of the German Ocean." This fact was too momentous to pass without investigation. The result is unfavourable to the hypothesis which hinges upon it, and then such an inquiry is deprecated as too minute;—the eye of Science is too microscopic, and the discrepancies it discovers are "of no account in determining the character and antiquity of any formation." Natural History is appealed to, as supplying the proofs of an alleged fact: but as the volume of Nature is not sealed, the page is again opened, the reading is extended, and we then perceive truths which conduct to far other conclusions, Let the testimony which has been summoned, be applied, not in part, but to its real extent: let the evidence be taken in its absolute amount, as the scrutiny of science rigidly demands. If that species of evidence be as estimable as practical observers have declared, the process of collecting it must not be despised. If we would draw inferences from the consideration of natural phænomena, we are not at liberty to refuse the salutary application of those tests by which, in the physical sciences, any theory must stand or fall.

I will not do such injustice to Mr. Robberds as to suppose (p. 252, 253) that he upholds the exploded doctrines against the extension of knowledge on account of its occasional misapplication;—the liberality of his mind is too widely known to give rise for a single moment to such a conclusion. I am quite disposed to agree with him, that investigations into subordinate detail have, in some cases, been pursued to an unnecessary and almost trifling minuteness, until the primary object of pursuit has been lost sight of, and that there may be in these instances a tendency to overrate the value of such details. We might go further, and assert that the progress of science and its legitimate objects are not facilitated by the prevailing disposition to create endless subdivisions, to perceive petty distinctions, and to bestow new names; by all which our elementary books are increased in bulk and crowded with interminable varieties and synonyms, through whose mazes the perplexed student is doomed to wander. These are inconveniences from which few branches of natural philosophy are exempt.

But let us not, on the other hand, underrate the aid which geology derives from the collateral sciences. The cultivation of any one department leads insensibly and unexpectedly to the development of another, and the truths which the sciences establish cannot with safety be rejected or defied. By the

 free

free communication and the ultimate concentration of these truths, we shall be conducted with slow, but with less erring steps, to the more obscure and sublime parts of the system.

LV. *Remarks on the Principle of Compound Interest. In reply to* F. R. S. *By* A Correspondent.

If the interest of a hundred pounds for a day be a penny, it will be 365 pence for a year, according to the principles of simple interest.

But in all questions respecting pecuniary affairs extending to many years, and in all transactions respecting annuities, it becomes necessary to adopt the principle of compound interest, which allows a repeated investment of the capital and interest either yearly or monthly, or at shorter periods, without any limit: although this principle can only be employed in commerce under particular restraints imposed by the laws of usury.

It is lawful, for instance, to receive $\frac{5}{365}$*l.* in common years, if not in leap years, each day that 100*l.* is in the hands of a borrower: but at the end of the year, it is only lawful to receive 5*l.* for the use of the 100*l.* without any interest on the interest.

"Dr. Price and his followers" appear to consider the two supposed transactions as regulated by the *same rate* of interest.

The sense in which Dr. Young has understood the "*same rate*" comprehends the supposition of the possibility of laying out the interest from day to day, to be improved at compound interest; by means of which the 5*l.* would receive an addition of about half a year's simple interest on itself, or about half a crown, at the end of the year: making a yearly interest of about 5*l.* 2*s.* 6*d.*; which he considers as more correctly the *same rate* with $\frac{5}{365}$*l.* a-day. If this is erroneous, Dr. Young is in the wrong.

It is allowable to use the word *same* in either sense, provided that the definition be borne in mind: but when the definition is forgotten, the confusion may lead to errors in practice. It has been asserted, for instance, that the value of a perpetual annuity payable yearly is exactly equal to that of the same annual sum supposed to be paid "momently" in equal portions: because this result is obtained by supposing the annual interest to be divided through the moments, according to the principle of simple and not of compound interest: and in this manner mathematicians, even of *deserved reputation*, have been able to convince themselves of the truth of a paradox a paradox so revolting to common sense. The fact is, that the obviously greater value of an annuity beginning immediately, and payable hourly or daily at the option of the receiver, is reduced, in their method of computation, by the virtual increase of the discount, to the bare value of an equal annuity of which the payments are all accumulated at the *ends* of the respective years; a change which certainly could not be a matter of indifference either to the payer or to the receiver, and neither of them "would have the least difficulty in determining which of the two Doctors he should prefer to follow," though their *determinations* might be somewhat at variance.

Dr. Young has not asserted that this difference is the same in *all* cases of annuities; although he has taken for granted that it is the same in annuities on lives as in perpetual annuities, because the present value of the remotest possible payments is in both cases evanescent.

The objector has certainly not understood the nature of the argument by which Dr. Young has attempted to prove the inaccuracy, or rather the impropriety, of Dr. Price's estimation of the *identity* of the rate of interest. The tenor of that argument is, that when Dr. Price supposes he is reckoning on two annuities at the same rate of interest, he is in fact employing different rates: for that the discount on 10 shillings, receivable at a certain moment, as a payment of the half-yearly annuity, *is* greater, *in the computation*, than the discount on 10 shillings receivable at the same moment as half of an annual payment; and therefore that the rate of interest cannot properly be called *the same* in the two cases. The objector makes Dr. Young assert that the discount *ought* to be greater when the payment is half-yearly; which is quite a different question, because this comparison relates to one half of the whole number of half-yearly payments only.

When the sense of an author is mistaken, it is easy to triumph over the supposed absurdity of his conclusions: and in *this manner* your correspondent has perfectly succeeded in exposing the errors of those who do not *understand* Dr. Price's solution! With the greater part of his last paragraph, however, I fully agree, though many might be inclined to oppose to him the high authority of Mr. Morgan, whose testimony, though somewhat vague, seems greatly calculated "to encourage the popular delusion of the improved healthiness and greater longevity of the people of this kingdom," which the objector seems so much to deplore.

Waterloo-place, 2d Oct. 1827. F. R. S. L.

LVI. *On*

LVI. *On the Polar Lights, or Aurora Borealis and Australis. By* Professor Hansteen*.

1. *Observations on* M. Arago's *Memoir on the Influence of distant Polar Lights on the Magnetic Needle.*

ALTHOUGH the observation of M. Arago—*that distant polar lights, even if they are not seen in a given place, exercise an evident influence on the direction of the magnetic needle there*,—is not entirely new, it is nevertheless of great interest with respect to the theory of the polar light, showing that this meteor is not, like rain, thunder, lightning, &c. the effect of a local agency in a small portion of the atmosphere, but rather a disturbance of the equilibrium in the whole magnetic system of our globe.

The experiment was made as early as the 5th of April, 1741, by Professor Celsius, in Upsal, and the instrument-maker, Graham, in London. Celsius found on that day the needle becoming restless at 2 o'clock P.M., so that at 5 o'clock it was 1° 40′ more west than it had been at 10 o'clock A.M. At 5 o'clock 18 minutes it had receded to the E. by 20′, and 6 minutes later it went again 18′ westward. From that time till half-past 8 o'clock next morning it went back to its usual position. In the evening an aurora borealis was seen. A few weeks previously Celsius had requested Mr. Graham in London, also to observe his needle on the same day, in order to ascertain whether the same irregular motions were observable in two places so far apart at the same time: on the same day, viz. Sunday, 5th of April, Mr. Graham observed in London such extraordinary and frequent irregularities in the needle as he had never seen before, sometimes even at intervals of from 2 to 3 minutes. But Mr. G. makes no mention of any aurora borealis. (See my Magnetism of the Earth, p. 413.)

The aurora borealis at Edinburgh not having been perceived in all those days when the magnetic needle showed such irregular motions in Paris, I searched my meteorological diary, in order to discover whether any traces of aurora borealis had been discovered in Christiania. On the 13th of March 1825, 10 o'clock 21′ P.M., the sky was overcast, so that no aurora could be seen. On the 30th and 31st the sky was perfectly clear, but there is no mention of aurora. It is nevertheless possible that weak traces may have been seen towards the North, which I did not observe, the windows near which

* From Schweigger's *Jahrbuch der Chemie*, Band xviii. p. 353.

my

my thermometers are suspended, and from which I observe the appearance of the sky, lying towards N.E. and S.W. The 21st of April the sky was overcast. At Stördalen, near Drontheim, where meteorological observations are made by Mr. Heyerdahl, the rector, the 30th and 31st of March were snowy, and the 1st of April, a cloudy sky; the 17th of August, at 10 P.M. we had rain at Christiania; at Vadso, near Wardöehusw, at 9 P.M., by Mr. Stockfleth, the rector,'s observation, the atmosphere was dull; the 21st at half-past 11, cloudy; the 25th, at 11 o'clock 40 minutes, at Christiania, clear, with aurora borealis, which was also observed by Mr. Prebend Herzberg, at Hardanger; the 29th at midnight, clear, a little cloudy near the horizon. But in the meteorological tables of July, August, and September, communicated during my absence by Mr. Holmboe, the lecturer, on a journey round the gulf of Bothnia, the observation occurs: "In the latter days of August, auroræ were seen on several evenings," which, according to what I have been told by Mr. Holmboe, had been unobserved by himself, but seen by others. The 10th, Mr. H. saw, at Christiania, a beautiful arch of aurora below *Ursa Major*, and therefore the same which had been seen a little before at Leith. The 7th of October in the evening, the sky was overcast at Christiania; the 3rd and 4th of November, it snowed there in the evening: at Bergen, however, an aurora was seen by Mr. Bohr. The 22nd, the sky at Christiania was clear, but no aurora is noticed.

Mr. Holmboe's observation relative to the many auroræ boreales seen in the latter part of August, makes it very probable that they appeared during the 21st, 22nd, and 23rd, of that month; which were the days when the irregularities of the needle were observed in Paris, without any auroræ having been noticed in Edinburgh: and we should consequently not be obliged to assume with M. Arago, "that there are other unknown causes which act on the motions of the magnetic needle."

The following observations, with respect to the above remarks of M. Arago on the spontaneous action of the aurora on the magnetic needle in very distant places, are rather interesting. From the 23rd till the 31st of August, I resided at Haparanda, near Torneo. In order to discover whether the usual variation in magnetic intensity which I found in Christiania, was also observable near the arctic circle, I observed from a window of the inn, the time of 300 horizontal oscillations with one of Dollond's cylinders, as follows.

Day.

Day.		Time of 300 Vibrations.	Thermometer in the room.	Weather.
24th Aug.	1h 55′ P.M.	882″,30	+11°,3	Dull.
	8 47 —	880 ,64	9 ,4	Dull.
25th —	8 10 A.M.	881 ,54	8 ,7	Clear.
	0 7 P.M.	882 ,39	9 ,7	Mixed.
	8 53 —	881 ,51	9 ,0	Clear.
26th —	7 58 A.M.	882 ,50	7 ,7	Clear.
	0 12 P.M.	882 ,76	8 ,0	Dull.
	8 22 —	888 ,19	7 ,95	Dull.
27th —	7 27 A.M.	881 ,47	7 ,3	Dull, strong N. wind.

The hours mentioned are after the chronometer giving the time of Christiania, to which we must add about one hour to make it equal to that of Torneo.—By a previous investigation on the influence of temperature on the time of oscillation of this cylinder, I have found that when the normal temperature to which all observations are reduced is fixed at $= t$, the temperature during the observation at $= t'$, the time of oscillation observed at $=$ T, the reduction of the time of oscillation to the normal temperature is $= -\text{T}\,.\,(t'-t)\,.\,0\cdot00047$ *.) If we reduce the above time of oscillation, after this formula, to the mean temperature $t = +\ 80^{\circ}\cdot 8$, we obtain the following time of 300 oscillations with Dollond's cylinder in a temperature of + 80°·8, at Haparanda, near Torneo.

	9 o'Clock A.M.	1 o'Clock P.M.	9¾ o'Clock P.M.
24th Aug.	...	881″·35	880″·39
25th —	881″·58	882 ·02	881 ·43
26th —	882 ·95	883 ·08	(887 ·83)
27th —	882 ·09	...	...
Mean =	882″·21	882″·15	880″·91

If the evening observation of the 26th of August is omitted, we see that in the mean of Torneo, the least intensity (least time of oscillation) takes place between 9 o'clock A.M. and 10 o'clock P.M., and the greatest intensity (shortest time of oscillation), in the evening, as at Christiania. The mean of the three times of observation is . . . . . . =881″·79

26th Aug. 9⅓ P.M. was found . . . . . . =887 ·83

Irregularity . . . . 6″·04

* This reduction is found different for different cylinders, and, as it appears, larger for those of smaller than those of greater diameter.

The

The sky not being clear, no trace of the aurora borealis could be seen in the evening, which was observed at Christiania and Hardanger*. It was just the time when M. Arago noticed an unusual irregularity in the motion of the magnetic needle. Thence we may conclude: 1. That the irregular variations in the *central direction* of the magnetic powers of the earth, which evince themselves by the wandering of the magnetic needle in a certain part on the surface of the earth, is also connected with a contemporaneous change in the magnitude of the power's intensity. 2. That these irregular changes show themselves at the same time in places at great distances from each other (the distance between Paris and Tornea is about 300 geographical miles), and probably extends from pole to pole. 3. That therefore they do not consist of any local action on the needle of the material substance emanating from the surface of the earth, which forms the aurora borealis; but are caused by a general disturbance of the whole globe: *i. e.* by a removal of the equilibrium in the whole system of magnetic powers, which is again a cause of the polar lights.

As it is probable that M. Arago will proceed in his daily observations of the deviation of the needle, I shall, in order to contribute something towards our knowledge respecting the connection of the aurora and magnetism, note for the future with more care than hitherto, even the smallest traces of aurora that may appear in Christiania; and I have also requested other naturalists in various places in Norway, to notice all similar phænomena that may come to their knowledge, and to send me a list of them every six or twelve months, by which the Edinburgh observations may be considerably improved.

* An irregularity exceeding 6″ I only observed once at Christiania, viz. the 24th of June 1820, when the same cylinder had 300 oscillations in the following times:

$8\frac{1}{2}$	A.M.	811″·81
$10\frac{1}{2}$		812 ·35
$4\frac{1}{2}$	P.M.	810 ·15
7		810 ·25
11		818 ·35
$11\frac{1}{4}$		815 ·96
12		811 ·68

Here we have from $4\frac{1}{2}$ to 7 P.M. an irregularity of 8″ 1, and in the following three quarters till midnight of 4″ 3. So near the summer solstice one may at Christiania, at midnight, read a book without a light, which renders it impossible to perceive any aurora.

2. *Additional Observations and Corrections to some Observations on Polar Lights and Polar Nebulæ.*

The hasty observations to which I now offer a supplement, were not intended for the public at large, but were added by way of notes to a notice of Capt. Scoresby's re-discovery of the east coast of Greenland, in the *Magazin for Naturvidenskaberne*, 1824. vol. i. p. 85, &c. But having been received in the Edinburgh Philosophical Journal, where they had become either disfigured or unintelligible by translation, and afterwards inserted in the *Jahrb. der Chem. und Phys.* acpanied by notes of Mr. Kæmtz, I feel myself bound to furnish the following corrections and additions.

1. The columns of light shooting up from the northern horizon towards the zenith are not connected, but consist of short parallel rays (or, as Dalton observes, cylinders of light), whose direction nearly coincides with that of the dipping-needle. For when these columns of light pass by the zenith, they seem to be broken off on that point, and form what is called the *crown*. Let NS (Plate III. fig. 1.) be an arch of the circumference of the earth (a part of the magnetic meridian through the point C), F*f*, E*e*, D*d*, &c. the parallel rays of light, forming the mass of the aurora. If the observer at C turns his eye in the direction of CF, CE, CD, a part of each column of light is covered by that lying nearest to it in front, and the whole mass of light from F to Z and from Z to D seems connected. But in the direction of CZ, where the line of vision is parallel to the columns of light, one only sees the transversal section of the column; and as these columns of light are at a considerable distance from each other, the blue sky appears through them. If the eye turns towards the west or east, the line of vision again ceases to be parallel with the rays, and there also the mass of light seems to be connected. It must therefore appear as if rays of light were rising from the whole horizon towards the magnetic zenith Z. In the magnetic meridian these rays or columns of light seem to be perpendicular towards the horizon; but towards the east and west they have a perceptible inclination towards the south, which I have always noticed with strong auroræ. We shall have the clearest form of such an aurora by placing a globe so that its axis shall not be perpendicular to the horizon, but deviate about 18° or 20° from the vertical line; the meridians then represent the apparent direction of the streaks of light, and the parallel circle in 80° latitude, or the pole itself, represents the crown (fig. 2.). Thus every observer will see the

the crown in his magnetic zenith—and it is therefore, not the same crown which is seen by different observers in different places—as little as two observers see the same rainbow. It is therefore incorrect to try to fix the perpendicular height of the crown above the surface of the earth from observations made in two places lying far apart. 2. When the *arch of polar light* rises so high as to reach the magnetic zenith, *this arch* seems also interrupted in this place; whence we may conclude that it also consists of short rays parallel to each other. 3. The rays often form themselves into a regular ring (DE, fig. 3.), *i. e.* they dart forth from a small zone of the surface of the earth, the centre of which lies somewhere north of Hudson's Bay. Thus the regular arches are formed. If BC is the horizon for the observer in C, then FCB is the elevation of the lowest edge of the arch, and ECB the highest. If the arch keeps for some time at rest in the same elevation, and another observer in the same magnetic meridian, but north or south of C, has also observed at the same time the elevation of the point F or E, the perpendicular height FG of the arch above the surface of the earth may be determined from it. Mairan, from a great number of such parallactic observations, found this height to be above 100 geographical miles. This must of course also refer to the rays, of which the whole remainder of the aurora consists. The substance of this light, therefore, does not receive its luminous property till it is far beyond the atmosphere. 4. Whilst the substance of the aurora runs through the atmosphere it is not luminous; for I have never known the aurora to have been seen before a mountain or any other elevated object. On the contrary, it seems to have the opposite effect of making the atmosphere opaque. For when the aurora shows itself, the sky is often within a few minutes overcast by an opaque veil, and as rapidly clears up again. The cause is probably this: that the substance of the aurora condenses the aqueous vapours existing in the air in a transparent state. Whence we may also explain the dark segment underneath the arch, and the *black rays of the aurora borealis*, which have been described by various older Northern observers, and which I have frequently noticed myself. The line of vision runs in the direction CF, or CA (fig. 1.) through a long region of the atmosphere which has become opaque by the rays of the substance of the aurora. Ff', Ec' having passed through, whence the whole space FCA below the aurora has a dark colour approaching to black. The nearer the place of the observer C is to the northern limit f' of the aurora, the less dark this obscure segment will

 be

be to him; and experience also teaches that in the north of Norway it has merely a grayish tint, and still further north is not seen at all. If the aurora goes far beyond the zenith of the observer towards the south, he may also there observe a dark segment DCB beneath it. If single rays IH (fig. 3.) rise between the arch FE and the place of the observer C, the latter sees the arch FE through the space of air KI leaning on the arch FE, which is darkened by the rays IH, and sees them move before the luminous back-ground like columns of smoke, and with the violence peculiar to the rays of the aurora. 5. If the observer in C (fig. 1.) sees the aurora reach beyond the magnetic zenith, he is surrounded on all sides by the substance of the polar light issuing from the earth: in this case, if the development is rapid, and he stands in the open field far from any extraneous sounds or noise, he will frequently hear a noise resembling the buzzing caused by the effervescence of a mixture of acid and alkali; but if the aurora does not reach his zenith, *i. e.* if he stands beyond the region from which the emanation takes place, and sees it low in the north or south, as in C (fig. 3.), he will not hear such a noise. It is therefore natural that the people in the North often hear a sound attending this phænomenon, whilst the Southern observer perceives nothing of it; the sound being so slight that it cannot be heard at a distance.

The properties of the polar lights mentioned above seem to be inexplicable, if we assume that it is produced by electric currents in the atmosphere. It seems indisputable that the direction of the rays of the aurora, like that of the *dipping-needle*, is determined by the attractive and repulsive powers of the terrestrial magnetism. The phænomenon of light seems to arise when the intensity of the terrestrial magnetism has risen to an unusual height, and this intensity seems to be considerably weakened during the development of the polar light. But we have not known hitherto any such elastic fluids in the magnet, by the union of which, phænomena of light appear, as in the two opposite electricities. It is therefore still to be discovered what kind of substance it is which seems at once to partake of the properties of electricity and magnetism.

In the *Magazin for Naturvidenskaberne*, vol. ii. pp. 98, 99, I have advanced the following hypothesis, as an attempt towards an explanation of the electro-magnetic phænomena. In the completed galvanic circuit, the conductor is traversed in an opposite direction by the opposite electricities. Every positive elementary particle strives to combine with a negative one;

thus

thus united in pairs they neutralize each other, and their *electric* power disappears. But in this neutral state they perhaps appear as elastic fluid *elementary magnets*, which so surround the surface of the polar wire that all north poles are turned on one side, and all south poles on another; and the axis of every elementary magnet is the tangent of the circular section of the conducting wire. Owing to the constantly aggregating quantity of electricity from both ends of the wire, and the expansive nature of electricity, these elementary magnets are forced out of the surface of the wire with a velocity equal perhaps to that of light itself. As long, therefore, as the circuit is uninterrupted, the wire is surrounded by a cylindrical atmosphere of neutralized molecules combined in pairs, each pair of which has a magnetic north pole and south pole, and a neutral point. Let ABCD (fig. 5.) represent a section of a conducting wire turning towards the zinc pole of a galvanic apparatus. The neutralized electric pairs of molecules NS flow from all points of the circle ABCD towards the direction of the radii ZE, ZF &c., (like the circular waves round a stone dropt into the water,) in a manner that on imagining oneself to be in the point Z, all the magnetic north poles N will lie to the left, and all the south poles S to the right. By this means an innumerable quantity of circular elastic fluid magnets is formed round the conducting wire, in which magnets every point may be considered as the neutral, every north pole being immediately touched by a south pole, which impedes its free action. One might obtain such a circular magnet without free poles by forming a connected steel ring, touching it at the same time in several points of the circumference with the south poles of different magnets, and then moving these poles round the ring from right to left. This steel ring would then have no perceptible poles; but if it were any where broken in two, the surface of the fracture on the left hand would appear as a free south pole, and that on the right hand as a north pole. In this manner it may be easily explained, why the intensity decreases in the ratio of the simple distances from the axis of the conductor: for if the radius ZE is double the size of Z*e*, the same quantity of electricity which first filled the circle *ef*, must afterwards fill the doubly large circle ESF, and consequently the intensity must decrease in the same proportion as the distance increases. Hence it may also be easily explained why the electro-magnetic action freely penetrates the conducting bodies as well as the non-conducting. For the un-neutralized electric molecules excite in every body instantaneously the opposite state, and are therefore

fore attached to the body; but those that are neutralized cannot do it, and have therefore a perfectly free passage. According to this hypothesis, magnetism would be nothing but neutralized electricity. It is therefore possible that the aurora consists of such neutralized pairs of molecules which here, as in the completed electric circuit, obey the laws of attraction and repulsion of the magnet. I present this as a simple hypothesis, and confess that there still remain various obscurities not easily to be solved. But it is not to be expected that in such an obscure and difficult subject the truth should be discovered in a first attempt.

Dr. Kæmtz, in p. 212, 213, quotes the descriptions of the southern lights by Cook and Forster, as seen by them during their circumnavigation of the south pole in 1772—1775. The astronomers Wales and Bayley, who accompanied Cook in this voyage, express themselves more decidedly on this point, viz. in the work published by the Board of Longitude, entitled, "Astronomical Observations, made in the Course of a Voyage towards the South Pole and round the World, in the Years 1772—1775: by Wm. Wales, F.R.S. and Wm. Bayley." London, 1779; from which I will quote the following:

"Meteorological observations on board the Adventure, by W. Bayley. (P. 209.) 25th Feb. 1773, Lat. 51° 41′ S., Long. 110° 30′·6, Gr. Decl. 20° 52½ W.—This evening we saw for the first time the southern light; it was so luminous that one could have read large print by it. (P. 218.) 26th Feb. 1774, Lat. 55° 25′ S., Long. 2° 28′ E., Declination 9° 25′ W.—Seen a southern light, but not very luminous."

Meteorological observations made on board the Resolution, by W. Wales.

P. 343, Tuesday, 16th Feb. 1773. "Mr. Pickersgill saw a southern light."

P. 343. Wednesday, 17th Feb. 1773, Lat. 57° 34′ S., Long. 83° 23′ E. Decl. 40° 40′ W. "About 1 o'clock in the morning, Mr. Clerke, who was on duty, told us, that the same phænomenon which had been seen the night before by Mr. Pickersgill, was to be seen in a very luminous state. I got up, and found that it was just the same phænomenon which we call aurora borealis in England. The natural state of the sky, except in the *south-eastern* quarter, and at an elevation of nearly 10° round the whole horizon, a white haze through which the stars of the third magnitude could be just distinguished. The horizon around was covered with thick clouds, from which many streams of a pale reddish light rose, shooting up towards the horizon. These streams did not possess the motion which they are sometimes seen to have in England, but

but were perfectly steady, except a slight tremulous motion which some of them had near the edges."

Friday, 19th Feb. 1773, Lat. 58° 49′ S., Long. 91° 2′ E., Decl. 41° 51′ W. "During the night the southern lights were sometimes very bright, and the colours much more variegated and lively than on Wednesday night; their motion too was greater, and the phænomenon upon the whole extremely beautiful."

Saturday, 20th Feb. 1773, Lat. 58° 55′ S., Long. 92° 45′ E. Decl. 40° 31′ W. "At 9 o'clock P.M. the southern light shot up very brilliantly on the *east point* of the horizon in a single steady pillar with a pale reddish light. Its direction was not exactly towards the zenith, but gradually declined towards the south, and at a greater height became so much weaker that it disappeared near S.E. at an elevation of 45°."

[This was probably the segment of an arch, the highest point of which would have fallen between east and south if the arch had been perfect.—*Hansteen.*]

"Saturday, 6th March 1773. Lat. 59° 56′ S. Long. 119° 1′ E. —Seen a southern light."

Sunday, 7th March 1773. Lat. 59° 44′ S. Long. 120° 18′ E. —"Seen a northern light."

"Monday, 15th March 1773. Lat. 58° 52′ S. Long. 142° 24′ E., Decl. 1° 42′ W. The southern lights were at times very brilliant and exceedingly beautiful, their colour being lively and their motion quick and remarkable."

"Thursday, 18th March, 1773, Lat. 56° 5′ S. Long. 150° 10′ Decl. 14° 44′ E. Soon after 9 o'clock P.M. it was very clear, and the southern lights were extremely brilliant and beautiful, and appeared of a semicircular or rainbow-like, form, *the two ends of which were nearly in the east and west points of the horizon.*"

[Thus in the highest point of the meridian. Let it at the same time be remarked that the declination was on the 15th March, 1° 42′ W.; the 16th, 0° 27′ E., and the 18th and following days between 13° and 14° E. Thus in this spot near Van Diemen's Land the magnetic meridian nearly coincides with the geographical. Compare with this, Tab. IV. and VI. of the atlas accompanying my work on Terrestrial Magnetism.—*Hansteen.*]

"This arch on being seen first went a considerable part north of the zenith, but rose by degrees, turning, as it were, on its diameter, and stopped, after having passed the zenith, just in the southern horizon. This light was at one time so bright that we could see our shadows on the deck."

[I think the different positions of this arch may be explained by fig. 4; where WAO represents the first position of the

the arch, and WBO, or *wbo*, the last. Here the semicircle WAO has turned round the diameter WO towards the position WBO.—*Hansteen.*]

P. 353. Tuesday, 15th March 1774. "It will not be improper to remark, that during our whole long trip of this year towards the south we never once saw a southern light: I must confess that I do not remember a single night which had been sufficiently clear."

[From the beginning of the year 1774 till the 16th March of the same year, the voyage in the South Sea was confined between 224° and 266° Long. Captain Cook penetrating during that time as far as 71° 10′. Nevertheless no southern lights were observed during those three months, partly because, as Mr. Wales observes, the weather was seldom bright enough; partly because at that period the nights in the southern hemisphere are light; partly perhaps also, because the development of light in the vicinity of the South American magnetic pole seems by far less strong than it is near the Australasian in the South Indian Ocean.—*Hansteen.*]

LVII. *Description of New Succulent Plants. By* A. H. HAWORTH, *Esq. F.L.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

HEREWITH you will receive my tenth Decade of new Succulent Plants, which you will probably find more interesting than any of those which have preceded: because it not only describes ten new species of the genus *Gasteria* (a branch of the Aloëan family), but gives a monographical sketch of the subject, and an enumeration of all the species and varieties of *Gasteria* hitherto known; together with a table elucidating the dichotomous distribution of the whole.

But in thus sketching so extensively, the writer has been under the necessity of regulating the locations and characters of a few of his articles from small incipient plants. These, however, he will amend if requisite, in future communications.

In the year 1732 the best delineator of plants (and even mosses) the world had then seen, Dillenius*, figured in his *Hortus Elthamensis* nine Aloëan plants, one of which is an *Agave;* and but one of the remaining eight belongs to the genus

* Of this great botanist Linnæus is recorded to have said,—" Nullius in Angliâ, qui genera curat vel intelligit, preterquam Dillenius." He was Professor of Botany at Oxford, and author of the *Historia Muscorum*, &c. &c.

genus *Gasteria.* This however was, and even still remains, an unrivalled figure of the *Gasteria carinata.*

In 1759, the well-known Dr. (afterwards Sir John) Hill, of "voluminous memory," published his *Hortus Kewensis* (the first public list of a Garden, now the richest in the known world), in which he gives but twelve species of his genus Aloe, one of which afterwards became the *Aletris uvaria*, and latterly, the type of the genus *Tritoma:* and one other, the *Agave virginica* of Authors. And of Sir John's ten remaining Aloes, only one (his *Aloe disticha*) belongs to the genus *Gasteria.*

Nevertheless it must not be forgotten, that although Sir John Hill enumerated but twelve species of Aloe; Miller, in the last edition of his Dictionary, had published the year before (1768), what he considered as 23 species of Aloe; of which however, one is the above-mentioned *Tritoma uvaria*, and another the aforesaid *Agave virginica.* And of the remaining twenty-one, three only belong to *Gasteria*, the subject of our present Decade; viz. No. 13, *Aloe linguiforme* (meliùs *linguiformis*); No. 20, *Aloe verrucosa;* and No. 21, the above-mentioned *Aloe carinata.*

Prior to that period, Linnæus had published, in 1764, the second edition of his *Species Plantarum*, enumerating in it eight species of Aloe only; and one of these was *Tritoma uvaria;* a remarkable proof this, of the inferiority of his judgement to that of Dillenius and Miller,—on those subjects only perhaps. And yet, like them, he had recently seen the then matchless Gardens of the Dutch. On another occasion he exclaimed, "Vidi equidem, vidi his oculis puerilibus olim, nec res fallit."

Aloe disticha is the only species which Linnæus gave in the year 1764, which belongs to the genus *Gasteria;* but it must not be concealed, that he distinctly separates it into three marked *varieties*, two of which form two sections in the present Essay; and the third, by the best editor of the subsequent edition of the same work of Linnæus of 1764, became Willdenow's genus *Rhipidodendron!*

Prior to this æra, however, it should be stated that Boerhaave (the greatest physician of the time, and of whom the writer's uncle, Dr. Hardy, was the last surviving pupil) had figured one species belonging to *Gasteria*, and described three others. And that Commeline and Tilli had figured two each; although Munting in 1680, in his History of Aloes, the *Aloïdarium*, had not given one.

In the year 1789 the first edition of Aiton's *Hortus Kewensis* appeared, in three volumes octavo,—a work which for its accuracy has been called Classical in this country. It gave us still but thirteen species of Aloe, and amongst these but four species belonging to the present genus *Gasteria;* viz. *verrucosa*, *carinata*, *maculata*, and *lingua;* and did not make a single section of the genus *Aloe* for them, nor even for the aforesaid genus *Rhipidodendron*.

Three years after this period, in 1792, (although I had long before collected Aloes,) I first beheld the rich Gardens of London and Kew; and published in 1794 all the *Mesembryanthema* then known, and also sixty species and many varieties of Aloes in *nominated sections*, in the 7th volume of the Transactions of the Linnæan Society in 1804; in a paper which was given to the Society three years before. The *Gasteriæ* were published in that paper in a separate section called CURVIFLORÆ, in number, seven species, and five named varieties; and *Rhipidodendron* was given in a distinct division called ANOMALÆ, along with the still anomalous *Aloe variegata*.

A little prior to this, Thunberg had published his Dissertations on Aloe, &c. which I have not to this day seen, and cannot profit by: but the present subject was not there much advanced, as appears by the second edition of Aiton's *Hortus Kewensis*, in five volumes octavo, between the years 1810 and 1813, adopting nearly all the writers' then published species and varieties of succulent plants.

The genus *Aloe* by Willdenow, in his edition of Linnæus's *Species Plantarum*, did not appear till 1799; but seventeen distinct species being given, and only four of the Gasterian species amongst them, with little improvement, and without one single section.

But in 1809, Willdenow in his *Enumeratio* (many Aloes having then been published in the *Botan. Magazine*, from my own collection) gives twenty-five species of Aloe, and in two *sections;* having still only three species belonging to *Gasteria*, and these not even in a distinct section. And it was not until 1811, in a work which I never saw, and which is cited *Willd. Bermerk*, that he divided *Aloe* into, I believe, three genera, viz. *Aloe*, *Apicra*, and *Rhipidodendron*.

Before this, however, in 1809, Duval, in his *Catalogue* of the Succulent Plants *in Horto Alençonio*, divided *Aloe* into three genera, with good characters; viz. *Aloe*, *Gasteria*, and *Haworthia;* the last of which Willdenow included in his genus *Apicra*. Duval's genus belongs to the species with a bilabiated flower, but he includes the *Aloe spiralis* of prior Authors, which has a regular flower; and which therefore I place in the genus *Apicra*. Of this last genus I have not seen

Willdenow's

Willdenow's generic characters. Duval in the above work gives six species of the genus *Gasteria**.

In 1812, in the *Synopsis Plantarum Succulentarum*, I gave twelve species of *Gasteria* and two varieties, dividing them into two sections.

In 1817, His Serene Highness the Prince de Salm Dyck published, as the division of *Aloe* called *Curvifloræ*, in his *Catalog. Raison.* nineteen species of Duval's genus *Gasteria*, with many varieties; and very kindly presented to me a copy of his work. Some of the varieties and even species, however, he afterwards relinquished in another work, which he also presented to me; viz. *Observationes Botanicæ in Horto Dykensi notatæ*, published in 1821, in which he gives without any sections (*et inter alia*) twenty species of "*Aloes Curvifloræ, Gasteriæ* Haw." and twelve varieties; with full descriptions of the whole in Latin.

And here rests our knowledge of the subject-matter of my tenth Decade of new Succulent Plants. Your readers will find hereunder the *Gasterian* genus more than doubled in its number of both species and varieties; and all arranged and described under nominated sections, upon the most improved principles of the dichotomous system; briefly, it is true,—but from living specimens, technically and scientifically, and with characteristics throughout,—from one whose experience and opportunities ought at least to improve, upon all that has been already published upon this subject; and who remains ever,

Very respectfully yours, &c.

Chelsea, Sept. 1827. A. H. Haworth.

Decas decima Plantarum novarum Succulentarum.

Classis et Ordo. Hexandria Monogynia.

Gasteria. *Calyx* petaloideus curvus obclavatus, basi staminifer. *Capsula* parùm costata. *Fruticulæ vix caulescentes foliis Aloïum; floribus* pendulis. *Duval. Plantæ Succulentæ in Horto Alençonio, A.D.* 1809. *p.* 6.

Obs.—*Herbæ* vel *suffrutices* africanæ succulentæ *foliis* linguiformibus ensiformibusve mucronatis guttatis maculatisve bifariis, vel obliquè spiraliter multifariis, et tunc ætate, semper inæquilateraliter plùs minùs triquetro-carinatis: *floribus* genitalibus inclusis: a medio ad basin ventricosis et plùs minùs coccineis, et apicem versus gracilioribus roseis, sive viridibus.

Hæcce plantæ, in sectiones dichotomas per tabulam, ut infrà, naturalissimè ac certissimè dividuntur.

* This work M. Duval obligingly sent to me though a stranger; and I soon after sent to him my *Synopsis Pl. Succ.*, yet do not know whether he ever received it.

GASTERIARUM Conspectus dichotomus.

- * Breviflorae
  - † Caulescentes
    1. *pulchra.*
    2. *maculata*, α, β.
    3. *picta*, n. s. $\alpha \perp \beta$
  - †† Acaules
    - † Albidiores
      4. *formosa*, n. s.
      5. *fasciata*, α, β.
      6. *retata*, n. s.
    - †† Viridiores
      7. *nigricans* $\alpha \perp \beta$.
      8. *crassifolia.*
      9. *brevifolia* $\alpha \perp \beta$.
- ** Longifloræ
  - † Bifariæ
    - † Læviores
      - † Retusifoliæ
        10. *obtusifolia.*
      - †† Acutiores
        - Gracilifloræ
          11. *mollis.*
        - Crassifloræ
          12. *subnigricans* $\alpha \perp \beta$.
          13. *disticha* $\alpha \perp \beta$.
          14. *conspurcata.*
          15. *angulata* $\alpha \perp \beta$.
          16. *sulcata.*
          17. *excavata* $\alpha \perp \beta$.
          18. *angustifolia* $\alpha \perp \beta$.
          19. *lævis.*
    - †† Verrucosæ
      20. *subverrucosa* $\alpha \perp \beta$.
      21. *verrucosa* $\alpha \perp \beta$.
      22. *intermedia* $\alpha \perp \beta - \gamma.\ \delta.$
  - †† Multifariæ.
    - † Asperæ
      23. *repens.*
      24. *parva*, n. s.
      25. *decipiens.*
      26. *carinata* $\alpha \perp \beta$.
      27. *strigata*, n. s.
      28. *læta*, n. s. $\alpha \perp \beta$.
      29. *subcarinata* $\alpha \perp \beta$.
    - †† Glabriores
      - † Brevifoliæ
        30. *undata*, n. s.
        31. *glabra* $\alpha \perp \beta$.
        32. *nitida* $\alpha \perp \beta$.
        33. *trigona* $\alpha \perp \beta$.
        34. *obtusa.*
      - †† Longifoliæ.
        35. *acinacifolia* $\alpha \perp \beta$.
        36. *nitens* $\alpha \perp \beta$.
        37. *venusta*, n. s.
        38. *pluripuncta*, n. s. $\alpha \perp \beta$.
        39. *ensifolia.*
        40. *candicans.*
        41. *linita*, n. s.
        42. *bicolor.*

* *Sectio* Breviflorae: floribus quàm in speciebus sequentibus plùs minùs brevioribus. Propagines nullæ.

† Caulescentes: *caudice* senecto sæpè sesquipedali; *foliis* spiraliter multifariis inæquilateraliter carinato-trigonis obliquis nitentibus lævibus, albo nigroque perlepidè marmoratis fasciatisve, *scapo* elato ramoso.

pulchra. G. (marbled narrow shrubby) foliis acutè ensiformi-
1. bus: pedunculo floribus subangustis sublongiore.

Gasteria pulchra. *Synops. Succ.* 86. Aloe maculata, v. pulchra. *Ait. Hort. Kew. ed.* 1 & 2.—Aloe maculata. *Bot. Mag.* 765. Aloe pulchra. *Salm. Al. Curvifl.* 43.—Aloe, &c. *Mill. Ic. ad Dict. t.* 292.

maculata. G. (short, marbled, shrubby) foliis angustè lingui-
2. formibus obliquè subundatis obtusis (cum mucrone): floribus grossis pedunculo longioribus.

G. maculata. *Synops. Succ.* 85. Aloe maculata v. obliqua. *Ait. Hort. Kew. ed.* 1 & 2. Aloe lingua *α Bot. Mag.* 979. Aloe obliqua. *Salm. Al. Curvifl.* 42.

Obs.—*Folia* breviora quàm in priore, latiora undato-obliquiora obtusioraque: *floribus* necnon valdè crassioribus.

β. *fallax:* candidior: foliis quasi ad oculum tuberculato-exasperantibus: sed vere tactu lævissimis.

In horto amici Dom. Hitchin.

picta. G. (marbled, shrubby) foliis ensiformibus planis ob-
3. tusis (cum mucrone) pedunculis flore grosso brevi-
* oribus.

Obs. Nova, quoque pulcherrima species, nuper è Capite Bonæ Spei, in locis natalibus, ubi invenit amicus Dom. Bowie.

Florebat in regio horto Kewensi cum nativis plantis præcedentium à Dom. Bowie etiam missis.

Obs.—*Caudex* adhuc brevis. *Folia* duplò longiora, quàm in priore (cui simillima), planiora minùs carinata, maculis albis minùs claris, *scapo* simplici (vix semper) longissimo.

Hæcce tres descriptiones à plantis nativis florentibus factæ sunt in regio horto Kewensi, Maio 1826.

†† Acaules:

†† Acaules: caule vix ullo: bifariæ.

+ Albidiores foliis magìs albo marmoratis quàm in sequentibus.

formosa. G. (beautiful marbled) foliis linguiformibus lævibus
4. integris obtusis cum acumine albo nigroque fasciatis.
* *Obs.* Nova species à Dom. Hitchin nomine *Al. marmorata.*

fasciata. G. (short bifarious, marbled) foliis distichis brevibus
5. crassis biconvexis obtusis integerrimis, albo perlepidè fasciatis.

Aloe nigricans γ fasciata. *Salm. Al. Curv. p.* 64.

Obs. Communicavit illustr. Princeps de Salm Dyck. Bona ac pulchra species: *foliis* arctè imbricatis brevioribus et obtusioribus quàm in præcedentibus. *Flores* ferè omnium brevissimi et crassiores ut in G. *nigricante*, sed pallidiores, atque (apud nos) spicâ simplici bipedali.

β. *laxa*, foliis laxè imbricantibus.

In horto amici Dom. Hitchin. Varietas singularis.

retata. G. (chequering leaved) foliis subensiformibus, albo
6. maculatis, sobolium perlepidè albo reticulantibus.
* Communicavit amicus Dom. Hitchin. Nuper à Capite Bonæ Spei. Parvas plantas solùm vidi, cum unico folio adultiore, turgido et lateraliter obtusè subcarinato, saturatè viridi, albo submaculato, acuminato. Sobolium folia omnia variè albo reticulantia. Post priorem fortè locanda?

++ Viridiores, foliis distichis viridioribus obtusis maculis longè obscurioribus.

nigricans. G. (dark distichous) foliis linguiformibus brevibus
7. obtusis nigro-viridibus, obsoletè maculato-punctatis; marginibus cartilagineis integris.

Gasteria nigricans. *Synops. Succ.* 86. (exclus. synom. *Bot. Mag.* 1322, quod ad G. *excavatam* pertinet; quod vide.) Aloe lingua, v. crassifolia. *Ait. Hort. Kew. ed.* 1 & 2.—Al. lingua β. *Bot. Mag. t.* 838.

Obs.—Scapus paniculatus: *flores* crassissimi et ferè brevissimi, et confertissimi.

Aloe nigricans α latifolia. *Salm. Al. Curvifl.* 63.

β. *marmorata*: foliis brevioribus angustioribus, maculis obsoletis marmoratis confluentibus, apice obtusis vel obliquè acutis integris. Aloe nigricans δ marmorata. *Salm. Al. Curvifl.* 64.

Obs.

Obs. Folia duo viventia, à Principe de Salm-Dyck, solùm vidi. Fortè propria species.

crassifolia. G. (short, thick-leaved) foliis angustè linguiformi-
8. bus brevissimis, deorsum subcylindraceis, obsoletissimè marmorato-punctatis.

Aloe nigricans β crassifolia. *Salm. Al. Curvifl.* 64.—*Cat. Rais.* 16.

Obs. A prioribus longè differt, in foliis brevioribus, saturatioribus; atque ab omnibus distinguitur foliis crassissimis. *Flores* non vidi.

brevifolia. G. (short-leaved) foliis parabolico-linguiformibus
9. brevissimis, punctis pallidis: floribus parùm abbreviatis.

Gasteria brevifolia. *Synops. Succ.* 84.

Obs. Propagines ferè omninò nullæ, sed variat, α, *lætè-virens;* foliis pallidè viridibus scapo solitario. β, *perviridis;* foliis saturatè viridibus, sæpiùs scapis duobus.

** Longifloræ, floribus longioribus quàm in prioribus.

† Bifariæ, foliis bifariis.

+ Læviores, foliis magìs lævibus.

++ Retusifoliæ, foliis retusis, propaginibus nullis.

obtusifolia. G. (short, broad, retuse) foliis brevibus brevissi-
10. misve, latis lorato-linguiformibus, parvipunctis perviridibus.

Aloe obtusifolia. *Salm. Al. Curvifl.* 62.

Obs. Nomen *retusifoliæ* aptius fuisset: divisionis nomen itaque suprà proposui.

++ ++ Acutiores: foliis acutioribus.

└ Graciliflore, floribus omnium cognitarum gracilioribus: propaginibus paucissimis.

mollis. G. (acute soft, slender-flowered) foliis linguiformibus
11. acutis brevibus crassis mollibus sordidè viridibus submaculatis, apicem cuspidatam versus serrulatis.

Gasteria mollis. *Nob. in Suppl. Pl. Succ.* 47. & 203.

└└ Crassifloræ, floribus medio valdè incrassatis, sive magìs ventricosis.

subnigricans. G. (acute dark-leaved) foliis linguiformibus acu-
12. tiusculis

tiusculis parcè subtuberculato-punctatis. Aloe pseudo-nigricans. *Salm. Cat. Rais.* 17. et *Al. Curvifl.* 67.

Obs. Nomen hybridum emendavi. *Folia* nitidiuscula, a basi latiora glabra: medio tuberculata: apice cartilagineo. Propagines ferè nullæ. *Salm. l. c.*

β. *glabrior:* foliis latioribus obtusioribus glabrioribus saturatioribus clariúsque maculatioribus: necnon mihi adhuc brevioribus.

disticha. G. (blunt, serrate, great spotted) foliis linguiformi-
13. bus perviridibus glabris obtusis, sparsè puncto-maculatis, lateribus acutis supernè tuberculato-denticulatis.

Aloë disticha α *Linn. Sp. Pl.* i. 459. Aloe lingua. *Salm. Cat. Rais. D'Al.* 17. et *Al. Curvifl. p.* 60: nec *Thunberg.* vel *Nob.* Neque *Aloe linguiforme* (rectiùs *linguiformis*). *Mill. Dict. ed.* 8. seu *Linn. Fil. in Suppl. Pl.*—nec *Ait. Hort. Kew. Nobis*ve. Gasteria denticulata. *Nob. in Suppl. Pl. p.* 50. Aloe Afric. &c. maculis ab utraque parte albicantibus. *Com. Hort. Amst.* 2. *t.* 8.

α. *minor;* ut suprà.

β. *major:* foliis latioribus crassioribus obtusioribus, &c. Ambas varietates accepi à Principe de Salm Dyck, A.D. 1819.

Obs.—Var. γ *of Revis. Pl. Succ. p.* 204, yet exists at Kew, but in a bad state, and seems to be nearer to *Gasteria nitida*, where I shall now place it, rather than under the present article.

The specific name of *disticha* ought, by right of priority, to remain with the present species, although appropriate to many others; and notwithstanding Linnæus blended with it, as marked varieties, the *Gasteria carinata* (so well figured by Dillenius); and even the old *Aloe plicatilis* figured by Commeline. Sad testimonies these, of the low state of that great man's knowledge in *this* department of Botany. Nor did Willdenow in his celebrated edition of the *Species Plantarum* explain the old *Aloe plicatilis* much better; this fine arborescent plant being merely his variety α, (instead of β with Linnæus,) and at the very same time also his own *Aloe plicatilis;* adopted doubtless from Aiton's *Hortus Kewensis*, although he afterwards erected it into his own genus *Rhipidodendron;* retaining still the specific name of *distichum:* thereby restoring the ancient specific name of Linnæus; but not the old generic one of the plant; which according to his own account was

Kumara

Kumara disticha of *p.* 7. *t.* 4. of *Medic. Theodora*; a work unknown to the writer, and a name which, in right of priority, he ought to have retained. Willdenow joined to the above plant, as a second species of *Rhipidodendron*, the celebrated Quiver-tree of the Caffrarians; the *Aloe dichotoma* of Aiton's *Hortus Kewensis*, so well represented and figured in Patterson's *Travels in Caffraria*, in four separate tables, &c.

It must be further observed, that our present plant (the *Al. disticha* α of Linn.) does not appear to be the *Aloe linguiforme* (meliùs *linguiformis*) of Miller, or *A. lingua* of *Ait. Hort. Kew.*—*my own* publications—or those of *other* English writers: nor do I even suppose the plant itself was in any English collection before the year 1819, when I introduced it as above, from the Prince de Salm Dyck, as his *Aloe lingua.*

And it should moreover be remarked, that the *Aloe disticha* of *Mill. Dict. ed.* 8. *No.* 5, is the *Aloe perfoliata*, var. *saponaria* of *Ait. Hort. Kew. ed.* 1.—And that Miller's *Aloe linguiforme* (meliùs *linguiformis*), *Dict. ed.* 8. *No.* 13, is our common Tongue-aloe, the *Gasteria excavata* of the present Decade: and that Miller's variety of it, mentioned by him as *much more spotted*, is in all probability the *Gasteria angulata* of this Decade; because the plant cited by Miller as figured by Commeline in *t.* 8 of his 2d vol. of *Hort. Amst.* (as above mentioned) could scarcely belong to the plant he described; as it was not, I presume, then in England. For, had that been the case, being a very free and durable plant, it would have remained, which does not appear to have been the case. The only *Gasteriæ* I can trace as having been in England before the year 1819, are the twelve species of the *Synopsis Plantarum Succulentarum.*

Neither can we close this long digression without finally stating, that the subject of the present article (the original *Aloe disticha* α of Linn.) is not the *Aloe lingua* of Thunb., a plant whose stem is six feet high; nor, consequently, the *Aloe linguifolia* of *Linn. Fil.* (both of which synonyms are very clearly no other than the above and often-mentioned *Rhipidodendron distichum* of Willdenow).

conspurcata. G. (dense small spotted) foliis latè linguiformi-
14. bus obtusis perviridibus numerosissimè pallido-punctulatis; lateribus subtuberculato-denticulatis truncatis.

Aloe conspurcata, *Salm. Cat. Rais. d'Al.* 18.—et *Al. Curvifl. p.* 58.

Obs. Communicavit Princeps de Salm Dyck. Ab omnibus facilè distinguitur foliorum maculis confluentibus numerosissimis, obscurioribus.

angulata. G. (large, bar-spotted) foliis latè linguiformibus
15. longis pallidè viridibus albo fasciatim maculatis, lateribus truncatis.

Aloe angulata. *Salm. Cat. Rais. d'Al.* 17.—*Al. Curvifl.* 56.—*Willd.* in opere quod non vidi. Aloe lingua, longifolia. *Nob. in Linn. Tr. v.* 7. *p.* 17.—Al. lingua β. *Bot. Mag.* 1322. *f.* 3. ex nostrâ plantâ sed mala. Aloe linguiforme, var. *much more spotted. Mill. Dict. ed.* 8. *No.* 13.

Gasteria longifolia. *Synops. Pl. Succ.* 89.

β. *var.* α Dom. Hitchin, adhuc minor.

Obs. My name of *longifolia* ceases to be retainable for this plant, from later discoveries of far longer-leaved species: that of *angulata* is therefore adopted; and in consequence of it, or for similar reasons, my old names of the two subsequent species have also been changed by the Prince de Salm Dyck, as follows:

sulcata. G. (angle-sided, furrowed) foliis latè linguiformibus
16. subretusis angulato-truncatis lætè viridibus pallidè maculatis, sulcis longitudinalibus.

Gasteria angulata. *Synops. Pl. Succ. p.* 88.

Aloe obscura var. truncata. *Salm. Cat. Rais. d'Al.* 15.—Aloe sulcata. *Salm. Al. Curvifl. p.* 54.—Aloe lingua β. *Bot. Mag.* 1322. *f.* 5. ex nostra planta.

excavata. G. (common, acute, hollow-leaved) foliis linguiformi-
17. bus acutis obscurè albido-conspurcatis, basi excavatis, lateribus truncatis.

Gasteria latifolia. *Synops. Pl. Succ.* 87.

Aloe excavata et Al. obscura, *Willd. Bemerk.* (secundùm Salm.) opus quod non vidi.—*Salm. Al. Curvifl. p.* 54. Aloe linguiforme (rectiùs linguiformis). *Mill. Dict. ed.* 8. *No.* 13, (*exclus. Commelini synon.*)—nec *Linn. Suppl. Pl.* —Aloe disticha δ. *Linn. Sp. Pl.* i. 459.—Aloe africana foliis planis lateribus conjugatis carinatis, floribus rubris *Mill. Icon. Dict. t.* 19 : icon optima.—Al. lingua β. *Bot. Mag.* 1322. f. 4.

Page and plate 1322 of *Bot. Mag.* include all the distichous *Gasteriæ* (save *G. verrucosa*) then known in England; and the figures were all from my own plants, although

although I have mistakenly said in *Synops. Succ. Pl.* page 87, that figure 4 belonged to *G. nigricans.*

β. foliis obliquè spiraliter multifariis. *Synop. Pl. Succ. l. c.*—Varietas modo crescendi propaginum foliis longum per tempus distichis.—Vix mera ætatis varietas.

angustifolia. G. (narrow dull-spotted) sobolifer: foliis incur-
18. vulis linguiformibus unisulcatis biconvexis obtusis atroviridibus, maculis parvis obscuris subglabris numerosissimis.

Gasteria angustifolia. *Synops. Pl. Succ.* 88.

Aloe ling. angustifolia. *Nob. in Linn. Tr. v.* 7. *p.* 13.—Aloe conspurcata, *var.* unilateralis. *Salm. Cat. Rais. d'Al.* 18.—Aloe angustifolia. *Salm. Al. Curvifl.* 57.—Aloe lingua *β. Bot. Mag.* 1322. *f.* 2. ex nostrâ plantâ.

β. foliis longioribus.

Obs. In ambabus varietatibus propagines valdè frequentes.

lævis. G. (marbling, small-dotted, smooth) simplex: foliis in-
19. curvulis linguiformibus turgidis obtusis sordidè virentibus, maculis parvis obscuris marmoreo-dilutis lævissimis.

Aloe lævis. *Salm. Cat. Rais. d'Al.* 16.—Aloe angustifolia, *var.* lævis. *Salm. Al. Curvifl.* 58.

Obs. Propagines rarissimæ. Salm. *in loco.* Communicavit (cum sequente) Princeps de Salm Dyck.

Flores non vidi.

++ Verrucosæ foliis distichis paginis plùs minùs exasperatim verrucosis.

subverrucosa. G. (roughish white-spotted) subsimplex: foliis
20. linguiformibus suprà planiusculis obtusis perviridibus, albo serialiter tuberculato-maculatis.

Aloe subverrucosa. *Salm. Cat. Rais. d'Al.* 18.—*Al. Curvifl.* 67.

α. grandipunctata: foliis tuberculis latis confluentibus fasciatìm pictis. *Salm. l. c.*

β. parvipunctata: foliis longioribus latioribus maculis minutis, apice paucioribus ac distinctis, et ad basin, præcipuè subtùs creberrimis confluentibus. An propria species? *Salm. l. c.*

Obs. Parvas plantas hujus speciei solùm vidi. An rectè locanda? An *Gasteriæ distichæ*, suprà, affinior?

verrucosa. G. (Pearl-tongue) sobolifer, asperrima: foliis flex-
21.

nosis

uosis ensiformibus, inflexo-concavis atro-viridibus, creberrimè margaritaceo-tuberculatis.

Gasteria verrucosa. *Synops. Succ.* 91.

Aloe verrucosa. *Salm. Cat. Rais. d'Al.* 19. et *Al. Curvifl.* 71.—*Bot. Mag. t.* 837.

β. *latifolia:* foliis robustioribus latioribus et brevioribus. *Salm. l. c.*

Obs. Hæc species etiam variat casualiter: foliis longitudinaliter luteo-striatis. *Salm. l. c.*

intermedia. G. (rough intermediate) foliis angustè linguiformibus attenuato-obtusiusculis lætè viridibus albo creberrimè tuberculatis scabris, apice cartilagineo asperrimis.
22.

Gasteria intermedia. *Synops. Succ.* 89.

Aloe intermedia, α, β, γ. *Salm. Cat. Rais. d'Al.* 19 (*synonymis* De Cand., Comm., et Till. *exclusis.*)—Aloeintermedia. *Salm. Al. Curvifl.* 69.—Aloe lingua. *Bot. Mag. t.* 1322. *f.* 1. ex nostrâ plantâ; *cum synonymis pessimis.*

β. *asperrima:* foliis perviridibus suprà planiusculis asperioribus paulò brevioribus et latioribus, tuberculis majoribus; propaginibus paucioribus, sed satis frequentibus. An propria species? *Salm. l. c.*

γ. *lævior:* tuberculis paucioribus.

δ. *longior:* foliis longioribus acutioribus.

†† Multifariæ: (many-fared) foliis per ætatem et rectè spiraliter multifariis acutis cuspidatis inæquilateraliter trigonis acutéque carinantibus.

+ Asperæ, foliis plùs minùs tuberculatim exasperatis.

repens. G. (small-pale rough) præsobolifer: foliis verrucosis brevibus inæquali-trigono-ensiformibus inflexo-cavis lætè viridibus denticulatis.
23.

Gasteria repens. *Nob. in Revis. Pl. Succ.* 48.

Obs. Communicavit amicus Dom. Hitchin. Quasi hybrida inter *G. intermediam* et *G. carinatam*, at longè minor, verrucis innumeris albicantibus, præcipuè sparsis, rariùs subserialibus. *Flores* non vidi: sed apud Kew. florebat.

parva. G. (dwarf, rough) sobolifer: foliis brevissimis maximè inæquali-trigonis ensiformibus tuberculato-asperiusculis.
24.
*

Obs. Apud Dom. Hitchin solùm vidi. Omnium adhuc cognitarum minima. Patriam nescio neque flores: neque an rectè locanda.

decipiens.

decipiens. G. (the dark deceiving) simplex: foliis erecto-ex-
25. pansis, ovato-acuminatis inflexo-concavis supernè ca-
* rinatis; nigro-viridibus, granulis confluentibus nigris: antiquis lævescentibus.

Haworthia nigricans. *Nob. in Phil. Mag. Oct.* 1824.

Obs. This very singular plant was published as above, on its recent arrival from Africa, and its native leaves described; which have altered since amazingly: and to our extreme surprise the plant produced last autumn the strong tall flower-stem and bracteæ of a common *Gasteria*, and soon after evolved the proper flowers of one, with more coloured tips than usual! Wherefore, its first specific name being occupied, I have called it *decipiens.*

Obs. A.D. 1827, August. The plant has now three flower-stems of moderate height, in full and very high-coloured bloom: its leaves are much larger, and become nearly smooth, and more polished than formerly.

carinata. G. (keeled, sparsed rough) sobolifer: foliis rectis
26. inæqualiter trigonis, tuberculis sparsis scaberrimis albicantibus.

Gasteria carinata. *Synops. Pl. Succ.* 87.

Aloe disticha β. *Linn. Spec. Pl.* 459.—Aloe africana, &c. *Dill.Elth.* 18.*fig. optima.*—Aloe carinata. *Aiton, Willd., Salm,* &c. *Bot. Mag.* 1331, figurâ dextrâ exceptâ quæ ad *Gasteriam glabram α* pertinet.

β. foliis pallidioribus pulchrioribus. *Nob. in Revis. Pl. Succ.* 203.

strigata. G. (keeled strigate rough) sobolifer: foliis rectis
27. inæquali-trigonis, tuberculis magnis albis subconfluen-
*. tibus sæpè strigatis.

Obs. Ex Africâ australiore in locis natalibus invenit amicus Dom. Bowie, misitque ad regium hortum Kewensem circiter annum 1824.

Obs. G. carinatæ simillima, at major, foliis undique magis fasciatim, quàm strigatim sparsimve maculatis, seu verrucosis: verrucis paucioribus albidioribus magis distantibus et majoribus: necnon in paginâ superiore lævioribus, seu minùs exstantibus quàm in inferiore. Foliorum strigæ apicem versus præcipuè.

lætipuncta. G. (neat light-spotted) foliis rectis inæqualiter tri-
28. gonis, tuberculis magnis numerosis præalbis pulcher-
* rimè sparsis.

Obs. In Africâ australiore in spontaneis locis invenit, misitque

misitque ad regium hortum Kewensem, amicus Dom. Bowie, anno 1821.

Obs. G. carinatæ simillima, at fortè minor, foliis adhuc angustioribus, tuberculis majoribus seu clarioribus concinnioribus magisque albis, vix in fascias, potiùsque sparsis.

β. Gasteria denticulata γ. *Nob. in Revis. Succ.* 204.

Obs. The variety β, from bad health is yet small, but it may, perhaps, ultimately prove a variety of this, or a new and *smoother* species; rather than a variety of the very blunt leaved *Gasteria denticulata* (*disticha*, above). The leaves are now acute, roughish, and very neatly spotted; and rough-edged towards their points, with one side thicker than the other, and rougher and blunter tubercled there than elsewhere; indicating, perhaps, a change to the trigonous form.

subcarinata. G. (slight keeled roughish) sobolifer: foliis in-
29. æqualissimè trigonis rectis, punctis albicantibus sparsis asperis, suprà planiusculis.

Gasteria subcarinata. *Nob. Suppl. Pl. Succ.* 49.

Aloe subcarinata. *Salm. Cat. Rais. d'Al.* 14.—et *Al. Curvifl. p.* 51.—Aloe pseudo-angulata. *Salm. Cat. Rais. d'Al.* 16.

β. foliis viridi saturatioribus asperioribus: subindè casualiter albo eleganter striatis. *Salm.* in locis suprà citatis.

++ Glabriores foliis semper glabratis, ætate multifariis et inæquilateri-carinatim trigonis.

Breviorifoliæ, foliis floribusque brevioribus quàm in sequenti subsectione cavo-inflexis.

undata. G. (oblique wave-leaved) nitescens: foliis inæqualis-
30. simè trigonis rectis undato-obliquis atro-viridibus gla-
* bris, albido sparsim multiguttatis; marginibus tuberculato-asperis.

Obs. Nova species. Communicavit amicus Dom. Hitchin. *Flores* non vidi. *Folia* multifaria et inæquali carinatim trigona. *G. subcarinatæ* affinis, at distincta, foliis nitentioribus. Forsan hybrida species.

glabra. G. (large thick recurving) simplex: sublævis: foliis
31. patenti-recurvulis perviridibus præcarinatis, albo valdè sparsè punctatis: marginibus tuberculatis. *Synops. Pl. Succ.* 87.

Aloe glabra. *Salm. Cat. Rais. d'Al.* 13.—*Al. Curvifl.* 49.

Aloe carinata, *Bot. Mag.* 1331. *figurâ sinistrâ folii* (ex nostrâ

nostrâ plantâ) *solùm:* et cum verbis sequentibus solùm, ("*nunc in meras maculas subsidentibus.*") p. 1331.
β. *minor:* foliis brevioribus. *Salm. l. c.*

nitida. G. (short, polished upright) simplex: foliis subæquilateri-trigonis rectis cavis lævissimè lucidis, albo-maculatis.
32.
Aloe nitida. *Salm. Cat. Rais. d'Al.* 13.—et *Al. Curvifl.* 47.
α, *parvipunctata:* foliis saturatè viridibus seu viridissimis, maculis parvis sparsis distantibus: floribus miniatis. *Salm. l. c.* Variat floribus pallidioribus.
β. *grandipunctata:* foliis perviridibus, sed quàm α pallidioribus, maculis duplò majoribus serialibus. *Salm. l. c.*
Obs.—*Flores,* ni fallor, longè pallidiores quàm in α. —An propria species?

trigona. G. (acute smooth upright) sobolifer: foliis inæquali-trigonis attenuatis obliquè carinatis lætè viridibus; maculis permultis albis fasciatis; marginibus tuberculatis.
33.
Aloe trigona α elongata. *Salm.*: *Al. Curvifl.* 45.—Aloe acinacifolia γ angustifolia; et δ lætè virens. *Salm. Cat. Rais. d'Al.* 13.
Obs. Folia subcava nitida, in junioribus glabra, senecta pedalia; propagines valdè frequentes. *Salm. l. c.*
β. *minor:* secundùm Dom. Hitchin, et in ejus horto.

obtusa. G. (obtuser smooth) simplex: foliis inæquilateri-trigonis obliquè carinatis excavatis viridibus, maculis multis albis fasciatis; marginibus tuberculatis apiceque subobtusatis.
34.
Aloe trigona β obtusa. *Salm. Al. Curvifl.* 46.—Aloe nitida γ brevifolia. *Salm. Cat. Rais. d'Al.* 23, excluso synonymo. *Suppl. Pl. Succ.* 48.
Obs. Folia suprà valdè excavata, et in junioribus rugosa, 6—9-pollicaria, ad basin tripollicaria latitudine. Propagines verò nullæ. *Salm. l. c.*

++ ++ LONGIFOLIÆ: foliis adultis longioribus, uti etiam floribus ferè semper longissimis.

acinacifolia. G. (great barred, scimitar-leaved) foliis inæqualitriquetris acinaciformibus longissimis nitidis lætè viridibus; maculis pallidis fascialibus, marginibus interruptè cartilagineis.
35.
Aloe acinacifolia α et β. *Salm. Cat. Rais. d'Al.* 12.—et *Al. Curvifl.* 44.

α. *major:*

α. *major:* (omnium Gasteriarum) foliis plusquam bipedalibus: floribus sesquiuncialibus: propaginibus nullis.

β. *minor:* propaginibus frequentibus. In horto Dom. Hitchin.

nitens. G. (sparse-spotted scimitar) foliis subacinaciformibus
36. lætè viridibus crassissimis longissimis maculis pallidis sparsis conjunctisve marmorescentibus.

α. foliis erectioribus ut suprà, subindè sesquipedalibus, corollis sesquiuncialibus.

Gasteria nitens. *Nob. Suppl. Pl. Succ.* 48.—Gasteria acinacifolia β. *Nob. Revis. Pl. Succ.* 48.

β. foliis brevioribus patentioribus, magìs maculatis, floribus minùs elongatis. *Nob. Revis. Pl. Succ.* 205.

Obs. A G. *acinacifoliâ* distinguitur punctis maculisque sparsis, nec fascialibus. *Flores* (Junio) ferè ut in G. *candicante*, apice apertiores, pedunculis longis debiliter flaccidè dependentibus ob ponderosos perigonios.

Neque varietas est *Gasteriæ trigonæ* suprà, ut putavit illustriss. Princeps de Salm Dyck, in *Al. Curvifl.* *p.* 46: quæ longissimè minor, longéque alia.

venusta. G. (clear sparse-spotted scimitar) foliis ensiformibus
37. longis nitentibus, maculis numerosis asperiusculis albis, marginibus valdè cartilagineis, obtusè interruptis.

Obs. In regio horto Kewensi vidi pulchrè crescentem. *Folia* adhuc juvenilia pedalia bifaria et sine carinis; sed vix semper. Etiam undique asperiuscula, ut in juvenilibus plantis G. *acinacifoliæ*, à quo dignoscitur foliis minoribus, macrioribus (adhuc) maculis longè clarioribus distinctioribus, albis, et pulchrioribus.

pluripuncta. G. (sharp, many-spotted) foliis ensiformibus acu-
38. minatis viridibus, albo creberrimè tuberculatim exas-
* perantibus.

Obs. Exemplaria nativa incompleta, nuper à Capite Bonæ Spei, apud Dom. Hitchin, solùm vidi, foliis adhuc asperiusculis; sed propria species.

β. foliis adhuc, magìs maculatis.

ensifolia. G. (long, sword-leaved) foliis longissimis inæquali-
39. ter carinato-trigonis s. ensiformibus, confluentèr multiguttatis.

Gasteria ensifolia. *Nob. in Phil. Mag. Oct.* 1825. *p.* 282.

Obs. Hæcce nobilis *Gasteriæ* species nunc (Jan. 1826) gerit

gerit folium magnum centrale inæqualiter altè carinato-trigonum, et mox, sine dubio, ut in omnibus carinatis, multifariam evadat speciem.

Obs. In regio horto Kewensi viget, ex Africa australiore missa à peregrinatore acuto et in succulentis plantis detectore facile optimo, amico J. Bowie.—*G. acinacifoliæ* fortassè nimis affinis. *Flores* ut in eâ.

candicans. G. (stout, sugar-marbled-white) foliis longis inæ-
40. quali-carinato-trigonis, subacinaciformibus glabriusculis, maculis albis maximè confluentibus.

Gasteria candicans. *Nob. Revis. Pl. Succ.* 46, et 205.

Obs. Folia multifaria et pedalia vidi sæpeque albo quasi saccharata. *Flores* (Junio) pallidiores, magìs penduli (ob pedunculos debiles) longiores, minúsque curvati quàm in aliis sectionibus, subbiunciales, grossi, apice rectiusculi seu minùs patuli, pallido albove marginati. *Bracteæ* pedunculos æquantes.

linita. G. (smear-painted) foliis linguiformibus subobtusis
41. mucronatis, sordidè albo fuscoque marmoratis: mar-
* ginibus latè cartilagineis.

Communicavit amicus Dom. Hitchin.

Obs. In Capite Bonæ Spei invenit D. Bowie, misitque ad regium hortum Kewensem. *Folia* adhuc disticha non pedalia, atro-viridia, albo sordidè marmorata maculatave, subtusque sæpè quasi pigmento albo linita, marginibus interrupto asperis, apice lævioribus. In ætate, fortassè carinata et tunc multifaria (*folia*) fuerint affinium more. Precedenti affinis, at valdè distincta. *Flores* non vidi.

bicolor. G. (half-marbled, lightest green) foliis angustè lingui-
42. formibus obtusis biconvexis lævissimis pallidis, imis subtùs maculato-marmorescentibus.

Gasteria bicolor. *Nob. in Phil. Mag.* 1826.

Obs. In Africa australi invenit in locis naturalibus, necnon misit ad regium hortum Kensensem amicus Dominus Bowie. *Folia* adhuc pedalia erecta disticha sine carinis; at fortè mox carinata atque multifaria evadant, ut in affinibus. Species insignis nondum floruit. Dec. 1826.

Obs.—Flores formosi uniformes visi sunt in omnibus adultis *Gasteriis;* æstatis tempore; copiosè in quolibet anno.

P.S. Fortassè per totam *Decadem* nimis laconicè scripsi. Sed nunquam oblitum est, verbositas ruinam scientiæ alit.

LVIII. *Meteorological Register kept at Funchal, in Madeira, in the Year* 1826; *with some prefatory Observations on the Climate of that Island, &c.* *By* C. HEINEKEN, *M.D.**

FUNCHAL forms the area to a natural amphitheatre on the south side of the island of Madeira: it is open to the sea towards the south, and shut in on the north by mountains rising to the height of 5000 feet, and gradually declining east and west.

The following Journal is, I am aware, imperfect; but when I plead continued ill-health on the one hand, and renounce all scientific pretensions on the other, I trust that it will be received with every allowance. Mr. Kirwan's observations on the climate of Madeira, I have never seen. Those published by Dr. Gourlay, twenty-five years ago, were made by the late Mr. James Murdoch, at a place called the *Valle*, about 400 feet above the level of the sea, and do not therefore apply to the city of Funchal: besides, they are given only in monthly maxima, minima, and means; and we are left totally in the dark as to the *mode* by which these different results were obtained. Mr. Bowdich in his "History of Madeira," did not pretend to enter into this subject in detail; and I am not conscious that any other person has attempted it. These, and the intermediate situation of Madeira, which makes it an important connecting link between the tropical and northern climates, are the reasons which have induced me to obtrude myself upon the public, as one who has incurred the sarcasm of the amiable Johnson, by "registering the changes of the wind, in order to die fully convinced that the wind is changeable!"

The periods of the day at which the observations have been made, are not those which I should have selected, had a free choice been offered me; but all others were precluded, either by the state of my health, or the nature of my avocations. I selected therefore the least eligible, because they were likely to prove the least interrupted.—As it regards the barometer, the results have not, I think, been greatly influenced by this circumstance; its oscillations during the four-and-twenty hours being comparatively, and upon an average, so trifling: but the deductions from the hygrometer are I fear of but little value; one observation of this instrument during the day is obviously inadequate, and I have not therefore attempted to form a mean from it. I regret this the more, because it is a very interesting, and here at least nearly novel, subject of investiga-

* Communicated by the Author.

tion;

tion; and because the instrument challenges and deserves every attention—an observation after sun-set was for at least nine months in twelve incompatible with health, and one at noon or a few hours later would necessarily have been so often interrupted, that it would have been an incumbrance rather than otherwise. During many of our heavy rains it (the hygrometer) has shown several degrees of dryness, but this has invariably occurred with the wind at N. or N.W.; at S. or S.W. it has seldom reached a degree before deposition has taken place; and this apparent discrepancy is, I think, fully accounted for, by supposing that as the former (the N. and N.W. rains) originate upon, and come to us from, the mountains; we are drenched by rains which *belong* (if I may be allowed the term) to those regions, our own *immediate atmosphere* remaining for a time at least *below the point of saturation.* Throughout the year 1826 the sirocco visited us seldom, and then was generally either incomplete in its character or partial in its influence; but in a former year the hygrometer during its prevalence once showed 45 degrees of dryness, and even then æther failed to produce a deposition. I take for granted that it is very well established; although I am ignorant of the explanation, why the sirocco here is so perfectly dry, and that of the Mediterranean so loaded with moisture. It reaches us immediately from the coast of Africa, after passing over about 300 miles of sea: not a cloud is to be seen during its continuance; the whole atmosphere is of one uniform unvaried blue, of a peculiar character, as though viewed through what a painter would term a very thin warm aërial haze; it blows from the E.S.E., *lasts almost invariably three days*, and encounters you like the puffs from the mouth of an oven or furnace; the eyes and lips feel much as they do when exposed to a keen easterly wind on a frosty day in a northern climate: birds and insects seem to suffer from it more or less, and fowls confined in a close yard generally droop. Furniture warps and cracks; books gape as they do when exposed to a fire, and it is generally inconvenient and oppressive; but I have never heard (although no precautions are taken to avoid it by the labouring classes) that any ill effects have been produced by it. Some have asserted that it has raised the thermometer as high as 130° in the sun, and 95° in the shade; but I doubt the accuracy of the observations: for in the course of four years I have never seen it raised above 85° in *perfect shade.* There is, however, nothing in which observers are so likely to differ as in their results of the *maximum heat in the shade* in this place. I do not believe that it is to be *accurately obtained*, but by a *series of observations*,

tions, made upon *several instruments*, in *various situations;* for the sun is so vertical that *no one thermometer permanently fixed* in *one given place*, *can* remain during the four-and-twenty hours uninfluenced by either its *direct* or *reflected* rays:—few persons *would*, and *none* I believe hitherto *have*, attempted such a troublesome mode of accuracy; and as I only profess to give the indications of a single self-registering instrument, I have, after many trials in a variety of situations, adopted that which appears to be the least objectionable; namely, an unoccupied room, having another over it (for if only covered by the roof it will be palpably influenced by the sun), and the door and windows of which are constantly open. As to the probable average of rain which one year with another falls at the level of Funchal, I incline to differ from Mr. Bowdich. He estimates it at 40 inches, and it is true that 43·35 inches have this year fallen; but so much rain has not before been remembered by the oldest inhabitant, and last year the amount was only 20·43 inches, making an average of 31·89. 30 inches I should therefore consider much nearer to the truth. The autumnal rains commence generally towards the end of September, and terminate in December; they have more the character of violent and intermitting showers, than incessant daily rains. The winter rains set in, and prevail more or less throughout January and February, and are far more decided, and tropical. March and April are showery and windy. May fine, with a passing shower; and in June, July, August, and part of September, we seldom have a drop of rain. At the level of the city no perceptible dew is produced; but up the mountains it is profuse, and all meteorological observations are here circumscribed to a degree which is unknown in an extensive and tolerably plain country. Tables for Funchal belong to *that locality only*, and *cannot* in any way be made to suit the *island generally;* and until a *series* of observations shall be made at *different heights* and in *different aspects*, it is *mere deception* to apply *any deductions* to *Madeira as a whole*. At the moment that I am writing the sun is shining, and in these lower regions the day is lovely; sea-ward the atmosphere is cloudless, and there is more need of a parasol than an umbrella: but I have only to look out from a north window, to see it raining in torrents upon the summits of the mountains.

For the hourly observations of the barometer on the 4th and 5th of December I am indebted to Mr. Wilkinson, a gentleman who to accuracy and scientific knowledge adds a most enviable share of industry and perseverance. I am fully sensible that standing by themselves they are of little comparative

rative value, but I hope ere long to obtain others from the same source. The symprisometer I obtained accidentally, and in time only for the December observations: but as it is to me a new instrument, I know nothing of its merits, and give the results merely because I happened to note them at the time as a matter of inquiry. It has hitherto agreed very nearly on an average with what I believe to be an accurate barometer, and differed but slightly (after allowing for corrections) in each observation; and it certainly possesses an advantage in correcting itself for temperature. During three years the highest at which the barometer has ever stood is 30·62;—in Gourlay's tables 31· is mentioned: but at 400 feet above the sea (the height of Mr. Murdoch's instrument), I will venture to assert that such a phænomenon never occurred even a quarter of a century ago. $28\frac{1}{2}$ inches are also given by him as the lowest; but when I observe in the same tables 26·9 (an evident misprint), I cannot but be charitable enough to give him the benefit of a compositor's blunder, especially when during the period before mentioned (three years) 29·39 is the lowest point to which the mercury has ever descended, and when it was *then* so *notoriously low* as to be the theme of general remark with several who paid attention to the matter. Generally the barometer is highest with a N.E. wind, and lowest with a S.W.; the great predominance of the first will, perhaps, appear extraordinary; but they are in fact the skirtings of the trade-winds. I do not pretend to great accuracy as to the precise points of the compass from which the winds have blown; upon a mountainous speck in the ocean such as this, the only method of judging is to look to sea with a glass; for all indicators on shore, or in the bay, serve only to deceive. During the greater part of the year we have a sea and land breeze morning and evening; and with this exception it is rare that the wind changes more than once during the four-and-twenty hours; for weeks, sometimes almost for months without intermission, it will come from the N., N.E., and E., and then we have our finest weather; to the S. of E. we look for a sirocco, and if it lingers at the S. or passes to the W. of it, heavy warm rain invariably follows, and continues as long as it remains in that quarter; to the N. of W. it becomes more violent, and always attended with cold, squally, severe (for this climate) weather; and in the winter with snow upon the summits of the mountains, and most of the little thunder and lightning which occur here—reaching the E. of N. it becomes again fine; and the sailor's mode of judging of the weather is here peculiarly applicable. I never remember a week's week's sunshine when the wind had passed from the W. to the N.E. by the S. No snow has yet appeared upon the mountains (December 31st), but the winter has notwithstanding been severe. More thunder and lightning has occurred than usual, for we have very little; indeed, a gold-leaf electrometer (not however in a favourable situation) has seldom been at all affected, and then only very slightly.

The quantity of rain from the eastward was so unusual a circumstance as to excite the observation and astonishment of every one. As a general law, to which but few exceptions will arise, it may be said that our rains are periodical, and that they come from the westward of N. and S.; and that to the eastward of those points of the compass it is all fine open weather. I have perhaps been needlessly precise in the description of the instruments used, and their positions; but as I felt that my name could give no weight to their results, I was unwilling to deprive them of the benefit which could accrue from a certificate of the mode in which they were conducted.

In the course of the summer I ascended Pico Ruivo, the highest point of the island, and by Newman's iron cistern barometer and Daniell's hygrometer made the height 6069 feet above the level of the sea; See Note (*a*). Bowdich gives it as 6164 feet; but then he states Mr. Veitch's turret, where his lower instrument hung, to be 154 feet in height, whereas it appears to me to be only 97; See Note (*b*). Admitting this to be the case we agree within 38 feet; no great matter of difference in such a height and under such opposite circumstances; for our instruments were different, and our data taken from contrary sides of the island: it should in candour also be stated, that my observations were conducted with one barometer only; and should his height of the turret appear to have been erroneous, the error I am presuaded did not originate with him; for when already embarked for Africa, he wrote me a note which has been destroyed, and the particulars of which I cannot now remember, requesting me to desire his publisher to make some alteration in the height of *that identical turret.* What this alteration was I cannot recall to mind; but I have no doubt that the publisher neglected to make the correction, and that the error is thus satisfactorily accounted for.

Funchal, Madeira, Dec. 31, 1826. C. Heineken, M.D.

Note

Note (*a*).

St. George's Beach, 10 A.M. . . . . Bar. 30·230 Temp. of Mercury 75°.
Cistern 4 feet above sea . . . . 114 deducted for ditto . . . . =30·116 Log. 47879
Mr. Welsh's house, St. Anne's, 11 A.M. . . . . 29·171 Temp. of Mercury 69°.
094 deducted for ditto . . . . =29·077 46355

Approx. height in fathoms 1524
6
Ditto ditto in feet . . . 9144

Temp. of air at lower station 75° Dew point 67°
Ditto upper 68° Ditto 65°
2)143
71·5 mean

Expansion of air at 71·5 = 0822
1,0000
0822
Sp. Gr. corrected for temp. 9178

Expansion of air for vapour at 67° . . . . = 0233
Increase of density for ditto . . . . = 0137
0096

Expans. of air for vapour at 65° 0219
Increase of density ditto 0129
0090

0096
0090
2)0186
0093 mean

And 9178—0093=9085 correct Sp. Gr.

	Sp. Gr. of air.		Stand.		Ap. ht.		Cor. ht.
Then	9085	:	1,0000	::	914	:	1006

4 ht. of cist. above sea.
Feet 1010 Mr. W.'s house.

Mr. Welsh's house 6 A.M. . . . . . Bar. 29·102 Temp. of Merc. 67°
094 deducted for ditto
29·008 Log. 46252
Pico Ruivo 20 feet fr. summit ½ p. 10 A.M. 24·290 Temp. of Merc. 60°
063 deducted for ditto
24·227 Log. 38442
7810
6
Approx. ht. 4686,0

Lower temp. of air 65° Dew point 57°
Upper ditto . . . . 60° Ditto 56°
2)125
62·5 mean.

Expansion of air at 62·5 =0635
1,0000
0635
9365 Sp. Gr. corrected for temp.

Expansion of air for vapour at 57°=0169
Increase of density for ditto . . . 0102
0067

Expansion of air for vapour at 56° 1064
Increase of density for ditto . . . 0099
0065

0067
0065
2)0132
0066 mean.

And 9365—0066=9299 correct Sp. Gr.

	Sp. Gr.		Stand.		Ap. ht.		Cor. ht.
Then	9299	:	1,0000	::	4686	:	5039

20 below the summit.
1010 Mr. W.'s house.
Correct height of the Peak 6069 feet.

Note (*b*).

Funchal Beach Bar. 30·173 Temp. of Merc. 74°
Cistern 8 feet above sea 111 deducted for do.
30·062 Log. 47802

Mr. Veitch's turret . 30·080 Temp. of Merc. 74°
111 deducted for do.
29·969 Log. 47667

135
6
App. height 81,0 ft.

Expansion of air for vapour at 69° 0248
Increase of density for ditto 0146
0102

And 9136 − 0104 = 9032 correct Sp. Gr.

	Sp. Gr.		Stand.		Ap. ht.		Cor. ht.	
Then	9032	:	1,0000	: :	81	:	89	corrected height
							8	height of cistern.
Mr. Veitch's turret							97	feet.

Lower temp. of air 73° Dew point 69°
Upper ditto . . . . 74 Ditto . . . 70
2)147
73·5 mean.

Expansion of air at 73·5 = 0864

1,0000
0864
9136 Sp. Gr. of air corrected for temp.

Expansion of air for vapour at 70° 0257
Increase of density for ditto 0150
0107

0102
0107
—— = 2)0209
0104

N. B. As I am indebted to Mr. Daniell's tables for these corrections, I have given them almost in his words. He will I trust excuse the plagiarism, for I could substitute none more appropriate. The symprisometer, as nearly as I can ascertain with an instrument which is new to me, gives the height about 100 feet. Upon the beach is a pillar of 103 *measured feet* from the level of the sea. The eye carried from the parapet of Mr. Veitch's turret to the horizon, cuts this pillar five courses of stones below its summit; and allowing to each course one foot, this also will bring the height of the turret below 100 feet. I think, therefore, that 97 feet may fairly be considered its real measurement.

JANUARY 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	30·23	62				30·15	63	64	53		W.	Fine
2	30·20	62				30·13	63	64	55		NW.	Showers
3	30·02	63				29·80	65	67	58		W.	Showers [on m^ns
4	29·70	63				29·63	62	66	50	1·30	NW.	H^y r^n with snow
5	29·70	61				29·80	61	67	52		NW.	Showers
6	29·87	60				29·79	60	63	53	·41	NW.	Rain [melt^d
7	29·83	60				29·90	61	65	53		N.	Cl^y snow n^y
8	30·10	60				30·10	61	69	52		N.	Fine
9	30·10	60				30·04	61	67	55		W.	Small rain
10	30·11	61				30·13	64	66	54	·06	W.	Fine
11	30·10	62				30·05	64	67	57		W.	Slight showers
12	30·12	65				30·12	65	67	54	·05	W.	Slight showers
13	30·14	63				30·18	62	67	55		W.	Overcast
14	30·23	62				30·25	64	67	52		NE.	Fine
15	30·24	62				30·23	63	68	57		E.	Fine
16	30·20	62						66	57		SE.	Fine [storm
17	30·00	63				29·85	63	65	58	·10	SE.	Overc. P.M. thun^r
18	29·85	63				29·94	63	64	58	·17	SE.	Showers
19	30·00	63				30·11	63	67	60		SE.	Overcast
20	30·15	62	62	50	12	30·10	62	68	57		SE.	Overcast
21	30·08	63	62	53	9	30·06	62	64	55		E.	Overcast
22	30·07	61	62	52	10	30·12	62	66	53		E.	Overcast
23	30·20	61	61	52	9	30·20	62	67	52		NE.	Fine
24	30·23	61	61	46	15	30·21	61	66	52		NE.	Fine
25	30·19	61	61	43	18	30·03	62	64	51		W.	Do. P.M. shower
26	29·88	61	60	53	7	29·91	60	63	54	·34	NW.	Showers, snow
27	29·98	61	61	51	10	30·01	60	66	52		NW.	Fine [on mount^s
28	30·11	61	60	46	14	30·09	61	64	53		NW.	Fine
29	30·07	60	60	55	5	30·00	61	60	53	·63	NW.	Do. P.M. rain
30	30·15	60	60	52	8	30·20	60	66	53		NW.	Sh^rs much snow
31	29·94	59	60	57	3			64	53	2·26	W.	Heavy rain

Pressure. Corr^d for Expans.
Max. 30·25 at 64° =30·165
Min. 29·630 62 =29·547
Mean 30·049 62 =29·977

Temperature.
Max. .... 69°
Min. 50
Mean .... 59·9

Dew Point.
Max. 57° Min. 43°

Dryness.
Max. 18 Min. 3

Rain 5·32 inches.

Winds.

N.	N.E.	E.	S.E.	W.	N.W.	
2	3	3	5	9	9	=31.

Remarks.—A severe winter month, with much more snow than usual; on 31st the Hygrometer showed 3° of dryness during the heaviest and most universal rain that has occurred for some time.

FEBRUARY 1826.

	10 o'Clock A.M.					10 o'Clock P.M.						
	Barometer.		Hygrometer			Barometer.		Therm.		Rain.	Wind	Weather.
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	29·91	59	60	51	9	30·03	60	62	53		W.	Fine
2	30·16	60	60	55	5	30·23	61	62	54		W.	Fine
3	30·24	61	61	57	4	30·23	63	63	53		W.	Overcast
4	30·37	61	61	40	21	30·41	62	63	53		E.	Fine
5	30·46	62	62	50	12	30·46	62	63	54		NE.	Fine
6	30·47	62	62	51	11	30·45	62	64	54		NE.	Fine
7	30·42	62	62	54	8	30·39	63	63	58		NE.	Fine
8	30·41	62·5	63	59	4	30·36	62·5	64	56		W.	Overcast
9	30·26	62·5	63	58	5	30·21	63	62	51		W.	Fine
10	30·16	62	62	48	14	30·15	61	62	52		NW.	Do. in town,
11	30·23	61·5	62	54	8	30·31	61	63	55		NW.	Do. [sn. hills
12	30·40	62	62	59	3	30·40	63	63	51		NW.	
13	30·42	61·5	62	54	8	30·38	62	63	55		E.	Fine
14	30·35	61·5	62	55	7	30·31	62	63	56		W.	Overcast
15	30·35	62	62	57	5	30·41	63	63	55		NW.	Fine
16	30·50	63	64	55	9	30·50	64	65	55		E.	Fine
17	30·52	64	65	56	9	30·55	67	65	56		E.	Fine
18	30·59	64	65	52	13	30·59	64	64	53		E.	Fine
19	30·56	63	65	52	12	30·50	64	64	52		E.	Fine
20	30·47	63	64	50	14	30·48	63	64	51		E.	Fine
21	30·51	63	64	58	5	30·51	63	63·5	52		E.	Fine
22	30·52	63	63	58	5	30·49	63	64	53		NE.	Fine
23	30·46	63	63	54	9	30·44	62	63	56		E.	Overcast
24	30·49	63	64	52	12	30·46	64	64	53		E.	Fine
25	30·45	63·5	64	51	13	30·40	64	65	53		NE.	Fine
26	30·31	63·5	64	51	13	30·25	64	64	53		E.	Fine
27	30·29	63·5	64	52	12	30·29	64	66	55		E.	Fine
28	30·35	65	68	54	24	30·30	66	68	63		SE.	Sirocco

Pressure.

			Corr[d] for Expans.
Max.	30·59	at 64°	=30·505
Min.	29·91	59	=29·838
Mean	30·378	63	=30·292

Temperature.

Max. .... 68°
Min. 51
Mean .... 58·8

Dew Point.

Max. 59° Min. 40°

Dryness.

Max. 24 Min. 3

Rain none.

Winds.

N.E.	E.	S.E.	W.	N.W.	
5	12	1	6	4	=28.

Remarks.—A very fine open month.

MARCH 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	–			
1	30·25	65	70	40	30	30·22	66	68	58		SE.	Sirocco
2	30·23	66	67	55	12	30·20	66	67	55		SE.	Very little do.
3	30·23	65	65	57	8	30·23	66	67	56		NE.	Fine
4	30·25	65	66	53	13	30·27	66	67	57		NE.	Fine
5	30·34	66	66	60	6	30·32	67	67	58		E.	Fine
6	30·30	66	67	50	17	30·21	66	68	56		NE.	Fine
7	30·15	65·5	67	49	18	30·06	68	68	60		E.	Fine [hazy
8	30·02	67	67	57	10	30·02	69	69	60		E.	Very thick and
9	30·00	68	67	60	7	30·05	69	68	62		E.	Do.P.M.W. clear
10	30·08	69	68	59	9	30·20	67	68	56	·26	W.	Shower, P.M. fine
11	30·18	67	68	53	15	30·11	67	67	56		E.	Fine
12	30·05	67	68	57	11	30·02	69	69	57		E.	Fine
13	30·05	68	68	59	9	30·04	69	67	58		E.	Fine
14	30·10	67	67	56	11	30·12	69	67	60		E.	Fine
15	30·19	67	67	56	11	30·23	67	67	58		NE.	Fine
16	30·23	66	67	50	17	30·15	67	67	57		NE.	Fine
17	30·09	66	66	49	17	30·03	66	67	57		NW.	P.M. slight show^{r}
18	30·02	65	65	55	10	30·03	65	67	57	·10	NW.	Showers
19	30·10	66	66	55	11	30·12	65	67	57	0·5	NW.	Showers
20	30·14	65	65	56	9	30·11	68	67	55		NW.	Fine
21	30·08	65	65	53	12	30·13	65	66	54		W.	Fine
22	30·19	64	65	47	18	30·16	65·5	66	56		NW.	Fine
23	30·10	64	64	47	19	29·90	67	67	53	·42	NW.	Do., P.M. rain
24	29·78	64	64	58	6	29·68	66	64	52	·66	W.	Do., P.M. rain, sn.
25	29·79	63	64	52	12	29·85	65	64	52	0·6	W.	Fine [on hills
26	29·94	61·5	61	47	14	30·01	61	64	51		NW.	Fine
27	30·07	61	61	46	15	30·07	61·5	63	53	·40	NW.	Rain at night
28	29·93	62	62	60	2	29·81	62	63	55	1·73	W.	Rain
29	29·79	63	64	60	4	29·92	62	64	55	0·4	SE.	Shower [show^{r}.
30	30·02	61	62	50	12	30·05	61	63	55		E.	Fine, P.M. W.
31	30·03	62	63	49	14	30·11	66	65	54		NE.	Do.

Pressure. Corr^{d} for Expans.

Max. 30·34 at 66° =30·254
Min. 29·68 66 =29·595
Mean 30·083 66 =29·998

Temperature.

Max. .... 69°
Min. 51
Mean .... 61·3

Dew Point.

Max. 60° Min. 40°

Dryness.

Max. 30 Min. 2

Rain 3·72 inches.

Winds.

N.E.	E.	S.E.	W.	N.W.	
6	9	3	5	8	= 31.

Remarks.—The former part of the month was warmer and more settled than has been long remembered; from the 16th it was as usual, rainy and windy.

APRIL 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	30·02	62·5	64	55	9	30·06	65	64	56		NE.	Fine
2	30·09	64	65	57	8	30·15	67	64	56		NE.	Fine
3	30·18	64·5	67	52	15	30·21	65	65	55		E.	Fine
4	30·21	63	65	56	9	30·18	65	65	55		W.	Fine
5	30·18	65	67	49	18	30·16	67	66	60		SE.	Partial sirocco
6	30·10	66	69	47	22	30·10	66	67	65		SE.	Partial sirocco
7	30·07	66	67	55	12	30·12	65	66	57		E.	Fine
8	30·14	65	67	50	17	30·17	65	66	57		NE.	Fine
9	30·20	65	66	50	16	30·26	65	66	57		NE.	Fine
10	30·22	65	66	53	13	30·21	67	66	58		NE.	Fine
11	30·21	65·5	65	59	6	30·25	66	67	59		NE.	Fine
12	30·29	66	67	62	5	30·30	67	68	57		NE.	Fine
13	30·29	66	67	62	5	30·22	68	68	62		NE.	Fine
14	30·14	67	66	60	6	30·05	67	68	61		NE.	Fine
15	30·05	68	73	46	27	30·06	68	68	64		SE.	Sirocco
16	30·10	68	73	46	27	30·13	70	71	63		SE.	Sirocco
17	30·08	70	72	52	20	30·11	71	72	59		W.	Fine
18	30·06	69	68	62	6	30·01	70	72	62		W.	Fine
19	29·97	69	68	58	10	29·93	68	70	57		W.	Fine
20	29·86	68	68	56	12	29·83	69	70	58		W.	Fine
21	29·88	67	67	53	14	29·99	67	68	55	·04	N.	Fine
22	30·08	65	65	48	17	30·12	67	68	56		N.	Fine
23	30·10	65	64	50	14	30·14	67	66	57		N.	Cloudy
24	30·13	65	65	48	17	30·19	65	66	57		NE.	Fine
25	30·19	65	65	54	11	30·20	65	66	59		NE.	Fine
26	30·18	65	65	53	12	30·18	66	66	58		NE.	Fine
27	30·17	65	65	60	5	30·18	67	67	58		NE.	Cloudy
28	30·17	66	66	59	7	30·19	67	66	57		N.	Cloudy
29	30·18	66	65	59	6	30·16	67	69	59		NE.	Cloudy
30	30·12	65	65	51	14	30·12	65	66	60		E.	Cloudy

Pressure.		Corr^d for Expans.
Max. 30·30	at 67°	=30·214
Min. 29·83	69	=29·732
Mean 30·140	67	=29·959

Temperature.

Max. .... 72°
Min. 55
Mean .... 62·8

Dew Point.

Max. 62° Min. 46°

Dryness.

Max. 27 Min. 5

Rain 0·04 inches.

Winds.

N.	N.E.	E.	S.E.	W.	
5	14	3	4	4	= 30.

Remarks.—A fine spring month; the former part remarkably warm.

MAY 1826.

	10 o'Clock A.M.					10 o'Clock P.M.						
	Barometer.		Hygrometer.			Barometer.		Ther.		Rain.	Wind	Weather.
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	−			
1	30·09	65	65	57	14	30·05	66	66	60		E.	Cloudy
2	30·01	65	65	48	17	29·98	65	66	60		NE.	Cloudy
3	29·96	65	65	62	3	29·95	66	66	57	·21	W.	Rain
4	29·96	65	65	60	5	29·97	66	67	61		W.	Fine
5	29·91	66	67	59	8	29·86	65	66	60	·30	SW.	Do. Cl^y. P.M. rain
6	29·78	65	65	64	1	29·74	66	66	61	1·15	S.	Rain
7	29·75	67	67	65	2	29·84	68	68	62	·10	W.	Shower
8	29·86	68	67·5	65	2·5	29·88	67	68	60	·69	W.	Rain
9	29·90	67	67	64	3	29·94	67	68	62	·25	NW.	Showers
10	29·93	67	67	65	2	29·93	67	69	62	·10	W.	Showers
11	29·91	67	67	65	2	29·92	67	68	60	·14	W.	Showers
12	29·91	67	67	61	6	30·04	67	68	55	·13	NE.	Fine
13	30·06	65·5	65	57	14	30·14	65	66	59		NE.	Fine
14	30·16	65	66	55	11	30·20	65	66	57		NE.	Cloudy
15	30·20	65	65	56	9	30·21	66	67	57		NE.	Fine
16	30·23	65	65	52	13	30·24	65	66	57		NE.	Fine
17	30·23	64·5	64	54	10	30·24	65	66	57		NE.	Fine
18	30·23	64·5	65	52	13	30·23	65	68	59		NE.	Fine
19	30·23	66	66	56	10	30·21	66	66	60		NE.	Fine
20	30·18	66	66	62	4	30·18	68	68	58		NE.	Cloudy
21	30·14	67	67	56	11	30·13	67	68	61		NE.	Cloudy
22	30·09	67	67	60	7	30·08	67	68	58		NE.	Fine
23	30·07	66				30·10	67	68	58		NE.	Fine
24	30·14	66				30·15	67	68	58		NE.	Fine
25	30·15	66				30·18	67	68	59		NE.	Fine
26	30·19	66				30·22	66	68	57		N.	Overcast
27	30·22	65				30·19	66	68	59		NE.	Fine
28	30·13	66				30·16	67	69	59		E.	P.M. N.E. fine
29	30·10	66	66	54	12	30·04	66	68	59		N.	Fine, rain at n^t.
30	29·94	66	67	56	11	30·06	67	68	59	·22	W.	Fine, rain at n^t.
31	30·06	66	66	58	8	30·20	67	68	60		NE.	Fine, rain at n^t.

Pressure. Corr^d for Expans.

Max.	30·24 at	65°	=30·155
Min.	29·74	66	=29·655
Mean	30·063	66	=29·978

Temperature.

Max. .... 69°
Min. 55
Mean .... 63·2

Dew Point.
Max. 65° Min. 48°

Dryness.
Max. 17 Min. 1

Rain 3·29 inches.

Winds.

N.	N.E.	E.	S.	SW.	W.	NW.	
2	17	2	1	1	7	1	=31.

Remarks.—The former part of the month unusually wet; the latter cold but seasonable.

JUNE 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	30·19	67	67	58	9	30·17	67	68	58		N.	Cloudy
2	30·13	66·5	67	55	12	30·10	67	68	61		W.	Fine
3	30·14	67	67	57	10	30·22	67	68	62		W.	Small rain
4	30·29	67				30·31	68	69	59	·02	E.	Fine
5	30·29	68				30·27	69	69	60		NE.	Fine
6	30·22	68	68	58	10	30·21	68	69	63		NE.	Overcast
7	30·17	68·5	68	59	9	30·13	69	68	62		NE.	Overcast
8	30·07	68	68	57	11	30·05	69	69	59		W.	Overcast
9	30·05	68	68	60	8	30·06	69	69	61		N.	Overcast
10	30·10	68·5	69	59	10	30·19	68	69	60		NE.	Overcast
11	30·21	68·5	68	54	14	30·30	68·5	69	59		E.	Overcast
12	30·30	68	69	55	14	30·28	68	69	59		E.	Fine
13	30·27	68	68	56	12	30·24	68	69	61		NE.	Fine
14	30·17	68	68	60	8	30·13	68	69	62		N.	Fine
15	30·10	69	69	62	7	30·13	70	70	64		W.	Thick & overc.
16	30·16	69	69	61	8	30·18	70	70	64		W.	Thick & overc.
17	30·20	69·5	69	59	10	30·16	69	71	63		W.	Cloudy
18	30·12	69·5	69	57	12	30·10	69	70	61		W.	Cloudy
19	30·07	69	69	63	6	30·06	69	71	65		W.	Cloudy
20	30·05	70	70	62	8	30·09	70	71	63		W.	Fine
21	30·07	69·5	70	60	10	30·10	70	71	63		NW.	Fine
22	30·10	70	70	58	12	30·10	70	70	64		NW.	Overcast
23	30·10	70				30·15	70	71	62		NW	Overcast
24	30·15	70	70	62	8	30·17	70	70	64		NE.	Overcast
25	30·15	69·5	69	58	11	30·18	70	70	65		NE.	Overcast
26	30·20	70	71	55	16	30·23	70	70	61		NE.	Overcast
27	30·21	70	70	60	10	30·20	70	71	63		NE.	Fine
28	30·20	70	71	65	6	30·20	70	71	64		E.	Fine
29	30·17	70·5	71	65	6	30·20	71	73	63		E.	Very fine
30	30·20	71	71	67	4	30·24	72	72	63		E.	Very fine

Pressure.		Corr[d] for Expans.	
Max.	30·31	at 68°	=30·210
Min.	30·05	70	=29·952
Mean	30·166	69	=30·068

Temperature.

Max. .... 73°
Min. 58
Mean .... 65·8

Dew Point.

Max. 67° Min. 54°

Dryness.

Max. 16 Min. 4

Rain 0·02 inches.

Winds.

N.	N.E.	E.	W	N.W.	
3	12	3	9	3	=30.

Remarks.—The former part of the month cloudy and cold; the latter warm and fine.

[To be continued.]

LIX. *Description of the Skulls of two apparently undescribed Species of Dolphins, which are in the British Museum. By* J. E. Gray, *Esq. F.G.S. &c.**

UNTIL Camper, Spix, Cuvier, Rudolphi and Blainville, published their dissertations on the bones of the Cetaceous animals, the species were enveloped in such complete confusion that it was impossible to distinguish one from another; and all these authors have proved that the best characters are to be drawn from the examination of the skull.

Having recently an occasion to examine the skulls of the species of this genus which are in the collection of the British Museum, there were two which appeared to differ from any of the skulls which had been figured. And one of them (and perhaps the other) having been collected from quite a different part of the world from the species which they most nearly resemble, I have been induced to notice them provisionally as new species, from the examination of the skulls alone.

The first specimen was brought to this country and presented to the Museum by Capt. P. P. King, R.N., when he returned from his survey of the Coast of New Holland; and therefore I am desirous of dedicating the species to him, and shall propose to call it *Delphinus* (Delphinapterus?) *Kingii.*

The form and structure of the skull most nearly approaches *Delphinus Leucas* found in the North Seas, and figured by Pallas in his Travels, t. 69. and the skull by Cuvier, *Oss. Foss.* v. t. 22. f. 56. The skull under consideration differs from the latter figures by the shortness of the beak compared with the length of the head, which is less than one-half; and in the narrowness of the exposed portion of the maxillæ which edges the front of the blowers. The cavity of the brain is rather more globular, and the blower more anterior. The upper jaw contains 9 or 10, and the lower jaw 9 teeth on each side; they are small, conical, and recurved.

The length from the tip of the beak to the front of the blowers, 8 inches; from the back of the blower on the back of the head to the top of the occipital bone 5 inches; breadth of the blower $2\frac{1}{4}$; length of the blower $1\frac{1}{2}$; breadth of the head between the outer edge of the post-orbital apophysis, 9 inches; between the outer wing of the temporal bone $7\frac{1}{2}$ inches; the height of the head to the top of the occipital crest, $8\frac{1}{2}$ inches; breadth of the beak at the commencement of the cheek-bones, 5 inches.

I have no account of the animal from which this skull was

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taken;

taken; but there is great reason to think that it will be found to offer some external characters which will distinguish it from the *White Beluga* of the North Seas, to which the skull is so nearly allied. It may perhaps be one of the species recently described by *Quoy* and *Gaimard*, as they saw them floating in the water.

This head differs greatly in the length of its beak from the *Delphinus* (Delphinapterus) *Peronii* of Lacepède, which is figured by Cuvier, *Oss. Foss.* t. 21. f. 4 & 5, which was brought home by Mr. Hunter the surgeon to the same expedition.

The second specimen has no habitat affixed to it in the Museum. It was there before my appointment. It agrees with the head of *Delphinus griseus*, figured by Cuvier, *Oss. Foss.* v. t. 22. f. 1 & 2. in almost every particular; except that it has 11 teeth on each side in the upper jaw, and 10 in the lower; while that species has seldom more than 2 or 3, in the lower jaw only.

In this latter character it agrees with the *Delphinus Orca* of Otho Fabricius, the skull of which is figured by Cuvier, t. 22. f. 3 & 4; but it differs from it in the small size of the temporal fossæ and in the width of the temporal ridge and the great size of the space for the attachment of the occipital muscles, and also in its small size, which is even less than the former species.

Length from the tip of the beak $8\frac{3}{4}$ inches; from the beak of the blower to the front of the occipital hole 6 inches; length of the blower 2 inches; breadth of the blower 2 inches; breadth of the head between the outer edge of the post-orbital apophysis $9\frac{1}{2}$ inches; head between the wing of the temporal bones 8 inches; height of the head to the occipital crest 7 inches; breadth of the beak at the commencement of the cheek-bones $4\frac{1}{2}$ inches; length of the temporal fossæ $3\frac{1}{2}$ inches; breadth of the attachment of the occipital muscles 7 inches.

I propose to call this species, on account of its appearing to connect two species, *Delphinus intermedius.*

LX. *On the Position of the Focus in the Eye.* *By Mr.* J. R. Rumball, *M.R.C.S.**

I HAVE at various times made many optical experiments; and as some of them are directly opposed to the generally received opinion, "that the images of objects which enter the eye, are painted in an inverted position upon the retina," I hope you will not consider an account of them unworthy a place in your valuable Magazine.

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I shall

I shall reserve for a future occasion some remarks which I have to make upon the errors, apparent in the diagrams of all the books I have consulted upon this subject, and confine myself at present to my own proposition and its proofs.

I affirm that "the images of objects are *not* inverted upon the retina, but that every image is painted there in a point;" in other words, "*the focus of the eye is upon, and not anterior to, the retina.*"

I herewith send you two experiments, which I purpose to follow up by some others, equally conclusive; but which would occupy too much space if inserted in this paper.

Exp. 1.—Dissect off the coats of the eye at the posterior extremity of its axis. Hold it up between the finger and thumb, and the vitreous humour will protrude as in fig. 1. A, the

Fig. 1.

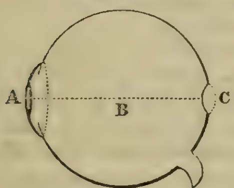
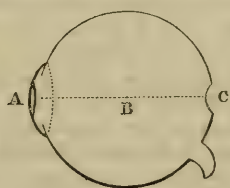


Fig. 2.



pupil. B. the axis of the eye. C. the protruding vitreous humour. Look through the eye, and pass a probe or other object backwards and forwards before the pupil. *Its apparent motion will be the reverse of its real one.*

Exp. 2.—Cut away the protruding vitreous humour, and the eye will assume the shape of fig. 2. A. the pupil. B. the axis of the eye. C. the depression occasioned by the abstraction of the humour.

Now pass a probe up and down before the pupil; and upon looking through the eye, the probe's real and apparent motion will be the same.

In the first experiment the axis of the eye is slightly elongated; and as the image of the probe is inverted, we have sufficient proof that the rays of light proceeding from it must have crossed each other, before their exit from the eye. *The focus is therefore within the axis.*

In the second experiment, the axis is shortened and the real and apparent position of the object being the same, demonstrates that the rays have not crossed; consequently the focus of the eye is now without, or posterior to it. But as the retina is situate between the point where the first Experiment proves the rays to have crossed, and the point which the second Experiment determines to be anterior to their crossing; and as these extreme points approximate closely to each other, as in the first case the focus is within, and in the second without the axis, and as the retina is situate between them,—I consider my proposition established; viz. "*that the focus of the eye is upon the retina.*"

J. R. Rumball.

LXI. *On the Rectification of the Ellipse. By the Rev.* Brice Bronwin*.

THE well known differential expression of the length of the arc is $dx\sqrt{1 - e^2 x \sin^2 x} = d\frac{e}{2}\sqrt{\frac{2(2-e^2)}{e^2} + 2\cos 2x}$ $= \frac{e}{2}\, dx\sqrt{p + 2\cos z}$ by substitution.

Assume $\sqrt{p + 2\cos z} = A + \overset{1}{A}\cos z + \overset{2}{A}\cos 2z + \&c.$ Difference both sides for z, divide by dz, multiply by $p + 2\cos z$; the result will be the same as if both sides had been multiplied by $\sin z$, being the expression of $\sin z\sqrt{p+2\cos z}$. By equating therefore the coefficients of the like terms in both the developed expressions of this quantity, we get $\frac{5}{2}\overset{2}{A} + p\overset{1}{A} - A = 0$, $\frac{7}{2}\overset{3}{A} + 2p\overset{2}{A} + \frac{1}{2}\overset{1}{A} = 0$, $\frac{9}{2}\overset{4}{A} + 3p\overset{3}{A} + \frac{3}{2}\overset{2}{A} = 0$. The quantities $\overset{2}{A}$, $\overset{3}{A}$, &c. are therefore known when A and $\overset{1}{A}$ are known.

Resuming therefore $\sqrt{p+2\cos z} = A + \overset{1}{A}\cos z + \overset{2}{A}\cos 2z + \&c.$; difference both sides for p, and then multiply by $p + 2\cos z$; the result will be the expression of $\frac{1}{2}\sqrt{p + 2\cos z}$. Equating therefore the coefficients of like terms in both expressions of that quantity, we obtain $\frac{d\overset{1}{A}}{dp} + p\frac{dA}{dp} - \frac{1}{2}A = 0$, $\frac{d\overset{2}{A}}{dp} + p\frac{d\overset{1}{A}}{dp} + 2\frac{dA}{dp} - \frac{1}{2}\overset{1}{A} = 0$, &c. Eliminating $\frac{d\overset{2}{A}}{dp}$ from the second of these by means of the first of the former equations, we have $p\frac{d\overset{1}{A}}{dp} + 4\frac{dA}{dp} - \frac{3}{2}\overset{1}{A} = 0$. Eliminating $\frac{d\overset{1}{A}}{dp}$ from

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from this last by the first of the second set of equations we obtain $\overline{p^2-4}\cdot\frac{dA}{dp} - \frac{1}{2}pA + \frac{3}{2}\overset{1}{A} = 0$. Differencing this and eliminating $\frac{dA}{dp}$, we get finally $\overline{p^2-4}\cdot\frac{d^2A}{dp^2} + \frac{1}{4}A = 0$. Thus we can find both A and $\overset{1}{A}$.

Mr. Knight in the 3rd volume of Leybourn's Repository, has shown us how to find $\overset{1}{A}$ from A by means of an integral, and seems to intimate that it had not been done before. Perhaps therefore the conclusions here arrived at are found for the first time; and if we could integrate the above differential equation of the 2nd order in finite terms, we should have a finite expression for the quadrantal arc of an ellipse. I fear, however, this cannot be done. Still the equation is not useless; we may derive from it a multitude of different series expressive of the elliptic arc, and such as perhaps otherwise could not have been deduced.

Putting y for A and $2x$ for p to simplify, the preceding equation becomes $\overline{x^2-1}\cdot\frac{d^2y}{dx^2} + \frac{1}{4}y = 0$. Assume $y = a x^{\frac{1}{2}} + \overset{1}{a} x^{-\frac{3}{2}} + \overset{2}{a} x^{-\frac{7}{2}} +$ &c., and substituting for y and $\frac{d^2y}{dx^2}$ in the preceding; we get $\overset{1}{a} = -\frac{1}{3\cdot5+1}a = -\frac{1}{4^2}a$, $\overset{2}{a} = -\frac{3\cdot5}{(3\cdot5+1)(7\cdot9+1)}a = -\frac{3\cdot5}{4^2\cdot8^2}a$, $\overset{3}{a} = -\frac{3\cdot5\cdot7\cdot9}{4^2\cdot8^2\cdot12^2}a$ &c., where by making c nothing, or x infinite, a is found $=\sqrt{2}$. Hence if we put $t = \frac{1}{x}$ we find the length of the elliptic quadrant $= \frac{e}{2}\cdot\frac{\pi}{2}\cdot y = \frac{\pi}{2}\sqrt{\overline{1-\frac{1}{2}c^2}}$ into $1 - \frac{1}{4^2}t^2 - \frac{3\cdot5}{4^2\cdot8^2}t^4 - \frac{3\cdot5\cdot7\cdot9}{4^2\cdot8^2\cdot12^2}t^6$ &c.; which is the same as it would have been found if we had expanded $\sqrt{\overline{1-c^2\sin^2x}}$ by the binomial theorem, and developed the powers of sin x in cosines of the multiple arcs; and is the same, under a form a little different, with the second series, page 196, vol. i. of Leybourn's Math. Repository, and there ascribed to Legendre and Euler.

Let us now consider y a function of $v = x - \sqrt{\overline{x^2-1}} = \frac{1-c}{1+c}$, where c = semi-conjugate. Then $\frac{dy}{dx} = \frac{dy}{dv}\cdot\frac{dv}{dx}$,

 $\frac{d^2y}{dx^2}$

$\frac{d^2y}{dx^2} = \frac{d^2y}{dv^2} \cdot \frac{dv^2}{dx^2} + \frac{dy}{dv} \cdot \frac{d^2v}{dx^2} = \frac{d^2y}{dv^2} \cdot \frac{v^2}{x^2-1} + \frac{dy}{dv} \cdot \frac{2v}{v^2-1} \cdot \frac{1}{x^2-1}$; and the equation $\overline{x^2-1} \cdot \frac{d^2y}{dx^2} + \frac{1}{4} y = 0$ is changed into $v^2 \cdot \overline{1-v^2} \cdot \frac{d^2y}{dv^2} + 2v \frac{dy}{dv} + \frac{1}{4}(1-v^2) y = 0$.

Assume $y = a v^{-\frac{1}{2}} + \overset{1}{a} v^{\frac{3}{2}} + \overset{2}{a} v^{\frac{7}{2}}$ + &c., and substitute for $\frac{d^2y}{dv^2}$, $\frac{dy}{dv}$, and y in the preceding, and we shall find $\overset{1}{a} = \frac{1}{2^2} a$, $\overset{2}{a} = \frac{1}{2^2 . 4^2} a$, $\overset{3}{a} = \frac{3^2}{2^2 . 4^2 . 6^2} a$, $\overset{4}{a} = \frac{3^2 . 5^2}{2^2 . 4^2 . 6^2 . 8^2} a$, where a is found $= 1$. This gives the elliptic quadrant $= \frac{e}{2} \cdot \frac{\pi}{2} \cdot y = \frac{\pi}{2(1+v)}$ into $1 + \frac{1}{2^2} v^2 + \frac{1}{2^2 . 4^2} v^4 + \frac{3^2}{2^2 . 4^2 . 6^2} v^6$ + &c.; which is the first series in the place above referred to in Leybourn, the inventor of which is Mr. Ivory.

Next let us consider y a function of $u = \frac{x-1}{x+1} = c^2 =$ square of semi-conjugate. Then the equation will become $u(1-u)^2 \frac{d^2y}{du^2} - 2u(1-u)\frac{dy}{du} + \frac{1}{4} y = 0$. Making $y = a u^m + \overset{1}{a} u^{m+1}$ + &c., we find $m = 1$, or $= 0$. But in the second case a also $= 0$, and the two series become the same; viz. $y = au + \overset{1}{a} u^2 + \overset{2}{a} u^3$ + &c.; whence we obtain $\overset{1}{a} = \left(2 - \frac{1}{4}\right)\frac{a}{2}$, $\overset{2}{a} = \left(8 - \frac{1}{4}\right)\frac{\overset{1}{a}}{6} - \frac{2a}{6}$, $\overset{3}{a} = \left(18 - \frac{1}{4}\right)\frac{\overset{2}{a}}{12} - \frac{6\overset{1}{a}}{12}$, $\overset{4}{a} = \left(32 - \frac{1}{4}\right)\frac{\overset{3}{a}}{20} - \frac{12\overset{2}{a}}{20}$, &c. where the law of continuance is evident, and a remains arbitrary. This series is to be multiplied by $\frac{e}{2} = \frac{1}{2}\sqrt{1-u}$; hence it becomes nothing both when $u = 0$, or $u = 1$, that is when $e = 1$ and $e = 0$. It must therefore only be regarded as a particular solution; it will, however, be useful in seeking the complete one.

Make $y = p + q$ log. Cu. Then substituting this value of y and its differentials in our equation, and making the part multiplied by log. Cu separately $= 0$, we shall have the same equation in q as we had in y; consequently the value of y just found may be taken for that of q; we shall have moreover $u(1-u)^2 \frac{d^2p}{du^2} - 2u(1-u)\frac{dp}{du} + \frac{1}{4} p + 2(1-u)^2 \frac{dq}{du} -$

$(1-u)$

$(1-u)^2 \frac{q}{u} - 2(1-u)\,q = 0$. The assumption to be made here is $p = b + b^1 u + b^2 u^2 + b^3 u^3 + \&c.$; where b will be determined in terms of a. Retaining therefore a in q as an arbitrary, we shall have two arbitraries a and C, and consequently a complete solution. And no doubt should be led to the third series in the place in Leybourn already referred to, and which is Euler's.

If we were to suppose y a function of $e = \sqrt{\frac{2}{x+1}}$, we should find $e^2 \frac{d^2 y}{d e^2} + 3e \frac{d y}{d e} + \frac{y}{1-e^2} = 0$. No doubt this would lead to a series well known.

If we were to suppose y a function of $w = \frac{1-e}{1+e}$, we should find $(w - w^2 - w^3 + w^4) \frac{d^2 y}{d w^2} - (4w - 2w^2 + 2w^3) \frac{dy}{dw} + (1+w)\,y = 0$. This is much like the equation between y and u, and would probably be troublesome to manage. Therefore as my paper is nearly filled, I shall not proceed with it further.

BRICE BRONWIN.

LXII. *Notices respecting New Books.*

A Manual of Chemistry chiefly for the Use of Pupils of Mechanics' Institutions. By ANDREW FYFE, *M.D. F.R.S.E.* Edinb. 12mo. pp. 340.

Experiments illustrative of Chemical Science; systematically arranged, &c. By the Author of "The Elements of Chemical Science." Glasgow, 12mo. pp. 157.

IT is so little to our taste and inclination to find fault with any attempt, however humble, to extend the knowledge of chemical science, that we have long delayed noticing the first of the above-mentioned works. Upon considering, however, that it is not less a duty to expose the faults of authors than to display their merits, we have at length resolved to subject Dr. Fyfe's work to a short, but we trust not to too severe an examination.

To begin at the beginning. The author informs us that the arrangement which he has adopted "is that long ago recommended by Dr. Black." On referring, however, to the Lectures of that illustrious philosopher, it strikes us that it is impossible to discover the similarity which they are stated to possess. Dr. Black's arrangement, as far as we think it needful to quote it, is the following:—Of chemistry in general—Of heat in general—General effects of mixture—Chemical apparatus—Chemical history of bodies—Salts—Earths—Fixed air—Azote, &c. Now Dr. Fyfe's order is, Attraction—Specific gravity—Heat—Light—Atmosphere—Oxygen—Azote—Hydrogen,

gen, &c. The question is not which is the better arrangement, but their dissimilarity is obvious.

Under the head of Attraction we look in vain in Dr. Fyfe's work for any account of the doctrine of definite proportions or atomic theory. It is singular that so great an omission should have been committed by Dr. Fyfe, knowing as he must the very important changes which this doctrine has effected in the science of which he was treating: for not only has it reduced the facts which were known previously to its existence, to order and regularity, but it has given to chemical investigations a certainty of which they were before comparatively destitute. It is indeed by this doctrine that we are enabled to explain certain facts respecting affinity, which were before referred to the action of mass.

Although we have thus begun at the commencement of Dr. Fyfe's work, we have neither time, space, nor inclination to follow it minutely to the close: nor is it to be considered that the mistakes or omissions which we shall point out, are all that the work contains. We have opened the book almost at random; and unfortunately it is difficult not to find various statements which require careful revision, or not to observe numerous deficiencies to be supplied.

When treating of Nitric Acid, it would in our judgement be reasonable to suppose that the mode of preparing it would be given, as well as a statement of the proportions of oxygen and azote of which it is constituted: but there is not one word on either subject. When the author, in a subsequent part of the work, is treating of Nitrate of Potash, we do indeed find that the acid is to be procured by decomposing that salt with sulphuric acid: and it is perhaps natural that Dr. Fyfe should recommend for this purpose, the very disadvantageous proportions of sulphuric acid and nitrate of potash, to be found in the Edinburgh Pharmacopœia.

Although no mention is made of the proportions of oxygen and azote which form nitric acid, yet that they do constitute it is certainly admitted by our author: and he informs us, "that if a metal, as iron, be put into it, the iron acquires oxygen and is converted into an oxide." "This experiment shows," adds Dr. Fyfe, "that oxygen is one of the ingredients of the acid." Now it appears to us, that neither the fact nor the inference deduced from it are properly stated; for in the first place the acid must be either diluted or heated, in order to insure action between it and the iron; and in the next place, as that which would prove too much in any case is not allowed to prove any thing, we cannot admit that the oxidizement of the iron proves that the oxygen is derived from the nitric acid. If this proof were allowed, then muriatic acid must also contain oxygen; for when diluted with water, iron is oxidized by their mutual action.

The properties of Carbonic Acid are treated of next. And here again we have no account of the proportions of its constituents, or of the mode of obtaining it by decomposing a carbonate with an acid. It is indeed true that this method of preparing it is to be found, where it ought not to be, viz. under the head of Lime; but even there

there its composition is not given, nor has Dr. Fyfe stated the quantities of acid and base which constitute carbonate of lime.

With respect to Muriatic Acid, treated of at p. 133, the mode of preparation is omitted, as in the cases already noticed; it is however supplied when Muriate of Soda is described. But shall we obtain credit when we mention that Dr. Fyfe has not, either when treating of its properties or preparation, mentioned that muriatic acid is constituted of chlorine and hydrogen, or even that it is a compound substance?

To proceed from acids to salts: the first that attracts our notice is Super-oximuriate of Potass. The account of this salt is at once defective and redundant: it is deficient as to composition, preparation and properties, but redundant as to the account of its uses; for space is uselessly allotted to an account of Forsyth's Detonating Lock, which an account of the nature of the salt would have properly occupied. So extremely defective, indeed, is the account of the salt in question, that the name of Chlorate of Potash,—which perhaps, except at Edinburgh, is now universally adopted,— is not even mentioned; nor, as far as we have searched, is such an acid as the Chloric described in any part of the work. This salt is extremely interesting to the chemist, as affording him oxygen gas in the greatest purity: but of the latter circumstance no notice whatever is taken by Dr. Fyfe. As, however, contrary to the received modes of proceeding, he describes nitric acid when treating of the salt which yields it, it occurred to us that we might meet with chlorate of potash under the head of Oxygen Gas. We accordingly referred back to p. 106, and there met with the following passage:—"Oxigen gas is procured from compounds, into which it enters as a component part. That generally used is the substance sold under the name of *Manganese*.—See *Manganese*."—Determined however not to be foiled, if success were attainable, we turned to Manganese, but found nothing whatever about chlorate of potash.

If it were possible, we should gladly seize any opportunity of speaking well of the work before us. But what can we say for an author, who in treating of a very important salt, calls it by an obsolete name, and does not mention its modern one, though in general use? who omits not only to state the composition of the salt in question, but does not in any part of his work mention the nature of the acid which it contains? and lastly, takes scarcely any notice of the most useful purpose to which it is applied!

In concluding, we would remark that even where facts are correctly stated, and which frequently is not the case, the language in which the information is contained is extremely loose and vague, and quite unworthy of the subject. As an example, we take the following passage from p. 213:—

"All the acids act easily on zinc, owing to its powerful attraction for oxigen. The action between it and sulphuric acid is interesting, as it affords a method of procuring hydrogen in a state of purity, and another substance also much employed in the arts, *sulphate* of zinc, or *white vitriol*. For this purpose, having put an ounce of zinc into a retort, pour on it an equal quantity of oil of vitriol, diluted with five

of

of water. Here the oxigen of the water unites with the zinc, and the hydrogen is set free; the oxid then combines with the sulphuric acid, to form the *sulphate*. The hydrogen obtained by this process, is much purer than that got by iron. It has less unpleasant odour, and it is of less specific gravity. By evaporating the solution in the retort, crystals are obtained. This is the mode by which the purest white vitriol is prepared; but that of commerce is generally procured by a process similar to that by which green and blue vitriol are formed, by exposing the ore of zinc and sulphur to air and moisture, by which both combine with oxigen to form sulphuric acid and oxid, which unite, and form the sulphate."

In the first place the hydrogen is not quite pure, for it is not free from smell; and at p. 108. Dr. Fyfe allows that the smell of hydrogen is derived from its impurity. Indeed the hydrogen though at first stated to be pure, is afterwards mentioned to be so only comparatively. The oxygen of the water is properly represented as combining with the zinc; but the hydrogen is not so clearly stated to be derived from the decomposition of the water, nor that when it is set free it is in the gaseous form. That hydrogen "is got by iron" may be a Scotticism; but it is neither an English nor a chemical expression; the metal not possessing, as far as we know, any such power as that here attributed to it. We may certainly obtain crystals of sulphate of zinc by evaporating the solution in the retort; but we think most persons would prefer to transfer it to a basin either of glass or porcelain. The ore of zinc and sulphur is an incorrect expression for sulphuret of zinc; it is an ore of zinc, but not of sulphur. Indeed we never heard of an ore of the last mentioned substance, nor do *both* combine with the oxygen to form sulphuric acid and oxide; the former *only* becomes an acid; the latter only an oxide.

In a future edition we would strongly recommend Dr. Fyfe to give more precise directions for performing processes, and to state the composition of bodies more in detail.

The second work, as announced by the title, contains directions for performing various chemical experiments; and it is the production, says the advertisement, of "one allowed to be a successful experimentalist." We have not minutely examined the details; but as far as we have proceeded, the experiments are collected from chemical authors well known to the public. We do not however question the assertion that some are original.

On the subject of Definite Proportions the author (Mr. John Murray if we mistake not) has committed one of the most glaring faults we ever remember to have seen in print. He has actually, twice stated that Dr. Thomson assumes the numerical representation of oxygen gas to be 1.1111, instead of 1; thus mistaking and misstating the specific gravity of the gas in question for its atomic weight. As the natural consequences of these blunders, he has also given the atomic weights of hydrogen, chlorine, muriatic acid and water, all erroneously.

LXIII. *Pro-*

LXIII. *Proceedings of Learned Societies.*

MEDICO-BOTANICAL SOCIETY.

THIS Society resumed its sittings on the 12th of October, at its apartments in Sackville Street; Sir James M'Gregor, M.D. F.R.S., in the chair.

Presents were received of the seeds of *Argemone Mexicana* and *Genista tinctoria*, &c. from various members; and of a collection of Dissertations from Professor Thunberg of Upsal.

The director, Mr. Frost, then delivered the Annual Oration, the subject being the Progress and Prospects of the Society.

A notice was read, offering a reward of 25*l.*, or a gold medal of equal value, for an accurate description of the plant which yields myrrh, which is merely supposed to be the produce of the *Amyris Kataf.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

June 11.—At this sitting various prizes were adjudged. In 1825, the Academy proposed the following as a prize question in Natural Science. *A general and comparative history of the circulation of the blood in the four classes of vertebrated animals, before and after birth, and at different ages.*

Only one memoir was received, and with the motto *Natura non facit saltus.* The memoir was extremely long, and not judged worthy of the prize, which is 3000 francs; and the subject is continued to the year 1829.

Great Mathematical Prize. These questions were proposed in 1822 for 1824, and continued for the year 1826 and again to 1827. The subjects are the following: 1st. *To determine by numerous experiments the density which is acquired by liquids, and especially by mercury, water, alcohol and sulphuric æther, by pressure equivalent to the weight of several atmospheres.*—2dly. *To measure the effects of heat produced by these compressions.* The Academy received two essays. That with the motto "*Si les observations précises font naitre les théories, la précision des théories, provoque à son tour la précision des observations,* (Méc. Céleste)" was judged worthy of the prize. The authors are MM. Colladou and Sturm of Geneva.

Astronomical Prize, founded by M. Lalande.—The Academy, as proposed by the Committee, decided this year to divide the prize between M. Pons, Director of the Observatory at Florence, and M. Gambart, director of that at Marseilles, they having discovered, observed or calculated, the three latest comets.—The Academy greatly regretted that it had no means of expressing the high value which it attaches to the interesting astronomical researches which have occupied M. Valz of Nismes, with a constancy and ability worthy of the highest praise.

Experimental Physiological Prize, founded by M. de Montyon.—This was adjudged to M. Adolphe Brongniart, for his essay on the generation of vegetables.

Prize founded by M. de Montyon "*to the discoverer of a method of* *rendering*

rendering a trade less insalubrious."—This prize was not adjudged; only one communication was worthy of notice, and the consideration of that was deferred.

Prize founded by M. de Montyon "*to those who have improved the healing art.*"—Two prizes were adjudged;—one of 10,000 francs to MM. Pelletier and Caventou for the discovery of sulphate of quina.—Another, of the same amount, to M. Civiale, as being the first who had practised the breaking of the calculus in the living subject, and who had operated successfully upon many persons. The Academy however gave medals of encouragement, of less value, to several other persons.

Prize relating to Statistics, founded by M. Montyon.—A gold medal was adjudged to M. Brayer of the Department of L'Aisne for a statistical description of that Department; and another to M. Cavoleau, for a work entitled *Œnologie Française,* containing a statistical account of all the vineyards of France.

Prizes proposed by the Royal Academy for 1828, 1829, *and* 1830.

New Grand Prize relating to Natural Sciences. *A description, accompanied with figures sufficiently explanatory, of the origin and distribution of the nerves in fishes.*—The prize is a gold medal of the value of 3000 fr. The Memoirs to be sent before the 1st of January 1830 to the Secretary of the Institute.

Grand Prize in Natural Sciences. *A general and comparative history of the circulation of the blood in the four classes of vertebrated animals, before and after birth, and at different ages.* The memoirs to be sent before the 1st of January 1829.—The prize a medal of the value of 3000 francs.

Grand Mathematical Prize. *To examine the minutiæ of the phænomena of the resistance of water, by carefully determining by correct experiments the pressures sustained separately by a great number of points conveniently selected, upon the anterior, lateral, and lower parts of a body, when it is exposed to the shock of water in motion, and when it moves in the same fluid in a state of rest; to measure the motion of the water in several points of the undulations which are near the bodies; to construct the curves which are formed by them; to determine the point at which their deviations commence before the body: lastly, to establish if possible, by these experiments, empirical formulæ, which are to be afterwards compared with all the experiments previously made on the same subject.* The Prize is a gold medal of the value of 3000 francs. Memoirs to be sent to the Secretary of the Academy before 1st of January, 1828.

Grand Mathematical Prize.—The prize relative to the calculation of the perturbations of the elliptical motion of comets not having been adjudged, the Academy proposes the same subject in the following terms:—*It invites the attention of geometers to this theory, in order to give rise to a fresh examination of the methods, and to the perfecting of them. It also requires that these methods should be applied to the comet of* 1759; *and to one of the two other comets, whose periodical re-*

turn

turn has been already ascertained.—The prize is a gold medal of 3000 francs. The memoirs to be sent to the Secretary before the 1st of January 1829.

M. Alhumbert's Prize.—This prize is for the encouragement of arts and sciences; its amount is 12,000 francs. The subject is the following: *To describe in a complete manner and with figures the changes which the skeleton and muscles of frogs and salamanders undergo at the different periods of their life.* Memoirs (post paid) to be sent to the Secretary before 1st January 1829.

Lalande's Astronomical Prize.—A gold medal of the value of 625 francs to be adjudged annually to any person who shall have made the most interesting observation, or published the most useful memoir relating to Astronomy.

M. de Montyon's Prize for Experimental Physiology.—This is a gold medal of the value of 895 francs, for any work, either printed or in manuscript, which shall be adjudged to have most contributed to the progress of experimental physiology. Memoirs to be sent (post paid) to the Secretary of the Academy before 1st of January 1828.

M. de Montyon's Mechanical Prize.—This prize to be given to him who in the opinion of the Academy shall best contribute to the improvement of useful instruments, either in Agriculture, the Arts, or Sciences.—A gold medal of the value of 1500 francs. Memoirs with descriptions or models to be sent to the Academy before 1st of January 1828.

LXIV. *Intelligence and Miscellaneous Articles.*

LABARRAQUE'S DISINFECTING SODA-LIQUID.

On the 20th of February last a paper by Dr. Granville, was read before the Royal Society, On the composition of M. Labarraque's disinfecting liquid. This paper was printed in the Quarterly Journal of Science for April. According to Dr. Granville, the disinfecting soda-liquid is a mixture of chloride of sodium and chlorate of soda.

Before the publication of Dr. Granville's paper, I showed in the Philosophical Magazine and Annals for May, that it was impossible that this should be the composition of the substance in question; and for two reasons: first, The proportion of common salt employed by M. Labarraque does not contain chlorine sufficient to convert the carbonate of soda into the salts mentioned by Dr. Granville; and secondly, I proved that the solution of carbonate of soda absorbed chlorine, without evolving carbonic acid.

In the last Number of the Quarterly Journal, Mr. Faraday has examined the subject with his usual precision, and has confirmed the opinions which I had before expressed, and added several important facts. He found that when the proportions of the several substances directed to be employed by M. Labarraque were operated upon, that "from the beginning to the end of the operation not a particle of carbonic acid was disengaged from the solution [of carbonate of soda], although chlorine was readily absorbed." Mr. Faraday finds, as

 I have

I have also stated, that the disinfecting solution may be evaporated to dryness and retain bleaching power. He observes, that "notwithstanding the perfect manner in which the chlorine may be separated by crystallization and slow evaporation, yet it is certain that by quick evaporation a substance apparently quite dry may be obtained which yet possesses strong bleaching power." He considers that the carbonate of soda acts as a more simple substance with the chlorine: when, however, excess of chlorine is passed into a solution of carbonate of soda, carbonic acid is evolved, and muriate and chlorate of soda are formed, in the same manner as when caustic soda is employed. With respect to Dr. Granville's opinion, Mr. Faraday concludes with the following observations. "It would seem as if I were unacquainted with Dr. Granville's paper on this subject, published in the last volume of this Journal, p. 371, were I to close my remarks without taking any notice of it. Unfortunately, Dr. Granville has mistaken M. Labarraque's direction; and by passing chlorine to 'complete saturation' through the carbonate, instead of using the quantities directed, has failed in obtaining Labarraque's really curious and very important liquid; to which in consequence not one of his observations or experiments applies, although the latter are quite correct in themselves."

PREPARATION OF SPONGY PLATINA.

M. Dœbereiner procures the above substance by the following process:—Mix muriate of platina with neutral tartrate of soda dissolved in water; this mixture is to be heated in a glass tube (about $\frac{1}{2}$ or $\frac{3}{4}$ of an inch in diameter, and 20 to 30 inches in length) until the fluid is rendered slightly turbid, and it is afterwards to be exposed for several days to the sun's rays. The greater part of the platina is separated from solution, and deposited in the state of minute laminæ of a grayish-black colour, on the sides of the glass. The tube and its contents are to be put into a glass vessel containing water, and it is to be filled with hydrogen gas. The platina deposited upon the glass becomes almost immediately white and shining like silver. The platina may be then readily detached from the glass. During the reduction of the platina by this process, the tartaric acid is partly converted into carbonic and formic acid. As the inflammation of the hydrogen is caused by abstracting a portion of the caloric from the oxygen, effected by the platina, the smaller the laminæ of the metal are, the more readily is the incandescence produced. Spongy platina (for the lamps for instantaneous light) is prepared of great power by moistening the muriate of ammonia and platina with a concentrated solution of ammonia; the paste formed is to be heated to redness in an earthen or platina crucible.—*Hensman's Repertoire de Chimie, &c.*

TEST OF NITRIC ACID.

Dr. Liebig proposes the following process for detecting the presence of nitric acid: the fluid to be examined is to be mixed with as much sulphate of indigo as will give it a distinct blue colour, and after

after adding a few drops of sulphuric acid, the mixture is to be boiled.

If the fluid contains a nitrate, the blue colour will be discharged, or only rendered yellow if the quantity of nitrate is very minute. Dr. Liebig states that by this process nitric acid may be detected when there is not more than a four-hundredth of it present; by adding a little common salt to the fluid before applying the heat, even a five-hundredth of nitric acid may be readily detected.—*Ann. de Chimie*, xxxv. 80.

CARBOAZOTIC ACID.

Dr. Liebig treats fine indigo broken into small fragments, with eight or ten times its weight of nitric acid of moderate strength and in a gentle heat; fresh nitric acid is to be added as long as an extrication of red vapours is occasioned by it. When the liquid has become cold, a large quantity of semi-transparent yellow crystals will be formed, which are to be washed with cold water, then dissolved in hot, and re-crystallized; these crystals, which Dr. Liebig calls carboazotic acid, have the form of yellow brilliant plates, and if the operation is well conducted, no artificial tannin is obtained during the action of the nitric acid upon the indigo.

From the mean of several experiments this acid appears to be composed of

$12\frac{1}{2}$ atoms of carbon	....	93·75	or	31·5128
$2\frac{1}{2}$ ...... azote	....	43·75	..	14·7060
16 ...... oxygen	....	160·	..	53·7812
		297·3		100·

This acid may also be obtained by treating silk with ten or twelve times its weight of nitric acid, but it yields much less than indigo.

Carboazotate of potash is composed of 83·79 acid + 1621 base. It crystallizes in long yellow quadrilateral needles, which are semi-transparent and very brilliant. It dissolves in 260 parts of water at 59 Fahr. and in much less boiling water. Carboazotic acid also forms crystalline salts with soda, ammonia, barytes, lime, magnesia, and also with the oxides of copper, silver, and protoxide of mercury.

All these salts detonate, and more powerfully when heated in close vessels than when heated in the air; but with those bases that yield oxygen most readily, the explosion occurred with the least force.—*Ann. de Chimie*, xxv. 72.

ANHYDROUS SULPHITE OF AMMONIA.

M. Dœbereiner combined four volumes of dry sulphurous acid gas with eight volumes of ammoniacal gas, equally dry; a brownish yellow vapour was formed, which soon condensed into a light brown substance. On the addition of an atom of water it became colourless, and hydrous crystals were formed. Equal volumes of sulphurous acid and ammoniacal gas form supersulphite of ammonia, in which the second atom of acid occupies the place of an atom of water: four volumes of the vapour of water are replaced by four volumes of sulphurous acid gas. In the conversion of subcarbonate of ammonia into

into neutral carbonate, eight volumes of ammoniacal gas are necessarily replaced by the same number of volumes of the vapour of water.—*Hensman's Repertoire de Chimie, &c.*

SEPARATION OF SELENIUM FROM SULPHUR.

According to Berzelius these substances may be readily separated by the following process. When sulphuret of selenium is fused with carbonate of potash, the alkali not being in excess, the fused mass dissolved in water, leaves selenium undissolved and free from sulphur. Some of the sulphuret of selenium from Lukawitz in Bohemia was dissolved in potash, and the solution was converted into hyposulphite by exposure to the air at the temperature of 65° Fahr., 11¼ hundredths of the sulphuret submitted to experiment were precipitated, and found to be pure selenium. The solution being of a deeper red colour than that of the common sulphuret, a piece of sulphur was put into it, and it was boiled for a moment: a quarter of a grain of selenium was precipitated, and perfectly free from sulphur.

A solution of a neutral seleniate, or of one with excess of base, is soon rendered turbid by having sulphuretted hydrogen gas passed into it. At first pure selenium separates; afterwards sulphuret of selenium: and lastly, mere sulphur;—the solution should be considerably dilute. When the solution is concentrated, the precipitate formed is of a flame-yellow colour; but it soon becomes of a brownish-black colour; and sulphur is deposited, and sometimes crystallized at the surface of the deposit.—*Ibid.*

STEARIC ACID FROM WAX.

M. Frommherz, apothecary of Fribourg, observed that some oil obtained by the distillation of wax which had been kept in a cold place, deposited small brilliant pearly laminæ; they were repeatedly washed with alcohol to separate the oil, and were found to be stearic acid.

Supposing that the substance called butter of wax might also be stearic acid, some was prepared by distilling white wax. A white matter was collected in the neck of the retort, which was repeatedly washed with boiling water, and then dissolved in alcohol. By cooling, small shining slender acicular crystals were obtained, which were lighter than water, fusible at 130° of Fahr., inodorous, and acid.

The alcohol in which these crystals were deposited did not contain any oleic acid; and it is to be observed, that if the heat do not exceed that required for the distillation of the wax, no sebacic acid is formed; and the product contains only stearic acid mixed with empyreumatic oil. Sebacic acid is formed at a higher temperature.

The absence of oleic acid in the products of the distillation of wax would appear to be a distinctive character of it; and it will occur in fatty bodies, in the inverse proportion of their consistence.—*Ibid.*

EXTRICATION OF HEAT BY COMPRESSION OF GASES.

Mons. D. Colladon finds that to ignite amadou, atmospheric air must be reduced to one-thirteenth of its volume; and to inflame sulphur,

phur, to one-eighteenth. Chlorine by compression gives a weak violet-coloured light.

M. Colladou has also ascertained by means of an instrument which he invented; first, That the alteration of temperature is not exactly proportional to the change of volume which gases suffer: secondly, That when the volume of the gas is inconsiderable, the increase of temperature indicates the diminution of volume: thirdly, That a dilated gas is more heated by compression than one which is compressed.—*Ibid.*

METAL OF ALUMINA.

M. Oersted is stated to have obtained the metal of alumina by employing the chloride of that earth. Pure alumina is heated to redness, and then intimately mixed with powdered charcoal: the mixture is introduced into a porcelain tube; and after heating to redness, dry chlorine gas is passed over it. The charcoal reduces the alumina, the metal combines with the chlorine, and oxide of carbon is also formed. The chloride of aluminum is soft, crystalline, and evaporates at a little above the temperature of boiling water; it readily attracts moisture from the air, and becomes hot when water is added to it. By mixing with an amalgam of potassium, containing much of the latter, and immediately heating the mixture, chloride of potassium is formed, and the metal of the alumina combines with the mercury. The amalgam quickly oxidizes by exposure to the air. Being subjected to distillation out of the contact of air, the mercury is volatilized, and a metallic button is left, which has the colour and splendour of tin. M. Oersted has ascertained many properties belonging to the new metal and its amalgam, which he promises to publish speedily.—*Ibid.*

NEW METALS IN THE URALIAN PLATINA.

The discovery of these metals by Professor Osann of Dorpat is announced as follows, in Hensman's *Repertoire de Chimie* for September last.

"I have discovered in the platina of the Uralian mountains three metals, the properties of which are different from those of every other known metal. One of them occurs in the residuum left by the solution of the platina in aqua-regia, which is sold at the Mint in Petersburgh. I have as yet found it only in one specimen of the metal.

"The oxide crystallizes in long prisms from the nitro-muriatic solution of the platina: these crystals sublime without undergoing any change, but at a higher temperature than required for the sublimation of oxide of osmium. Subjected to the blowpipe a portion of the salt sublimes, while another is reduced to a globule of metal. Sulphuret of ammonia converts the reduced metal to a gray sulphuret, which readily fuses, and burning in the air, it is converted into oxide.

"The second metal is found in the nitro-muriatic solution of the same platina; it possesses the following properties. The solution yields white acicular crystals, which soften in the heat of melting glass, and are reduced to the metallic state. Hydrogen reduces it to a metal of a gray colour with a tint of red: this metal did not melt, but retained the crystalline form of the oxide. Aqua-regia readily

readily dissolves it, and sulphuret of ammonia precipitates it of a brown colour: the precipitate being roasted in contact with air, becomes of a blackish-brown colour. These two metals are found in very small quantity in the Uralian platina, the latter in greater proportion than the former.

"The third metal is also found in the nitro-muriatic solution of platina: this metal possesses the singular property of forming an alloy with iron, which is not decomposable by nitric acid. By fusing this alloy with caustic potash and nitrate of potash, the iron is sufficiently separated to be taken up by nitric acid; the residuum after this separation is a dark green coloured powder, and is the oxide of the metal. When put upon a piece of platina and heated to whiteness, the powder is blackened but not reduced; but when exposed to the flame of the blowpipe, it becomes a metallic mass of considerable lustre. The metal thus obtained has the following properties: It is insoluble in nitro-muriatic acid, even when heated; when fused with caustic potash and nitre, it yields a brownish coloured mass, which softened by water deposits a gray powder still retaining some lustre; the alkali dissolves nothing, and the powder is merely the metal in a divided state, in which aqua-regia attacks it slowly, and converts it eventually into green oxide. By directing a current of hydrogen gas upon the heated oxide, combustion resembling that of gunpowder ensues; and a blackish powder is formed, which by the long re-action of the hydrogen is completely reduced. The metal thus reduced has a gray colour, nearly resembling recently formed spongy platina. When heated in contact with the air it becomes black, and continues so even at a white heat: it differs in this respect from rhodium, which is oxidated by heat, and at a higher temperature is again reduced."

NEW ALKALI IN HEMLOCK.

Professor Ficinus of Dresden has discovered a new alkali in the *Æthusa Cynapium* (*Linn.*) to which he has given the name of Cynopia. It is crystallizable, and soluble in water and alcohol, but not in æther. The crystals are in the form of a rhombic prism, which is also that of the crystals of the sulphate.—*Hensman's Repertoire.*

HEAT EVOLVED BY COMPRESSING WATER.

M. Despretz has ascertained that when water is compressed by a force equal to 20 atmospheres, that one sixty-sixth part of a degree of heat is evolved.—*Royal Institution Journal, Oct.* 1827.

IODOUS ACID.

According to M. Wohler, the iodous acid of M. Sementini is nothing more than a mixture of chloride of iodine and iodine. When saturated with carbonate of soda, the iodine in solution is precipitated, and on evaporating to dryness and heating it strongly the residue fuses, and by proper tests is found to be a mixture of chloride and iodide of sodium.

These statements apply only to the iodous acid: as to the oxide of iodine, no source of chlorine exists in the process last described by M. Sementini.—*Ibid.*

MANGANESIC

MANGANESIC ACID AND MANGANESATE OF POTASH.

M. Unverdorben finds that when manganesate of potash is distilled with a little anhydrous sulphuric acid, that manganesic acid is evolved in the form of a red transparent gas, which dissolves in water, forming a red solution. The gas frequently decomposes spontaneously in the retort, with explosion, producing oxide of manganese and oxygen.

Manganesate of potash was analysed by distilling it with excess of sulphuric acid, collecting the oxygen disengaged, and estimating the proportion of protoxide of manganese and salts of potash remaining in the retort. According to these experiments the acid consists of

Manganese	58·74	
Oxygen	41·26	
		100

And the manganesate of potash, of

Potash	25·63	
Manganesic acid	52·44	
Water	21·93	
		00·00

Ann. des Mines, 1827, p. 145.

The composition of a manganesic acid as above stated, differs very materially from that assigned by Dr. Thomson in his "Attempt, &c." It is there mentioned as consisting of

1 atom of manganese	28 or	46·66
4 atoms of oxygen	32 or	53·33
	60	99·99

Edit.

INDELIBLE WRITING-INK.

The following, recommended as a process for preparing indelible writing-ink, or at least as a sort of approximation to it,—is copied from the last Number of the Royal Institution Journal: "Let a saturated solution of indigo and madder in boiling water be made in such proportion as to give a purple tint; add to it from one-sixth to one-eighth of its weight of sulphuric acid, according to the thickness and strength of the paper to be used: this makes an ink which flows pretty freely from the pen, and when writing which has been executed with it, is exposed to a considerable but gradual heat from the fire, it becomes completely black, the letters being burnt in and charred by the action of the sulphuric acid. *If the acid has not been used in sufficient quantity to destroy the texture of the paper, and reduce it to the state of tinder, the colour may be discharged by the oxymuriatic and oxalic acids and their compounds, though not without great difficulty.* When the full proportion of acid has been employed, a little crumpling and rubbing of the paper reduces the carbonaceous matter of the letters to powder; but by putting a black ground behind them they may be preserved, and thus a species of indelible writing-ink is procured, (for the letters are, in a manner, stamped out of the paper,) which might be useful for some purposes, perhaps for the signature of bank notes."

DRACINE,—A NEW SUBSTANCE FOUND IN DRAGON'S-BLOOD.

M. Melandri finds that the colouring matter of Dragon's-blood is soluble in alcohol and oil, and also in hot water, but in small proportion: the aqueous solution is bitter, astringent, and of a fine purple colour; by cooling, it becomes opake and red. By the tests of gelatine and sulphate of iron, it does not appear to contain either tannin or gallic acid.

A portion of dragon's-blood was dissolved in strong alcohol, the solution was evaporated till it was much concentrated, and then poured into cold water, but in which a spongy mass was precipitated; this after being washed with cold water was saturated with water containing $\frac{1}{100}$ of sulphuric acid, and at about 61° of Fahr. chemical action appeared to occur. The sediment, being well washed with water, was of a fine red colour, varying according to the state of aggregation; it was tasteless and inodorous, flexible, and became fluid at 131° Fahr. This substance, called by the discoverer *Dracine*, has some analogy with the vegeto-alkalis, although its affinity for acids is but slight. The sulphate is obtainable by adding sulphuric acid diluted with alcohol to an alcoholic solution of dracine, precipitating the mixture by cold water, and then applying a little heat; the sulphate of dracine collects at the bottom, and is to be washed with cold water until litmus-paper ceases to be reddened by the washings; it is then to be dissolved in hot water. The solution is reddened by the smallest quantity of alkalies, and may be used as a very sensible test of their presence; dracine is also a good test for acids, being rendered yellow by them. The smallest quantity of carbonate of lime in filtering paper may be detected by sulphate of dracine, the yellow solution instantly becoming red by its action.—*Bull. Univ.* c. xi. p. 157.

ORGANIC REMAINS OF THE ALLUVIUM AND DILUVIUM IN SUSSEX.

In the alluvial and diluvial deposits of Sussex the remains of animals hitherto discovered are very few, compared with those found in other counties of England. Mr. Mantell mentions but two kinds as having been noticed, (Geology of Sussex, p. 284,) viz.: the elephant, and horse. A short time since some labourers who were employed in deepening the bed of the river Ouse, which flows through a chalk valley by Lewes, and empties itself into the sea at Newhaven, discovered in a bed of sand beneath the blue alluvial clay that forms the marshy tract called Lewes levels, the entire skeleton of a deer of a very large size. The horns were quite perfect, and measured 3 feet in height, and 3 feet 2 inches at their greatest width. The antlers had seven points, and resembled in their form those figured by Cuvier, of the Canadian deer. The greater part of the skeleton was destroyed by the carelessness of the workmen, and a few bones only preserved. Of these the tibia measures $14\frac{1}{2}$ inches in length, and the ulna 15 inches to the end of the oluranon. The ramus of the lower jaw (imperfect) 11 inches. These remains are in Mr. Mantell's collection at Castle Place, Lewes. Still more recently bones of the deer

deer have been found in the diluvial gravel that forms the low line of cliffs to the west of Brighton, at Copperas Gap, near Southwick. These, like all the other bones that have been discovered in this bed, were broken, and promiscuously intermingled with the soil. Two teeth of a species of deer, and portions of several humeri, were identified. Part of the tusk of an elephant was also found with them, and pebbles of granite, in a state of decomposition. Teeth of the Asiatic elephant have been met with in the loam-pits at Hooe.—The Rev. H. Hoper of Pontslade has these interesting remains in his possession.

SCIENTIFIC BOOKS.

Just Published.

The Enigmatical Entertainer and Mathematical Associate for 1828.

Preparing for Publication.

A Third Edition of Mr. Bakewell's Introduction to Geology, greatly enlarged, will be published early in January next. This work will contain all the recent discoveries in geology, and numerous geological observations made by the author in various parts of Great Britain and the Continent of Europe since the publication of the last edition.

AURORA BOREALIS.

Gosport Observatory, Sept. 26, 1827.

At nine o'clock last evening a bright yellow light appeared in the N.W. quarter, behind a low stationary *cirrostratus* cloud, and gradually extended from N. to W.N.W.: it continued to increase in altitude and width, and at ten had a brighter appearance than the strongest crepuscule that appears in this latitude in a clear sky about the time of the summer solstice; but neither lucid columns of light nor coruscations yet presented themselves. At half-past ten the aurora had formed itself into a tolerably well-defined arc of intense light, whose base extended from N. to W.; and at a quarter before eleven, perpendicular lucid columns and vivid coruscations of this subtile fluid appeared in quick succession. So brilliant was the aurora at eleven, that the streamers reached 8 or 9 degrees higher than *Polaris*, and their apparent base was nearly horizontal with the star *Beta* in Ursa Major. At this time the coruscations, which appeared to spring up from a much greater northerly distance than the columns were, reached to the constellation Cassiopeia, which was nearly in the zenith. Soon after eleven, a column of light six degrees in width gradually rose from the position of the before-mentioned star, and when it had reached an altitude of 70 degrees, it changed from a light yellow to a blood-red colour, which with the more elevated and vivid flashes that frequently reached twenty degrees South of the zenith, gave the aurora an awfully grand appearance, which it would be difficult to paint or express. This wide column remained perfect upwards of an hour, alternately waving and increasing in brilliancy, and ultimately passed through the gradation of colours which is sometimes seen in the clouds near the horizon at sunset, as lake, purple, light crimson, &c.: it became

 apparently

apparently stationary in the N.E. by E. point, and its eastern red edge was very well defined in the dark blue sky. Two more columns of light nearly similar in colour and width soon afterwards sprang up, one in due North, the other in N.W., and passed the zenith several degrees to the southward: these three large variegated columns presented a very grand appearance.

At half-past eleven the aurora suddenly changed to light red; and from about this time till twelve o'clock the apex of the arc of light was within four or five degrees of the polar star; consequently the hemisphere from N.E. by E. to S.W. by W. was so brilliantly illuminated as to appear like the reflection of a great conflagration, whilst the white coruscations which flashed through the atmosphere quicker than sheet-lightning in sultry summer evenings, formed whole but irregularly-shaped arches from these points of the horizon through the zenith nearly. At one A.M. lofty perpendicular columns emanated from the aurora in the western point; and at this time the northern hemisphere was filled with long and short streamers varying in width and brilliancy, and often terminating in very pointed forms. The coruscations from the N.E. and W. frequently met each other in the zenith, and enlightened the scattered portions of *cirrostratus* even to within thirty degrees of the southern horizon; and from the clouds being stationary, it is probable that the atmosphere was serene and undisturbed in their vicinity. Soon after 2 A.M. the aurora grew faint and gradually disappeared. The lustre of the stars of the first, second, and third magnitude was very little diminished in any part of the heavens where the vivid flashes of the aurora intervened. The diffusion of the coruscations through the atmosphere caused twelve accensions or meteors to appear at intervals in different quarters, but most of them were to the northward; and it also had the effect of increasing the temperature of the external air near the ground half a degree between the hours of observation, notwithstanding the wind blew fresh from the South. This was the finest aurora borealis that has been observed here during the last seventeen years. In sixteen hours after its disappearance, heavy rain and a gale of wind came on from S.E. by E. (to which quarter the coruscations mostly tended); the common result here of the diffusion of a superabundance of electrical fluid in the lower atmosphere. An aurora borealis of extraordinary beauty is reported to have been seen all over Denmark in the night of the 8th instant, while the moon shone in full splendour.

LIST OF NEW PATENTS.

To Joseph Hall, and Thomas Hall, of Leeds, for an improvement in the making of metallic blocks for drawing off liquids.—Dated the 11th of October 1827.—2 months allowed to enrol specification.

To Elias Carter, of Exeter, for a new covering for the roofs of houses, &c.—11th of October.—6 months.

To Joshua Horton, of West Bromwich, boiler-maker, for a new method of forming and making of hollow cylinders, guns, ordnance retorts,

retorts, and various other hollow and useful articles in wrought iron, in steel, or composed of both those metals.—11th of October.—6 months.

To Goldsworthy Gurney, of Argyle-street, Hanover-square, surgeon, for improvements in locomotive engines, and other apparatus connected therewith.—11th of October.—6 months.

To James Stokes, of Cornhill, London, for improvements in making, boiling, burning, clarifying, or preparing raw or Muscovado bastard sugar and molasses.—11th of October.—6 months.

To John Wright, of Princes-street, Leicester-square, for improvements in window-sashes.—11th of October.—6 months.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·34 Sept. 1. Wind NE.—Min. 29·50 Sept. 23. Wind SW.
Range of the mercury 0·84.
Mean barometrical pressure for the month . . . . 29·986
Spaces described by the rising and falling of the mercury . . . 3·700
Greatest variation in 24 hours 0·460.—Number of changes 30.
Therm. Max. 73° Sept. 18. Wind W.—Min. 49° Sept. 20. Wind NE.
Range 24°.—Mean temp. of exter. air 60°·77. For 30 days with ☉ in ♍ 60·53
Max. var. in 24 hours 24°·00—Mean temp. of spring water at 8 A.M. 55°·25

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 27th . . . 100°
Greatest dryness of the air in the afternoon of the 2nd 40
Range of the index 60
Mean at 2 P.M. 54°·6—Mean at 8 A.M. 64°·6—Mean at 8 P.M. 67·5
—— of three observations each day at 8, 2, and 8 o'clock . . 62·2
Evaporation for the month 2·85 inch.
Rain near ground 3·835 inch.—Rain 23 feet high 3·545 inch.
Prevailing Wind NE.

Summary of the Weather.

A clear sky, $3\frac{1}{2}$; fine, with various modifications of clouds, 14; an overcast sky without rain, 6; foggy $\frac{1}{2}$; rain, 6.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
18	10	28	3	24	26	18

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	8	1	3	4	6	2	4	30

General Observations.—The first part of this month to the 8th was fine, calm and dry, with a constant North-east wind; and the latter part was very generally wet, chiefly by night, with frequent fluctuations in the mercurial column of the barometer: but although the changes for this period amount to 30, yet the aggregate of the spaces described by the motion of the mercury is only $3\frac{7}{10}$ inches.

The mean temperature of the external air this month is nearly a degree higher than the mean of September for the last eleven years.

At six minutes before 9 o'clock on the 1st instant, a large pear-shaped light red meteor appeared from the N.W. It commenced its flight from between the stars *Alpha* and *Beta* in the constellation *Hercules*, and burst in in several parts between *Aquila* and *Delphinus*, similar to the bursting of a charged bomb. Its motion was comparatively slow, perhaps from its great altitude, as it was about five seconds of time in moving through a space of 56 degrees. Its light was so strong as to be distinguished on the ground from that of the moon, whose age was between ten and eleven days.

On the morning of the 11th a parhelion appeared at 8 o'clock on the eastern side of, and 22° 48′ distant from, the sun's centre, just without the edge of a faint solar halo; there were several modifications of clouds about at that time, and the *cirrocumuli* in round flocks ultimately changed from a silvery colour to a black electrical appearance; and the first of the equinoctial gales followed after sunset.

On the 12th, 16th, 18th, 23rd, 25th, and 30th, solar lights and shades were very conspicuous; they were sometimes produced by reflecting in diverging forms from the surfaces of dense *cumulostratus* clouds, at other times by passing through their apertures and most attenuated parts; but immediately after these clouds had passed the sun's disc, the lights and shades disappeared.

From these appearances, together with those last month, both solar lights and shades are evidently produced by the rays passing through the openings in the clouds, and also by reflection upwards or downwards, according to their position, from their dense surfaces, which bar the passage of the direct rays: they may be also produced by dense lofty imperceptible vapours, and perhaps by thick clouds of dust at a considerable height from the ground.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one parhelion, one solar, and one lunar halo, twenty meteors, one aurora borealis on the 25th, four rainbows, lightning on the 6th and on the 28th, with passing thunder-showers on the latter day; and five gales of wind, or days on which they have prevailed, namely, one from N., one from S.E., and three from the S.W.

REMARKS.

London.—Sept. 1. Cloudy and fine. 2—8. Fine. 9. Cloudy: rain began at 7 P.M. 10. Showery. 11. Cloudy. 12. Rainy. 13. Showery. 14—17. Fine. 18. Rainy evening. 19. Fine. 20. Cloudy. 21, 22. Fine. 23, 24. Showery. 25. Fine. 26. Morning fine, wet evening. 27, 28. Fine. 29. Rainy. 30. Fine.

Penzance.—Sept. 1. Fair. 2—4. Clear. 5, 6. Fair. 7. Clear. 8. Fair. 9—11. Rain. 12. Clear: showers. 13. Clear. 14—18. Fair. 19. Fair: misty. 20. Fair. 21. Rain. 22, 23. Clear: showers. 24. Showers. 25. Rain. 26. Fair: showers. At about 9 P.M. there was a luminous appearance in the north-east, which soon afterward proved to be a brilliant aurora borealis. 27. Fair. 28, 29. Fair: showers. 30. Fair.—Rain guage ground level.

Boston.—Sept. 1, 2. Fine. 3—9. Cloudy. 10. Cloudy: rain, P.M. 11. Fine: rain, P.M. 12. Fine. 13. Fine: rain, A.M. 14. Cloudy. 15. Fine. 16, 17. Cloudy. 18. Cloudy: rain, P.M. 19. Fine. 20. Cloudy: rain, A.M. 21. Cloudy. 22. Fine: rain, A.M. 23—25. Fine. 26. Fine: rain, P.M. with rainbow. 27. Fine. 28. Foggy. 29. Fine: rain, A.M. 30. Fine.

RESULTS.

Winds, NE. 9: E. 1: SE. 4: S. 6: SW. 8: NW. 2.

London.—Barometer: Mean height for the month	30·13 inch.
Thermometer: Mean height for the month	59·05°
Evaporation	2·40 inch.
Rain	3·26.

Meteoro-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.										
Sept. 1	30·50	30·48	30·04	30·04	30·34	30·28	29·85	74	44	66	53	70	54	59	E.	NE.	NE.	E.	...	...	...	...	...	...
2	30·48	30·46	30·02	30·00	30·31	30·27	29·80	75	45	66	54	71	52	58·5	SE.	NE.	NE.	N.	...	...	...	...	...	...
3	30·46	30·45	30·00	30·00	30·30	30·28	29·76	75	45	67	56	68	54	59	NE.	SE.	NE.	calm	...	0·65	...	...	...	...
4	30·45	30·38	30·15	30·10	30·30	30·26	29·76	64	44	63	54	64	55	56	NE.	SE.	NE.	calm	...	...	...	...	...	...
○ 5	30·42	30·38	30·10	30·10	30·23	30·22	29·76	73	45	63	51	68	52	58·5	NE.	E.	NE.	calm	...	...	...	...	...	...
6	30·42	30·41	30·10	30·10	30·28	30·26	29·80	74	43	64	51	68	50	56·5	NE.	E.	NE.	calm	...	·45	...	...	...	...
7	30·41	30·40	30·10	30·10	30·27	30·24	29·75	68	43	64	53	68	55	57	NE.	E.	NE.	NE.	...	...	...	...	...	...
8	30·40	30·14	30·00	29·80	30·24	30·17	29·68	69	54	62	53	69	58	57·5	NE.	SE.	NE.	calm	...	...	...	...	...	...
9	30·14	30·07	29·80	29·78	30·00	29·94	29·42	73	53	64	54	69	57	61	S.	SW.	SE.	W.	...	·40	0·84	...	0·660	...
10	30·07	29·99	29·78	29·70	29·92	29·90	29·22	74	55	64	56	72	62	60	SW.	SW.	SW.	W.	...	...	·03	0·600	·080	0·35
11	29·99	29·89	29·68	29·60	29·83	29·78	29·11	74	57	66	56	73	57	67	SW.	SW.	S.	SW.	·97	...	·11	0·805	·175	·09
12	30·06	29·89	29·78	29·58	29·82	29·72	29·05	59	52	62	54	67	51	61·5	SW.	W.	SW.	W.	...	·15	·23	0·120	·160	·09
☾ 13	30·35	30·06	30·02	29·90	30·15	29·94	29·32	64	47	62	52	64	50	56·5	NW.	NW.	NW.	NW.	...	...	·05	0·155	·025	...
14	30·40	30·35	30·16	30·12	30·25	30·24	29·55	69	49	62	51	68	59	56·5	SW.	W.	SW.	W.	...	...	...	...	...	·08
15	30·43	30·40	30·20	30·20	30·32	30·28	29·65	69	55	62	53	71	58	62	SW.	N.	NW.	NW.	...	·30	...	...	...	...
16	30·45	30·43	30·20	30·20	30·31	30·28	29·67	74	56	64	56	71	57	64·5	NE.	N.	N.	calm	...	...	...	...	...	...
17	30·45	30·40	30·20	30·18	30·32	30·28	29·72	74	54	68	59	69	60	62	NE.	N.	W.	E.	...	...	...	...	...	...
18	30·40	30·30	30·12	30·12	30·26	30·21	29·58	71	48	65	58	73	49	61	S.	NW.	NW.	calm	...	·20	·15	...	·020	...
19	30·30	29·95	30·12	30·10	30·24	30·20	29·55	60	45	62	53	61	51	52	NW.	NW.	NW.	NW.	...	...	·30	...	·620	·08
20	29·99	29·95	29·90	29·90	29·90	29·78	29·38	60	44	62	54	65	49	47	NE.	NW.	N.	S.	...	...	·01	...	...	·20
● 21	29·95	29·79	29·80	29·70	29·84	29·67	29·25	67	54	64	55	67	54	52	S.	W.	SW.	W.	...	·10	...	...	·140	...
22	29·79	29·72	29·52	29·50	29·59	29·53	29·05	72	45	56	49	62	51	53	SW.	W.	SW.	NW.	...	...	·20	0·335	·310	...
23	29·79	29·77	29·40	29·40	29·61	29·50	29·00	67	44	58	48	61	50	52·5	SW.	SW.	SW.	W.	·95	...	·35	0·160	·260	·02
24	29·86	29·79	29·54	29·44	29·70	29·66	29·17	67	47	60	49	64	52	56	S.	SW.	S.	NW.	...	·25	·10	0·120	·320	·08
25	29·79	29·78	29·54	29·40	29·70	29·64	29·15	68	48	58	49	64	55	56	SW.	S.	S.	W.	...	...	...	0·320	...	...
26	29·79	29·78	29·46	29·40	29·60	29·57	29·11	72	57	60	52	67	59	59	S.	SE.	SE.	S.	...	...	·49	...	·300	...
27	29·86	29·79	29·60	29·56	29·71	29·57	29·07	72	49	62	49	68	56	63	SE.	N.	SE.	E.	...	·15	...	...	...	·32
☽ 28	29·86	29·82	29·60	29·54	29·70	29·67	29·15	67	47	60	55	66	56	59	S.	NW.	SE.	calm	...	...	...	...	·725	...
29	29·92	29·86	29·64	29·62	29·70	29·56	29·16	63	46	58	54	66	50	60	SE.	NW.	W.	E.	...	...	·40	...	·040	...
30	29·94	29·92	29·74	29·74	29·78	29·76	29·17	69	51	58	50	64	55	59	SE.	S.	N.	W.	·48	·20	...	0·205	...	·4
Aver.:	30·50	29·72	30·20	29·40	30·34	29·50	29·42	75	43	68	48	73	49	58					2·40	2·85	3·26	2·820	3·835	1·27

Calendar of the Meetings of the Scientific Bodies of London for 1827–8.

Societies.	Time of Meeting.	November.	December.	January.	February.	March.	April.	May.	June.
Royal . . . . Somerset-House.	Thursday, $8\frac{1}{2}$ P.M.	15,22,30*	6, 13, 20	10,17,24,31	7, 14, 21, 28	6,13,20,27	17, 24	1, 8, 15, 22	5, 12, 19
Antiquaries . Somerset-House.	Thursday, 8 P.M.	15, 22, 29	6, 13, 20	10,17,24,31	7, 14, 21, 28	5,13,20,27	17, 28*	1, 8, 15, 22	5, 12, 19
Linnæan . . Soho-Square.	Tuesday, 8 P.M.	6, 20	4, 18	15	5, 19	4, 18	1, 15	6, 24*	3, 17
Zoolog. Club Soho-Square.	Tuesday, 8 P.M.	13,27,29*	11	8, 22	12, 26	11, 25	8, 22	13, 27	10, 24
Horticultural Regent-Street.	Tuesday, 1 P.M.	6, 20	4, 18	1, 15	5, 19	4, 18	1, 15	1*, 6, 20	3, 17
Geological . . Bedford-St. Co.G.	Friday, 8 P.M.	2, 16	7, 21	4, 18	1, 15*	7, 21	18	2, 16	6, 20
Astronomical Lincoln's Inn-Fds.	Friday, 8 P.M.	9	14	11	8*	14	11	9	13
Royal Institut. Albemarle-St.	Friday, $8\frac{1}{2}$ P.M.	...	...	25	1,8,15,22,29	7,14,21,28	18, 25	2,9,16,23,30	6, 13
Royal Asiatic Grafton-Street.	Saturday, 2 P.M.	3, 17	1, 15	5, 19	2, 16	1, 15*	19	3, 17	7, 21
Royal Society of Literature Parliament-St.	Wednesday, 3 P.M.	7, 21	5, 19	2, 16	6, 20	5, 19	2, 16, 24*	7, 21	4, 16

* ANNIVERSARIES.—Royal, Nov. 30, 3 P.M.—Antiquaries, April 28.—Linnæan, May 24, 1 P.M.—Horticultural, May 1.—Geological, Feb. 15, 2 P.M. —Astronomical, Feb. 8, 3 P.M.—Asiatic, March 15, 1 P.M.—Royal Society of Literature, April 24.—Zoological Society, April 29, 1 P.M.

[Copies of the Calendar on Cards may be had at the Office of the Philosophical Magazine and Annals, Red Lion Court.]

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

DECEMBER 1827.

LXV. *On the Dispersion of Light. By* M. RUDBERG, *of Stockholm.**

IN investigating the principle, according to which, for the explanation of the dispersion of light on the system of undulations, we must suppose that when the light passes from the air into a more refractive medium, the lengths of the undulations are much contracted, in fact much shorter,—I have found that the following relation appears to exist between the length of the undulation of a certain colour in the air, and the corresponding one in any other substance:

$$L = a \, . \, l^m;$$

l being the length of the undulation in the air, and L that in another substance; and lastly, a, m two numbers which vary only with the nature of the substance.

I have taken the lengths of the undulations in the air as determined by M. Fraunhofer, (Gilbert's *Annals*, 1823, p. 337) viz.

In the red,	C	=	0·00002422 of an inch.
orange,	D	=	0·00002175
green,	E	=	0·00001945
blue,	F	=	0·00001794
indigo,	G	=	0·00001587
violet,	H	=	0·00001464

The lengths of the undulations in other substances being always in the inverse ratio of the indices of refraction, I have inserted them in the first column of each of the following tables, according to the indices of refraction which M. Fraunhofer has published, in his Memoir on the determination of refringent and dispersive power (Schumacher's *Astronomische Abhandlungen*, 1823).

* Communicated by the Author.

The comparison of the observed with the calculated lengths is shown in the following tables:

(Schumacher, *Astronom. Abhandl.* 1823. p. 31.)

Flint-glass. No. 13.

$L = 1{\cdot}044.l^{1{\cdot}05.}$

	Observed lengths.	Calculated lengths.
C	0·00001486	....
D	0·00001330	0·00001328
E	0·00001184	0·00001181
F	0·00001088	0·00001084
G	0·00000955	0·00000953
H	0·00000876	....

Crown-glass. No. 9.

$L = 0{\cdot}87045.l^{1{\cdot}0267.}$

	Observed lengths.	Calculated lengths.
C	0·00001587	....
D	0·00001422	0·00001420
E	0 00001268	0·00001267
F	0·00001168	0·00001166
G	0·00001029	0 00001028
H	0·00000946	....

Flint-glass. No. 23.

$L = 1{\cdot}040.l^{1{\cdot}0496.}$

	Observed lengths.	Calculated lengths.
C	0·00001487	....
D	0·00001331	0·00001328
E	0·00001185	0·00001181
F	0·00001089	0·00001085
G	0·00000956	0·00000954
H	0·00000876	....

Oil of Turpentine.

$L = 0{\cdot}93479.l^{1{\cdot}0299.}$

	Observed lengths.	Calculated lengths.
C	0·00001646	....
D	0·00001476	0·00001475
E	0·00001315	0·00001313
F	0·00001210	0·00001208
G	0·00001066	0·00001065
H	0·00000980	....

Flint-glass. No. 3.

$L = 1{\cdot}00218.l^{1{\cdot}0446.}$

	Observed lengths.	Calculated lengths.
C	0·00001510	....
D	0·00001352	0·00001350
E	0·00001204	0·00001201
F	0·00001107	0·00001104
G	0·00000973	0·00000971
H	0·00000892	....

Crown-glass. No. 13.

$L = 0{\cdot}85515.l^{1{\cdot}025.}$

	Observed lengths.	Calculated lengths.
C	0·00001587	....
D	0·00001423	0·00001422
E	0 00001270	0·00001268
F	0·00001169	0·00001167
G	0·00001030	0·00001029
H	0·00000947	....

Flint-glass. No. 30.

$L = 1{\cdot}0356.l^{1{\cdot}049.}$

	Observed lengths.	Calculated lengths.
C	0·00001490	....
D	0·00001334	0·00001331
E	0·00001187	0·00001184
F	0·00001091	0·00001088
G	0·00000958	0·00000956
H	0·00000878	....

Water.

$L = 0{\cdot}91343.l^{1{\cdot}0184.}$

	Observed lengths.	Calculated lengths.
C	0·00001818	....
D	0·00001631	0·00001630
E	0·00001456	0·00001454
F	0·00001341	0·00001340
G	0·00001183	0·00001182
H	0·00001089	....

Although

Although the differences between the calculated and the observed lengths are very small, they are not without influence when we wish to pass from the lengths of undulation to the indices of refraction. But the lengths of undulation in the air not being themselves certainly accurate to a thousandth, and the indices of refraction having a precision at least ten times greater, I have taken the relation between the indices such as it is derived from the supposed ratio to the lengths of undulation, and have then compared it with observation. Calling the index of refraction N, we have:

$$N = \frac{1}{a\,l^{m-1}}$$

in which, if $N'\,l'$ belong to another ray:

$$\frac{N}{N'} = \left(\frac{l'}{l}\right)^{m-1}$$

In another substance, if n, n' are the indices corresponding to the same lengths l, l', we shall have: $n = \frac{1}{b\,l^{p-1}}$, and also $\frac{n}{n'} = \left(\frac{l}{l'}\right)^{p-1}$, whence, consequently, if $r = \frac{m-1}{p-1}$:

$$\frac{N}{N'} = \left(\frac{n}{n'}\right)^r.$$

In which equation r ought to be constant. In taking for N the indices of flint-glass, No. 13, and for n the indices of water, we find, For the rays B and C : $r = 2{\cdot}0430$

B and D : $r = 2{\cdot}2928$

B and E : $r = 2{\cdot}3802$

B and F : $r = 2{\cdot}4203$

B and G : $r = 2{\cdot}5476$

B and H : $r = 2{\cdot}6402$.

These values are not precisely the same: the difference between them however has not a great influence in the present case, a small variation in the indices producing a considerable one in r; if, for example, instead of the index of the ray C in flint-glass, which $= 1{\cdot}6297$, $1{\cdot}6298$ be taken, we have $r = 2{\cdot}1452$ instead of $2{\cdot}0430$; also in taking the mean $= 2{\cdot}39$ of all these values, and calculating the indices of flint-glass according to those of water, according to the formula:

$$N = N'.\left(\frac{n}{n'}\right)^{2{\cdot}39}$$

we have the second column of this table:

Observation.	Calculation.	Observation.	Calculation.
B 1·6277	....	F 1·6482	1·6479
C 1·6297	1·6300	G 1·6602	1·6582
D 1·6350	1·6354	H 1·6710	1·6669
E 1·6420	1·6420		

A similar

A similar calculation for the same flint-glass, and the crown-glass, No. 13, gives the following values of r:

According to the rays

B and C : $r = 1{\cdot}8718$	B and F : $r = 1{\cdot}9140$
B and D : $r = 1{\cdot}8970$	B and G : $r = 1{\cdot}9475$
B and E : $r = 1{\cdot}9090$	B and H : $r = 1{\cdot}9748$

Mean value $r = 1{\cdot}919$.

And calculating by the formula $N = N' \left(\frac{n}{n'}\right)^{1{\cdot}919}$

we have for the indices of flint-glass the following values:

C = 1·6298	F = 1·6483
D = 1·6351	G = 1·6598
E = 1·6421	H = 1·6698

Also in comparing, finally, the flint-glass, No. 13, with the crown-glass, Lett. M, we find the subjoined values of r:

According to the rays

B and C : $r = 1{\cdot}5118$	B and F : $r = 1{\cdot}6277$
B and D : $r = 1{\cdot}5833$	B and G : $r = 1{\cdot}6498$
B and E : $r = 1{\cdot}6233$	B and H : $r = 1{\cdot}6590$

Mean value $r = 1{\cdot}609$.

And calculating the indices of flint-glass by the formula:

$$N = N' \left(\frac{n}{n'}\right)^{1{\cdot}61} \text{ we obtain:}$$

C = 1·6297	F = 1·6480
D = 1·6351	G = 1·6595
E = 1·6419	H = 1·6697

Supplement.

If we calculate the indices of refraction of flint-glass, No. 13, according to the formula:

$$N = 1{\cdot}6277 \left(\frac{n}{n'}\right)^{2{\cdot}6}$$

in which the exponent 2·6 is such, that it belongs to the rays B, and the rays H, in flint-glass and in water, which rays, being the most distant from each other, afford the most certain result, we have the second column of the following table:

Observation.	Calculation.	Observation.	Calculation.
B 1·6277	....	F 1·6482	1·6497
C 1·6297	1·6302	G 1·6602	1·6609
D 1·6350	1·6357	H 1·6710	1·6704
E 1·6420	1·6433		

In returning from flint-glass, No. 13, to water, and calculating the indices of water according to the formula:

$$n = 1{\cdot}3309 \left(\frac{N}{N'}\right)^{0{\cdot}39}$$

We

We find

Observation.	Calculation.	Observation.	Calculation.
B 1·3309	....	F 1·3378	1·3374
C 1·3317	1·3315	G 1·3412	1·3412
D 1·3335	1·3333	H 1·3441	1·3446
E 1·3358	1·3354		

In comparing flint-glass, No. 13, and oil of turpentine, the values of r are,

According to the rays

B and C : $r = 1·806$	B and F : $r = 1·649$
B and D : $r = 1·689$	B and G : $r = 1·657$
B and E : $r = 1·653$	B and H : $r = 1·670$

And calculating the indices of flint-glass, according to the formula

$$N = 1·6277\left(\frac{n}{n'}\right)^{1·67}$$

We have

Observation.	Calculation.	Observation.	Calculation.
B 1·6277	....	F 1·6482	1·6481
C 1·6297	1·6295	G 1·6602	1·6605
D 1·6350	1·6349	H 1·6710	1·6710
E 1·6420	1·6421		

In comparing flint-glass, No. 13, with flint-glass, No. 3, we have the following values of r:

According to the rays

B and C : $r = 1·093$	B and F : $r = 1·120$
B and D : $r = 1·105$	B and G : $r = 1·116$
B and E : $r = 1·125$	B and H : $r = 1·111$

And calculating the indices of flint-glass, No. 13, according to the formula

$$N = 1·6277\left(\frac{n}{n'}\right)^{1·1}$$

We have

Observation.	Calculation.	Observation.	Calculation.
B 1·6277	.....	F 1·6482	1·6478
C 1·6297	1·6297	G 1·6602	1·6599
D 1·6350	1·6349	H 1·6710	1·6706
E 1·6420	1·6417		

In comparing flint-glass, No. 13, with the solution of potash, we have for r:

According to the rays

B and C : $r = 1·910$	B and F : $r = 2·067$
B and D : $r = 1·959$	B and G : $r = 2·161$
B and E : $r = 2·045$	B and H : $r = 2·213$

And calculating the indices of flint-glass, No. 13, according to the formula

$$N = 1·6277\left(\frac{n}{n'}\right)^{2·2}$$

We

We have

Observation.	Calculation.	Observation.	Calculation.
B 1·6277	....	F 1·6482	1·6497
C 1·6297	1·6300	G 1·6602	1·6609
D 1·6350	1·6359	H 1·6710	1·6704
E 1·6420	1·6431		

LXVI. *Description of a Rain-gauge.* *By* John Taylor, *Esq. F.R.S. &c.*

[With an Engraving.]

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE object of this little invention was to construct an instrument by which the quantity of rain might be registered for a long period, and observed at any shorter intervals, without any manipulation which might require the attention of a person skilful in such matters.

I wished to place rain-gauges at different stations where machinery worked by water-power was employed, that I might compare the effect of various states of the weather upon the efficiency of the engines; and I was desirous of having an instrument, of which a common observer could give a correct report, and which would perform correctly, without any management or interference, for a considerable length of time, and which would thus of itself check the reports which might be made at intervals. Others may find uses for such a contrivance; and therefore as it seems to answer its purpose very well, I send you a description of it if you deem it worthy a place in your Magazine.

The instrument has been very beautifully made for me, by Mr. Henry Russell, of King's Square, Goswell-street Road, to whom I am indebted also for the arrangement of the train of wheels, and for combining all the parts into a very compact and convenient form.

As Mr. Bevan some time since gave a notice that he had constructed a self-registering rain-gauge, and promised a description of it in your Magazine, I delayed proceeding with mine until he had favoured the public with the account; and then finding that the principle of his very ingenious instrument differed from mine, and that it required so much attention as is necessary to keep the clock attached to it constantly going, I preferred my own, as being independent of any care of this sort.

The plate (Plate IV.) shows the rain-gauge in two views: fig. 1. being designed to exhibit how the water is received and

and measured; and fig. 2. the arrangement of the wheels and hands, which indicate the quantity that has passed through the instrument.

The whole is inclosed in a japanned tin case, of which the upper part is the receiving funnel, from which the water drips into a smaller funnel and pipe, which conducts it to a water-wheel A, having three buckets. One of these buckets is always detained in a position to receive the flow of water by the bent lever and weight B, which presses against two of the three pins *c c c*, and keeps the wheel still, until a quantity of water has entered the bucket sufficient to raise the weight, when it instantly escapes; and the next bucket is presented in the same position, to receive another charge, being detained by the next pin coming in contact with the lever. A slight spring *d* is attached, which takes the pin as it rises to retard the velocity of the wheel, and to prevent its overshooting its proper position. For the same reason the lever is slightly notched where the pins rub against it in their passage.

The instrument is adjusted to the proper measure of water by the weight on the bent arm, which screws backward and forward for this purpose, and admits of its being regulated to great accuracy.

It is evident that such an instrument, if the principle can be correctly applied, would measure any flow of water which might not be too large for being conveniently submitted to an apparatus of this kind.

Fig. 2. shows the arrangement of the wheels and graduated circles for denoting the quantity of rain. The large funnel has an area of 72 square inches, and the water-wheel is adjusted to revolve once with 7·2 cubic inches, or what is equal to 1-10th of an inch in depth, on the area of the funnel. Thus each bucket will upset with 2·4 cubic inches; but as the wheel may not be exactly in equilibrium, it is better to adjust it to one complete revolution, by suffering 7·2 cubic inches to drip in and correct it by the counterpoise.

On the axis of the water-wheel is a pinion of 8 teeth (*e*) driving a wheel of 80 teeth (*f*), which thus revolves once for ten revolutions of the water-wheel, and by a hand on the arbor indicates one inch by completing the circle, and the divisions into tenths as marked on the circle; but as each bucket carries the hand but one-third of a division, the observations may be made to 1-30th of an inch in depth.

To extend the register of the quantity to a long period, the other wheels are added; and on the arbor of the wheel *f* is a pinion of 8 teeth (*g*), driving a wheel of 80 teeth (*h*), on the arbor of which is a pinion of 20 teeth (*i*), driving the wheel of 80 teeth (*k*), on the arbor of which is another hand pointing to to a circle divided into 40 parts, each of which represents one revolution of the wheel (*f*) and its hand: consequently the upper hand indicates an inch in depth for every division, and goes on to 40, which is more than the usual fall of rain in one year; while the lower hand points out the tenth parts of each inch, or by subdividing to the thirtieth part of each inch.

A glass is inserted in the case, to render the graduated circles and hands visible, and the whole may be placed on a suitable stand where it is properly exposed. I would recommend, however, that the lower part should be in a greenhouse or other low building, the upper funnel being then connected by a pipe.—A bottle may be placed under the pipe at the bottom of the gauge, so as to receive all the water which passes through it. If this bottle be graduated by a measure of 7·2 cubic inches, each division will indicate $\frac{1}{10}$th of an inch in depth; and thus the accuracy of the instrument may be checked, and any irregularity detected until the action is proved to be correct.

In the construction the use of oxidizable metals should be avoided, for the three pins on the edge of the water-wheel, for the axle of the water-wheel, and for the centre which supports the counterpoise,—all which should be of silver or platina. As the whole is much exposed to rust, I should recommend that iron or steel should not be employed in any part. It will be obvious that the instrument possesses the advantage of exposing very little water to the effects of evaporation.

I am, &c.

Coed Dû, Flintshire, Oct. 1, 1827. JOHN TAYLOR.

LXVII. *Some further Remarks on the Genus Chameleon, with the Description of an undescribed Species. By* JOHN EDWARD GRAY, *Esq. F.G.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN the last Number but one of your Journal, I described the species of the genus *Chameleo*, which had come under my observation. Since that period, Mr. Bell has presented to the British Museum a specimen of *Chameleo Tigris* of Kuhl, which proves to be a very distinct species most nearly allied to *Chameleo pumillus*, but easily distinguished by the central occipital ridge.

In examining the valuable museum of Mr. Joshua Brookes, I have observed an undescribed species of the genus, which with his usual kindness and liberality he has permitted me to describe, and to have a drawing made of it.—I hasten to lay the description before your readers, as it presents a peculiar form which has not yet been found in the genus.

The chief peculiarities consist in the eyebrows being provided

vided with triangular horn-like projections, and the tail being shorter than the body, and very thick and compressed at the base. In naming the species, I think that I cannot do better than dedicate it to its scientific and industrious proprietor, in whose collection it has been for a considerable time.

Chameleo Brookesiana.

Superciliis elevatis angularibus, denticulatis; occipite plano; fronte concavo; squamis parvis irregularibus; caudâ brevi basi compresso-incrassatâ dorsi lateribus, mento antice, membranorumque marginibus serie squamarum parvarum spinosarum instructis.

Inhab. ——— *Mus. Dom. Brookes.*

Length $2\frac{1}{4}$, body $1\frac{1}{4}$, tail 1, inches long.

Eyebrows elevated triangular, edges denticulated, innerside keeled, denticulated; forehead concave, especially in the centre; cheeks nearly flat; occiput flat with a slightly elevated ridge on each side, commencing from the tip of the superciliary elevations and nearly meeting on each side; head covered with small irregular scales; front of chin with 4—6 spine-like scales; body and tail compressed, edges not denticulated; side of the back just at the origin of each of the ribs, with a series of rather large conical spine-like scales, and the legs with scattered similar scales; rest of the scales of the body and tail small and irregular; tail base very thick, compressed.

The above-described individual is probably young. Nothing is known of its country; but it is probably from Africa.

P.S. Being engaged on an illustrated monograph of this genus, I shall be happy to see any specimen that may be in private collections. J. E. G.

British Museum, Nov. 1, 1827.

LXVIII. *A Monograph of the Genus Teredo of Linné, with descriptive Characters of the Species in the British Museum. By* John Edward Gray, *Esq. F.G.S. &c.**

THIS genus is best distinguished from *Pholas* by the absence of the back-valves, and by being always provided with a shelly sheath. The pallet at the sides of the trachea of the animal appears to form a very good character to distinguish the species.

* *Pallet unknown* (probably like the next).

Fistulana personata, Lam. A. M. *Teredina personata*, Lam. Ann. Mus. xii. t. 43. f. 67. *Teredo*. Sow. Foss. Shells.

Tubes cylindrical ——? Shell globular. Valves as long as high. Anterior gape, an equilateral triangle; hinder gape ovate. Front lobe of the valves recurved, hinder lobe concen-

* Communicated by the Author.

 trically

trically striated, internal tooth ——? Pallet? Fossil: Highgate.

Teredo antenauta. Sow. Foss. Shells.

Differs from the above in being larger; but I have no specimen to make an accurate description. Fossil: Highgate.

Serpula arenaria, Linn. ed. 10. *Serpula polythalama*, Linn. *Septaria arenaria*, Lam. *Teredo*, Home, Phil. Trans. and Anat. Comp. ii. t. 41. 4. Rumph. t. 41. f. D. E. Seba, iii. t. 94.

Tube very large, club-shaped, thick, behind closed, rounded, upper part divided into two tubular canals.

The valves are not in the Museum; but it is evidently a *Teredo*, and the division of the mouth is not enough to form a genus, as it is partially found in other species.

** *Pallet broad lamellar.*

Teredo navalis, Linn. Turton. Sow. Gen. Turton, Bivalves. Adanson, Acad. Sci. Par. t. 9. f. 1—8. cop. Ency. Méth. t. 187. f. 1—3. and t. 167. f. 4. 5.

Tubes long subcylindrical, tortuous end chambered. Shell globular, anterior gape roundish triangular; hinder one ovate lanceolate; the lower edge of the anterior lobe straight. Valves two-thirds as long as high; internal process broad, parallel to the valves; pallets broad, ovate, lamellar and blunt.

Teredo Malleolus. Turton, Bivalves. t. 2. f. 19. The pallet, "Shell with the valve earshaped behind, and the auricules reflected. The pallet transverse mallet-shaped." Turt.

Teredo nana. Turton, Bivalves, t. 2. f. 6. 7.

"Shell with the valves rounded, and without auricules behind, a strong tooth on the margin above the tooth." Turt. Bivalves.

*** *Pallet ovate lamellar, end denticulated. Fistulana*, Lam.

Teredo Clava, Gmel. *Fistulana gregata*, Lam.

Ency. Méth. t. 167. f. 6—15.

Tubes club-shaped, contorted, aggregate and rounded, closed; mouth half-divided lamellar; shell ring-like, valves very short and high; front gape rounded; hinder one ovate; internal tooth linear and dilated hatchet-shaped; pallet broad lanceolate and acute; sides denticulated.

Fistulana corniformis, Lam., which I have not seen, does not appear to differ by the figures much, if at all, from this species.

The other species of *Fistulana* of Lamarck are all the cases of *Gastrochæna*, except perhaps *Fistulana clava*, which if it does form a separate genus, it must be placed near the *Gastrochænæ*, and not with the *Teredines*.

**** *Pallet long, base setaceous, end subarticulately divided, sheathed and pinnately lobed.*

Teredo bipalmulata, Lam. Syst. *T. palmulatus*, Lam. Hist. *Taret de Pondicherry*, Adanson, Acad. Sci. Par. 1759. t. 9. f. 12.

Tubes

Tubes cylindrical, thin; shell ovate, globular; front lobe narrow triangular, lower edge rounded, very finely concentrically striated, central band thin; hinder portion smooth, the hinder dorsal edge ovate, expanded (not recurved) serving for the attachment of the abductor muscles; internal tooth broad, compressed sickle-shaped, at right angles with the inner side of the shell. Pallets base short, setaceous end compressed, end broad, pinnated and articulated. Sumatra: Mr. H. Stuchbury.

Teredo carinata. n.

Tubes long, cylindrical; shell subglobular; front lobe triangular, lower edge straight, rather oblique, concentrically furrowed; medial band thin; hinder portion concentrically wrinkled, posterior dorsal margin expanded, recurved, parallel with the hinge; edge deeply keeled internally; internal tooth compressed, curved, placed obliquely with respect to the inner surface of the valve. Pallet base short, setaceous end compressed, end broad, linear lanceolate, pinnately articulated. Sumatra: Mr. H. Stuchbury.

Teredo bipennata, Turt. Conch. Dict. f. 28. 40. *Teredo navalis.* Home, Phil. Trans. and Comp. Anat. ii. t. 43. (bad).

Exactly like the latter in shape, but twice as large, and the front lobe is rather larger, and the hinder portion nearly smooth, and the inner edge is not so much keeled; and the pallet differs in having a very long setaceous stern (6 inches long), and in the end being short and pinnately articulated, with the ends of the joint long and filiform. "Tube thick, strong, without any transverse concamerated partitions." Turton. Drifted wood, British Channel. J. Bulwer, Esq.

In the above disposition I have studiously avoided giving any new names to the species, which is too much the fault of modern works.

In this arrangement of the genus, I have united four genera of Lamarck, but they appear to have been founded on very slight grounds.

I am informed by Professor Blainville, that our *Teredo* is not the *Teredo navalis* of French naturalists, and that he has called the one here described *T. nigra.* I am certain that is the one figured by the authors to which I have referred, and the older figures are so bad that it is impossible to determine whether theirs is distinct.

The *Teredo dorsata* is a kind of *Pholas*, being destitute of any tube, and being provided with back-valves. It is perhaps the link that connects the two genera.

LXIX. *Meteorological Register kept at Funchal, in Madeira, in the Year* 1826; *&c. By* C. Heineken, *M.D.*

[Concluded from p. 376.]

JULY

JULY 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	30·27	71	72	67	5	30·29	73	73	64		E.	Very fine
2	30·28	72	72	67	5	30·28	73	74	64		NE.	Very fine
3	30·24	72	72	63	9	30·23	72	74	64		NE.	Very fine
4	30·19	72	72	66	6	30·18	71	73	64		NE.	Very fine
5	30·18	72	72	69	3	30·23	73	74	64	·03	W.	P.M. rain
6	30·21	72	72	69	3	30·22	72	73	65		W.	Thick on hills
7	30·20	72	73	69	4	30·20	73	74	64		NE.	Fine
8	30·20	72	72	68	4	30·20	73	74	66		NE.	Fine
9	30·16	72	72	69	3	30·16	74	75	65		NE.	Fine
10	30·14	72	72	62	10	30·16	72	75	66		NE.	Cloudy
11	30·14	72	72	65	7	30·15	73	75	67		NE.	Cloudy
12	30·13	73	73	63	10	30·15	73	75	66		NE.	Cloudy
13	30·15	72	72	62	10	30·18	73	75	63		NE.	Cloudy
14	30·18	72	72	64	8	30·17	73	75	65		NE.	Fine P.M. Cloudy
15	30·19	73	73	64	9	30·20	72	75	64		NE.	Fine P.M. Cloudy
16	30·21	73	72	62	10	30·18	71	74	63		NE.	Fine
17	30·18	72	72	65	7	30·18	73	75	65		NE.	Fine
18	30·17	73	73	66	7	30·18	73	75	65		NE.	Fine
19	30·17	73	74	65	9	30·17	73	75	65		NE.	Fine
20	30·16	74	74	65	9	30·15	74	76	66		NE.	Fine
21	30·12	74	73	65	8	30·13	74	76	65		NE.	Fine
22	30·14	74	74	69	5	30·14	73	76			NE.	Fine
23	30·14	73	73	65	8	30·17	73	76	65		NE.	Fine
24	30·14	72	72	64	8	30·17	73	76			NE.	Fine
25	30·05	72	72	65	7						W.	Cloudy
26												
27												
28												
29						29·91	73	75	67	·68	W.	Heavy shower
30	29·95	73	74	70	4	29·96	73	75	68		W.	Fine
31	29·95	73	73	71	2	30·10	74	75	66	·06	W.	Shower

Pressure. Corr[d] for Expans.
Max. 30·29 at 73° =30·179
Min. 29·91 73 =29·806
Mean 30·346 73 =30·234

Temperature.
Max. .... 76°
Min. 63
Mean .... 67·5

Dew Point.
Max. 71° Min. 62°

Dryness.
Max. 10 Min. 2

Rain 0·77 inches.

Winds.
N.E. E. W.
21 1 9 =31.

Remarks.—A cool and cloudy month; the heavy rain on the 29th was unusual

AUGUST 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	30·15	73	73	70	3	30·18	73	75	65		NE.	Fine
2	30·16	72	73	64	9	30·13	72	75	65		NE.	Fine
3	30·11	73	74	71	3	30·10	73	75	66		NE.	Fine
4	30·09	73·5	74	65	9	30·10	74	75	67		NE.	Fine
5	30·05	74	75	66	9	30·03	75	76	67		NE.	Fine
6	29·97	75	75	70	5	30·03	75	78	69		E.	Partial sirocco
7	30·07	75·5	76	71	5	30·10	76·5	79	69		E.	Hot and thick
8	30·10	76	76	74	2	30·07	76·5	80	71		E.	Hot and thick
9	30·09	76	76	73	3	30·08	76	79	67		W.	Thick and hazy
10	30·06	75·5	75	73	2	30·09	75	79	67		NE.	Fine
11	30·10	75	75	70	5	30·12	75	78	66		NE.	Fine
12	30·13	74	74	64	10	30·10	74	78	66		NE.	Fine
13	30·15	74	74	66	8	30·15	74·5	79	66		NE.	Fine
14	30·15	74	74	69	5	30·13	75	80	67		NE.	Fine
15	30·12	74·5	74	68	6	30·14	75	80	67		NE.	Fine
16	30·16	75	75	66	9	30·15	76	79	66		NE.	Fine
17	30·14	75	75	67	8	30·14	75	79	67		NE.	Fine
18	30·11	75	75	72	3	30·13	76	81	67		NE.	Fine
19	30·13	76	75	72	3	30·14	75	80	66		NE.	Fine
20	30·14	75	76	65	11	30·13	75	80	66		N.	Fine
21	30·11	75	76	71	4	30·10	76	80	67		N.	Fine
22	30·09	75	75	72	3	30·11	76	82	68		NE.	Fine [Sirocco
23	30·10	76	77	74	3	30·06	76	82	69		SE.	Partial Cloudy
24	30·00	76	76	75	1	29·98	77	81	69		SE.	Cloudless Sky
25	30·02	76	76	74	2	30·06	77	80	70		W.	Heavy and hazy
26	30·12	76	76	72	4	30·13	76	78	65		E.	Cloudy
27	30·19	75	75	64	11	30·20	75	79	65		NE.	Fine
28	30·15	74	74	69	5	30·08	74	78	65		NE.	Fine
29	30·05	74	75	67	8	30·06	75	80	68		N.	Fine [Sirocco
30	30·11	76	77	69	8	30·15	76	81	67		E.	Partial Cloudy
31	30·23	75·5	76	68	8	30·22	75·5	81	66		NE.	Fine

Pressure.		Corr^d for Expans.
Max. 30·23	at 75°	=30·119
Min. 29·97	75	=29·859
Mean 30·108	75	=29·997

Temperature.

Max. .... 82°
Min. 65
Mean .... 72·9

Dew Point.

Max. 75° Min. 64°

Dryness.

Max. 11 Min. 1

Rain none.

Winds.

N.	N.E.	E.	S.E.	W.	
3	19	5	2	2	=31

Remarks.—A fine summer month.

SEPTEMBER 1826.

	10 o'Clock A.M.					10 o'Clock P.M.				Rain.	Wind	Weather.
	Barometer.		Hygrometer.			Barometer.		Ther.				
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	—			
1	30·22	76	76	72	4	30·18	75	79	66		NE.	Cloudy, P.M. rain
2	30·11	75	76	63	13	30·10	75	79	66		NE.	Cloudy
3	30·05	74	74	65	9	30·11	74	77	64		W.	Slight showers
4	30·17	73	74	69	5	30·22	74	81	66		E.	Fine P.M. few
5	30·24	74	74	69	5	30·23	74	80	67	·06	NE.	Shower [drops
6	30·25	74	74	66	8	30·23	74	77	65		NE.	Cloudy
7	30·23	75	75	71	4	30·19	74	80	69		NE.	Overcast
8	30·14	75	75	70	5	30·12	75	79	65		NE.	Overcast
9	30·12	75	75	69	6	30·10	75	79	66		NE.	Overcast
10	30·11	75	75	69	6	30·17	75	81	66		NE.	Overcast
11	30·13	75	75	69	6	30·20	76	82	65		NE.	Fine
12	30·09	75	75	66	9	30·12	75	80	66		NE.	Fine
13	30·09	75	75	73	2	30·07	75	80	66		NE.	Fine
14	30·06	74	73	69	4	30·15	75	82	65		NE.	Fine
15	30·17	75	75	70	9	30·15	75	82	66		NE.	Fine
16	30·08	75	75	71	5	30·07	74·5	79	64		NW.	Shower
17	30·08	74	74	73	3	30·05	73·5	78	66	·32	W.	Shower
18	30·02	74	74	73	1	30·06	74·5	80	64	·04	W.	Shower
19	30·12	75	76	72	4	30·15	75	80	65		W.	Fine
20	30·16	75	75	73	2	30·16	75	79	68		W.	Cloudy
21	30·15	75	75	74	1	30·13	74	78	66		W.	Small drisl. rain
22	30·09	74	74	73	1	30·04	75	79	67	·52	SW.	Rain A.M., fine
23	30·08	75	75	74	1	30·09	75	79	65	·04	W.	Slight showers
24	30·10	75	75·5	74·5	1	30·07	75	80	67		W.	Cloudy
25	30·00	75	75	74	1	29·98	76	82	70	·16	W.	Showers
26	29·95	75·5	75·5	74·5	1	29·96	75	79	69	·13	W.	Showers
27	30·05	76	76	75	1	30·14	76	80	66	·14	W.	Fine
28	30·15	75	75	74	1	30·15	75	79	67		W.	Fine
29	30·10	75	76	75	1	30·10	76	84	69		E.	Thick part. sir.
30	30·07	76	76	75	1	30·10	76	84	67		E.	Thick part. sir.

Pressure.		Corr^d for Expans.
Max. 30·25	at 74°	=30·150
Min. 29·95	75	=29·839
Mean 30·116	75	=30·005

Temperature.

Max. .... 85°
Min. 64
Mean .... 73·1

Dew Point.

Max. 75° Min. 63°

Dryness.

Max. 13 Min. 1

Rain 1·41 inches.

Winds.

N.E.	E.	S.W.	W.	N.W.	
13	3	2	11	1	=30.

Remarks.—A fine month, but much more rain than in general.

OCTOBER 1826.

	10 o'Clock A.M.					10 o'Clock P.M.						
	Barometer.		Hygrometer			Barometer.		Ther.		Rain.	Wind	Weather.
	Bar.	Th.	Air.	D.P.	Dry.	Bar.	Th.	+	−			
1	30·12	76	76	75	1	30·15	76		64		E.	Fine
2	30·20	73	74	63	11	30·22	75		66		E.	Fine
3	30·24	75	75	71	4	30·22	75		64		NE.	Fine
4	30·22	74	74	66	8	30·16	75		66		NE.	Fine
5	30·12	75	75	74	1	30·12	74		65	·03	E.	Shower
6	30·11	73	75	64	11	30·12	74		63		NE.	Fine
7	30·15	73	74	64	10	30·15	75		63		NE.	Fine
8	30·17	73	72	70	2	30·15	73		63		NE.	Overcast
9	30·17	72·5	72	65	7	30·17	74		63		NE.	Fine
10	30·17	73	73	65	8	30·18	74		62		NE.	Fine
11	30·15	73	74	65	9	30·13	74		64		NE.	Fine
12						30·07	74	77	62		NE.	Fine
13	30·02	74	74	68	6	29·99	74	77	63		NE.	Fine
14	29·97	73	74	70	4	29·96	74				NE.	Fine
15	29·98	74	74	67	7	29·99	74	76			NE.	Fine
16	30·01	74	74	63	11	30·01	73	75	60	·15	NE.	Cloudy P.M. sh.
17	29·94	71	71	68	3	29·84	74	74	58	·24	NW.	Rain
18	29·83	71	71	66	11	29·80	70	72	62		W.	Fine P.M. shower
19	29·80	71	71	68	3	29·85	70	71	62	·11	W.	Showers
20	29·94	71	71	69	2	29·98	72	73	65	·07	W.	Showers
21	30·00	72	71	70	1	30·01	73	72	60	·43	W.	Heavy do. P.M.
22	30·03	71	71	68	3	29·99	71	73	60		NW.	Fine
23	29·97	71	71	63	8	29·97	71	73	59		N.	Fine
24	30·01	70	70	64	6	30·07	70	71	61	·08	NE.	Do. night show.
25	30·12	70	70	66	4	30·19	71	72	64	·03	NE.	Cloudy ditto
26	30·20	71	72	70	2	30·20	71	73	63	·02	NE.	Cloudy ditto
27	30·13	71	72	67	5	30·10	72	73	62		SE.	Fine
28	30·06	71	72	70	2	30·03	72	72	63		SE.	Cloudy
29	30·02	71	71	69	2	30·00	71	71	61	·02	N.	Do. show. night
30	29·99	71	72	66	6	29·99	71	73	61		NE.	Fine
31	29·98	73	73	70	3	29·96	72	74	64		NE.	Fine

Pressure.		Corr[d] for Expans.
Max. 30·24	at 75°	=30·129
Min. 29·80	71	=29·702
Mean 30·059	73	=29·948

Temperature.

Max. .... 77°
Min. 58
Mean .... 62·5

Dew Point.

Max. 75° Min. 60°

Dryness.

Max. 11 Min. 1

Rain 1·18 inches.

Winds.

N.	N.E.	E.	S.E.	W.	N.W.	
2	18	3	2	4	2	=31.

Remarks.—The first part of the month unseasonably warm; the remainder such as October generally is.

NOVEMBER 1826.

	10 o'Clock A.M.					10 o'Clock P.M.						
	Barometer.		Hygrometer.			Barometer.		Ther.		Rain.	Wind	Weather.
	Bar.	Th.	Air.	D. P.	Dry.	Bar.	Th.	+	−			
1	29·91	71	71	70	1	29·82	71	72	63	1·61	SE.	Rain P.M.
2	29·74	71	70	69·5	0·5	29·72	71	72	63	1·83	SW.	Rain P.M. fine
3	29·75	70	70	69·5	0·5	29·74	71	71	63	1·45	SW.	Rain P.M. fine
4	29·77	71	71	70	0·5	29·81	71	72	63	2·62	W.	Rain P.M. fine
5	29·84	71	71	70	1	29·86	72	72	61	·21	W.	Showers P.M.
6	29·92	69	68	61	7	29·97	69	70	60	·96	E.	Fine
7	30·00	67	67	59	8	29·96	67	68	63	·32	NE.	Showers
8	29·90	66	66	61	5	29·88	67	68	57	·08	NE.	Showers
9	29·90	68	69	62	7	29·89	65	67·5	55		NE.	Fine
10	29·93	66	66	63	3	30·00	65	68	54		NE.	Fine
11	30·06	66	67	60	7	30·13	67	68	60		NE.	Fine
12	30·18	66	66	63	3	30·22	68	68	56		NE.	Overcast
13	30·29	68	67	65	2	30·27	68	69	54		E.	Fine
14	30·29	65	66	59	7	30·24	67	67	58		N.	Fine
15	30·23	66	67	56	11	30·15	67	67	59		N.	Overcast
16	30·10	66	66	57	9	30·10	67	67	59	1·04	N.	Night, rain
17	29·98	67	66	65	1	29·94	67	68	59		E.	Night, rain
18	29·87	66·5	67	60	7	29·76	67	68	56	·63	NE.	Showers
19	29·72	66	67	64	3	29·80	66	67	57		NE.	Slight showers
20	29·93	66	67	65	2	29·90	68	70	60		NE.	Fine, show. P.M.
21	29·88	67	67	66	1	29·84	68	69	59		SW.	Showers
22	29·78	68	65	65	0·0	29·72	69	67	59	2·27	W.	Rain, thund. & li.
23	29·67	66	66	65·5	0·5	29·58	68	67	58	1·21	W.	Rain, ditto
24	29·52	66·5	66·5	66	0·5	29·39	69	68	57	1·63	W.	Rain, ditto
25	29·57	67	66	65	1	29·87	67	67	55	2·63	NW.	Rain, P.M. NE.
26	30·05	64	64	55	9	30·14	65	65	53		NE.	Fine
27	30·20	63	64	58	6	30·25	64	65	54	·12	NE.	Do. P.M. showers
28	30·33	63	64	56	8	30·38	64	64	52		NE.	Very fine
29	30·39	63·5	64	56	8	30·38	64	64	52		NE.	Very fine
30	30·27	62	63	60	3	30·18	64	64	54		NE.	Very fine

Pressure.		Corr^d for Expans.
Max. 30·39	at 63°	=30·304
Min. 29·39	67	=29·294
Mean 29·964	67	=29·866

Temperature.

Max. .... 72°
Min. 52
Mean .... 62·8

Dew Point.

Max. 70° Min. 55°

Dryness.

Max. 11 Min. 0

Rain 18·61 inches!

Winds.

N.	N.E.	E.	S.E.	S.	SW.	W.	N.W.	
3	14	3	1	1	3	5	1	=31.

Remarks.—More rain has fallen than was perhaps ever known to fall in the same time, by the oldest inhabitant; it amounts to within 1·82 in. of the *whole* that fell during last year!

DECEMBER 1826.

	10 o'Clock A.M.						10 o'Clock P.M.							
	Symp.	Barometer.		Hygrometer			Symp.	Barometer.		Ther.		Rain.	Wind	Weather.
		Bar.	Th.	Air.	D.P.	Dry.		Bar.	Th.	+	−			
1	30·14	30·21	62	62	55	7	30·15	30·22	64	64	54		NE.	Very fine
2	30·20	30·26	63	64	59	5	30·20	30·28	66	68	57		NE.	Very fine
3	30·25	30·31	64	65	63	2	30·25	30·34	66	65	57		E.	Overcast
4	30·34	30·37	64	64	60	4	30·30	30·37	65	66	58		NE.	Overcast
5													NE.	Overcast
6	30·30	30·34	65	66	56	10	30·25	30·33	65	66	55		NE.	Overcast
7	30·26	30·32	65·5	67	59	8	30·20	30·29	66	66	59		NE.	Fine
8	30·20	30·25	64·5	65	59	6	30·15	30·22	66	65	58		NE.	Overcast
9	30·12	30·19	64	64	55	9	30·01	30·11	65	65	59		W.	Overcast
10	30·00	30·08	64	65	57	8	29·94	30·02	65	65·5	59		W.	Overcast
11	29·98	30·05	65	65	59	6	29·93	30·03	65	67	57		W.	Overcast
12	29·99	30·07	64	65	51	14	30·02	30·13	67	68	56		E.	Fine, slight
13	30·12	30·20	67	67	61	6	30·17	30·25	67	68	58		E.	D° [siroc.
14	30·17	30·24	66	66	59	7	30·05	30·16	68	66	56		NE.	Overcast
15	30·04	30·11	64	65	56	9	29·99	30·09	67	66	55		NE.	Overcast
16	30·02	30·09	64	65	55	10	30·02	30·11	66	66	54		NE.	Fine
17	30·04	30·10	65	65	56	9	30·04	30·11	65	66	55		NE.	Fine
18	30·11	30·18	65	66	59	7	30·12	30·23	66	65	54		NE.	Fine
19	30·21	30·27	65	66	57	9	30·26	30·33	65	65	53		NE.	Fine
20	30·30	30·36	63	64	57	7	30·23	30·34	65	65	52		E.	Fine
21	30·21	30·27	63	64	57	7	30·01	30·17	64	64	55		E.	Fine
22	30·02	30·07	64	65	60	5	29·92	30·10	65	64	57		E.	Fine
23	29·86	29·94	64	62	62	0	29·80	29·88	66	64	57	1·20	SE.	Rain
24	29·76	29·83	64	64	63	1	29·73	29·80	65·5	64	55	·51	SW.	Shwrs P.M.
25	29·72	29·79	65	65	64	1	29·71	29·79	64	66	56		E.	Fine [thr lg
26	29·76	29·83	63	62	61	1	29·71	29·78	64	63	56	3·70	SE.	Hy rain, thr
27	29·68	29·75	62	63	62	1	29·64	29·74	65	63	55	3·33	E.	Rain [& lig
28	29·79	29·85	63	63	62	1	29·85	29·92	66	66	56	·15	N.	Showers
29	29·99	30·04	65	66	65	1	30·11	30·18	66	65	57	·10	NE.	Showers
30	30·19	30·24	65	65	62	4	30·20	30·27	65	66	60		NE.	Fine
31	30·24	30·28	65	67	53	14	30·21	30·28	66	68	54		E.	Fine

Pressure.		Corrd for Expans.	*Symprisometer.*
Max. 30·37	at 64°	=30·284	Max. 30·340
Min. 29·74	64	=29·655	Min. 29·640
Mean 30·127	64	=30·042	Mean 30·054

Temperature.

Max. .... 68°
Min. 52
Mean .... 60·8

Dew Point.

Max. 65° Min. 51°

Dryness.

Max. 14 Min. 0

Rain 8·99 inches.

Winds.

N.	N.E.	E.	S.E.	SW.	W.	
1	15	9	2	1	3	=31.

Remarks.—Much heavy rain from the Eastward was perhaps never before known. On the whole a seasonable Month.

ANNUAL RESULTS (1826).

Pressure.	Corrected.	*Temperature.*	*Dew Point.*	*Dryness.*
Max. 30·590	30·505	Max. 84°	Max. 75°	Max. 30°
Min. 29·390	29·294	Min. 50	Min. 40	Min. 0
Mean 30·133	30·030	Mean 64·3	Total amount of rain 43·35 in.	

Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	
21	157	56	20	2	7	74	29	=366.

1825.

Rain.—Jan.	1·83	April	1·66	July	0·72	Oct.	0·67
Feb.	1·79	May	1·	Aug.	1·62	Nov.	1·80
March	2·32	June	1·05	Sept.	2·56	Dec.	3·41

Total 20·43 inches.

Mean quantity of rain for 1825 and 1826, 31·89 inches.

From January 1824 to December 1826 (three years).

Pressure.—Maximum 30·62 Minimum 29·39 Range 1·23.
Temperature.—Maximum 85° Minimum 50° Range 35°.

Diurnal Range of the Barometer, December 4th and 5th 1826, at a Height of 36 feet above the Level of the Sea.

o'Clock.				o'Clock.			
P.M. 3	30·400	67	Convex	A.M. 6	30·420	62·5	Level
4	30·393	66	Convex	7	30·421	63	Convex
5	30·400	65·5	Convex slightly	8	30·431	63	Concave slight.
6	30·421	64·5	Convex	9	30·434	63	Concave slight.
7	30·421	64	Level	10	30·434	63	Concave slight.
8	30·450	64	Level	11	30·434	63·5	Level
9	30·433	63·5	Convex slightly	12	30·421	64	Level
10	30·433	63·5	Convex	P.M. 1	30·400	64	Level
11	30·433	63·5	Level	2	30·391	64·5	Level
12	30·441	63·5	Level	3	30·391	65	Level
A.M. 1	30·430	63	Level	4	30·373	65	Level
2	30·422	63	Convex slightly	5	30·391	65	Level
3	30·423	63·5	Convex	6	30·400	64	Level
4	30·423	63·5	Convex slightly	7	30·400	63·5	Level
5	30·421	62·5	Level	8	30·450	64	Level

Mean	30·418	at	63° 9′	
Maximum	30·450	...	64	8 P.M.
Minimum	30·373	...	65	4 P.M.

6 A.M. nearest to the mean.

N.B. The Barometer is one of Newman's Mountain instruments with an iron cistern, into which its Thermometer plunges;—it hangs within-doors, at a window with a South aspect, fifteen feet from the ground, and eighty-nine above the level of the sea.—The Hygrometer is Daniell's, used at the same window, but kept in a dry close cupboard.—Symprisometer, Adie's patent Mountain, in same situation as Barometer.—Maximum Thermometer, is a horizontal one by Newman; hangs in an unoccupied room on the first story; aspect North, and window and door always open.—Minimum, horizontal, by Dollond; against a wall with North aspect, and sheltered from rain; both fifteen feet from the ground, and eighty-nine above the sea.—The Rain-gauge is one of Newman's full-size copper ones; on the top of the house twenty-five feet above the ground, and ninety-nine above the sea.

LXX. *On*

LXX. *On the Integration of Linear Differential Equations having Constant Coefficients and last Term any Function of the Indeterminate Quantity x.* By John Herapath, *Esq.*

Definition.—FOR the sake of brevity I shall constantly in the following researches put $dx = 1$, which will of course not appear in any of the differential expressions.

It is not intended here to treat of the manner of integrating differential equations of the form

$$\frac{d^n y}{dx^n} + A\frac{d^{n-1}y}{dx^{n-1}} + \ldots R\frac{dy}{dx} + Sy = 0$$

or, as it is proposed to write them,

$$d^n y + Ad^{n-1}y + \ldots\ldots Rdy + Sy = 0 \qquad (1)$$

in which A, B, . . . R, S are constant quantities. One property of these equations which we shall find serviceable in our future inquiries is obvious enough; namely, that pe^{rx} and its differentials, being everywhere substituted for y and its differentials, will satisfy the conditions of (1), when p is a constant and r a root of the equivalent algebraic equation

$$r^n + Ar^{n-1} + Br^{n-2} + \ldots Rr + S = 0 \qquad (2)$$

From this simple property we may at once advance to the integration of

$$d^n y + Ad^{n-1}y + \ldots\ldots Sy = X \qquad (3)$$

in which X is any function of x; but as it might appear somewhat abrupt to launch forth so suddenly into all the difficulties and generality of the subject, I propose to begin with the integration of two or three of the inferior cases.

Suppose we had to integrate the equation of the first order

$$dy + Ay = X.$$

If in this equation $X = 0$ the integral is evidently

$$y = ce^{-Ax}$$

c being an arbitrary constant. Now putting for the arbitrary constant a function p of x, it is clear that whatever be the value or form of y, the indeterminate expression pe^{-Ax} must contain it. The only difficulty therefore lies in determining p so as to satisfy the conditions of the proposed differential. For y and its differentials let us substitute their equivalents from $y = pe^{-Ax}$, which produce

 Ay

$$\left.\begin{array}{l} \mathrm{A}y = \mathrm{A}p\,e^{-\mathrm{A}x} \\ dy = -\mathrm{A}p\,e^{-\mathrm{A}x} + e^{-\mathrm{A}x}dp \end{array}\right\} = \mathrm{X},$$

or, $$e^{-\mathrm{A}x}\,dp = \mathrm{X}.$$

Dividing by $e^{-\mathrm{A}x}$ and integrating, there results

$$p = c + \int \mathrm{X}\,e^{-\mathrm{A}x},$$

and consequently

$$y = p\,e^{-\mathrm{A}x} = e^{-\mathrm{A}x}\{c + \int \mathrm{X}\,e^{\mathrm{A}x}\} \quad \ldots\ldots (4)$$

the complete integral sought, c being the arbitrary constant.

A first glance would lead one to suppose that this process is virtually the same as Lagrange's; but in the two subsequent cases the difference will be obvious enough.

Given the differential of the second order

$$d^2y + \mathrm{A}dy + \mathrm{B}y = \mathrm{X}.$$

Pursuing the same train of reasoning we demonstrate that the complete integral or value of y must be contained in $p\,e^{rx}$ assuming p to be an indeterminate function of x, and r a root of the equivalent algebraic equation

$$r^2 + \mathrm{A}r + \mathrm{B} = 0.$$

Substituting therefore for y, &c. their values we obtain

$$\left.\begin{array}{l} \mathrm{B}y = \mathrm{B}p\,e^{rx} \\ \mathrm{A}dy = \mathrm{A}rp\,e^{rx} + \mathrm{A}e^{rx}dp \\ d^2y = r^2p\,e^{rx} + 2r\,e^{rx}\,dp + e^{rx}\,d^2p \end{array}\right\} = \mathrm{X}$$

which on account of the value of r gives

$$(\mathrm{A} + 2r)\,e^{rx}\,dp + e^{rx}d^2p = \mathrm{X}.$$

Whence by division and integration

$$(\mathrm{A} + 2r)\,p + dp = c + \int \mathrm{X}\,e^{-rx},$$

and if the right hand member be called X_1 we have by the preceding example

$$p = e^{-(\mathrm{A}+2r)x}\{c_1 + \int \mathrm{X}_1\,e^{(\mathrm{A}+2r)x}.$$

Therefore multiplying by e^{rx} we have

$$y = e^{-(\mathrm{A}+r)x}\{c_1 + \int \mathrm{X}_1\,e^{(\mathrm{A}+2r)x}$$

or $$y = e^{-(\mathrm{A}+r)x}\{c_1 + \int e^{(\mathrm{A}+2r)x}\{c + \int \mathrm{X}\,e^{-rx} \qquad (5)$$

for

for the complete integral after restoring the value of X_{11}, each vinculum $\{$ being understood to include all the quantities on the right of it, and c, c_1 the two arbitrary constants of integration.

Let us now take for a third example the integration of

$$d^3y + Ad^2y + Bdy + Cy = X.$$

Then, as before, since every value y could have must be comprehended in $p e^{rx}$ in which r is a root of the equivalent algebraic equation,

$$\left.\begin{aligned} Cy &= Cpe^{rx} \\ Bdy &= B\{pre^{rx} + e^{rx}dp\} \\ Ad^2y &= A\{pr^2e^{rx} + 2re^{rx}dp + e^{rx}d^2p\} \\ d^3y &= pr^3e^{rx} + 3r^2e^{rx}dp + 3re^{rx}d^2p + e^{rx}d^3p \end{aligned}\right\} = X;$$

or by taking the sum and considering that r is such a quantity as makes the sum of the first vertical column of the development vanish

$$(B + 2Ar + 3r^2)e^{rx}dp + (A + 3r)e^{rx}d^2p + e^{rx}d^3p = X.$$

Dividing this by e^{rx} and integrating, we get

$$(B + 2Ar + 3r^2)p + (A + 3r)dp + d^2p = c + \int Xe^{-rx},$$

or $$B_1p + A_1dp + d^2p = X_1,$$

putting A_1, B_1 for the coefficients of p, dp, and X_1 for the right-hand member, which is a function of x. This equation being a linear differential of the second order with respect to p its integral is obtained from the preceding example; for instance it is

$$p = e^{-(A_1+r_1)x}\{c_2 + \int e^{(A_1+2r_1)x}\{c_1 + \int X_1 e^{-r_1x},$$

where r_1 is a root of a quadratic whose coefficients are, 1, A_1, and B_1. Multiplying this value by e^{rx} and restoring the values of A_1, B_1, and X_1 there results

$$y = e^{-(A+2r+r_1)x}\{c_2 + \int e^{(A+3r+2r_1)x}\{c_1 + \int e^{-r_1x}\{c + \int Xe^{-rx} \quad \ldots\ldots \quad (6)$$

the integral or value of y complete with all the arbitrary constants required by the successive integrations. We shall presently examine the truth of these solutions and investigate the law connecting the variable exponents as well as some other properties of the solutions.

General

General Integration of Linear Equations.

Having laid open an easy method of proceeding from the integration of equations of an inferior to those of a superior order, we shall now take up the integration of the equation

$$d^n y + A\,d^{n-1}y + B\,d^{n-2}y + \ldots\ldots R y = X \quad (7)$$

on principles much more general than those we have employed in the preceding cases.

It has already been remarked, and is indeed obvious enough, that if r be either of the roots of

$$r^n + A r^{n-1} + B r^{n-2} + \ldots\ldots R = 0 \quad (8)$$

$y = e^{rx}$ is a value that will satisfy the conditions of (7) when the function $X = 0$. Consequently, p being a function of x, our business will be to determine its form so that $y = p\,e^{rx}$ may be the complete integral sought. This it is manifest can only be done from the properties of the given equation, in a manner similar to that in which we have already gone. For instance, because $y = p\,e^{rx}$.

$$dy = e^{rx}\{rp + dp\} = e^{rx}\{r + d\}\,p$$

$$d^2 y = e^{rx}\{r^2 p + 2r\,dp + d^2 p\} = e^{rx}\{r + d\}^2 p$$

$$d^3 y = e^{rx}\{r^3 p + 3r^2 dp + 3r\,d^2 p + d^3 p\} = e^{rx}\{r + d\}^3\,p$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$d^n y = e^{rx}\{r^n p + n r^{n-1} dp + n\tfrac{n-1}{2} r^{n-2} d^2 p + \ldots d^n p\} = e^{rx}\{r + d\}^n p.$$

Hence employing the contracted operations which relate to p

$$X = \left\{\begin{matrix} d^n y \\ A d^{n-1} y \\ B d^{n-2} y \\ C d^{n-3} y \\ \ldots \\ \ldots \\ \ldots \\ \ldots \end{matrix}\right\} = e^{rx}\{(r+d)^n p + A(r+d)^{n-1} p + B(r+d)^{n-2} p + \ldots\ldots$$

But r being a root of (8), the sum of all the first terms or coefficients of p vanish. Dividing therefore by e^{rx}, and putting $2_n, 3_n, 4_n \ldots$ and $2_{n-1}, 3_{n-1}, 4_{n-1}, \ldots$ for the binomial

nomial coefficients of the 2d, 3d, 4th, . . . coefficients of the nth and $n-1$th powers respectively, the equation becomes

$$e^{-rx}\mathrm{X} = \left.\begin{array}{l} 2_n r^{n-1} \\ \mathrm{A}2_{n-1} r^{n-2} \\ \mathrm{B}2_{n-2} r^{n-3} \\ \mathrm{C}2_{n-3} r^{n-4} \\ \cdots \\ \cdots \\ \cdots \end{array}\right| dp + \left.\begin{array}{l} 3_n r^{n-2} \\ \mathrm{A}3_{n-1} r^{n-3} \\ \mathrm{B}3_{n-2} r^{n-4} \\ \mathrm{C}3_{n-3} r^{n-5} \\ \cdots \\ \cdots \\ \cdots \end{array}\right| d^2 p + \ldots \quad d^n p$$

or, $\mathrm{X}_1 = d^{n-1}p + \mathrm{A}_1 d^{n-2}p + \mathrm{B}_1 d^{n-3}p + \ldots \mathrm{Q}_1 p$ (9)
after integrating and representing $c + \int \mathrm{X}e^{-rx}$ by X_1 and the coefficients of the above development by A_1, B_1, C_1, ... Now this being a linear equation with respect to p of one degree lower than the one proposed (7), may by a similar process be depressed to another,

$$\mathrm{X}_2 = d^{n-2}p_1 + \mathrm{A}_2 d^{n-3}p_1 + \mathrm{B}_2 d^{n-4}p_1 + \ldots - \mathrm{P}_2 p_1$$

wherein p_1, X_2 are new functions of x, and A_2, B_2, . . . other numerical coefficients. In the same way this last may be depressed to

$$\mathrm{X}_3 = d^{n-3}p_2 + \mathrm{A}_3 d^{n-4}p_2 + \mathrm{B}_3 d^{n-5}p_2 + \ldots \mathrm{O}_3 p_2$$

and so on continually to

$$\mathrm{X}_t = d^{n-t}p_{t-1} + \mathrm{A}_t d^{n-t-1}p_{t-1} + \mathrm{B}_t d^{n-t-2}p_{t-1} + \ldots (10)$$

and if $t = n$ at length to

$$\mathrm{X}_{n-1} = dp_{n-2} + \mathrm{A}_{n-1}p_{n-2}$$

and $\qquad \mathrm{X}_n = p_{n-1}$ . . . . (11)

one of the numeral coefficients disappearing at each step of depression.

Again, we have seen that if r be a root of (8), the equivalent algebraic equation of (7),

$$\mathrm{X}_1 = c + \int \mathrm{X}e^{-xr}$$

And by the same assumptions we have

$$\mathrm{X}_2 = c_1 + \int \mathrm{X}_1 e^{-x\mathrm{R}}$$

$$\mathrm{X}_3 = c_2 + \int \mathrm{X}_2 e^{-x\mathrm{R}_1}$$

. . . .

. . . .

$$\mathrm{X}_t = c_{t-1} + \int \mathrm{X}_{t-1} e^{-x\mathrm{R}_{t-2}} \ldots . (12)$$

supposing R, R_1, R_2, . . . R_{t-2} are respectively the roots of the

the equivalent algebraic equations of the $n-1$th, $n-2$th, ... $n-t+1$th dimensions. Therefore by this result and (11)

$$p_{n-1} = X_n = c_{n-1} + \int X_{n-1} e^{-xR_{n-2}} \ldots (13)$$

But according to the process we have followed

$$y = p e^{xr}$$
$$p = p_1 e^{xR}$$
$$p_1 = p_2 e^{xR_1}$$
$$p_2 = p_3 e^{xR_2}$$
$$\cdots\cdots$$
$$\cdots\cdots$$
$$p_{t-1} = p_t e^{xR_{t-1}}$$

and hence,

$$p_t = p_{t-1} e^{-xR_{t-1}} = p_{t-2} e^{-x(R_{t-1} + R_{t-2})} = p_{t-3} e^{-x(R_{t-1} + R_{t-2} + R_{t-3})}$$

and generally

$$p_t = p_{t-v} e^{-x(R_{t-1} + R_{t-2} + \ldots R_{t-v})}$$

Now by the law of derivation which we have followed, $R_0 = R$, and $p_0 = p$; putting therefore $v = t$

$$p_t = p e^{-x(R_{t-1} + R_{t-2} + \ldots R)}$$

or,
$$y = p e^{xr} = p_t e^{x(R_{t-1} + R_{t-2} + \ldots R + r)} \quad \ldots . (14)$$

a general expression for the value of y. Changing therefore t into $n-1$

$$y = p_{n-1} e^{x(R_{n-2} + R_{n-3} + \ldots R + r)} = X_n e^{x(R_{n-2} + R_{n-3} + \ldots R + r)} \quad (15)$$

which since (12) gives a general relation between X_n, X_{n-1}, X_{n-2}, ... is the integral of (7) the equation proposed. But forasmuch as the quantities R, R_1, R_2, ... have no relation assigned them, this solution can hardly be considered as practically serviceable; and we shall hence endeavour to investigate their general dependence on each other and on r.

If the proposed equation (7) did not contain X, it is clear that, since $p e^{rx}$ which is one value of y and would in that case have p a constant, p_t would be a constant; and consequently the exponent $R_{t-1} + R_{t-2} + \ldots R + r$ must be a root

root of the equivalent algebraic equation (8). And because this must be the case for every value of t from 0 to $n-2$,

$$\begin{array}{lcl}
r = r & & R = r_1 - r \\
r_1 = r + R & & R_1 = r_2 - r_1 \\
r_2 = r + R + R_1 & & R_2 = r_3 - r_2 \\
r_3 = r + R_1 + R_2 & \text{which give} & R_3 = r_4 - r_3 \ldots (16) \\
\cdots\cdots & & \cdots\cdots \\
\cdots\cdots & & \cdots\cdots \\
r_{n-2} = r + R + R_1 + \ldots R_{n-3} & & R_{n-2} = r_{n-1} - r_{n-2} \\
r_{n-1} = r + R + R_1 + \ldots R_{n-3} + R_{n-2} & &
\end{array}$$

supposing $r, r_1, r_2, \ldots r_{n-1}$ to be the n roots of (8). Hence (15) becomes

$$y = e^{xr_{n-1}} X_n = e^{xr_{n-1}} \Big\{ c_{n-1} + \int X_{n-1} e^{x(r_{n-2} - r_{n-1})}$$

$$\text{or } y = e^{xr_{n-1}} \times$$

$$\Big\{ c_{n-1} + \int e^{x(r_{n-2} - r_{n-1})} \Big\{ c_{n-2} + \int e^{x(r_{n-3} - r_{n-2})} \Big\{ c_{n-3} + \cdot\cdot \int e^{-xr} X. \quad (17)$$

which is the integral in a calculable form, free from all intermediate reductions, and containing all the n roots of the equivalent algebraic equation, and n arbitrary constants.

By partially performing the integrations (17) is transformed into

$$y = e^{xr_{n-1}} c_{n-1} + \frac{e^{xr_{n-2}} c_{n-2}}{r_{n-2} - r_{n-1}} + \frac{e^{xr_{n-3}} c_{n-3}}{r_{n-3} - r_{n-2}} + \ldots\ldots \frac{e^{xr} c}{r - r_1}$$

$$+ e^{xr_{n-1}} \int e^{x(r_{n-2} - r_{n-1})} \int e^{x(r_{n-3} - r_{n-2})} \ldots\ldots \int e^{-xr} X \ldots (18)$$

which in many cases is a more convenient form than (17), and, as we shall hereafter see, particularly serviceable in establishing the completeness of the solution.

☞ In consequence of the difficulty of printing correctly (17), (18), and the preceding formula, the author informs the reader that each n, $n-1$, $n-2, \ldots$ belongs to the r above it, that x is a factor to each r, and the whole product an exponent to e; also in (18) that c_{n-1}, c_{n-2}, c_{n-3}, are each factors to its contiguous power of e.

[To be continued.]

 LXXI. *On*

LXXI. *On the Action of Iodine and Fluosilicic Acid.* By M. VARVINSKY, *Engineer in the Russian Mines**.

IT appeared to me probable that action would occur between iodine and fluosilicic acid gas, on account of the weakness of the affinities of the elements of the gas for each other; and I imagined that some new compound would be formed by these two bodies. I communicated this idea to Dr. Ure of Glasgow, and he kindly allowed me to verify it by experiment in his laboratory; the results of these trials were as follows: on causing fluosilicic acid gas, evolved from a retort, to mix with the vapour of iodine which was formed in the receiver adapted to the retort, I observed that the receiver became lined with a white crust. When the action had evidently ceased, I took off the receiver and poured water into it; a portion of silica was immediately precipitated in a gelatinous state, and I obtained a yellowish solution, the colour of which was attributable to an excess of iodine, for by evaporation it became perfectly colourless. The solution was filtered, and being strongly acid it was saturated by the addition of carbonate of ammonia; carbonic acid was liberated with effervescence, and at the same time silica was deposited. I took the precaution of adding excess of carbonate of ammonia, so that on filtering the liquor it was found to be alkaline (A). On evaporating this solution in a platina crucible to the consistence of a syrup, I saw with surprise that it not only gradually lost its alkalinity, but that although it originally restored blue colours which had been reddened, it became eventually perfectly acid. Lastly, on carrying the evaporation still further, I obtained some beautiful small crystals, or rather plates, of a bright golden yellow colour. The crystals possessed the property of an acid, for they reddened blue vegetable colours; they were more soluble in hot than in cold water, and with a strong solution of potash they formed a gelatinous salt of a very disagreeable taste.

In order to determine whether fluoric and iodic acids were present in these crystals, I treated a solution of them with nitrate of barytes; this salt produces a precipitate with difficulty, and not without the assistance of heat. Solution of starch mixed with another portion of the solution, produced no change in it, unless sulphuric acid was added; in this case the blue colour denoted the presence of the iodine.

May it not be concluded from these experiments, that fluoric and iodic acids are both contained in these crystals, and that

* Communicated by the Author.

they

they exist in chemical combination? I am inclined to think that these crystals are not fluo-iodic acid, and I am convinced that they contain no ammonia; for on pouring sulphuric acid into the solution (A) boiling in a platina crucible, I observed a yellowish gaseous substance arise, which condensed in the form of a yellow crust on the sides of a glass receiver: having dissolved this crust in water and evaporated the solution, I obtained yellow crystals perfectly similar to those already described.

I thus send you the imperfect results of experiments still more imperfect; but being on a journey, I am unable to pursue these researches as far as I could wish. But if you should esteem my attempts not altogether useless, you will greatly oblige me by giving them a place in your Journal; for it seems to me no abuse of the attention of chemists to direct it to new objects of research.

LXXII. *On some new double Chromates. By Mr.* Henry Stokes*.

In Dr. Thomson's "First Principles of Chemistry," when speaking of chromate of zinc, he says: "this salt was obtained by mixing solutions of sulphate of zinc and chromate of potash in the atomic proportions. The chromate of zinc precipitated in the state of a yellow powder. The supernatant liquid being still yellow, was concentrated. It yielded two distinct sets of crystals intimately mixed with each other; namely, bichromate of potash and sulphate of zinc, tinged yellow by chromate of potash." Vol. ii. p. 357.

On preparing by this process some chromate of zinc, a good deal of this yellow salt formed, along with bichromate of potash: but on examining the form of the salt, I immediately saw that it was not that of sulphate of zinc, being a flat rhombic prism, with the acute angles truncated. It being hence evident that it was a distinct salt, I set about examining it.

In the first place it was obvious from its yellow colour that it contained chromic acid. When an acid solution of nitrate of barytes was added, a white precipitate fell down, showing the presence of sulphuric acid; carbonate of soda occasioned a white flocculent precipitate of carbonate of zinc: potash was also suspected to be present. 50 grains of the crystals were dissolved in distilled water, and a solution of nitrate of silver added while any precipitate fell down: this precipitate of

* Communicated by the Author.

 chromate

chromate of silver was of a deep red colour, and when dried weighed 0·6 grain.

21·25 : 6·5 : : 0·6 : 0·18 = chromic acid.

To the solution which passed through, nitrate of barytes was added, sulphate of barytes precipitated, which when heated weighed 54·09 grains.

14·75 : 5· : : 54·09 : 18·33 = sulphuric acid.

The solution was now evaporated to one half, and sulphate and muriate of soda added so as to separate the excess of barytes and silver in solution: the precipitate was separated, and carbonate of soda added to the solution which passed through: carbonate of zinc fell down, which when dried weighed 14·45 grains; 14·2 grains when heated to redness were reduced to 9·7 grains:

14·2 : 9·7 : : 14·45 : 9·87 = oxide of zinc.

50 grains of the crystals were heated in a small platina crucible over a spirit-lamp, and found to lose 12·6 grains. This was water. By this heat the chromic acid cannot be decomposed; but on exposing the dry salt to a strong red heat, an additional loss of 0·1 grain was sustained; and on digesting the dry mass in water, an insoluble precipitate remained of oxide of chrome. To the clear solution 51·5 grains of pure carbonate of potash were added; the carbonate of zinc was separated, and the solution evaporated to dryness: sulphuric acid was then added to decompose all the carbonate of potash, and the whole again evaporated to dryness; the salt obtained when dried was found to weigh 81·05 grains.

11 : 6 :: 81·05 : 44·2 potash. Of this 44·2, 35·31 grains belong to the carbonate of potash added; for

8·75 : 6 : : 51·5 : 35·31—then 44·2—35·31 = 8·91 the potash of the salt.

Analysis.		
	Sulphuric acid	18·33
	Chromic acid	0·18
	Oxide of zinc	9·87
	Potash	8·91
	Water	12·60
	Loss	0·11
		50·00 grains.

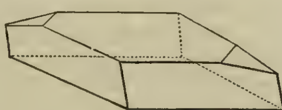
In preparing chromate of nickel, I mixed chromate of potash and sulphate of nickel in the atomic proportions. At first no precipitate took place; but on heating the solution, the chromate of nickel fell down copiously. To insure its total separation the solution was boiled to dryness, the residuum digested

digested in water, and the precipitate collected on a filtre. The solution which passed through had a deep red colour, and on cooling deposited crystals of a fine grass-green colour in the form of oblique rhombic prisms, with the acute angles truncated just like the salt obtained from the sulphate of zinc; and on analysis it proved to be very similarly composed, for by the above process it afforded the following results:

Sulphuric acid	18·260
Chromic acid	·978
Oxide of nickel	8·200
Potash	9·862
Water	12·700
	50·000 grains.

A similar salt may be obtained by mixing chromate of potash and sulphate of copper. It is of a light green colour, and has precisely the same form as the salts already described. In every instance after the first crop of crystals had been removed, and the solution further evaporated, bichromate was formed.

The crystalline form of all these salts appeared to be the same. The annexed figure represents their shape; they are pretty soluble in water, and undergo no change from exposure to the atmosphere. What appears most remarkable about these salts, is the small quantity of chromic acid which they contain; and in this respect they seem to have some analogy to the orange phosphate of lead described by Mr. Vernon in the Philosophical Magazine for May last.



As these salts, as far as I know, have never been described, and as Dr. Thomson appears to mistake their nature, I publish this brief notice of them now, although it is two years since I first prepared them.

Dublin, Oct. 2, 1827.

LXXIII. *On Nitric Acid, and on a peculiar Sulphate of Potash.* *By* R. Phillips, *F.R.S. L. & E.*

HAVING lately had occasion to attend to the preparation of nitric acid, some circumstances occurred with respect both to the acid and the residual salt, which have not, I believe, been noticed by any chemical author. Intending to prepare

pare nitric acid of the greatest strength, I mixed in a retort 70 parts of nitrate of potash with an equal weight of sulphuric acid of sp. gr. 1·8442 at 60°. The heat was continued for eight hours; during the latter four of which, the contents of the retort were in complete fusion, and in the quarter of an hour previous to discontinuing the operation, only one drop of nitric acid was obtained.

The nitric acid procured was of a yellow colour, with a tint of red; it weighed 46·13 parts, and the salt remaining in the retort amounted to 92·87 parts, showing that one part only of the 140 of nitre and sulphuric acid employed was lost during the operation.

The sp. gr. of the nitric acid at 60° was 1·5033, and I found by one experiment that 615·4 grains of it decomposed 456·8 of carbonate of lime; and by another 586·8 decomposed 435·6 of the same substance. These results agree almost precisely, and show, that 46·13 parts, the whole quantity of acid produced from 70 of nitre, would dissolve 34·24 parts of carbonate of lime. An atom of nitre being represented by 102, the atom of acid 54, which it contains, is equivalent to 50 or one atom of carbonate of lime; the acid contained in 70 of nitre should therefore dissolve 34·31 of this substance, from which, if we deduct 34·24, the solvent power of the acid actually obtained, there will be a deficiency of only 00·07 of a part,—a loss which, considering the nature of the operation, will be considered as very small. As 50 of carbonate of lime are equivalent to 54 real nitric acid, 34·24 the quantity dissolved by 46·13 of the liquid acid, indicate 36·98 of real acid, the product therefore consists of

Real nitric acid . . .	36·98	or	80·16
Water	9·15		19·84
	46·13		100·00

Supposing this acid to be a definite compound of 2 atoms of acid 108, and 3 of water 27, it would consist of

Real acid	36·90	or	80
Water	9·23		20
	46·13		100

I have mentioned that the bisulphate of potash remaining in the retort weighed 92·87 parts: now 102 or one atom of nitre decomposed by two atoms of sulphuric acid 80, would yield 128 one atom of anhydrous bisulphate of potash, or if combined with 9, one atom of water, it would be 137; as then 102 : 137 : : 70 : 94—exceeding the quantity I obtained by by only 1·13 part. We may then regard the various products of the operation as follows:

2 atoms of nitric acid, combined with
3 atoms of water, and
2 atoms of bisulphate of potash, containing
2 atoms of water.

Supposing, however, two atoms of nitre to be decomposed by four atoms of sulphuric acid, the above products appear to contain one atom too much water; for the strongest sulphuric acid consists, as is well known, of an atom of acid and one of water. It will, however, be observed, that I made use of sulphuric acid of sp. gr. 1·8442; and this, according to Dr. Ure's table, with which my experiments nearly agree, consists of nearly 79·1 acid, and 20·9 water. Now a compound of four atoms sulphuric acid = 160, and five atoms water = 45, would consist very nearly of 78·1 acid, and 21·9 water, differing only one per cent from the composition of the acid which I used, as inferred from Dr. Ure's table: this circumstance will of course account for the presence of the additional atom of water in the products.

To the supersulphate of potash remaining in the retort, I added nearly an equal weight of water: by the application of heat, the salt was readily dissolved, without ebullition, and consequently, with but little diminution of the water. The salt obtained by the cooling of the solution consisted of extremely minute filaments resembling asbestos in appearance; a part of the residual solution was so retained by the capillary attraction of the crystals, that it could not be separated by draining, and it was necessary to absorb it by filtering paper.

The primary form of bisulphate of potash is either a right rhombic prism, or an octahedron with a rhombic base, and the crystals are usually so flat as to be tabular: it appeared to be improbable that the acicular crystals which I have now described, should be a variety of either of the primary forms above mentioned. I thought they might, however, be bisulphate of potash containing more or less than the two atoms of water, which are known to exist in it in its common form. To determine this point I made the following experiments: 100 grains of the filamentous salt, which had been dried by exposure to the air in a moderately warm room, were dissolved in water, and solution of muriate of barytes was added as long as precipitation took place; the sulphate of barytes after washing and igniting, taking the mean of two experiments, weighed 154·75 grains, equivalent to 52·45 of sulphuric acid.

To expel the water of crystallization, as well as the excess of acid, 100 grains of the filamentous salt were heated to redness

ness in a platina crucible; the neutral sulphate of potash remaining weighed 78·4 grains, and consequently 21·6 of sulphuric acid and water were expelled. Now as 88 of sulphate of potash contain 40 of sulphuric acid, 78·4 contain 35·6, which deducted from 52·45, the whole quantity contained in 100 grains of the salt, leave 16·85 as the sulphuric acid expelled by heat, with 4·75 of water of crystallization; it will be observed that the quantity of sulphuric acid separated by heat from the supersulphate, is as near one half that remaining in the neutral salt, as 16·85 to 17·8: the salt in question appears therefore to be sesquisulphate of potash, or to consist of

		Theory.	Experiment.
3 atoms sulphuric acid . .	120	53·33	52·45
2 atoms potash	96	42·66	42·80
1 atom water	9	4·00	4·75
	225	99·99	100·00

Or it may be regarded as constituted of

2 atoms of sulphate of potash	176
1 atom of liquid sulphuric acid	49
	225

It is extremely difficult to procure the sesquisulphate of potash free from bisulphate; and on repeating an attempt to prepare it, as nearly as possible in the mode already described, I procured a large quantity of bisulphate and a small proportion of sesquisulphate. I am well aware that different salts are obtainable by using different quantities of water, for the same proportions of acid and base will yield either sulphate, sesquisulphate, or bisulphate of potash; and I have found that in order to procure bisulphate, the solution must be much concentrated. But I am unacquainted with the precise circumstance to which the production of sesquisulphate is to be ascribed; it may perhaps be referrible to some peculiarity of temperature in influencing the time of cooling.

In the sesquisulphate subjected to analysis, minute crystals of bisulphate of potash could occasionally be detected; and very pure bisulphate was obtained by evaporating the residual solution after separating the acicular salt; and eventually the solution became extremely acid, and ceased to afford crystals of any kind.

When a mixture of crystallized bisulphate and sesquisulphate of potash is exposed to the air, while it retains some of the solution from which it was crystallized, a formation of arborescent crystals occurs at the surface. I have not yet collected a sufficient quantity of this salt for examination, but it is probably only sesquisulphate.

LXXIV. *Notes*

LXXIV. *Notes on the Geological Structure of Cader Idris.* *By* Arthur Aikin, *Esq. F.G.S. &c.**

CADER IDRIS is a mountain ridge running almost E. and W. to the south of, and nearly parallel with, the course of the river that flows from the town of Dolgelle to the sea at Barmouth. Its length is about eight miles and a half in a straight line. It consists of a winding summit ridge somewhat depressed at each end, but on the whole considerably higher than the neighbouring mountains. Its western extremity terminates on the sea-shore in a steep slope ending in a cliff, the entire height of which is about 1500 feet: its eastern extremity called Geygraig, is 2267 feet high, the summit is 2914 feet, and the other points are of intermediate altitudes between those just mentioned, and were ascertained trigonometrically by Lieut.-Col. Colby and his assistants.

The northern face of the mountain is so steep as to be in few places accessible, and, in many, approaches to perpendicular. The southern face is a long slope with a varied undulating surface, consisting chiefly of bog and sheep-walk, and concealing from view the subjacent rock. The summit overlooks two remarkable hollows. One, of a semicircular figure, commonly called the crater, forms part of the northern face: the two extremities of the semicircle are respectively 2894 and 2655 feet high. The descent to the Goat's Pool (Llyn y Gafer), which lies in the bottom of the crater, is above 1000 feet, and almost perpendicular: the pool itself is 1835 feet above the level of the sea.

The other hollow, called Cwm y Cae, is on the S.E. of the summit: it is of an irregular elliptical figure, with a break or opening on the S.E. into the valley of Tal y Llyn: at the bottom of this hollow is also a pool about 1660 feet above the level of the sea.

The hill called Mynydd pen y Coed is the highest of those which stand out on the southern slope of the mountain, being 2504 feet. The upper part is but little obstructed by vegetation; and wherever the rock can be seen it evidently consists of beds of slate, very regular, and rising N.E. by N. at an angle of about 35°: on the N.E. end, however, of the hill, near the edge of Cwm y Cae, the beds, without changing their direction, bend up sharply so as to increase their angle to about 50°. Induced by this circumstance, I took a line of section from this part of the hill by the edge of Cwm y Cae, over Craig y Cae, to the margin of the crater, and thence to the Goat's Pool. Mynydd pen y Coed consists almost entirely

* From the Geological Transactions, Second Series, vol. ii. p. 273.

of strata of soft bluish-grey clay-slate. The beds which lie in succession below this, and occupy the ground to the edge of the crater, are grauwacke, consisting of coarse blackish-blue slate inclosing concretions of splintery quartz more or less earthy, and compact splintery quartz with imbedded crystals of pyrites, in parts more or less ochrey and cellular, probably from decomposition of the pyrites.

To these succeed beds of quartz-rock differing from the preceding only in being more vitreous, and these last rest upon a bluish-grey quartz-rock rendered porphyritic by a few crystals of felspar.

These beds all rise N.E. by N., but their angle of elevation is continually increasing, and the last bed forms the summit of Craig y Cae. The space from this to the margin of the crater is occupied by alternations, in nearly vertical beds, of soft glossy clay-slate, of coarser slate with ochrey spots and small cells, and of grauwacke with concretions of quartz, in some beds rendered porphyritic by imbedded crystals of felspar as well as quartz. Porphyritic quartz-rock also occurs, as well as slaty potstone, that is, slate intimately mixed with steatite. There is also inserted about the middle of this series a single bed of a brownish-grey rock, which effervesces strongly for a short time with acids, and by exposure to weather becomes porous and of a darker brown. It appears to be ferruginous quartz, intimately mixed with carbonate of lime.

The next bed (which forms part of the summit of Cader Idris) is composed of globular concretions a foot or more in diameter, which separate into compact angular pieces. This rock is excessively hard and strongly resists decomposition; in consequence of which it appears as a ridge or wall, a few feet higher than the adjacent beds. It contains numerous specks of pyrites, and has small roundish prominences on the weathered surface, somewhat resembling the orifices of tubes. In very thin shivers it melts into a black glass, and is probably a trap-rock.

The perpendicular wall of the crater is between 900 and 1000 feet in height, and presents the following series of beds from the top.

Three or four alternations of slate and grauwacke.

Fine-grained greenstone in irregular columns.

Slate.

Fine-grained columnar greenstone similar to the preceding.

Slate inclosing a bed of calcareous iron-shot quartz.

A porphyritic rock, consisting of quartzy compact felspar containing, imbedded, crystals of felspar, and a green matter which decomposes by exposure to weather, leaving cells lined with red oxide of iron. This bed rises about N.W. by N., and is composed of irregular columns from 6 to 14 feet long.

It

It ascends obliquely on the west, to that part of the ridge of the mountain called the Saddle, 2655 feet high, which is overspread with columns similar to those just mentioned, except that they are mostly lying prostrate: it also forms the basin in which the Goat's Pool at the bottom of the crater is contained.

On the northern side of the pool is a kind of mound consisting of a rock composed of chlorite, calcareous spar, quartz and glassy felspar. The calcareous spar is white, or of a dull purplish colour. There are several of these beds; of which the upper is a kind of conglomerate imbedding roundish masses of the same kind of rock, but harder, and with a smaller proportion of calcareous spar, and large angular pieces of slate. Below this is a bed harder and more compact than the former, and arranged in irregular columns like starch. This rests upon a more slaty rock of the same kind.

All these beds decompose by exposure to weather through the decay of the calcareous spar, showing the greater part of the mass to consist of this ingredient: some of the decomposed pieces are very light and spongy, and I presume are the substance which by some persons has been described as cellular lava. The relation of these beds to the columnar porphyry which precedes them I have not been able satisfactorily to make out in this place; but upon the whole I am inclined to think that they rest upon the porphyry, as this latter rock makes its appearance on the north as well as on the south of them.

These calcareous beds being readily distinguishable from the others among which they occur, I endeavoured to trace their extent. To the east, as far as the ridge of the mountain stretches, the valley in which they ought to be found is so covered with peat bog and with large masses that have fallen from the cliffs above, that I could not examine it with the requisite minuteness: I obtained, however, a very good section of the descent northwards from Geygraig (the eastern extremity of the mountain), without finding these beds; and I conclude that they do not exist there. I then examined the foot of the ridge to the west of the crater, and had the satisfaction of finding what I sought below Twrr mawr, one of the points of the mountain 2148 feet high. An inaccessible precipice of irregular columnar shafts occupies the first 700 or 800 feet from the top, which, lower down, breaking into groups or steps, allows of examination. Here I found a columnar porphyry, evidently the same rock as is associated with the calcareous beds at the Goat's Pool. Three of these façades or steps consist of nearly vertical coarsely slaty tables, which are again divided by vertical joints at right angles to the tabular structure, and thus form tolerably regular quadrangular co-

 lumns.

lumns. Between every two steps of porphyry lies a bed or step of green calcareous rock disposed in rude irregular columns; and, still lower down, is the same calcareous rock in coarsely slaty beds, resting on hummocks of soft and not very crystalline greenstone, and interstratified with common and indurated slate. The beds rise N.W. at an angle of about 85°.

The point or projection of the mountain next to the west of Twrr mawr is called Mynydd Coeswyn, and presents appearances very similar to those described. The top is slate, resting on a very hard compact porphyritic greenstone in thick curved tabular masses, which are rudely subdivided into rhomboidal columns, the convex side of the curve being outwards. At the foot of the porphyry are beds of common blue slate rising W. by N.; then comes the green calcareous rock, imbedding angular pieces of slate; then nearly vertical beds of a heavy black slate; then imperfectly slaty grey schist; then beds of the green calcareous rock in which the general slaty structure may be seen at a little distance, but also passing into the large lenticular and obscurely columnar structure. These latter beds rest immediately on columnar greenstone; which at the plane of junction, but no where else, incloses or has involved masses of the calcareous rock, so cellular as to appear like large sponges.

A very remarkable mountain (the name of which I could not discover, and which I therefore for distinction's sake call the Stony Mountain) extends for about two miles parallel to Cader Idris, forming the northern boundary of the little valley in which the Goat's Pool and another small lake are situated. The height of it is about 1700 feet, and it is distinguished from most others in its neighbourhood by several characters. On looking towards Cader Idris from Dolgelle, the white roundish crags of this mountain are very conspicuous; and their height is such, that only one peak of Cader Idris appears beyond and above them. On a nearer examination it is found to be composed of rounded tubercular crags and hemispherical bosses of trap, like enormous ovens, rising group above group, the intervals between which are filled up with coarse pasture and bog. The surface of the bosses is comparatively smooth, and generally reticulated by veins of quartz usually not more than half an inch thick; some of them, however, are considerably wider; and areas occur occasionally, four or five yards across, of white massive quartz several inches thick, with an obscurely slaty structure adhering to the surface of the trap. Many of the groups, when seen in profile, appear to be of a very irregular and thick slaty structure; but, when viewed in front or looking down upon them, are evidently clusters of columns

columns laterally aggregated and intersected by oblique irregular joints. The columnar structure is remarkably perfect on the northern side of the mountain, where terraces of columns sloping towards the body of the mountain, (each terrace in succession retired behind the lower one,) present a flight of enormous steps, and show this remarkable character of the trap-rocks in great perfection. The base of each group generally consists of slaty greenstone and porphyritic slate surrounding the group, and rising up to it on all sides, and therefore evidently a member of this trap formation. On the slaty greenstone rests a bed of highly indurated siliceous schist, which like the other regular strata rises N. by E. or W.

On the south side of the mountain may be seen the connection of the green calcareous rock with those just mentioned: its lowest bed rests on a porphyritic grauwacke, and this latter on a slaty greenstone.

About half way between Dolgelle, and the Stony Mountain, on the Towyn road, is a large quarry of syenite. It is composed of green hornblende intimately mixed with compact felspar, and is rendered porphyritic by concretions of lamellar flesh-red felspar. It is traversed by numerous veins of quartz, in which are imbedded fibres and curved crystals of pale wine-yellow thallite. On the S.W. flank of this syenite are applied nearly vertical beds of schist running about N.W. and S.E. Those beds that immediately rest on the syenite are considerably indurated, the others are less so. Beds of potstone are found very near the schist, and probably interstratified with it. The northern side of the syenite is covered by a soft dark green steatitical rock obscurely slaty, on which rest beds of a green slaty rock not very different from the former, and these are covered by beds of soft blue slate: all these beds rise at a high angle S.E., that is, towards the nucleus of syenite; and the green steatitical beds are penetrated by veins of quartz and thallite, stained green by carbonate of copper.

The descent northwards from Geygraig, the eastern extremity of Cader Idris, also shows in a very interesting manner the connection of the trap with the stratified beds. The southwestern part of the flat surface of Geygraig is covered by peat, so as to conceal the subjacent rock; but the north-eastern end consists of a few beds of unaltered blue slate, to which succeed beds of schist disturbed in position, in some places indurated, in others cellular; and in every part containing imbedded lumps of very hard porphyritic greenstone: sometimes the schist contains numerous small glands of quartz and of felspar, and is largely mixed with siliceous schistus of a dull green colour. This rests on highly inclined beds of a porphyritic

phyritic slate, and this latter on imperfect amygdaloid, which itself rests on the columnar basaltiform trap.

On descending from Geygraig to Dolgelle by the edge of the valley of the Aren, first occur craggy hummocks of massive trap more or less columnar, surrounded by slaty trap in mantle-shaped strata, and covered in different parts by beds of a shining micaceous clay-slate. Near Dolgelle the river flows over beds alternately slaty and massive; the former are distinctly steatitical, and will probably be considered as potstone; the latter differ from them in being harder and consisting of a larger proportion of quartz. Sometimes they contain carbonate of lime, and then effervesce briskly for a short time with acids.

On tracing up the Dolgelle river, the Ynnion, for a few miles towards Bala, no rock occurs *in situ* except massive crystalline greenstone; but on turning to the N. up a small lateral valley, I found beds of the same kind of rock as appear at Dolgelle in the bed of the Aren, some of which are sufficiently calcareous to burn into a reddish-brown sandy lime, and which therefore may perhaps be called limestone: it contains no organic remains. Beds very similar to these, and in all probability a continuation of them, appear at the foot of the hill at Llaneltid bridge over the Mawddach, on the road from Dolgelle to Barmouth, where they rise N.W. at an angle of about 20°, and form the upper members of a series of the grauwacke formation, composed of common blue slate, of finely foliated gray slate more or less calcareous, and of coarse green slate. These slaty beds alternate with sandstone consisting chiefly of quartz, in grains varying from a very minute size to that of hazle-nuts, intermixed with calcareous spar, the whole cemented together partly by quartz and partly by carbonate of lime. This sandstone, in the lower beds, is less calcareous, and mixed more or less with scales of slate; it also by exposure to weather splits into masses somewhat like starch, which stand nearly at right angles to their planes of stratification.

From the facts above detailed, it will I think be evident, that Cader Idris and the ground between that mountain and the Mawddach, as well as the northern boundary of the valley, consist of various well-known transition rocks rising in general N. by E. or W.: that the beds both at the northern and southern extremities are at low angles not greater than 20°; that the intermediate beds are at high angles approaching to vertical; that they rest upon and are interrupted by trap-rocks more or less columnar; that the trap-rocks themselves are surrounded in many places by mantle-form strata, which in some instances are obviously of the same materials as the trap, and differs differ only in structure,—but which sometimes bear a less obvious resemblance to the trap; and, from exhibiting transitions from that rock to those which compose the regular strata, are probably the latter, more or less changed by contiguity with the trap.

LXXV. *Notices respecting New Books.*

Illustrations of the Geology of Sussex: containing a General View of the Geological Relations of the South-Eastern Part of England; with Figures and Descriptions of the Fossils of Tilgate Forest. By GIDEON MANTELL, *F.R.S., Fellow of the Royal College of Surgeons, &c. &c.* London, 1827. 4to. pp. 92. Plates XXII. and a Map.

THE author of this work has set an example to men of a scientific turn of mind resident in districts of the country of particular interest in a geological point of view, which we hope to see extensively and zealously followed. We allude to his careful and minute investigation of the strata and organic remains occurring in his vicinity in Sussex, which he has communicated to the public in his former work on the Geology of Sussex, as well as through the medium of some other publications, and now also in the volume before us. Anticipating great improvements in geological science from the detailed examination of formations by those who, living on the spot, can devote much more time and attention to them than other geologists who may visit them are able ordinarily to bestow, we should wish that every important assemblage of strata in our island had its respective local inquirer; who, bringing the general ascertained facts of the science to bear upon the peculiar phænomena of his own district, might obtain results reciprocally illustrating those general facts, with the same success that has attended the active labours of Mr. Mantell.

In the preface to this work it is stated, that although but four years have elapsed since the publication of the author's former volume, yet the new and important facts that have subsequently been established, render a different classification of the strata indispensible; and the adoption also, in some instances, of a new nomenclature. He has therefore found it necessary to take a general view of the geological relations of the whole county; and although a repetition of some portions of the first volume has thereby been rendered unavoidable, yet the whole subject is placed before the reader in a more interesting and intelligible point of view, than could possibly have been obtained by any other arrangement. The interest of Mr. Mantell's former work, however, is not by this means diminished; for that contains, he states, a more extended description of the Fossils of the Chalk Formation than any other publication; and it is also a record of numerous facts relating to the geology of the South of England. The present work consists of two parts, the first of which contains a comprehensive view of the geological relations of the county of Sussex, and the adjoining parts of Hampshire, Surrey, and Kent; the second part is occupied by descriptions of the organic remains in the strata of Tilgate Forest.

The following tabular arrangement of the strata of Sussex, with which the first division of the work commences, will convey a correct idea of the subject, and at the same time evince the precision of Mr. Mantell's investigations. "The

" *The* STRATA *of* SUSSEX *arranged according to their* ORDER *of* SUPERPOSITION*: commencing with the uppermost or newest Deposit.*

Formations.	*Subdivisions and Mineralogical Characters.*	*Organic Remains, Observations, &c.*	*Localities.*
	ALLUVIAL DEPOSITS.		
ALLUVIUM. *The effect of causes still in action.*	Tufaceous deposits.	Incrustations of tufa on moss, leaves, &c.	Springs near Pounceford, Folkington, and Tower Hill near Horsham.
	Blue clay, silt, sand, and gravel.	Trunks of trees, marine and fresh-water shells of recent species.	Valleys of the Arun, Adur, Ouse, and Cuckmere.
	Peat and subterranean forests.	Trunks and branches of trees, leaves, hazel nuts, &c.	Lewes and Pevensey Levels, Felpham, Little Horsted, Isfield, Hastings.
	Sand, gravel, and comminuted shells, drifted inland from the sea shore.	Comminuted shells, &c.	Cliffs near Shoreham.
DILUVIUM. *The effect of causes no longer in action.*	Clay, loam, and gravel.	Bones and teeth of the horse and elephant.	Indiscriminately on the surface and in the valleys of the older formations.
	Chalk rubble and partially rolled flints, &c.	The same.	Cliffs from near Shoreham to Rottingdean.
	Boulders and pebbles of sandstone and ferruginous breccia.	Siliceous sandstone, the grey wethers of the western counties.	Summits and valleys of the Downs, Brighton, Falmer, Alfriston, Lewes.
	TERTIARY FORMATIONS. *(Partly Marine, and partly Fresh Water.)*		
PLASTIC CLAY.	Clay, sand, and gravel.	Cerithia, Cyclades, Ostreæ, Cyrenæ, teeth of fishes, &c. Leaves of terrestrial vegetables; cone of an unknown plant. Subsulphate of alumine, gypsum, surturbrand, &c.	Castle Hill, near Newhaven. Chimting Castle, near Seaford. Falmer, Lewes, &c.

London Clay.	Blue clay.	Ampullariæ, Turritellæ, Venericardiæ, and other marine shells peculiar to the London clay. Vertebræ, teeth, and palates of fishes.	Bracklesham Bay, Stubbington, &c.
	Grey calcareous sandstone.	Pectunculi, vermiculariæ, ampullariæ, teeth of fishes.	Bognor, Barn, and Mixen rocks.

SECONDARY FORMATIONS.

(*Marine*)

Chalk.	Chalk with flints. (*Craie blanche.*)	Nodules and veins of flint; pyrites, chalcedony, crystallized carbonate of lime, &c. Ammonitæ, nautili, belemnitæ, &c. fishes, crustaceæ, echini; vertebræ, and other bones of saurian animals; zoophytes, wood, marine plants.	Upper portion of the South Downs.
	Chalk without flints.	Pyrites; calcareous spar; fishes, crustaceæ, shells, zoophytes, echini, wood, marine plants.	Lower division of the South Downs.
	Grey marl. (*Craie tufeau.*)	Calcareous spar, pyrites, ammonitæ, turrilitæ, scaphitæ, echini; fishes rarely; crustaceæ; marine plants.	Base of the Downs; Hamsey, Southbourn, Lewes, &c.
	Firestone—or upper green sand. (*Craie chloritée ou glauconie crayeuse.*)	Marl, with an intermixture of green sand. Ostrea carinata, cirri, ammonitæ, turrilitæ, and other fossils of the grey marl.	Southbornu, Steyning, Bignor, Nursted; on the Northern edge of the Grey Marl.
	Galt or Folkstone marl—a blue marl, with veins of red ochre.	Gypsum; sulphuret of iron; nuculæ, belemnitæ, ammonitæ, nautili, catilli, inocerami; fishes, crustaceæ, &c.	Willingdon, Ringmer, Newtimber, near Arundel, &c.
Shanklin Sand.	Sand of various colours; green, grey, white, and ferruginous. Beds and concretions of chert, &c. Ironstone.	Casts of gervilliæ, trigoniæ, patellæ, modiolæ, venericardiæ, &c.	Pevensey, Chilley, Langley Point, Laughton, Ditchling, Wiston, Parham, Haslemere, &c.

(*Fresh Water?*)

Formations.	*Subdivisions and Mineralogical Characters.*	*Organic Remains, Observations, &c.*	*Localities.*
Weald Clay.	Septaria of argillaceous iron-stone.	Abounds with the remains of Cypris-faba; also Paludinæ, Cyrenæ, and scales of fishes.	Resting-Oak-Hill n^r Cooksbridge, Harting Combe, &c.
	Blue clay with beds of Sussex marble.	Viviparæ and Paludinæ; Cypris-faba.	Weald of Sussex, from Laughton to near Petworth.
Hastings Sands and Clays.	Fawn-coloured sand and friable sandstone.	Lignite; imperfect remains of ferns.	Bexhill, Horsted, Fletching, Eridge Park.
	Strata of Tilgate Forest. Sand and friable sandstone. Compact calciferous sandstone. (*Tilgate stone.*) Conglomeritic sandstone. Blue clay or marl.	Teeth and bones of Megalosaurus, Iguanodon, Plesiosaurus, and Crocodile; turtles, birds, fishes; arborescent ferns and palms, shells of the genera unio, paludina, cyrena, mactra, &c. Viviparæ and bones—rarely.	Hastings, Ore, Chailey, Tilgate Forest, Horsham, Loxwood, &c.
	White sand, friable sandstone, grit, &c. alternating with clay, &c.	Several species of fern; lignite; the sandstone contains immense quantities of bivalves, probably tellinæ or cyrenæ.	Rye, Winchelsea, Hastings, East Grinstead, Worth, Crawley, Tunbridge Wells, &c.
	Ashburnham beds; bluish grey limestone, alternating with blue clay, and sandstone shale. The lowermost strata in Sussex; contain beds of the *Tilgate stone.*	Immense quantities of the casts of bivalve shells, resembling tellinæ or cyrenæ. Lignite and carbonized vegetables.	Archer's Wood near Battel, Brightling, near Burwash, Pounceford, Hurstgreen, Rotherfield, Darvel's Wood, Etchingham.

Mr. Mantell next proceeds to a detailed account, occupying fifty pages, of the strata enumerated in the foregoing table, which is illustrated by a geological map and sections, and also by a view of a quarry in Tilgate Forest, forming the frontispiece to the work.

In the beginning of the second part, he observes: "Before we enter upon the description of the fossils of Tilgate Forest, let us for a moment consider what would be the nature of an estuary, formed by a mighty river, flowing in a tropical climate over sandstone rocks and argillaceous strata, through a country clothed with palms, arborescent ferns, and the usual vegetable productions of equinoctial regions, and inhabited by turtles, crocodiles, and other amphibious reptiles? In such a deposit we should expect to find sand more or less consolidated with layers of clay and silt: containing water-worn fragments of the harder portions of the rocks in the form of pebbles or gravel; bones, teeth, and scales, more or less rolled, of the amphibiæ that had lived and died on the borders of the river; the branches and stems, and leaves of the vegetables that grew on its banks, intermingled with fresh water shells, and a small proportion of marine productions; a few bones of aqnatic birds might also probably be observed:—the strata of Tilgate Forest present precisely such characters, and such an assemblage of animal and vegetable remains."

The strata of Tilgate Forest, as may be seen in the Tabular View, belong to the formation recently denominated by Dr. Fitton "the Hastings Sands;" formerly designated as the Iron-sand formation. They consist of the following principal members: "*Sand* and friable sandstone, of various shades of green, yellow, and ferruginous; surface oftentimes deeply furrowed: *Tilgate stone*, very fine compact bluish or greenish grey sandstone, in lenticular masses; surface oftentimes covered with mamillary concretions; the lower beds frequently conglomeritic, and containing large quartz pebbles: *Clay* or *Marl* of a bluish grey colour, alternating with sand, sandstone, and shale."

The vegetable remains of these strata are first described. They consist of the petrified trunks of large plants, belonging to that tribe of vegetables of the ancient world which is so common in the carboniferous strata, and appears to have held an intermediate place between the *Equiseti* and the Palms; of the stems of a gigantic monocotyledonous vegetable, bearing some analogy to the *Cacti* and *Euphorbiæ*; the foliage of ferns, and the stalks of arundinaceous plants. Of these, five are described at length; viz. *Clathraria Lyellii*, Mantell; *C. anomala*, Geol. Trans. N. S. i. 423, belonging to the same tribe as the Lepidodendron of Sternberg; *Endogenites erosa*, allied to the Palms; *Hymenopteris psilotides*, an elegant fern, referred by A. Brongniart to his *Sphænopteris*; *Pecopteris reticulata*, another fern, approaching to the tropical *Nephrodia*; and *Carpolithus Mantellii*, a rare fossil fruit, resembling the grains or kernels of some kinds of palms, such as the *areca*. The shells found in the Tilgate strata occur for the most part in the state of casts; they consist of species of the genera *Cyrena*, *Unio*, *Mya*, *Paludina*, and *Vivipara*. The remains of fishes consist of the detached bones, teeth, and scales, of several kinds; but as no united part of the skeletons, or of the scaly coverings has been found, it is scarcely possible to ascertain how far they are related to existing species. Four kinds of teeth, some palates, scales, fins, and vertebræ, are described by Mr. Mantell.

These strata contain the remains of at least *three* very distinct kinds of Chelonian reptiles; viz. a fresh-water species allied to the *Trionyx;* an unknown species of *Emys,* resembling a fossil fresh water turtle found in the Jura limestone; and a marine species of *Chelonia,* related to the fossil turtle of Maestricht. These are well described and figured in the work.

"Saurian Animals, or Lizards. Of the family of the Saurians," observes Mr. Mantell, "the bones, teeth, and scales, of at least four genera of gigantic species, have been discovered in the strata of Tilgate Forest, namely, Crocodile, Plesiosaurus, Megalosaurus, and Iguanodon; but hitherto no connected portions of their skeletons have come under our observation. The teeth, both in form and structure, present such striking differences as to be readily distinguished from each other, and from those of existing species; but the bones possess so many characters in common, that when we consider the broken and detached state in which they occur, and their intermixture with the debris of turtles, of vegetables, of fishes, and of shells, the difficulty of the attempt to identify the bones of the respective animals, seems almost insurmountable to observers so distant from any collection of comparative anatomy, as ourselves. We therefore claim the indulgence of the reader should the results of our investigations appear to be in some respects inconclusive and unsatisfactory; since, under such circumstances, rigorous conclusions must not be expected. We shall first describe the teeth, and such of the bones as are referrible, with but little doubt, to one or other of the above-mentioned genera; and afterwards notice those osseous remains, which we are unable to appropriate with any degree of certainty or probability."

The Crocodilian remains in the Hastings strata consist of the teeth, scales, *vertebræ,* ribs, some bones of the extremities, and *os frontis* of a species first stated by Mr. Mantell to approach very nearly to the crocodile with concave vertebræ found at Havre, and since pronounced by Cuvier to be almost identical with the fossil crocodile of Caen, which belongs to the Gavials.

Of the *Megalosaurus Bucklandii,* a gigantic animal, the remains of which were first discovered by Dr. Buckland in the Stonesfield-slate, the teeth, ribs, *vertebræ,* and other bones, have been found in the strata of Tilgate Forest.

The remains of the *Iguanodon,* a gigantic herbivorous Saurian discovered by Mr. Mantell himself, are described at some length, and portions of nine engravings devoted to the illustration of them.

"*Iguanodon.* The discovery of the teeth and other remains of a nondescript herbivorous reptile in the strata of Tilgate Forest, a reptile 'encore plus extraordinaire que tous ceux dont nous avons connoissance *,' is to us one of the most gratifying results of our labours. The first specimens of the teeth were found by Mrs. Mantell, in the

* Cuvier, Oss. Foss. tome v. 2nd part. p. 351.

course

coarse conglomerate of the Forest in the spring of 1822 *; and we have subsequently collected a most interesting series, displaying every gradation of form, from the perfect tooth in the young animal to the last stage, that of a mere bony stump worn away by mastication. These teeth are comparatively rare; and the only locality in which they have hitherto been noticed, is in the immediate vicinity of Tilgate Forest; they have not been discovered in any other part of England. Their external form is so remarkable, and bears so striking a resemblance to the grinders of the herbivorous mammalia, that when the tooth figured in Plate XIV. fig. 14, first came under our notice, its analogy to the incisors of the rhinoceros led us to suspect whether the deposit in which it was found might not be of diluvial origin. Subsequent discoveries proved that these teeth belonged to an unknown herbivorous reptile; but their structure was so extraordinary, that we determined to obtain, if possible, the opinion of Baron Cuvier upon the subject. We accordingly transmitted specimens to our kind friend Mr. Lyell, who was then residing in Paris, and by whom they were presented to that illustrious naturalist. M. Cuvier favoured us with the following remarks on the fossils submitted to his examination. "Ces dents me sont certainement inconnues; elles ne sont point d'un animal carnassier, et cependant je crois qu'elles appartiennent, vu leur peu de complication, leur dentelure sur les bords, et la couche mince d'émail qui les revêt, à l'ordre des reptiles; à l'apparence extérieure on pourrait aussi les prendre pour des dents de poissons analogues aux tetrodons, ou aux diodons; mais leur structure intérieure est fort différente de celles là. N'aurions-nous pas ici un animal nouveau, un reptile herbivore? et de même qu' actuellement chez les mammifères terrestres, c'est parmi les herbivores que l'on trouve les espèces à plus grande taille, de même aussi chez les reptiles d'autrefois, alors qu'ils étoient les seuls animaux terrestres, les plus grands d'entr'eux ne se seraient-ils point nourris de végétaux? Une partie des grands os que vous possidez appartiendrait à cet animal, unique jusqu'à présent dans son gènre. Le tems confirmera ou infirmera cette idèe, puisqu'il est impossible qu'on ne trouve pas un jour une partie du squelette réunie à des portions de mâchoires portant des dents. Si vous pouviez obtenir de ces dents adhérentes encore à une portion un peu considérable de mâchoire, je crois que l'on pourrait résoudre le problème." In the second part of the fifth volume of the *Ossemens Fossiles*, the learned author figures several of these teeth, and minutely describes their form and structure †. From the resemblance of the perfect specimens to the teeth of certain species of Iguana, we proposed to distinguish the fossil animal by the name of *Iguanodon*; and a memoir on the extraordinary dentature of the original was read before the Royal Society, and honoured with a place in its Transactions ‡.

* See vol. i. p. 54. No. 40, 41.

† Oss. Foss. vol. v. p. 350.

‡ "Notice on the *Iguanodon*, a newly discovered fossil herbivorous reptile, from the sandstone of Tilgate Forest, in Sussex." *Phil. Transactions*, 1825, Part I.

The

The author then describes in detail the organization of the teeth of this animal, after which he proceeds.

If we attempt to discover among the recent lizards a dentature at all analogous, we shall find among the Iguanas alone any kind of resemblance; yet even here we cannot fail to remark, that in this as in every other instance, if there be a general analogy, there are also striking and important differences in the structure of the primitive animals of our planet, and of those which are its present inhabitants. Of the Iguana there are several species; but the only skull we have had an opportunity of examining, is that of an individual from Barbadoes, we believe of *I. tuberculata* *. Fig. 6. Plate XX. is a view of the inner surface of the right side of the upper jaw of this animal, of the natural size, and which is magnified four diameters at fig. 5 †. The teeth are slightly convex externally, and have a ridge down the middle; they are slightly concave on the inner surface; different views of the crown of a tooth are seen, largely magnified, in fig. 4. Plate XVII. In the angular form of the crown and its serrated edges, they strikingly resemble the fossil, fig. 11. of the same plate. The new teeth are formed at the bases of the old ones, and lie in a depression at the root of the fang, as is beautifully shown in the magnified drawing, fig. 5. The jaw throws up a lateral parapet on the outside of the teeth; but they have no alveoli, nor any internal protection but the gum. From the above observations, it appears that the fossil teeth bear a greater analogy to those of the recent Iguana, both in their form and in the process by which dentition is effected, than to those of the crocodiles, monitors, and other living saurians. But notwithstanding this general resemblance, the remarkable characters resulting from the act of mastication, separate the original animal from all known genera. None of the existing reptiles perform mastication; their food or prey is taken by the teeth or tongue; so that a moveable covering of the jaws, similar to the lips and cheeks of the mammalia, is not necessary, either for confining substances subjected to the action of teeth as organs of mastication, or for the purposes of seizing or reaching food ‡. The herbivorous amphibiæ gnaw off the vegetable productions on which they feed, but do not chew them. Now 'as every organic individual forms an entire system of its own, all the

* Oss. Foss. vol. v. Plate XVI. Figs. 24, 25.

"The Iguanas are natives of many parts of America and the West India Islands, and are rarely met with any where north or south of the tropics. They are from three to five feet long from the end of the snout to the tip of the tail. They inhabit rocky and woody places, and feed on insects and vegetables. Many of the Bahama islands abound with them; they nestle in hollow rocks and trees; their eggs have a thin skin like those of the turtle. Though they are not amphibious, they are said to keep under water an hour. When they swim they do not use their feet, but place them close to their body, and guide themselves with their tails; they *swallow all they eat whole*. They are so impatient of cold, that they scarcely appear out of their holes but when the sun shines." Shaw's Zoology, vol. iii. p. 199.

† We are indebted to the kindness of Mr. Clift for the original drawings of thess parts of the recent Iguana.

‡ Rees's Cyclopæd. art. "Reptiles."

parts

parts of which mutually correspond, and concur to produce a certain definite purpose by reciprocal re-action, or by combining towards the same end,' it follows from the peculiar structure of the fossil teeth alone, that the muscles which moved the jaws, and the bones to which they were attached, were widely different from those of any of the living lizards; and consequently the form of the head of the Iguanodon must have been modified by these causes, and have differed from those of existing reptiles. Since the vegetable remains with which the teeth of the Iguanodon are associated consist principally of those tribes of plants that are furnished with tough thick stems, and which probably were the principal food of the original animal, we may be permitted to remark, that this peculiar structure of the teeth seems to have been required to enable the animal to accommodate itself to the condition in which it was placed. Hereafter, perhaps, some more fortunate observers may discover a portion of the head and jaw, and be able to confirm or refute our conjectures."

Some bones, &c., supposed to be referrible to the *Iguanodon*, are next described; and among them a *horn*, apparently belonging to that animal.

"We have now to request the reader's attention," observes Mr. Mantell, "to a very remarkable appendage, with which there is every reason to believe the *Iguanodon* was provided. This is no less than a *horn*, equal in size, and not very different in form, to the lesser horn of the rhinoceros. This unique relic is represented of the natural size, Plate XX. fig. 8. It is externally of a dark brown colour; and while some parts of its surface are smooth, others are rugous and furrowed, as if by the passage of blood-vessels. Its base is of an irregular oval form, and slightly concave. It possesses an osseous structure, and appears to have no internal cavity. It is evident that it was not united to the skull by a bony union, as are the horns of the mammalia. The nature of this extraordinary fossil was for some time unknown; and it is to the discrimination of M. Pentland, to whom a cast of it was shown by Professor Buckland, that we are indebted for the suggestion that it probably belonged to a saurian animal. It is well known that some reptiles of that order have bony or horny projections on their foreheads; and it is not a little curious, that among the Iguanas the horned species most prevail. The *Iguana cornuta*, which is a native of St. Domingo, resembles the common Iguana in size, colour, and general proportions; on the front of the head, between the eyes and nostrils, are seated four rather large, scaly tubercles; behind which rises by an *osseous conical horn or process covered a single scale**. That our fossil was such an appendage, there can be no doubt; and its surface bears marks of the impression of an integument by which it was covered, and probably attached to the skull. This fact establishes another remarkable analogy between the *Iguanodon* and the animal from which its name is derived."

The vertebræ, teeth, and other bones of some *Plesiosauri*, also occur among the fossils described in this work; and the catalogue terminates

* Shaw's Zoology, vol. iii. Part I. p. 203.

with

with an account of a few bones of birds which the Tilgate strata afford.

The work concludes with the subjoined interesting remarks.

"We cannot leave this subject without offering a few general remarks on the probable condition of the country, through which the waters flowed that deposited the strata of Tilgate Forest; and on the nature of its animal and vegetable productions. Whether it were an island or a continent, may not be determined; but that it was diversified by hill and valley, and enjoyed a climate of a higher temperature than any part of modern Europe, is more than probable. Several kinds of ferns appear to have constituted the immediate vegetable clothing of the soil: the elegant *Hymenopteris psilotoides*, which probably never attained a greater height than three or four feet, and the beautiful *Pecopteris reticulata*, of still lesser growth, being abundant every where. It is easy to conceive what would be the appearance of the valleys and plains covered with these plants, from that presented by modern tracts, where the common ferns so generally prevail. But the loftier vegetables were so entirely distinct from any that are now known to exist in European countries, that we seek in vain for any thing at all analogous without the tropics. The forests of *Clathrariæ* and *Endogenitæ*, (the plants of which, like some of the recent arborescent ferns, probably attained a height of thirty or forty feet,) must have borne a much greater resemblance to those of tropical regions, than to any that now occur in temperate climates. That the soil was of a sandy nature on the hills and less elevated parts of the country, and argillaceous in the plains and marshes, may be inferred from the vegetable remains, and from the nature of the substances in which they are enclosed. Sand and clay every where prevail in the Hastings strata; nor is it unworthy of remark, that the recent vegetables to which the fossil plants bear the greatest analogy, affect soils of this description. If we attempt to pourtray the animals of this ancient country, our description will possess more of the character of a romance, than of a legitimate deduction from established facts. Turtles, of various kinds, must have been seen on the banks of its rivers or lakes, and groups of enormous crocodiles basking in the fens and shallows.

"The gigantic *Megalosaurus*, and yet more gigantic *Iguanodon*, to whom the groves of palms and arborescent ferns would be mere beds of reeds, must have been of such prodigious magnitude, that the existing animal creation presents us with no fit objects of comparison. Imagine an animal of the lizard tribe, three or four times as large as the largest crocodile, having jaws, with teeth equal in size to the incisors of the rhinoceros, and crested with horns; such a creature must have been the Iguanodon! Nor were the inhabitants of the waters much less wonderful; witness the Plesiosaurus, which only required wings to be a flying dragon; the fishes resembling *Siluri, Balistæ*, &c."

The plates to this work are well executed in lithography; and we think it so excellent a specimen of the results of local geological research, that we hope, contrary to a remark in the preface, to see some further publications by Mr. Mantell on the geology of his native county."

LXXVI. *Pro-*

LXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE Royal Society re-assembled, for the session 1827-8, on November the 15th, when the following papers were read:

The Croonian Lecture;—On the muscles peculiar to organs of sense in particular quadrupeds and fishes; by Sir E. Home, Bart. V.P.R.S.

Experiments to determine the difference in length of the seconds pendulum in London and Paris; by Capt. E. Sabine, R.A. F.R.S.

Nov. 22.—On a peculiarity in the structure of the *ductus communis choledochus*, and of the pancreatic duct in man; by John Davy, M.D. F.R.S.

Observations on the action of the mineral acids on copper, under different circumstances; by John Davy, M.D. F.R.S.

On the structure of the knee-joint in the *Echidna setosa* and *Ornithorynchus paradoxus*; by Dr. R. Knox, F.R.S. E.

LINNÆAN SOCIETY.

Nov. 6—The Society first assembled for the session this evening; A. B. Lambert, Esq. V.P. in the chair.

A paper was read, entitled "An Account of a new species of *Pinus*, native of California;" by Mr. D. Douglas, A.L.S.: communicated by Mr. Sabine.

This plant covers large districts about a hundred miles from the ocean in latitude 43° north, and extends as far to the south as 40°. It attains its greatest size on low hills, where the soil consists of pure sand. The trunk grows from 150 to above 200 feet in height, varying from 20 to near 60 feet in circumference. The resin which exudes from the trees when they are partly burned, loses its usual flavour, and acquires a sweet taste, in which state it is used by the natives as sugar. The seeds are eaten roasted, or in coarse cakes. This tree is most nearly allied to *Pinus Strobus*. Spec. Char. "P. Lambertiana; foliis quinis rigidis scabriusculis, vaginis brevissimis, strobilis crassis longissimis cylindricis; squamis laxis rotundatis."

A paper was also read, entitled "Remarks on the *Antilope Chickara;*" by Robert Hills, Esq. F.L.S.

In this communication is described a specimen of the Chickara brought alive to this country, but which lately died. Its skeleton is deposited in the museum of the College of Surgeons.

Nov. 20.—The following papers were read: The generic characters of Formicaleo of Dr. Leach, with the description of two new species; by the Rev. Lansdown Guilding, B.A. F.L.S. M.W. & G.S. &c.

The writer states that though the larvæ swarm under every rock and shelter in the island, yet not a single perfect insect had been found.

The two new species are,

1. F. *Leachii*, fuscescens, flavido maculatus, alis hyalinis, subfalcatis, immaculatis: neuris ciliatis: oculis cupreis, pedibus pallidis. —In the dry sands of St. Vincent.

2. F. *tarsalis*, nigro-fuscescens, flavido maculatus, alis hyalinis, immaculatis,

maculatis, subfalcatis: neuris interruptè nigris: stigmate nigro: pedibus flaventibus, atro-variis.—Demerara.

Observations connected with M. de Gimbernat's communication. By John Murray, F.S.A. F.L.S. &c. &c.

This paper relates to the extraordinary production discovered by M. Gimbernat in the thermal waters in the island of Ischia; which, when recently cut from the rock, resembled raw flesh; it soon putrified, evolving ammonia. Dr. Schoner, of Copenhagen, was of opinion that it might be allied to *Oscillatoria thermalis.*

A letter was read from the President, appointing R. Brown, Esq., Vice-President, in place of the late venerable Dr. Goodenough Lord Bishop of Carlisle.

A letter from General Hardwicke was read, On the trees found at great depths in peat bogs in the county of Armagh.

Sir W. Betham states that these bogs are full of fine timber, chiefly oak and fir. Some cones accompanied the letter, which were stated by Mr. Lambert (from the chair) to be those of the *Pinus sylvestris*, though the cones are smaller than usual, approaching to the size and form of those of *Pinus Pumilio.*

A letter from J. Creswell, Esq. F.L.S., stated that a fish weighing one cwt. had been taken in the river Exe, unknown to the oldest fishermen. It is supposed to be the fish called Ombrina in the Mediterranean; the *Sciæna cirrhosa* Linn., *Umbrina* of Cuvier.

GEOLOGICAL SOCIETY.

Nov. 2.—The Society having this evening assembled for the session of 1827-8:—An extract was read, of a letter from Captain P. P. King, R.N., to Dr. Fitton, P.G.S., dated at Rio de Janeiro, 10th June, 1827:—with some observations on the specimens sent home by Capt. King; by the President.

The expedition under Capt. King, for the purpose of surveying the Straits of Magellan, left Monte Video on the 19th of November, 1826; and after putting into Port St. Elena, about lat. 45° south, and remaining for a day or two in the vicinity of Cape Fairweather, continued for ninety days within the Strait; during which time, its shores, to the east of Cape Froward, were surveyed under the superintendence of Capt. King himself; while his consort, under Capt. Stokes, examined the western entrance. The map and specimens sent to England, contain the results of these operations; and Capt. King intended to sail within a short time after the date of his letter, for the purpose of continuing the survey.

The coast at Port St. Elena is described by Capt. King as consisting of porphyritic claystone; of which the hills, from 300 to 400 feet high, are entirely composed. The specimens from thence consist of claystone, compact felspar, and hyperstene rock; and the beach affords a conglomerate, consisting of rounded fragments of these substances, cemented by carbonate of lime containing portions of shells, and resembling the recent calcareous conglomerates which abound on the shores of Asia Minor, Australia, and several other parts of the world.

Cape

Cape Fairweather is near the southern extremity of a range of coast, occupying between two and three degrees on the east of Patagonia; a great part of which is described in the Admiralty Chart, as being "like the coast of Kent, and consisting of steep *chalk hills;*" one of the prominences being named, from this supposed resemblance, "Beachy Head." This, however, from Capt. King's statement, would appear to be erroneous:—the whole coast examined by him, was found to be composed of horizontal strata of clay, which may be traced for several miles in unbroken continuity; the cliffs being from 300 to 400 feet in height, and entirely bare of vegetation. Some of the specimens, however, from this quarter, consist of a white marl, not unlike certain varieties of the lower chalk; and with these, are portions of a greenish sand-rock, much resembling that of the upper green-sand formation, and of a clay having many of the properties of fuller's earth. The pebbles of the shore consist of quartz, red jasper, hornstone, and flinty slate; but do not contain any stone resembling chalk flint.

Cape Virgins at the north-eastern entrance of the Straits of Magellan, consists of clay cliffs, like those of Cape Fairweather; and between these two Capes the coast is of the same character.

What may be called the eastern branch of the Straits, from Cape Virgins to Cape Froward, though its general course is from north-east to south-west, varies considerably in width and direction; but from thence to the western entrance, the direction is nearly straight, from south-east to north-west,—and the width much more uniform; and one of the principal points already determined by Capt. King's survey, is that the fissure constituting this portion of the Strait is continued in the same direction, for about a hundred miles towards the south-east from Cape Froward,—through St. Gabriel's Channel, and a deep inlet discovered by Capt. King, and named "Admiralty Sound," which runs nearly fifty miles into the interior of Terra del Fuego. This separation of the land, by a narrow rectilinear channel of such great length, being analogous to the division of Scotland by the chain of Lochs, on the line of the Caledonian Canal.

In proceeding westward from the eastern entrance, the coast gradually changes its character; and primitive rocks appear about Cape Negro near Elizabeth Island, where mountains of slate rise to the height of from 2000 to 3000 feet. Capt. King remarks that the direction of all the ranges, commencing at Port Famine about thirty miles from Cape Froward, is towards the S.E.; and that all the sounds and openings of the land in Terra del Fuego trend in the same direction; this being also the direction of the strata, which dip towards the south. This coincidence in the direction of the mountain ranges, has been carefully expressed on Capt. King's map; and he supposes that a similar structure holds good throughout the western branch of the Strait, from Cape Froward to the entrance on that side.

The specimens from Freshwater Bay, about 120 miles from Cape Virgins, on the Patagonian side of the Strait, consist of highly crystalline greenstone and hyperstene rock, resembling those of Scotland; and the pebbles and boulders on the shore, are of granite, hornstone, sienitic rock, quartz and flinty slate.

 The

The vicinity of Mount Tarn and Eagle Bay, about midway between Port Famine and Cape Froward, affords also porphyritic and crystalline rocks, abounding in hornblende, or hyperstene; with grauwacke, siliceous slate, and gray splintery limestone. The slate of Mount Tarn contains traces of organic remains.—The specimens from the south side of this eastern branch of the Strait consist of mica-slate approaching to gneiss, found at the entrance of St. Magdalen's Sound; and at Card Point on the south-west of St. Gabriel's Channel. The rocks at Cape Waterfall near Card Point, are of clay-slate; and the shores of Admiralty Sound afford granite and various porphyritic rocks; including clinkstone-porphyry, and greenish compact felspar. Capt. King mentions his having observed also reddish quartzose-sandstone, resembling that of the old-red-sandstone formation of Europe: and he remarks, that the soil over this rock is barren, while that above the slate produces luxuriant vegetation; beeches of great size growing there within a few feet of the water-side. In general, the hills in this part of Terra del Fuego appear to be of slate: they rise to the height of 3000 feet, and are covered with snow and ice. Mount Sarmiento, however, which is more than 5000 feet high, appears, from the shape of its summit, to be volcanic; and was called by the navigator, after whom it was named, "The Snowy Volcano."

The specimens sent by Capt. Stokes from the western branch of the Straits of Magellan, all consist of primitive rocks: Cape Notch, Cape Tamar, and the Scilly Islands affording granite; Port Gallant, and Cape Victory, gneiss and mica-slate; and Valentine's Bay, clay-slate much resembling that of Port Famine. These places are all on the north of the Strait. On the southern side, in Terra del Fuego, Cape Upright affords granite and gneiss; and the latter rock is found also at Tuesday Harbour, and in the neighbourhood of Cape Pillar: the columnar mass, from which that remarkable point was named, is composed of mica-slate.

Of the specimens sent by Capt. King from this remote quarter of the globe, it may be remarked, in general, that they agree perfectly with the rocks of Europe and other parts of the world;—the resemblance amounting, in several cases, to almost complete identity.

The reading was begun of a paper "On the Geology of Tor and Babbacombe Bays, Devon;" by H. T. De la Beche, Esq. F.R.S. &c.

Nov. 16.—The reading of Mr. De la Beche's paper, begun at the last Meeting, was concluded.

The coasts of Babbacombe and Tor Bays are composed of new red sandstone, carboniferous limestone, old red sandstone, and trap-rocks: and the sections presented by the cliffs exhibit various marks of disturbance, which the author conceives to have been caused by the intrusion of trap among the strata subsequently to their deposition.

1. The *new red-Sandstone* here consists of red conglomerate resembling that of Heavitree and Exeter, being made up of portions of old red sandstone, carboniferous limestone, shale, quartz, graywacke, and porphyry, with small crystals of felspar:—the whole cemented by a red paste, and occasionally interstratified with red sandstone and marl. The conglomerate is regarded by the author as the lowest part of the new red sandstone formation, and as the equivalent of the

röthe

röthe tödte liegende of Germany. And the fragments of porphyry included in it, are supposed to be the remains of pre-existing trap-rocks; both from their rounded form, and their admixture with the detritus of other formations inferior to the new red-sandstone.

This red conglomerate occupies three small districts:—1. That of St. Mary Church and Watcombe. 2. Tor-Moham. 3. Paington.—The first extending along the coast from the Ness Point (Teignmouth) to Oddicombe Sands; with the exception of an insulated mass of carboniferous limestone at Petit Tor, which is bounded by the conglomerate, and partially overlaid by it.

The conglomerate of Tor-Moham, connected with that of St. Mary Church by an isthmus, is of similar composition, and rests upon carboniferous limestone and old red sandstone.

Near Paington, the conglomerate abuts against the old red sandstone; and having fallen from the cliff in considerable quantity, near Livermeed and Preston Sands, has the appearance of underlying the latter.

2. *Carboniferous Limestone.*—The rocks of this formation in the neighbourhood of Torquay, have hitherto been regarded as belonging to the transition series; but the author supposes them to be identified with the carboniferous or mountain limestone, by their mineralogical characters and organic remains. The limestone is of a gray colour, traversed by numerous veins of carbonate of lime, is occasionally interstratified with marl, and generally reposes upon argillaceous shale,—the lower limestone shale of the carboniferous series. In the vicinity of trap, however, it assumes a semi-crystalline structure, and thus affords the numerous varieties of the well-known Babbacombe marble.

Very remarkable curves and contortions in the limestone strata are visible near Torquay; the disturbed beds in general dipping away from the old red sandstone. And on the west of Babbacombe, the coast exhibits the limestone and shale in great confusion; particularly where it is in contact with the trap of the promontory called Black Head.

At Saltern-Cove, near Goodrington, the limestone is intermixed with, and disturbed by, trap,—which appears to have imparted to it the character of serpentine, and to have so altered the calcareous rock that it does not effervesce with acids.

The author gives a general list of the organic remains in this deposit: including trilobites, encrinites, corals, nautili, orthoceræ, and several species of testaceous mollusca characteristic of the carboniferous limestone. A very singular fossil also is figured, which appears to have been attached in the manner of the Alcyonia; but whether it is to be classed with the corals, or considered as intermediate between the crinöidea and echinodermata, has not yet been determined.

The cavern called Kent's Hole, near Torquay, on the N.E., lately celebrated from its containing the remains of various antediluvian animals, is in this carboniferous limestone.

3. *Old Red Sandstone.*—This formation, which occupies a considerable space in this country, is well exposed at Cockington, where the

the sandstone is compact, micaceous and siliceous, and associated with a slaty rock. Near Ockham, and N.N.W. of Paington, the lowest beds lose their red colour, becoming more schistose; and these, as well as the grit and slate of Meedfoot Sands, seem to pass into grauwacke. The old red sandstone is extensively overlaid by unconformable beds of the new red conglomerate at Chelston near Cockington, and in other places.

4. *Grauwacke.*—At Westerland, there is a schistose and micaceous variety of grauwacke, containing stems of encrinites, corals, and bivalve shells.

5. *Trap Rocks.*—The connection of these rocks with the disturbed state of the stratified deposits, constitutes the chief interest of the tract described in this paper. A small headland, east of Babbacombe, consists of greenstone containing much iron pyrites, and traversed by veins of quartz, jasper, &c.; the contiguous limestones being semi-crystalline. On the west of the same place, another headland is composed of porphyritic greenstone, and occasionally amygdaloidal:—and here the trap is protruded upwards into the overlying argillaceous slate of the carboniferous limestone; the adjacent beds of shale being broken, much contorted, and some portions of them even included in the mass of trap; whilst the limestone in the upper part of the cliff also is much dislocated. In the inaccessible cliffs near Oddicombe Sands, the trap has intruded itself among the limestone and shale, the beds of which are much altered in character, and so broken up near the summit, that they are with difficulty distinguished from each other. The largest mass of trap on this part of the coast is at Black Head; it is remarkable as inclosing a large detached portion of the contorted limestone.

Near to a great fault at Oddicombe Sands, the argillaceous slate is elevated to the top of the cliff, and the adjoining new red conglomerate also rises, as if forced up by the same movement which had affected the slate.

The author conceives that the appearances of the coast which he has described, point out two distinct geological epochs:—1st. That of the formation of the new red conglomerate, after the limestone and shale had been partially broken up. 2ndly. The intrusion of the trap, at a period subsequent to the deposition of the conglomerate, and new red sandstone. And besides attributing the disturbed state of this region to the operation of trap, the author is disposed to refer to the same period and agency, the great dislocations in the oolitic series on the east of the tract which he has described; and to connect with the convulsion by which he supposes that disturbance to have been produced, the greater catastrophe which elevated the chalk of the Isle of Wight,—and even, possibly, that which threw up the main ridge of the Alps.

A paper was read, entitled, "Supplementary Remarks on the Strata of the Oolitic Series, and the Rocks associated with them, in Sutherland, Ross, and the Hebrides;" by Roderick Impey Murchison, Esq., Sec. G.S. F.R.S., &c.

The author, in company with Professor Sedgwick, having visited, during

during the last summer, the districts which he described in a former memoir, (Geol. Trans. 2nd series, vol. ii. part 2.) has been enabled to make some additional observations, and to collect further specimens illustrative of the strata of the oolitic series, and their associated rocks in the north of Scotland.

1. On the connection of the primary rocks with the secondary deposits on the east coasts of Sutherland and Ross.—The Ord of Caithness, and the mountainous ridge connected with it, which had been described as consisting of a rock made up of felspar, quartz, and a decomposing green substance, is now ascertained to contain well-crystallized mica. This granite, on its northern flank, supports the old red conglomerate; whilst to the south it occupies a cliff on and near the shore, the verge of which affords a remarkable breccia, compounded from all the beds of the oolitic series which occur upon this coast. These appearances were cursorily noticed in the author's paper above referred to; they are now described more in detail: and it is shown, that this breccia of sandstone shale, fossils and limestone, is tilted off from the granite, wherever that rock protrudes upon the shore; whilst the strata are regularly developed when the granite recedes into the interior:—and since the amount of disturbance is in every case proportioned to the greater or less proximity of the granite, the author infers, that this rock was elevated subsequently to the deposition of the oolitic strata. Thin beds of primary slaty rocks have been observed in several places, interposed between the secondary beds and the granite: and the greater portion of the Sutors of Cromarty consist of felspathose gneiss; which rock, however, is in some situations so much charged with veins of granite, that the whole has a granitiform aspect, whilst in other places the mass when decomposed strongly resembles the rock of the Ord of Caithness above mentioned.

That the granite of this coast has been elevated, is further rendered probable, by the position of the red conglomerate on the tops of the granitic mountains; thus giving to that deposit the appearance of overlying the more recent formations of the oolitic series, to which they are in fact superior in point of height above the sea.

Without dissenting from the opinions of other geologists, as to the formation of veins in gneiss by the injection of granite in a state of softness, the author states that Mr. Sedgwick and himself were led to a different hypothesis, in order to account for the appearance of the brecciated secondary beds in contact with the granite of this coast: and they suppose that the latter rock must have been upheaved, not in a liquid form, but in a state of solidity, since no veins or portions of the granite are to be met with in or above the breccia.

2. Denudation of Braambury, and Hare Hills.—These hills, the highest in geological position of the Brora district, and celebrated for their quarries of siliceous white sandstone abounding in fossils, afford, upon their sides and summits, distinct traces of a strong diluvial current, which has swept them free of covering matter, depositing in the plain of Clyne Milltown, a mass composed of the debris of the denuded hills, mixed with boulders of the coarse red conglomerate. A

large

large portion of the turf having recently been removed, the surface of the rock is now seen to be scored with parallel lines, precisely similar to those observed in other places, and described by Sir J. Hall, Dr. Buckland, &c. And in this case, although the surface of the ground is very unequal, and the dip and bearings of the denuded strata vary considerably, the direction of the markings is uniformly from N.N.W. to E.S.E.

3. Hebrides, and Mainland of the West Coast.—Pitchstone, a mineral not previously found in Scotland in association with the more recent stratified rocks, has been discovered by Professor Sedgwick and Mr. Murchison in two places; forming portions of trap dykes,—one of which cuts through the lias and inferior oolite at Carsaig Mull; the other traverses the cornbrash and forest marble, at Beal near Portree in Skye. The identity of the various secondary strata in the Isles of Mull, Skye, Pabba, Scalpa, Rasay, &c., is now established by the numerous organic remains which they have been found to contain, many of which belong to new species, but the greater number are well known as characteristic fossils of the oolitic formations in England.

The former vast, and perhaps continuous, extent of these deposits on the western coast, is further rendered probable, by their having been observed by Professor Sedgwick and the author, on the N.E. coast of Mull, at and near Tobermory; and at Applecross on the west coast of Ross-shire. In the latter place, lias-limestone, similar to that on the opposite shores of Skye and Rasay, rests conformably upon the new red conglomerate; and as the same fact had been previously remarked on the east coast, near Cromarty, evidence is thus afforded that the members of the oolitic series of Scotland, generally, were of subsequent formation to that great mechanical deposit; being lodged, apparently, in the basins or undulations presented by its surface.

A letter was read from G. W. Featherstonhaugh, Esq. to W. H. Fitton, M.D. P.G.S. &c. &c.; containing an account of an excavation in the chalk at Norwich.

The writer, having learnt that an extensive cavity in the chalk of Heigham Hill near Norwich, had been discovered about four years ago, in consequence of the workmen who were digging a well, having suddenly sunk into a vault, examined the place; and he describes the excavation as consisting of various galleries, (a plan of which is annexed to his letter,) of about eight feet in height, from two to five feet in breadth, and occupying a total length of 4600 feet. He conceives that the object of this laborious work, was to extract the flints, which were used in great quantity in the construction of the ancient buildings and walls of Norwich; since the nodules of flint have been almost entirely removed from the catacombs, while the chalk itself is left. And he states in support of this opinion, that upon re-opening the original entrance, which had been blocked up by ruins, the date 1571, with the name of one of the workmen, was found written on the side of the cavern:—a year which corresponds with a period, when the walls of the town are known to have been repaired with flints, and various buildings formed of them.

ROYAL

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Fourteenth Annual Report of the Council.

Pursuant to the resolution passed at the last annual meeting, the third volume of the Society's Transactions has been sent to the press; but the Council are sorry to report, that owing to various unavoidable delays, it is not yet completed; 300 pages, however, have been printed, and the Editors have pledged themselves that the volume shall be published during the present year.

The Council have the satisfaction of again announcing the continued prosperity of this Institution; and of the Museum in particular: indeed so great has been the increase of minerals during the last three years, that a new arrangement of the Museum had become necessary; which has been performed by the Curator with his usual zeal and ability.

Since the last anniversary, Sir Charles Lemon, Bart. has presented a numerous and valuable collection of organic remains from various parts of the countries bordering on the Mediterranean. These organic remains, together with those from the Malvern Hills, (presented by the same gentleman on a former occasion,) have been arranged in a new cabinet, in connection with those of the Paris Basin, of Sussex, of the Isle of Wight, and other places; the whole forming a series illustrative of this department of geology, in which the Museum was defective previous to these various donations.

The thanks of the Society are also due to another of its members, Dr. Davy, for excellent specimens of rocks, lavas, and various volcanic productions, which he himself collected last autumn, in Sicily, the Lipari Islands, and the neighbourhood of Naples. These minerals, together with those presented a few years ago by his illustrious brother, and by John Guillemard, Esq., exhibit beautiful and instructive examples of the volcanic formations of the Mediterranean. The Council beg to refer to the Curator's Report for a more particular account of the donations of minerals.

In consequence of this great increase of minerals, another room will be speedily wanted for the extension of the Museum; and the Council are glad to state, that the one now occupied by the Penzance Library may be procured, as this Institution is about to be removed to a larger apartment.

In order to complete the Geological Map of Cornwall, the Council beg to recommend the distribution of lithographic copies, on an enlarged scale, of the various portions of the map of the county, to members residing in the different districts. Several gentlemen have offered to take the space of a few miles, and to mark the boundaries of the different kinds of rocks, and as far as possible the course of the principal veins. In this way much may be effected in a comparatively short period, and by the united labours of the members this great object may be at length accomplished.

Professor Savi, of the mineralogical chair of the University of Pisa, has proposed, through the Rev. Canon Rogers, an exchange of minerals: but the Council regret exceedingly that they are unable to complete

plete their present engagements for want of duplicate specimens; and they therefore take this occasion to remind the members and friends of the Society, that they would render an essential service by the donation of Cornish minerals; as they would thereby promote the intercourse with other Institutions, by which the Museum would be greatly enriched.

In the course of the past year, the Society has had to regret the death of its worthy Treasurer, Henry Boase, Esq., a gentleman who ardently assisted Dr. Paris in the original formation of the Society, and whose unwearied services, both as its Treasurer and as a contributor to its Transactions, cannot be too highly appreciated.

(By Order) HENRY S. BOASE,
October 12, 1827. Secretary.

The following papers have been read since the last Report:—On the produce of the copper mines of Europe and Asia, and particularly those of Armenia. By John Hawkins, Esq. F.R.S.—On the occurrence of bowlder-stones and pebbles in the cliffs of the western part of Cornwall. By Joseph Carne, Esq. F.R.S.—On the formation of a singular variety of sparry ironstone: and on the origin of the substance of veins. By Henry S. Boase, M.D.—On the tin of the island of Banca. By the late Sir Thomas Stamford Raffles, F.R.S.—On the temperature of mines. By Robert Were Fox, Esq.—On the resemblance which subsists between St. Michael's Mount in Cornwall, and Mont St. Michel in Normandy. By Joseph Carne, Esq.—On the stream works of the western part of Cornwall. By Henry S. Boase, M.D.—Remarks on the geology of the neighbourhood of Nice, &c.: illustrative of a series of specimens presented to the Society. By Sir Charles Lemon, Bart.—An account of the native copper of Condorrow mine, in the parish of Camborne. By Edward W. W. Pendarves, Esq. M.P., F.R.S.—On the silver mines of Cero De Pasco in Peru. By Mr. Hodge.—An account of the quantity of tin produced in Cornwall in the year ending with the midsummer quarter 1827. By Joseph Carne, Esq.—An account of the produce of the copper mines of Cornwall, in ore, copper, and money, in the year ending the 30th June 1827. By Mr. Alfred Jenkin.

At the anniversary meeting, held on the 12th of October 1827, Davies Gilbert, Esq. M.P., V.P.R.S. &c. *President*, in the chair; the Report of the Council being read, it was resolved, That it be printed and circulated amongst the members.

The following gentlemen were then chosen Officers and Council for the ensuing year:—*President:* Davies Gilbert, Esq. M.P., &c. &c. —*Vice-Presidents:* Sir Christopher Hawkins, Bart.; Humphry M. Grylls, Esq.; Robert Were Fox, Esq.; The Rev. William Veale. —*Secretary:* Henry S. Boase, M.D.—*Treasurer:* Joseph Carne, Esq.—*Librarian:* T. F. Barham, M.D.—*Curator:* Edward C. Giddy, Esq.—*Assistant Secretary:* Richard Moyle, Esq.—*Council:* John Batten, Esq.; William Bolitho, Esq.; L. C. Daubuz, Esq.; Alfred Fox, Esq.; Pascoe Grenfell, Jun. Esq.; James Plomer, Esq.; William Reynolds, Esq.; Lieut.-Gen. Tench; William Williams, Esq.; Mr. W. J. Henwood.—*Ordinary Members elected since the last Report:* John

John Bullocke, Esq., Falmouth; John Samuel Enys, Esq., of Enys; Richard Fox, M.D., Falmouth; Charles James Fox, M.D., Plymouth; John Gilbert, Esq., Eastbourne; Day Le Grice, Esq., Trereife; James Plomer, Esq., Helston; The Rev. Thomas Robyns, Marystone; Thomas Robins, Esq., Leskeard; John Terrell, Esq., London.

LXXVII. *Intelligence and Miscellaneous Articles.*

NEW FORM OF AN EXPERIMENT IN ELECTRO-MAGNETISM; BY MR. JOHN LEWTHWAITE.

IT had frequently occurred to me that Mr. Faraday's experiment of the wire rotating round the magnet, and the magnet round the wire in two separate vessels, might be simplified by making the wire and magnet to rotate round each other in one vessel, and the plan I adopted is as follows: ABCD is a glass vessel with a hole in the bottom, through which passes the wire EF with a hook at

Fig. 1.

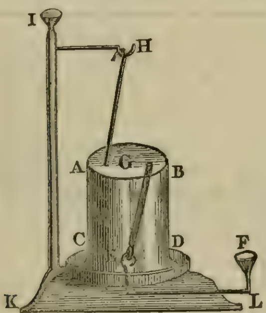
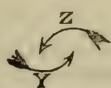


Fig. 2.



E, and a cup at F. To the hook E is affixed by means of a thread, the magnet GE floating nearly perpendicular in mercury; HG is a copper wire bent at H into the form of a hook, and amalgamated at both ends to insure perfect contact with the mercury in the glass vessel, and also with the hook H of the support HI which screws into the mahogany stand KL. The apparatus being thus arranged, a single zinc plate, six inches square, placed in a copper vessel with diluted nitrous acid, and a communication being made by means of a wire between the negative or copper side of the battery and the cup I, also another communication with the positive or zinc of the battery with the cup F, the wire and magnet will immediately commence a rotatory motion about each other, and that too in the direction of the arrows represented in fig. 2. viz. the wire HG will move in the direction of Y, and the magnet in the direction of Z.

Rotherhithe, July 10, 1827.

ARGILLACEOUS CARBONATE OF IRON.

Dr. Colquhoun has analysed many specimens of this ore, and has

 given

given the annexed tabular view of their composition in the last Number of Dr. Brewster's Journal.

	(*a.*)	(*b.*)	(*c.*)	(*d.*)	(*e.*)	(*f.*)	(*g.*)	(*h.*)	(*i.*)
Water	—	0·99	—	—	—	—	—	—	—
Carbonic acid . .	32·53	33·63	31·86	30·76	26·35	33·10	32·24	35·17	34·27
Protoxide of iron .	35·22	45·84	42·15	38·80	36·47	47·33	43·73	53·03	42·35
Protoxide of manganese . . .	0·00	0·20	0·00	0·07	0·17	0·13	0·00	0·00	—
Lime	8·62	1·90	4·93	5·30	1·97	2·00	2·10	3·33	3·78
Magnesia . . .	5·19	5·90	4·80	6·70	2·70	2·20	2·77	1·77	4·95
Silica	9·56	7·83	9·73	10·87	19·90	6·63	9·70	1·40	
Alumina . . .	5·34	2·53	3·77	6·20	8·03	4·30	5·13	0·63	
Peroxide of iron .	1·16	0·00	0·80	0·33	0·40	0·33	0·47	0·23	12·70
Carbonaceous or Bituminous matter	2·13	1·86	2·33	1·87	2·10	1·70	1·50	3·03	
Sulphur	0·62	0·00	0·00	0·16	0·00	0·22	0·02	0·00	
Moisture and loss .	—	—	—	—	1·91	2·26	2·34	1·41	1·95
	100·37	100·68	100·37	101·06	100·00	100·00	100·00	100·00	100·00

The specimens were obtained from the following places: (*a*) Crossbasket, about seven miles south-east from Glasgow. Sp. gr. 3·1793. (*b*) Ditto. Sp. gr. 3·3801. (*c*) Ditto. Sp. gr. 3·2699. (*d*) Ditto. Sp. gr. 3·1175. (*e*) Near the Clyde Iron Works, about four miles south-east from Glasgow. Sp. gr. 3·1482. (*f*) Ditto. Sp. gr. 3·2109. (*g*) Easterhouse, near the line of the Monkland Canal, and about six miles east from Glasgow. Sp. gr. 3·3109. (*h*) Neighbourhood of Airdrie, about ten miles from Glasgow. Sp. gr. 3·0553. (*i*) Vicinity of Crossbasket.—*Edinburgh Journal of Science, Oct.* 1827.

BERTHIERITE,—A NEW MINERAL SPECIES.

The following is extracted from Mr. Haidinger's account of this substance.

The Berthierite is an ore of antimony in the œconomical acceptance of the word; as it consists of four atoms of sulphuret of antimony, and of three atoms of protosulphuret of iron, the antimony being combined with twice as much sulphur as the iron. It occurs at Chazelles, in Auvergne, in a vein which promises to be very productive. It had been worked for some time, but was again abandoned on account of the bad quality of the antimony extracted. M. Berthier has imagined the following process, by which the metal obtained becomes perfectly pure. The mineral, without previous roasting, is to be melted with about one-third, or a little less, of its weight of metallic iron, to which is added a small quantity of sulphate of soda mixed with charcoal.

In regard to its external appearance, Berthierite much resembles some of the other species of the genus antimony-glance, as the common gray antimony, and the Jamesonite, and also the zinkenite. It occurs in elongated imbedded prisms, with a single pretty distinct longitudinal cleavage. Its colour is a dark steel-gray, inclining to pinchbeck-brown, with a metallic lustre. These properties are not sufficient to characterize the mineral, and a further account is promised. —*Ibid.*

STERNBERGITE,

STERNBERGITE,—A NEW MINERAL SPECIES.

I. Description.

Fundamental form. A scalene four-sided pyramid. P=128° 49′, 84° 28′, 118° 0′. Fig. 1.

$$a : b : c = 1 : \sqrt{1{\cdot}422} : \sqrt{0{\cdot}484}.$$

Simple forms. $P - \infty (a)$; $P\ (f)$; $P + 1\ (g) = 122°\ 17', 68°\ 22', 146°\ 34'$; $(\breve{Pr})^3\ (d) = 92°\ 28', 107°\ 17', 131°\ 17'$, $\breve{Pr} + 1\ (b) = 61°\ 35'$ $\frac{5}{4}\ \breve{Pr} + 3\ (c) = 13°\ 36'$, $Pr + \infty\ (i)$; $\frac{4}{3}\ \overline{Pr} - 3\ (h) = 153°\ 2'$.

Various combinations among these forms have been observed, one of them is represented, Fig. 2. They have all more or less the aspect of rhombic plates, with angles of 119° 30′, and 60° 30′, which is the base of the fundamental pyramid; often the acute angle is truncated.

Fig. 1.

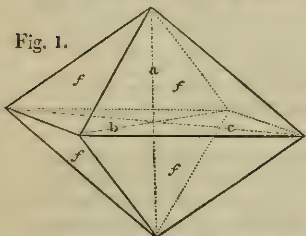
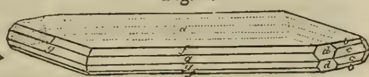


Fig. 2.



Cleavage highly perfect, and easily obtained parallel to the face *a*. No trace of cleavage in other direction of the lamellæ, which may be torn asunder like thin sheet-lead.

Surface of *a* delicately streaked parallel to the edges of combination with *h*, that is parallel to the long diagonal of the rhombic plates. Lustre more considerable upon these than upon the remaining faces, which are deeply streaked parallel to their intersections with *a*.

Lustre metallic. Colour dark pinchbeck-brown, rather darker than the colour of magnetic pyrites. Streak black. Tarnish, often violet-blue on all the faces except *a*.

Very sectile. Thin laminæ perfectly flexible. Hardness = 1·0...1·5, little superior to talc. Specific gravity = 4·215.

Compound varieties. Twin-crystals, joined parallel to a face of $P + \infty$. Generally several crystals are joined in an irregular manner, and implanted together, being fixed to their support with one of their sides, so as to produce rose-like aggregations and globules, with a drusy surface. Massive varieties usually present the aspect of a coarse-grained mica.

II. Observations.

1. The two specimens from which the preceding description is drawn up I first saw when in Prague in March 1826. They were pointed out to me as something not agreeing in several respects with the known species, by Professor Zippe, one of them in the collection of the National Museum, the other in the collection of Gubernialrath Neumann; the latter specimen was designated on the ticket as a pinchbeck-

pinchbeck-brown problematical fossil, crystallized in six-sided tables. Both these gentlemen entrusted me liberally with the specimens for examination, the only specimens then known to exist. I am happy to learn that Mr. Zippe has succeeded in finding out a few more specimens, in rummaging over some old store of minerals.

2. There exists a considerable deal of resemblance, as appears also from the characters given, between the Sternbergite, and the black tellurium, the flexible sulphuret of silver, and the rhombohedral molybdena-glance. As a species it is sufficiently distinct from all of them. On account of that resemblance it must receive its place in the order Glance of the system of Mohs; but whether as a genus of its own, or along with some one or the other of those enumerated, is as yet uncertain, while these species themselves are so imperfectly known. No systematic denomination can therefore be at present proposed for the new species. The name of Sternbergite, in proposing which I concur with my friends Neumann and Zippe, is particularly appropriate, as the species to which it applies was first observed in a public collection, belonging to an establishment chiefly formed by the exertions of that learned and patriotic nobleman, Count Caspar Sternberg.

3. No chemical analysis has yet been given of this substance. When treated with the blowpipe it gives in the glass tube a strong odour of sulphurous acid, loses its lustre, and becomes dark-grey and friable. Alone on charcoal it burns with a blue flame and sulphurous odour, and melts into a globule, generally hollow, with a crystalline surface, and covered with metallic silver. The globule acts strongly on the magnetic needle, and before the blowpipe has all the properties of sulphuret of iron. It communicates to fluxes the ordinary colours produced by iron, red while hot, and yellow on cooling, in the oxidating flame, greenish in the reducing flame. Borax very readily takes away the iron, and leaves a button of metallic silver. It appears therefore to consist of sulphuret of silver, combined with a large quantity of sulphuret of iron.

4. The locality of this interesting species is Joachimsthal in Bohemia. It must have been found at a rather remote period, as the specimens were discovered in old collections; and it is likely enough, on account of the economical value of Sternbergite as an ore of silver, that most of it has been melted down long ago. Moreover, it is chiefly accompanied with other ores of silver, as the red silver, the brittle silver, or prismatic melane-glance, and others.—*Haidinger in Brewster's Journal.*

JALAPIA.

Some time since, Mr. Hume, jun., of Long Acre, announced that he had discovered a vegeto-alkaline principle in jalap, to which he gave the name of Jalapia; it was obtained by digesting the root coarsely powdered in strong acetic acid for twelve or fourteen days: the acetic acid dissolves the alkali in question, and when ammonia is added to the solution, the jalapia is precipitated, which being dissolved in dilute sulphuric acid, yields crystals of sulphate of jalapia, a grain of which, according to Mr. Hume, acts as a cathartic.

M. Pelletier

M. Pelletier has examined this supposed salt of jalapia, and finds that it contains no peculiar vegeto-alkali whatever, but that it is composed of sulphate of lime and sulphate of ammonia. M. Pelletier remarks that if, as Mr. Hume supposes, it has acted as a purgative, the effect must have been produced by a "*forte prevention*," which has been known to be the case with other inert substances.—*Journal de Pharmacie, Aug.* 1827.

GLAUCOLITE,—A NEW MINERAL SPECIES.

This substance was first described by Mr. Sokoloff in the *Russian Journal of Mines**. It is found massive, presenting traces of cleavage. The fracture is splintery and uneven; lustre vitreous; colour lavender-blue, passing into green. It is translucent on the edges. Hardness $=5\cdot0 \ldots 6\cdot0$. Specific gravity $=2\cdot721$ Bergemann; $=2\cdot9$ John.

According to Dr. Bergemann†, it consists of

Silica . . .	54·58
Alumina . .	29·77
Potash . .	4·57
Lime . . .	11·08
	100·

Before the blowpipe it melts difficultly on the edges, but is soluble in borax and salt of phosphorus. It loses its colour.

It occurs in compact felspar and granular limestone, with talc, near Lake Baikal, in Siberia.

EFFECTS OF OIL OF CROTON ON THE EYE.

M. Commensuy of Rheims, accidentally received a few drops of the oil of Croton in the eye: he washed it plentifully with water, but notwithstanding this he suffered extreme pain, and in less than a quarter of an hour his eye became very much inflamed, as well as the side of the face; he suffered noise in the ears and a kind of vertigo, for he could not stand without the fear of falling. He suffered horribly during fourteen hours; but by the application of leeches, &c. the inflammation was reduced, and in a week he was able to attend to business, the eye remaining only a little weak.—*Journal de Pharmacie.*

CHLOROCYANIC ACID.

M. Serullas has obtained this compound, which he terms cyanuret of chlorine, in a separate state, and perfectly pure, with great facility by the following process. Moisten cyanuret of mercury with chlorine, stop the flasks, and put them in a dark place; in eight or ten hours the contents are entirely converted into perchloride of mercury and cyanuret of chlorine. This compound is extremely deleterious; very soluble in water, and still more so in alcohol: it suffers no change in these fluids, and may be separated from solution in water by passing it over chloride of calcium into bottles placed in a freezing mixture, where it becomes solid.

* *Poggendorff's Annalen der Physik u. Chemie*, 1827, Number ii. p. 267.

† *Von Leonhard's Handbuch der Oryktognosie*, 2d Edit. p. 741.

One

One of the most remarkable properties of the cyanuret of chlorine, is that of crystallizing in long transparent needles at 0° Fahr.; and at about 20° or 25° above this it becomes liquid, or under a pressure of four atmospheres, and it remains so in tubes hermetically sealed.

When bottles containing cyanuret of mercury, either moistened with or dissolved in chlorine, are exposed to the sun's rays, no cyanuret of chlorine is obtained; but instead, if a yellow oily fluid is formed, which is denser than water, and which is probably a mixture of chloride of azote and protochloride of carbon, it is readily converted by heat into azote, carbonic acid, muriatic acid, and perchloride of carbon. Without the assistance of heat the same effects are produced, but very slowly; it yielded by analysis an atom of chlorine and one of cyanogen, which is the same composition as that stated by M. Gay Lussac.—*Ibid, Sept.* 1827.

EXAMINATION OF CINCHALONA.

M. Tilloy of Dijon states that in 1825 about 500 pounds of *Kina Kalissaya* were imported into France: it was fine in appearance, very distinctly bitter, and by analysis yielded the same proportions of colouring and fatty matter as in good bark of the same kind; but it yielded no quina. M. Tilloy gives the following as a ready process for determining whether bark contains quina. Take one ounce of bark reduced to coarse powder, and put it into twelve ounces of alcohol of sp. gr. 867, and heat to about 110° to 120° for an hour; pour off the alcohol, repeat the digestion with fresh alcohol, mix the liquors, and add acetate or subacetate of lead sufficient to precipitate the colour of the kinic acid; set it by, filter, add to the liquid a few drops of sulphuric acid to separate the lead of any acetate which may have been used in excess; filter and distil: there remains sulphate or acetate of quina. according to the quantity of sulphuric acid employed, mixed with a fatty matter, which adheres to the vessel, pour off the solution and add ammonia to it, which immediately precipitates the quina; but if it be added in excess, it re-dissolves the quina, but a little sulphuric acid will again precipitate it. The quina washed with warm water, treated with sulphuric acid and a little animal charcoal, gives very white sulphate of quina; in six hours M. Tilloy obtained nine grains of sulphate of quina from an ounce of bark.—*Ibid. Oct.* 1827.

COMMON SUGAR EXISTING IN THE FORM OF GRAINS IN THE FLOWERS OF RHODODENDRON PONTICUM.

M. Jaeger discovered, in April 1825, on a plant of *Rhododendron ponticum*, which he kept in his room, and which was covered with flowers, grains of common sugar, pure and of a white colour, on the inner surface of the upper division of the corolla. The quantity of grains collected from about 140 flowers amounted to 275 centigrammes. The mean weight of each grain was two centigrammes. The physical and chemical properties of these grains approach so much to those of common sugar, that no essential difference could be detected between the two substances.

LIST

LIST OF NEW PATENTS.

To James Smethurst, of New Bond-street, for an improvement upon lamps.—Dated the 6th of November, 1827.—2 months allowed to enrol specification.

To Frederick Foveaux Weiss, of the Strand, surgeon's instrument-maker, for improvements in the construction of spurs.—6th of November.—2 months.

To James White, of Paradise-street, Lambeth, engineer, a machine or apparatus for filtering, which he denominates an artificial spring.—8th of November.—2 months.

To John Platt, of Salford, near Manchester, fustian-dresser, for certain improvements in machinery for combing wool, and other fibrous materials: communicated from abroad.—10th of November. —6 months.

To William Collier, of Salford, fustian-shearer, for certain improvements in the power-loom for weaving: communicated from abroad.—10th of November.—6 months.

To John Walker, of Weymouth-street, Mary-le-bone, esquire, for an improved caster for furniture.—17th of November.—2 mon.

To Henry Pinkus, of Philadelphia, for an improved method of purifying carburetted hydrogen gas for the purpose of illumination. —17th of November.—6 months.

To Samuel Sevill, of Brownshill in the parish of Bisley, Gloucestershire, clothier, for his improvements applicable to raising the pile, and dressing woollen and other cloths.—20th of November.—6 months.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·36 Oct. 4. Wind N.—Min. 29·07. Oct. 22. Wind S.E.
Range of the mercury 1·29.
Mean barometrical pressure for the month . . . . 29·731
——— for the lunar period ending the 20th instant . . . . . 29·732
——— for 14 days with the moon in North declination . . . 29·738
——— for 15 days with the moon in South declination . . . . 29·725
Spaces described by the rising and falling of the mercury . . . 7·090
Greatest variation in 24 hours 0·580.—Number of changes 15.
Therm. Max. 68° Oct. 2. Wind S.E.—Min. 35° Oct. 31. Wind N.
Range 33°.—Mean temp. of exter. air 55°·56. For 30 days with ☉ in ♎ 57·47
Max. var. in 24 hours 21°·00—Mean temp. of spring water at 8 A.M. 55°·71

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 2d . . . . 100°
Greatest dryness of the air in the afternoon of the 6th 49
Range of the index 51
Mean at 2 P.M. 66°·2—Mean at 8 A.M. 75°·8—Mean at 8 P.M. 81·0
—— of three observations each day at 8, 2, and 8 o'clock . . 74·3
Evaporation for the month 1·35 inch.
Rain near ground 4·835 inch.—Rain 23 feet high 4·460 inch.
Prevailing Wind S.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 10½; an overcast sky without rain, 10; foggy 2; rain, 5½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
22	12	31	1	22	19	18

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	4½	5½	5	4	6	2	2	31

General Observations.—The first part of this month to the 8th was fine and dry, and the latter part humid, wet, and windy. On the 8th, 9th, 10th, and 28th, nearly 3½ inches of rain fell here, when chronic rheumatism and colds were prevalent complaints in this town and neighbourhood. A northerly wind on the 13th lessened the temperature of the atmosphere considerably, and made it a winter-like day; but a South-west wind with rain on the following day increased it again, and it kept up warm till the 27th, afterwards it was cold to the close of the month, with hoar-frost in the mornings.

On the evening of the 17th a faint appearance of an aurora borealis was observed here; and one at Manchester on the evening of the 6th in the moon-light.

It is remarkable that in the morning of the 18th, linear, ramified, and converging plumose *cirri* appeared *beneath* beds of *cirrocumulus* in round flocks; and these modifications were beautifully tinged with prismatic colours, both within and two or three degrees without a solar halo, whose horizontal radii were respectively 22¾ degrees. In the afternoon of the 19th, a beautiful rainbow appeared from a quarter before five till five o'clock. The diameter of the interior bow to the outside of the colours measured 84° 5′, and of the exterior, 101° 10′, and their distance from each other was 8° 17′. The colours of the interior bow downward were red, yellow, green, and faint blue, beneath which four narrow parallel bows appeared of a deep red, with collateral modifications of colours, which bows were formed by successive reflections from the interior bow. The breadth of the exterior bow was nearly two degrees, notwithstanding the *nimbus*, in which it appeared was only of a turbid hue, and its colours were reversed from the order of those in the exterior, which showed that the former was formed by reflection from the latter. The sun was in the horizon, and within a few minutes of setting when the bows disappeared.

The mercury in the barometer on the 25th and 28th slowly rose whilst it rained. This is an anomaly in meteorology in this latitude, as it seldom happens. On the 25th, its rising was occasioned by a simultaneous dry wind setting in above the rain from the N.W., as discovered by the motion of *cirrus* from that point nearly when the *nimbus* began to disperse, thereby closely uniting the particles of the air, so as to increase the weight of the atmospheric column in our zenith; and on the 28th its rising was also occasioned by a wind from the N.E. blowing over a light current from the S.W. till it succeeded it at noon.

The mean temperature of the external air this month is only 2½ degrees higher than the mean of October for the last eleven years.

The temperature of spring water did not arrive at its maximum till the 26th, consequently the earth had but just attained its greatest heat for this year; and it decreased half a degree by the end of the month.

The

The following are the days on which the migrations of the swallows to and from this neighbourhood have been successively noted for the last twelve years,—a circumstance which serves to point out their wonderful instinct, in regard to the changes that take place in the temperature of the atmosphere soon after the vernal and autumnal equinoxes.

ARRIVAL.		DEPARTURE.		Weeks.	Days.
1816 April 23rd.	....	Not observed.			
1817 —— 21st.	....	————			
1818 —— 25th.	....	September 29th.	diff. ...	22	3
1819 —— 22nd.	....	October 1st. ...	diff. ...	23	1
1820 —— 26th.	....	——— 13th.	diff. ...	24	2
1821 —— 15th.	....	——— 8th. ...	diff. ...	25	1
1822 —— 27th.	....	September 28th.	diff. ...	22	0
1823 —— 23rd.	....	Not observed.			
1824 —— 18th.	....	October 18th.	diff. ...	26	1
1825 —— 13th.	....	——— 11th.	diff. ...	25	6
1826 —— 18th.	....	September 27th.	diff. ...	23	1
1827 —— 13th.	....	October 3rd.	diff. ...	24	4

The greatest deviation in the time of their arrival is a *fortnight*, and in the time of their departure *four weeks*, which may be considered as regular, and in some instances more uniform than the springs and falls of those years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one parhelion, three solar halos, one meteor, one aurora borealis, ten rainbows, lightning and thunder on the 10th and 11th, and seven gales of wind, namely, one from N., one from E., one from S.E., one from S., and three from S.W.

REMARKS.

London.—Oct. 1. Cloudy with rain. 2, 3. Cloudy. 4. Cloudy and fine. 5—7. Fine. 8. Fine: cloudy: rain in the night. 9, 10. Rainy. 11. A thunder-storm about 1 in the morning; afternoon showery. 12. Cloudy. 13—15. Fine. 16. Cloudy. 17, 18. Fine. 19. Rainy morning. 20. Cloudy. 21. Fine: cloudy. 22. Rainy. 23. Cloudy and fine. 24—26. Cloudy. 27. Fine day: rainy night. 28. Rainy. 29. Fine. 30. Cloudy. 31. Fine.

Penzance.—Oct. 1. Rain. 2. Fair. 3. Fair: misty rain. 4. Misty rain. 5, 6. Clear. 7. Clear: rain. 8—10. Rain. 11. Showers. 12, 13. Clear. 14—18. Misty rain. 19, 20. Showers. 21, 22. Rain. 23. Showers. 24, 25. Rain. 26. Fair. 27, 28. Rain. 29, 30. Clear. 31. Fair.—Rain-gauge ground level.

Boston.—Oct. 1. Cloudy. 2. Misty. 3. Cloudy. 4—6. Fine. 7. Foggy. 8. Cloudy: rain at night. 9. Cloudy: rain, A.M. and P.M. 10. Cloudy: stormy night, with rain. 11. Cloudy: rain A.M. and rain at night. 12. Cloudy. 13, 14. Fine. 15, 16. Cloudy. 17—19. Fine. 20. Rain: heavy rain, A.M. 21. Cloudy. 22. Cloudy: rain A.M. 23. Cloudy: shower of rain, with thunder and lightning, P.M. 24. Foggy. 25. Fine: rain P.M. 26. Fine. 27. Cloudy: rain P.M. 28. Rain. 29—31. Cloudy.

RESULTS.

Winds, N. 1: NE. 3: E. 2: SE. 11: S. 3: SW. 3: W. 1: NW. 7.

London.—Barometer: Mean height for the month .... 29·927 inch.

Thermometer: Mean height for the month .... 53·435°

Evaporation .... 1·47 inch.

Rain .... 4·49.

Meteoro-

Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.										
Oct. 1	30·06	29·94	29·56	29·54	29·80	29·76	29·22	68	51	58	52	67	55	59·5	SE.	SE.	E.	E.	...	...	·08	...	...	...
2	30·37	30·06	29·80	29·68	30·03	29·86	29·35	67	46	61	53	68	52	58	SE.	SE.	NE.	calm	...	...	...	0·370	...	...
3	30·51	30·37	30·14	30·10	30·30	30·20	29·65	64	44	60	54	63	54	57·5	SE.	SE.	S.	W.	...	0·10	...	...	...	...
4	30·52	30·51	30·22	30·22	30·36	30·35	29·81	67	44	60	54	64	51	58	NW.	SE.	NE.	calm	...	...	...	...	...	...
○ 5	30·52	30·32	30·20	30·14	30·34	30·28	29·83	70	42	62	54	64	51	58	NE.	SE.	NE.	E.	...	...	...	...	...	...
6	30·32	30·15	30·06	30·00	30·15	30·05	29·70	67	37	63	53	63	50	54	NE.	SE.	NE.	calm	...	·20	...	...	...	...
7	30·15	29·85	29·86	29·80	29·95	29·84	29·45	64	39	60	52	63	55	48·5	SE.	SW.	SE.	calm	...	...	...	...	...	...
8	29·85	29·48	29·44	29·30	29·65	29·38	29·15	65	49	60	54	63	56	54	S.	SW.	S.	SE.	...	...	·27	1·630	0·490	...
9	29·55	29·48	29·22	29·10	29·31	29·26	28·80	63	49	60	54	64	51	56	S.	NW.	SW.	S.	...	·25	·46	...	1·010	·03
10	29·55	29·39	29·26	29·22	29·34	29·08	28·85	58	47	55	50	57	49	52·5	SE.	N.	E.	W.	...	...	·93	0·450	·960	·20
11	29·55	29·39	29·30	29·22	29·28	29·22	28·70	57	46	52	45	54	48	50	S.	NW.	SW.	W.	...	...	·15	...	·280	·70
12	29·67	29·55	29·50	29·48	29·54	29·41	28·82	59	32	52	48	59	43	52	W.	NW.	W.	W.	...	·10	...	...	...	·17
☾ 13	29·92	29·67	29·56	29·50	29·53	29·44	29·05	65	46	54	46	53	44	46	NW.	NW.	N.	W.	...	...	...	...	...	...
14	30·09	29·92	29·70	29·64	29·83	29·74	29·30	56	48	55	45	57	53	48·5	NW.	W.	NW.	calm	...	...	...	0·300	·130	...
15	30·10	30·09	29·76	29·76	29·95	29·92	29·42	64	56	58	50	62	57	52	SW.	W.	SW.	W.	...	·15	...	...	·040	...
16	30·10	30·03	29·76	29·74	29·95	29·90	29·35	63	48	60	55	63	52	61	SW.	SW.	SW.	S.	...	...	...	0·500	...	...
17	30·03	29·93	29·72	29·70	29·84	29·78	29·35	62	47	58	55	59	52	56	SE.	S.	SE.	NW.	·97	...	...	...	...	...
18	29·93	29·93	29·64	29·60	29·70	29·70	29·32	65	48	58	54	63	54	52	E.	SE.	E.	E.	...	·15	...	0·200	...	...
19	29·93	29·90	29·60	29·60	29·75	29·75	29·25	63	46	59	54	63	53	58	E.	SE.	E.	E.	...	...	·84	...	·020	...
● 20	29·90	29·79	29·60	29·58	29·70	29·68	29·22	63	44	59	53	64	50	58·5	NW.	S.	SE.	calm	...	...	·02	...	...	·70
21	29·79	29·51	29·56	29·22	29·58	29·46	29·07	59	50	56	52	62	54	55	SE.	SE.	SE.	E.	...	·10	·07	...	·110	·06
22	29·51	29·34	29·10	28·78	29·26	29·07	28·93	63	52	56	54	61	54	55	SE.	SW.	SE.	E.	...	...	·51	...	·440	...
23	29·81	29·34	29·40	28·90	29·38	29·11	28·66	62	44	55	50	63	49	56	SW.	W.	S.	S.	...	...	·04	0·435	·020	·09
24	30·21	29·81	29·66	29·60	29·91	29·60	29·15	58	48	58	49	60	54	49	NW.	W.	W.	calm	...	·10	·03	...	·140	·05
25	30·26	30·21	29·80	29·70	30·06	30·00	29·60	60	51	61	54	60	54	50·5	SE.	SW.	S.	calm	...	...	·08	0·545	·105	...
26	30·21	29·90	29·80	29·76	30·06	29·90	29·58	62	48	58	55	62	55	56·5	SE.	SW.	SE.	W.	...	...	...	...	...	·25
☽ 27	29·90	29·56	29·54	29·44	29·70	29·52	29·25	62	52	57	53	64	56	52·5	SE.	S.	SE.	calm	...	·05	·30	...	·050	...
28	30·09	29·56	29·40	29·22	29·56	29·31	29·0	48	32	56	53	59	41	48	NE.	S.	NE.	N.	...	...	·69	0·775	1·000	·30
29	30·15	30·09	29·90	29·70	30·00	29·80	29·50	63	34	52	44	50	37	42	N.	E.	NE.	N.	...	...	...	0·900	...	·04
30	30·09	29·90	29·92	29·82	29·95	29·78	29·50	50	49	53	42	51	45	46	NW.	NW.	NW.	calm	...	...	...	...	·040	...
31	29·99	29·90	29·80	29·76	29·77	29·62	29·31	53	34	54	48	56	35	47·5	NW.	N.	NW.	NW.	·44	·15	·02	...	...	...
Aver. :	30·52	29·34	30·22	28·78	30·36	29·07	29·26	70	32	63	42	68	35	53·1					1·47	1·35	4·49	6·105	4·835	2·59

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END OF THE SECOND VOLUME.

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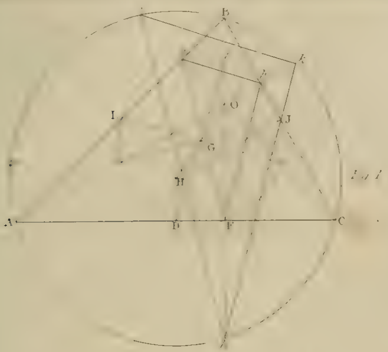
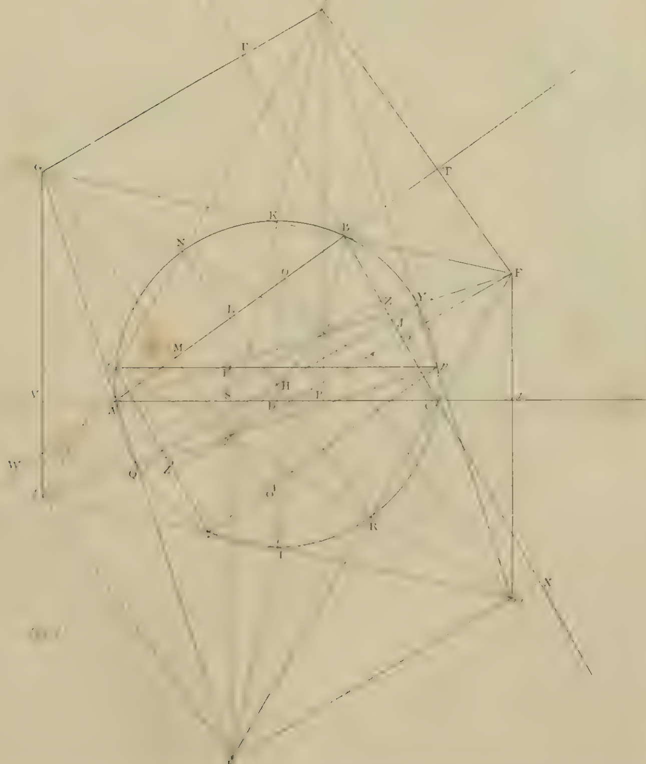
1827.





Illustrations of Natural Embankments, on the Norfolk and Suffolk Coasts.

Fig. 1

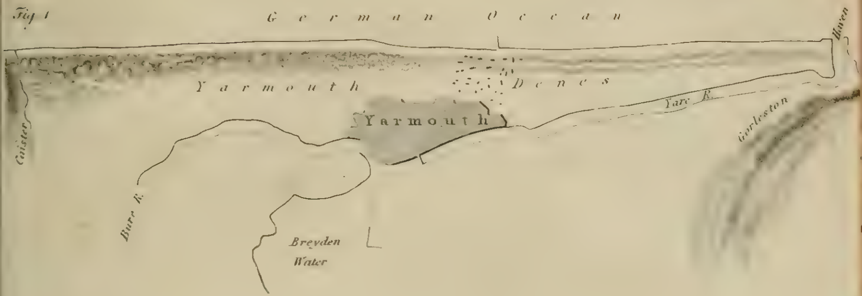


Fig. 2

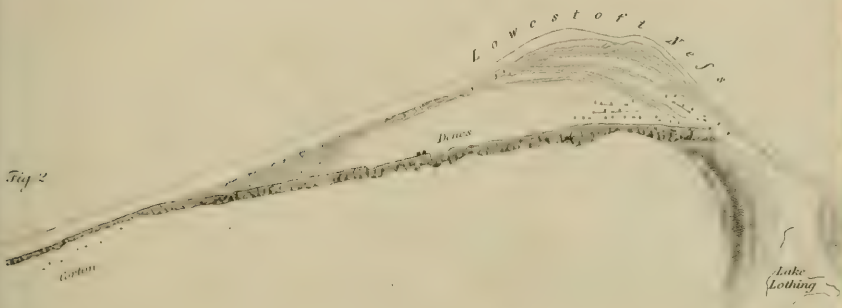


Fig. 3

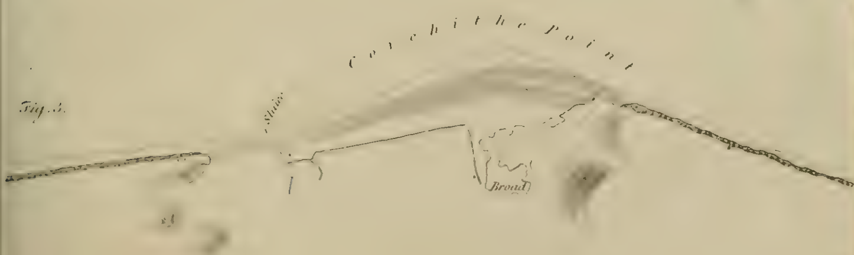
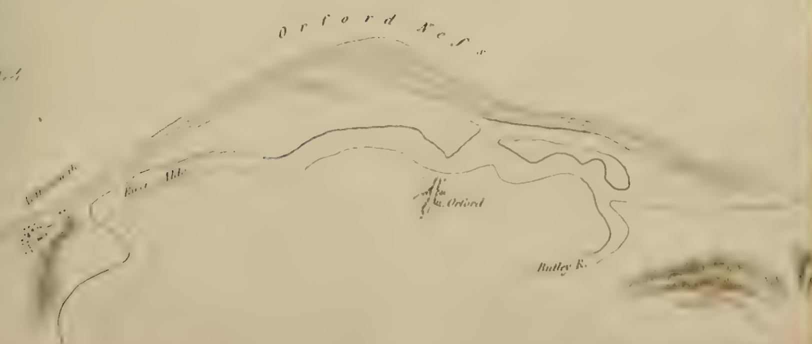
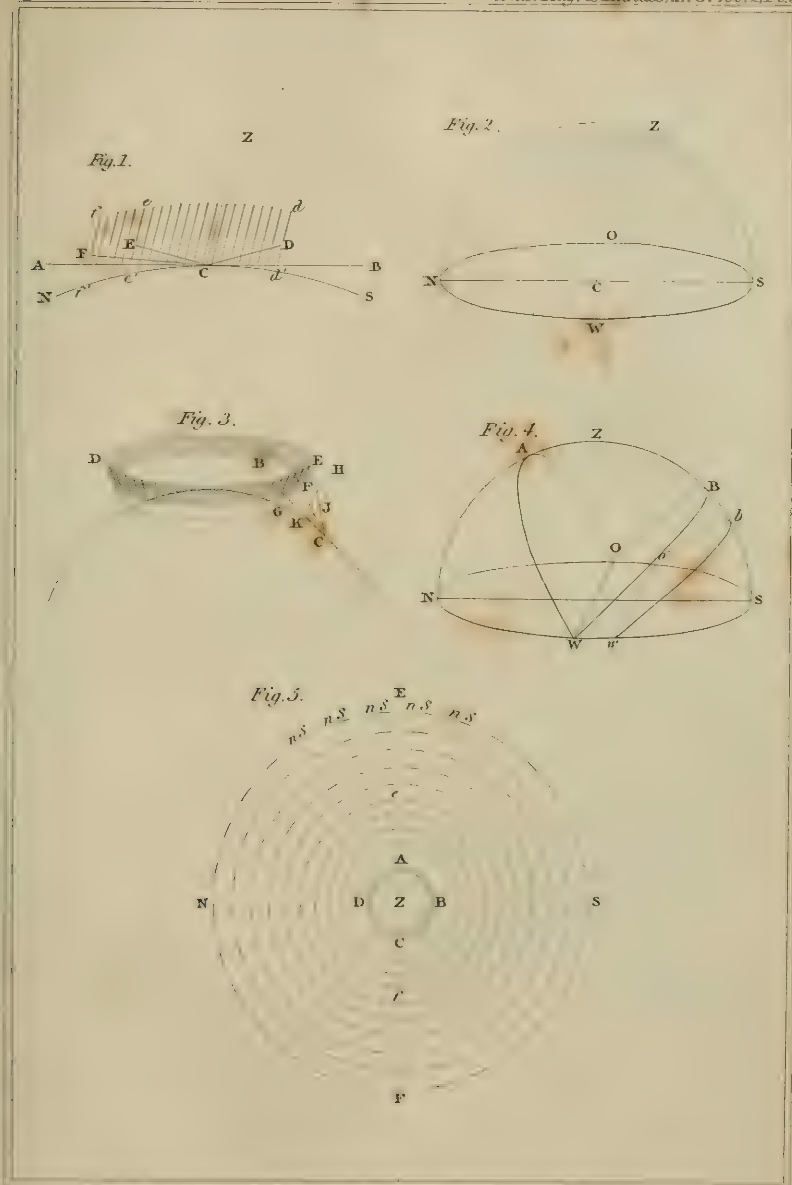


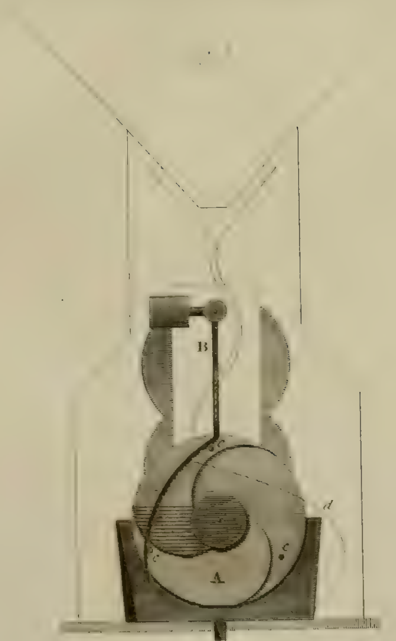
Fig. 4

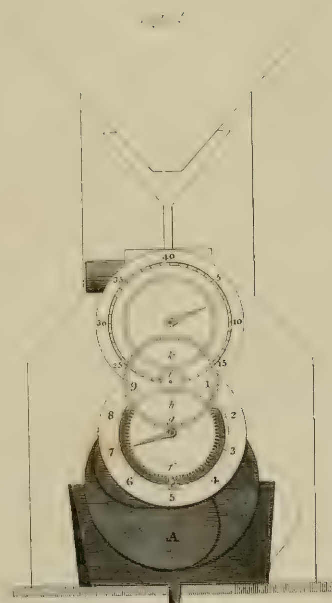


R.C. Taylor



Hansteen's illustrations of the forms of the Polar Lights.





S. Porter sculp.

Mr. J. Taylor's Rain Gauge.

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