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THE EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

BRITISH MUSEUM NATURAL HISTORY BRANCH

CONDUCTED BY

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Erratum.—In our last Number the first wood-cut at p. 238 was reversed.

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ERRATA.

Page 354, line 16, *for* p. 10, *read* p. 350.
Page 356, line 7, *for* p. 10, &c., *read* p. 351, &c.

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On the Temperature of the Frith of Forth, and on the Specific Gravity of its Water. By JOHN DAVY, M.D., F.R.S., Lon. and Ed., Inspector-General of Army Hospitals, L.R. (Communicated by the Author.)

WHEN my attention was first directed to the subject ot inquiry announced in the heading of this notice, it was my design to have carried on from month to month a regular series of observations on the temperature of the sea of the Frith of Forth, and on the specific gravity of its water, with the expectation that the results collected for two or three years might be of some value, and especially in connexion with the climate of the coast of Scotland and its islands.

Interrupted in this design by leaving Edinburgh, the observations I have now to offer are few in number, reaching only from September 1842 to last April; few, however, as they are, I would hope that they may not be altogether useless, and sure I am, they will not be so, should they lead to more extended inquiry.

The temperature of the sea, as given in the following table, was ascertained by immersing a thermometer, the scale of which had been carefully corrected, in the water at the head of Leith Pier, or, as on two occasions marked by an asterisk, when the sea was too rough to make the observations there, at the stairs close to the Lighthouse; and the water, the specific gravity of which is given, was taken up at the same time and places.

Table shewing the Temperature of the Air and Sea, and the Specific Gravity of Sea Water, at Leith, from September 1842 *to April* 1843.

		Air.	Water.	Spec. Grav.	Tide.	Wind.
1842.						
September 30.	3 P.M.	55°	54°	1.0258	Low.	N.E.
October 29.	2 P.M.	43	45	1.0251	Half ebb.	N.
November 30.	3 P.M.	45	42	1.0256	Do.	W.
1843.						
January 5.	3 P.M.	39	42	1.0248	Two-third flow.	N.
February 5.*	3½ P.M.	35	38	1.0197	Half flow.	N.
Do. 19.*	3½ P.M.	39	38	1.0242	Do.	S.E.
March 4.	2 P.M.	39	38	1.0205	Do.	N.W.
April 2.	2 P.M.	55	44	1.0248	High.	W.

It may be useful to insert another table, for which I am indebted to Mr Adie junior, shewing the mean monthly temperature of the air, and the quantity of rain that fell in 1842, and in the spring months of the following year, in the neighbourhood of Edinburgh, viz. at Canaan Cottage, which is 246 feet above the mean height of the sea.

TABLE.

	Mean of Thermometer.	Rain.
1842.		Inch.
January,....................	35°.451	1.01
February,....................	39 .553	1.11
March,....................	42 .048	2.44
April,....................	45 .033	0.15
May,....................	51 .227	1.45
June,....................	57 .533	0.97
July,....................	56 .742	1.53
August,....................	59 .145	1.36
September,....................	53 .580	1.45
October,....................	44 .010	0.98
November,....................	39 .830	1.63
December,....................	45 .350	1.79
1843.		
January,....................	38 .194	1.69
February,....................	32 .732	1.38
March,....................	40 .038	0.99

Comparing the mean monthly temperature of the air, as given in this table, with the temperature of the sea for the months in which it was determined, it appears, as might be expected, that there is in the latter a greater uniformity than is observable in the former.

The summer of 1842 was one of unusually high temperature, as was also the autumn, followed by a winter of ordinary coldness, with the exception of the month of December; on which account, perhaps, the sea may have been somewhat warmer this year than usual. And owing to the unusual dryness of the whole year, the specific gravity of the sea-water may have been a little higher than ordinary. The influence of rain in lowering the specific gravity is indicated in the results obtained in January, February, March, and April. The very low specific gravity of the water on the 5th of February was probably occasioned by an unusual influx of fresh water into that part of the harbour from which the portion tried was taken, brought down by the river (the Water of Leith) that enters the sea near the port. In confirmation of this, it may be remarked, that, on the 2d of April, after some heavy showers, when water taken up at the pier-head was of the specific gravity 1.0248, another portion collected at the Light-house stairs, on the harbour side, was so low as 1.0062, seeming to indicate that the fresh water there might even have been floating on the salt.

The small range of temperature of the sea and air incumbent, and the general uniformity of the specific gravity of the sea water, as shewn in the first table, putting aside the exception just referred to, is each well marked and remarkable. The equability of temperature of the air, no doubt, mainly depended on that of the water, and this probably not so much on the depth and extent of the Firth itself, as on the circumstance of the tides to which it is subject, connecting, as it were, this narrow sea with the ocean.

A Greek of Corfu, Nicander Nucius, who visited Britain in the reign of Charles V., happily describes our island as "laved continuously by the ebb and flow of the ocean." To him, accustomed to his own constant lake-like sea, this perpetual flux and reflux, alternately inundating and leaving dry a great extent of coast, may well have appeared surprising and peculiar; whilst to us, familiar with the appearance, it excites no such feeling, has a common-place character, and is apt to be overlooked as to its consequences.

That the comparative mildness of the climate of Great Britain, so different from what might be expected from its latitude, and especially of its northern shores, is chiefly owing to the peculiar manner in which they are "laved continuously by the ebb and flow of the ocean," can hardly be doubted, keeping in mind, that the sea which washes them is shallow rather than deep. Compare our ports always open, even the most northern, in the severest winters, with those of certain tideless seas, as the Baltic and the northern shallow portion of the Black Sea annually closed by ice, and yet a large portion of the former is not farther north than the Frith of Forth, on the shores of which the arbutus flourishes; and the other is not less than four degrees farther south, than the most southern part of England, where the myrtle thrives in the open air, and its berries ripen. The flux and reflux of the sea may be considered as compensating for want of depth of water on our coasts; and the tide having its source in the ocean, may be likened to streams exempt from freezing, from the circumstance of their flowing out of deep lakes, reservoirs of warmth, such as many of those of the Highlands of Scotland, and which in themselves have a marked mitigating effect on the climate of their banks.

THE OAKS, AMBLESIDE, *Nov.* 4. 1813.

Contributions towards establishing the General Character of the Fossil Plants of the genus Sigillaria. By WILLIAM KING, Esq., Curator of the Museum of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne. With a Plate. (Communicated by the Author.)

None of the vegetable forms constituting the flora of the carboniferous epoch have excited more attention as it regards their general character, than those which are included in the genus *Sigillaria*.*

* The following are the synonyms of this fossil:—*Phytolithus Dawsoni*, *P. tes-*

They are extremely abundant in the great coal-formations of the British Islands, and are found in similar, and probably contemporaneous deposits on the Continent, and in North America.

In some instances they appear to have attained a considerable size: M. Adolphe Brongniart mentions a specimen discovered in one of the coal-mines of Kunzwerk, near Essen, in Westphalia, measuring forty feet in length, while others do not appear to have exceeded ten feet in an advanced stage of their growth.

The genus *Sigillaria* may be readily distinguished by the stems of its various species being externally marked with a number of longitudinal furrows, and by each of the raised spaces or ribs between the furrows having a single row of scars, in some cases nearly in contact, and in others considerably apart.

Various opinions have been entertained respecting the situation of *Sigillaria* in the vegetable kingdom. Schlotheim supposed it to be allied to the palms; Von Martius to the Cactuses and the Euphorbias; Brongniart to the Tree Ferns; while Lindley and Hutton, as stated in the "Fossil Flora," appear inclined to agree with Von Martius, but do not give any decided opinion on the matter. A few years have now elapsed since these opinions were advanced. The late discovery, however, of a silicified specimen of this plant, exhibiting its constituent tissues in a nearly complete state, has induced Brongniart, the only author who has since written on the subject, entirely to change his opinion. The importance of this discovery will, it is hoped, be deemed a sufficient excuse for the following abstract of the Memoir which that celebrated botanist has published on this specimen.*

In the neighbourhood of Autun in France, occur the remains of various vegetable fossils, such as *Psaronius, Medullosa, Colpoxylon,* and *Coniferae,* beautifully silicified, and displaying the nature of their respective tissues. In addition to these, a small fragment of another fossil in the same condition has been found and presented to the Museum of Paris; it is externally marked with certain characters which identify it with

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sellatus, and *P. notatus* of Steinhauer (1818); *Palmacites* of Schlotheim (1820); *Alveolaria* (1820, as a division of *Lepidodendron,* and in 1826 changed to *Favularia*); *Syringodendron* (1820), and *Rhytidolepis* (1822) of Sternberg; *Cactites* and *Euphorbites* of Von Martius (1821); *Sigillaria* and *Clathraria* (?) of Brongniart (1822). The generic name *Syringodendron,* established by Sternberg, from decorticated and demicorticated (?) specimens, is undoubtedly the one which, in justice to its author, ought to be given to this fossil; but from the general currency of Brongniart's name *Sigillaria,* there appears little chance of the latter superseding the former.

* "Observations sur la structure interieure du *Sigillaria elegans,* comparée à celle de *Lepidodendron,* et de *Stigmaria,* et à celle des vegetaux vivants."—*Archives du Museum d'Histoire Naturelle,* tom. i.

Sigillaria elegans,—a species common in the coal-mines of Eschweiler, near Aix-la-Chapelle, and of Werden, near Dusseldorf.

By making various microscopic sections of this specimen, M. Brongniart has been enabled to study nearly the whole of its internal structure. A complete transverse section shews it to consist of the following parts, proceeding from the periphery to the centre.* First, a broad external zone, *a*, *a*·, *a*··, answering to the bark; second, a narrow radiated cylinder *b*, supposed to constitute the ligneous system;† third, a broken but regular circle of bundles *c*, considered to represent the medullary sheath; and, fourth, a large column *d* in the situation of the pith. In addition, there are on the outside of the ligneous cylinder, in some places in contact with it, and in others at a little distance, several apparently isolated bundles *c*·, which, it is supposed, were connected with the medullary sheath, and continued into the leaves or other external appendages; as such, they may be termed leaf-cords. Of these parts, the tissue of the pith and the bark is, for the most part, destroyed, and replaced by amorphous mineral matter; where it is preserved, which is the case in a few places, there are seen traces of *parenchyma* and *prosenchyma*: the tissue of the ligneous cylinder, of the medullary sheath, and of the leaf-cords, on the contrary, is distinctly preserved. We are, consequently, furnished with the most essential character of the internal structure of this plant.‡

In order to establish the true relations of *Sigillaria elegans*, it will be necessary to examine in detail, and under a high magnifying power, the character and arrangement of its cellular, woody, and vascular tissues. On a transverse section, the broken but regular circle of bundles supposed to represent the medullary sheath, or vascular system, gives to

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* *Vide* the diagram (fig. 1, Plate I.), which is copied from a drawing by Brongniart, three times the natural size of the specimen.

† Brongniart, when speaking of this cylinder, appears to be undecided as to whether it should be called ligneous or vascular. For reasons which will be more fully noticed hereafter, I intend in this paper to apply the term *ligneous* to it, and that of vascular to the broken circle of bundles lying within it. Although the tissues composing the ligneous cylinder and the vascular system, or medullary sheath of *Sigillaria elegans*, differ in many respects from the woody and the vascular tissues of ordinary Dicotyledons, yet the relative situation of these parts is eminently in favour of the proposed nomenclature.

‡ The figure of Artis' *Rhytidolepis fibrosa* (Plate IX.), which is a true *Sigillaria*, shews by the two concentric rings (which are probably the inner and outer margins of the ligneous cylinder and medullary sheath conjoined), that the ligneous system of this genus does not increase in proportion to the size acquired by the stem. Perhaps the two concentric rings of this plant represent two ligneous cylinders, placed one within the other, as in *Encephalartos spiralis*, and other *Cycadaceæ* (a supposition of which I am certainly not much in favour); if so this character would go far to prove that Artis' fossil is generically distinct from Brongniart's *Sigillaria elegans*.

the inner part of the radiated cylinder a festooned appearance, in consequence of each bundle forming, as it were, the segment of a circle, having its convexity towards the supposed pith. The vessels composing these bundles are irregularly angular, and arranged without any order, with the exception that the widest, which are also the longest, immediately adjoin the pith, while the narrowest and shortest are on the opposite side: the extremities of both kinds terminate in obtuse cones. By means of longitudinal sections, it is ascertained that the largest vessels have their walls marked with both transverse and oblique lines or bars, united together at the angles; in some parts of the vessels, the bars appear to cross each other, which gives them a reticulated appearance. Brongniart assimilates these large vessels with some that are common in Ferns and Lycopods. The smallest (between which and the latter a regular gradation may be traced), possess true continuous spiral fibres, coiled parallel to each other, and without any appearance of reticulation. Whether these are true spiral vessels, is a question over which there appears to hang some doubt. From their shortness, and the probable existence of an appreciable membrane between the fibres, Brongniart says, that one might be disposed to conclude that they belonged to that form of vascular tissue known under the name of ducts, or false spiral vessels, and that they are intermediate to true spiral vessels, and the scalariform tissue of Ferns and Lycopods.

These vessels are found in the same position as the true spiral vessels of dicotyledonous plants, but in the latter they do not constitute bundles so large, nor so well defined, as in *Sigillaria elegans*. In the Dicotyledons, each bundle of spiral vessels corresponds directly with the inner side of a ligneous bundle included between two medullary rays: often the vessels are very small: when they are numerous, the smallest are placed on the side adjoining the pith, or in the centre, and the largest on that side next to the radiated or ligneous cylinder. In *Sigillaria elegans*, however, each bundle of the medullary sheath embraces several narrow ligneous bundles, intersected by a number of medullary rays, and the arrangement of the vessels is the very reverse of that just stated.

Thus, although these bundles have some resemblance, by their position, to those of the medullary sheath of ordinary Dicotyledons, they possess a character which distinguishes them from those of the latter plants, and which as yet has not been met with in any vegetable form now living.

The ligneous cylinder, unlike the vascular circle, or medullary sheath, is divided by narrow radiating spaces, possessing no remains of tissue, but which radiating spaces, in all probability, originally constituted the medullary rays. The vessels, or more properly speaking, tubular cells composing this cylinder, are uniform as regards the markings on their walls, and are rather variable in shape. A transverse section of a single bundle intervening two medullary rays, often exhibits the orifices of the vessels of a round, oval, or hexagonal form, clearly defined.

These vessels also vary in diameter; they increase gradually from the inner to the outer side of the cylinder. By means of longitudinal sections, made both parallel and at right angles to the medullary rays, the vessels are seen to be regularly marked with parallel transverse bars on all their walls; this is an important character, for, in the Conifers and Cycases, the cells or vessels composing the ligneous system are in general *only* marked on their lateral walls, that is, on those which correspond with the direction of the medullary rays. Another difference remains to be mentioned; in the latter plants the vessels are generally marked with what are termed disks, but in *Sigillaria elegans*, the vessels, as already stated, are marked with parallel transverse bars.

The bundles or cords which Brongniart considers were connected with the medullary sheath, and continued into the leaves or other external appendages, are, as previously mentioned, situated externally to the ligneous cylinder, and consequently they must have been imbedded in the surrounding zone of cellular tissue. On a transverse section, these cords are in some cases nearly cylindrical, and in others rather oblong, and the vessels composing them have in general irregular angular orifices; on longitudinal sections, we discover that the vessels are formed of a fine membrane, and that they have their walls transversely barred. Like the vessels of the medullary sheath, they are irregularly grouped, and not arranged in radiating series, in the manner of those forming the ligneous cylinder.

The external zone or bark possesses nothing very remarkable, except its varying width, and this appears to be due to the ligneous and vascular parts being eccentric, as in *Lepidodendron* and *Stigmaria*, and not central, as in ordinary woody trees.* The inner, and by far the largest, part of the bark, *a*, appears, from the few traces remaining, to have consisted of a delicate *parenchyma*. The cuticle, or external part, which is extremely well preserved in one place, is formed of a stronger tissue, and consists of two different layers, *a·* *a··*, in close contact. The cells of

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* Brongniart says, that the Lycopods agree with these fossils in the occasional eccentricity of their vascular system.

I have considerable doubt as to this eccentricity being a character of *Lepidodendron*, *Stigmaria*, and *Sigillaria*, when living. It is due, I strongly suspect, to the destruction of the mass of delicate cellular tissue in which the axis was imbedded. Steinhauer was the first, I believe, to advance this opinion. He very justly observes, that the destruction of the surrounding tissue would leave the axis unsupported, and that the latter would necessarily sink to the under side of the fossil. The creeping habit of the branches of *Stigmaria* is extremely favourable to such a position of their axis resulting from this contingency. With reference to *Lepidodendron*, I have seen specimens in which the vascular cylinder is nearly central. And as regards *Sigillaria*, Artis' figure of *Rhytidolepis fibrosa* (Pl. ix.), demonstrates that its ligneo-vascular axis was not eccentric.

the inner layer are elongated in the longitudinal direction of the stem, and arranged parallel to each other; they have all the appearance of that kind of tissue called *Prosenchyma.* The cells of the outer layer are not in parallel lines, but in some cases are very regularly arranged, while in others they are less so; they constitute ordinary cellular tissue or *parenchyma.* The superficial cells differ from the latter in having a thicker membrane.

I shall now briefly advert to some points of difference and of resemblance which Brongniart mentions as existing between *Sigillaria elegans,* and some other plants, both recent and fossil. Among existing Exogens, the ligneous cylinder is variously constituted. In the Angiospermous division of this class it is formed of a mixture of woody fibre or fusiform cells, of *bothrenchyma,* and occasionally of other forms of tissue, and in the Gymnospermous division, for example the Conifers and the Cycases, it is generally formed of one kind.* Now, the ligneous cylinder of *Sigillaria elegans* is formed of an uniform tissue as in the last division. In *Stigmaria, Anabathra,*† and some other allied fossils which vegetated during the same geological epoch, the same uniformity of structure occurs. So far the recent and fossil plants resemble each other. But when a further comparison is attempted, we find that all the points of agreement have been exhausted. In most of the living Conifers and Cycases, it is well known that the ligneous vessels are marked only on those walls which correspond with the direction of the medullary rays; but in the fossils just mentioned, the whole of the walls are marked. We have here a remarkable difference, to which may be added another. In the fossils, it is the character of the vessels to be marked with transverse and reticulating bars, but in nearly all *Coniferæ,* and in a great number of *Cycadaceæ,* the vessels are characterized with what are generally called disks.

Respecting the first difference, botanists are acquainted with a few

* It would appear, however, from the observations of Professor D. Don, that, in *Cycadaceæ,* there is not such a general uniformity in the vessels of their ligneous system. And with respect to *Coniferæ,* Lindley mentions the Yew (*Taxus baccata*) and *Abies Douglassi* as exceptions; Goeppart, in his Memoir entitled "*De Coniferarum Structura Anatomica,*" has given some longitudinal sections of the former, exhibiting its structure.

† I defer entering into any particulars respecting these two fossils, until the close of this paper. It is necessary, however, in this place, to extract the following from Brongniart's "Observations," with reference to *Stigmaria.* "The internal bundles which form the medullary sheath of *Sigillaria elegans,* and which are so completely distinguished by the irregular and non-radial arrangement of their constituent vessels, are completely absent in *Stigmaria.* There would thus appear to be the same difference between these fossils as is observable between the *stems* or *branches* of any dicotyledonous plant in which the ligneous cylinder is accompanied, on its inner side, by the bundles of the medullary sheath, and their *roots,* which are deprived of these bundles."

plants which offer an exception to the general rule, and which, in this respect, are allied with the fossils. Thus Mohl has shewn that in *Ephedra,* which approximates to the Conifers in many respects, the ligneous vessels are discigerous on the whole of their walls.

Brongniart also points out an analogous character in *Zamia integrifolia* of South America.* The same is also to be found in plants lower in the vegetable series, as the Ferns and the Lycopods of existing creation, and in *Lepidodendron, Psaronius,* and others belonging to the carboniferous epoch. The discs on the walls of the ligneous vessels of *Coniferæ* and *Cycadaceæ* are round or oval; but in *Zamia integrifolia,* they have more resemblance to the transverse and reticulating bars of *Sigillaria elegans* in being considerably elongated. In the latter, however, they are more regular. This regularity and elongation of the bars are still more developed in the Ferns and the Lycopods.

It follows from these facts, that *Sigillaria elegans, Anabathra,* and *Stigmaria,* are allied to the Gymnospermous Dicotyledons by the uniformity of the tissue composing their ligneous and vascular systems; and to the Vascular Cryptogams by the regularity and form of the bars on the walls of the vessels of those systems.

It has been previously mentioned, that in *Sigillaria elegans* the vascular system, or medullary sheath, is formed of a broken but regular circle of bundles, and that the constituent vessels are arranged irregularly, and not in radiating series like the vessels of the ligneous cylinder. It may also be added, that these bundles are slightly separated from the ligneous cylinder on the inside of which they are placed. According to Brongniart, nothing of the kind is to be found in the allied fossils *Stigmaria* and *Anabathra,* nor does it appear that a similar character occurs in any recent plants. In some of the latter, however, we have instances of vascular bundles being widely separated from the ligneous cylinder, and dispersed irregularly throughout the pith. This certainly appears to be a modified arrangement, and as such may be so far an analogous case. Very different families display this arrangement; Mirbel has observed it in *Nyctago hortensis,* and in some umbelliferous plants; Schultz has remarked it in *Piperaceæ,* and in several *Nyctaginaceæ;* Brongniart has observed it in various *Echinocacti,* in *Echeveria grandiflora,* and in *Plantago princeps;* Decaisne has studied it in *Phytolacca dioica,* and in various *Melastomaceæ;* he is convinced, however, that it does not occur in some other plants of the same families, nor even in all the plants of the same genus. In short, it appears to be often absent in closely allied forms; thus, among the Cactuses, it exists in certain species, and

* Brongniart thinks it probable that the other American species, or the true *Zamiæ,* will be found to possess the same character; in this respect they will differ from the Australian and African *Cycas* and *Encephalartos,* which appear to have their vessels marked with discs only on those walls which coincide with the direction of the medullary rays.

not in others. A similar arrangement is to be seen in some Tree Ferns, especially in a New Zealand species of *Cyathea*, in which are observable some small vascular bundles disposed irregularly throughout the centre or pith, in addition to the larger ones constituting the nearly regular but broken circle, so characteristic of the stems of the Vascular Cryptogams.

Besides *Stigmaria* and *Anabathra*, there are other two remarkable fossils, which Brongniart compares with *Sigillaria elegans*, namely, *Lepidodendron* and *Protopteris Cotteana*, Corda (*Lepidodendron punctatum*, Sternb.). A transverse section of the stem of the first, shews that it is chiefly composed of cellular tissue, in which is imbedded an excentrically situated and continuous circle, composed of vessels, or tubular cells, transversely barred on all their walls. This circle exhibits no traces of medullary rays, nor are the vessels composing it arranged in radiating series. It would therefore appear, that the circle constituted the vascular system, and as such it may be considered analogous to the medullary sheath of *Sigillaria elegans*.

Protopteris Cotteana, which was formerly included by Brongniart in his first section of the genus *Sigillaria*, is undoubtedly a Tree Fern, and appears, from certain peculiarities of its small bundles of vessels dispersed throughout the central cellular tissue, and of the large bundles situated externally to the last, and forming a narrow sinuous cylinder, to have been a representative, in the carboniferous epoch, of the existing genus *Dicksonia*.

Having adverted to Tree Ferns, and as Brongniart formerly maintained that *Sigillaria* was related to this family, the question naturally arises as to how far these two forms resemble or differ from each other. In answer, Brongniart himself says, I admit at once that the external structure of the latter, as far as *Sigillaria elegans* has been made known to us, is essentially different from that of Ferns, and the allied families, by some very important characters; the nature of the ligneous or vascular tissue certainly establishes some analogy between them; but its arrangement is altogether different. In short, the arrangement of the tissue in radiating series, is a character foreign to all Cryptogams; it is, on the contrary, characteristic of the Dicotyledons; but it belongs, with numerous modifications, to the whole of this immense division of the vegetable kingdom from the Gymnosperms, the organization of which is comparatively simple, to those families whose structure is more complicated. It therefore appears impossible to doubt that *Sigillaria* belongs to the class of Dicotyledonous plants. But is it to the group of Gymnosperms, so abundant in a fossil state, and characteristic of the carboniferous strata, or to the Angiospermous division, which, as far as is known, appears to be absent from those ancient formations? The uniformity of the tissue, and the absence of true spiral vessels in the ligneous cylinder, are characters proper to the Gymnosperms, but are rarely found amongst the true Angiospermous Dicotyledons. All the probabilities, then, are in favour of this fossil being a Gymnosperm. It would be difficult, however, to establish this in a positive manner; for there are numerous differences between *Sigillaria elegans* and the plants at present known as belonging to the last division. For example,

1*st*, No known Conifer nor Cycas possesses the medullary sheath of *Sigillaria elegans*, as regards its form and the non-radial arrangement of its constituent vessels.

2*d*, In no Conifer is the ligneous cylinder composed of tubes or elongated cells transversely barred ; on the contrary, coniferous wood is formed of tubes characterised with disks, and *these* only on the walls parallel to the direction of the medullary rays.

3*d*, Amongst the Cycases, *Zamia integrifolia* is the only known plant possessing vessels which are barred or reticulated on all their walls ; but this character is not so regular as in the fossil under consideration.*

Thus, between *Zamia integrifolia* and *Sigillaria elegans*, there is a strong point of agreement. It requires to be mentioned, however, that the former does not possess the peculiar medullary sheath of the latter, nor are the vessels of the ligneous cylinder so regularly arranged in radiating series.

Brongniart, in the next place, refers to the Cactuses and the Euphorbias which constitute two remarkable families of the Angiospermous division of the Dicotyledons. The genera *Melocactus* and *Echinocactus* are particularly noticed. From transverse sections it appears, that between these plants and *Sigillaria elegans* there is a remarkable general resemblance, particularly in the presence of apparently isolated vascular bundles within the central column of cellular tissue or pith, as displayed in one of the species. An examination, however, of the encircling zone shews an important difference between this part and what has been called the ligneous cylinder of the fossil ; it is, in fact, formed of different tissues, namely, true spiral vessels, ducts, and a peculiar ligneous fibre. Hence Brongniart is of opinion that *Sigillaria elegans* cannot, with any propriety, be considered a Cactus. The same objection may be urged against its being considered an Euphorbia.

" Thus," to conclude this abstract in the language of Brongniart, " our researches among the Angiospermous Dicotyledons do not enable us to discover any decided analogy between these plants and *Sigillaria*. Ought we, therefore, to assert positively that no analogy does exist ? I think not, for the variety of characters in the organization of the stems of these plants ought to induce one to suppose that we shall yet find among them

* There is every reason to believe that in this respect the Jurassic Cycases approximated more to *Sigillaria elegans* than *Zamia integrifolia*. It is stated in Dr Buckland's Bridgewater Treatise, that Dr Brown has discovered in the genus *Mantellia* only scalariform vessels :—" The existence of vessels with discs peculiar to recent *Cycadeæ* and *Coniferæ*, has not yet been ascertained."—(Vol. i. p. 99, foot note ; and vol. ii. p. 100, foot note). Mr Morris of Kensington has kindly placed in my hands some sections of Cycadeous wood from the Jurassic rocks of Yorkshire, and I find the vessels are marked with transverse bars very similar to those of *Sigillaria elegans*.

a structure analogous to that of *Sigillaria elegans.* However, in the present state of our knowledge, a much more striking analogy exists as to their internal structure between the fossil and some *Cycadaceæ.*

"This analogy is particularly striking between *Stigmaria* and *Zamia integrifolia*; but, on the other hand, the difference in the external form, both as it regards the stem and the leaves, is very great; and as to *Sigillaria,* of which the leaves are unknown, and of which the stems have often, by their external characters, more resemblance to those of the Cycases, it may be objected, 1*st,* The presence of its medullary sheath formed of large bundles, of which no traces are to be found in any of the latter plants; 2*d,* The absence in the coal-formation of leaves analogous to those of existing *Cycadaceæ.*

"All these considerations lead to the conclusion, that *Sigillaria* and *Stigmaria* constitute a peculiar and extinct family belonging probably to the Gymnospermous division of the Dicotyledons, but of which neither the fruit nor the leaves are as yet known. These fossils, it is evident, are clearly related to each other, and *Anabathra* ought likewise to form a part of the same group. Perhaps *Stigmaria* is only the root of *Sigillaria.* The uniformity of the external characters observable on specimens of the first of these genera would then correspond to the trifling variety of form required in roots belonging to various species; further, the creeping habit of this fossil, and the form of its scars, agree very well with this supposition; against which, however, may be urged the regularity of the quincuncial arrangement of the appendages (or fibrils?), a regularity which has, nevertheless, been found in the roots of some aquatic plants.

"This supposition will explain the origin of the singular dome-shaped specimens which have been described by Messrs Lindley and Hutton in the "Fossil Flora," and from which branch out, in a radiating and horizontally creeping manner, the dichotomous stems of *Stigmaria.* The presence of a pith in the centre of the ligneous cylinder of the fossil is not a very serious objection, for I have ascertained that the roots of several *Zamiæ* possess a perfectly distinct and rather voluminous pith; this structure is particularly apparent in the roots of *Zamia pungens,* the tissue of which displays, though on a smaller scale, an arrangement very analogous to what is observable in *Stigmaria.*"

Before concluding with Brongniart as to the situation of *Sigillaria* in the vegetable kingdom from its internal structure, it will be necessary to examine some other characters of this genus, and also to consider some plants of the same geological age, and others which are now living; for I hope to be able to shew that considerations of this kind will materially affect the conclusion in question.

Within the last three years the collection of fossil plants in the Newcastle Museum has been enriched with two nearly complete stems of the so-called *Sigillaria reniformis* in a decorticated state. Both specimens,*

* For these specimens the Society, in whose Museum they are placed, is indebted to the liberality of the Messrs Panton, Kirk, and partners.

together with another at present in the Sunderland Athenæum, were obtained from three separate but nearly contiguous sinkings at North Biddick Colliery, on the Wear, at a depth of 140 feet below the surface. They were found in a perpendicular position, each with its base or rootstock resting on a thin layer of impure coal, not exceeding three inches in thickness. The stratum in which these specimens were imbedded consists of a greyish-coloured arenaceous shale, passing into a carbonaceous state at its junction with the subordinate coal-seam. The material of which the specimens are composed is the same as their matrix.

The most perfect stem, and the one to which the following remarks will be confined, is 7 feet 6 inches in height: its diameter is not uniform throughout; at the base it is 2 feet three inches; two feet higher, it is 1 foot 10 inches; from this part the thickness rapidly increases to about one-third of the height of the specimen, where it is 2 feet; at this point the diameter becomes suddenly reduced to 1 foot 6 inches, which thickness is preserved until within a foot of the top, where another increase takes place. The increase of diameter at the top appears to be due to the stem dividing itself into eight or nine branch-like divisions. Unfortunately these supposed branches are broken off so much below their actual departure from the main stem, as to afford only a slight indication of their existence. This character, was, however, more obvious in the Sunderland specimen; but, unfortunately, circumstances not allowing of its being permanently placed when it was first obtained, the part which exhibited the branches has been destroyed. In support of *Sigillaria* having had a branching stem, I may here mention, that a specimen which was sent to Mr Hutton for examination by Mr Dawes of Manchester, was clearly a branch which had been broken off at its junction with the parent stem. Some other specimens sent by the same gentleman exhibited a similar character; and Brongniart mentions that the Kunzwerk specimen, previously alluded to, was divided into two equal branches. In consequence of a horizontal fracture occurring at that part of the stem where, as before mentioned, the diameter becomes suddenly increased, it may be supposed that this augmentation is due to the fracture, especially as the same is observable on the other specimen in the Newcastle Museum. I am inclined, however, to think that it is due to the stem being unequal in its horizontal growth.

All the specimens are distinctly ribbed and furrowed longitudinally. The most perfect one in this respect has some of its ribs furcated; in one instance there are two furcations on one rib; by this means the ribs become considerably narrower at the top than at the bottom; in general, however, this diminution is caused by the ribs decreasing gradually in width in the ascending direction; at the base where they are nearly obsolete, the average width is about three inches, but at the top, where they are distinctly marked, it is reduced to less than an inch. On the middle of each rib is a row of scars in triple series, that is, three are placed in close contact with each other in a horizontal direction. The middle scar of each series is very small, compared with those lateral to it; and hence they have rather the appearance of being in pairs. Brongniart's figure of *Sigillaria lævigata,* shews the triple nature of the series where the specimen is decorticated. Now, as each series of scars undoubtedly resulted from one bundle of vessels passing from the internal vascular system into a single leaf or other external appendage, it will be necessary to bear this in mind so as to distinguish those scars from another character hereafter to be mentioned under the name of leaf-scars. The distance of the series of vascular scars from each other longitudinally, and on corresponding parts of different stems of *Sigillaria,* appears to vary, at least this is the case with the three individuals under consideration. On the one, in the Sunderland Athenæum, they are much closer to each other at the bottom than at the top, but on the specimen in the Newcastle Museum, already described, they are nearly at equal distances from each other (generally $\frac{7}{8}$ of an inch), throughout its whole length. The other Newcastle specimen offers some disparity in this respect; at the bottom, where it is very much wrinkled transversely, the vascular scars are an inch and a quarter from each other; from two to four feet from the base there is only half an inch space between them; and on the remaining part of the stem, this space is increased to an inch. This singular approximation of the vascular scars on the middle of the stem, does not apear to arise from any downward pressure, for this part shews no appearance of wrinkles, but, on the contrary, a perfectly smooth surface. These evidences, and others hereafter to be adduced, incline me to think that the stems of *Sigillaria* were subject to irregularities in their longitudinal development. This last specimen affords another remarkable character, the existence of which might be readily doubted, were it not too obviously displayed; on the lower part of the stem and completely round it, the median line of the ribs is sunk below the level of the furrows, so that the former are actually concave instead of being convex.* As I intend hereafter to advert to the origin of this remarkable character, any further allusion to it at present may be conveniently deferred.

Besides the vascular scars, the whole surface of the ribs in a great many places is marked with longitudinal *striæ.* In general these *striæ* are straight, but occasionally they are flexuose, especially where the ribs are broad, and in this case they turn in towards the vascular scars. Lindley and Hutton's figures of *Sigillaria reniformis* (Pl. 57), and the so-called *Sigillaria flexuosa* (Pl. 205), afford a good illustration of both kinds.

I shall now consider some other characters of *Sigillaria,* and for the present waive any further particulars connected with the specimens which have just occupied our attention.

The most obvious features characteristic of the stems of this genus are

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* *Vide* Plate I, diagram 2, which shews the ribs *a* reduced to half the width of the originals; *b* indicates the furrows.

the ribs, the furrows, and the scars on their surface; specimens shewing the original or outer surface of the cuticle are rather uncommon; when this surface is exhibited, a line of scars may be seen running up the middle of each rib, and possessing a character which distinguishes them from those already mentioned under the name of vascular scars, and which character is entirely due to the presence of the cuticle. On the North Biddick specimens, from their being decorticated, we merely observe the scars left by the vessels which proceeded from the internal vascular system into the leaves; whereas, on those specimens which still retain the cuticle, we observe not only the vascular scars, but likewise the impressions left by the fallen leaves. Hence, the latter have been termed leaf-scars. Corticated examples of *Sigillaria* shew each of these impressions to consist of a defined border surrounding an area, in which are situated the triple series of scars left by the vessels previously mentioned.* The leaf-scars in young branches of the common ash and the plane-tree offer a good illustration of those under consideration.

The remains of *Sigillaria* are generally found very much flattened, and consequently the leaf-scars seldom shew any appearance of standing out in relief; this, however, it would appear was not the case originally, at least in some species, for I have seen a beautiful specimen belonging to Mr Dawes, preserved in ironstone, having the lower margin of the scars projecting about a quarter of an inch beyond the level of the ribs.

The details already given as to the internal structure of *Sigillaria elegans* shew that its cuticle is composed of two distinct layers, the innermost one of *prosenchyma*, and the outermost of *parenchyma*, or ordinary cellular tissue; from this peculiarity it may be supposed that the cuticle is in some cases separable into two portions, and that single specimens of *Sigillaria* ought to be found, shewing various surfaces of the cuticle. As yet, I have not seen this in any of the specimens which have come under my notice, but I strongly suspect that some in this state are figured in Brongniart's "History of Vegetable Fossils." I will revert to those shortly. The cuticle of *Lepidodendron*, according to Mr Witham,† is similarly constituted, and in confirmation of this, it may be mentioned, that I have seen corticated specimens of this genus having the outer surface of both layers of the cuticle exposed.‡ Had Steinhauer been aware of the compound nature of the cuticle of *Lepido*-

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* The triple nature of the vascular scars, is decidedly more obvious in corticated specimens than in those which are decorticated.

† "The internal structure of Fossil Vegetables found in the Carboniferous and Oolitic deposits of Great Britain, described and illustrated," 4to, 1833.

‡ The specimen of *Lepidodendron*, figured by Steininger, appears to exhibit the *inner* surface of each layer of the cuticle, and also an impression of the outer surface of the outermost layer. *Vide* "Geognostische Beschreibung des Landes zwischen die Untern Saar und dem Rheine," 1840.

dendron, I have little doubt that his description of the various impressions of this fossil, under the name of *Phytolithus cancellatus,* would have been in every respect complete.*

With reference to *Sigillaria,* let it be supposed that the diagrams 3, 4, and 5, of Plate I. represent its cuticle under the various appearances, and in the different states of division as just supposed, and it will be seen in what manner several different surfaces may be afforded by a single specimen.

Let us suppose, that, in the third diagram, the cuticle *a b* of *Sigillaria* is preserved in a fragment of slate, which is divided where the specimen is imbedded,—that the cuticle adhered to that half or division marked B, and that its original or external surface is exposed. Now it is evident, that, in this case, there will be exposed on the other half of the slate A, a surface which will differ from the former, in displaying the leaf scars placed on wide furrows instead of ribs. Brongniart's† figures of *Sigillaria lævigata* (Pl. 143, fig. 1.) and *Sig. Sauli* (Pl. 151), represent the original surface of these species, while that of *Sig. pyriformis* (Pl. 153, fig. 3) shews merely an impression similar to that which ought to be seen on the division A.

With respect to the fourth diagram, let it be supposed, that, instead of the external surface of the cuticle being exposed, we have the opposite or internal one in this slate; the surface of that division of the slate marked A, will consequently be characterized with broad furrows, having no leaf-scars, but merely vascular scars; the latter will also be exhibited on the other division marked B, but, instead of being on broad furrows, they will be situated on ribs. The three North Biddick specimens are in this latter state, an illustration of which may be seen by consulting Lindley and Hutton's figure of *Sigillaria reniformis* (Pl. 57), while the figure in the "Fossil Flora" of the so-called *Sig. alternans* (Pl. 56), exhibits the inner surface of the cuticle, and consequently the appearance displayed on that division marked A. ‡

As regards the fifth diagram, it is to be supposed that the slate has been divided in such a manner as to exhibit the surface of both layers *a* and *b* of the cuticle, where they were in contact with each other, thereby leaving a layer on each of the divisions A and B. The cases already noticed have clearly afforded four different appearances; the present one

* American Philosophical Transactions, New Series, vol. 1, p. 280, &c.

† All the references at present made to Brongniart's figures must be understood to apply to his "Histoire des Vegetaux Fossiles," &c.

‡ I am perfectly satisfied as to what is said in the text respecting *Sigillaria alternans* being correct, for I have examined the specimen itself which Lindley and Hutton have figured. In this note I cannot but acknowledge the kindness and liberality of the latter gentleman, in allowing me the unreserved examination of his invaluable collection of fossil plants, especially when it is considered, that he was fully aware that my principal object was to support an opinion directly opposed to what he has advanced in the "Fossil Flora."

affords us two more, which, it will presently be seen, differ from the others. These latter, however, will vary according to the thickness of either layer of the cuticle; on those specimens in which this layer is thick, only the vascular scars will be seen; while on those in which it is thin, there will also be seen, though faintly, an outline of the leaf-scars. The surface of both divisions of the slate, A and B, may agree in this respect, the only difference being, that the scars will be placed on broad furrows in the former, and on ribs in the latter. I have for some time suspected that the two fossils which Brongniart has published under the names of *Syringodendron pachyderma* and *Syr. cyclostigma* (Pl. 166), as well as some figured by Sternberg (Pl. 13, fig. 1), and Lindley and Hutton (*Syr. organum*, Pl. 70), are true *Sigillariæ*, and that they ought to be classed in the last division, that is, with those specimens which exhibit *only* the outer surface of the innermost layer of the cuticle. The reason for this I consider is involved in the fact, that the whole of those fossils shew the vascular scars most distinctly on the outer surface of the exposed cuticle, while the leaf scars appear to be absent. Another argument in favour of this view is afforded by *Syr. organum* just referred to, and some other specimens which have come under my notice: when they are held up to the light in favourable directions, these last shew appearances which resemble the usual form and size of leaf-scars, but which are precisely as if they were caused by impressions that had passed through an intervening layer of cuticle from the leaf scars. It requires to be mentioned, however, that I am quite as much in favour of the opinion that the outer surface of the outermost layer of the cuticle is the one exposed on these specimens; but I cannot agree with Brongniart in supposing that the peculiarity of their scars renders it necessary to place them in a genus distinct from *Sigillaria*. I am, on the contrary, inclined to think that this peculiarity is merely due to the falling off of the leaves, and the subsequent increase of the stem,—two circumstances which, it is evident, would tend to obliterate all traces of the margins of the leaf-scars. Besides the scars, the surface of both divisions, as well as that of B, in diagram 4, will exhibit a number of longitudinal *striæ** which constitute an important character, inasmuch as it enables us to distinguish these surfaces from the original or external one of the cuticle, which, on the contrary, is in general marked with transverse lines, either straight, sinuous, or angular.† As the longitudinal *striæ* are always seen on the surfaces resulting from the innermost layer of the cuticle,‡ I am inclined to think

* *Vide* the decorticated parts of *Sigillaria pachyderma* (Plate 150, fig. 1) and *Sig. Schlotheimi* (Pl. 152, fig. 4), as figured by Brongniart in his " Vegetaux Fossiles."

† *Vide* Brongniart's figures of *Sig. pachyderma* and *Sig. contracta* (Plate 147, fig. 2).

‡ The innermost layer of the cuticle of *Lepidodendron* is also formed of *prosenchyma*, and the surfaces resulting from it exhibit similar longitudinal *striae*. Casts of the ligneous cylinder of *Stigmaria*, likewise shew impressions of this ind, which are evidently caused by the elongated vessels of which it is formed.

that they are due to the arrangement in linear series of the elongated cells composing the prosenchymatous tissue of this layer, and as such are to be distinguished from the lines which are occasionally seen running up the lateral parts of the ribs of some corticated specimens, which shew the outer surface of the cuticle,* and which lines I look upon as having a totally distinct origin, as resulting in fact from the horizontal extension of the stem. It is by means of these longitudinal *striæ* that I have arrived at the conclusion, that Lindley and Hutton's figure of *Sigillaria flexuosa* does not exhibit the original or external surface of the cuticle of this so-called species.

It has been previously remarked, that I have not met with any single specimens of this fossil shewing different surfaces of the cuticle: it is probable, however, that *Sigillaria hexagona* (Brong. Pl. 155) is in this state; the part shewing the leaf-scars with a well-defined border is obviously the external surface of the plant,—that which shews these scars with a less defined border, appears to be the external surface of the inner layer of the cuticle,—and the one which exhibits this character of the scars very obscurely, is probably the impression of the inner surface of the same layer. That the last part alluded to does not exhibit the matrix on which the specimen is preserved, worn down below the impression which ought to result from the inner surface of the inner layer of the cuticle, is, I think, fully proved by the longitudinal *striæ* thereon exposed being so strongly marked. I would also suggest that the various appearances exhibited by the figures of *Sigillaria Knorri* (Brong. Pl. 156, fig. 3) may be similarly produced.

In none of the descriptions of *Sigillaria* that have come under my notice, is there any mention made of this plant possessing axillary buds. That such constituted one of its characters can admit of little if any doubt, after an examination of the outline † which is given in our plate of illustrations, and which is a *facsimile* of one of the leaf-scars of a specimen in the Newcastle Museum, resembling Brongniart's *Sigillaria scutellata*, in the width of its ribs and the distance of the scars from each other What I wish to call attention to in this outline is the prominence *c* above the margin of the leaf scar *a*, a character which is seen above the whole of the scars exhibited in the specimen.‡ Now, their situation relatively to the leaf-scars being the same as that of the axillary buds of Dicotyledons, and their occurrence above all the scars of the specimen, is, I think, equivalent to a demonstration that these prominences are the remains of the axillary buds of the plant on which they are preserved. This character, taken in connexion with what has been stated of the branching

* *Vide* Brongniart's figure of *Sigillaria reniformis.*

† *Vide* Plate I, figure 6—*a* the vascular scar—*b* the triple series of vascular scars—*c* the axillary bud.

‡ I have observed similar prominences, though they are not so obvious, on some other specimens of *Sigillaria*; but instead of being a little above the leaf scar, they are in close contact with its superior margin,—a position which, it is evident, must render it difficult to see them.

of the North Biddick and other specimens, is, in my estimation, quite conclusive in proving that the branches of *Sigillaria* originated, as in the higher vegetable forms, from the axils of the primary leaves.*

In accordance with the preceding remarks respecting the bipartition of the cuticle of *Sigillaria,* we are naturally led to arrange a number of specimens, in the states represented by the diagrams, under three separate divisions. These divisions may be severally termed *corticated, demicorticated,* and *decorticated.*

The corticated division will embrace those specimens having the entire cuticle preserved; the demicorticated such as have only one of the layers of this part remaining, and the decorticated those which are divested of both layers.

Corticated specimens will exhibit two different appearances accordingly, as the inner or outer surface of the entire cuticle may be exposed; for instance, in the latter case we shall have leaf-scars on the ribs, but in the former there will be vascular scars on broad furrows. Examples of the first kind are of exceedingly rare occurrence in the North of England coal-field; nearly all that are published in the "Fossil Flora" from this district are either decorticated or exhibit only the inner surface of one or other of the two layers composing the cuticle: the figures representing *Sigillaria alternans, Sig. catenulata, Sig. reniformis* (Tab. 71), and some others, may be taken as examples of the last kind.

Demicorticated specimens are extremely difficult to recognise, on account of the appearance which they present being somewhat similar to that displayed on the inner surface of the inner layer of the cuticle, and consequently they may resemble the kinds last mentioned. Demicorticated specimens are also divisible into two kinds, the one having the vascular scars on ribs, and the other on broad furrows.† As already suggested, Brongniart's figures of the genus *Syringodendron,* and I may also add some of those given by Sternberg‡ and Lindley and Hutton as ex-

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* I have also observed axillary buds on several specimens of *Lepidodendron,*—a character which completely proves that the branches of this plant have had an origin similar to those of *Sigillaria,* and not from a division of the *point* of the main stem at certain stages of its developement, as is generally considered. Sternberg's figures of *Lepidodendron aculeatum* (Tab. vi. fig. 2; Tab. viii. fig. 1. B. a. b.), are the only published ones known to me that exhibit these axillary buds; the fossil is merely an impression, consequently, the figures shew them in the state of impressions.

† The fact already stated respecting the occasional flexuous appearance of the longitudinal *striæ,* on the ribs of the North Biddick specimens, will in a great measure shew that this character is not sufficient to constitute a specific difference.

‡ The specimen on which Sternberg founded his genus *Syringodendron* (Tab. xiii. fig. 1), appears to be nothing more than a decorticated portion of *Stigmaria* (root of *Sigillaria*) belonging to a part adjacent to the stock. The irregularities of the furrows, and the irregular arrangement of the vascular scars of this specimen

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Fig. 1.

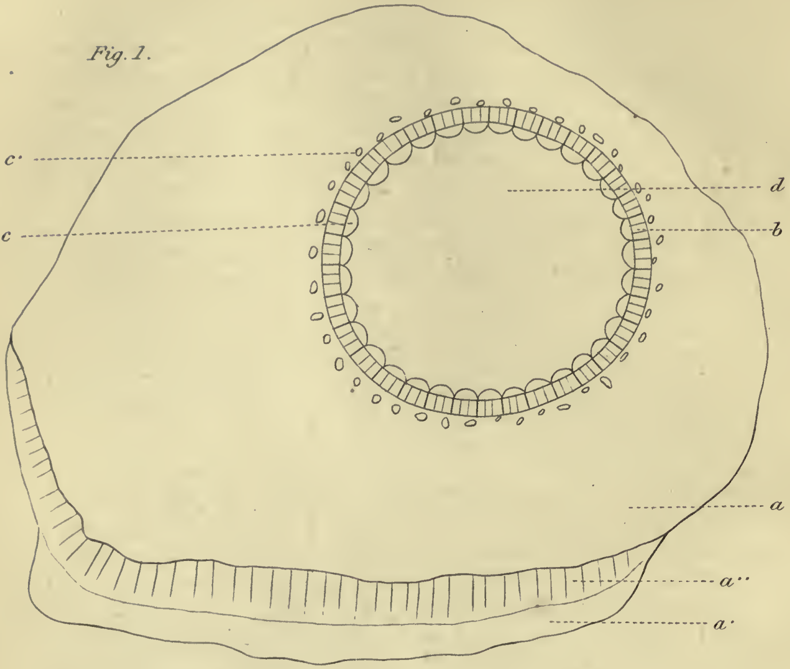


Fig. 2.

Fig. 3.

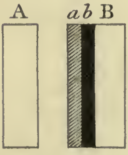


Fig. 4.

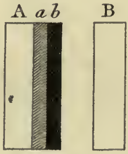


Fig. 5.

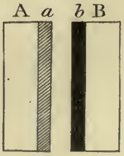
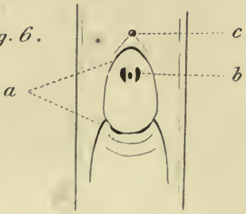


Fig. 6.



amples of this genus, may represent different species of *Sigillaria* in a demicorticated state.

Decorticated specimens are by far the most abundant. Lindley and Hutton's figure of *Sigillaria reniformis* (Pl. 57), affords a good illustration of the kind displayed on that division of the diagram 4, marked B; while Brongniart's representation of *Sig. pyriformis* (Pl. 153, fig. 3), shews the appearance on the division marked A of diagram 3; the former exhibits an impression of the inner surface of the cuticle, and the latter shews an impression of the outer or original surface of this part.* I have a single observation to make respecting specimens of the first division, which is, that they expose the vascular scars situated on convex ribs, with the exception of the previously noticed specimen from North Biddick. It is necessary to bear this in mind, as it is highly probable that hand specimens may be found displaying the same character, and as such will be otherwise inexplicable.

What has now been stated will, I think, make clear the mode in which one specimen of *Sigillaria* may afford six different appearances; should this be admitted, it necessarily follows, that, of the sixty or more species which have been described of this genus, a great many are purely factitious: other considerations to be adduced hereafter will tend still further to reduce this number.

(*To be continued.*)

United States Exploring Expedition.

England and France have long been honourable rivals on the ocean, as well in exploring as in warlike expeditions. The voyages of Cook, Vancouver, Flinders, Parry, Beechey, King, Fitzroy, and Ross, are conspicuous in the annals of English navigation; while France is no less honoured by her explorations under Bougainville, La Perouse, Labillardière, Duperrey, Freycinet, and D'Urville. Both countries have looked beyond the mere discovery of new lands, new commercial resources, and territorial aggrandizement. Their efforts have been directed towards an increase of knowledge in every branch of science; and there are few regions, from the equator to the poles, which have not been tracked by their vessels. What-

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are precisely the same as is seen on the origins of the branches of this fossil; that is, when the characters of the stem become blended with those of the root.

* The so-called *Sigillaria monastachya* (Pl. 72) of Lindley and Hutton, which I perceive is erroneously considered by Presl a species of Ulodendron (Sternberg, parts 7 and 8, p. 186) is, I am convinced, a decorticated isolated rib of *Sig. reniformis*. The specimen figured in the Fossil Flora is in the Newcastle Museum.

ever could illustrate the condition or resources of the regions visited; the customs, languages, or history of their unknown tribes; or the motion of the winds, the waters, the world, or the stars, has been thought worthy of observation. Cook was dispatched to the Pacific Ocean expressly to observe the transit of Venus, and Sir Joseph Banks and Forster accompanied him at different times in his voyages around the world. In the late voyage of Fitzroy, Mr Darwin was associated with the expedition, and made large contributions to science. France has outstripped England in the liberality with which her expeditions have been fitted out, and in the magnificence of her publications. The many folio volumes of plates, published as the result of the voyages of Freycinet, Duperrey, and D'Urville, and those of Napoleon's expedition into Egypt, are among the most splendid productions of the age. They are a noble gift from France to the world.

America has at last taken her part in the labours of exploration. An Exploring Expedition has been sent out, and has returned. It was organized on a plan honourable to a nation that is second to none in enterprise and general education; and its results, when published, will, it is believed, equal in amount and interest those of any expedition that has preceded it. The expedition sailed under the command of Lieutenant Charles Wilkes, who was aided by intelligent officers, well-fitted for the duties to which they were called; and the large number of charts that have been made in the course of the cruise, evince alike the energy of the commander, and the industry and skill of all engaged in the surveys. The duties have been extremely laborious, beyond the conception of the comfortable house-dweller at home. The loss of one schooner with all hands, including two officers; the total wreck of another vessel—the sloop of war Peacock—stripping the crew of everything but their lives; the massacre of two officers by the savages of the Feejee islands, and of a sailor by the treacherous Kingsmill Islanders, are the only fatal disasters; but they are a few only of its perils. Indeed, there were dangers everywhere, by land as well as by sea. The personal adventures in the course of the cruise, told as simple tales, without exaggeration, would make a volume full of startling incidents, and replete with interest.

It is gratifying to learn that the country will soon be put in possession of the facts collected. Thus far those engaged in it have alone been benefited. They have collected information that will be invaluable to them as men of intelligence and members of society. It remains for them to give this information to the country, that the people who have borne the expense may also partake of the profits. The affairs of the expedition are in the hands of the Library Committee of Congress, and, under their direction, Captain Wilkes has been put in charge of the history of the voyage, the charts, and philosophical observations, and the other departments of science are placed in the hands of those that had charge of them during the voyage. Each will prepare his own reports, reap his own honours, and be held responsible for his own facts. The extent of the work cannot be definitely stated: the plates will form several folio volumes in the style of the voyage of the Astrolabe.

As the country is much interested to know what has been done by the expedition, it is proposed to give, in as brief a manner as possible, some idea of the material on hand for publication, and the general character and extent of the collections. Our acquaintance with the gentlemen of the expedition enables us to state many particulars which have not yet appeared in print, the accuracy of which may be relied on.

We prelude our remarks, by giving the track of the vessels as laid down in Captain Wilkes's synopsis of the cruise.

On August 19, 1838, the vessels left the Capes of the Chesapeake, and sailed for Rio Janeiro, making short calls at Madeira and the Cape Verds. From Rio, on the 6th January following they proceeded to Rio Negro, on the northern confines of Patagonia, and thence to Nassau Bay, in Terra del Fuego, just west of Cape Horn. From this place, the Peacock, Porpoise, and the two schooners, made cruizes in different directions towards the pole; but the season was too far advanced for much success, as it was already February 24th before they sailed. The schooner Flying Fish, notwithstanding, reached latitude 70° 14′ S., nearly the highest attained by Cook, and not far from the same longitude. The ship Relief was ordered to enter a southern channel opening into the Straits of Magellan, but met with constant gales, and barely escaped being wrecked, after a loss of four anchors, at an anchorage she made under Noir Island, to escape the rocks of a lee coast. The Vincennes remained at Nassau Bay to carry on surveys and magnetic observations. In May of 1839, the vessels were again together at Valparaiso, with the exception of one schooner, the Sea Gull, which was lost in a gale shortly after leaving Nassau Bay. The vessels sailed on the 6th of June for Callao, Peru, and from here, the Relief, having proved ill-adapted for such a voyage, was dispatched home. On the 12th July, the squadron left the South American coast and sailed west, visiting and surveying fourteen or fifteen of the Paumotu Islands, two of the Society Islands, and all the Navigator group; and on the 28th November reached Sydney, New South Wales.

The vessels next proceeded on their second Antarctic cruise. Land was first discovered in longitude 160° E., and latitude 66° 30′ S. The Vincennes and Porpoise pursued the barrier of ice to the westward as far as 97° E. longitude, seeing the land at intervals for 1500 miles. When the barrier of ice permitted, the Vincennes sailed along "within from three-fourths of a mile to ten miles of the land." In a place they called Piner's Bay, soundings were obtained in 30 fathoms, and they had hopes of soon landing on the rocks; but a storm came up suddenly, which lasted for thirty-six hours, and drove the vessel far to the leeward; they consequently pushed on with their explorations to the westward, hoping for some more accessible place, but were disappointed.*

Large masses of rock were collected from the icy barrier in close proximity to the land, which are now deposited in the National Gallery at the Patent Office. Two of the masses, one of basalt, and the other of compact red sandstone, weigh each about eighty pounds. Besides these, there are many smaller specimens of gray and flesh-coloured granite, gneiss, white and red sandstone, basalt, and reddish clay or earth. The Peacock was enclosed in the ice soon after reaching it, when penetrating towards an appearance of land ahead, and for twenty hours they were barely hoping for life. They had obtained soundings in 320 fathoms.† On the 24th of February,

* See the Synopsis of the Cruise by Captain Wilkes.

† There has been much incredulity in the country with regard to the discovery

1840, the Vincennes left the ice, and by the 24th of April, all the vessels were together at Tongatabu. During the Antarctic cruise, the scientific gentlemen were occupied making observations and collections in New Holland and New Zealand; they joined the squadron at the latter place.

After delaying a day or two at Tongatabu, the squadron proceeded to the Feejees, where nearly four months were industriously occupied in surveys and various scientific observations. Thence they sailed for the Sandwich Islands, passing on the way, and surveying several small coral islands. The Vincennes spent the winter at this group, and in the course of it, the pendulum, and other philosophical instruments, were carried to the very summit of Mauna Loa, an elevation of 14,000 feet. Occasionally, at sunset, they observed the sublime spectacle of the shadow of this mountain dome projected upon the eastern skies.

During the same time the Peacock and schooner Flying Fish were cruising in the equatorial regions of the Pacific, visiting and surveying numerous scattered coral islands, besides the Navigator's and the Kingsmill group, and others of the Caroline Archipelago. The Porpoise made charts of several of the Paumotu Islands not before surveyed, and touched again at Tahiti.

In the Spring of 1841, the Vincennes and Porpoise were early on the coast of Oregon. The Peacock and Flying Fish arrived there in July; and, while attempting to enter the Columbia, the Peacock met with her disaster. There were several land expeditions into the interior of Oregon, of from 500 to 1000 miles each, and one of about 800 miles from the Columbia River to San Francisco, in California.

The vessels left California in November 1841, touched for supplies at the Sandwich Islands, and proceeded to Manilla,

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of this land, owing probably to mistaking the dispute with the French with regard to priority of discovery, for a dispute with regard to discovery itself. The facts here stated set the subject at rest. Within a few weeks, acknowledgments have reached this country from the French expedition, yielding the priority to the American expedition, and it will be so stated in their forthcoming publications. The part of the line of land which Ross is said to have sailed over, was a discovery claimed by Bellamy, and which Captain Wilkes added to the chart he sent Captain Ross, with Bellamy's name accidentally omitted in copying.

in the Philippines; thence to Mindanao, and through the Sooloo Archipelago, and the Straits of Balabac, to Singapore, which place they reached in February of 1842. They proceeded thence by the Straits of Sunda to the Cape of Good Hope, and passing by St Helena, the squadron arrived at New York in June of 1842, having been absent from the country about three years and ten months, and having sailed between 80,000 and 90,000 miles.

The number of islands surveyed during the cruise of the exploring expedition is about 280, besides 800 miles on the streams and coast of Oregon, and 1500 miles laid down along the land and icy barrier of the Antarctic continent. Numerous islands of doubtful existence have been looked for, shoals have been examined, reefs discovered and laid down, harbours surveyed, and many for the first time made known; and the latitudes and longitudes of the points visited have been determined with all possible precision. Very many of the doubtful points in the geography of the Pacific have been cleared up, and the expedition is prepared to supply our navigators with the most complete map of the ocean ever published.

Next to Oregon, the Feejee group may be considered the most important of the unexplored regions visited by the squadron. This group is a perfect labyrinth of lofty islands and coral reefs, and many disastrous wrecks have already occurred to our trading vessels in those seas. The islands are visited for biche-da-mar,* tortoise shell, and sandal wood; and there is no part of the year in which there are not some Yankee cruisers threading their dangerous way among its thousand reefs. The whole number of islands in the group is about 150; one of these contains about 4000 square miles, and another is but little smaller. They are rich and fertile, and will one day rank first in the Pacific for resources, as they are now first in extent and number. The harbours are numerous and convenient.

* The biche-da-mar is a kind of sea-slug, a sluggish, cucumber-shaped animal, that lives about the reefs. It is boiled, and dried over a smoking fire, and carried in ship-loads to the Chinese market, where it is esteemed a great delicacy.

Much might be said of Samoa, or the Navigator Islands, which, though less extensive, are more beautiful than the Feejees, and contain at least five times as much fertile land, in proportion to their extent, as the Sandwich Islands. But our remarks would lengthen out beyond allowed limits, should we speak even cursorily of the various regions that have been examined.

A few unknown islands were fallen in with, and one was discovered at midnight, just in time to avoid its reefs. But many such discoveries are not to be met with at this late day. At the island referred to, the natives were so completely ignorant of white men, as to believe them inhabitants of the sun ; for they thought that the great ship, or " floating island," as they called it, might sail off from the sun when it comes to the surface of the sea at night, or leaves it in the morning. All their little property was brought out by the terrified people, as a peace-offering to their imagined deities ; and when the boats shoved off from the shore, they pointed to the sun, and asked in their language, " you going back again ?"

Observations with the magnetic needle, thermometer, and barometer, have been constantly made throughout the cruise. The deep sea lead with a self-registering thermometer attached, has been sent down in the various seas passed over, and many interesting facts have been observed, that throw light upon the upper and under currents of the ocean. Observations were also made on shooting stars, the zodiacal light, the aurora australis, tides, the course and rotatory character of gales, &c. &c.

The manners and customs, mode of life, superstitions and religious observances, traditions, &c. of the people met with in the course of the cruise, received constant attention, and complete collections were made of their implements, manufactures, articles of dress, &c. These collections are now nearly arranged in the Hall or National Gallery at the Patent Office. Separate cases, or parts of cases, are allotted to the different islands, or groups of islands; and when labelled throughout, which is now in progress, the condition of the various tribes or races, and the degree of civilization among them, will be at once apparent to the eye. By a walk through the National Gallery, we travel with more than rail-road speed over the Pacific, and examine into their various productions, and the relative intelligence of the savages. The degradation of the New Hollander stands out in bold relief in contrast with the more advanced, though no less barbarous, Feejee. With the former, a war-club, and one or two other implements of war, including a small elliptical shield, is their all,—there are no dresses, no household utensils, for they use neither, and live without houses. Two cases* are filled with articles of Feejee manufacture, and among them are war-clubs of various kinds, spears, bows and arrows, native cloth of numerous patterns, dresses of the men and women, with bracelets and necklaces of shells and human teeth, wigs of Feejee hair, shewing the mode of dressing the head, native combs, paint for painting the face, their pillows (a stick like a broom-handle supported on short legs at each end), musical instruments, models of canoes,—indeed, all the arts and manufactures of the island, are well represented ; and were the chief Veindovi living, a visit to the Hall with Veindovi at hand, would be little less interesting than visiting the islands themselves. One advantage at least—no danger would be apprehended from a ferocious race of cannibals, that are ready to attack all intruders into those seas. Several Feejee skulls are to be found in a separate case, containing the skulls collected by the expedition ; among them, one bears the marks of the fire in a large burnt spot on the top of the head. Early one morning, soon after the Peacock came to anchor off a small Feejee town, she was boarded by a large number of natives, who came off with their half-eaten bones in their hands, the remains of the past night's cannibal feast. They continued eating the human flesh on deck, as unconsciously as we would eat an apple. One had the skull just referred to in his hand, and as he consented to part with it for some trifle, he gouged out the remaining eye, and went on eating off its muscles. This fact, so revolting, is here stated on account of the prevalent unwillingness to admit that cannibalism actually exists among the savages. This was seen both by men and officers, and from the facts collected, there can be no doubt of their enter-

* The glass-cases in the hall measure twelve feet by four, and are eight feet high.

taining an actual relish for human flesh. The pottery of the Feejees is among the most remarkable of their manufactures, as this art is not known to the Polynesian races. Collections equally curious were obtained at other places, but we must pass them by without remark.

The portfolios of the artists are rich in scenes of every kind, illustrating the islands or regions visited, and their inhabitants. The scenery of the islands, their mountains and forests, their villages, with interior and exterior views of huts and public houses—their spirit-houses or temples—fortifications—household utensils—canoes—the natives in council—dressed and painted for war—the domestic scenes of the village—costumes—tattooing—modes of cooking, eating, drinking cava, taking and curing fish, swimming, gambling, and other amusements—their war-dances, club-dances, jugglery, and numerous other particulars illustrating their manners and customs, have been sketched with fidelity. The portraits, too, are numerous, and so faithful, that the natives, who had not seen them taken, on beholding them, would cry out with surprise the name of the individual represented.

The number of sketches of scenes and scenery amounts to more than 500, besides 500 others of headlands; the number of portraits is about 200. They have been taken at all the places visited, from Madeira, where the vessels first stopped, throughout the cruise, to St Helena. It is unnecessary to enumerate the particular regions.

The principal importance of the observations and sketches illustrating the different races, consists in their bearing upon the history of these races, their migrations, and their physical and moral characteristics. These subjects, in connection with the study of languages, which together constitute the science of ethnography, received special attention during the cruise. The opportunities for observation have been unusually good, and the information collected will prove, it is believed, highly interesting. Only a few of the results can here be alluded to.

It has been long known that the inhabitants of the principal groups scattered over the Pacific to the east of the Feejee islands, those usually included under the general name of Polynesia, belong to one race, and, in fact, are one people speaking dialects of one general language closely allied to the Malay. Materials have been obtained for a comparative grammar and dictionary of the most important dialects (including those of the Sandwich, Society, Friendly, Navigator, and Hervey Islands, and New Zealand), and from this comparison, and the traditions of several of those islands, it is believed that the original seat of the population, viz. in the Navigator Islands, has been satisfactorily determined, and the course of the migrations has been traced out by which the different groups were peopled.

The vast island or continent of New Holland has heretofore been generally supposed to be inhabited by numerous tribes speaking languages entirely distinct. An opportunity, however, was found of obtaining a grammatical analysis of the languages of the inhabitants of two tribes living more than two hundred miles apart, and ignorant of each other's existence, which has resulted in shewing a clear and intimate resemblance, not merely in the great mass of words, but in the inflections and minute peculiarities of the two languages. By the aid of several vocabularies, the comparison has been extended across the entire continent, and has afforded fair grounds for believing that the inhabitants of New Holland, like those of Polynesia, are one people, speaking languages derived from a common origin. Much information was obtained from the missionaries and others concerning the character, usages, and religious belief of this singular race.

The inhabitants of the extensive and populous Feejee group have been viewed with peculiar interest, from their position between the yellow Polynesian tribes on the east, and the Oceanic Negroes on the west. The result of inquiries, pursued with care during a stay of nearly four months, has been to throw new and unexpected light on the origin of this people, and their connection with the neighbouring races. A mass of minute information in regard to the customs, traditions, and languages of these islanders, including a grammar and a dictionary of about 3000 words, will be given to the public.

The Kingsmill Islands are another interesting group, first accurately surveyed by the vessels of the expedition. They lie in the western part of the Pacific, directly under the equator. They are sixteen in number, all of coral formation, the highest land on any of them rising not more than twenty feet above the level of the sea, and their united superficies not exceeding 150 square miles. They afford no stone but coral, no quadrupeds but rats, and not more than thirty species of plants. Yet on this confined space, thus scantily endowed by nature, was found a dense population of more than 60,000 souls, in a state not inferior, as regards civilization, to any of the other islands of the Pacific. It is obvious that the character and customs of this people, as modified by their peculiar condition, must have presented much that was novel and striking. By the aid of two sailors who were fortunately found living on those islands, one of whom had been detained there five years without an opportunity of escaping, these points were minutely examined, the relations of the language determined, and the probable origin of the natives ascertained.

In the territory of Oregon, vocabularies have been obtained of twenty-six languages belonging to thirteen distinct families, a surprising and unexampled number to be found in so small a space. In general, where a multitude of unrelated idioms have been believed to exist, more careful researches, by discovering resemblances and affinities before unperceived, have greatly reduced the number. On the north-west coast of America, however, this rule does not hold good, and careful investigation, instead of diminishing, has actually increased the number of languages between which no connection can be proved. On the other hand, traces of affinity have been discovered where none were supposed to exist; and it is worthy of note, that one family of languages has been found extending from the vicinity of Bheering's Straits to some distance south of the Columbia River.

At Singapore the expedition procured from an American missionary there resident, a collection made by him with great pains, and at considerable expense, of valuable Malay and Bugis manuscripts relating to the history, mythology, laws, and customs of the East India islands. Since the loss of the splendid collection of Sir Stamford Raffles, which was burned along with the vessel in which it had been shipped for England, this is believed to be the best in existence. It is likely to be of great service hereafter, not less to the historian than the philologist.

The birds of the expedition already make a fine display in the National Gallery, although but two-thirds are yet arranged. In all there are about 1000 species collected, and double that number of specimens. Contrary to expectation, many of the birds of Oceania were found to have a very limited range. Some of the groups have species peculiar to themselves, and several insessorial species were found to be confined to a single island. About fifty new species were obtained.

The field for mammalia afforded by the voyage has been very limited. None of the Pacific islands, including New Zealand, contain any native mammalia, except bats. Much interesting information was, however, obtained relative to species met with on the continents visited, and a few new species were collected.

The following is a list of the number of species in the other departments of zoology, as nearly as can now be determined:—

Fishes,	829	Shells,	2000
Reptiles,	140	Zoophytes, exclusive of Corals,	300
Crustacea,	900	Corals,	450
Insects,	1500		

Of these the number of new species, is nearly as follows:—

Fishes,	250	Shells,	250
Reptiles,	40	Zoophytes, exclusive of Corals,	200
Crustacea,	600	Corals,	100
Insects,	500		

The following catalogue contains the number of species of reptiles and fishes collected at the islands and countries visited:—

	Fishes.	Reptiles.
Madeira and Cape Verds,	12	6
Rio Janeiro,	104	25
Patagonia and Tierra del Fuego,	14	5
Valparaiso,	32	11
Peru,	56	10
Paumotu Islands and Tahiti,	87	7
Samoa (or Navigators'),	64	8

	Fishes.	Reptiles.
Australia,	30	18
New Zealand,	25	6
Tongatabu and Feejees,	131	15
Sandwich Islands, about	100	4
Oregon, about	60	15
California,	20	2
Sooloo Sea,	18	8
Manilla,	32	1
Singapore,	21	9
Cape of Good Hope,	4	
At Sea,	9	

Of the 600 new species of crustacea, about 200 are oceanic species, of many of which, even the genera or families are unknown. The ocean swarms with minute crustacea, and it is seldom that a hand-net is thrown in good weather without bringing up some novelty. In some seas they are so numerous as to colour the ocean red over many square miles of surface, as was observed off the South American coast, near Valparaiso. These are the red or bloody waters that have been described. When thus numerous, these animals are often called whale's feed; and it is believed that they are actually the food of the "right whale." Each animal is not over a 12th of an inch long; yet they swarm in such numbers as to afford subsistence to these monsters of the deep. The fibrous network of whalebone, in the roof of the whale's mouth, is fitted to strain out these animals from the water, which passes through, and is ejected by the spout-holes. Many minute dissections have been made of these and other crustacea, and some interesting physiological facts brought to light. As the species are often transparent, nearly all the processes of life, even to the motion of every muscle and every particle that floats in the blood, are open to view.

The Anatifa (a species of barnacle) has been traced through its metamorphoses, from the young state, when it resembles a Cypris, and swims at large with distinct compound eyes, to the adult animal; and its connection with crustacea is placed beyond doubt.

The collection of corals at the National Gallery is one of its principal attractions. The great beauty and variety of these productions is not conceived of, even by those best acquainted with other collections in our country. These are the material that constitutes the immense reefs of the Pacific and East Indies, some of which exceed 1000 square miles in extent. More than three-fourths of all the islands of this great ocean have been built up through the labours of the coral animal. The formation of these islands, and the growth of the coral animal, the filling up and opening of harbours, and the rising of reefs,—all interesting subjects of discussion,—received particular attention; and the number of coral islands visited, and reefs examined, have afforded unusual opportunities for these investigations. Coloured drawings have been made of a large number of coral animals, which will convey some idea of their singular beauty and richness of colours. Many of these animals are wholly unknown to science, as this is a branch of zoology to which comparatively little attention has heretofore been paid, on account of the inaccessible regions in which they occur.

The following is the number of zoological drawings made during the cruize, in the departments of science here enumerated:—

Reptiles,	75	species.
Fish,	260	...
Mollusca (shells with the animals),	500	...
Zoophytes, exclusive of corals,	350	...
Corals,	140	...
Crustacea,	500	...

The variety and beauty of marine animals in the coral seas of the Pacific are beyond description. Like birds in our forests, fish of brilliant colours sport among the coral groves, and various mollusca cover the bottom with living flowers. A new world of beings is here opened to an inhabitant of our cold climate; and many of these productions are so unlike the ordinary forms of life, that it is difficult, without seeing them, to believe in their existence. Those that have looked over the beautiful coloured drawings by the artists of the expedition, are aware that this description falls short of the truth.

A large number of new species yet remains to be drawn. While there were so many things requiring immediate attention, it was impossible to sketch all; and those were selected for sketching on the spot, whose forms and colours were most liable to change.

Ten thousand species of plants, and upwards of fifty thousand specimens, constitute the Herbarium of the expedition. The following catalogue gives the number of species collected at the several places visited:—

Madeira,	300	Feejee Islands,	786
Cape Verds,	60	Coral Islands,	29
Brazil,	980	Sandwich Islands,	883
Rio Negro (Patagonia),	150	Oregon,	1218
Terra del Fuego,	220	California,	519
Chili,	442	Manilla,	381
Peru,	820	Singapore,	80
Tahiti,	288	Mindanao,	102
Samoa (Navigators' Islands),	457	Sooloo Islands,	58
New South Wales,	787	Mangsi Islands,	80
New Zealand,	398	Cape of Good Hope,	300
Auckland Islands,	50	St Helena,	20
Tongatabu,	236		
			9646

Including the mosses, lichens, and sea-weeds, the number will exceed 10,000. Besides dried specimens, 204 living plants were brought home, and are now in the green-house in the yard of the Patent Office, along with many others raised from seeds. The kinds of seeds obtained amount to 1156. Many of the expedition plants are now growing in the various greenhouses of the country, and also in England and other parts of Europe. Specimens of different woods have been preserved, the most interesting of which are those of large arborescent species of Oxalis, Viola, Repogonum, Piper, Geranium, Argyroxiphium, Dracophyllum, Rubus, Bromelia, Lobelia, and Compositæ of various kinds, besides sections of the Tree Ferns and Palms of the Tropics. There are coloured drawings of 180 species of plants beautifully executed.

Besides the observations at which we have glanced, in the departments of zoology and botany, particular attention was paid to the geographical distribution of plants and animals, and many important facts have been ascertained. The reports on this subject, with the accompanying illustrative maps, will be found to be among the most interesting of the results of the expedition. This subject bears upon the distribution of fossil animals, and the early history of our globe, and is exciting much attention among those interested in geological investigations.

The regions examined by the expedition have been highly interesting in a geological point of view. The islands of the Pacific east of New Caledonia are either basaltic or coralline. A large number of the latter (as already stated) have been examined, and much that is important has been brought to light. The facts strongly confirm Darwin's theory with regard to the formation of these islands, but lead to very different conclusions respecting the areas of subsidence and elevation in the Pacific. Numerous facts bearing upon this subject were collected. The basaltic islands are of various ages, from the most recent volcanic to a very remote period, probably as far back as the middle of the secondary era. The older islands are remarkable for their singular topographical features. There is scarcely any part of the world where such profound gorges, and sharp and lofty peaks and ridges, are thrown together in a manner so remarkable. On one of the high ridges of Tahiti (Society group), about 6000 feet above the sea, the summit edge is so sharp, and the sides of the mountain so nearly vertical, that the adventurous traveller may sit astride of it, and look down a precipice of 1000 feet on either side. In no other way except by thus balancing and pushing himself along is it possible, for about 30 feet, to advance towards the summit before him—yet 1000 feet higher—for the bushes which are growing on the crest elsewhere and serve as a balustrade, are here wanting. The famous coral bed on the mountains of Tahiti, was looked for without success.

The Sandwich Islands contain basaltic rocks of all ages, from the most recent volcanic to the most ancient in the Pacific, besides coral rocks and elevated reefs ; and they are full of interest, both as regards the structure and formation of igneous and limestone rocks, and geological dynamics. The lofty precipices and examples of shattered mountains before the eye, are astounding to those who see only the little steeps of a few hundred feet at most in the surface of our own country. There is evidence that the island of Oahu is the shattered remnant of two lofty volcanic mountains. A precipice on this island, upwards of twenty miles long, and from 1000 to 3000 feet high, is apparently a section of one of those volcanic mountains or domes, along which it was rent in two, when the greater part was tumbled off and submerged in the ocean.

Oahu is fringed in part with a coral reef 25 feet out of water ; and similar proofs of still greater elevation are met with on the other islands.

New Holland afforded the expedition a collection of coal plants from the coal region ; the coal is bituminous, and the beds are extensive. Large collections were also obtained of fossil shells and corals (about 180 species in all), from the sandstone next the coal. The geology of the coal region, and of the overlying sandstone, and the fossiliferous sandstone below, together with the trap dykes and beds, will prove highly interesting. These are the only rocks observed.

About 100 species of fossils, including vertebræ of Cetacea, and remains of four species of fish, crabs, echini, and shells, were collected from a clayey sandstone, near Astoria, on the Columbia. Various explorations were made in the interior of Oregon, and on a jaunt overland to California.

The Andes were ascended both in Chili and Peru, and in the latter an ammonite was obtained at a height of 16,000 feet.

The collections at the National gallery contain suites of specimens from all the regions visited, including gems, and gold and iron ores from Brazil, the copper and some of the silver ores of Peru and Chili, besides others illustrating the general geological structure of these countries.

But our remarks have already extended to an unexpected length. The facts enumerated, although but here and there one from the mass which have been collected, are sufficient to evince that the nation which has done honour to itself in sending out an exploring expedition so liberally organized, will have no reason to be disappointed in the results. European nations already appreciate it, and speak higher praise than has yet been heard on this side of the waters. The advantages accruing to commerce alone, from the large number of surveys made, reefs discovered and laid down, unknown harbours examined, resources of islands and countries investigated—and from the permanent footing on which intercourse with the Pacific islands has been placed by the settlement of long standing difficulties, and the ratification of treaties, and the impression produced by an armed force, more than repay for expenditures. The expedition has performed the duties of an ordinary squadron in the Pacific, and has accomplished in this way manyfold more in that ocean than any squadron that ever left our country; and if the expenses of keeping the vessels in commission are cancelled on this score, the sum which remains for the extraordinary duties performed will be but small.

But while we render to those whose labours have obtained the results of the expedition their full due credit, we cannot forget that there are others, and one in particular, whose zeal and untiring exertions in planning, and urging forward to its completion, this enterprise, deserve more than a passing acknowledgment. M. J. N. Reynolds was left behind, yet though unrewarded for his efforts by the pleasure of accompanying the expedition and adding to its laurels, his distinguished merits will not be forgotten or disregarded by his countrymen. —*The American Journal of Science and Arts*, vol. xliv., No. 2, p. 393.

On the occurrence of Fossil Human Bones of the præhistorical World in South America.

This notice is an extract* from a letter of Dr Lund of Lagoa Santa, South America, who, for the last six years, has been engaged in examining the animal remains found in the chalk caves of the interior of Brazil, and is now publishing a work in the Danish language, which bears the title, Blik paa Brasiliens Dyreverdu, &c., or, "A glance at the animal creation which inhabited Brazil immediately before the present geological epoch, and the now existing order of things."

* The above extract was communicated to Professor Silliman, by the Rev. E. E. Salisbury, Professor of Oriental Languages in Yale College, and inserted in the forty-fourth volume of the American Journal of Science and the Arts.

The information it contains is, indeed, nothing decisive as to the existence of the human species, contemporaneously with those great extinct animals whose remains are found fossilized in the earth's strata. But, as relating to the first instance of the discovery of human bones in a fossil state, it is of some interest. After mentioning that, up to the date of his letter, he had discovered in 200 chalk caves of Brazil, 115 species of mammalia, of which not more than 88 are now known to exist there, the writer proceeds :—

"In the midst of these numerous proofs of an order of things quite different from the present, I yet have never found the slightest trace of the existence of man. I supposed, therefore, that this question was decided, that human bones nowhere occur, when unexpectedly, after six years toil, I had the good fortune to find these bones ; and, indeed, under circumstances which admit of speaking with some certainty in favour of their occurring again. These bones I fell upon in a cave, mingled with the bones of decidedly extinct animals, as, for example, of the *Platyonyx Bucklandii*, *Chlamydotherium Humboldtii*, *C. Majus*, *Dasypus sulcatus*, *Hydrochœrus sulcidens*, *&c.*, which directed my whole attention to these remarkable remains. Besides, they all bore the stamp of genuine fossil bones, inasmuch as they were partly converted to stone, and partly impregnated with small particles of the oxide of iron, which not only gave them an extraordinary weight, but even to some of them a metallic glistening. As to the great age of these bones no doubt can exist ; but whether they date from the times of those animals, with the bones of which they were found lying together, in company, is a question which does not admit of being determined with equal certainty, since the cave is on the edge of a lake, by which the waters are yearly driven into it in the rainy season. Not only, therefore, might animal remains by degrees come there, but those brought there by the flowing of the water at later periods might also mingle with the earlier. This supposition has, in fact, received confirmation, in that, among the bones of extinct animals, there are also those of races still living. The condition of the latter, too, of which some appear to differ little from fresh bones, leads to this view, while others have reached the half metallic state spoken of, and between the two sorts a third and more numerous variety is distinguishable, which has reached a middle state in decomposition. A similar difference was observed also in the human bones, by which their varying gradations of age are clearly manifested. Yet all are so altered, as well in their constituent parts as in the joining, that one cannot deny them a high antiquity; and even should they not have come there contemporaneously with the bones of extinct animal races, still they have a sufficient interest in this respect. From the investigations of European students of nature, it results, that no land animal, of which the bones appear in a truly fossil state, has lived within our historical period, and that they, consequently, mount up over 3000 years. If this conclusion is applied also to the human bones existing in a like state, they too are of a like antiquity. Since, however, the process of fossilization is as yet little known, especially if the time necessary to this transformation comes into question; and, if it is true that this time varies according to circumstances, we can attain only to a very indefinite approximation. Be it nevertheless as it may, in any case these bones must have a high antiquity, not only far outreaching the discovery of America, but even surpassing all historical documents of our race, since up to this time no fossilized human bones have been before met with. But hence it results, that Brazil was peopled at a very remote period, and probably before our historical era; and the inquiries which therefore urge themselves upon us are these: Who were these oldest inhabitants? from what race were they descended? and what was their manner of life, and their natural quality of mind. Happily, these questions may be easily solved. Being in possession of several more or less perfect skulls, I was able to define the position which they have occupied in the anthropological system; and, in fact, the narrow head, the prominent cheek-bones, the angle of the face, the formation of the jaw and of the cavities of the eyes, shew that those skulls belong to the American race. The Mongolian tribes come, as is well known, the nearest to it, and the most striking difference between the two is the greater flattening of the head in the former species. In this point the discovered skulls not only accord with those of the American tribes, but some of them are to such a degree pressed in, that the forehead almost entirely disappears. It is known, that the human figures which were sculptured upon the ancient Mexican monuments are of a wholly peculiar conformation, and that the cranium, retreating backwards, was made to disappear immediately above the eyes. This anomaly, which in general is ascribed either to an artificial disfiguring of the head, or to the taste of the artists, thus receives a natural explanation, since it is proved that a race of men lived in these regions which possessed this conformation of the head.* The skeletons found belonged to both sexes, and were of ordinary size, though two male skeletons shewed a larger size. After these brief observations on the corporeal constitution of the primitive inhabitants of Brazil, we will also take into consideration their probable mental condition, and the degree of their cultivation. Since now it is proved that the development of the mental powers stands in direct relation to that of the brain, it follows, from the formation of the skulls found, that the intellectual life of their possessors must have played a very inferior part, and that their progress in arts and crafts must have been in the highest degree meagre. This inference is confirmed by the discovery of an utensil of the most imperfect make, which lay buried alongside of the skeletons. It was a semi-spherical hornstone of ten inches in circumference, which was worn smooth on the flat side, and evidently must have served to bruise seeds and other hard substances. As it cannot be my object, on this occasion, to make a thorough exposition of the present subject, a labour which I give over to abler hands, I have merely to observe farther, that I have found human fossil bones in two other caves also, which were almost without any gelatinous part, and therefore easily friable, and shewed a white fracture. Unfortunately, however, they did not occur with other animal bones, so that the question of chief importance, in respect to the longer existence of the human race on this earth, remains still undecided."

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* It is still probable, however, that this configuration of cranium was the result of artificial causes. (Professor Silliman.)

Report on M. Alcide d'Orbigny's Memoir, entitled General Considerations on the Geology of South America. By M. Elie de Beaumont.

Regarded as a whole, the portion of the American Continent situated to the south of the equator, exhibits a great variety of orographical configuration. On the east we have an immense group of low mountains, forming a mass whose branches extend from some degrees south of the Line as far as the mouth of La Plata; while on the west, we have the Cordillera, whose elevated summits commence near the Straits of Magellan, and extend into Columbia, forming a ridge which follows different directions, and from which rise the highest peaks of the New World. Between these two great systems, commencing from the south of Patagonia, a nearly level plain skirts the Cordillera, occupies the intervening space comprised between that important chain and the mountain mass of Brazil, passes from the basin of the Plata into that of the Amazon, afterwards expands to the east, and embraces to a long distance the two banks of that vast river.

Gneiss Formation.—In South America, as over the surface of the whole globe, the rocks which constitute the first formations of the series of stratified rocks are crystalline; they are chiefly gneiss. These rocks are especially developed in the eastern part of the Continent, where the modern geological products are less prevalent than in the western. All geologists who have visited Rio Janeiro have pointed out the gneiss formation. Messrs Clausen and Pissis have ascertained its existence over the greater part of the surface comprised between the course of the Rio San-Francisco and the sea, from the 16th to the 27th degree of southern latitude. M. d'Orbigny has found it again at Maldonado, at Monte-Video, and in the *Banda Orientale.* M. Parchappe recognised it in the chain of the Tandil. M. d'Orbigny discovered an immense belt of the same rock, occupying a mean breadth of half a degree, having a length of upwards of 340 English miles, and traversing the whole province of Chiquitos.

The old rocks are almost every where composed of the same elements; and these are, at Rio-Janeiro and in the province of Chiquitos, porphyritic or granitoidal gneisses, which are superimposed on granite, and form the support of fine grained gneisses, or of mica-slates containing garnets and gnenatites; at Monte-Video, and at Maldonado, there are blackish and finely laminated gneisses; and at Tandil, according to the determination of M. Cordier, there are tabular varieties of petrosilex.

In Brazil, and in the east of the province of Chiquitos, the gneiss everywhere supports transition clay-slates. But when the latter are awanting, the gneiss is succeeded by formations of a much more modern date; for at Conception, at San Ignacio, and at Santa Anna de Chiquitos, portions of the Patagonian territory formation repose on the gneiss. M. Pissio has pointed out tertiary deposits, resembling the molasse of Europe, reposing on the gneiss of the environs of Bahia. At Monte-Video, and in the Pampas, the gneiss is surrounded by the Pampian tertiary formation; and, lastly, at Chiquitos, it is covered by the recent alluvia.

Silurian System.—The most ancient beds which M. d'Orbigny has found superimposed on the rocks of a decidedly crystalline character in South America, present, wherever he has seen them, a very uniform composition. They are, in their lower portions, coarse clay-slates (*des phyllades schistoides*), having a blue colour, and often containing chiastolite; and they pass in their middle portions into fine grained clay-slates (*des phyllades satinés*), having a rose colour. These two series of beds, which are the most developed, possessing frequently a thickness of several hundred yards, do not contain any traces of organized beings. Above these there are sandstone flags (*des phyllades grésiformes*), or very micaceous slaty sandstones (*grès phylladifères très micacés*), whose thickness is upwards of 160 feet.

In these last mentioned beds, M. d'Orbigny has collected fossils, which, however, are rare, and which belong to the genera *Cruziana*, *Orthis*, *Lingula*, *Calymene*, *Asaphus*, and *Graptolithus*. Of ten species of these genera, *eight* are extremely analogous to the species of the silurian strata of Europe, and *three*, viz., *Calymene macrophthalma*, *Cruziana rugosa*, and *Graptolithus dentatus*, are even identical with them. We may say, therefore, that these fossils, as a whole, have the same aspect, the same *facies*, as those of the Silurian formations of Europe. Here is an identical zoological physiognomy transported thousands of leagues. The rocks also present mineralogically many analogies to those of the Silurian formations of Europe. This double relation, conjoined with the position these formations occupy below all the other fossiliferous deposits of South America, has naturally led M. d'Orbigny to refer them to the Silurian system, as established by Mr Murchison; and it is probable that they are at least very nearly connected with it.

These Silurian formations of South America occupy spaces of considerable magnitude, and occur at points very remote from one another. They present themselves along nearly the whole eastern border of the Bolivian plateau, forming a band which follows the Andes, properly so called, or the eastern Cordillera, and is parallel to the granitic rocks, from Sorata to Illimani, a distance of more than 300 English miles. To the east of the eastern Cordillera they are still more developed, and form a band of nearly 40 miles in breadth, and upwards of 600 miles in length, included between the plains of Santa Cruz de la Sierra on the east, and the 72d degree on the west. They thus form an immense band on the east, as well as on the west of the eastern chain, and which runs from N.W. to S.E., but is much more developed to the east than to the west of the chain. In the region included between the Andes and Brazil, we again find the Silurian series in the south of the province of Chiquitos, near Tapera, near San-Juan, to the north of the Sierra of Santiago, and to the south of that of Sunsas. They there constitute a band running from E.S.E. to W.N.W., and having a length of upwards of 150 miles. There, as in the Andes, they present, at their lower portion, a blue coarse slate, supporting fine rose-coloured clay-slates, on which repose yellowish slates. M. d'Orbigny was, however, unable to find any trace of organized bodies in these beds, of which the first has a thickness of at least 600 feet, while the others are not more than 50 or 60 feet thick.

In Bolivia, the Silurian strata possess an interest of a very direct kind, inasmuch as they contain the richest of the Bolivian gold mines, and also some mines of silver. Wherever gold is met with *in situ,* it occurs in veins of milky quartz, which traverse the lower beds, viz. the coarse clay-slates. It is in such a geological position, that the ore is mined on the slopes of Illimani, at Oruro, at Potosi, &c. If we consider that all the localities where gold is obtained by washing, are situated in valleys where the clay-slates have been much dislocated and denuded, as, for example, at the Rio de la Paz, at Tipoani, at the Rio de Suri, at the Rio de Choquecamata, &c., we must naturally conclude that the ore is derived from the same slates.

Devonian System.—Wherever M. d'Orbigny observed the Silurian series, it was succeeded by an enormous mass of hard quartzose sandstones or quartzites, which, from their position and fossils, he regards as the representative of the Devonian formation, or the old red sandstone. This extensively distributed system is composed of whitish or yellowish compact quartzose sandstones, without traces of fossils, passing, in their lower portions, into blackish or ferruginous, very micaceous, slaty sandstones, and only then containing the remains of organized bodies, sometimes in large beds, at other times disseminated through the strata of rock. These rocks are almost always superimposed on the Silurian series, and most frequently in a conformable manner. They are succeeded, but unconformably, by carboniferous strata, which are characterized, in a particular manner, by the fossils they contain.

This great quartzose deposit presents itself over as large spaces as the Silurian system, which it everywhere accompanies; and it is distributed in the same way. On each side of the band of Silurian rocks of the eastern chain of the Andes, it forms, for about 450 miles, another great parallel band, independently of the detached portions scattered through the interior of the Silurian band. There is also a great development of these quartzose sandstones on the Silurian formation of the eastern part of the province of Chiquitos.

Apart from his own personal observations, M. d'Orbigny has learned that these same quartzose formations abound in Brazil, in the chain of Parecys, in that of Diamantino, to the west of Motogrosso, and in those chains which are to the east of Cuyaba, mountains which follow the same direction as those of Chiquitos, and which, according to M. d'Orbigny, belong to the same system. Perhaps, he adds, the same rocks are to be found more to the east, in the province of Minas-Geraës, a supposition which seems to be confirmed by the meritorious labours of M. Pissio, which were lately laid before the Academy of Sciences.

MM. Humboldt and Eschwege have, for a long time, fixed the attention of observers on the rocks of stratified quartz which occupy vast areas in South America, to the south of the Equator, in Peru as well as in Brazil.* These stratified quartzes were divided among various primitive and secondary formations; perhaps a judicious application of the principle of metamorphism, such as was lately suggested by the memoir of M. Pissio,† may enable us to include all under one and the same formation, the Devonian formation of M. d'Orbigny. The exact determination of the age of the quartzose sandstones of Bolivia is thus an important question for the geology of South America, and even, we may say, of the greater part of the Southern Hemisphere, if, as we may presume, the quartzose sandstones of Table Mountain, near the Cape of Good Hope, belong to the same formation.

In the Devonian formation of the province of Chiquitos, M. d'Orbigny did not observe a single trace of fossils; whereas he observed fossils several times in the lower parts of the sandstones of the same system in Bolivia, especially at Achacaché, near the lake of Titicaca, in the environs of Cochabamba, near Totora, and at Challuani, in the province of Mizque, in the provinces of Tocopaya and Yamparaes, in the department of Chuquisaca. These fossils, which belong to the genera *Spirifer*, *Orthis*, and *Terebratula*, are always in the state of impressions, and occur in widely extending but very thin layers, between the laminæ of the rocks. Of seven species of these different genera which M. d'Orbigny brought from Bolivia, four have the greatest resemblance to fossils of the Devonian system of Europe. Some of the others ap-

* Humboldt, Essai géognostique sur le gisement des roches dans les deux hémisphères, p. 91, 96.

† Comptes Rendus, vol. xvii., p. 34. Meeting of 3d July 1843.

proach fossils, which, in Europe, occur in the Silurian rocks. Everything, then, leads us to refer this great quartzose deposit to the palæozoic formations. Its connection with the slate formation which it covers, does not allow of its being removed far from it; and as the union of these slates with the Silurian system seems to us reasonable, the reference of the great quartzose deposit to the Devonian system or old red sandstone, appears to us the most judicious that can be made in the present state of our knowledge.

Carboniferous System.—The system of Devonian quartzose sandstones is succeeded in Bolivia, and in some other parts of South America, by another series of beds which M. d'Orbigny refers to the carboniferous system. This new series of beds consists in its lower part of compact grey limestone containing siliceous nodules, analogous to the carboniferous limestone of English authors, and perfectly similar to that of Visé near Liège, and to the limestones of many parts of the British Islands. This limestone has been more particularly noticed in the islands of Quebaya (Lake of Titicaca.) At other points (at Yarbichambi), the lower parts of the same system present compact, yellow, or rose-coloured calcareous sandstones. These beds contain numerous fossils. In the islands of Quebaya, and at Yarbichambi, they are succeeded in conformable stratification by red non-argillaceous quartzose sandstones, which are friable and destitute of fossils. It is from observations made on these two points that M. d'Orbigny has concluded, that he may refer to the carboniferous series all the friable rose-coloured argillaceous sandstones which repose on the Devonian system, and which are inferior to the presumed Triassic variegated clays.

The system of beds, of which we have just indicated the composition, presents itself at a great number of points, and is distributed throughout nearly the whole breadth of the American continent. The Morro of Arica, washed by the waves of the Pacific Ocean, is composed, at its base, of a limestone, which seems to belong to the carboniferous epoch, judging from the impressions of a *productus* contained in one of the specimens collected by M. d'Orbigny, and from numerous organic remains observed by him at the locality. This limestone occupies but a very limited space, and the first points, advancing to the east, where the carboniferous formation acquires any development, are on the great Bolivian plateau. M. d'Orbigny there observed several chains of it, such as the Apocheta de la Paz, the hills of Aja, and of Aygachi de las Penas, all the islands of Quebaya and of Periti in the lake of Titicaca, and more to the south the hills of Guallamarca and of Pucara, and some other patches. In general, the carboniferous strata are distributed chiefly to the east and to the west of the great Bolivian system, where they attain, especially to the east, an elevation of more than 13,000 feet. The carboniferous formation likewise forms, in the Chiquitian system, summits whose height is sometimes nearly 5000 feet, both in several chains of that system in the east and north of the province, and also more to the east in the province of Minas-Geraës.

The different beds united by M. d'Orbigny under the denomination of the carboniferous system are nevertheless divided, as I have already said, into two distinct series, the one consisting chiefly of limestones, and the other of sandstones; the first being the lower and fossiliferous, the last the upper and without fossils; and these two series, which occur united on the great Bolivian plateau, are elsewhere separated, for M. d'Orbigny found only the upper reddish sandstones, and never the limestones, to the east of the plateau, and on the Chiquitian system. There is therefore an important difference in position between these two series of beds. This difference might induce us to doubt whether the upper series is really to be regarded as belonging to the carboniferous system, and might lead us to refer it with as much probability to some one of the systems which succeed the carboniferous group in Europe, for example, to the red sandstone. The lower series is in fact the only one which can with great probability be referred to the carboniferous system. It is indeed only in the limestones, and in the calcareous sandstones of the inferior series, that M. d'Orbigny has found fossil organic bodies. He met with them at Yarbichambi, and in the islands of Quebaya and Periti, in the lake of Titicaca. The shells are in an excellent state of preservation, and retain all the necessary zoological characters.

These fossils belong to the genera *Solarium* or *Euomphalus*, *Pleurotomaria*, *Natica*, *Pecten*, *Trigonia*, *Terebratula*, *Spirifer*, *Orthis*, *Leptaena*, *Productus*, *Turbinolia*, *Ceriopora*, and *Retopora*. Of twenty-six species collected by M. d'Orbigny, twelve, or nearly the half, have the greatest analogy with the fossils of the carboniferous series of Europe, and of these, three, viz., *Spirifer Pentlandi*, *Spirifer Roissyi*, and *Productus Villiersi*, are quite identical with the same species from Belgium and Russia. We have the same genera; also species having a common *facies*, and three of them quite identical. The whole *facies* of the fossils is so analogous, that at first sight we might imagine that we were looking at the usual species met with in the carboniferous rocks of Europe.

Among the fossils which do not belong to the carboniferous series of Europe we must remark a Trigonia (*Trigonia antiqua*), a genus which has not hitherto been noticed below the Jurassic formations. This curious discovery shews that M. d'Orbigny has understood how to ascertain, not only the resemblances of the American formations to ours, but also the differences between them,—differences which might well be expected at a distance 6000 miles, and which it is only surprising have not been found to be more considerable.

After the Silurian and Devonian periods, the American seas thus supported a different fauna from that of the two first epochs, and one completely analogous in character to that which lived during the carboniferous period in the seas of Europe. This analogy does not now exist between the faunas of the seas of Europe and of South America; and, as M. d'Orbigny remarks, it indicates, in the ancient geological periods, an uniformity of climate no longer observable. These inferences have so much the more weight, from being in this case supported on a triple basis. We have already spoken of the fossils which have induced M. d'Orbigny to refer the system of clay-slates of the Bolivian mountains to the Silurian system of Mr Murchison, and the system of quartzose sandstones to the Devonian system. Here, then, we have in South America, three members of the great palæozoic system, succeeding one another in the same order as the members of the same system in Europe, with which they have respectively the greatest analogy, Now, although we may retain some doubts as to the exactitude of the identification of these different formations considered individually, it seems difficult to avoid regarding it as certain that the palæozoic system of South America corresponds as a whole with that of Europe, and that it is subdivided in an analogous manner. This great fact, which has been completely demonstrated by M. d'Orbigny, appears to us to be one of the most important with which geology has been enriched of late years.

Triassic System.—Succeeding the palæozoic formations, and immediately above the carboniferous sandstones of M. d'Orbigny, there is in South America a system of beds which he refers to the Trias of Europe, and which Mr Pentland* likewise considers as its representative. This identification we regard as indicated with plausibility by the observations made, but still not so rigorously established as that of the palæozoic system.

The presumed triassic deposits of Bolivia are composed of an alternation of magnesian limestones, of variegated clays, and of friable argillaceous sandstones. The lowest beds consist of a magnesian compact limestone, which is frequently divided into very thin waved laminæ. M. d'Orbigny has seen this member of the formation, having but a small thickness, near Laguillos, and in the valley of Miraflor. Above these limestones there lie, at the same localities, laminated rose-coloured or variegated clays, which often abound in crystals of gypsum. Above the clays, in the valley of Miraflor, there are compact magnesian limestones, in which M. d'Orbigny discovered numerous fossils, but unfortunately he is unable to particularize more than one species, the others having been lost. This species, the *Chemnitzia potonensis*, belongs to a new genus of turreted shells, which approaches the Melanias.

The rocks mentioned above are very analogous to those which constitute the trias group in Europe. The limestones of the *Muschelkalk*, in the north-east of France, and in the department of the Var, as well as the *grès bigarrés* of the

* It is much to be regretted that Mr Pentland has not yet published the valuable results of his extensive scientific investigations in South America.—Edit.

same districts, have recalled to M. d'Orbigny the aspect of the rocks of Bolivia, which he compares with them. The American formations, whose nature and position thus lead us to refer them, at least provisionally, to the Trias, but whose paleontological characters are still almost unknown, seem reduced to occupy, at the present day, and that in the form of large detached portions, the two slopes of the eastern Cordillera of the Bolivian system, and they there attain, at their highest elevation, a height of 4000 yards above the level of the sea. They are probably the remains of a great whole which covered the surface before the occurrence of the geological catastrophes that impressed the present forms on its external physiognomy.

Absence of the Jurassic System.—One of the most remarkable circumstances in American Geology is the absence of the Jurassic formations, a fact announced a long while ago by Von Buch. M. d'Orbigny did not find a single fossil which seems to belong to that period. The only exception to the general rule hitherto brought forward is, that M. d'Orbigny saw some Jurassic Terebratulæ among the fossils collected by M. Domeyko from a limestone in Chili.

Cretaceous Period.—The deposits of the cretaceous period seem, on the contrary, to have been very much developed on the American continent, as is proved by the collections of fossils made by Von Humboldt, Boussingault, Degenhardt, and by the geologists of Dumont d'Urville's last voyage, Doctors Hombron and Le Guillou. They occur from Columbia to Tierra del Fuego, or over the whole length of South America, whilst nevertheless they are interrupted in the middle. At that epoch there lived in America as in Europe, particular forms of Ammonite and Ancyloceras, &c.; and independently of the general resemblance of the forms, there were in Columbia and in the Parisian basin enough of identical species to induce us to suppose, that there was a direct communication between the European and the Columbian portions of the Cretaceous sea. It is well known that this sea formed in France two great distinct basins, the Parisian and the Mediterranean. It appears that this same sea covered with its waters not only a considerable portion of Columbia, but generally a large part of the regions situated on the north, the west, and the south of the continent which then existed in those latitudes. The identity of the fossils of the chalk formation with those of the same formation in Europe, is not so great as regards the south of the American continent as it is in respect to the north, a circumstance which of course indicates a less direct communication. Perhaps we may infer the existence of some long piece of land, which continued as far as America the separation existing in Europe between the Parisian basin and the basin of the Mediterranean.

Tertiary System.—Another geological fact, and it is one of the most remarkable, is the immense extent of the Tertiary system in South America. When we compare it with the small basins disseminated throughout Europe, we are led to believe, along with M. d'Orbigny, that the smallness of the latter is an exception. The tertiary basin of tbe Pampas terminates and sinks under the Atlantic ocean, from the mouth of La Plata to the Straits of Magellan. Proceeding to the north from this last point, its limits, more or less remote from the Cordilleras, are still uncertain ; but every thing leads us to believe, that the deposit of this epoch occupies the plains to the very base of the lateral chains. Advancing still farther to the north, the tertiary basin of the Pampas extends to the foot of the primitive hills of the province of Chiquitos ; and it even appears that it is prolonged on all sides without interruption through these hills, into the great basin of the Amazon. Regarding only the portion situated to the south of the primitive hills of Chiquitos, the tertiary basin of the Pampas extends in the direction of the meridian, from the 17th to the 52d degree of south latitude, over a distance of upwards of 2400 miles. Its greatest breadth is about 800 miles.

Throughout this vast extent, and even at the foot of the northern declivity of the hills of Chiquitos, M. d'Orbigny has distinguished, in the American tertiary deposit, three different series, belonging to three successive epochs, viz. 1. The lower beds, destitute of organic remains, which he designates by the name of the *Guaranian Tertiary Formation ;* 2. A middle portion, evidently marine, containing shells belonging to extinct species, and which he calls the *Patagonian Tertiary Formation :* and, 3. An upper portion, containing only skeletons of mammifera, which he terms the *Pampean Loam* (*Limon Pampéen*). The last is only covered by the deposits of the present period.

The *Guaranian Tertiary formation* is generally composed of three conformable beds. The first consists of ferruginous sandstone, often abounding in nodules of red oxide or hydrate of iron, and in very beautiful agates (*sardoines*) of various colours, whose angles are much rounded. It has a thickness of upwards of 300 feet, where it is most fully developed. The second bed, termed by M. d'Orbigny *Calcaire à fer hydraté*, is a greyish-white argillaceous limestone, filled with harder nodules, which are often very compact, with pebbles of quartz, and with rounded grains of hydrate of iron. Its greatest thickness is about 13 feet. The third bed constitutes the upper part of the Guaranian formation, and is composed of grey gypseous clay, filled with hard nodules. It is of the same nature as the preceding bed, but contains no hydrate of iron, that substance being replaced by a large number of nodules of gypsum disseminated in beds through the clay. Its greatest thickness is about 13 feet. M. d'Orbigny found no fossils in any of these three beds. All of them occur with great uniformity in the province of Corientes ; and are not absolutely horizontal, but, on the contrary, exhibit undulations and other variations. The upper gypseous clays retain the water, and thus there are produced, at the surface, immense marshes and numerous small lakes, which give rise to one of the most remarkable features of the topography of the country. Beyond the great basin of the Pampas, M. d'Orbigny found the Guaranian Tertiary Formation in the provinces of Chiquitos and Moxos, and even between the 12th and 13th degrees of southern latitude, near San Ramon and San Joaquin, and at the fort of Beira. The points where it is apparent in the province of Moxos seem to form part of a horizontal deposit, an arrangement which leads to the belief that the Guaranian formation levelled the inequalities of the surface before the deposition of the Pampean formation which reposes on it.

The second system of tertiary beds, termed by M. d'Orbigny the *Patagonian Tertiary Formation*, occupies a much greater extent than the Guaranian tertiary formation. M. d'Orbigny refers, indeed, to this deposit, the whole tertiary beds of Patagonia, which are marine, but also contain some terrestrial or fluviatile organic remains, transported probably by rivers. He also includes under it the marine deposits of the province of Entre-Rios; and, in comparing these with the deposits of Patagonia, he finds that the two groups are divisible into the following series :—1. A lower portion, composed of marine sandstones, containing extinct species of mollusca. 2. A little higher up, there are in both groups sandstones, in which bones of mammiferæ and pieces of fossil wood are met with. 3. To this succeed, in the north, alternations of sandstone and clay abounding in gypsum, and, in the south, blue sandstones. 4. The upper portion, in the north as well as the south, consists of an alternation of limestones and sandstones containing the *Ostrea Patagonica*; and, reposing on them, marine conglomerates, including in both groups, and thus at a distance of upwards of 600 miles, three identical species of fossils which prove their contemporaneity. There is thus every where an analogy, not only in relative thickness and composition, but also in organic remains; and this similitude has determined M. d'Orbigny to regard the whole as belonging to one and the same epoch.

M. d'Orbigny made many curious observations on the Patagonian tertiary formation; but of these we shall only cite a few. At Ensenada de Ros, to the south of the Rio Negro in Patagonia, one of the beds of sandstone presented a great number of bones, which, however, the hardness of the rock frequently prevented him from obtaining. Among other remains he discovered bones of the *Megamys Patagonensis*, an animal four times larger than any living species of the order Rodentia. These consisted of a *tibia* with its *rotula*, whose relative position with regard to each other was such, as to shew that they must have been deposited while their ligaments still caused them to adhere together. They were found under a mass of marine sandstones, containing shells and beds of oysters, more than 650 feet thick. These oysters all belong to one species, of which the beds occupy a very well-marked region in the province of Entre-Rios, as well as along the whole coast of Patagonia. It is evident that these shells lived together in numbers, and have not suffered any derangement, for they are every where met with in their natural position, and with their two valves united. Judging from analogy, we may believe, M. d'Orbigny remarks, that the basin in which they lived had but little depth, and that the water did not rise more than about thirty feet above these beds of oysters. The oysters, like all the other shells met with in the tertiary beds of the Pampas and of Patagonia, appear to M. d'Orbigny to be different from those occurring at the present day in the same regions. He thinks, indeed, that not one of these fossil species is now to be met with alive. The bones of mammifera belong likewise to extinct species, and even to extinct genera.

The coasts of Chili are fringed, like those of Patagonia, by a tertiary deposit that M. d'Orbigny did not himself examine, but which the notes and the collections placed at his disposal by several travellers have enabled him to describe, and of which he has determined and figured the fossil shells. The fossil species of the tertiary beds of Chili (those from the perfectly modern deposits excepted) no longer occur in a living state on the same coasts. In this respect the tertiary formation of Chili corresponds with the Patagonian tertiary formation ; but it is a very curious fact, that, notwithstanding this similitude, which would seem to refer them to nearly the same geological period, these two formations, although situated in the same latitude, do not contain fossils common to both. Not only is there not a single identical species, but even the series of genera is altogether distinct,—a fact which appears to indicate, that, notwithstanding their geographical approximation, these two formations have been deposited in different seas.

After having compared, in a paleontological point of view, the tertiary formations of the two sides of South America, M. d'Orbigny compares them in the same manner with those of Europe, in order to endeavour to assign them an age in the long series of tertiary periods. The result of this investigation is to establish that the following conditions apply equally to the tertiary formations of the Parisian basin, and to the tertiary formations of the two slopes of the Cordilleras. 1. None of the fossil species are met with in a living state on the neighbouring coasts; 2. None of the species have identical analogues even in remote seas (M. d'Orbigny applies this conclusion to the basin of Paris itself, refusing to admit, along with the greater number of paleontologists, that some of the numerous fossils of that basin have living analogues); 3. The genera, when they occur in the neighbouring seas, are now in regions which are warmer and nearer the equator; 4. A great number of the genera met with in a fossil state are now awanting in the neighbouring seas, and sometimes have even ceased to exist. These different circumstances lead M. d'Orbigny to conclude, that the Patagonian and Chilian tertiary formations both belong to the most ancient tertiary period, whence it would result that they are contemporaneous, *or nearly so.* This last distinction is important, for if it were proved that the contemporaneity of the two formations must have been absolute, it would likewise be necessary to conclude with M. d'Orbigny, that during the period of the deposition of these formations, the two seas where they were formed, must have been separated in the same degree as they now are, viz. those which wash the east and west coasts of America, and which, according to M. d'Orbigny, do not contain analogous shells any more than the tertiary formations do. If, on the contrary, as may be maintained, the facts observed indicate only an approximative contemporaneity, the conclusion relative to the existence of a continuous chain of mountains between these two seas, still leaves room for desiring farther information.

The third of the great divisions which M. d'Orbigny distinguishes in the tertiary formations of South America, the *Pampean formation*, differs essentially from the two series of tertiary beds on which it reposes, in the simplicity of its composition, and, so to speak, in the unity of its mass. It is a great bed of reddish argillaceous earth, generally containing layers of pale brown calcareous concretions. These concretions are hard where they are most compact, and are traversed, as Mr Darwin has already observed, by small linear cavities, which contribute to give them the characteristic aspect of fresh water limestone.* They sometimes become so numerous

* Darwin, Zoology of the Voyage of the Beagle; Introduction, p. 4.

that they unite in such a way as to form continuous layers, or even to constitute the whole mass. The Pampean formation exhibits no distinct stratification; no division into different beds can be remarked; it is in fact only one single bed. It is true there are in certain places portions more or less hard, more or less arenaceous; but these portions, far from being limited by horizontal lines, as we always see in beds tranquilly deposited from water, form a mass in which indistinct zones only can be distinguished, zones which cannot be followed for any distance in any of the natural sections of the *falaises*.

The earthy mass of the Pampean formation, with its calcareous nodules, reminds us of the *löss* of the banks of the Rhine, the *limon* of the plateaus of Picardy, and the analogous deposits observable at some points in the environs of Paris, It is one of the best characterized and best developed examples of those unstratified sedimentary deposits which geologists, following the example of M. d'Omalius d'Halloy, now term *Limon.** It is in this sense that M. d'Orbigny has adopted the denomination of *Limon Pampéen*, which seems to us preferable to that of Pampean clay (*Argile Pampéene*) which he had previously employed. The term *Tosca*, used in the country itself, might have been introduced into science, had it not been already appropriated to designate a deposit of a different composition in the Canary Islands.

The absence of true stratification induces M. d'Orbigny to suppose that the Pampean loam was deposited in a very short space of time, in consequence of a great movement of water. The only fossils found in it are bones of mammifera, which are sometimes very numerous, and of which the largest and the most remarkable belong to large Pachydermata and to gigantic Edentata, accompanied by some Rodentia and a small number of Carnivora. The Pampean loam or *tosca* forms the uniform surface of the great basin of the Pampas, rising gradually from the level of the ocean, towards the north and west,

* The following is the definition given by M. d'Omalius d'Halloy :—" Limon is an argillaceous earth, which is rarely sufficiently pure to be regarded as a true clay. It occurs chiefly in plains and in low valleys."—*Elements de Geologie*, 1835. —EDIT.

up to a height of 300 feet. Its thickness is often pretty considerable. In an artesian well, bored in 1837 at Buenos Ayres by order of Governor Rivadavia, it was found at a depth of nearly 100 feet; beneath, the Patagonian tertiary sands were met with, which afforded abundance of water.

From Buenos Ayres to San Pedro, a distance of upwards of 90 miles, the Pampean bed is seen forming, without interruption, the pretty elevated *falaises* of the Plata and the Parana. The *falaises* exhibit, when the river is low, immense beds, known in the country by the name of *Tosca*. These consist of the same clay, more or less indurated, always cavernous, or filled with calcareous nodules, and containing bones of mammifera. At Santa-Fe-Bajada, on the left bank of the Parana, the Pampean deposit reposes on the Patagonian tertiary formation, which is filled with marine remains. It also forms the right bank, and continues to do so upwards as far as Goya and Correntes. It ceases to present itself generally throughout the plains of Chiquitos, of Santa-Cruz-de-la-Sierra, and of Moxos; but it seems to exist there under the alluvial covering; nay, it probably occupies in these provinces a surface equal to that which it forms in the Pampas. From thence it appears to connect itself on the south with the superficial deposit of the Pampas, and on the north with the upper basin of the Amazon.

The Pampean bed does not present itself solely in the low plains, for beyond the regions explored by himself, M. d'Obigny thinks he recognises it in the lower bed of diluvium which, according to M. Clausen, fills a portion of the caverns of the Province of Minas-Geraës in Brazil. According to M. Lund, the interior of the caverns of Brazil is more or less filled with a red earth, identical with the red earth which forms the superficial bed of the country. This bed, which varies from 10 to 50 feet in thickness, covers indiscriminately, and without interruption, the plains, the valleys, the hills, and even the gentle slopes of the highest mountains, up to a height of 6500 feet. It consists chiefly of clay, containing subordinate beds of gravel, and of quartz pebbles. It is often so ferruginous, that the particles of iron become transformed into a pisolitic mineral similar to that which fills the fissures of the Jura,* where the fact was long ago noticed by M. Brongniart. It is extremely probable that this superficial deposit of reddish earth, which exists also at Rio Janeiro, unites in a continuous manner with the great deposit of the Pampas, from which it differs only in the mixture of quartz pebbles derived from the subjacent soil. M. Lund, on his part, ascribes the red loam of Brazil to a great irruption of water which, covering all that part of the globe, exterminated the beings which inhabited it. Whatever modification this hypothesis may afterwards receive, it seems to us evident, that, at all events, the extension of the Pampean bed over the mountains of Brazil, if it were completely proved, would overturn the contrary hypothesis which consists in regarding the Pampean loam merely as a deposit, formed tranquilly at the mouth of a great river. Now, this extension of the Pampean formation to the mountains of Brazil, appears to us so much the more probable, because those mountains are not the only ones in South America on which traces are met with of the existence of an analogous deposit. The same bed, indeed, presents itself at a much greater height on the flanks of the Bolivian Andes, where it fills small basins at Tariji and at Cochabamba, at a height of about 8400 feet above the ocean, and where it covers the whole great Bolivian Plateau, at a mean absolute height of about 13,000 feet.

The Pampean deposit, occurring in this manner at all heights of the basins, formed of rocks of all epochs, is naturally found in contact with beds of the most different descriptions. On the great Bolivian Plateau, it reposes on the Silurian, Devonian, Carboniferous, and Triassic systems, and likewise on Trachytes; at Cochabamba, on the two first; at Moxos, on the Guaranian tertiary formation; and, lastly, in the Pampas, on the Patagonian tertiary beds. But, notwithstanding this diversity of the fundamental rocks, wherever it is observed, and whatever may be its height, it invariably forms a horizontal bed, and its composition is fundamentally nearly uniform: in the Pampas it is a reddish loamy bed of great thickness; at Chiquitos and Moxos it is nearly identical, and on the banks of the Rio-

* Lund, Coup d'œil sur les espèces éteintes de mammifères fossiles du Brésil, (*Annales des Sciences Naturelles*, t. xi. p. 214 and 230; 1839.)

Piray it is merely mixed with clay ; on the elevated plateaus of the Andes it still exhibits a composition analogous to what it presents in the Pampas; and on the mountains of Brazil, the only difference is, that it contains pebbles.

The fossils which it contains in these various positions are of a not less uniform nature. They are solely bones of terrestrial mammifera. These bones occur in prodigious quantity, and amply compensate in interest the absence of marine remains. By observing with attention the elevated *falaises* of the banks of the Parana formed by the *Tosca*, which is the loam in its most normal and most developed form, various portions of skeletons of large animals are seen projecting from the escarpment, exhibited, as it were, in an immense natural museum. These bones, mistaken at first for bones of giants, have struck the inhabitants of the country for a long period ; and the names of many of the localities of the Pampas, and of the banks of the Parana, have been derived from them, such as *the stream of the animal*, *the hill of the giant*, &c. At a later period, Science noticed the subject. Falkner says that he found in the Pampas *the shell of an animal composed of hexagonal bones*, of which each had a diameter of at least an inch.* The carapace was nearly 3 yards long, and resembled in every respect that of the armadillos, but of immense proportions. As these notices leave no doubt on the subject, here we have, well-ascertained, in 1770, the presence in the Pampas, not only of fossil bones, but also of that cara-

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* Falkner's observations on his discovery are curious, and worthy of quotation. He says: "I myself found the shell of an animal, composed of little hexagonal bones, each bone an inch in diameter at least; and the shell was near three yards over. It seemed, in all respects, except its size, to be the upper part of the shell of the armadillo; which, in these times, is not above a span in breadth. Some of my companions found also, near the river Parana, an entire skeleton of a monstrous alligator. I myself saw part of the vertebræ, each bone of which was near four inches thick, and about six inches broad. Upon an anatomical survey of the bones, I was pretty well assured, that this extraordinary increase did not proceed from any acquisition of foreign matter; as I found that the bony fibres were bigger in proportion as the bones were larger. The bases of the teeth were entire, though the roots were worn away, and exactly resembled in figure the basis of a human tooth, and not of that of any other animal I ever saw. These things are well known to all who live in these countries; otherwise, I should not have dared to write them."—*Description of Patagonia and the adjoining parts of South America. By Thomas Falkner; Hereford*, 1774, p. 55.—Edit.

pace of a large cuirassed quadruped, whose relations with the skeleton to which it belongs have recently given rise to discussions among zoologists. Since 1770, the Pampas have become celebrated by the discovery of the famous skeleton of the Megatherium found at Lujan, which was sent to the King of Spain by the Viceroy of Buenos-Ayres, and was described by Cuvier and M. Garrega. In 1827, M. d'Orbigny collected the fossil bones of several species in the Pampas, at San-Nicolas to the north of Buenos-Ayres, on the Parana and near Bajada, in the province of Entre-Rios. Some years afterwards, Mr Darwin discovered in the Pampas a great number of remains of mammifera, which Mr Owen has described with the greatest care in the "Zoology of the Voyage of the Beagle." Since that time, MM. Tadeo Vilardebo, Bernardo Berro, and Arsène Isabele, found, in 1838, on the banks of the Podemal, one of the tributaries of the Rio Santa-Lucia, in the *Banda Orientale* (Republic of Uraguay), the skeleton of an enormous animal, still provided with its carapace, and to which they have given the name of *Dasypus giganteus.* Lastly, in 1841, M. Pedro de Angelis discovered in the Pampean bed, at a distance of about seventeen miles north of Buenos-Ayres, the skeleton of the Mylodon robustus, which is now deposited in the Museum of the Royal College of Surgeons in London, and which Mr Owen has described in a special work, that has excited, in the highest degree, the attention both of zoologists and of geologists.* There was also found at the same locality an osseous carapace analogous to that of the armadillos, but of gigantic dimensions.

If we follow the Pampean deposit beyond the Pampas, we find that the valley of Torija, situated in the south of the republic of Bolivia, in the last eastern lateral chains of the eastern Cordillera, has for a long time been quoted as a locality of fossil bones. This valley forms a small basin furrowed in the east by a water-course, and it is on the banks of the latter that an immense number of bones occur in a gravelly loam, in which the animals seem to be nearly entire. M. d'Orbigny has ascertained the occurrence in this deposit of the *Mastodon Andium* of Cuvier, and he thinks that he can refer to the same

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* Owen's Description of the Skeleton of the *Mylodon robustus,* 1842.

bed the remains which Humboldt noticed in other parts of the Andes. It is known that that illustrious traveller collected in 1802, on the plateau of Quito, teeth of elephants and mastodons, which were examined by Cuvier. Probably the teeth brought home by Dombey the traveller, came from the same localities. Humboldt likewise discovered the teeth of *Mastodon angustidens* near Santa-Fe de Bogota, in Columbia, and bones of elephants at Cumanacoa, near Cumana. Bones of the elephant have not hitherto been found in the Pampean formation, but Mr Darwin discovered in this deposit, near Santa-Fe Bajada, bones of mastodons associated, and this is a curious circumstance, with bones of the horse. Previously, M. Auguste de Saint-Hilaire had sent to the Museum of Paris a tooth of a mastodon, collected at Villa do Fanado, in Brazil.

MM. Clausen and Lund have subsequently examined the caverns of the province of Minaes Geraës, and have collected a considerable quantity of bones of quadrupeds. The number of species already distinguished by them amounts to more than 100. They seem to have belonged to the same fauna as those whose remains occur in the Pampean deposit, for the identical species of the genera *Megalonyx*, *Megatherium*, *Holophorus*, and *Mastodon*, present themselves simultaneously in the Pampas, and in the caverns of Brazil into which the Pampian mud has penetrated, and whose entrances it surrounds. This circumstance is so much the more remarkable, because it is a distance of more than 1200 miles from the province of Minaes Geraës, where these caverns are, to the *falaises* of the Parana, near San Pedro, which are the richest in fossils, and because this same loam occupies an extent of surface on the Pampas, chiefly to the south-west of the Parana, which of itself is nearly as great as the half of France. This fact harmonises with many others, tending to shew, that the continent of South America is fashioned on the great scale; and that, in order to explain its origin, we can only call in the aid of simple and great causes.*

To be concluded in our next Number.

* *Comptes Rendus de l'Academie des Sciences.* The Report was given in to the Academy on the 28th August 1843. The Committee consisted of Messrs Alexandre Brongniart, Dufrenoy, and Elie de Beaumont; the last-mentioned being the reporter.

Fables and prejudices regarding Serpents. By Dr H. Schlegel.*

The serpent performed a grand part in *antiquity*, and still plays it among most barbarous or demi-civilized nations. Numerous causes have been assigned for this phenomenon. Man, intimidated by his aversion for these animals, which is in him in some degree innate, has only learnt from experience, how small a number of these reptiles are formidable by their poisonous qualities, while others conceal, under the same delusive appearances, a mild and inoffensive character.

A thousand different properties, which are successively detected in serpents, have opened to man a vast field of meditation, and, in furnishing ample materials to dress out his religious ideas, have presented him with an infinite number of mythic allegories. He has drawn from them symbols, and has ended in offering to those dreaded animals a worship founded on the most diverse and conflicting motives. It would seem to be natural to man to avail himself even of the animals which are noxious, for procuring the means of preservation from the evils which they cause: hence the practice, established from the most remote times, of extracting from serpents remedies against their bites; while, on the other hand, man sought to appease their fury by revering them as divinities. The ancients, employing often the most prominent characteristics of animals in their allegories, discovered, in the habits of serpents, in their qualities, or even in their form, an inexhaustible fund for setting to work their own fertile imagination, which heated itself invariably in embellishing the observations they had made from nature. It is to these various causes, and to circumstances perhaps little known at this time, that we should attribute the fear, mingled with hatred and veneration, with which the serpent has inspired the human race.

In the mythology of most ancient nations, there are traces

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* From Dr Traill's excellent translation of Dr Schlegel's valuable "Essay on the Physiognomy of Serpents," about to be published. This work we recommend to the student of Ophiology.

which attest that the idea of the serpent as the *evil principle* prevailed from the most remote antiquity. The serpent is represented as the cause of the first transgression and fall of man; and Arimanes, assuming the form of a serpent, seeks in vain to overcome his antagonist Orosmandes, who represents the good principle in the idealism of the ancient Persians.

It is believed that the ancient Greeks made choice of the allegory of the great serpent killed by the arrows of Apollo to represent the pestilential vapours, emanating from the marshy slime which covered the earth after the deluge, or after annual inundations, and which could only be dissipated by the rays of the sun; afterwards, this Python became the attribute of Apollo and his priestesses at Delphi, and it subsequently served for the emblem of Foretelling and Divination. Analogous circumstances probably gave rise to the fable of the Lernæan Hydra, exterminated by the labours of Hercules and his companion Iolas. Among the ancient Egyptians, the serpent was the symbol of fertility. They represented, under the form of a serpent, inclosed by a circle, or entwined around a globe, the Cneph of their cosmogony, who is the same as Ammon, or the Agathodemon, the spirit or soul of creation, the principle of all that lives, who governs and enlightens the world.* The priests of that people kept in the temples living serpents; and when dead, interred them in those sanctuaries of superstition.†

As an emblem of Prudence and of Circumspection, the serpent was the constant attribute of Æsculapius, and the same veneration was paid to those reptiles, as to the father or the God of medicine and magic.‡ The Ophites were Christian sectaries, who, towards the second century of our era, established a worship which was particularly distinguished from that of the Gnostics in this, that they adored a living serpent; conforming themselves to the ancient traditions of their race, they regarded that animal as the image of Wisdom, and of

* Eusebii, Pred. Evang., 33; Horopollo, ap. i. 2; Creutzer, Symb. L. i. 507 and 824.

† Ælian, xvii. 5; Herodotus, ii. 74.

‡ Pausanius, ii. 26–28.

the sensual emotions which it awakens.* The monuments of the Mexicans, of the Japanese, and of many other nations who owe the foundation of their civilization to the ancient inhabitants of Asia, attest that the serpent played also a part more or less important in their religious mysteries; but time and the relations which exist between those nations and Europeans, have partly abolished these usages; and at this day it is only among negro tribes, and on the west coast of Africa, that the serpent figures among divinities of the first rank.

It does not enter into the plan of my work to explain or even to allude to the numerous allegories which the serpent represented among the ancients. Every one knows that the snakes armed the hand of Discord, no less than the whip of the Furies, and that the head of the Eumenides bristled with serpents; the two snakes twisted around the caduceus of Mercury is the type of insinuating eloquence; the circle formed of a snake biting its own tail, without beginning and without end, was the chosen symbol of eternity; the celerity of movements uniformly repeated to execute progressive motion, became the emblem of the swiftness of time, and the succession of the infinity of ages; the fables, lastly, of Achelous, of Jupiter metamorphosed into a serpent to captivate the object of his love, and many others, attest that the ancients attributed to the serpent qualities the most opposite, and that the same being, according to them, united at the same time force with timidity, beauty with a shape which inspired horror, mildness with cunning or deceit.

We ought to attribute to causes similar to those we have mentioned, to that superstition—an inheritance of human nature—the innumerable errors which, even to our times, have disfigured the history of serpents. A vast number of those fables, invented in the infancy of the human race, and transmitted to posterity by classic authors, are spread abroad so as to acquire popularity from the authority which is accorded to those writers. To prove this assertion, it is sufficient to recollect

* Mosheim, Gesch. der Schlangenbr. p. 1.

what several modern authors have repeated in their works, that hogs kill snakes to feed upon them, and that serpents find in milk a great dainty; errors which date from the times of Aristotle* and Pliny,† but propagated in Europe, in America, and other parts of the world. We read in the same authors,‡ that the ichneumon, to defend itself against the bites of snakes, bedaubs itself with mud, and that it eats a certain herb which those reptiles hold in aversion. This prejudice, which rests on the simple fact that the little mammiferæ we speak of, as well as many others, are the natural enemies of serpents, is preserved in various parts of the East Indies. The plant which possesses the virtue of repelling snakes, according to Kaemfer,§ is the *Ophiorhiza Mungos*, according to others, the *Aristolochia indica*, which the jugglers of those countries pretend to use with success; but the experiments of Russell ‖ have demonstrated that all these qualities repose only on popular prejudices. The same holds good with regard to the employment of the *Polygala Senega*¶, a plant celebrated among many tribes of North America; while other nations reject it, to make use of plants of the genera *Prenanthes*, *Lactuca*, *Helianthus*, *Spiræa*, &c., the efficacy of which, as antidotes against the poison, are as little proved as that of the former. Modern travellers of great name have furnished some curious facts relating to a plant,** to which the inhabitants of Colombia attribute the same qualities as those ascribed to the Aristolochia in India; but it is much to be desired that these experiments were repeated by persons familiar with the nature of serpents.†† It will be superfluous to repeat all that the ancients have invented concerning the innumerable antidotes of which

* Hist. Anim., ix. 2.
† Hist. Natur. viii. 14.
‡ Aristotelis, ix. 7. Plin., viii. 36.
§ Amœnitates Exoticæ, i. p. 305.
‖ Indian Serpents, i. p. 86.
¶ Palisot Bauvais, Ap. Latreille, iii. p. 90.
** Plantæ Equinox. ii. pl. 105.
†† [The author perhaps is not aware of the curious experiments on the rattlesnake with the leaves of the Fraxinus Americana, by Judge Woodruffe, published in Silliman's Journal for 1833.—*Tr.*]

they vaunt the efficacy. On consulting the passages of Pliny* to which we refer, it will be seen that the ancients recommend indiscriminately, for this purpose, the most heterogeneous substances; but that the attempts which they made were the result of the grossest empiricism. Deceptions of this nature are practised in India and Ceylon, where they sell pastilles and pills of different kinds, arbitrarily composed of substances from the vegetable, animal, and mineral kingdoms, and which merely act on the imagination of the sufferer.†

We have stated above, that the practice of extracting from serpents the remedies against their bite, dates from remote antiquity: Antonius, physician to Augustus, employed vipers in several diseases;‡ but it was not until the time of Nero, when the physician Andromachus of Crete,§ invented the *theriaca*, that the practice became general. The theriac was an arbitrary compound of heterogeneous medicaments, and was afterwards employed in maladies of the most opposite nature: it was compounded in the middle ages in almost all the cities of Europe, particularly in its southern parts: at this day, the practice of including the snake in the composition of this medicament is only retained in Italy, where the theriac is still made in various places. In Sicily it is prepared at Palermo. That of Venice is very celebrated: there they use millions of the Vipera aspis, which is common in the vicinity of that city.|| The great manufacture of theriac which exists at Naples, under the protection of the government, is a private speculation, at the head of which stands the learned Professor Delle Chiaje; there they use indiscriminately every species of serpent, although they prefer the vipers named *viperiere* by the peasants, who bring them alive in baskets. M. Siebold assures me that they frequently employ a species of theriac in China and Japan; the inhabitants of the Lioukiou Isles extract medicaments from the Hydrophis colubrina; and

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* Hist. Natur., 28, 42, 29, 15, 17, 20, 21, 22, 25, 26, 32, 17, 19, &c.

† Russell, i. p. 74; Davy, Ceylon, p. 100.

‡ Plin. 30, 39.

§ Galen, de Antidotis, lib. i. cap. 6.

|| MS. note communicated by the late Dr Michahelles.

at the Isle of Banka, the Chinese reckon the bile of the great python a precious remedy against many diseases.* I pass over the use made in the middle ages of different parts of the snake, to each of which was attributed salutary qualities ; in our days they are wholly laid aside.

It is only in recent times that those experiments have been instituted on the effects of the bites of snakes, which we have related elsewhere: the ancients, as many people still do, reputed indiscriminately all serpents venomous ; they placed the seat of their deadly weapon in the tongue, or in the end of the tail, and ascribed to the bite of each species, according to their fancy, a different train of mischiefs.† Civilization is unable to destroy these errors, and one is astonished to hear them repeated by well-informed persons ; to see republished in several works the story of the three sons of a colonist, successively dying at long intervals, of a wound caused by the fang of a rattlesnake remaining in the boot of their father, who had first died of the bite : a story which the inhabitants of Surinam, as well as those of the United States, are pleased to repeat to strangers passing through their country. One is astonished to hear of sea-snakes of monstrous size ; of boas from forty to fifty feet long that attack men, oxen, tigers, and swallow them whole, after having covered them with a frothy saliva : absurdities that bring to recollection those fables of winged monsters or dragons, of which the mythology of the ancient people of Asia has preserved the remembrance, and of which the wayward fancy of the Chinese has multiplied the forms. What shall we say on reading in modern works of great reputation, descriptions of the marvellous effects produced on serpents by music ; when travellers of talent tell us they have seen young snakes retreat into the mouth of their mother, every time that they were menaced with danger ! Unfortunately naturalists, in classing such fables with the number of facts, have often embellished with them their descriptions, and thus have contributed to give them universal acceptation.

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* Olivier, Lund en Zeetogten, ii. p. 447.

† See Lucan, Pharsalia, ix. 937 ; Nicander, de heriaca.

Who, for instance, will not be struck with the description which Latreille and Lacepede have drawn up of the habits of the boa, and of other serpents of great size! How many qualities have not these philosophers attributed to those beings, which have never existed, except in their own imaginations!

Every one has heard of the pretended magic power which serpents are said to exercise over small animals, when they wish to catch them: there are few works on natural history which have not treated of this phenomenon, contradicted by some, and defended by others, without their being able to arrive at a satisfactory conclusion. I shall hot here repeat the absurdities which travellers have written on this head, and which are sometimes extremely curious:* suffice it to say, that these tales, of which the traces may be found in several classic authors,† are particularly in vogue in North America, while they are unknown in the East Indies and in Europe, countries rich in serpents of every species. This observation is too curious not to merit some attention, as it shews how a fact, true or supposed, may be so spread as to become popular. Many causes might have given rise to the origin of the pretended *power of fascination* of serpents. It is true that most animals appear absolutely ignorant of the danger which menaces them, when they find themselves in the presence of enemies as cruel as serpents; we often see them walk over the bodies of those reptiles, pick at their head, bite them, or lie down familiarly beside them: but we need not also deny, that an animal, unexpectedly surprised, attacked by so formidable an adversary, seeing his menacing attitude, his movements performed with such celerity, may be so seized with fear, as, at the first moment, to be deprived of its faculties, and rendered incapable of avoiding the fatal blow, which is inflicted at the moment when it perceives itself assailed. Mr Barton Smith, in a memoir expressly written to refute all that has been advanced on the fascination of the rattlesnake, re-

* See Levaillant 2de Voyage, i. p. 93; Barrow, Trav. p. 120.

† Ælian, ii. 21; Pomponius Mela, i. 19.

lates several instances which prove that birds do not shew themselves afraid, except when the serpent approaches their nests to seize their young. Then one may see the terrified parents fly around their enemy, uttering plaintive cries, just as our warblers do when any one stops in the vicinity cf their nests. It may also be, that the animals which it is pretended had been seen fluttering around the snake, and at last falling into his mouth, have been already wounded by his poison-fangs; a supposition which perfectly corresponds to the way in which venomous serpents master their prey. Many tree-snakes seize their prey by twisting their slender tails around their victim: Dampier* has several times been a witness of this spectacle: observing a bird flapping its wings, and uttering cries, without flying, this traveller perceived that the poor bird was locked in the folds of a snake, when he attempted to lay hold of it. Russel† presented one day a fowl to a Dipsas, and the bird in a short time gave signs of death; not conceiving how the bite of a snake not poisonous, and so small, could produce such an effect, he carefully examined the fowl, and found the folds of the tail of the snake around the neck of the bird, which would have perished, had he not disengaged it. Many birds of small size are accustomed to pursue birds of prey, and other enemies of their race, or to fly about the place where the object of their hatred lies concealed: there is reason to believe that this phenomenon, known in Europe to every observer, also takes place in exotic regions; and perhaps this is also one of the circumstances which have contributed to the invention of the stories which have been related of the power of fascination in serpents.

But I have too long interrupted the progress of my work, in exposing the numerous errors which have disfigured one of the most beautiful parts of natural science; and I believe I ought to omit the fables concerning the basilisk, the hybrid snakes produced by the congress of eels and serpents, and the other tales as strange as absurd, which are still believed by many persons. Yet, before terminating this division of my

* Voyages, iii. p. 275.

† Russel, i. p. 20.

work, I shall notice the magic power which certain persons pretend to be able to exercise over snakes. This pretended art, which formed at all times, and among various nations, the occupation of a particular caste, consists in certain tricks which the serpents execute at the will of the *conjurors*, who have trained them expressly for the purpose: as they chiefly make use of the Naja tripudians and Naja haje, I have, in these two articles, stated the manner in which they employ serpents in those tricks.*

Such conjurors exist now in the Indian Peninsula, and in Egypt;† those of the latter country boast themselves to be the descendants of the Psylli,‡—a tribe who inhabited ancient Lybia and India, and were celebrated for their skill in curing the bites of snakes, and securing themselves against them. Another people inhabiting Italy, but less known, were the Marsi;§ we know still less of the Ophigenoi, whose country was Greece.‖

Among the more civilized people of Europe, persons who pretend to possess the art of fascinating serpents, are very rarely to be met: they consist most frequently of ignorant charlatans, who impose on the lower orders, seeking to alarm them by playing familiarly with serpents, while they are only thus familiar with the innocuous. M. Lenz has given in his work¶ the history and tragic end of one of those pretended conjurors, who paid with his life for a temerity, founded on absolute ignorance of the nature of vipers.

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* [The author here alludes to the descriptive part of his work, not yet translated.]

† Geoffroy, Descrip. del Egypte, xxiv., p. 88.

‡ Plin. vii. 2; Ælian, 16, 37, 17, 27; Lucan, ix. 891. Consult also the paper of Mr Spalding, entitled Uber die Zauberei durch Schlangen, and inserted in the Memoirs of the Academy of Berlin, 1804–11, Historico-philosophical Class, p. 9.

§ Virgil, En. vii. 750; Silius Italicus, viii. 495.

‖ Plinius, vii. 2; Ælian, xii. 39.

¶ Page 192.

Notices of Earthquake-shocks felt in Great Britain, and especially in Scotland, with inferences suggested by these notices as to the causes of the Shock. By David Milne, Esq., F.R.S.E., M.W.S., F.G.S., &c. Communicated by the Author.

(Continued from Vol. XXXV. page 159.)

Having now fully detailed the phenomena attending the shock of 23d October 1839, and suggested the inferences as to the probable cause of such shocks, which these phenomena seem to warrant, we shall proceed to describe shocks of a subsequent date, offering also, in regard to them, such remarks as may be suggested by the effects observed.

8th January 1840.—The shocks under this date, as felt at Comrie, appear from the Register kept there* to have been very slight.

On the same day, about 10 p. m., a shock was felt in Ireland, which, perhaps, it is not out of place to notice. An account of it was transmitted by Mr Paterson of Belfast, from which the following extracts are made. "The shock was most felt in the barony of Innishowen. This barony, which is the northern extremity of the county of Donegal, forms a peninsula, lying between Lough Foyle on the east, and Lough Swilly on the west. The sound by which the shock was accompanied, was heard in the parish of Cloncha, 20 miles north of the city of Londonderry; and also in the several parishes of Moville, Culdaff, Donagh, Clonmany, and Fahan. The duration of the shock is estimated at about 20″. The vibration of the ground was very sensibly felt, especially in high and rocky situations, and but slightly in low and marshy grounds. It is compared to the kind of tremor caused by the passing of a loaded cart. No building of any kind was injured by it, nor was any wall cracked or thrown down. It seemed to proceed from E. to W. or from NE. to SW. The shock at Malin is stated to have been 'heaviest' at the commencement and termination; perhaps, by a well-known law of mo-

* See page 85.

tion, the concussion would appear greatest at these times, if of the same actual intensity throughout.

"The noise by which the shock was accompanied, seemed on the surface of the earth, or immediately beneath it. In another locality, a crackling noise was said to have been heard in the air, and was compared to that which occasionally accompanies the aurora borealis. By some, the noise was mistaken for thunder, a supposition at once contradicted by the cloudless aspect of the sky. One gentleman states, that, like the sound of a passing coach, it increased in loudness, and gradually died away. He estimated its duration at 3′. To another, it seemed less than 1′. That the comparison of the sound to that of the firing of cannon at sea, or the rolling of distant thunder, conveys a good idea of its character, is proved by the fact, that it was taken by several people at Culdaff for the firing of cannon by a vessel in distress off the Pollard Strand, which is situated to the westward of that place. The night was beautifully clear. All agree in describing it as a fine starlight night. The weather for some time, both before and after, was unusually fine for the month of January.

"Persons who had, during a residence abroad, experienced a similar sensation, at once pronounced it to be the vibration caused by an earthquake. One lady near Moville, when she felt the motion of her seat, turned pale with affright, and dropped from her hand a book she had been reading. At Goory, the residence of John Harvey sen. Esq., the drawing-room windows were made to rattle. In one instance near Malin, a tea-tray, suspended on a nail, was thrown down. In a place six miles SW. of Malin, some flags in a kitchen floor were shaken considerably, and moved from their places."

18*th January* 1840.—The Comrie Register indicates two shocks on this day; the one at 9^h 45′ P. M., the other at 10 P. M.

Mr Colquhoun of Clathick, a place about three miles east of Comrie, happened to be at home, and, within a couple of hours after the shock, wrote a short account of it, as felt in his house; from which the following extracts are made:

"All in the room felt it come, from west to east. We were sitting round the fire, and were alarmed by a very smart shock. The little dog started up terribly confused. The vibration was very rapid, and as if running along sharply under the floor, making the things shake a little. On the second occasion, there was only a single report, with but a slight movement. I looked at the barometer about five minutes after the shock, and did not find it lower than it had been an hour previously."

Mr Colquhoun accompanied his letter with some extracts from his Meteorological Register, which prove that the weather, both at and before the time of the shock, was marked by peculiar wetness. These extracts are as follows:

"1840, *January* 11.—Fog all day with some rain. Rain *all night.*

"12*th.*—Tremendous gusts of wind from SW. with heavy storms of rain. Some rain fell during the night.

"13*th.*—Thermometer 45° (north aspect). Gloomy all day, with high wind, and some showers from SW.

"14*th.*—" Thermometer 44°. Fine, clear, *mild* morning. Towards evening again, showers, with high wind from SW.

"15*th.*—Warm, oppressive feeling in the air. High SW. wind at 11 A. M. Heavy showers of rain and high wind till evening; and *rain all night* from west.

"16*th.*—Wind came round to NW. bringing showers of snow on the hills and rain below. P. M. high NW. wind, with storms every now and then.

"17*th.*—Slight frost in latter part of the night. Fine and clear sunshine all day. Wind from NW. Halo round the moon. Rain came on before midnight.

"18*th.*—A little fresh now on the hills. Wind changed to SW. about 9 P. M., becoming quite *muggy* and warm; coming from SW. in terrible gusts, and drenching *storms of rain all day*, with hardly any cessation. Wind increased in violence till sunset, then lulled for half an hour; recommenced at 7 P. M. with greater force from nearly due west, and blew almost a hurricane, till 20 minutes before 10, when the first shock took place. Wind unabated, but veering round to the north. Deluges of rain beating against the house all night, but with occasional glimpses of clear moonlight.

"19*th.*—The gale continues, and is more severe, with heavier storms of rain than we have had this season. 10 A. M.

wind from NW.; a little fresh snow on the hills. 12 A. M. wind still blowing a severe gale from NW.

"Walked to church to-day quite dry-footed, notwithstanding all the rain which has fallen,—a strong proof of the porous nature of the soil here."

7th April 1840.—On this day, as mentioned in the newspapers, "a powerful shock was felt in CRIEFF, the motion of the earth being from west to east. It was accompanied by a rolling hollow sound, which resembled distant thunder. At Comrie, it was severely felt at the same time; and in the neighbourhood of that place, it caused several bells to ring."

15th September 1840.—On the evening of this day, as a correspondent at NEW ROSS, in Ireland, writes, that town "was visited with a dreadful gale of wind. About 7 P. M. it turned to a hurricane; a few minutes after that hour, there was felt the shock of an earthquake. I saw the candlestick on my table tremble, and the light appeared to emit a faint blue colour. At this time a gust of wind came on, with such terrific violence, that windows were blown in, chimneys were prostrated, while two doors fastened with iron-bars were rent asunder in the timber-yard of James Galavan. The sheds were stripped of slates. The shock which the inhabitants of New Ross experienced, will never be obliterated from their recollection."

26th October 1840.—The instruments at COMRIE were moved by the shock mentioned in the register. The one meant to indicate shocks from below, was depressed three quarters of an inch, indicating that the earth's surface had been raised to that amount. Another instrument, on a different principle, but also constructed so as to be affected only by the same kind of shocks, indicated an elevation of the ground to the extent of half an inch.

In regard to the amount of *horizontal* movement, as measured by an instrument on the principle of the ordinary pendulum, the lower end of it made a rut or furrow in flour half an inch from the centre, and in a direction W. by N.

From these data, it may be inferred, that the shock had come upwards to the earth's surface, at Comrie, in a direction W. by N., and at an angle of from 45° to 60° with the horizon.

31*st January* 1841.—This date is noticed, merely because, on the morning of that day, and almost at the same hour, a shock was felt at Comrie and in Wales. At Comrie it occurred about 2 A. M. At Carmarthen, if the account in the newspapers is to be trusted, it occurred "between 3 and 4 o'clock A. M. It was a smart shock, accompanied by a very visible tremor of the earth, and a rumbling noise similar to the sound of distant thunder." Several other towns in Wales are mentioned, where a shock was felt at the same instant. It is added, that "similar shocks were observed about the month of last November in the neighbourhood of Llanstephan."

10*th March* 1841.—At COMRIE, the two inverted pendulums, though of different lengths (the one 30 inches, and the other 128 inches), had their points thrown to the west half an inch.

22*d March* 1841.—At COMRIE, these instruments were again affected, as on the 10th, though not quite to the same extent.

19*th and* 21*st April* 1841.—On these days, shocks occurred at and near OBAN, in Argyleshire, which do not appear to have been felt at Comrie.

On the first of these days, they occurred at $5^h\ 30'$ A. M., 11^h A. M., and $2^h\ 30'$ P. M.

On the last mentioned day, "the shock occurred at $1^h\ 35'$ A. M., and was felt severely at the Lismore Lighthouse. The watcher there was greatly alarmed, by a loud noise resembling that of a cannon discharged at a short distance. The noise was accompanied with a shaking of the lighthouse tower,—the top of which is 103 feet above the sea. The tremor was such as to cause the reflector frame and glasses to tingle. The people at the bottom of the lighthouse heard the noise, but were not sensible of the tremulous motion. The Ferry-house at Connal (which is about nine miles east of the lighthouse) was rent by the shock. No effect was produced on the barometer. At the time of the shock, there was a smart breeze blowing from the north."

4*th July* 1841.—The shock under this date does not appear in the Comrie Register. The only place from which any account was received describing its occurrence is KINLOCHMOIDART, in Argyleshire, situated immediately to the north of the point of Ardnamurchan. Mr Robertson says of this shock, in a letter written next morning,—" We felt a slight shock, accompanied with a rumbling noise, last night about 9^h $30'$. It was much slighter than the one felt here in October 1839."

23d, 25th, and 26th July 1841.—Of these dates, shocks were felt at Comrie. Mr M'Farlane writes:—The first "occurred about 1 A. M.,—say 2 on our scale of intensity. The second occurred at 4^h $45'$ P. M.,—3 of our scale. The third was this morning about 3 o'clock, said to be also pretty loud. Your instrument in my garden (to indicate a vertical upheave of the ground) was moved by one of the last or both (I cannot say precisely, not having looked in the interval) nearly one degree or half an inch. The long one in the steeple was also inclined, when I looked at it this morning, about the same distance from its centre to the west. My own dip one had its index depressed about half an inch, and vibrated east and west, from its point of rest, nearly the same distance. The quake yesterday, I heard distinctly. A few seconds after the shock, the window-sashes of the room where I was, quivered audibly. The weather has been remarkably mild and warm for some days back. Neither the barometer nor thermometer seemed to be sensibly affected."

From these data furnished by the seismometers, it is evident that at Comrie there was, on the 26th July, an upheave of the ground from the westward, caused by a force which acted from below in a direction making an angle with the horizon of 40° to 60°.

30th July 1841.—On this occasion also, the seismometers were affected. Mr M'Farlane reports, that the two inverted pendulums in his house vibrated to the extent of half an inch, and in a direction south and north, which is different from previous indications. At Tomperran (about one and a half mile east of Comrie), an instrument on the principle of the common pendulum, vibrated east and west. The instruments for shewing vertical movements, were but slightly affected.

Notwithstanding the slight nature of the effects of this shock on the instruments, Mr M'Farlane reports, that it was very severe, though not so violent as the one which occurred in October 1839. Reckoning this former one at 10, he says, the shock of 30th July 1841 may be reckoned 8. He adds, that "it was distinctly *double*, the latter part, if any thing, more severe than the first:—the noise and shake *awful*, at least I felt them so in the house, and those out of doors gave the same account. It is difficult to account for the smallness of its effects on the instruments. Perhaps the vibrations of the ground, though violent, were short and frequent, and thereby interrupted the natural swing of the pendulums. I recollect some person, on the occurrence of one of the former severe earthquakes, describes his feelings as if on horseback when the animal shakes itself. Somewhat such were my impressions on the 30th, even before I had looked at the instruments. It is said here, that there were twelve shocks that day. I felt nine myself. There was one about 8 A. M. pretty smart, and none else till the great one about 2½ P. M. *Immediately* after it, there were two or three slight shocks, and about an hour afterwards a loud one. The weather was cold and inclined to stormy, about the time of the severe shock, and for a day or two before and after."

In a subsequent letter, dated 9th August 1841, by which time Mr M'Farlane had visited different places in and near Comrie affected by the shock, he gives some details, which it may not be out of place here to notice.

A house at Garrichrow (about two miles west of Comrie), "was so severely handled, that three out of four chimney-tops will require to be rebuilt or repaired, and there is a rent in the west gable of the house." "A man from Comrie, who happened to be working at the time on the hill behind, describes the shock as awful indeed, and he says the trees around him were so much agitated, that he thought they would have been torn up by the roots; but he cannot remember exactly in what direction they waved, but thinks it was E. and W. The wall of one of the houses of Ross (the neat suburb of our city) was rent, and the miller's house (you'll recollect it), in spite of its numerous abutments, has had its rents much enlarged. This shock, I learn, has been felt as far east, at least, as Newburgh, about thirty-eight miles from Garrichrow, as far to the west as Dalmally—the distance of which I do not exactly know (but probably it may be about the same)—as far north as Glenlyon, thirty miles, and south as Alloa and Stirling, from twenty to thirty miles."

"I have since seen several of the shattered buildings: one of the chimney-tops in Dunira House is slightly rent; one in the stables behind, very much; two of the gardener's cottage behind that again, considerably; one so much, that it was taken down, and rebuilding when I saw it. The directions in which the stones have been moved, seem to have been various." "It is remarkable that neither the *row of chimney-stalks on the hot-house wall, of the same construction* with those of the gardener's cottage (octagonal), nor those of the game-keeper's house, which is a little up the hill, have been injured. The gardener told me the foundation of all the buildings thereabout (except the last) was upon a gravelly soil, but how near the subjacent rock he could not guess. A person who happened to be on the hill or rising ground immediately to the west of Dunira House, told me, as a proof of the shock having come even there from the west, that after the shock passed him he *heard* the rattle of the slates, and of the building about Dunira. But this you'll at once see was not evidence of the alleged fact. Another, standing on the hill above, said, that he thought he *saw* the disturbance or wave of the woods pass eastward. This was near Comrie. Those in the wooden shade, at the saw-mill near Dunira, saw the roof *open* for a moment; and when they rushed from the shade, they observed that the water in the mill-lead was for a short time dammed backwards, and raised about four inches above its former level at that place; and from this and other indications they judged that the earth there was raised directly upwards *about six inches.* There were traces of electricity in the clouds at the time, and other peculiarities in the appearance of the sky, but nothing amounting to the least hint, so far as I could judge, that we were to be so roughly handled. I recollect before, of noting the appearance of the sky as lurid and particularly sombre when we had quakings below; but I have frequently since, seen the same or even stronger marks of the same kind, and yet all pass peaceably off; and, on the other hand, earthquakes, when the sky was clear and open. Even a course of previous wet weather, which, from its being hitherto an almost constant forerunner of the violent and frequent shocks, warranted the inference that they were somehow connected, does not seem to be a *sine qua non;* so that the remark of a facetious old man, once well known here as 'Deacon Reid,' will hold good, who, long ago, in the first series of our earthquakes, had been paying particular attention to the phenomena, and being asked if he had made out whether they affected the weather or the weather them, replied, that he had attended particularly to that point, and all that he could make of them was, that there was 'aye *some* kind o' wather when they happened.' I may here add, as it was omitted in its proper place, that the only difference I could observe in the circumstances of the *shattered* chimneys about Dunira was, that they were all on walls or gables, running S. and N., while those untouched, had the walls on which they stood E. and W. Dykes were thrown down in many places."

Some of the facts mentioned in Mr Macfarlane's letter, and especially as to those walls which were rent and those which escaped, can be at once explained, when regard is had to their situation. The spot from which, as already shewn, the shocks in the Comrie district emanate, is situated about a mile or half-a-mile to the north or north-east of Dunira; and, therefore, shocks affecting the surface of the earth at Dunira, would crack walls running north and south, by lifting one portion of them before the other,—but could produce no such effect on walls running east and west.

West of Dalchonzie House (situated W.SW. of Comrie about 2 miles), the following effects were perceived by David Robertson, labourer in Comrie, who related them to me. He stated that he was mowing hay, at the above place, and had laid himself down on the ground to take his dinner or rest himself, about 2½ P. M. There were two thumps in the earth, with an interval of a moment between. He felt himself heaved up each time, and he saw the trees shake. There was a rake lying before him within a few feet; which, on the occasion of the first thump, was thrown up off the ground 8 or 9 inches,—and on the occasion of the second thump, 4 or 5 inches. The rake was not thrown up perpendicularly, but a little to the south. There was a sound with the shock, which went south also.

At *Crieff*, the effects of the shock are thus described in a paragraph which appeared in the newspapers published a few days afterwards:

"On the afternoon of Friday last, about half-past two o'clock, Crieff was visited with two fearful shocks of earthquake in close succession. Each was accompanied by a prolonged hollow sound, the motion of the earth being from west to east. Every house in town shook severely, and furniture was displaced. The inhabitants flocked to the streets in great numbers, being greatly afraid. It was said that this shock was *nearly* as powerful as the one felt here on the 23d of October 1839. It was likewise felt severely at St Fillans. The same day a shock was felt at Glenlyon and adjacent places, about three o'clock P.M. It seemed there to proceed with great rapidity from east or north-east to west. The loud noise resembled exactly that of a heavy coach driving furiously upon hard ground, so much so that in many places people ran out to obtain a sight of it. Some declared they felt considerable tremor of the earth, but this was not generally the case. It is said that a slight shock was also felt on the same day about noon."

A correspondent in the neighbourhood of *Dunkeld* writes:—"The shock was very sensibly felt here, as well as at Logierait and neighbourhood. A hollow, growling sound, somewhat like distant thunder, was heard some minutes after two P. M., as if proceeding from nearly north-west to south-east. The houses shook, the windows rattled, and the chairs, &c. in some instances, danced in their places. I myself felt as if the chair I sat on, and the floor, had suddenly sunk some inches; and there was an undulatory motion for about four or six seconds, which I cannot describe. The sound continued some seconds longer. The barometer was about 28½ at the time."

At *Aberfeldy*, the earthquake was felt about half-past two o'clock P.M. It appeared to proceed from the south-west to the north-west, and continued a few seconds.

Farther information having been applied for from the neighbourhood of Aberfeldy, the following particulars are given from the letter of a correspondent in a parish immediately adjoining. He says, that the most prevalent impression was, that the concussion came from the south or south-west—that some persons "felt a sort of undulating motion as if they were sailing in a boat, and others felt the earth heaving from below."

At *Aithrey Castle*, near Stirling, the shock produced "a rumbling noise as of chairs or tables drawn along the floor of the room immediately above."

At *Glenfinnart* in Argyleshire, the shock was felt in the manner described in a letter from Mr James Dennistoun, advocate, who was on a visit there. "I had just left the lunch-table, the party remaining round it; *all* distinctly heard the rumbling noise, and felt the shaking, which, for a moment, they supposed occasioned by a carriage, or by some unaccountable movement of furniture by the servants. When it was ascertained that neither of these could be the real cause, the party unanimously exclaimed—"An earthquake!" Mrs D., who was confined to bed by a sick headache, had been startled out of a slumber by the noise, and felt *five distinct rockings of the bed, crossways;* and though she had never felt an earthquake, at once concluded it was one. Her bed was up one low storey, and the motion seemed to undulate *east and west.* Had I been aware of it, I should have noted the barometer and thermometer; but am not aware of anything remarkable, except that we had for three successive days a strong north-west wind most unseasonably cold, with a good deal of rain. On the 28th, the wind was most violent and squally; on the 29th, windy with cold and heavy showers; on the 30th, strong gusts, with occasional rain. Glenfinnart is on the west side of Loch Long, in a narrow valley which runs westward."

9*th September* 1841.—On the shocks of this date, Mr Macfarlane of Comrie reported as follows:—"On the night between the 9th and 10th September, we had several smart shocks. The next morning, the association's instruments indicated as follows:—

"The steeple one was inclined to the south three-quarters of an inch; the Comrie House one was inclined to the north half an inch. The severest shock was at ten minutes to twelve, the next a quarter past two, and the third at half-past four. The first, I have marked in the register, 5; the second, 3; and the third, 1. No damage that I have heard of has been done. The weather, for the two preceding days, was remarkably wet and close, much resembling that in which the shocks occurred in 1839,—so much so was the sky the evening previous, that I was remarking to some folks, that it looked very like an earthquake night. But I have more than once observed the same misty and lurid sky, without any shock,—so that, after all, the thing may be a mere coincidence."

12th September 1841.—On the day here mentioned, a phenomenon occurred on the south coast of IRELAND, which was most probably caused by an earthquake in the ocean. The account to be now given, is extracted from a paragraph published at the time in a Wexford newspaper. It is interesting to observe that the sea *retired* in the first instance,—just as happened on the occasion of the Lisbon earthquakes in 1755 and 1761,—and that a few minutes before the reflux of the sea, there were "a number of short, loud, but rather smothered reports like cannons." It is not difficult to understand how submarine eruptions should produce, where they occurred, a momentary elevation of the sea, whereby the waters would be drawn from all the adjoining parts. The reports which occurred simultaneously with these eruptions, are strongly corroborative of the electrical theory.

Singular reflux in the Sea.—Sunday, the 12th (September 1841) inst. was a misty dark day, with wind S.SW. to S. About noon, the low growl of distant thunder was heard, and the wind lulled, which rendered the fog more dense. At Kilmore, ten miles south of Wexford, and directly opposite the Saltee Islands, the attention of the inhabitants was attracted about noon-time, to a number of short, loud, but rather smothered reports, like cannons, and it was supposed that they proceeded from some ship bewildered by the fog. The tide had flowed pretty well at the time, and the fishing boats in the pier were all afloat, when, in the space of two or three minutes, the water *receded* from the pier, and some walked *dryshod* where that short space before the boats had been floating in *five or six feet* water. In the course of a few minutes the waters began to return, much in same way as they had receded, and the tide continued to rise for the usual time. There was no extraordinary commotion, only an increased surf. After repeated rolls of thunder, and some heavy showers, the sky cleared up. It is the belief generally that this singular motion was the effect of an earthquake, whose shocks have of late been so frequently experienced in Scotland."

26*th November* 1841.—That this shock was pretty severe, is proved by extracts from the following letters.

At Comrie, it was felt, as the Rev. Mr M'Kenzie (then minister of the parish) wrote,—" About 12^h 40′ both the sound and the shock were very distinct; the former very like the sound that would be occasioned by a carriage passing along the street, with great rapidity; the latter causing the house to shake three times as much as a carriage would have done."

At Dunira, as Sir David Dundas wrote,—" A shock was felt about 1^h 15′ p.m., accompanied with a noise, evidently proceeding from NE. to SW. The day before had been very wet and rainy; but during the night it cleared up to hard frost. Some snow, but not to any depth, had fallen during the early part of the morning. A sharp frost succeeded, and at the time the shock took place, it was freezing hard. I was shooting at the time, with an English gentleman, who told me, that he observed the trees shake in a very peculiar way, and that it was very much the same sort of thing that he had felt in Rannoch, on the night of the 23d October.

20*th December* 1841.—At Kintail, in Ross-shire, a severe shock occurred at 4 p.m., which greatly alarmed the inhabitants of several parishes. " There was nothing peculiar in the state of the weather, or the appearance of the day, unless it might be a stillness and calmness in the atmosphere, which, although remarked at the time, is not uncommon at even this season of the year. There was no recurrence of the shock. The noise which invariably accompanies such visitations, like the rushing of water or the rattling of a carriage, was very distinct. We have before noticed the extreme prevalence of lightning, with occasional thunder, in the west and north Highlands, this winter; but the above is the only case of earthquake in the same districts which has come to our knowledge."

A few remarks may here be offered in regard to the most material inferences from the foregoing notices of shocks in 1840 and 1841:—

(1.) That there was a movement of the earth's surface, seems proved by the facts stated under dates 10th and 22d March 1841, and 26th and 30th July 1841. These indicate that the ground was moved, not merely horizontally, but also vertically from its previous position, to which, however, it immediately returned. An undulation of the ground was in fact produced.

(2.) That in the Comrie district, these shocks emanated from a point situated a little to the west of the Melville Monument. At Comrie, Clathick, and Crieff, the shocks on 18th January, 7th April, 26th and 30th July 1840, were respectively perceived to come from the *west*. At Dalchonzie, the shock of 30th July was felt and seen to come from the *north*, sloping upwards. At Dunira, the shocks on 30th July 1840 and 26th November 1841, came from the NE. In Argyleshire, the shock of 30th July 1840 came from the east, whilst at Aberfeldy, it came from the SW. Any person acquainted with the relative situation of these places, or who studies them on a map, will perceive that the shocks felt at them, must have emanated from the point just indicated.

(3.) That in most cases, atmospherical phenomena accompanied the shocks.

Explosions, or crackling sounds in the air, are related under dates 8th January 1840, 21st April and 12th September 1841. This phenomenon is the more remarkable, as the reports were, on the last of these occasions, heard at sea, and were mistaken for the firing of cannon, so that they could not be confounded with the subterranean noise, which also accompanies the shocks.

Several of the shocks took place in violent hurricanes or storms, as is shewn under dates 18th January and 15th September 1840.

Having thus noticed all the shocks of any severity which are known to have occurred in Great Britain, during the years 1839, 1840, and 1841, it may not be uninteresting to record briefly the shocks felt during the same period in other parts of the world. Data will thus be afforded for ascertaining—(1.) Whether the shocks in this country occur at or about the same time as those in foreign countries,—an opinion very generally entertained;* and (2.) Whether the shocks in this country, present the same general features as those in foreign countries.

On Cutting Half-Round, and other Curved-Faced Files. By the late Sir JOHN ROBISON, K.H., F.R.S.E., F.R.S.S.A. (Communicated by the Royal Scottish Society of Arts.)†

IT is well known to workmen that, although it be an easy matter to get *flat-faced* files of almost any required degree of smoothness and regularity of surface, a *half-round* file having an approach to such smoothness or regularity is altogether unattainable at any cost.

A method having occurred to me of striking half-round or even round files with the same smoothness, and with nearly the same accuracy of figure, as the flat files, I beg leave to submit the process to the Society, that it may, through these means, become known to those to whom it may be of use.

To form a half-round file, either convex or concave, I pro-

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* Mr Maclaren of Edinburgh has supported this view. Thus, with reference to the earthquake at Zante, on the 30th October 1840, he observes, "Here we have again a remarkable example of the coincident occurrence of earthquakes in distant parts of the world. The town of Zante has been ruined by a series of terrific shocks between the 24th and 30th October; and our paper of 31st October records a shock on the 26th of that month at Comrie. The distance between these localities is nearly 1700 miles. The bearing is SE. and NW. It may be remembered, that the great earthquake of 23d October 1839, was felt simultaneously at Comrie, in Piedmont, and at Reggio in Calabria,—that is, over a line coinciding in direction with the present, and only a little shorter. If we prolong the line connecting Reggio and Comrie, farther north, it strikes Mount Hecla, and may thus be said to have an active volcano at each extremity—Etna at the one and Hecla at the other. It is extremely probable, that we shall hear of earthquakes, between the 24th and 30th October, in the Alps or Apennines, or at other intermediate localities."—(*Scotsman* of December 1840.)

† Read before the Society, 12th December 1842.

pose that blanks should be prepared as if for thin equalling-files (*i.e.* of uniform thickness and breadth throughout), and that they should be struck on one or both faces, of the degree of fineness required. This having been done, I propose that, by means of a screw-press and swages of copper, or other soft metal, they should have the required degree of curvature impressed on them before being hardened, and that, in this manner, files with curved faces, but with the teeth of equable depth all across them, should be obtained.

In a similar manner I propose to form three-quarters round, or even cylindric, smooth files, by cutting flat blanks on one face, and then bending them on steel mandrils into a tubular form previous to hardening them.

On communicating this plan to the eminent manufacturer Mr Stubbs of Warrington, I learned from him that something of this kind had been attempted by his house, but abandoned on account of the difficulty experienced in getting the files into the curved shape after they were struck. Mr Stubbs at the same time sent me a file so made, thirty years before. This file at once explained how the difficulty had arisen, as, instead of the blank having been made of uniform thickness and breadth, it has been fashioned like an ordinary crossing file, and therefore not susceptible of being squeezed into the regular curved shape by simple pressure. If Mr Stubbs had thought of making the flat blank, he would no doubt have succeeded better; and the formation of tubular files, which he acknowledges never occurred to him, must have followed the other at a short interval.

EDINBURGH, *November* 14, 1842.

REPORT OF COMMITTEE.

Agreeably to the remit made to us by the Royal Scottish Society of Arts, of date the 13th December last, on a paper read by Sir John Robison, K.H., on cutting and manufacturing half-round and round files,—your Committee beg to report, that they have also had remitted to them three half-round pillar files, manufactured by Messrs Johnson, Cammell, & Co., Sheffield, which were exhibited at the Society's meeting, on the 23d instant, made in the manner suggested by Sir John. Two of these files were cut on the convex side only, and the other on the concave side also, and appear to your Committee to be very fair specimens as a first trial, although they are not so straight, and of so uniform curvature, as might be required.

Your Committee are quite satisfied that the plan proposed by Sir John Robison is eligible, and that, when suitable tools have been provided for creasing them into their proper form, and the workmen have had experience in using them, they will be able to produce half-round files very superior to those cut in the usual way for certain kinds of work. The smoothness of the cut will give the new files a decided preference.

Your Committee beg to remark, that the file cut on the concave side is the only one they have ever seen. As to making three-quarter round or even cylindrical files on the principle suggested by Sir John Robison, your Committee see no objection;—difficulties may occur to the makers in the first instance, but it is to be hoped these difficulties may be overcome by experience.

On the whole, your Committee conceive that Sir John Robison's plan of cutting and forming half-round and cylindric files with continuous teeth, will be a decided improvement in a certain class of files, and that the improvements are deserving of the marked approbation of the Society.

James Milne, Convener.

Edinburgh, 30*th January* 1843.

Extract from Letter Messrs Johnson, Cammell, & Co., *Sheffield, to the late* Sir John Robison, K.H., F.R.S.E., *February* 13, 1843.

The plan we have hitherto adopted in turning the half rounds, has been by top and bottom hand-swages of block-tin, that is, by placing the heated file on the bottom swage or groove, and turning it by hammering the top swage or mandrel into the bottom groove. This was the simplest method we could adopt to try the practicability of your communication; but we find it is not sufficiently powerful to effect and ensure in all cases a regular curve, and which we suspect has, in a great measure, been the cause of our failing in the hardening.

We have now ordered tools, say swages, &c. of copper, to be applied under the screw-press, and by obtaining the curve the whole length of the file gradually, and by one pressure, we hope will obviate our previous difficulties; and not leave those cranks or weak places, caused by the hand-blow, and which in all cases shrunk or twisted in the hardening. We expect in a few days to send you a few specimens from this process. We have also considered your plan of turning by copper rolls, but fear, from the causes herein named, it will not be very practicable. In all cases of steel-rolling, the steel has a strong tendency to follow and curl up after the rolls; in ordinary cases of plain steel it can be easily remedied and straightened; in this case the file would curl up after the rolls on the convex side, and in adopting any plan to keep it straight from the rolls, it would be liable to open out or disarrange the curvature of the file and injure the teeth. Again, we suppose in rolling there would be a danger of flattening or injuring the tooth of the file, unless the metal forming the rolls was nearly as soft as the heated steel; and if of the same temper, the rolls would be continually out of order and require renewing.

We make these few remarks, desirous of putting you in possession of our mode of proceeding, and the reasons; and which may probably elicit some improvement from your scientific experience.

We could easily by various means turn the heated steel into any form of curve, but in adopting the means, the object in view must be to preserve the proper sharpness of the tooth, which in this heated state by undue pressure or by hard metals is so liable to be injured; we trust, however, our next specimens will prove that these difficulties have been overcome. JOHNSON, CAMMELL, & Co.

JAMES TOD, Esq., Secretary,
Royal Scottish Society of Arts. SHEFFIELD, *November* 7, 1843.

Sir,—We are favoured with your esteemed letters of the 2d and 4th instant, with their enclosures, and are obliged by your kind attention.

We now have pleasure to hand you (as requested) extract from our letter of February 13th last, to the late Sir John Robison, explaining the mode we then adopted in the manufacture of the half-round files from steel of parallel thickness, as suggested by him, viz., by means of a screw-press and swages of copper, and which we see is the plan named in the paper communicated to the Society.

From the specimens sent you last week, you will perceive we have deviated from the plan first suggested "of cutting them from blanks of steel prepared as for a thin equalling-file." We, however, did not give up this plan until, from practical experience, we found its working very uncertain and irregular, for the file being of uniform thickness, the edges presented an equal or greater degree of resistance to pressure, than the centre; and the top swage or boss coming in contact with the centre of the file previous to any other part, caused it to bend more freely than the edges, producing various degrees of curvature in the same file. Again, if our top boss was so shaped as to create an earlier and freer pressure on the edges, to ensure a more uniform curve, we then endangered the sharpness of the teeth on those parts of each side of the file, convex and concave, first receiving such undue pressure. These objections and difficulties are all overcome or lessened by our present mode of cutting and turning the files from steel with slightly tapered edges, on one side, thus, 
The flat surface is cut with a continuous tooth, and can be turned either convex or concave, and the tapered surface can either be cut in ridges or left *safe*, or uncut, which, from the following extract, you will perceive was Sir John's first idea. In Sir John Robison's first communication, November 10, 1842, after recounting the difficulty of obtaining a smooth half-round file or one of equal continuous tooth, he says,—

"I propose to overcome this difficulty, and to cut half-round files as truly smooth as flat ones are now struck, by making the blanks of rolled steel-plate; by striking them in the flat state, and by afterwards giving them the degree of curvature required by means of a screw-press, and tin or copper swages; or else by passing them between grooved and furrowed rolls (of soft metal for the *struck side*)," evidently implying that one side would be unstruck or uncut. In fact, for some time at first we only attempted to cut one side of the file, and frequently now we are requested to leave sometimes the concave and at others the convex surface blank or uncut.

We trust these crude remarks may be of interest, as giving a reason for our discontinuing to cut and press these files from steel of *parallel* thickness, and if you choose to embody any part of them in your report, you are at perfect liberty. We have not the slightest objections to Sir John's paper being printed in your Transactions; and if any of our remarks are not perfectly clear, we shall be happy in giving further explanations.*

It must be highly satisfactory for the friends of the late Sir John Robison (and your Society in so freely noticing it), to learn that the invention is considered by eminent engineers and practical men, one of the first and best improvements in file-manufacturing of the day. Always at your command, we are most respectfully, Sir, your very obedient servants,

JOHNSON, CAMMELL, & Co.

An Account of Experiments with Thermo-Hydro-Electrical Currents, with an Examination of the Metals exposed to Thermo-Electric Action. By M. R. ADIE of Liverpool. Concluded from p. 353 of preceding Volume (Vol. XXXV.) Communicated by the Author.

19. The arrangements described (17 and 18) appeared to me to be worthy of further attention, to ascertain the variations in the amount of chemical action performed by two opposing thermo-electrical currents, which are much influenced by climate. The experiments made with these currents, now about to be given, have been conducted in the midst of a large town, where there are many local disturbing causes to affect the results; they should be received, then, more for shewing the general action of the currents under examination, than as correct results for this climate. The season through which they have extended has been remarkable for passing from one extreme to another; beginning with a very cloudy, bad spring, next an indifferent summer, and then a fine autumn, the autumn including six weeks, from the middle of August to the end of September, of the most powerful sunshine for the season of the year ever remembered.

In April last a thermo-electric battery, fig. 1, was placed, with the upper half exposed to sun and sky, and the lower half screened; the voltameter contained silver poles, and argento-cyanide of potassium in solu-

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* Messrs Johnson, Cammell, & Co., obtained the Society's Honorary Silver Medal for their specimens of Files cut on Sir John Robison's method, 13th November 1843.

tion; it was attached near to the upper joints of the battery, on the south face, and, like them, varied its temperature with the weather. The poles weighed 1 grain each on 21st April. On 21st August, after four months' exposure, the poles were reweighed, when the difference in weight was found to be 0.12 of a grain; the negative pole for the day current being the heavier, shewing that the silver has been deposited by solar radiation.

20. When the voltameter attached to the battery is maintained at a uniform temperature of about 90° by means of a sand-bath, the current generated chiefly by astral radiation, preponderated over the day current in April, when the sun was more than twelve hours above the horizon. To examine the influence of the fluctuations of the weather on this arrangement: A battery, fig. 1, was placed with the upper half exposed to sun and sky; two copper wires were soldered to its poles, then conducted inside the house, and permanently attached to a galvanometer fixed at the distance of 15 feet from the battery. With this instrument I was enabled to watch through the summer the changes in the force and direction of the currents. The summary of these observations is, 1*st*, For a clear sky, the indications are constant as to direction; from one to one and a half hours after sunrise, to within the same period of sunset, the exposed joints of the battery are heated, and for the remainder of the twenty-four hours, the same joints are cooled by radiation. The time stated betwixt the rising or the setting of the sun, and the change in the direction of the thermo-electrical currents, is given as the average interval; for there are occasions when the time intervening betwixt the change of the currents and the rising or setting of the sun is much longer or shorter. The most frequent cause of these changes in this interval is, when the general aspect of the sky is clear, with clouds on the horizon near where the sun rises or sets. 2*d*, In dry cloudy weather the action of astral or solar radiation is much checked; but this state of the sky rarely alters the direction of the current due to the period of night or day at which the observation is made. 3*d*, In wet unsettled weather: Rain falling for hours without intermission, nearly suspends the action of the battery either by night or day. But by day the light or intermitting summer showers produce by far the greatest variations in the force and direction of the currents generated by the weather. The showery weather, so common during our last spring and summer, by wetting the exposed and previously warmed portion of the battery, produced a thermo-electrical current corresponding to that derived from the radiation to a clear midnight sky, and often exceeding it in intensity.

21. The continuation through the summer and autumn of the experiments where the battery, fig. 1, was employed to deposit silver in a voltameter at a uniform temperature of 90°, enables me now to state the observed changes through the four seasons. From January to the end of April there was a constant deposit of silver by the current due to astral radiation; the rate of this deposit gradually decreasing as the

spring advanced. From May to the 21st June the two opposing currents were on the whole period so nearly counterbalanced, that I could discern no action; but at the beginning of May the night-current slightly preponderated. In July, August, and September, the day-current was slowly depositing silver. October there was no perceptible action. November the current due to astral radiation again begun to deposit silver, and this action may be expected to continue till next spring or summer.

These results differ considerably from the experiment where the voltameter was exposed like the battery; they shew a much feebler action through June and July, when the sun was nearly two-thirds of the time above the horizon, than in the first experiment (19); in it the opposing currents developed by day showers, and by astral radiation, are nearly powerless, on account of the reduced temperature of the voltameter (11). In the other, where the decomposing cell is maintained at an equal and elevated temperature, the current caused by day showers, or by astral radiation, is as effective as the solar radiation current. To the showery unsettled weather of May, June, and July, I can alone attribute the minute action observed; for in September, when the weather had changed to be remarkably fine, the solar current became more effective in depositing silver than it had been in June.

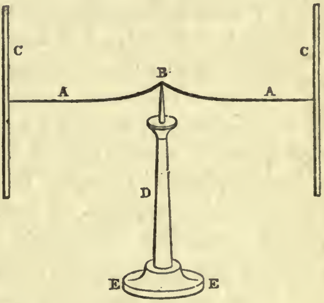
22. The galvanometer permanently attached to a thermo-electric battery (20), was at mid-summer exposed for two or three hours every morning to the solar rays; this deflected the needles of the instrument as much as 20°, when the helix was not connected with the battery. M. Becquerel and others have already noticed similar deflections, in experiments of this kind, where they were found to be caused by small uprising currents of heated air; but, at the time, as I thought that a change in the magnetic intensity of the upper needle, from its elevated temperature, and also local currents in the copper coils of wire, might operate to produce a part of these deflections, I removed first the coils of wire, which made no change; next the diaphragm or compass card interposed betwixt the two needles, so that the rays of the sun now acted equally on both of them; this alteration increased the deflection, shewing streams of heated air rising from the upper needle to be the active force which produced the deflection of 20° in the complete instrument, when in the sun's rays.

23. While engaged with the last experiments, they appeared to me to point out where to look for the explanation of a curious instrument, Mr Watt's sun needle, which I had seen many years before, under my father's care, rotating by the action of the solar rays; and as I am not aware that the cause of this rotation has yet been shewn, I will briefly state the result of some experiments, which refer it to small uprising streams of air, generated in their highest energy on metallic surfaces.

Mr Watt's instrument consists of a light piece of wood, with agate centre, in form resembling the needle of a mariner's compass; on the extremities of each of its arms there is fixed a star, each star composed of an equal number of sewing-needles, about 25; these needles are magnetized to saturation. The arrangement is made to float on a fine steel point, by which the agate centre is supported, and the whole covered by a thin glass shade to protect it from currents of air. A slow rotation of the stars round their centre of support, is produced by the rays of the bright sun, which continues so long as the air surrounding the needle receives heat from them. The needles in the stars are in planes vertical to the plane of the horizon. On turning them through 90°, so as to be parallel to it, the rotation entirely ceases.

Two slips of watch-spring, 3 inches long, of the same weight, and magnetized equally, were fixed on the ends of a light piece of wood with agate centre, similar to that used for Mr Watt's sun needle.

Fig. 5.



A A. A light horizontal needle of wood.

B. Agate centre.

C C. Pieces of metal of equal weight.

D. Support with fine steel point.

E E. Stand. Weight of needle and agate 5 grains, exclusive of the pieces of metal C C.

The planes of C C are inclined about 5° to the vertical plane, the one dipping to the right side of the arms A A, the other to the left, so as to balance on B the centre.

Under a thin glass shade exposed to the solar rays, these two pieces of watch-spring rotated as well as Mr Watt's needle; and as this took place without reference to whether the poles pointing to the earth were like or unlike, I was led to infer that magnetism had nothing to do with the rotation: which was soon confirmed by changing the magnets for pieces of unmagnetized watch-spring; then for slips of copper; and next for pieces of bismuth, the most imperfect conductor of heat among the common metals; with all of these the rotations were easily obtained. To succeed with magnetised pieces of steel is more uncertain, for if the arrangement is not very nearly astatic, the needle will only oscillate to and from its point of repose, or change its position in azimuth as the sun passes through the sky.

The sun, in this country, is not powerful enough to produce rotations when inferior conductors of heat are substituted for the metallic slips C C; wood, cork, charcoal, were tried in June without success. As these organic substances exposed as much surface to the action of currents, which the sun's rays may be expected to give rise to within the shade, their remaining at rest shews, that the other rotations with copper or bismuth were not produced by those currents.

A familiar illustration of the force derived from small uprising currents of heated air, is shewn by two equal weights in a delicate balance, where the beam stands at zero when the weights are of equal temperature. Heat one of those weights 20° degrees above the other, and replace it in its scale, then it appears 100th or 200ths of a grain lighter than at the first weighing, occasioned by the streams of air now rising from its surface. On the surfaces of both pieces of metal C C, this minute force may be developed by the power of the solar rays, and by slightly inclining the slips of metal out of the vertical plane, so as to get the force of the streams of air, rising from each piece, to act in the same direction, the rotation in question is produced. The experiment succeeded best under a large shade, where the contained air is not soon heated by the sun's rays; with thin slips of metal the motion commences in 25″ or 30″ after unscreening; but with stout pieces of metal, which I have had as heavy as 35 grains each, it is some time after exposure to the sun until the motion begins, and again the rotation endures, for a corresponding period, after the needle is screened. 4 or 6 arms, each carrying a slip of metal like C C, may be used with advantage, to give steadiness to the rotations. With a 6-arm arrangement I obtained regular rotations by the rays from a clear coal-fire, but not under a glass shade; for this experiment, side-screens must be used to prevent the interference of currents of air. The rotations produced by the fine bright sun of mid-summer form striking and beautiful experiments. I took great interest in them in June last, from the expectation that simple arrangements, like fig. 5, might serve for actinometer measurements; but at present I am afraid that the force which turns them is too feeble to admit of being formed into a scale for this purpose. Where the pieces of metal on the extremities of the arms A A are magnetized, however much the forms or number of pieces of those slips of metal may be varied, there will always remain in the whole arrangement some degree of magnetic polarity. Several variations in the action of Mr Watt's needles can be produced by changing the intensity of this polarity; when the magnetic force is very feeble, and the metal surfaces well placed for the development of the uprising streams of air, the rotation takes place; but when either the magnets are so placed to give rise to feeble uprising currents of air, or the magnetic polarity is increased, then the needle is deflected out of its statical position, like the galvanometer needles (22), and there is no rotation, the change of the sun's place in the sky varying the amount of that deflection.

Cold streams of air playing on the side of the shade act on the same principle as the solar radiation, for the radiation on the suddenly cooled side of the shade then gives rise to streamlets of air descending from the surfaces of the metallic slips. On these views, a needle which I am informed points to the direction of the wind, may be explained; and in all of the arrangements, I think it will be found that it is *the streamlets of air rising off, or falling from, the metallic surfaces, which produce the rotations or changes in the position of the needles.*

Hydro-Electrical Currents.

24. As the decomposing cell, fig. 4, attached to a pair of zinc and copper plates of half an inch superficies each, excited by water, pure at the commencement of the experiment, could perform a greater variety of chemical decompositions, than a thermo-electrical current derived from a bismuth and antimony joint heated till the bismuth fuses, it appeared to me to be readily applicable for some experiments with hydro-electrical currents.

Platina poles, sheathed with glass that they might present a very small surface to the fluid to be decomposed, were inserted in the voltameter, which was afterwards filled with water acidulated by sulphuric acid. The lowest electro-motive-force capable of effecting decomposition in the above arrangement, was derived from 3 pair of zinc and copper plates excited by water only; with a saturated solution of common salt in the decomposing cell, instead of sulphuric acid and water, 4 pairs of those plates were necessary. Although the size of each plate was only half a superficial inch, the minute bells of gas might be seen for hours to rise from the surfaces of both poles.

25. The difference betwixt the intensity of electricity from hydro and thermo sources is well shewn in two experiments with the voltameter, fig. 4, filled with a saturated solution of iodide of potassium in water, and a pair of pure silver poles for the first experiment, and copper poles for the second. The iodides of silver and copper, although insoluble in water, are readily dissolved in a solution of the iodide of potassium. The most intense thermo-electrical current I could command produced not a trace of decomposition in the voltameter with silver poles and iodide of potassium in solution. But if a hydro-current from a single pair is passed through the same voltameter for a few minutes, a new compound is formed, the iodide of silver dissolved in iodide of potassium; this a thermo-current can readily decompose, so that in the voltameter, where before no effect could be produced by this agent, silver now rapidly deposits on the negative pole through its influence. With copper poles instead of silver, the second experiment differs from the first, in so far, that there is a minute action by a thermo-current, before a hydro-current has been passed through the voltameter; but after the application of a hydro-current, the rate of deposition of the metal undergoes a marked change, and the iodide of copper is now found in the solution.

26. With the voltameter, fig. 4, arranged with glass-sheathed platina poles, so as to expose a surface of $\frac{1}{2500}$ of a superficial inch to the electrolite, the electrical spark appears, whenever the gas, disengaged at the poles, is given off with sufficient rapidity, to envelope them in its atmosphere. Thus a surface small enough to give a spark on the pole where the hydrogen appears, is often too large for the development of the spark where the oxygen appears; this is clearly shewn in a voltameter, where the superficies of one pole is greater than the other, for

when the large pole is on the hydrogen side, sparks may be obtained in both gases; then reverse the voltameter, and the hydrogen side only will shew sparks.

27. The cell, fig. 4, was used to carry on electrolysis under great pressure, when it appeared that a high pressure from hydrogen could change the action from one where gas was eliminated, to another where metal was deposited in lieu of the hydrogen. This takes place with many of the metals, but the account of an experiment with copper poles decomposing water acidulated with sulphuric acid, should sufficiently illustrate this kind of action.

The voltameter is hermetically sealed; electrolysis begins with the formation of the black oxide of copper at the positive pole, and bells of hydrogen rise from the negative pole; the gas eliminated, rapidly increases the pressure in the voltameter; a blue solution of the sulphate of copper soon appears between the two poles; hydrogen then gradually ceases to rise, the last bubbles of gas adhere to the pole; at this stage, with the voltameter in the focus of a microscope, a brown film appears to coat the gas-bells attached to the pole, which changes to be more and more opaque, until the exterior surface of a beautiful sphere of copper appears in the field of the instrument. Now nip off the point of the capillary tube, so as to remove the pressure in the voltameter; immediately, the original action recommences, viz., oxide of copper at the positive pole, and hydrogen at the negative pole, and continues until the solution of the sulphate of copper becomes strong enough to allow the precipitation of the metal to go on under atmospheric pressure. Here the compressing force merely effects a change quickly, which, under ordinary circumstances, would have been brought about by time, so that with compounds of an evanescent kind, this mode of manipulating may be useful.

28. A number of decomposing cells, fig. 4, were prepared with a pair of poles, of each of the undernoted metals, and filled with a saturated solution of pure common salt; to ascertain how many of them could be oxidized at the positive, and deposited in a metallic form at the negative pole, through this wide-spread solution. Deposits on the negative pole appeared in leaf-like forms while using poles of silver, copper, tin, zinc. The last metal is difficult and uncertain; in the majority of the experiments, the cells burst before any zinc appeared. Deposits on the negative pole were obtained in a pulverulent form; for antimony, palladium, and platinum, black; cobalt, dark brown; gold, dark purple; arsenic, a flocculent dark brown; mercury, whitish grey. The antimony and platinum required a high pressure before their deposits appeared. Iron, lead, bismuth, cadmium, and nickel poles, eliminated hydrogen until they burst the strongest cells; but of these five exceptions, three of them can be transferred through another element of sea-water, iodide of potassium or sodium: when this solution is substituted for common salt, lead is obtained in beautiful leaves at the negative pole, bismuth and cadmium as dark powders. In the 16 pairs of poles of different metals tried, there are only two from which a metallic deposit on the negative pole cannot

be obtained with solutions in the decomposing cell which are found in the waters of the ocean. Of these two, nickel, I could not procure in a form to admit of high pressures being well tested; for the other, iron. In a strong voltameter filled with a saturated solution of common salt, and poles of this metal, the decomposition of water proceeds readily by the electro-motive force of one hydro-couple giving off hydrogen at the negative, and a large dusky green insoluble deposit at the positive pole; after three or four days' slow action, the bulky deposit assumes a clearer but dark hue, and eventually the pressure from the hydrogen bursts the vessel.

The currents for these decompositions were all of them derived from spring or rain water acting on 1, 2, 3, or 4 pairs of small zinc and copper plates; the number of the plates being varied to supply the electrical force required for the poles of the metal under examination.

The Metals exposed to Thermo-Electric action.

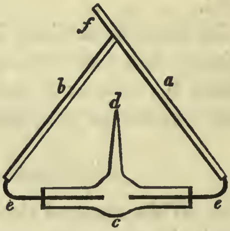
29. While engaged decomposing metallic salts by a thermo-electric couple of copper and antimony fused together to form a joint, and excited by enveloping the joints in a blue gas-flame, I was struck by the rapid destruction of those joints, few of them enduring 48 hours. As both of the metals were much oxydized, I at first attributed to the oxidation the change noticed. But the same appearance presented itself in the battery employed for the experiments described (9); there the joints are not in the flame, and as the parts of the bars around them are only coated with a thin film of oxide, corresponding in appearance to a full straw colour on steel, the disruption of the joint which separated with a slight touch, could not be assigned to the oxydation of the metals. From further investigations of this change in the fracture, I found it a constant accompaniment of the unequal heating of thermo-electric bars at temperatures above 300°; but it is so very slow, that forty days of the most energetic action of antimony and bismuth couples is necessary to render the first trace of a dull fracture distinctly visible; and with bars of other metals less adapted for developing thermo-electricity than antimony and bismuth, the disruption is not so rapid, the temperature of the joints being equal.

At low temperatures, it would be a valuable experiment which could establish this change in the joint. So far as those I have instituted for this purpose have gone, there is not a trace of the action; the joints of a battery, fig. 1, weathered for a year, are as sound as new ones. But if the thermo-current given during 40 days' action of the battery, fig. 2 (9), must be developed, by a weathered battery, to effect decompositions to the same extent as in the first, then an estimate founded on this view, with the experiments (11, 13, 19) taken as data, shews a period of *from 40 to 50 years* to be required to produce the change sought for.

30. When the joints were found to be subject to a gradual change, an experiment was made to ascertain if a long-used joint lost any of its

thermo-electrical energy. With the arrangement described (9), the rate of decomposition per diem by the battery was taken at the end of 90 days' continued action, when I was certain, from previous experiment, that the joints had greatly changed; they were then resoldered, and the voltameter attached, in as nearly as practicable the same condition as for the first rate. This experiment shewed no alteration in the rate, from which we may infer, that thermo-electrical batteries are capable, at moderate temperatures, to generate their currents for almost an indefinite length of time.

Fig. 6.



Two single pairs of antimony and bismuth bars, fig. 5, decomposed sulphate of copper, and Argento-cyanide of potassium, for two months respectively.

a antimony bar, *b* bismuth bar, weight of the pair 19 grains, *f* projecting point made to touch a blue flame, *e e* the poles, *d c* decomposing cell same as fig. 4.

A thermo couple of antimony and bismuth bars, soldered by pure bismuth, and excited like battery described (9) for 120 days, separated in the joint, at the end of this period, by a light touch; *and a small quantity of the bismuth at the part where it has been in contact with the antimony, rubbed off in a pulverulent form. The antimony at the soldering was unaltered.* This experiment appears to me to be of value, to shew the nature of the change in metals attendant on thermo-electrical actions.

31. I now entered on a series of experiments, to ascertain the alterations in the texture of metals, and their changes in density from annealing, as shewn by the balance, their fractures, and the galvanometer; of these it will be sufficient here briefly to notice the results.

Cast bars of antimony, bismuth, and zinc, never changed their fractures, although annealed for four months at a temperature little lower than their melting heat. The grain of some of the specimens tried, was of a fine unnatural texture, occasioned by casting in a cold metal mould, yet the annealing made no alteration in this grain. Rolled zinc, where the crystals have been bruised, undergoes a marked change by annealing at temperatures 30° higher than our summer weather; but the extent of the change to a larger grain is much governed by the annealing temperature; thus, with a temperature of 150°, it is impossible to produce the same size of grain as 250° gives, although the time of annealing the first is prolonged ever so much beyond the period allowed the second; but annealings at low temperatures required more time for the full development of their change than was necessary for higher temperatures.

With iron I have found the same order to prevail in the results; but it requires high temperatures for all those changes, to be recognised by the balance or the fracture. Iron softened at 400° for 3 months, did not sensibly change the specific gravity. Soft steel retains its density with remarkable uniformity; although, in hardening, the loss exceeds

1 per cent. in density, this process was repeated fifty successive times, without any change in the specific gravity, when tested in a soft state.

Antimony and bismuth bars, when often heated and immersed in cold water, lose density on each repetition of the heating and immersion; they become hollow in the centre, and full of fissures; zinc bars continue solid under the same treatment. The following metals lose density by annealing:—Bismuth, zinc, lead, tin, copper, silver, iron, gold, platinum. Antimony cast in a cold mould, and hard steel, gain density by annealing. Zinc, cadmium, lead, and tin, gain density when poured on ice, compared with the same pieces poured on a flat surface, and slowly cooled. Antimony and bismuth shewed no change in density when treated like zinc, cadmium, &c.; but when cast in a confined cold metal mould, in bars similar to those used for battery, fig. 1, they lose about $\frac{1}{50}$th of their specific gravity. Copper and silver lose density when poured on ice, or on cold metal surfaces.

In these experiments, bismuth and antimony resemble hard steel, in so far as the process of hardening is concerned; but in the annealing, antimony only follows the same change as the steel, and gains density. Bismuth, when cast in a metal mould immersed in dry snow, loses $\frac{1}{450}$ part of its density in the lower portion of the bar, yet a still further loss of about $\frac{1}{100}$ part is produced by annealing; while antimony, cast under similar circumstances, suffers the same loss of density; but with annealing always regains part of the loss.

In comparing the changes in the densities of the metals for equal intervals of temperature, in the foregoing experiments, bismuth passes through the widest range of variations, then follow zinc and antimony.

32. Experiments with antimony and bismuth couples were now made, to ascertain if, while they were engaged to develope thermo-currents, the same changes in density took place; the mean density of three specimens of bismuth changed during twenty-one days' action from S.G. 9.853 to 9.838, three specimens of antimony for the same period S. G. 6.645 to 6.670. The direction of these changes agree with the former experiments where the metals are operated on singly. At first sight I expected they would exercise much influence on the energy of thremo-electrical currents developed by antimony and bismuth; but from a number of clear experiments with bars in different states combined in thermo couples, I have found the more nearly the bars approach *to their natural density, when in a soft state,* the better they are adapted for energetic action in combination with another metal. The experiment which first shewed me this fact, should sufficiently illustrate the whole of the class. A bismuth bar, combined with a bar of soft steel, and attached to the galvanometer, was heated by oil to 95°, the deflection in the galvanometer was 67°. The bar of steel was then untied to harden it; after hardening, the arrangement was now replaced, to be in every respect the same as before, when the couple of bismuth and hard steel shewed a diminution of 2° in the deflection of the galvanometer needles. This experiment was many times repeated with other elements, all of which agreed in shewing a result similar to the above, and to develope the greatest amount of electricity from a given couple. *Both sides must be thoroughly annealed;* hard bismuth,

in connection with hard antimony, are not so effective for thermo-bars, as these metals in a soft quiescent state.

33. When thermo-electrical currents developed in wires or thin pieces of *one metal* are examined, an opposite order prevails; for the further a portion of the metal can be forced from its natural density, the better will the arrangement answer as a thermo-couple; a hard and soft piece of any metal form a pair of elements, and their point of junction corresponds to an ordinary thermo-joint. A slip of watch-spring with a part in its centre hardened by heating and quenching in water, is a good specimen of this kind of thermo couple; for by heating the point of junction of the two densities only 10° above the adjoining parts, distinct evidence of a thermo-electrical current is given.

M. Becquerel has shewn, that, when a portion of wire is hammered or twisted, a current of electricity is found passing when the part is heated; here the density of the metal has been changed, and the galvanometer merely tells, that, in all probability, the texture of the wire is slowly equalizing; however this may be, *the experiment suggests an application of the galvanometer, which renders it a most delicate test of any change in the density of metals.* For this purpose, a wire of the metal which the operator desires to test, take for example iron, is repeatedly drawn through the draw-plate to harden it; this hardened wire is then *filed* into two halves; the contiguous parts where it has been cut are now tied together without twisting, and the necessary connections of the other extremities made with the galvanometer. On heating their points of contact, no electricity is detected. But when one of the halves of the wire has been annealed for three weeks at 400°, then again tied to the hard half, and the foregoing experiment repeated, the galvanometer immediately shews the passage of a current from the soft half to the hard, thus proving that the temperature of 400° has effected a change in the iron, which so far as I have been able to test, cannot be detected either by examining the fracture or trying the specific gravity.

34. The direction of the thermo-electrical current developed betwixt hard and soft portions of the same metal, either in two separate pieces, or, what is more convenient, in one continuous piece, where the nature of the metal examined admits of this, was now tested in bismuth and 7 of the malleable metals, *and for all of them found to be from the side of the joint which was losing density to the hard part of the wire or bar.* Steel, hardened and made dense by hammering, agreed with the above 8 metals. But steel, hardened by quenching in water, and antimony cast in a cold mould, and formed into couples, with soft steel for the former, and antimony cast in a mould heated nearly to redness, for the latter, the direction of the electrical current, with reference to hardness, appeared to be reversed; yet there is no change, when examined, with reference to the action in the joint, for in the two last cases the hard portions are *gaining density,* while in the 8 other metals the hard portions *lose density.*

The consideration of these experiments with metals in different states of density, together with the experiment described (30), where an energetic thermo-current appears to be produced in a vast quantity, from a slow disintegration of the bismuth, suggest to me the view, that the thermo-electrical current in a joint, *as a general principle, passes from the side which is changing towards that which remains more stationary*; and to develope it in its highest energy, the couple should consist of a changeable half (bismuth) and of a fixed half (antimony). To this view it may be objected, that the experiments given above shew that antimony is a metal which can be forced through considerable changes in density, but they also shew that such alterations are injurious to its action as a negative thermo-electric (32); and further, it differs from other metals in gaining density by annealing, so as to approach to its natural statical state. When both sides of a thermo-couple are changing, then the current represents the balance betwixt the two actions. Where a reversal of the current is obtained by varying the temperature, the balance of change is first on one side then on the other; a good experiment of this kind is shewn with an antimony and iron thermo-couple. The iron is the thermo-negative metal for all temperatures below 160°; at this point the two sides of the joint change alike; and for higher temperatures antimony is the negative metal. An extension of this experiment, with iron and antimony elements, shews its great value for explaining the nature of thermo-electrical action; for when the antimony has been cast in a *cold mould*, the reversal of the current, at a temperature about 160°, is then constant and most decided; but if the antimony is cast in a *mould heated nearly to redness* and slowly cooled, the iron is on the positive side from the commencement of the heating of the elements, the reversal of the current disappears.

The view I have attempted to trace above, as governing in all thermo-electrical arrangements, appears to me to be applicable to hydro and frictional sources of this agent, where the current passes from the side disintegrating or changing towards the stationary element of the arrangement which continues unaltered; and that the numerous instances of variations in the direction of frictional-electrical currents may be explained on the same principle as the reversal of a thermo-current by a change of temperature, viz., that the greatest amount of change is in the rubber when it excites glass, and, again, when the same rubber excites resin, that the resin then undergoes the most rapid change.

From the last experiment with iron and antimony, it will be apparent, that in batteries, fig. 1, to be excited by the weather, the iron might with advantage supply the place of the antimony; and I would recommend it to be so applied, but for the practical difficulty of soldering pure bismuth to iron. From some other experiments with bismuth, as the thermo-positive metal, attached to lead, tin, and alloys of these metals, I could recognise no diminution in the thermo-electrical energy for these couples, when heated in oil to 95°, compared with a couple of bismuth and antimony; so that if the form of the battery, fig. 1, described at the commencement, should prove troublesome, on account of the fragile nature of the long bars, lead may well supply the place of the antimony used for them. Copper and zinc, in connection with bismuth, stand a little below lead. Antimony cast in a hot mould is much more brittle than when cast in the same mould cold.

R. Adie.

Errata *in the portion of the paper in Volume* 35.

Page 347, line 19, *for* " 80 inches circuit" *read* " 180 inches circuit"
... 348, the letters *c* and *d* omitted at the poles of fig. 1.
... 348, last line, *for* " lightest" *read* " highest"
... 350, line 7, *for* " with my" *read* " with these"
... 350, line 35, *for* " pole" *read* " pile"
... 351, line 22, *for* " electrolice" *read* " electrolite."

N.B.—Authors ought to be more correct in their M.S. in order to prevent lists of errata.—Edit.

On the Determination of the Index of Refraction by the Sextant, and also by means of an Instrument depending on a new Optical Method of ascertaining the Angles of Prisms. By Mr William Swan, Teacher of Mathematics, Edinburgh. (With a Plate.) Communicated by the Royal Scottish Society of Arts.*

The powers of transparent substances in refracting and dispersing the rays of light, present phenomena of the most interesting description; and the accurate determination of such physical properties has always been regarded as an important branch of experimental science. While these inquiries are valuable in a scientific point of view, they are also of obvious utility in relation to the useful arts, as affording the means of constructing with accuracy many of the most important optical instruments; and, accordingly, the examination of refractive and dispersive powers has occupied the attention, not only of merely scientific observers, but also of the most eminent practical opticians. Newton examined a considerable number of substances; and his sagacious conjecture of the inflammable nature of the diamond, and also of one of the ingredients of water, from their great absolute refractive powers, has been remarkably verified by modern discoveries. This affords an example of the importance of the refractive index as a physical character; and it also shews the valuable results that might be obtained by a more perfect knowledge of the connexion between the chemical constitution of bodies and their optical properties.

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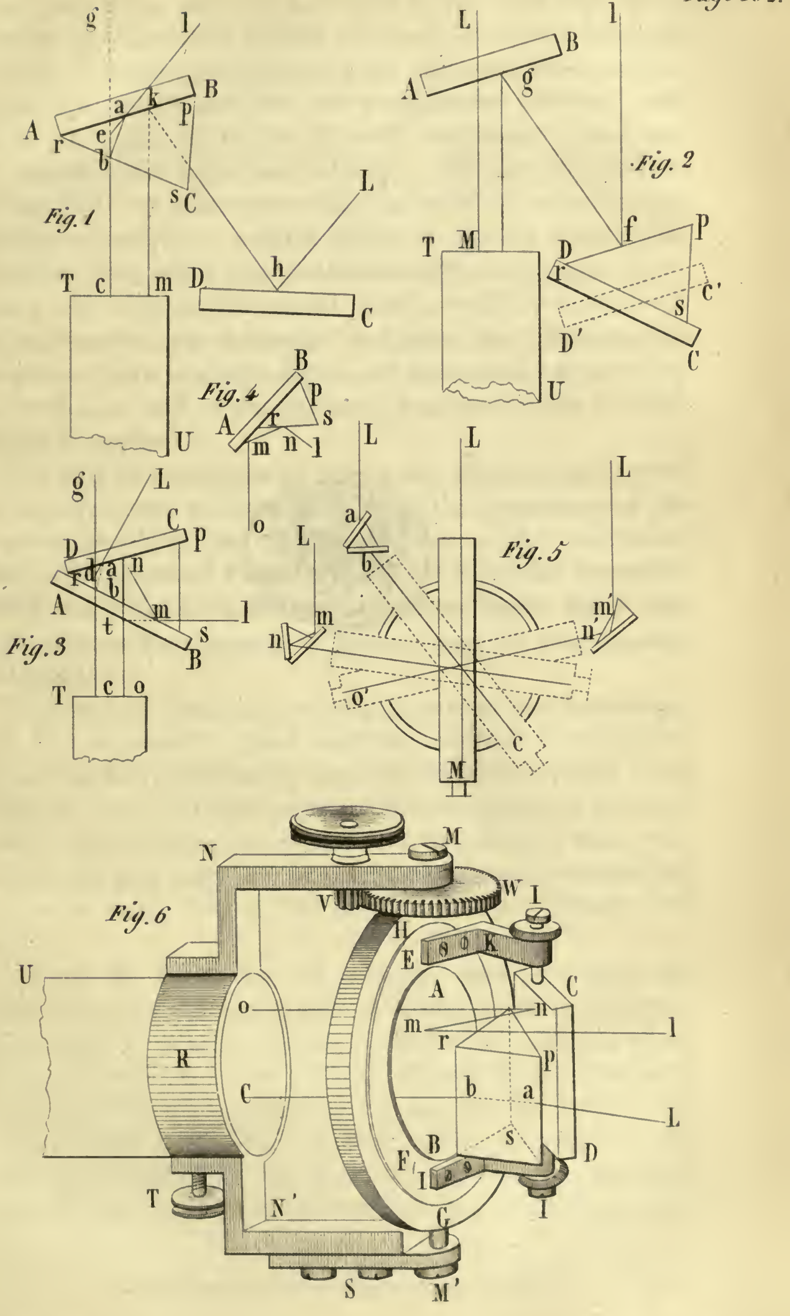
* The paper of which this is an abstract, was read before the Society, 12th June 1843; and the Society's Gold Medal, value 15 Sovereigns, awarded 13th November 1843.



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Mr. Swan On the determination of refractive powers

BRITISH MUSEUM
NATURAL HISTORY.

In later times, the chief accessions to our knowledge of refractive and dispersive powers are due to Sir David Brewster and Dr Wollaston ; but although the labours of these philosophers have greatly extended our acquaintance with such subjects, much may yet be done to verify and render more exact the results which have been obtained. Sir John Herschel observes, that " an accurate re-examination of the refractive and dispersive powers of natural bodies of strictly determinate chemical composition and identifiable nature, although, doubtless, a task of great labour and extent, would be a most valuable present to optical science;" and again, that " Fraunhofer's researches shew to what a degree of refinement the subject may be carried, as well as the important practical results to which it may be applied."*

It would be superfluous to adduce any illustrations in proof of the correctness of these views, or of the importance of the inquiries to which they refer, in addition to the example of the justly celebrated Fraunhofer, whose admirable researches afford one of the finest instances of the connexion which subsists between the extension of science, and the improvement of the arts.

If, then, it be desirable not only to extend our knowledge, but also to render it more accurate, we shall best promote these objects by facilitating our methods of observation ; and, although we are already possessed of instruments by means of which tolerably accurate results may be obtained with great facility, we may yet find room for improvement in simplifying our more exact, but at the same time, more difficult processes.

Among the latter class of methods, must be ranked the most direct and obvious process of finding the index of refraction, which consists in forming a prism of the substance whose refractive power is to be determined, and then ascertaining the inclination of its refracting surfaces, and the deviation of a ray of light transmitted through them. In order to find the index of refraction from such observations, it is necessary to place the prism, with its face inclined, at a determinate angle

* Encyclopædia Metropolitana, " Optics," p. 573.

to the incident ray ; and this is best done by turning the prism round in a plane perpendicular to its refracting surfaces, until the deviation of the transmitted rays is observed to be a minimum. When this is the case, the incident and emergent rays are equally inclined to the faces of the prism, and the index of refraction can be deduced from the observed angles by a simple formula. This arrangement was adopted by Newton, and afterwards by Fraunhofer ; and the extreme exactness of which it is capable, has been shewn by the singularly accurate results obtained by the latter observer. If, therefore, it could be simplified in its details, so as to lessen the trouble of observation attendant on it in its present form, it might be rendered more generally available for increasing our knowledge of refractive powers. From actual experience, in using an apparatus constructed after Fraunhofer's plan, I have found that the manner in which the prism is mounted is productive of great inconvenience. This arises from the adjustments of the faces of the prism mutually affecting each other, so that it is only after many approximate trials that both faces can be made perpendicular to the limb of the theodolite, by which the deviation of the refracted rays is measured. Another inconvenience also attaches, as far as I am aware, to all the existing modifications of this method, in as much as different instruments are required for measuring the angle of the prism, and the deviation of the refracted rays.

Wollaston's goniometer has indeed been used to determine both of these angles ; but for the latter purpose, it is a most defective instrument, as it is only by the aid of marks made on the walls of the apartment in which the experiment is conducted, that the deviation of the refracted rays is ascertained.*

In endeavouring to simplify this method of ascertaining refractive powers, I devised two different processes, and, as one of these was the means of suggesting the other, I shall describe them in the order in which they occurred to me.

I. The first is a new application of the sextant, and will be best understood by reference to figs. 1 and 2 (Plate II.), in which

* Edinburgh Encyclopædia, "Optics," p. 506.

T U represents part of the telescope of the sextant, and A B, C D, sections of the horizon and index-glasses, by a plane perpendicular to their surfaces. Let the prism of which *p r s* (fig. 1) is a section, be applied, with its surface slightly moistened, to the horizon-glass, and it will adhere in any required position by the cohesive attraction of the film of fluid between it and the glass. The telescope can now receive simultaneously rays, *l a b c*, passing through the horizon-glass, and refracted by the prism,* and L *h k m*, passing under the prism, and reflected from the index and horizon glasses. The images of a distant luminous object, formed by the refracted and reflected rays, may therefore be brought into coincidence at the focus of the eye-piece, as in the ordinary manner of using the sextant; and from the nature of the instrument, the angle indicated will be the inclination of the final direction *g c*, of the refracted rays to L *h*, the direction of the rays incident on the index-glass. Now, if the luminous object be so distant, that the parallax, due to the distance between the horizon and index-glasses, may be neglected, the inclination of *g c* to L *h* will be sensibly the same as *g e l*, the deviation of the refracted rays. Since, therefore, the angle indicated by the sextant is the inclination of *g c* to L *h*, the deviation of the refracted rays is determined.

If the prism be now placed upon the index-glass, (fig. 2) in the position *p r s*, its surface *p r* will form a new index-glass, inclined to the proper index-glass C D, at the angle *p r s*, which is to be determined. Let the image of a luminous object, seen by rays reflected from the index-glass, be made to coincide with the image of the same object seen directly; and if there be no parallax or index error, the surface D′ C′ of the index-glass will be parallel to the surface of the horizon-glass, and the index will be at zero. Again, let the image of the same object, formed by rays reflected from the surface *p r*, be made to coincide with the object seen directly, and the surface *p r* will be parallel to the horizon-glass. Now,

* In this, and also in the other figures, as the rays do not suffer any ultimate change of direction in passing through the parallel plates of glass, in order to simplify the diagrams, they are represented as suffering no refraction by these plates.

since by the motion of the index, the surfaces, of which D C *p r* are the sections, have been made successively parallel to the same plane, the index must have described an angle equal to *p r s*, the inclination of these surfaces. But as the index moved from zero, and the angle indicated on the limb of the sextant, is double of that actually described by the index, half the angle indicated will be equal to *p r s*, the angle of the prism, which will thus be ascertained.

It is obvious that, in these observations, the principal section of the prismm us tbe adjusted to the plane of reflexion at the mirrors of the sextant. This is easily effected by the hand; and the correctness of the adjustment will be ascertained by the possibility of making the images coincide accurately. It has also been supposed, for the sake of simplicity, that there is no index error or parallax; but the existence of either will not alter the determination of the angles. The object observed should be so distant, that the parallax may safely be assumed to be constant during the observations. Its amount, along with the index-error, may then be ascertained by direct observation, and applied as a correction to the angles.

The sun will be found a convenient object for conducting these experiments; and as dark glasses are necessary to moderate the intensity of the light, advantage may be taken of eye-pieces, which transmit only homogeneous rays. Of these, the most convenient is the combination of common red and smalt-blue glasses, recommended by Sir John Herschell,* which will be found to transmit the extreme red rays of the spectrum of perfectly definite refrangibility. I have also used a fluid eye-piece of the ammoniated sulphate of copper, which absorbs all but the extreme violet rays; and the flame of a spirit lamp, with a salted wick, in order to obtain yellow rays of definite refrangibility. This light is very convenient for such experiments, and is of the greater importance, as, according to Sir John Herschell, it occupies precisely the same place in the scale of refrangibility as Fraunhofer's dark line D in the solar spectrum.†

In order to ascertain the index of refraction from the obser-

* Encyc. Metropolitana, art. Optics, art. 503. † Ibid., art. 436.

vations now described, it is necessary to determine the inclination of the horizon glass to the optical axis of the telescope. This may be done by directing the telescope to a distant lamp, and then moving the index slowly until an observer at the lamp, with his eye near the flame, sees its image reflected from the index-glass. The angle indicated must then be noted; and to avoid as much as possible the effect of parallax, the index-glass should be covered, with the exception of a narrow portion over its centre; and the observer at the lamp should look through a hole twice as far from the flame as the centre of the index-glass is from the axis of the telescope. The angle indicated is double the complement of the inclination of the horizon-glass to the line of collimation.*

The index of refraction is then calculated by means of the following formulæ:—

Let $\theta =$ the angle of the prism;
$\delta =$ the deviation of the refracted rays;
$\alpha =$ the inclination of the horizon-glass to the line of collimation;
$\phi =$ the first angle of incidence;
$\phi' =$ the first angle of refraction;
$\psi =$ the angle of emergence;
$\psi' =$ the second angle of refraction; and,
$\mu = \frac{\sin\phi}{\sin\phi'}$, the index of refraction.

Then the following relations are easily obtained:

$$\phi = \frac{\pi}{2} + \delta - \alpha; \ \psi = \theta + \alpha - \frac{\pi}{2}; \ \phi' + \psi' = \theta.$$

Hence $\phi + \psi = \theta + \delta$, and $\phi - \psi = \pi + \delta - 2\,\alpha - \theta$:

From the law of refraction $\sin\phi = \mu\sin\phi'$ and $\sin\psi = \mu\sin\psi'$; and, therefore, $\frac{\sin\phi' - \sin\psi'}{\sin\phi' + \sin\psi'} = \frac{\sin\phi - \sin\psi}{\sin\phi + \sin\psi}$.

Hence $\tan\frac{1}{2}(\phi' - \psi') = \tan\frac{1}{2}(\phi - \psi)\cot\frac{1}{2}(\phi + \psi)\tan\frac{1}{2}(\phi' + \psi')$.

* This may be rendered definite by a pair of cross wires at the focus of the eye-piece, which the images should be made to intersect both in finding the deviation of the refracted ray, and the inclination of the horizon-glass to the line of collimation. The latter is a constant angle; and will not require to be re-determined at every new experiment.

From which, by substitution, we obtain

$$\tan \tfrac{1}{2} (\phi' - \psi') = \cot \tfrac{1}{2} (\theta + \delta) \cot \tfrac{1}{2} (\theta + 2\,\alpha - \delta) \tan \tfrac{1}{2}\,\theta.$$

Having found $\frac{1}{2} (\phi' - \psi')$ by this formula, we have

$$\phi' = \tfrac{1}{2} (\phi' + \psi') + \tfrac{1}{2} (\phi' - \psi') = \tfrac{1}{2}\,\theta + \tfrac{1}{2} (\phi' - \psi'); \text{ and}$$

$$\psi' = \tfrac{1}{2} (\phi' + \psi') - \tfrac{1}{2} (\phi' - \psi') = \tfrac{1}{2}\,\theta - \tfrac{1}{2} (\phi' - \psi').$$

$$\text{Then } \mu = \frac{\sin \phi}{\sin \phi'} \text{ or } \mu = \frac{\sin \psi}{\sin \psi'}.\text{*}$$

Both these values of μ may be calculated, in order to check the accuracy of the observations and calculation.

I have thought proper to describe this method of finding refractive powers, as it involves a new application of the sextant to stereometry, which may often be found useful when other goniometers cannot be obtained; and as it may enable individuals to ascertain refractive powers with considerable accuracy, who might otherwise be prevented from doing so by the want of apparatus specially intended for that purpose.

The applicability of the sextant as a goniometer, is necessarily limited by the graduation of the instrument not extending far beyond 120°; and, therefore, not permitting the angle of a prism, greatly exceeding 60°, to be measured. This range is, however, amply sufficient for determining the angles of such prisms as are generally used for finding the index of refraction.

II. The second method of finding refractive powers, which is now to be explained, was suggested, as has been already noticed, by the use of the sextant; and it depends for its efficiency on the well known optical principle of that instrument, which is, that the deviation of a ray of light, after two reflections in the same plane, is double the inclination of the reflecting surfaces. If, then, two plates of glass A B, C D (figs. 3, 5, and 6) be applied to the faces of the prism *p r s*, as represented in the figures, a ray of light *l m n o* being reflected in the glasses at the points *m n*, will suffer a deviation *n t m* double the inclination of the glasses A B, C D, or double the angle *p r s* of the prism. The angle *n t m*, and also the

* For the investigation of similar formulæ, the reader is referred to Coddington's Optics, chap. vii., or Biot, Traité de Physique, vol. iii. Dioptrique.

deviation $g\ d\ L$, of the ray L $a\ b\ c$ refracted by the prism, may be ascertained by mounting the prism with its glasses in front of the object-glass of the telescope of a theodolite, or other angular instrument, as represented in the figures. The prism being so placed as only to cover part of the object-glass, one half is appropriated to the reception of the refracted, and the other to that of the reflected rays; and the telescope being turned from the object, until the images produced by the refracted and reflected rays appear successively in the field, the corresponding deviations are easily ascertained.

If the surfaces of the prism are sufficiently flat and well polished to afford a distinct reflected image, a better arrangement for obtaining the angle of the prism, which dispenses with the use of the glass D C, is represented in figs. 4 and 5.

The apparatus by which the prism is mounted before the telescope, is represented in fig. 6. R is a collar fitted to the end of the telescope T U, to which it may be fastened by the clamping-screw T. From opposite extremities of the diameter of this collar, two arms N N′ project in the direction of the axis of the telescope, which are furnished at their extremities with the screws M M′, whose points are presented to each other. By means of the motion which the collar R has on the telescope, and by varying the length of the arm N′, which consists of two pieces fastened together with screws S, tapped into *one* piece, and moving in an elongated aperture in the other, the line joining the points of the screws M M′, may be made perpendicular to the limb of the instrument on which the angles are to be measured. The screws M M′ carry, between their points, a ring G H, whose plane is therefore perpendicular to the limb of the instrument; and which revolves freely round the line joining the points of the screws. The object of this motion is to adjust the prism to its position of minimum deviation; and for very nice observations, a slow motion may be given to the ring by the wheel and pinion W V, but this may be dispensed with in ordinary cases. Another ring E F, furnished with a piece of perfectly parallel plate-glass A B, is fitted into the ring G H, so as to revolve about its centre in the same plane with G H; and to F E are attached the pieces K I, furnished with the screws I I, which carry the glass C D. This glass has conical holes drilled in it, for the reception of the points of the screws I I; and having a free motion about the line joining their points, it accommodates itself to the surface of any prism that may be placed between it and the glass A B. The prism, having its faces slightly moistened, is thrust, like a wedge, between the glasses until both are found to be in accurate contact with its surfaces. This is ascertained to be the case by the fluid spreading uniformly between the prism and the glasses, and also by the colours produced by the thin plate of intervening air or fluid. When an accurate contact has been obtained, the prism will retain its place firmly, provided too much fluid has not been used; but if any error be apprehended from its shifting, a little soft wax applied at its base, to the glass A B, will prevent it from altering its position.

It will be sufficient to explain the use of this apparatus with the theodolite, in order to shew its applicability to any other angular instrument of the same kind; and the manner of using it with the sextant is so perfectly analogous to the method of observation already described, as to render any other description superfluous.

In using the theodolite, it is first necessary to make the line of collimation of the telescope parallel to the horizontal limb, which is then adjusted to the plane of the object to be observed, either by raising or lowering the object, or by adjusting the plane of the limb by the parallel plate-screws. The apparatus having been attached to the telescope, the outer ring G H is inclined at a small angle to the line of collimation, and the telescope is turned aside until the image of the object is seen by reflexion from the back of the glass A B. The reflected image is then brought into coincidence with the horizontal wire in the focus of the eye-piece, by turning the collar R upon the telescope, and the collar is made fast by the screw T. The telescope is now pointed 90° from the object, and the ring G H is turned until the reflected image is again visible. If it is now above or below the horizontal wire, it is once more made to coincide with it, by altering the length of the arm N′. The latter adjustment is rendered permanent by tightening the screws S. If the apparatus be in good adjustment, the reflected image of the luminous object should remain on the horizontal wire, at whatever angle the telescope is inclined to the object; and the process of adjustment ought to be repeated until this is the case. It will afterwards be generally sufficient to adjust the instrument by means of the collar R.

The apparatus being adjusted, the principal section of the prism, and consequently the plane of reflexion between the glasses, is brought into coincidence with the plane of the divided limb by means of the ring E F. The refracted image will now be upon the horizontal wire of the telescope; and by moving the ring G H, and following the motion of the refracted image, a point will be found where it stops and begins to move in the opposite direction. By means of the tangent screw, the cross-wires are made to intersect the image at this point, at which the deviation is a minimum, and the angle is read off. The telescope is then turned round, until the reflected image intersects the cross-wires, and the angle is read off. The direct bearing of the object being then taken, the differences between it and the other observed angles will give the deviations of the reflected and refracted rays, which must be corrected for the parallax due to the distance of the prism from the centre of the instrument.*

From these angles the index of refraction is easily calculated.

Let δ = the observed deviation of the refracted rays;

θ = the angle of the prism; and

ϱ = the observed deviation of the reflected rays.

Then, since the prism is in its position of minimum deviation, $\mu = \frac{\sin \frac{1}{2}(\theta + \delta)}{\sin \frac{1}{2}\theta}$; which expressed in terms of the observed angles gives $\mu = \frac{\sin(\frac{1}{2}\varrho + \frac{1}{2}\delta)}{\sin \frac{1}{2}\varrho}$.

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* If the distance of the observed object be great, the correction, in seconds, to be added to an observed angle α will be nearly $\frac{r \sin \alpha}{d \sin 1''}$; where d is the distance of the observed object, and r the distance of the prism from the centre of the theodoli e.

It is obvious, that this method of determining refractive powers is particularly well adapted for fluid substances. The fluid may either be contained in a hollow prism made of parallel plate glass, or a few drops may be retained by capillary attraction between the glasses of the instrument; in either case, the angle of the fluid prism will be ascertained in the manner already shown.

From a considerable number of observations, I have found that the method which has now been described, affords considerable facilities in determining refractive powers. The construction of the apparatus makes the adjustment of the surfaces of the prism quite independent of each other. For the rings E F, G H (fig. 6), and the face of the prism in contact with the glass A B, remain constantly perpendicular to the divided limb; and the face in contact with the glass C D, is directly adjusted by the motion of the ring E F.

The application of glass-plates to the surfaces of the prism also greatly facilitates the process, inasmuch as it renders it unnecessary to have the faces of the prism so highly polished as would otherwise be indispensably requisite. In fact, it is generally sufficient to grind the surfaces smooth with fine emery, when the fluid by which the prism is retained between the glasses will render it perfectly diaphanous. Sir David Brewster, who seems to have been the first to avail himself of this method of making prisms, observes, that, in measuring the refractive and dispersive powers of bodies that are incapable of receiving a good polish, "by cementing upon the two refracting planes pieces of parallel glass with a fluid of nearly the same refractive density, substances like horn, rock salt, and several of the gums, may be rendered perfectly transparent."*

In alluding to the circumstance, that the prism is included between two plates of glass, by whose inclination its refracting angle is ascertained, I am led to anticipate some objections to the accuracy of the process that may suggest themselves to the reader.

It may be thought, that not only will the refractive power of the glass-plates vitiate the result, but also that a source of

* Treatise on New Philosophical Instruments, p. 279.

error will arise from the glasses not applying with sufficient accuracy to the faces of prism. No appreciable error need be apprehended from the refractive power of the glasses, provided they be of the kind used for making the mirrors of sextants; and should any notable imperfection exist, it will manifest itself by the separation of the images reflected from the two surfaces of the defective glass.

With reference to the other source of error, I may refer to the results of actual observation, to shew within what narrow limits it may be confined; and in doing so, I have to acknowledge the kindness of Mr William Nicol, Mr John Adie, and Mr Alexander Bryson, to whom I am indebted for the use of most of the prisms whose angles have been determined by this method.

The angle of a flint-glass prism, belonging to Mr Adie, was taken five times, and the greatest and least result differed by only 50″, the object observed being a street lamp nearly 400 feet distant. The angle of the same prism was then determined by a double observation on the turrets of Trinity Chapel, Deanbridge, at a distance of about ¾ of a mile, and the result differed from the mean of several accurate experiments made some years ago by Mr Adie, by 30″. A plate-glass prism, examined in the same way, gave a result differing from Mr Adie's by 50″, and the refractive indices of these prisms differed from Mr Adie's results only in the fourth place of decimals. A similar agreement exists between many other observations I have made on different substances and the results obtained by former observers, so that I feel quite satisfied with the practical efficiency of the process.

EDINBURGH, 4 DUKE STREET, 10*th June* 1843.

On Solar Radiation.

The experiments mentioned in the text (page 215), referred to a curious inquiry which has occupied my attention for some years, namely, the loss of force which the sun's rays experience in passing through the earth's atmosphere. It might seem, at first sight, an impossible task to determine the comparative measure of the sun's heat, in the state in which it arrives at the earth's surface, and that which it would have attained were the atmosphere wholly removed. Some approximation to such a result has, however, been obtained by a very simple though indirect method. The thickness of air traversed by a sunbeam is, of course, least when the sun is vertical, and greatest when he is near the horizon; at intermediate elevations the heat is intermediate. Now, by comparing the thermometric effect of the sun's rays (which is the object of the actinometer), at several different thicknesses of atmosphere, the *law of extinction* is approximately found, and an inference is made as to what the intensity would be when the thickness of the atmosphere is nothing. This inference will be proportionally more accurate as the observations are pushed to a less thickness of interposed air; and I have shewn in the paper already referred to,* that the previous estimates had greatly underrated the intensity of the unimpaired solar-beam, and had also underrated the absorptive power of the atmosphere, owing to the observations on which they were founded having been made only when the sun-beam had already traversed a great thickness of air, when the law of absorption is very different from the law at small thicknesses.

Now, to obtain observations of solar heat at small thicknesses, we must, in the first place, ascend in the atmosphere, and also use the sun's rays when his elevation is greatest, that is, near the solstice. I mounted the Cramont in hopes of prosecuting these experiments, when the sun had still 21° of northern declination, and after having left below me a thickness of 9000 feet of the densest part of the atmosphere. Unfortunately, as we have seen, these delicate experiments were prevented by indifferent weather.

It will probably surprise many persons to be told, that even when the sun's rays shoot vertically through a pure atmosphere, as between the tropics, they lose in their passage (owing to the opacity of the air) very nearly half their

* Phiosophical Transactions for 1842, page 225.

intensity.* The intercepted heat goes, of course, to warm the air.

The object of this note is, however, to record a different set of observations, performed with an instrument of inferior delicacy to the actinometer, but still capable of yielding very remarkable results,—I mean Leslie's photometer. Its principle may be briefly described as measuring the difference of the heat absorbed by a dark and clear thermometer-ball. It is well known that this instrument gives, on some occasions, results which appear highly paradoxical, but which, if consistent, require to be explained, and ought, therefore, to be distinctly established. My observations with it were directed to two points.

1. To ascertain the effect of the presence of a coating of snow on the ground, in magnifying the apparent Solar Radiation. To this effect has been ascribed the extraordinary force of the sun's rays observed in arctic climates, and also some singular variations from one season to another, supposed to depend on the presence of snow on the ground.† Now, the few experiments which I obtained before breaking my instrument last summer (1842), gave me the following most striking results :—

Surrounded by grass, and exposed to direct sunshine, the photometer indicated,	78°
Exposed upon snow instead of grass, it rose to	121°

The whiteness of the snow is all important in this respect; dirty snow produces comparatively little effect, and so does ice; thus,

The photometer exposed on a dirty part of the Mer de Glace, stood at	70°
Placed upon a neighbouring patch of snow,	140°

This action fully explains the intensity of the effect of fresh snow upon the eyesight. I have myself found that exposure for several weeks to the moderate glare of sun-light reflected from a glacier surface, produced little effect upon me, whilst

* Phil. Trans. as above.

† Edin. New Phil. Journal 1841. A paper by Dr Richardson, with remarks.

I suffered severely from a single day spent amongst the pure snows of the highest summits.

2. On the photometric effect of the diffuse light reflected from the sky, Professor Kämtz first, I believe, announced the startling fact, that *half* the photometric effect of Leslie's instrument is due to the diffuse light of the sky, the other half only being the effect of the direct rays of a bright sun. This singular paradox also manifests itself by the fact, that cloudy weather, if the sun be not itself greatly obscured, apparently increases the effect of the solar radiation. Of the truth of both of these facts, I had also last year sufficient evidence, of which I shall quote one or two examples.

On the 28th June 1842, a warm and clear day, at 6000 feet above the sea, at $1^h\ 40^m$.

The photometer in the sun placed on the snow, stood at .	121°
An alpine pole, an inch in diameter, was then stuck into the snow, so as to throw its shadow on the instrument, thus intercepting the *direct* sun-beam only. It fell to . . .	82°
Leaving for the *direct* effect of the sun-light, ...	39°

the remaining 82° being derived from the reflection of scattered sky-light, and from the snowy surface.

At the same place and time, the photometer, in the sun, surrounded by grass, stood at	78°
Shaded by a stick as before, ...	30°
Direct sun effect,*	8

Now, if we look to 78° as the total effect of the sun's light and its reflection from the ground, in one of the hottest days of June, in a fine climate and 6000 feet above the sea,—it appears to be inconceivably small, when we know that the same instrument often stands at 120° in moderately fine weather in Scotland. What is the reason? The sky does not reflect so much light when it is pure as when it is milky, and its surface being immense, compared to the apparent surface of the

* The difference of this and the last is probably due, in a great measure, to the defect in the principle of the instrument, the momentary increment of heat necessary to maintain the temperature 1° above 120°, being greater than what is necessary to maintain it 1° above 77°, for example. There are likewise other sources of error.

sun (100,000 times greater), a small addition to its reflective power increases the photometric indication more than the thin haze of vapour which produces that effect diminishes the direct sun-light.

Again, on the 29th June, a clear and warm day, in the same position at 11 A.M.—

The photometer standing on snow, ...	114°
Shaded from the *direct* sun, ...	63°
Direct sun effect,	51°

I never observed these effects so strongly as on the 30th of June, a day of the most intense solar heat, when, at a height of 7600 feet, the sky exhibited a deep indigo tint, unusual for that moderate elevation. I was engaged for some hours in making trigonometrical observations, on an exposed promontory of rock, with scarcely any shelter from the piercing sun-beams. At length I was so exhausted as to be obliged to thrust my head now and then behind a stone for protection and relief.

Now, at this time, the photometer directly exposed on the rock to the sun, stood only at	88°
When shaded from the direct sun-beam, it fell to	22°
the smallest result for diffuse atmospheric radiation and reflection from the soil combined which I have witnessed. This, it will be observed, leaves for *direct* sun-heat, ...	66°

which is large, compared with the previous results. It is certain from these experiments, that the photometric effects thus measured bear no kind of proportion to the physiological effects of direct and reflected heat.—*Prof. Forbes's Travels through the Alps*, p. 416.

On the Progress of Ethnology. By Dr Hodgkin. (Communicated to this Journal by the Society.*)

Read before the Ethnological Society, London, Nov. 22, 1843.

The study of man, in its most extended sense, to which the term Anthropology is fitly applied, is a most complicated subject, presenting such various points that it admits of being divided into several departments, each of which may constitute or appertain to a separate science.

The physical conformation of man, and the consideration of the functions of his several organs, come within the province of the comparative anatomist and physiologist.

As an intelligent being, man is a subject for the metaphysician, and, in his compound character of an intellectual animal, is the object of contemplation and study for philosophers of various sects. Some, like Cabanis and Hope, may take a comprehensive view of the whole. Others treat of his progress individually in relation to his education and unlimited capability of cultivation. Others pursue the subject in relation to man as a gregarious animal, and are consequently occupied with the different branches of political economy, social government, and the like.

Man is also studied in relation to the lapse of time in which his race had existed; hence the group of general or particular historians.

Researches of these and analogous descriptions have exhibited man individually and collectively in so great a variety of conditions, as to render it a matter of special inquiry how and to what extent he may be influenced by the circumstances in which he is placed; individually as to diet, climate, mode of life, and inherited peculiarities,—collectively by government, religion, influence of surrounding nations, and dominant prejudices whence soever derived. Lord Kames, Falconer and Herder, may be mentioned amongst the investigators of these points.

* The Ethnological Society of London is formed for the purpose of inquiring into the distinguishing characteristics, physical and moral, of the varieties of mankind which inhabit, or have inhabited, the earth; and to ascertain the causes of such characteristics.— Edit.

When all these various subjects are more or less carefully viewed and considered in relation to man as an animal of a peculiar kind, it may very naturally be inquired, are all these differences to be ascribed to modifying circumstances acting upon beings essentially similar and of the same stock ?

Prichard and Lawrence put the supposed case of a previously uninformed individual seeing in contrast the extremes of colour, and the extremes of civilization and barbarism, by which he would almost necessarily be led to infer absolute distinctness of species. Lord Kames had previously stated that a similar inference must be drawn, were not the declaration of Scripture opposed to it.

Investigation of the innumerable and almost insensible gradations between these extremes, might as naturally induce the opposite idea, which is actually embraced by many of the best inquirers. Irrespective of these extreme views, writers of the highest antiquity have spoken of man as formed into various distinct groups which have been known as separate nations ; some distinguished by their languages, some by their colour, and many by their country, of whom it has been merely known that they inhabit such a territory and possess such and such peculiarities of custom. Facts of this kind are necessarily blended with the writings of historians and geographers from the most remote period down to the present time, and whether separated into a distinct study or not, the description of them has acquired the peculiar and appropriate name of Ethnography or the description of nations.

With the object of exhibiting the mode in which the human race, broken up into more or less distinct groups, is distributed over the face of the globe, various attempts have been made to give the geography of man, just as we have the geography of plants by Humboldt, the geography of insects by Latreille, and the geography of crustacea by Milne Edwards. Thus we have the geographical distribution of man attempted by Zimmermann.

Various Ethnographical maps, of greater or less extent, have been produced in this country and on the Continent, and a comprehensive scheme for the production of an ethnographical map of the world has been projected and commenced by Mr Greenough, of which, it is enough to say, that in design and in the collection of materials, it is well worthy of the author of the geological map of England.

Were the differences which are observed in mankind wholly the result of physical circumstances, we might expect that an ethnographical map of the world would exhibit peculiarities and physical characters bearing some relation to the parallels of latitude, or rather like the isothermal lines and the regions marked by the production of peculiar classes of plants, shewing the combined influence of latitude and elevation. That such is not the fact is abundantly manifest, although much of the materials for the construction of such a map remain to be collected. It is so far from being the case, that we find every shade of colour, from the white to the deepest black, in the same parallels of latitude, and even in juxtaposition with each other. We may further observe, that the individuals presenting these different characters, are very differently affected by the climate to which they are exposed. Are we hence to infer absolute distinctness of race, the one adapted to one climate and the other to another; as the tropical palm is distinct from the northern pine? Such a difference, however plausible it may at first appear, is by no means necessary, since it is equally consistent with probability to suppose that the descendants from one original stock, may, through a succession of generations, have become more particularly adapted for certain climates, which result being produced, the influence of a different climate cannot be equally tolerated; just as the metal iron, according to the particular mode in which it is treated, may be rendered ductile or brittle on the application of heat, and the varieties so produced cannot afterwards be mutually substituted for each other. These and numerous other difficulties have presented themselves to the consideration of those whose attention has been turned to the various conditions and appearances in which man is found on the surface of the globe. The study of this very interesting subject forms a branch of science, to which the name of Ethnology has been given. As I have heard remarked by the great Cuvier, with regard to Zoology in general, so in this particular branch of it, it is obvious that there are two modes in which the subject is susceptible of being treated. By the one method, we should proceed from the original stock or stocks to trace the numerous subdivisions or ramifications into which it has branched out; but to adopt this course, it would be essential that we possessed more numerous and more certain facts than either are or can be within our power of attainment. The oldest ethnographer, as well as the oldest historian, who was also the first inspired writer, has adopted this method; and in his enumeration and description of the nations, tribes, and families, of which he has left an account on record, he has traced them in the descending line from their great primogenitor. Even in comparatively recent times, attempts have been made to adopt this method, but the result has obviously been very incomplete and unsatisfactory. A partial attempt of the same kind was made by many of the ancient profane historians, whose writings are of the utmost value to those who may at present be engaged in the study of ethnology. Though in many instances constrained to admit the pre-existence of a class of men styled Aborigines or Autochthoni, supposed to be originally produced in the region or territory, in consequence of their arrival in it being beyond the reach of history or tradition, they almost invariably relate the accession of other inhabitants derived from some known stock, and who subsequently have been split into different families, whose subdivision and alliances are more or less accurately detailed. Such is the description, meagre as it is, which Sallust has given us of the peopling of Northern Africa. Such is the description of the introduction of Grecian and Asiatic families into Italy.

The other mode consists in endeavouring to trace the nations, tribes, and families of man in a retrograde direction towards their obscure origin. In this process, which is one of great labour and difficulty, and necessarily tedious, it is of the utmost importance not to be hastened by preconceived opinions, or the activity of the imagination, to arrive prematurely at undemonstrated conclusions. A theory may indeed be permitted as a lamp to the enquirer, but he must be careful that it do not lead him astray, like an *ignis fatuus* from *terra firma* to a pathless swamp. Of this description are the attempts which have been made to trace not only various Asiatic nations, but even the North American Indians to the lost tribes of the children of Israel, and the equally improved, but somewhat less improbable doctrine, that the entire continent of North and South America have been peopled by migrations from Asia, by the nearly approximating points of Kamtschatka and the Aleutian Islands.

The views of historians and naturalists, regarding the varieties exhibited by man, were ill defined and irregular, composed of a strange admixture of facts either well or ill observed, rumour and speculation, prior to the important step which was taken by a great natural historian of our own times. Professor Blumenbach directed his attention to this subject at so early a period as to make it the subject of his inaugural dissertation, and it continued to be one of the principal objects of his research through the course of his long life; in which he was enabled to bring to bear upon it his cultivated abilities as a zoologist and comparative anatomist, aided by accumulated well authenticated facts and specimens, which, by his own exertions, and the co-operations of his friends, he collected for its illustration.

Without disregarding the important advantage which a knowledge of the geographical distribution of man must necessarily render to the subject with which he was engaged, and without disregarding the more obvious distinctions marked by colour, he led the way in adopting the form and proportion of the cranium, as affording the most important and distinct indications by which to mark the distinctions and affinities of human races or varieties. In doing this he was not absolutely original, seeing that Camper had touched on the same subject in treating of the facial angle in the groups of inferior animals; and after applying this test in comparisons drawn between these amongst each other, and again between the inferior animals and man, he was led to make the same application in contrasting the varieties of man with each other. Blumenbach perceived the necessity which existed for looking at many characters besides that of the facial angle, and instituted a method for viewing skulls in different directions, for the purpose of contrasting them with each other. Not only may the proportions of the head and face greatly vary, and thereby affect the facial angle, but differences no less remarkable have long been noticed, with respect to the proportionate width of the cranium; and the distinction of long and round heads is probably one of the oldest distinctions which ordinary observation has adopted. The late Professor Blumenbach insisted on these characters, and having laboriously formed a collection of skulls from various nations, he published sketches of them in his *Decades*, accompanied by observations pointing out the peculiarities which, by this mode of observation, he had been led to detect. He therefore placed it in the power of others to pursue the inquiry in the same line, and gave a stimulus to the collection of comparative crania, which are now to be found in all the best anatomical museums, where they render valuable facilities to those who may be engaged in this interesting study. Mainly referring to the cranial characteristics, yet, as before stated, not neglecting the lights of history and geography, Blumenbach was led to establish five principal varieties—the Caucasian or Arab European, the Asiatic or Mongolian, the Malay or Polynesian, the American and the Ethiopic. The genius of that greatest of modern zoologists, Cuvier, was so much engrossed with characteristics of recent and extinct inferior animals, that he cannot be said to have devoted proportionate attention to the varieties of man; yet he did so sufficiently to be led to the persuasion, that, if we must admit certain grand divisions of the human race, determined by their respective localities, it would be necessary to reduce the five mentioned by Blumenbach to three, viz.: the Caucasian, the Mongolian, and the Ethiopian, since he does not find, in the Malay and the American, characteristics sufficiently marked to constitute separate divisions, though he has not determined to which of those which he has admitted they ought to be referred. He was very sensible of the great defect of materials already collected for the complete study of the varieties of man, and observes that the indifference of travellers to this subject has been incomprehensible. It is difficult to give an account of this indifference, but the omission is not, therefore, the less to be deplored.

The obvious relation which the divisions of Blumenbach bear to the geographical distribution of the human race, has probably concurred with Blumenbach's priority as an author, in causing his divisions to be more generally followed than those of any other writer on this subject; yet more extensive and more accurate research has exhibited the difficulty, I might say the impossibility, of reducing all the varieties of man with which we are already acquainted to his five grand divisions. Herder says that there are not four or five but many more varieties which may be recognised; and our countryman Dr Prichard, although admiring and following in the steps of Blumenbach, and adopting the five varieties which he has recognised, has found it necessary to subdivide and add. It has been by no means the major part of Dr Prichard's labour to establish the principal divisions in the varieties of man, which, as he has shewn, are not actually separated from each other by strong lines of demarcation. He has descended into details and pursued the investigation of the subordinate groups, both as respects their actual condition as described by most recent observers, and also their characters as noticed in former periods, whenever he has been able to obtain the descriptions of earlier writers. Though we cannot but admire and value such a work, which nothing but extraordinary ability and industry could have produced, it will readily be understood, that, as respects many, if not all of the subordinate groups, much must remain to be done, not merely in the collection of new facts, which continued observation and research may bring to light; but also in the verification of statements already made by observers, or writers of different degrees of credibility, of which the compiler could not always have the means of judging. The work of Dr Prichard has greatly advanced ethnological knowledge, and has brought the science to that state in which the united exertions of a community of enquirers becomes essential, and, as might reasonably be expected, it exhibits the necessity for those retouches which it has prepared the way for future labourers to make.

The merit of bringing about this kind of co-operation, which may be regarded as an epoch in the history of this science, belongs to the late Dr Edwards, whose attention appears to have been first called to the subject of Ethnology, by the observation of the marked difference of physical characters which he noticed in the population of France, as he was making a journey through the country. He sought to trace these varieties to their origin, and thus to connect them with history. He found the subject not only to expand, but to increase in interest, as he applied himself to the inquiry; and he published a very interesting essay on the subject, in the form of a letter to M. Thierry, in which he records the observations which he made, and the inferences which he had drawn from them. This essay is strongly marked by Dr Edwards's philosophic mind, and is well worthy of perusal and study.

Ethnology very much engrossed Dr Edwards's attention during the latter years of his life, and he designed to write a comprehensive work on the subject, but he has not left the materials which he had collected sufficiently advanced for publication.

He saw the importance of co-operation for the collection of perishing materials, and, acting on a suggestion which he had received, he succeeded in forming the Ethnological Society of Paris, by the union of some of the most distinguished members of the Institute and other savans in that city.

The Parisian Society, under the presidency of Dr Edwards, pursued its labours, and produced an interesting volume of Transactions, and the Committee of the British Association has widely circulated a comprehensive set of Ethnological Queries, which have produced several sets of answers from practical observers. The subject has likewise been advocated in successive years in sectional meetings. In Germany, and also in the United States of America, Ethnology, either as a whole or in its details, is now receiving increased and systematic attention, and some of the collateral sciences are pursued with special relation to it.

Philology, in particular, has taken an Ethnological turn, as will be readily conceded when we consider the remarkable works of Adelung, Vater, Wm. Von Humboldt, Bopp, Balby, and Klaproth, in Germany; of Marsden and Dr Prichard in England; and of Heckewelder and Du Ponceau, in America.

From this very circumstance, Ethnology may appear to be more exclusively within the province of the medical man and the linguist than is altogether desirable for the interests of the study, which really comes within the scope of every well-informed man, and more especially of every traveller.

The conclusion which we may be warranted in drawing from the preceding rapid sketch of the progress of Ethnology seems to be, that this science is in a very similar state to that of Geology, when, after having been the subject of the various theories of Whiston, Liebnitz, and Buffon, it commenced the foundations of its solid superstructure in a rational appeal to facts extensively observed, carefully recorded, and brought to the test of repeated investigation.

Whilst both theorists and exact observers were advancing speculations and producing facts, there existed another class highly estimable for the purity of their intentions and the sincerity of their piety, who imagined that the labours of the geologists were adverse to the interests of religion, and favourable to the attacks of the sceptic. So with respect to Ethnology, the attempt to define the divisions of man, as marked by his visible physical characters, has been dreaded as a refutation of the Scriptural account of the unity of our species.

I am glad to have this opportunity of expressing my firm persuasion, that religion has nothing to fear from the strictest scrutiny of the characters and history of the varieties of mankind, or from the geological study of the globe on which they are placed.

But, in Ethnography at least, we must be content to pursue the train of facts such as we find them ; and when a break interrupts our progress, be very careful not to connect detached portions, until we are satisfied that they really belong to each other.

But what are the points which Ethnology may, at present, assume as fixed, at least until some new conflicting evidence has been brought against them ? and what are the collateral studies by which its progress is to be assisted ?

In the first place, we may assume that man, as a genus as well as species, is one of the most recent of animals. The Paliosomatologist has not only failed to discover any traces of his existence coeval with that of many animals of every class, but he has refuted every pretended evidence which has been produced of his earlier existence.

Let us proceed to a later age, yet still remote from our own.

The investigation of works evidently artificial, and scattered over a wide extent of the earth's surface, has led to the detection of human osseous remains, accompanied by the productions of art, which sufficiently prove, that, at the time, and in the place, at which those bones were animated, human skill was extremely low. With little or no exception, these bones themselves, or the articles that accompany them, prove that the men to whom they belonged must have materially differed from the present inhabitants. They are also, in many instances, beyond the reach of historic information, but they are not on this account beyond the reach of research. We put together with care and expense the remains of an elk or a bear; and why should we not do the same for our own species?

The mere investigation of the skeleton can do much, and may do more; and further light is doubtless to be obtained by a careful comparison of the rude productions of art which will be found with such human remains of the less civilised races of man which may either now exist, or have been but recently exterminated.

So wide a diffusion of traces of the existence of man of the kind to which I am now alluding, of which the numerous tumuli scattered over the plains and downs of the South of England, and which, by conjecture rather than by proof, are referred to the times of the Druids; and similar traces in France, Germany, and Siberia, carry us back to a period which the ethnologist would do well to study as a whole, and, in the defect of written record or authenticated tradition, to gather all the scattered facts which the examination of such relics may have brought to light. Till this be done, almost any attempt to assign to them an origin distinct from that of any of the present families on the face of the globe, must be rejected, not merely as anti-scriptural, but as purely speculative. Indeed, we are already possessed of a few facts, which, so far from favouring the idea of a wide diffusion of an uncivilised pre-Adamic race, make it evident that the barbarous inhabitants of regions now the most civilised, were, in some striking particulars, very similar to uncivilised races now existing, whose origin and connexion with other families of man can be made the subject of research.

When we come to another division of the subject, which appears to be more recent in point of time, in which we have abundantly more numerous data, not only in more numerous relics of human remains, of works of art, of tradition, and even of history, but specimens of language, and of evidences of religious belief, we necessarily find the subject much more complicated; and if the means of arriving at truth are more availing and decisive, the temptations to conjecture, and the inducements to bias opinion, are much more numerous, varied, and powerful. It is this department of ethnology which should take in the whole globe, at the same time that it should promote the most minute local research. It evinces the necessity for a more extensive archæology than appears to have been as yet contemplated. Hitherto the divided efforts of antiquarians have been very much taken up with research designed to increase the local interest of a single nation, or perhaps the part of a nation—a town or village.

It is not my object to underrate such antiquarian labour; but I am anxious to see greater attention to a more comprehensive archæology, which may embrace the scattered materials thus laboriously brought out by local inquirers, and bear the same relation to them that universal botany does to the study of the flora of a particular district.

A single illustration will not merely render my object more intelligible, but will serve to shew that its attainment is not chimerical, and that important progress is already made in the work. Let us suppose the local antiquarian to be confining his patient research to that which our own city (London) may present. He may find the traces of the age of tumuli, and flint axes, in the occasional remains which the workman turns up in sinking a well, or making a sewer. He will find that his predecessors in this research have recorded similar discoveries, and he will perhaps regard the celebrated London stone, and the site of St Paul's, as possessing an interest associated with the same period. He will find there, scarcely less deeply buried, the more varied and beautiful relics of Roman occupation; and, instead of conjecture, he can bring history, inscriptions, coins, and language, as applied to localities, to illustrate this part of his archæology of London. With equal care he will collect, examine, and, by collateral evidence, elucidate the history and relics of Saxon and Norman London, down to the present time; and every fragment which can be made to illustrate its fires, its plagues, and its political occurrences, will be highly prized by him, although he may, to a great degree, exclude from his attention the relics of any other place, and possess but an imperfect knowledge even of English history.

The antiquarian of a wider range will feel a proportionately higher interest in the researches with which these very objects may be connected. He may regard them as a Roman historian, and contemplate the Britons yielding to their invaders, and, through Roman writers, become acquainted with the earliest records of the inhabitants of this country. He may be a profound English historian, and regard the history and relics of London with more than local interest, from their illustration of his extensive subject; or the relics of London may be contemplated by those who study European history through the obscurity of the dark ages, the struggles for the reformation of religion, and the revival of civilization, in which the city of London, and those who have lived and ruled in it, have performed so important a part.

But let us come to the same objects with a still more comprehensive archæological view, or, I would rather say, ethnologically.

Though we may not be able to make out any thing from the isolated fragments, which merely attest that there were in this country men of that age and class which are characterised by the use of stone knives and flint arrow-heads, and whose uncoffined bones were covered under mounds of earth; yet a wider survey will prove that these individuals were a part of a more extensive people; and more careful research in those situations in which successive conquests have produced less change, will lead us to the discovery of other traces of a widely spread race of men, which, by the aid of history, we feel warranted in connecting with the rude inhabitants of London itself, when the first Roman general brought his eagles to the banks of the Thames.

When the ethnologist, brought to take this extensive view of the subject, seriously applies himself to recover all that may yet be gleaned of a people who seem to be lost in oblivion, he will find, as Dr Prichard has fully shewn, that a great number and variety of positive facts may be satisfactorily ascertained. Thus, one author may have left the sketch of personal character, which, as an incidental passage, may seem to be of little importance, but which, when selected and applied to the present purpose, is invaluable. Another detached passage may regard the similarity or difference of language spoken in different parts of the country; and this record, in conjunction with a few names of men, places, rivers, and mountains, preserved in other passages, will enable the philologist to contribute his invaluable assistance to ethnology, and to determine not merely the language, but the dialect of such language, which has been spoken in a particular locality, in the most remote and obscure period of its history.

The interest of the subject does not stop here. When the apparently hopeless task of marking out the former territories and relations of a once very numerous and extensive human family, now reduced to a few detached remnants, has been to a good degree accomplished, a still more remarkable discovery has rewarded the sagacity and patient perseverance of the ethnologist. Similarity and analogy, too close to be the result of mere accident, have been shewn by the philologist to exist between the Sanscrit and the Greek; and the same relation is still more easily shewn to exist between the Greek and the Latin. There is perhaps nothing to excite astonishment in this discovery, since Greece is known to have borrowed a part of its civilization from the East, as the Romans, and those who preceded the Romans, did theirs from Greece and other people to the east of them. The most remarkable fact is that which the philological ethnologist has brought to light, viz. that the ancient Greek is not more certainly allied to the Sanscrit, than is the rude Celtic preserved as a living language in the Highlands of Scotland, the mountains of Wales, and the rudest parts of Ireland.

Thus, thanks to the ethnologist, not only is an affinity discovered between the ancient Londoner and his Roman invader, which neither of them suspected, but between him and those very people on the banks of the Ganges, whom the modern Londoner has invaded and subdued. No example can more strikingly exhibit what the ethnologist may do in discovering and establishing incontrovertible facts, where, at first, it would seem that even conjecture must be lost. But it is a broad sheet which extends from the Hebrides to Hindostan, and which requires to be filled up with countless details. The time which has elapsed between the arrival of the oriental Celt in the British islands and the present day, unmeasured by the chronologist, has produced countless invasions, emigrations, intermixtures of families, and changes in modes of life, by which the plastic animal man has been modified without any record of the changes so brought about. Hence the ethnologist, in demonstrating one grand fact, has opened the way to innumerable questions for his future solution.

If I have succeeded in making myself intelligible in these remarks, which have been designed to shew the increase of interest which results from connecting local and partial details with the comprehensive whole of which they are the part, it will at once be seen that the extension of the same principle is only limited by the habitable parts of the globe, and that if we succeed in establishing a group of mankind stretching from the Ultima Thule of the ancients to India beyond the Ganges, and that, in doing this, the partial absence of an historic record does not present an insuperable barrier, we should not be deterred from anticipating the like success with regard to other portions; although the assistance to be derived from history may be far less, or even wanting altogether. With regard to some of them, the subject of enquiry is obviously more simple. Thus, in the American races, though there are differences which sufficiently distinguish tribes from each other, they are consistent with so striking a unity of type, that the resemblance has been recongnised from the Chippewyans bordering on the Esquimax, to the Terra del Fuejians in the vicinity of Cape Horn.

The thinly but widely scattered inhabitants of the islands of the Pacific exhibit, in a most striking manner, the twofold proofs of community of stock and similarity of language; yet with such differences in each of these respects, as well as in their manners and traditions, as may, if they be timely studied, enable the ethnologist to demonstrate the course which this section of mankind has pursued, and to point out the changes which the lapse of ages has produced, quite as satisfactorily as the learned Niebuhr, and our countryman the late excellent Dr Arnold, have enabled to elucidate in early Italian history. But there is no group of mankind more naturally formed and more clearly defined by geographical limits than that which comprehends the African nations. There is none more beset with difficulties, and yet none which is richer in all the variety of materials to which I have alluded, as enabling the ethnologist, with the aid of coadjutors of various descriptions, to pursue the interesting investigation with the fullest assurance, that, if he cannot arrive at the ultimate truth, he may confidently approach to it, well repaid with interesting discoveries at almost every step which he takes.

Here we find physical characters the most strongly marked; languages innumerable, yet referrible to distinct groups characterized by their words, their structure, by distinct differences of pronunciation unknown in other languages, yet connected by affinities of which the philologist has obtained but a glimpse. Here we have the earliest historic records, whether sacred or profane, to assist us. Here we have the written, the carved, and the painted records of physical characters descending into ethnographical distinctions, which cannot fail to be recognised at the present day, of which it will be a sufficient example to mention a single Egyptian touch, in which are to be found the drawings of Copts, Foolahs, Jews, and the blackest negroes. Here we have various results of art, either preserved from the remotest periods, or still in operation amongst people who have been for ages cut off from the civilized world. Here ethnology may be assisted both by the study of the inferior animals which man has domesticated, and by that of the irreclaimably savage with which he has had to contend. I will not attempt to extend the present sketch, by adverting to other groups which remain to be mentioned; not doubting that the members of the Ethnological Society have anticipated me in the appreciation of the importance of the subject, and are well assured that it needs not only numerous labourers, but that, in a peculiar manner, it requires that a system of union and co-operation should be employed to give efficiency to exertions which must, to a great degree, be abortive, whilst they possess an isolated and individual character. It will be obvious, that a systematic investigation of whatever relates to them as a whole and in detail, will not only be the means of rescuing from oblivion much interesting matter which is in danger of being irretrievably lost; but will also afford the only satisfactory means of arriving at any certain knowledge of the affinities and similarities of the differences and distinctions which may exist between the people comprehended in these several groups. It has been asked, what has an Ethnological Society to do, and how is it to limit and define the labours which it may embrace? The preceding sketch of what has already been done for ethnology, seems to indicate, at least, three kinds of communications which such a society must desire to receive and employ,—

1*st*, Original information regarding any of the divisions or sub-divisions of the different groups of mankind, furnished by individuals who have enjoyed opportunities of collecting them, and in doing which they may be materially assisted by the sets of queries which have been drawn up for the use of travellers for this purpose.

2*d*, Papers of that kind of which the society has already excellent examples in the communications of Dr Richard King regarding the Esquimaux, in which personal knowledge of the people was seconded by a careful reference to original authorities and observations, from the earliest records of the race to the present time, with such constant regard to the past and present geographical distribution, as not only to enable him to confirm or rectify previous statements, but to furnish data respecting the limits and peculiarities of this group, which must be of the greatest value to those who may take up similar investigations concerning other groups geographically connected with them. Proceeding in this way, the society will secure the ground already made, and prepare it for richer cultivation by the accumulation of fresh facts.

A third class of papers may, very usefully, regard the method of investigation, the nature of the observations which are required, and the inferences which may be drawn from the materials furnished by those who have been engaged in different fields.

The cultivation of this branch of the subject will be like furnishing new formula to the mathematician, and new tests to the chemist, by the aid of which fresh observations may not only be made amongst groups of men of whom we are barely conscious of the existence, but even amongst those who may have been known for the longest period.

It is obvious that the subject of ethnology may be studied both analytically and synthetically. For the first, it is necessary to be in possession of criteria by which we may safely infer from physical characters the intermixtures or affinities which may exist, where we have little or no assistance from history or tradition to guide us. Even where these aids have been accessible comparatively little has been done.

With regard to the other, the synthetical branch, still less has been attempted. It is certain that there is abundance of curious and important matter to be made out, by applying this mode of investigation to our subject; since there are indications that certain combinations, of which the elements are known, have produced physical characters resembling those of groups of which the origin is most obscure. Not to mention minor examples which have fallen under my own observation, and which, although they were sufficient to suggest the value of this mode of enquiry, I have not had the means of carrying out; I would cite, by way of illustration, the more striking example which is furnished by a mixed race, resulting from the alliance of the woolly-headed African Negro with the stiff and straight-haired Indians of South America, which present a remarkable resemblance to some of the Australian natives.

The opportunities for examining a great variety of intermixtures, are more accessible at the present day than at any former period. The slave trade has conveyed Negroes from different parts of Africa, possessing widely different characters, to North and South America, to the West Indies, and to different parts of South Asia, where they have been blended with numerous other races in various proportions.

Malays have been transported to the Cape, there to be mixed with Europeans and Caffres, Hottentots and Negroes. The Hill Coolies of Asia have been conveyed to Australia, the West Indies, Mauritius, and Demerara; and, in a few instances, the Chinese, so long immovably attached to his native lands, has been induced to emigrate to a greater or less distance, and is perhaps already cultivating tea in America.

And lastly, there is scarcely a spot inhabited by man in which the proofs of the libidinous propensities of the white man do not exhibit the result of the intermixture of the European variety with those of every shade of colour, and of every diversity of cranial development. The interesting results of this synthetical process are not confined to physical characters. It is often asserted that they are to be seen in intellectual and moral qualities, though no systematic inquiry has been attempted in this direction.

Very great changes must likewise be effected in language through the same process, and these philosophical experiments are well worthy the attention of those who are engaged in the general investigation of language.

Lest it be thought that I exaggerate the interest which belongs to the inquiries included in ethnology in its most comprehensive sense, I will merely ask your attention to the following short quotations. Lawrence observes in his Lectures on Man, "It is only of late years, and principally through the labours of Blumenbach, that the natural history of man has begun to receive its due share of attention, and I have no hesitation in asserting, that, whether we regard the intrinsic importance of the questions that arise, and their relation to the affinities, migrations, and history of nations, or advert merely to the pleasure of the research, no subject will be found more worthy of minute investigation."

It is of the greatest importance, in a philosophical point of view, to obtain much more extensive information than we now possess of the physical and moral characters. A great number of curious problems in physiology, illustrative of the history of species, and the laws of their propagation, remain as yet imperfectly solved. The psychology of these races has been but little studied in an enlightened manner, and yet this is wanting, in order to complete the history of human nature and the philosophy of the human mind! How can this be obtained, when so many tribes shall have become extinct, and their thoughts shall have perished with them?

Route to India. By Charles Maclaren, Esq., F.R.S.E. With a Map. (Communicated by the Author.)

There has just appeared an interesting and well-timed pamphlet, in thirty pages, entitled "Inquiry into the Means of Establishing a Ship Navigation between the Mediteranean and the Red Seas; with a Map. By James Vetch, Capt. R.N,, F.R.S. London, 1843," in which the author has called attention to a subject of much importance. The British Government has the interests and happiness of a hundred millions of Asiatics confided to its care; and it is evident, that, to execute the trust thus devolving upon it efficiently, rapid and frequent communication with India is indispensable. By the application of steam-power, and the adoption of the route through the Mediteranean and Red Sea, in lieu of that by the Cape, a voyage of four months has been shortened nearly to a voyage of one, and India has been brought, morally and politically, four times nearer to England. There is now nothing to prevent a naturalist, a merchant, or a statesman, from paying a visit to India during the summer, spending six weeks in the country, and resuming his labours at home within the compass of four months. The beneficial effects of this facility and speed of communication upon our commerce with India are obvious. To the British Government, too, the advantage is immense. When a country is so distant from those who rule it, that eight months intervene between the writing of a dispatch and the arrival of an answer, it is evident that the administrative agents must act on their own discretion in nine cases out of ten, and frequently even in such important matters as questions of peace and war. Hitherto the British Government and the Board of Directors have not, properly speaking, governed India; they have merely had the privilege of naming its governors; and upon the personal character of these men, their wisdom, their foresight, their vigilance, the course of events have chiefly depended. So true is this, that the late Mr James Mill, a high authority, stated it as his opinion, that the fate and fortunes of India would have been nearly the same, although not one of the many voluminous instructions issued by the Directors and the Board of Control had never been penned. It ought not to be so. Abuses have, no doubt, grown up in India, and blunders have been committed, which would have been prevented by such a close superintendence on the part of the home authorities, as they will now be able to exercise.

The most defective part in the present line of communication is that between Alexandria and Suez. The passage from Bombay to Falmouth is generally accomplished in a calendar month, or a day or two more, and the distance is, pretty correctly, 7000 English miles. The journey from Alexandria to Suez, by the present route, is about 240 miles in length, and occupies, we believe, seven or eight days; and thus nearly one-fourth part of the time is consumed in passing over one-thirtieth part of the distance.

We learn from the Parliamentary Report of 1837,* that a railway from Alexandria to the Nile, and another from Cairo to Suez, was projected by the Pasha, and he had even procured the rails from England for part of the work. Neither, however, has yet been executed, or even, we believe, commenced; and if executed, they would not shorten the time spent in passing from Alexandria to Suez by more than two days, or three at the most, because they make no change in the most tedious part of the journey, that from Edfah to Cairo by the Nile.

The ancient canal of the Ptolemys was described in the *Edinburgh New Philosophical Journal* for October 1825, from a survey given in the *Description de l'Egypte.* To this article Captain Vetch repeatedly refers. The canal passed from the head of the Red Sea through the Bitter Lakes to the Serapeum (see the Map), and thence westward by a

* "On steam-communication with India," 15th July 1837.

valley, called Wadi Tomylat, to the eastern branch of the Nile at Babastis. The French, when masters of Egypt, proposed to restore this canal, and also to connect it with the Mediterranean by a branch passing from the Serapeum northward to Tineh, which is near the ancient Pelusium. It was intended to be about 18 feet deep, and the expense of both works was estimated only at L.691,000.

Nature presents extraordinary facilities for the execution of such a work. The mean level of the Red Sea was found by the French engineers to be nearly 30 feet above that of the Mediterranean, and a tract of low land extends across the isthmus, the whole of which, except a breadth of a few miles near Suez, is actually many feet under the mean level of the Red Sea. This sea would, therefore, serve as an inexhaustible reservoir to the canal, and the descent of 30 feet on a line of 75 or 80 miles, would give the current of salt-water a sufficient force to excavate and scour a navigable channel in the Mediterranean at Tineh.

Captain Vetch proposes that a canal should be carried from the head of the Red Sea direct to Tineh, without communicating with the Nile, and he has marked out three lines. The first, No. 1 in the map, runs right northward from Suez nearly in a straight line. The second, No. 2, passes through the Bitter Lakes to the Serapeum, and thence to Tineh. The third passes through the Bitter Lakes and the Lake of Themsah, and thence to Tineh. The difference in the length of the three lines would be trifling, but it would require a detailed survey to determine which was preferable. He proposes that the canal should be 21 feet deep, 96 feet wide at bottom, and 180 at the water-line at top. He calculates that the fall of 30 feet would give the current a velocity rather exceeding two miles per hour, and he estimates the cost of the work at L.2,000,000 sterling.

Such a canal would be productive of immense advantages. First, it would save five or six days on the transit through Egypt. The ship, instead of stopping at Alexandria, would proceed 200 miles farther east to Tineh. This would consume one day; another would carry her from Tineh to Suez through the canal. At present passengers and goods are unshipped at

Alexandria, put into boats and carried to Edfah by the Canal Mahmoudieh, transferred at Edfah to a steam-boat, which carries them up the Nile to Cairo, landed there, and then conveyed on the backs of camels 90 miles through the desert to Suez, where they embark in the steamer which carries them to Bombay. All this trouble and expense would be saved. But the second and greater advantage is, that the route by Egypt, which, owing to the inconveniences just mentioned, is only used for passengers and dispatches, would then be a great, perhaps the principal, thoroughfare for merchandise to and from India. Sailing vessels could be taken through the canal by tugs, and the Bombay steamers would receive coal by the Mediterranean, at a much cheaper rate.

We subjoin Captain Vetch's estimate, with his concluding remarks, and strongly recommend the pamphlet to public attention.

Estimate of the Expense of constructing a Canal between the two Seas, on Line of Project No. 1, *distance being seventy-five miles.*

Canal 21 feet deep, 96 feet wide at bottom, and 180 wide at top at water line; giving a sectional area of 322 square yards. Supposing the ground to be nearly level, and supposing it subject to slight depressions on the line connecting the surface of the two seas equal in amount to the slight elevations above the same line, then the quantity of excavation would also amount to 322 cubic yards in each yard of distance; and, from all accounts, the surface of the country must be pretty nearly as we have assumed it. The soil is light, yet, by several accounts, tenacious enough to stand without walling; and the absorption of water by the ground and the air being so great as to leave dry hollows from 20 to 54 feet below the level of the Red Sea, there is little danger or expense to be apprehended from the influx of water by springs or otherwise; and, under these conditions of the country, I should consider 8d. per cubic yard as a fair price for the excavation; for, though wages in the Levant may be only one-fifth of what they are in England, I do not expect that more work would be performed for the same money.

The total length of the canal being 75 miles, or 132,000 yards, the total quantity of excavation would be 42,504,000 cubic yards.

Estimate of Expense.

42,504,000 cubic yards of excavation, at 8d., .	L.1,416,800	0	0
Masonry in 64 gauged ribs and sundry works, .	60,000	0	0
Works at the two extremities, in piers, dredging, &c.,	200,000	0	0
	L.1,676,800	0	0
Contingencies, one-tenth, ...	167,680	0	0
Sundry works not enumerated, one-tenth, . .	167,680	0	0
Total,	L.2,012,160	0	0

From the above it will be seen that the cost will amount to about two millions sterling; and it would be difficult and fruitless to attempt any nearer estimate, until we are in possession of more detailed and precise data. No doubt, if the section of the canal was diminished, the cost might be reduced most materially; but my opinion is, that the size assumed will be required to give the sea river the momentum required to preserve its mouth navigable, and to admit with freedom the traffic which may be expected to pass between the two seas.

In the foregoing calculations, it has been assumed, that the straight line across the desert would meet with no serious natural obstacles; but if such should be found to exist, and we are driven westward and obliged to adopt the basin of the Bitter Lakes as a portion of the navigable channel, I should prefer running the line from Serapeum straight to Tineh, by project No. 2, a distance of forty-seven miles, which, together with thirteen and a half miles between the Bitter Lakes and Suez, gives sixty and a half miles of canal for construction. But this, I fear, would effect no saving in the estimate, as, from the great evaporation and absorption of the water of the Bitter Lake when filled, the channel of thirteen and a half miles from Suez would have to be nearly doubled in capacity to maintain the lake at the required level, and to preserve the salt water river flowing out of it at a constant and equable velocity; and with such an arrangement the lake might become the medium of absorbing the tides of the Red Sea, and of furnishing a stream issuing with a constant level and velocity. But I am strongly impressed with the idea that the basins of the lakes and lagoons lying between Suez and Lake Menzaleh have, from offering an apparent facility, drawn all former attempts at connecting the two seas from a truly permanent, effective, straight, and controllable channel, to one amongst shifting sands and unequal influences of several kinds, which have ended in defeating the object sought for.

In an economical point of view, it would not be desirable to convert the basin of the Bitter Lakes into a sheet of water. Its surface lies be-

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low the level of the Nile, and admits of being irrigated. The extent may be estimated roughly at twenty-seven miles by six, or 103,680 acres. And if the Nile was admitted pretty freely to flood it for several years, and afterwards more sparingly, for the purpose of irrigation, it is reasonable, that, with the slime and water which could thus be supplied, the tract might become highly valuable for tillage; and when the soil became fixed with a fruitful vegetation, it would be easy and useful to prolong the canal of the Long Valley or Wadis upon a uniform level through the distance occupied by the basin of the Bitter Lake, as a canal of irrigation, which might also serve for navigation to the small craft of the Nile to the vicinity of Suez, where, by means of locks, it might be connected with the great canal of salt water proposed between the two seas.

Whichever line be adopted to form a ship navigation between the two seas, the cost would not be far short of two millions sterling; and it is pretty clear, with such an expected outlay, neither states, rulers, nor companies would venture on the undertaking without some sufficient guarantee that the cost would be indemnified by the profits of the undertaking. Unfortunately, at present, it is not easy even to form an approximate estimate of the commerce that would pass through the channel; but, converting the Red Sea into a strait, or open channel, as this measure would do, it is obvious that this new passage would connect the whole shore of the Mediterranean with the east coast of Africa, and with the shores and islands of Asia, by a new route, and open a common highway of commerce between a greater extent of coasts than any other channel on the face of the globe.

In the years 1832 and 1833, the average tonnage from and to Great Britain with all places eastward of the Cape of Good Hope, amounted to 285,000 tons; and if we assume that the whole traffic of Europe, including that of Great Britain, passing through the Suez canal, would be three times the above quantity, we should probably be under the truth; and that assumption would give 855,000 tons annually, or about one-half of the tonnage passing the straits of Dover and Calais.

It must however be obvious, that, in opening a navigation so much shorter than the old one, and which consequently might be performed in much smaller vessels with less costly equipments, a great impulse would be given to trade in the new direction; and that entire new sources of commerce would be opened between the places adjacent to each extremity of the Red Sea, but which could not, under present circumstances, be attempted with any hopes of success from the length of voyage involved; and, with these considerations, it will not be deemed unreasonable to expect, that the commerce passing through the canal annually, would, in a short time, amount to one million tons, and might eventually reach two millions of tons; but restricting the estimate to one million tons, the following result would be obtained:—

Interest on two millions capital at 5 per cent., .	L.100,000 0 0
Management, and keeping works in repair, .	10,000 0 0
Toll to the Ruler of Egypt, ...	10,000 0 0
	L.120,000 0 0
Duty on one million tons, at 2s. 4⅘d., .	L120,000 0 0

So that, whatever greater traffic might arise, or whatever higher rate of duty it might be deemed prudent to exact, would operate as a bonus on the interest of 5 per cent.

Again, the official value of the exports and imports from and to Great Britain, with places eastward of the Cape, in the year 1828, amounted to sixteen millions sterling; and if we assume this as a third part of the amount of the imports and exports of all nations passing annually through the canal of Suez, we obtain forty-eight millions value on the amount: and taking the points into consideration stated in respect to the tonnage, we may estimate the annual value in round numbers at fifty millions sterling, the duty on which at ¼ per cent. would yield a revenue of L.125,000 per annum.

A good deal is alleged by those trading from Britain to the East Indies against the policy of any part of the British nation lending patronage to such an undertaking, which, it is presumed, would benefit the countries bordering the Mediterranean more than our own; though, if the canal in question would be the means of most materially shortening the distance between the two most important portions of the British Empire, little doubt can be entertained of the benefit conferred on the extensive commerce of the two countries, even though some other nations would receive a greater proportional advantage in the accomplishment of the measure; and though the commerce of other nations might increase in a greater ratio than the British, still all would participate in facilities to be obtained; and in the case of war arising, it is but too obvious, that the power possessing a naval superiority has the means of closing such a channel of commerce to its enemies, by stationing cruisers at each extremity. So much may be urged with a view of removing the prejudice of British interests against the measure; but it will readily be believed, that if the British fail to patronize the undertaking, other nations and powers will do so shortly: and it is therefore manifest, if British subjects were chiefly concerned in advancing the capital, and in executing and managing this great work, it would be vastly more for the benefit of Britain, than if any other nation or government lent their resources. But, undertake it who may, it is most probable that both the funds and the energies of execution will come from this country; and it is too probable, that if the measure is executed by any other parties than British, the work will be upon a cheaper and less effective plan of navigation, permitting only small craft to navigate, unfit for British commerce in the East, though sufficient for small traders in the Mediterranean, who would consequently, in such a case, reap the entire benefit. I am decidedly of opinion, that British capital and British energy would alone execute the work in a truly useful and permanent style. But the measure is daily becoming so much more obvious as one of practical facility, that it cannot long be postponed in some shape or another.

The conclusions may now be recapitulated in general terms :—

1*st*, That a ship canal between the two seas, which contemplates an extended commerce between the countries of Europe and the Indian Ocean, should be free from the effects of all fluctuating causes, arising from inundations or floods, &c.

2*d*, That it should be a measure irrespective of the commerce of Egypt and the Nile, or rather that it could not combine these objects in the same measure with any good results; though it would be the means of greatly improving the commerce of Egypt by accessory measures.

3*d*, That the mean fall from the level of the Red Sea to that of the Mediterranean (say 30 feet) is sufficient to keep the artificial channel clean, if the fall be properly economized ; and also that it would be able to preserve its mouth in the Bay of Pelusium in a navigable state at all seasons.

4*th*, That a navigation of still water with locks could not be long maintained with advantage, under all the circumstances of the case.

5*th*, That a broad and deep stream, like that of the Dardanelles, could not be produced by natural operations, assisted slightly by art; but that the attempt would be pregnant with mischief in some quarters, and result in disappointment.

6*th*, That a direct and perfectly controllable channel of a uniform size and shape and incline, would be the safest and most appropriate undertaking of which the circumstances permit, and under the imperfect information we possess.

It must, however, be confessed, that no definitive opinion can be given, or very satisfactory estimates assumed, until a new and detailed survey, having the express objects in view, is completed, comprehending the necessary levellings and borings and maritime surveys of the ports at the termini of the canal.

With respect to the land survey, were all the necessary persons and means duly prepared to commence operations in the beginning of October, it is probable the investigation might be completed in the beginning of the following May, and a true solution given to this great geographical, commercial, and engineering question.

As mankind multiply and make progress in the arts and civilization, new wants arise, and the ingenuity and industry of man is taxed to discover new sources of wealth, maintenance, and occupation ; and we find, under the dispensations of an allwise Providence, that at suitable seasons resources are unveiled which have been long provided but concealed until the fit occasion presents itself. Amongst the numerous administrations of the same wise and merciful design, it is not unreasonable to believe that the completion of navigable channels across the isthmuses of Suez and Darien are enterprizes amongst the events designed to minister to the growing wants and improvement of the human race.

Report of the Researches of M. Agassiz during his last two sojourns at l'Hotel-des-Neuchâtelois, upon the Lower Glacier of the Aar, in the years 1841 and 1842. By M. E. Desor.

(Concluded from Vol. xxxv., p. 313.)

Observations upon the Composition of Ice.—Observations upon the composition of ice had been demanded from all quarters, on account of the intrinsic importance of this subject, and on account of the very contradictory opinions which had been published regarding it. Accordingly, when we returned to the glacier of the Aar, in the year 1842, we took along with us a complete *armamentarium* of tubes, probes, weights, balances, and other apparatus of this kind; and M. Nicolet, in concert with M. Vogt, undertook the conduct of these observations. A kind of laboratory was constructed for the purpose, near our hut, to the north of it; and the day after our arrival M. Nicolet commenced operations.*

From the observations so made, it follows that the blue ice really contains the smallest quantity of air. It is true that the ice taken from the Gallery did not contain much more; but it ought to be remembered that there the entire mass of the glacier is as it were transformed into blue ice. Lastly, the white ice, which owes its dull appearance to the numerous air-bubbles it encloses, contains much more air than the blue ice. M. Hugi, as may be known to many of our readers, had previously discovered that, when ice is put into an enclosed bell-jar, supplied with a tube which is plunged into a mercurial trough, the mercury ascends in the tube during the course of the night, and descends again during the day; and from

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* For an account of these Experiments, see Jameson's Journal, vol. xxxiii., p. 401.

this fact, following his own mode of argument, he concluded that glaciers were endowed with a kind of respiration similar to that of organized beings, so that they inspired air during the night, and again expired it during the day. He endeavoured to classify this phenomenon in the natural order of physical facts; and hence he repeated this experiment upon air, then upon air and water, because these bodies enter into the composition of the ice of glaciers, and lastly upon water alone. The result which M. Nicolet obtained was that which might naturally have been expected; for he found that the rise of the mercury in the tube was nothing more than the result of the contraction of the air, occasioned by the greater cold of the night, and also, that it was the dilatation of the air, by the heat, which made it descend during the day.

Upon carefully repeating, however, this experiment of M. Hugi, in somewhat modified circumstances, we one day obtained a diametrically opposite result. A bell-jar, of the capacity of a decilitre, was filled with porous ice; it was then carefully stopped, and made to communicate with a mercurial trough, by means of a graduated tube whose diameter was four millimetres, the divisions being millimetres. The apparatus was carefully secured from the direct action of the solar rays, and surrounded with the powdery mass composing the nevé. For many hours nothing was observed in the tube but a slight depression of the mercury; but about mid-day, with a temperature of + 14°. 5. C. (58° F.), the mercury rose 1.5 cent. (2°.7 F.). During this time the ice was melting. In the course of the evening the mercury rose to the extent of 2 cent. (3°.6 F.), with a temperature of + 13° (55°.4 F); and the ice was now nearly all melted. The apparatus now continued stationary for several hours, and at sun-set the total ascent was found to have amounted to four centimetres. This experiment was repeated on many occasions, night and day, upon the nevé and on the ice, and always with the same result, so that we were abundantly supplied with the proof that a contraction is produced in the day-time during the gradual melting of the ice. This phenomenon, which is in direct opposition to the observations of M. Hugi, is only explicable upon the difference which exists in the density of ice and of water;

ice in assuming the liquid form, diminishing its volume, and thus occasioning the ascent of the mercury in the tube. Next morning, when all the ice was melted, the mercury was found to have still ascended; but on this occasion it was due solely to the contraction which the water and the air had undergone from the cold of the night, for as the heat of day prevailed, the mercury recommenced its descent; and these variations continued, after the same fashion, for many consecutive days. Hence it follows that the effect of the external temperature is very different, according as it acts upon water and upon ice. The ice in melting diminishes in volume, and hence the apparatus indicated a contraction of the mass during the day, so long as there was any ice in the jar. But the ice once thoroughly melted, this same temperature, which before caused the melting of the ice, now dilates the water produced by that melting, and causes a fall of the mercury during the day, and a rise during the night. It necessarily follows from this, that whatsoever of a mysterious character M. Hugi imagined he discovered in these facts, assigning a species of vitality to the glaciers, is to be viewed as wholly chimerical.

M. Hugi moreover alleged, that, during the night, ice absorbed the humidity of the air, and disengaged it during the day; and in this phenomenon he perceived another proof of the vitality of glaciers. On the contrary, however, M. Vogt has determined, by accurately weighing it, that ice exposed to the air, in a hot day, when the hygrometer indicates great dryness of the air, loses much by evaporation; which evaporation is in the direct ratio of the surface exposed to the atmosphere, and is equal in all kinds of congealed water, whether it be snow or the matter of the nevé, whether it be white ice or blue. On the other hand, when the dew is abundant, by the occurrence of nights at once cold and clear, ice is decidedly increased in weight, but in the direct ratio of the surface, and this increase is more considerable in proportion as the dew has been more abundant. Every morning, after a clear night, the surface of the glacier is found covered with a coat of dew. When the cold is severe, this dew is transformed into a coat of rime or hoar-frost reposing on the moraine, whilst on the surface of the ice it produces a delicate coating of remarkably pure ice. That the precipitation of these watery-vapours is the sole cause of the increase of weight the ice acquires during the night, is clearly proved by this circumstance, that, during the prevalence of cold weather, when the temperature of the day differs but little from that of the night, any pieces of ice which are freely exposed to the air undergo no difference in respect to their weight. The variations, then, which M. Hugi has observed in this matter, are entirely due to the drought or moisture of the atmosphere, and the variations of the temperature.

Observations upon the Crevasses.—At first sight it appears strange that the crevasses, which of all the phenomena of glaciers are those which have been most frequently described, and have most engaged the attention of travellers and the public, are those concerning which we possess the least accurate information. The crevasses, as every one admits, are the result of an internal tension. But the inquiry still remains, What is the cause of this tension? and this is the problem which remains to be solved. Repeated observations at different periods of the year, and under different atmospheric conditions, can alone sufficiently instruct us upon the point, and enable us to decide among the various explanations which have hitherto been proposed. According to our own observations, the probability is, that few crevasses are formed except during the summer; an opinion which coincides with that maintained by those mountaineers who are most familiar with the changes of the glaciers. During our sojourns in the years 1840 and 1841, we often heard reports, occasioned by the glaciers, during the night; but never actually witnessed their formation. The summer of 1842 appeared to have produced a great number; for not only did we hear many detonations during the night, but on many occasions we saw them open under our eyes. For an account of these observations, see as under.*

From these observations, it follows that the formation of the crevasses occurs chiefly during those cold nights which succeed to very wet and humid days,—a fact which appears decidedly to support the theory of infiltration. At all events the sudden

* Jameson's Journal, vol. xxxiii., p. 402.

formation of crevasses, which propagate themselves with extreme rapidity over an immense extent, appears to be a strong objection against the theory of semi-fluidity. How, in fact, is it possible to conceive a tension and rigidity, such as the circumstances require, if we once admit that the glacier is a semi-fluid body, moving after the manner of torrents of lava? Mr Forbes attempts no explanation on this point, and puts the difficulty aside.

I have elsewhere insisted* upon the differences which exist between those crevasses which are found in the middle and lower parts of a glacier, and those which are encountered upon the plains of the loftier snows. These latter are much broader, and are generally of an elliptical shape, wide in the middle, and contracted towards the extremities. They have usually also a tendency to close over at the top, and are often even covered over with a coating of snow.

There is another kind of cavity which has not hitherto been noticed, and whose formation is not less remarkable than that of the crevasses; I allude to those which may be called the *Meridian cavities*.

When we traverse a glacier, which is uniform, and but little inclined, as, for example, the two glaciers of the Aar and the glacier of Aletsch in their upper and middle parts, the glacier of Zermatt, that of Zmutt, and many others, we encounter on their surface a number of small cavities or holes, the bottoms of which are covered with gravel, and which are usually from half a foot to a foot deep, with a corresponding breadth, and a length extending from a foot to a foot and a half, and sometimes to two feet. With these cavities we were quite familiar, from having encountered many, but we had never bestowed any particular attention upon them. Our friend, M. F. Keller, was the first who was struck with the uniformity of their shape. He remarked, that they were all semicircular, and had their arc turned to the north, and the chord of the arc to the south. He also remarked, that to the south of each cavity, there was a heap or hillock of ice, and that the greatest depth of each cavity

* Bibl. Univ. Nov. 1841., (vol. xxxvi). Ascension de la Jungfrau. Also Jameson's Journal, vol. 32.

was towards the north. With these data he set to work to account for this particular arrangement, which he explains in nearly the following terms:—When a little gravel, from any cause, accumulates behind an elevation upon the surface of a glacier, it by degrees sinks down in the ice, and this produces a small hollow, or a miniature kind of basin. But as gravel absorbs much more caloric than ice, it follows, that it will be on the side where the sun's rays strike with the greatest intensity, and for the longest time, that this basin will most enlarge, and the gravel sink the deepest. Now, this side must necessarily be the northern, and hence the reason that every basin has its convexity turned to the north.

These small basins prove not a little useful to the mountain traveller, in affording him facilities for piloting himself with considerable certainty in spite of fogs, and even of ascertaining the hour of the day, however obscured the sun may be. For these objects, he has only to place his pole, or walking-staff, over the cavity, in such a way that it touches, on one side, the summit of the small icy elevation, and, on the other, the summit of the arc, and the line which his staff forms, will lie precisely north and south. A line at right angles to this, will consequently lie east and west. The meridian being thus known, it will be easy for him approximately to learn the hour, from the angle which the sun forms with the meridian line. It is, accordingly, on account of the facilities which these small excavations present on this point, that we designated them *Meridian cavities.* In Germany, they are called *Kellerlöcher*, in honour of our friend M. Keller.

Observations upon Temperature.—The experiments upon temperature were those which M. Agassiz prosecuted most zealously during his sojourns in the years 1841 and 1842. Not only were the variations of the external temperature made the subject of daily observations, often repeated from hour to hour, but he was especially anxious to observe what was the state of the temperature in the interior of the glacier. We have, on a former occasion, remarked, that many shafts or bores were cut with this object in view, to the depths, one of 100 feet, and another of 50, and several to the depths of 15, 20, and 25 feet. The bore to the depth of 200 feet, whereby we had hoped to reach the bottom, in the year 1842, was also exclusively used for this purpose, during the last eight days of our sojourn. Every evening we introduced a self-registering thermometer to the bottom of each of these bores, respectively 200, 100, 50 feet deep, and occasionally into a fourth 20 feet deep. There they were allowed to remain during the night, every care being taken, by means of such things as sheep-skins, blankets, &c. covered over again with plates of ice, to prevent the ingress of the external cold. Next morning, usually about six o'clock, MM. Agassiz and Girard went to examine the thermometers, both those on the surface, and the others in the ice. These experiments were repeated in 1841, for a period extending to nearly six weeks, and in 1842, during two consecutive months. Often also we reintroduced the instruments, leaving them during the day, and examining them in the evening, before the temperature fell below 32° F. We will not here, of course, enumerate the details. Suffice it to state, that the general result has been confirmatory of what the observations of the year 1840 had shewn, namely, that the temperature is nearly constant in the interior of the glacier. We have scarcely seen it oscillate beyond the limits of three-tenths of a degree; in other words, the self-registering thermometers have never indicated a higher temperature than 32° F., or a lower temperature than − 0°.3 (31°.46 F.) and this when the external temperature was as low as − 5° (23° F.), and even 6° C. (21°. 2.) Generally, the index stood precisely at 32° F.

But M. Agassiz was unwilling to limit himself to daily observations; for however important these might be, they supplied the temperature of the glacier only at a determinate epoch, namely, at that corresponding to the hottest season of the year. But where were the grounds for supposing that the temperature was the same at the other seasons of the year? And admitting that the night cold did not exercise any marked influence upon the interior of the glacier, did this authorize us to conclude, that the colds of winter, so severe and prolonged in these high regions, have not a greater influence upon the temperature of the glaciers? These objections had been foreseen in 1841, and M. Agassiz has published an account of the placing of the thermometers, in Jameson's Journal, vol. xxxiii. p. 277.

Made wise by previous experience,* we determined to be more careful in our use of means for withdrawing the second thermometrograph, which was twelve feet beneath the first. Having noticed the quantity of ice which daily disappeared from the surface, M. Agassiz determined to postpone any attempt, until near the termination of our stay. In fact, when we again thought of the undertaking, three weeks later, the surface of the glacier had experienced a sinking of many feet of ice, and our thermometrograph was so much nearer the surface. We now set to work, using the most cautious and sure means, and carefully superintending the workmen who were engaged in the service. M. F. Keller of Zurich, who at the time was of the party, imagined, that by heating iron bars, which might be introduced into the tin case, we might possibly easily melt the cylinder of ice. Two iron bars, therefore, an inch in diameter, were at the same time introduced into the tube, and around them was constructed, by means of large slabs, a kind of fire-place, in which a fire was lighted. The two bars were thus in the midst of a brazier; but although they were thereby heated to a very considerable temperature, it was found that they sank exceedingly slowly, so that at the close of the day, they had not melted even two inches of ice. This proceeding, therefore, was much too tedious, as well as very costly, from the quantity of wood which was required. We were forced, then, to return to the use of boiling water; and with this, it required no less than five days to melt the ice in the ice-cylinder, until we reached the thermometrograph. At last, however, we succeeded in detaching it, and it was with the keenest impatience that we waited for its being brought up. Our satisfaction was great when it reached us unhurt. The index was at — 0°.3. C. (31°. 46 F.) We immediately set to work to verify the zero point of our instrument, which we found perfectly correct. It is worthy of remark, that this indication — 0°.3, corresponds to the lowest which was given during the observa-

* The experiment with the first thermometrograph failed, as stated in this Journal, vol. xxxiii. p. 402.

tions which were made during the night, at the bottom of the shaft; so that it really appears that this is truly the lowest temperature to which the glacier is subjected.

That this observation may not rest isolated, M. Agassiz has taken measures whereby it may be verified. Before quitting the glacier in 1842, he again introduced two of Bunten's thermometrographs, and a small horizontal alcohol minimum thermometrograph into the glacier, at different depths. These two thermometrographs are in the same bore,—the one at the depth of fifteen feet, the second eight; the thermometer is at seven feet below the surface. We may hope thus completely to confirm next summer the observation of the last.

Observations upon the presence of Water in the interior of the Glacier.—The presence of water in the interior of the glacier constitutes one of the essential postulates of the theory of infiltration. It is clear that a quantity of water is continually engulphed within the glacier, as is proved by the numerous streams and rivulets which are nearly all lost in the crevasses previous to their having attained the extremity of the glacier. This, however, does not amount to a proof that this water quite penetrates throughout the mass, as it would do through a sponge. On the contrary, it appears more natural, at the first glance, to suppose that it would always flow straight on, and especially when we consider how exceedingly compact the ice everywhere is, when defended against the action of external agents. It was, therefore, a matter of great importance to determine, by direct experiments, that water was to be found in the interior of all ice; and the more so, as many scientific men, maintaining that the water did not penetrate beyond a certain depth, founded on this belief an objection against the theory of infiltration. Mr Hopkins was one of these, supporting his opinion in his learned memoir, entitled, *Theoretical Investigations on the motions of Glaciers.* The bores which were opened in the ice by M. Agassiz supplied a rare and happy occasion for making direct experiments upon this subject, previous to his determining to make experiments by means of the infiltration of coloured liquids. The results obtained by M. Agassiz, and detailed by him in Jameson's Journal, vol. xxxiii. p. 262, have been confirmed in the most satisfactory manner, by the account given of the experiments on infiltration in a former article.* Hence, it has now been demonstrated, that during the summer the whole mass of the glacier is thoroughly imbued with water at all depths to which we have hitherto reached; but in a manner somewhat unequal, according to the character of the ice, the state of the atmosphere, and the epochs and hours at which the observations are made.

After this, is it not astonishing to observe that a naturalist has recently affirmed that he has satisfied himself by direct experiment, not only that the glacier does not imbibe water, but, also, that it does not even contain moisture ? The following is the experiment: M. Hugi, for he is the individual in question, caused a canal or tunnel, 10 feet long, 3 inches high, and from 7 to 9 inches wide, to be excavated beneath a pool of water which he found on the surface; into this tunnel he introduced several tin-boxes, filled with the chloride of lime and other matters, which have a great attraction for water, the lids of the boxes being pierced with holes, and the mouth of the tunnel being carefully closed. After remaining about fourteen hours, it was found that the boxes had scarcely increased in weight, whilst similar ones exposed in the open air had become much heavier. Hence it was that M. Hugi concluded that there was no moisture in the glacier ? But M. Vogt has already answered this argument, by remarking, that it is quite natural that water, at the temperature of zero (32° F.), enclosed within a narrow compass, should not disengage much vapour; and that if M. Hugi had introduced a sponge instead of the chloride of lime into his tunnel, he would have found it soaked with water derived from its icy walls. I should not have alluded to this experiment, had it not been made by a naturalist who has travelled extensively over the glaciers. Moreover, it is the only experiment which M. Hugi mentions in his last work.

Observations regarding the advance of the Glacier.—The measures which were begun by M. Hugi at the glacier of the Aar, and prosecuted by M. Agassiz during the earlier years of his

* See Bibl. Univ.; Mars 1843 (Tome xliv.), p. 131; and this Journal, vol. xxxv. p. 290.

investigations, have had the fortunate result of determining the movement of glaciers beyond all doubt, and this not only at their extremity, but also in their upper portions, and of also determining, in an approximate manner, the sum of the annual movement. Once possessed of these facts, exact measurements become the more necessary, and it is anticipated in all quarters that they will occupy an important part in all future investigations.

The block of the Hotel des Neuchâtelois, which we had left in the month of August 1840 at the distance of 797 metres from the Abschwung, had advanced from it on the 5th September 1841 to the distance of 861 metres, and on the 4th of September 1842 to the distance of 943 metres; it had consequently advanced during the first year 64 metres, or 213 feet;* and, during the second, to the distance of 82 metres, or 273 feet; in all, 146 metres during two years; in other words 486 feet. During our sojourn upon the glacier in 1841, M. Agassiz, in concert with M. Escher, determined the position of five blocks, that he might thereby ascertain the relations which existed between the different portions of the glacier in respect to their movement; and whether, as is generally believed, the inferior parts of the glaciers really advance more rapidly than the upper parts, or, on the contrary, if the reverse occurs. It was also important to ascertain by experiment if the sides or margins of the glacier have a more rapid motion than the centre, or *vice versa*. With this object in view, M. Agassiz had marked out by line, across the glacier of the Aar, at the height of M. Hugi's hut, that is to say about 2000 feet lower than the Hotel des Neuchâtelois, a series of stakes corresponding with fixed points on the border of the valley, and planted to a depth of 10 feet in the ice.

Hence, in 1842, we hoped that we might obtain the first comparative results regarding the movement of the different portions of the glacier in the course of the year. These results have already been partially published in some of the Journals, taken from a series of letters addressed by M. Agassiz to M. Arago and the Baron de Humboldt.

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* The measures are in Swiss feet ($1 = 0^{m}.30$), which are very nearly the same as English feet.

The following is the result derived from the series of stakes arranged across the glacier, as measured on the 20th of July 1842. There were there six stakes, of which three were placed upon the Lauter-Aar branch, and three upon the Finster-Aar, and which we shall designate on both sides by the numbers 1, 2, 3, naming as No. 1, the stake which was nearest the centre of the glacier, or the middle moraine; as No. 2, the next; and as No. 3, the last, or that which was nearest to the border of the valley. Upon the branch of the Finster-Aar No. 1, had advanced 269 feet; No. 2, 225 feet; and No. 3, 160 feet. And upon the branch of Lauter-Aar, No. 1 had advanced 245.5 feet; No. 2, 209.5; and No. 3, 125.0 feet. The block of M. Hugi's old hut, which lay in the line of observation, had advanced in the same extent as No. 1 of the Finster-Aar branch, that is to say, to the extent of 269 feet.

Hence it results that the advance of the different portions of the glacier presents a curve whose convexity is inclined downwards. Far, then, from confirming the opinion which M. Agassiz had previously hazarded, that the margins advance more rapidly than the middle, we, on the contrary, find that the centre advances much more than the sides, almost even to double the extent, being in the ratio of 245 feet to 125 in the one case, and of 269 to 160 in the other.

The several blocks whose position had been determined with a view to ascertain the longitudinal advance of the glacier, have furnished results which are not less important. There are five of the blocks which we shall distinguish by the names A, B, C, D, and E. A was the block of the Hotel des Neuchâtelois, at a distance of 3077 feet from the Abschwung; B was the block of M. Hugi's hut, at the distance from the same point of 5176 feet; C was a large block above the *grand cone*, at the distance of 13,950 feet; D, another at the distance from the same point of 21,970 feet; and finally E, another, at the distance of 24,470 feet, and at about 3000 feet from the extremity of the glacier. Between the 5th of September 1841, in which the positions of these blocks was first determined, and the 5th of September 1842, when they were measured afresh, the following difference in the numbers was ascertained. A had advanced 274 feet, B 291, C 219, D 168, and E 265. But it must be noticed, that it was not possible to discover in a satisfactory manner, the original position of block E, and it was moreover apparent, that this block had rolled over to the bottom of the moraine, and had thus been displaced to an extent that could not be accurately appreciated. Regarding this result then as uncertain, we may deduce from the others that the glacier advances less rapidly towards its extremity than in the upper regions. According to these data, the most rapid movement must be placed in the neighbourhood of M. Hugi's hut, from which locality it decreases both in ascending and descending. This result, like that of the stakes, is directly contrary to the belief of most observers; but we shall see ere long, that it could scarcely be otherwise, provided the movement is really effected in consequence of infiltration, for, as the ice of the terminal regions is more compact than that of the upper ones, it is thereby less susceptible of imbibing so completely. It is true that Professor Forbes obtained on the Mer de Glace of Chamouni, results which were diametrically opposed to ours; and he adduced this circumstance as one of his reasons for rejecting the theory of infiltration. According to him the lower part of the glacier advances more rapidly than the upper part, in the ratio of 5 to 3. But it should be observed, that his data rest only upon observations made only during a few weeks in the height of summer, while M. Agassiz's have reference to the movement of the whole year. And it can in no degree excite surprise, if it be found, that, at the beginning of summer, the terminal portion during a certain period, really moves more rapidly than the upper portions, which at that time are still more or less covered with snow.

With the view of completing these measurements, M. Agassiz has come to the determination of getting a topographical map prepared, and applied to M. Wild, engineer, who joined us on the glacier of the Aar in the beginning of July 1842, that he might there commence a trigonometrical survey of the glacier. The map, which is to be in the proportion of one ten-thousandth part, being destined to form a part of M. Agassiz' new work, I shall not anticipate the details it is intended to illustrate. I shall limit myself to the remark, that, as to accuracy of execution, it may safely be placed by the side of the most beautiful specimens of mapping of our age. Not only are all the features of the glacier, which the scale permits, indicated with the greatest precision, but what will render it peculiarly valuable to all the observers of glaciers, M. Wild has thereon determined the position of quite a network of the blocks distributed on the surface of the glacier. These blocks, to the number of eighteen, are all associated with fixed points in the valley, so that, in time coming, every one may, by examining these points, ascertain how much any part of the glacier has advanced within a given period.

In addition to the general map, M. Agassiz has also caused to be represented, a transversal band, 500 feet in width, across the glacier, in a locality where it is very much crevassed. This band, which has been measured and levelled by M. Wild with the greatest accuracy, has been prepared on a very large scale, amounting to one two-thousandth part, so as to exhibit the most minute details. Not only are all the crevasses indicated upon it, but their relative form and dimensions are accurately measured, so that it may be easily ascertained, in the ensuing summer, what changes they have undergone, if they advance with the entire mass, or if they arrange themselves anew, or finally, if new ones occupy their place. Should this last alternative be the case, it would be demonstrated that the form of the crevasses depends essentially upon the windings and irregularities of the valley. This, in fact, has for long been the general supposition, though nothing like a demonstration has been supplied. Accordingly, it seems quite natural that Professor Forbes should have adopted this opinion, but we have been astonished that this philosopher, who very properly looks for exactitude in every thing, has propounded this opinion as a fact, since, having made only one sojourn on the *Mer de Glace*, it is impossible he could have obtained positive results from observations which require to be verified for a succession of years.

This same band, 500 feet wide, may be made subservient to the supply of positive information, respecting another and not less remarkable particular, namely, concerning the method in which the glaciers pass the projections of the rocks, and in the following way. In the space included by this band on the left bank, there exist two of these rocky promontories. These very marked projections obstruct the passage of the ice to an extent of not less than 600 feet, and force it to diverge into a lateral creek. According, therefore, to the manner in which the mass shall pass these projections, we may deduce conclusions for or against the several theories which have been proposed.

The two lines of the transversal band which we have above described, having been measured by Mr Wild between the 13th and 16th of July, he repeated the measurement on the 30th of August, and found that, during these 45 days, the central portion had already advanced considerably more than the sides, the former part having moved forward 30 feet 5 inches, whilst the movement of the latter parts of the glacier diminished towards the margin nearly in the ratio of its distance from the central moraine.

It is, moreover, of some consequence that we should be made acquainted with the relations of the movement in different directions. Accordingly, a large triangle was traced on the 12th of July, on the course of the Finster-Aar, immediately above the hotel. One of the sides A B, which was perpendicular to the axis of the glacier, measured 301 feet; the second A C, which was nearly parallel to the axis, measured 409 feet; and the third B C, the hypotenuse, measured 492 feet 4 inches. This triangle was again measured on two subsequent occasions, namely, on the 23d of July and on the 30th of August. On the first occasion, a slight difference only was remarked in the line A B, which was shortened about 7 inches; and on the 30th of August the following were the measurements:—The line A B equalled 299 feet 5 inches; the line A C 409 feet 6 inches; and the line B C 492 feet 5 inches. Hence it results, that the side perpendicular to the axis of the glacier had alone been shortened, as might naturally have been expected; whilst the side parallel to the axis of the glacier had, on the contrary, been elongated 6 inches, and the hypotenuse remained the same.

But our observations should not be limited to the determination of the annual advance in the different portions of the glacier. It was not less urgently required for theory, that we should have a knowledge of the exact manner of the daily movement, and especially of the difference between the diurnal and nocturnal movement of the entire mass. With this object in view, M. Agassiz caused a stake to be fixed in the ice, at the distance of about 500 feet from the margin of the left bank of the glacier. A telescope was fixed on a rock, and every morning an observation was made as to the distance the stake had moved from the vertical thread of the telescope. These observations, made quite regularly for nearly a month, from the 3d to the 26th of August, every morning and evening at seven o'clock, have given the mean advance at 3½ inches in the 24 hours. The proportion of the diurnal movement to the nocturnal has been as six to seven. This excess in favour of the night, however minute it may be, must be determined more accurately, as Professor Forbes has obtained a result completely opposite, at the Glacier des Bois. M. Agassiz explains this difference, by referring it to the difference in the hours at which the observations were made. He conceives that Professor Forbes, by including the hour from six to seven in the morning in the series of diurnal hours, has given to that series an hour which belongs to the nocturnal one, and that an hour in which the movement is probably most of all considerable. In fact, as soon as you commence to compare the diurnal temperature with the nocturnal, it is necessary that you place the commencement of the glacier day at the time that the melting of the ice begins to be seen, and the watery rivulets commence to run. Now, this period never commences before seven in the morning, even in the hottest days.

Regarding the extent of the movement (3½ inches), it may probably be considered very small, when compared to the sum-total of the annual movement. But here it ought to be remembered, that the stake in question indicates the advance of a portion of the glacier which is very near its margin ; now, it is probable that, judging from the relative movements of the margin and the centre mentioned above, if the stake, instead of having been placed at the distance of 500 feet from the margin, had been placed nearer the centre, where the advance is at its maximum, we should have obtained a result at least double of the one specified. In truth, the observations which have been made upon a stake fixed in the ice near the *Hotel des Neuchâtelois*, on the course of the Finster-Aar, quite demonstrate this. This stake, situate 300 feet from the moraine, was observed seven times in the space of 52 days, between the 13th of July and the 3d of September, and its progress was 40 feet 9 inches; in other words, 8 inches a-day. In the transversal band, the greatest advance, as stated above, has only been 30 feet 5 inches in 45 days, or 7 inches a-day, confirming the results supplied by the progress of the blocks, namely, that the movement diminishes from above downwards.

All the observations which we have just mentioned were made upon that part of the glacier which is below the *Abschwung*. But it was equally necessary to have measurements which would determine the movement of the upper parts, or those near the region of the nevé. For this purpose, we ascended on the 2d of September to the higher part of the glacier of Strahleck, there to determine, by means of the theodolite, the position of a series of blocks, the most elevated of which are situate beyond the second tributary glacier which descends from the Schreckhorn, at the height of at least 9000 feet. We could not have found a more convenient locality; for not only is the block very large, but there is another in the same line, nearer the middle, so that we shall thus obtain, in this part of the glacier also, the relative movement of the margins and the centre. The second block is placed some thousands of feet farther down, in front of the indented ridge of the Lauteraarhörner. The third is situate near the margin of the valley, upon the most inclined portion of the glacier. The fourth is placed in the great plain of the glacier of the Finster-Aar, upon the course of the glacier of the Altmann, in front of the Grunerhorn.

These notices, I trust, will suffice to exhibit the extent of the observations which it is necessary to make, in order to arrive at positive conclusions concerning the advance of the entire glacier. Those who have pursued with any degree of attention our studies upon glaciers, will thence infer that much still remains to be done upon this field, as novel as it is vast. A cardinal point, which, above others, it is necessary to know, is the difference between the summer and the winter progress of the glacier. It will, undoubtedly, be very difficult to procure accurate data bearing upon this point, on account of the inconstancy of the seasons. But as we now know the exact position of a great number of blocks at the termination of last summer, we shall at least soon have the power of appreciating the extent to which the mass shall have advanced between this period and the ensuing spring. We purpose to visit the glacier of the Aar with this object, and to verify the observations of the previous autumn, as soon as the snows shall have so far disappeared that the blocks may be seen. By a second time measuring these points at the close of summer, we shall know exactly the portion of the movement which belongs to the hot season, and that which belongs to the winter, comprised between the 5th of September and the commencement of the spring.

These, it is true, are but the first lineaments of a more extended labour, which undoubtedly will one day be effected with all the accuracy which the importance of the subject merits. We do not despair to see, ere long, a permanent observatory erected upon the margin of one of the great glaciers of the Alps, in which may be prosecuted from day to day, and from hour to hour, the progress of the glacier in all its relations with meteorological phenomena. And as all investigation brings along with it its own reward, we have no doubt that this will lead to satisfactory results.

Observations on the changes of the Level of the Surface.—The large volume of water which daily escapes from the glaciers at their terminal openings, clearly demonstrates that a notable portion of their mass is carried away by melting. Up to the present time, most observers have attributed this melting, in part at least, to the effect of central heat. Notwithstanding, when we ascend a glacier during a hot day, we cannot fail to be struck by the quantity of small streams which furrow the surface, and we are easily persuaded that this quantity greatly exceeds that which escapes underneath at the extremity of the glacier. But in this case, what becomes of this great mass of water? This is one of the great problems of the study of glaciers, and on its solution depends greatly the fate of the theory of infiltration. The first observations on the lowering of the surface by melting, or, to adopt the phrase of M. Agassiz, by the *ablation* of the surface, were made in the year 1841, and published in Jameson's Journal, vol. xxxiii. p. 275. When we revisited the glacier in the month of July 1842, the position of our cylinders was known by a small hillock of rubbish, like a gigantic mole-hill, in the midst of which our No. 13 stood, indicating a subsidence of about 4 feet. As the locality of this hole or bore is rigorously determined, and easily discovered, all may continue for the future to examine the amount of the ablation of the surface in a given time. Supposing that this ablation shall, on the average, amount to 10 feet a year, and that the glacier continues to progress at the same rate, in other words, at about the rate of 200 feet per annum, our cylinder No. 1 should arrive at the surface at the end of about fourteen years, and that at a distance of about 3000 feet from its original introduction.*

Of three stakes introduced into the ice close to the same locality (see p. 155), two appeared at a height above the surface of 3 feet 7 inches, and the third at the height of 3 feet 5 inches. The stakes placed transversely in the neighbourhood of M. Hugi's hut, a quarter of a league farther down, were measured somewhat later, on the 20th of July, and gave the following indications. The stake No. 1 of the Finster-Aar indicated a fall of 6 feet 5 inches; the stake No. 2 of 5 feet 5 inches; the stake No. 3 of 4 feet 4 inches; and the stake No. 1 of the Lauter-Aar of 5 feet 2 inches. The other two stakes B and C, of the Lauter-Aar, had lost their marks from friction. From these data it results that the central part of the glacier, that is to say, the portion which advances the most rapidly, was also the part which had undergone the most considerable ablation.

* In the paper by M. Agassiz quoted above, he speaks of 14 cylinders, and mentions that the 14th was one foot and a half under the surface in Septembe 1841 (p. 276.) According to M. Desor, there were only 13 cylinders, and it was the 13th which occupied the position just mentioned in 1841. In the same paper (pp. 276 and 277) M. Agassiz supposes 5 feet for the amount of annual ablation, and therefore calculates that 28 years must be required for the cylinder No. 1 reaching the surface, instead of 14 as now estimated by M. Desor. We mention this merely to assist our readers in their references.—Edit.

Now that we were somewhat acquainted with the annual ablation at a variety of points, we wished, moreover, to ascertain what was the amount of the daily ablation. A stake inserted in the ice, at the distance of some hundreds of feet from the *Hotel des Neuchâtelois,* upon the band of the Lauter-Aar, served for these experiments. We surrounded it with some flag-stones, placed on the surface of the ice, and went every evening and morning to observe the amount of the projections of the stakes. The observations were made without interruption from the 12th to the 22d of July, and yielded an *ablation* of 2 inches and 1 line per diem. The ablation occurs chiefly during the day, and is most abundant on calm days, as then the melting and the evaporation both act with great intensity: it is less abundant during rainy days, and is very inconsiderable, or does not take place at all on misty days, or when the surface of the glacier is covered with snow. Finally, it is not sensible during the night, except when it rains.

This loss of substance is so great in quantity, that at first sight we cannot but inquire, How does it happen, that, with such an ablation, the glaciers can have so protracted a course? for it is manifest that the difference of thickness between the origin and the termination of the glacier is not sufficient to account for the superficial loss. It must necessarily happen then, that the glacier repairs the loss some way or other; in other words, that somehow or other it renews itself during its progress. This renovation, or increase by intersusception, as M. Elie de Beaumont calls it, is so imperatively required by the actual state of matters, that the majority of authors have admitted it, whatever the theory be by which they account for it.

The cause which produces this interior renovation can be nothing else than the water which percolates into the interior capillary fissures, and which, congealing in them, dilates the mass of the glacier. The following comparison, though somewhat trivial, may perhaps facilitate the comprehension of this important fact. Let us suppose, for an instant, that instead of ice, the glacier were composed of a current of soft paste reposing on an inclined plain, and that every day there was introduced into this paste some leaven to make it ferment. Let us, moreover, suppose, that every day there was taken from this paste a portion nearly equal to the augmentation of the volume produced by the leaven, and it would thence result, that, in spite of the part daily removed, the parts would nearly preserve the same level. Now, this is precisely what happens in the glaciers. The leaven is the water which percolates daily into the ice, and which, dilating by congelation, makes the mass to swell, and thus repairs the loss of substance which the glacier undergoes at the surface. Only, the action of the leaven is in this case different at different seasons; and every thing leads to the conclusion that it is much more energetic in spring-time than in the midst of summer. In winter, when there is no water on the surface of the ice, nor in the crevasses, and when the falls of snow begin to replace the rains of autumn, the swelling must cease, as must also the movement.*

Observation on the Planet Venus at the time of its Superior Conjunction. By Thomas Dick, Esq. A.M.†

On Monday the 2d October 1843, the planet Venus passed the point of its superior conjunction with the sun at 4 h. 15 m. P.M. It is only at this period that Venus can present to an inhabitant of the earth a full enlightened hemisphere. At all other times this planet presents the appearance either of a crescent, a half moon, or a gibbous phase, when viewed with a good telescope. Most astronomical writers have asserted that it is impossible to see this planet at the time of its superior conjunction. Mr Benjamin Martin, an eminent mathematician, asserts, both in his "Gentleman and Lady's Philosophy," and in his "Philosophia Britannica," that "at and about her upper conjunction, *Venus cannot be seen*, by reason of her nearness to the sun;" and, again, "at her superior conjunction, Venus would appear a full enlightened hemisphere, *were it not that she is then lost in the sun's blaze*, or hid behind his body." In the Edinburgh Encyclopædia, article

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* From the *Bibliothèque Universelle de Genève*, Nos. 88 and 89. 1843.

† To Professor Jameson.

[Sir,—The above observation is sent for insertion in the "Edinburgh New Philosophical Journal," should you deem it worthy of being recorded. It is an observation, so far as my knowledge extends, that has never been previously made.]

Astronomy, it is said, when describing the phases of Mercury and Venus, "their luminous side is completely turned to the earth at the time of their superior conjunction, when they would appear like the full moon, *if they were not then eclipsed by the rays of the sun*." Other writers have expressed themselves in such language as the following:—"When Venus is on the other side of the sun, in which she appears in the same sign with him, *she cannot be seen in the heavens*, being lost in the effulgence of the solar rays," &c.

It is nearly thirty years since I published in Nicholson's "Philosophical Journal" (vol. xxxvi. p. 109—128), and other scientific periodicals, a series of observations made on the heavenly bodies in *the day time*, in which I shewed that Venus could be seen when within less than two degrees of the sun, either to the east or to the west of that luminary, having several times seen her in these positions; and, consequently, that she could be as easily seen at the same distance north or south of the sun at the moment of conjunction. But I had no opportunity of making this observation for many years, either on account of a cloudy atmosphere, or of the planet being too near the sun at the time of the conjunction. On the 2d of October last, the weather being favourable, the experiment was tried and found successful. At two o'clock P.M.—only two hours before the conjunction—I perceived the planet distinctly, and kept it in view for nearly ten minutes, till some dense clouds intercepted the view. It appeared tolerably distinct and well-defined, though not brilliant, and with a round full face, and its apparent path was distinctly traced several times across the field of view of the telescope. I perceived it afterwards, about half-past four P.M., only a few minutes after it had passed the point of conjunction, on which occasion it appeared less distinct than in the preceding observation, owing to the low altitude of the planet, being then only a few degrees above the horizon. The observations were made, not with an equatorial instrument, which I generally use in such observations, but with a good achromatic telescope 44½ inches focal distance, mounted on a common tripod, with a terrestrial power of 95 times. A conical tube, about ten inches long, was fixed on the object end of the telescope, at the extremity of which an aperture 1½ inch diameter, was placed, so as to intercept, as much as possible, the direct ingress of the solar rays. The top of the upper sash of the window of the place of observation was likewise so adjusted as to intercept the greater part of the sun's rays from entering the tube of the telescope. The sun's declination at that time was 3° 26′ south, and that of Venus 2° 12′ south; consequently the difference of declination was 1° 14′ = the distance of Venus from the sun's centre; and as the sun's semidiameter was about 16′, Venus was then only 58′ distant from the sun's northern limb, or 6′ less than two diameters of the sun.

This is the nearest approximation to the sun at which I have ever beheld this planet, and it demonstrates that Venus may be seen even when within a degree of the sun's margin; and it is perhaps the nearest position to that luminary in which this planet can be distinctly perceived. It shews that the light reflected from the surface of Venus is far more brilliant than that reflected from the surface of our moon; for no trace of this nocturnal luminary can be perceived, even when at a much greater distance from the sun, nor is there any other celestial body that can be seen within the limit now stated. This is the first observation, so far as my information extends, of Venus having been seen at the time of her superior conjunction.

The practical conclusion from such observations is, that at the superior conjunction of this planet, when its distance from the sun's margin is not less than 58′, *its polar and equatorial diameter may be measured* by a micrometer, when it will be determined whether or not Venus be of a spheroidal figure. The Earth, Mars, Jupiter, and Saturn are found to be not spheres but *spheroids*, having their polar shorter than their equatorial diameters. But the true figure of Venus has never yet been ascertained, because it is only at the superior conjunction that she presents a full enlightened hemisphere, and when both diameters can be measured, except at the time when she transits the sun's disk, which happens only twice in the course of 120 years. THOMAS DICK.*

BROUGHTY FERRY, NEAR DUNDEE,
November 1843.

* The planet Venus has been, at the superior conjunctions, seen several times at less angular distances from the sun than that mentioned by Dr Dick. On December 30, 1798, it was observed by M. Vidal at Mirepoix, in France, at 19′ distance from the sun's limb.—Connaissance des Tems pour l'An. xi. p. 240; pour l'An. xii. p. 333.

In a memoir by Dr Wollaston, "On the Finite Extent of the Atmosphere," *Suggestions for the better Ventilation of Sailing and Steam Vessels.* By Robert Ritchie, Esq., F.R.S.S.A., &c., Civil-Engineer, Edinburgh. (Communicated by the Royal Scottish Society of Arts.)*

The importance of ventilation, as regards ships, while it has not escaped the notice of scientific men in the past as well as the present time, appears never to have received sufficient public attention, although, if there is one place more than another in which the salubrity of the air is most essential, it is on shipboard. Its great importance, indeed, must be sufficiently apparent, when the navy of Britain, the enormous tonage of her mercantile marine, and the great number of men therein employed, are considered.

The confinement in the lower decks and close cabins of ships, which are often crowded with many persons generally both eating and sleeping there, cannot be avoided, and admits of little alleviation. In dwelling-houses, if the air be vitiated, doors and windows may be opened, and thus a current of fresh air may be allowed to pass through them; while in winter the open fire acts to a certain extent as a medium of ventilation. Very different is a crowded vessel in a

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published in the Philosophical Transactions for 1822, he has given an observation made by him of Venus, when the planet was 53 minutes distant from the sun. The telescope which he used was a small one of seven inches focal length, and one inch aperture. He has also given directions for rendering the planet visible at a less angular distance from the sun.

The reason why Venus is not observed near the superior conjunction with the meridian instruments in fixed observatories, is the necessity of employing troublesome apparatus for keeping off the sun's light, and the comparative inutility of such observations.

Measurements of Venus' diameter about the superior conjunction would be useless. On account of the small apparent magnitude of the planet at that time, and its proximity to the sun, it would be in the worst situation for the attainment of accuracy. A difference between the polar and equatorial diameters would be inappreciable, for, at the utmost, it can only be a small fractional part of the unavoidable errors of observation.

It may be added, that the planet Mercury has also been seen by M. Vidal at a very small distance from the sun.—Edit.

* (Abridged from a paper Read before the Royal Scottish Society of Arts, April 10. 1843, Illustrated by Diagrams and Models.)

gale of wind, with scuttles closed and hatches closely fastened down, and no means provided for fresh air below, but what can find its way by an opening of a few feet square, where the vitiated vapours of human exhalations from the healthy, the sick, perhaps the dying, come steaming up the same aperture down which the fresh air struggles to find its scanty way to the miserable inmates. Stormy weather is often prolonged for weeks, during which several hundreds of persons are almost continually confined to the lower deck. This I have myself witnessed, and their situation there can be no difficulty in imagining. The impurity of the atmosphere below is a chief source of the great injury to the general health which arises in long voyages, more especially in tropical climates. If the robust suffer from this impurity, how much more injurious must it be to the invalid or the delicate. In troop or transport ships, the constitutions of the men are frequently enfeebled in place of being strengthened by the preliminary voyage; and on board of ship, deficient ventilation always aggravates the horrors of sea-sickness—the olfactory nerves seeming to increase in acuteness. The smell of bilge-water, or that stagnant corrupt water which lodges in the bottom of tight vessels, and sends forth offensive odours of sulphurous hydrogen and other gases, combined with the closeness of the cabins in sailing vessels, few can endure; and this is often farther increased in steamers, by the odour of the hot rancid tallow used for greasing the engine. There are few, indeed, who are not fully aware of the annoyances resulting from such causes; and the close sickly effluvia of the cabin—from that of the large vessel to the common ferry-boat—makes many a one rather submit to the inclemency of the weather on deck, than to be overpowered with nausea below.

If the evils of imperfect ventilation are so palpably experienced even in the best cabins and sleeping-berths of all ships, and in those more especially where there are no side-ports, what must be the case in the steerage and fore-cabins of passage-boats, and still more in the confined berths which are commonly allotted to seamen in merchant ships when between decks? The common seamen in general suffer less from neglected ventilation than most other classes of persons at sea, which undoubtedly arises from their passing so great a portion of their time in the open air. Still, numerous examples even from that class might be given, to shew the importance of a regular supply of pure air between decks, and the constant renovation of that which has been vitiated. I need hardly allude to the extreme cases of direct poisoning from asphyxia by the fumes of mephitic gases, it being well known that many persons have perished from suffocation, by shutting themselves up in close cabins with stoves.* But the general evil is the slow undermining of the constitution, the debility, relaxation, and injury to health, often a proximate cause of fever engendered by confinement in, or breathing continually, noxious, tainted, and unrenewed air. In warm climates, these evil results are aggravated tenfold. The following remarks by a very intelligent naval friend on the African coast, contained in a letter which I recently received, describes the subject in forcible terms, and proves that even the insalubrity of the atmosphere from miasmatic influences, may be rendered more noxious from deficient ventilation.

"On the lower deck of our little craft, were stowed away 109 persons, ship-stores, cook's coppers, &c. Never did I before feel so much the importance of a thorough ventilation. To sleep in such an atmosphere is next to impossible, and when exhausted nature sinks into repose, it awakes with that sickly and feverish sensation, which betokens the derangement of your physical system, and that you have been inhaling a poison which is slowly but surely preying on the vitals of your constitution. To you who have devoted so much attention to this subject, it cannot excite surprise. That disease and death should be frequent, is only what every rational and scientific person would expect. Climate is blamed for every disease that appears in foreign stations, but I have not the slightest doubt that the want of a thorough method of ventilation on

* Several instances are mentioned by Dr Andrew Combe in his Elements of Physiology. In a large steam-ship I lately examined, I found eleven men and three boys in a cabin below, about 12 feet by 8 feet,—and no air or light admitted but by the fore hatch, which measured 3 feet by 2 feet! It is high time the comforts of seamen in the mercantile marine were more attended to, which would raise the status and increase the efficiency of this useful class.

ship-board has in very many cases laid the system open to disease, which in more favourable circumstances could have been easily removed. The man who could improve the present wretched system would be justly entitled to the thanks of every humane and benevolent individual," &c.*

Such is the condition of a small crowded war sloop. It may be said with some truth, however, that the evils of defective ventilation are less felt in large men-of-war than in other sailing vessels, as the regularity and attention to the cleanliness and comfort of the men, and the airing of the lower decks by the gun-ports, afford facilities unattainable in merchant ships. Still, in the lower or orlop deck of all ships, there is difficulty in establishing a constant uniform current of fresh air. The usual means adopted in sailing ships to supply air below are wind-sails; these are canvass funnels suspended so as to catch the wind and convey it downwards. The defects of this mode of ventilating the lower decks of ships were very ably pointed out a century ago (1741), in a communication by Dr Richard Mead, F.R.S., and William Watson, F.R.S. (Phil. Trans., vol. xlii. 1742), and several plans were suggested for introducing a better method. The chief inconvenience of wind-sail ventilation pointed out is, that it cannot be of use either in calms or gales; hence it is not to be relied on when most wanted.

The great sickness in war and transport, or troop and hospital ships, in the early part of last century, of which we have a striking instance in Lord Anson's voyage, 1741, where it is stated—" Our men dying four, five, and six in a day, and out of a crew, but three months before, of between 400 and 500 men, almost all of them in health and vigour, the lieutenant could

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* A farther corroboration appears frequently in the daily prints. As an example. —*Portsmouth, October* 7. 1843. The Cygnet 6, Lieutenant Wilson, arrived on Monday from the west coast of Africa. She has been 3½ years in commission, and has been remarkably healthy, and not a single death has occurred from fever of the country. This may in a great degree be attributed to her roomy and well ventilated lower deck; while the Rolla, Buzzard, Lynx, and Forrester, suffered dreadfully from want of proper *ventilation*. Most of these vessels had two or three commanders, and almost a *complete change* of officers and crew, during the *three years*.

not muster more than two quarter masters, and six foremast men capable of working," attracted the attention of the Royal Society of London; and, as remarked by Mr Watson, p. 172, Phil. Trans. 1742, "of these two very ingenious and industrious members, the Rev. Dr Stephen Hales and the Rev. Dr Desaguliers, the first of whom introduced an instrument which he called the ship's lungs, and the latter a machine, which was an improvement of the Hessian Bellows."

In the year 1735, Dr Desaguliers first brought forward an account of an instrument or machine for changing the air of a room, by forcing in fresh air or removing foul. (Phil. Trans. vol. xxxxix. 437.) This machine, which he named "Centrifugal bellows," he recommended for extracting the foul air from between the decks in ships, and supplying fresh air. It was very similar to the wind-fan used at this day. He considered it an improvement on, and more effectual than, Mr Papin's bellows, as being able either to suck out the foul air, or throw in fresh. It was evidently a revival of the plan of a ventilator communicated to the Royal Society of London by Denys Papin in 1705, who says, in a letter to Dr Fred. Slare—"I don't question you have seen the new contrivance, an account of which was printed at Leipsic, in Actis Eruditorum, anno 1699, with this title, 'Rotatilis suctor et pressor Hassiaeus,' and it may be applied for wind as well as water." "I have, therefore, improved the Hessian bellows," &c. (vol. xxiv. Phil. Trans.) The improvement of M. Papin consisted in making the vanes of the fan eccentric in place of concentric, or he changed its cylindrical into a spiral form, which adaptation has of late years been so highly recommended. By working it with his foot, he could produce a wind to raise a weight of two pounds—(Ree's Enc.) Dr Desaguliers did not, however, adopt M. Papin's plan of the eccentric vanes, and though he otherwise improved the machine, it was never very effective. Dr Ure remarks, in the Arch. Mag. 1837, "from the defectiveness of its construction, as well as the small area of its discharge-pipe, it was probably not misnamed a philosophical toy by Sir Joseph Ackworth, then first Lord of the Admiralty, who went down to Woolwich to see its performance in ventilating the hold of H.M.S. Kinsale." It is but fair, however, to mention, in justice to the ingenious and philanthropic Dr Desaguliers, that he complains of the great difficulty he had in getting a proper trial of his machine by the Lords of the Admiralty in 1740. Exper. Phil. vol. ii. 4to.

The wind-fan, or blowing-wheel of Dr Desaguliers on a large scale, was fitted up under his own directions in a chamber over the House of Commons, to draw the hot and foul air out of the House ; it was worked occasionally from 1736 to 1743. As Dr Desaguliers in his Experimental Phil. 1744, regrets much the opposition manifested to his ventilator, and the injustice it received, it is not likely it was ever much used in ships ; but it was an ingenious and simple contrivance, and if it had had the modern improvements of construction, it would have been more successful.

The blowing-fan is seemingly of much greater antiquity than these inventions. It is noticed in Agricola de Re Metallica (Paris, 1541.) It is obviously derived from the grain fanning-mill, which is generally supposed to have been introduced into Europe from the Chinese by the Dutch. In the Gent. Magazine, May 1747, a representation is given of a grain fanning-mill, seemingly an old invention, used in Silesia, easily converted into Dr Desagulier's machine.*

The value of the wind-fan as a ventilator is every day becoming more appreciated. After the lapse of a century the plan has been again revived, and it has been made use of in some steamers within these few years to ventilate and cool the furnace-room, and it was likewise adopted by Dr D. B. Reid for the general ventilation of the vessels fitted out for the late African expedition (Lit. Gazette, 1841), in the Victoria and Albert Steam Yacht, and in other instances. The blowing-fan is now much approved of for many ventilating purposes, such as for mines, factories, power-loom mills, &c., wherever steam-power can be readily obtained to propel it ; and where there is a surplus power. In several public buildings, such as the Old Bailey Session-House and Reform Club House,

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* Herbert, in his Encyclopædia, mentions, that the first blowing-machine on this principle of which we have a distinct account, was that invented by Teral, 1729 ; but it is clear that it was recommended by Papin for ventilating purposes many years before.

London, and others, it has been made use of for ventilation, driven by small steam-engines. Even in crowded sailing vessels, it would be found a very efficient machine, as described in the Arch. Mag. 1837. It might be still more applicable if constructed to be easily unshipped, and on such a scale as, while the quantity of air drawn off is not too much diminished, it might be easily worked by relays of a few men. Several important improvements have lately been made on the eccentric blowing-fan, such as the proper angle ascertained, and number of the vanes to effect the greatest discharge of air; and a farther improvement has lately been made by a house in Glasgow (Mechanics' and Engineers' Mag., June 1843), by expanding the capacity of the centre for the better admission of the air. By this simple modification, it is stated the capabilities of the fan are found experimentally nearly doubled, while the disagreeable noise attending the working of the common fan is wholly avoided.

The other plan alluded to in the communication to the Royal Society in 1742, was "the ship's lungs," the ventilator of the celebrated Stephen Hales, D.D., F.R.S., Rector of Farington, &c. Of the various schemes for ship ventilation in former times, this appears to have been the most successful, and is still well deserving of notice, and of being kept in view, although it is not now in use. Several papers in the Phil. Trans. of last century speak in high terms of its utility on shipboard. In May 1741, Dr Hales' description of his ventilator was read before the Royal Society of London. It is mentioned in the Gent. Mag. 1743, as curious, that the same year, 1741, in which Dr Hales brought forward his invention, one of a similar kind was invented by Martin Triewald, Captain of Mechanics to the King of Sweden; and that the like invention should have been made in Stockholm and London is very extraordinary.

In 1743, Dr Hales published his Description of Ventilators,* &c. In this treatise he notices the ventilator of Trie-

* A Description of Ventilators, whereby great quantities of fresh air may be conveyed into mines, ships, &c., vol. i., 8vo. London, 1743; vol. ii. 1758. A copy will be found in the British Museum.

wald, an account of which was read before the Royal Society of Sweden, 3d April 1742,* and a description of it published by order of his Swedish Majesty, called a deduction of the usefulness of his engine on board of ships only, and a translation of the work, was communicated by Cromwell Mortimer, M.D., Sec. R.S. to Dr Hales. M. Triewald's ventilator drew the foul air from under the decks of ships; the least it exhausted was 36,172 cubic feet of air per hour. In men-of-war and hospital ships this machine was placed on the upper deck, directly over the great, or any other hatch, and the pipe going down between decks drew out the unwholesome air, which was instantly supplied by fresh. In 1742, all the men-of-war of the Swedish navy were fitted with them, and in that year the king of France ordered all his navy to be provided with these ventilators.

Dr Hales, in his Treatise, also mentions the ventilator of Nathaniel Henshaw, M D., F.R.S., who printed an account of it in a treatise called Aero Chalinos, or Register of the Air, 1677. Dr Hales gives the following account of it:—In order to have the benefit of change of air without going out of the house, he would have a room, which he calls an air-chamber, to be built 12 feet square, and air-tight every where, with a very large pair of organ bellows to be placed in the room, to or from which air is to be conveyed through the wall by a copper pipe, with valves to open inwards or outwards as occasion shall require. With these bellows the air in the room is either to be condensed, made heavier by forcing air in, or lighter by conveying air out of the room. Should a person desire to be in air so heavy as to raise mercury three inches, then the air must be forcibly drove into the room by the bellows; and *vice versa*, it must be drawn out when it is required to have the air so much lighter than the outward air. The pressure to affect the air in the room would be immense; whether to condense or rarify it so much, it would be 38,304 lb. troy in the opposite direction. He proposes by this means to cure fevers, &c., the patient remaining in this chamber, as also to prevent sea-sickness, by confining the person in such a

* These Transactions are in the Library of the Royal Society of London.

cabin. Dr Hales says, some have said his ventilator and this are the same; but he declares their difference is wide in principle; besides, the make of his is very different from organ bellows. He then gives an account of his ventilator, which has the merit of being easily and cheaply constructed; and as it was much valued at the time, and long afterwards, I shall give a short description of it. It consisted of two oblong air-tight boxes, placed parallel to each other, having wooden boards, or midriffs, closely fitting the interior section of these boxes, hinged at one end, and drawn up or pushed down at the opposite end by means of upright iron rods attached to a lever, moving on a standard between the boxes. By the elevation or depression of the midriff, effected by the alternate raising and depressing the ends of the lever, the air was drawn in or forced out of the boxes, by a series of valves, some opening inwards and some outwards, at the opposite end from where the iron rods were; there was a casing placed over four of the valves which emitted the foul air, communicating with an air-trunk, or pipe, which led off the air in one body. But without drawings, it is not easy to convey a correct idea of the machine. It could be used either to throw in fresh, or to draw off foul air. Dr Hales describes the application of his ventilator to H. M. Ship Captain. The ventilator was about 10 feet long, 5 feet wide, and 2 feet deep. It was placed flush, or even, with the floor of the orlop deck, and the trunk or vitiated air-pipe went through the gun and upper decks, near the side of the ship to the top of the gunnel. Two men, standing on the orlop deck, worked the lever, which was 12 feet long; and, from the number of men in large ships, the labour was small. The lever and standard were made to unship, so that little obstruction arose from this machine. The air to supply that which was pumped off passed down the hold by the open hatches, or other means provided, and the current was so gentle as not to be perceptible. From the number of open passages, its motion was 105 times slower than when passed off at the orifice of the ventilator. When the hold is to be ventilated, the doors into the gunner's store-room are opened, and all the gratings on the gun-deck covered with tarpaulins, leaving all the doors open of those cabins or rooms which want ventilation. The ventilators were made of different sizes, according to the class of the ship. A large sized one for a first or second-rate is already given, and for a 50-gun ship Dr H. proposed it should be 4 feet 3 inches wide, 9 feet long, 1 foot 6 inches deep, trunk 10 square inches; for a 20-gun ship, 8 feet long, 4 feet wide, 1 foot 6 inches deep, pipe 9 square inches. Smaller ventilators were also made of only one box, portable for such a purpose as airing the bread-room of ships, sweetening water, &c., having an iron rod and cross handle to raise the midriff. One person could easily work it for a length of time. Dr Hales strongly recommended his ventilator for blowing air through water in showers for sweetening it; also for milk in a similar way, and for airing corn, and for a variety of other purposes.

In 1751, in reference to Dr Hales' ventilator, there are some experiments recorded in a letter from Captain H. Ellis, ship Earl of Halifax, Phil. Trans., London, shewing its utility,—that the ventilator was far from being inconvenient on shipboard, and that out of a ship's crew of 130 men, there was not one sick on board. So much was Dr Hales' ventilator afterwards valued for ship ventilation, that an order in 1756, for ventilating the fleet, was issued by the Lords of the Admiralty.—(Rees' Enc., and 3d edit. Enc. Brit.) The ventilators were fixed in the gunner's fore store-room, and generally a-head of the sail-room, varying according to the class of the ship. The power of the large ventilator was very great. At 60 strokes per minute, the speed at which the greatest quantity of air was found to be emitted, 75 tons of air were discharged, or 4500 tons per hour, or 108,000 in 24 hours; the velocity of the air upwards was at the rate of 3000 feet per minute, or at an average rate of 26 miles per hour. In the year 1752, Dr Hales' ventilators, it is recorded, were very successfully applied to ventilate Newgate prison, and placed in the upper part of the building, when, being worked by a wind-mill, they drew out the foul air from several wards.*—Gent. Mag. vol. xxii.

Notwithstanding the acknowledged success of these ventilators in ships, mines, and prisons, still on shipboard there must have been objections raised against them, as Dr Hales,

* Hales' Treatise, vol. ii., London, 1758.

in papers in the Phil. Trans., 1751, 1755, combats these, and regrets the want of their more general use in ships, for the preservation of the health of seamen. Whatever, therefore, may have been the cause of their discontinuance,—and, it is more than probable, it arose from the trouble of working them, rather than the space occupied, or even any noise they might create,—they gradually appear to have been disused in ships, after all the labour bestowed on them by their philanthropic inventor. No vestige of them seems now in existence, nor were they in use, when I first became acquainted with nautical affairs, nor were they in the recollection of the oldest seamen I have enquired at.

I have alluded to the communications read before the Royal Society in 1742, where the invention of Dr Desaguliers and Dr Hales are noticed. Dr Mead, F.R.S., then brought forward Mr Samuel Sutton's (brewer in London) "invention and method for ventilating ship-holds, the well, and other parts," and describes the plan as being successfully tried on board the hulk at Deptford. The plan was very simple; it merely consisted of a tube. of convenient size, of lead, and partly of copper, closely fitted into a hole in the ash-pit of the furnace of the ship's coppers, or cooking fire, and small branches communicated with this pipe from various places in the lower parts of the ship. The defects of this method of ventilating by tubes, were particularly examined into, and pointed out by Dr Hales; and, although the principle was highly esteemed, it does not appear to have met with much encouragement, or to have got into general use, although it possessed the advantage over the mechanical rival plans of ventilation then in vogue, of requiring no one to work it. It seems singular, that Dr Mead should attribute the invention of this principle of ventilation to Mr Sutton, and that he does not allude to a communication made to the Royal Society of London, seventy years before, recommending it; any merit Mr Sutton could claim, being his suggestion of applying the principle to ships. From the Royal Society of London Transactions it appears, Vol. I., July 3, 1665, Sir Robert Moray gave an account "how adits and mines were wrought at Liege without air-shafts." At the mouth of the adit, a chimney, 30 feet high, was raised, having a fire-grate cradle placed within it, with a close ash-pit, and a tube fitted below the fire communicated with parts where the air was to be replaced. It is a somewhat remarkable coincidence that this method is the very same, which, in 1836, 171 years after, has been successfully applied by Dr D. B. Reid, to extract the vitiated air from the present House of Commons. Indeed, it seems natural to suppose, that the principle of creating a current by fire-heat, or, in ventilation, drawing off foul air from one part to be replaced by fresh, must have been the most self-obvious principle which mankind in all ages and in all countries could apply; hence various attempts have been made to use this principle of ventilation at different times, and sometimes it has proved successful. Dr Desaguliers, in his Mechanical Phil. 1744, seems to claim the merit of having first applied fire-draught suction to buildings in this country, although he appears to have disapproved, or at least not recommended it for ships, and complains that Dr Hales seems to have overlooked, in his book, the great exertions he had made in this same field for twenty-eight years. He says, that in 1715 he published a translation of a French book called Mécanique de Feu, by Monsieur Gauger, entitled "Fire Improved," &c.; and that in 1723 for clearing the air of the House of Commons, foul air grates were fixed under his direction in the closets above the House to suck out the impure air. This appears to have been the first application to *buildings* of the principle of mine ventilation by fire-draught, recommended by Sir Robert Moray; and which plan, though long in use for mines, has only within these few years been revived in Britain for domestic purposes. But, perhaps, amongst the oldest plans for the ventilation of coal-pits is that of producing a current or increased circulation of air by the use of large fires* at the foot of the upcast shaft. It is somewhat singular that, when Sir G, O. Paul, Bart., in 1801, brought before the Society of Arts of London a grate which he had invented for consuming the foul air which col-

* It has perhaps been known for centuries that the circulation is increased by kindling a fire at the top or bottom of one of the shafts.

lects in the upper part of rooms, hospitals, &c., on the principle of sucking in air to the fire, as recommended by former inventors, he makes no allusion to the previous suggestions on the same subject.

Dr Desaguliers' objections to the use of foul-air-extracting tubes in ships, was *the danger of explosion* of gases collected in the hold when led to the fire. Notwithstanding both the objections of Dr Hales and Dr Desaguliers, Mr Sutton, who appears to have been a persevering man, obtained, after some opposition, a patent for his plan or invention, and Dr Mead, in the year 1749, published an account of it under the following title: "A New Method of Extracting Foul Air from Ships, in two letters to a friend, by Samuel Sutton; to which is annexed, two relations given thereof to the Royal Society, by Dr Mead and Mr Watson, F.R.S, and a discourse on the Scurvy, by Dr Mead" (London, 8vo.) In this publication, Dr Mead expresses himself in the strongest terms in favour of the plan. He says it is "an invention which does honour to our nation, and will in time be found of more public benefit than any discovery in mechanics which has been produced for these hundred years,"—that it can be applied to many other purposes of life, and deplores the evil spirit manifested to Mr Sutton's plan, and remarks that the preface was written before Mr Sutton "brought him the agreeable news that the Lords of the Admiralty had just given orders to provide all the ships of H. M. navy with this useful machine." Mr Sutton, in his letter, explains how, in 1739, his attention was first called to the subject from the sailors on board the fleet at Spithead being dangerously ill for want of fresh air; and after some experiments which he made to try the effect of currents of rarified air with common chimneys, he brought his plan under the notice of the Admiralty, and at length succeeded in getting it tried.* From the report of experiments on board H. M. S. Norwich, on a voyage to the coast of Africa, it did not in all respects come up to expectation. After peti-

* He mentions the names of Sir Charles Wager and Sir Jacob Ackworth, Surveyor of the Navy, and Admiral Boscawen, as having taken some interest in his plan.

tioning the Admiralty in 1743, he received "one hundred pounds as a reward for loss of time and expense, and from the wish of their Lordships to give encouragement to persons who may turn their thoughts to inventions that may tend to the advantage of the navy.*" Mr Sutton was not satisfied with the report on his invention; he answered the objections brought against it by the captain of the Norwich as to danger from fire by sparks coming down the tubes. This, he said, could easily be got the better of by lengthening the tubes so as to pass through the chimney, and thus all communication would be cut off between the sparks and the tubes. Mr Sutton likewise complains of the injustice done to his invention by Dr Hales, who makes no mention of it in his book, although he knew it had been brought before the Royal Society,—and that the Doctor used his influence to get his own machine used in the navy in preference. Mr Sutton, with some boasting, says that the benefits of his invention are "perpetual; while Dr Hales attempts to make the air in a ship wholesome by only a few hours' use of his ventilators." He adds, his (Dr Hales' ventilators) "are cumbersome, and take up room, and require many hands to work them; my pipes take up no room but what may well be spared, and stand in no need of manual labour; his cannot extract air from the well at the bottom; mine can."

Mr Sutton, however, like many inventors, was over sanguine; only a few years passed on, and both plans, as regards ships, were consigned to oblivion, although, undoubtedly, Mr Sutton's plan is more likely to be again revived.†

In a work called Observations on the Construction of Ships, by J. Braithwaite, May 1810, mention is made of a method of drawing air from the interior of ships. "It consists of leading tubes from the most remote parts of ships'

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* Dated 22d Oct. 1743. Signed T. Compton, R. Haddock.

† Another plan of ventilating the holds of ships was proposed, and brought under the notice of the Society of Arts of London in 1821, by Mr Jacob Perkins of London, who received a premium for it. He proposed to draw out the foul air by means of two tanks half filled with water, which communicated freely with each other, placed diagonally at opposite sides of the hold, that either the roll or pitch of the vessel might produce motion. The foul air is conveyed off by means of pipes or hose, &c. See Transactions, vol. 38.

holds, or between decks, to the galley-funnel, which, being constantly heated, the circulation of the air from the extremity of the tube to the heated end becomes the natural consequence; and as the hold is hotter than the air in the funnel after the fire is extinguished, the reverse will take place. These tubes can with the utmost facility be placed so as to prevent sparks or soot finding their way down, but which idea was so prevalent some years back that they were discarded from the navy. Tubes of two inches diameter of copper or iron may be conducted along the sides into the after-hold, pump-well, or any other part of the ship, with perfect security."

Notwithstanding this favourable opinion, this method of ventilating ships has not been extended, probably from the feeling of insecurity against fire. In steam-ships, however, where the powerful ascending current from the chimney-funnel can always be had recourse to, such a method of ventilation, with proper precautions to prevent danger from fire, may come into more general use. A very general but obvious error seems to have existed in the over estimation of the effect of tubes of *two inches* diameter, as already stated, to produce any very decided iufluence on large masses of air. The very limited portion which can possibly pass from such tubes to the galley-funnel in a sailing vessel, compared with the quantity of air requisite to be renewed, must in most cases make this plan of ventilating very ineffective. In a building which I lately visited, where a similar plan was adopted for ventilating it, the extracting air-tubes were only *two* inches in diameter for a crowded room of ordinary dimensions, supposing the velocity of the current moving even at a quick rate, there being no provision for the supply of fresh air, the effect of the abstraction of this small portion must have been little in comparison with the volume of air in process of vitiation. Hence, in any plan of ventilation for extracting air by means of tubes, these must be proportioned to the space; and unless combined with fresh air supplies, cannot be effective.*

* One of the most useful instances of this mode of ventilation that I have observed, was adopted several years ago at an extensive cotton-mill in the north of Scotland, where a direct communication was made on the different floors with the engine-chimney, to suck out or extract impure air, but no provision for fresh air was made, which is left to find its way through crevices or the windows when open.

As the plan of ventilating-tubes by Mr Sutton has been so long known and tried at different periods, it would appear with considerable success, it is well deserving of consideration whether this plan could not be so far improved as to render it a safe and useful mode of ventilation for sailing vessels—not *overcrowded* with passengers; for in the latter case, no plan of this kind can be adequate from the obvious difficulty of having tubes of sufficient size and number to produce a constant renovation of the air. But in ordinary circumstances, by enlarging, as much as conveniency will admit, the foul-air tubes from the respective ceilings or beams of the under-decks, cabins, bread-room, hold, and well, leading to the galley or cooking-fire; and, secondly, by providing ample fresh-air tubes to convey air down to the lower part of these places, a very beneficial system of ventilation could be maintained. But the difficulty, as regards communicating these tubes, with the galley-fire and danger from sparks, must, as a preliminary point, be got the better of, which, in my opinion, might be done, by making the horizontal foul-air trunk, which collects the air in one body from the different ramified tubes, pass through a cylindrical boiler of small diameter heated with steam or hot-water raised by the galley-fire, but entirely separated from it, at least the extracting pipe to have no connexion with the galley-fire, but simply passing through the steam or hot-water, and the foul-air escaping at a sufficient elevation. As the air, when rarified in the tube, will immediately receive motion, it is of no consequence as regards the principle of establishing a current, whether the air is heated by coming in contact with the fire itself in the ash-pit, or not. There will indeed be the difference of temperature between steam-heat and that of the ordinary fire, to diminish to this extent the velocity of the current; but this may be so far compensated by the enlargement of the air-extracting pipe in passing through the boiler, and by the uniformity of heat; besides which, what is most important, all danger from tubes communicating with the ash-pit and galley chimney will be entirely avoided. The same fuel which would create a draught by Mr Sutton's plan, would raise steam or hot-water. As salt-water would be probably used in the boiler, the evaporation might be made subservient to other purposes.

(To be concluded in next Number.)

Notice of Observations on the Developement of the Seminal Fluid and Organs of Generation in the Crustacea. By HARRY D. S. GOODSIR, Conservator of the Museum of the Royal College of Surgeons in Edinburgh.* Communicated by the Author.

Shortly before the male Crustacean comes into season, the generative system is observed to change its appearance very considerably, so as to assume a very different aspect from that which it generally bears. This change is first observed in the testicle itself, which, when inactive, is almost altogether lost in the folds of the liver. The first change which takes place is in the size of the gland, which is produced by means of the germinal cells,† in each acinus of the gland becoming active, secreting, and filled with small nucleated cells; this secretion goes on until the germinal cell is quite full, when it bursts and empties itself into the cavity of the acinus. After these small nucleated cells, or, as we shall now term them, "secondary cells," have lain in the cavity of the acinus for some time, they also begin to take on an active character as secretive cells, and become in their course downwards towards the vas deferens very much larger and at the sametime filled with young cells. We shall now then follow them in their course from the acinus down to the vas deferens. In the acinus we find the greater number merely increased slightly in size, a considerable number, however, already contain two, three, four, or more cells, and a few are even as large as those found in the epididymis. As we descend and examine the contents of the epididymis, which appears in this class of animals to be a kind of receptacle for the proper elimination of these cells, instead of finding a great number of simple nucleated cells, we find it containing, with very few exceptions, parent cells, large, and distended with young ones. We still, however, find a few of these parent cells with only one, two, or more young within them, and others still in the same state as when they left the acinus, from which they had originally been secreted. On descending still farther, and examining the contents of the vas deferens, we will find that these parent cells are *all* large and distended with young, and some of them even burst, with the young lying scattered around about the empty cell. These cells, however, which burst in the vas deferens are precocious, although we find examples of it taking place in the epi-

* Being an abstract of Part I. of Crustacealogical Researches, preparing for publication.

† On the Ultimate Secreting Structure, and on the Laws of its Functions. By Mr John Goodsir. In the Transac. of the Royal Soc. of Edinburgh, vol. xv. Part ii.

didymis, and even higher up the gland, for in general all these parent or secondary cells are thrown into the spermatheca of the female before they burst. Preparations, however, for this destruction of the parent cell seem to be going on in the vas deferens of the male a long time even before the testicle has arrived at its maximum state of periodic developement, for the difference between the cells in the epididymis and vas deferens is quite apparent. Those in the former are quite full of young cells, the walls of the parent cell lying tightly and closely upon, and binding them together as it were; whereas those in the latter, although quite distended, are not full altogether of cells, but with a fluid which appears to separate the young contained cells from one another, and from the walls of the parent cell. If a female crab can be procured shortly before spawning, the spermatheca will be found quite full of these young cells, which we shall henceforth term primary, or spermatozoal cells, with a few parent or secondary cells floating amongst them. If the crab is still farther advanced, we will find that these secondary cells have disappeared altogether. These cells in the spermatheca are suspended in a thick albuminous fluid of a milky colour. Throughout the whole course of the testicle and epididymis, we find floating in a thick glairy fluid, along with the cells, in masses of an irregular-shaped, clear-looking appearance, a substance which apparently forms the nutriment of the cells, for this substance is always found in greatest quantities high up the organ, where the secondary cells are in an active state of growth and secretion, and of course requiring a supply of nourishment; whereas in the vas deferens, after the cells have arrived to a state of maturity, little or none of this substance is found. How this substance is secreted, or in what part of the organ, I have not yet been able to ascertain. The above description of the secretion and developement of the seminal fluid refers to all the Crustacea. It will be found, however, that this fluid varies more or less in its character in almost each species. In the May and June Nos. of the Ann. des Sc. Nat., a translation of a paper will be found, "On the Seminal Fluid of the Crustacea and Cirripeda; by M. Kolliker of Zurich. That gentleman, in the above paper, looks upon the filiform bodies which are found in such numbers generally in the interior of the testicle of the lower crustacea, as the spermatozoa. Now, as far as my observations have gone, it appears to me that these are parasitic entozoa (Filaria). My observations had been directed to these animals sometime before I had seen M. Kolliker's paper, at which time I had made up my mind as to their parasitic character, and for the following reasons: From observations on the origin and developement of the spermatozoa in the higher Crustacea, it had been made out that these were developed from cells, and from cells only. Now in every instance these Filaria were always found to be the same, never changing their characters in the slightest degree. Besides, the developement of the seminal cells in these lower Crustacea, was always going on in the same way as it did in the higher, and without any appearance in the primary cells of the enclosed bodies, being like these Filaria. This, of course, also extends to M. Kolliker's observations on the same fluid in Cirripeds, for they must be looked upon now as Crustaceans also.*

As the ova pass through the spermatheca, they come in contact with the spermatozoa, and in this way are impregnated. They also appear to obtain in this organ a thick coating of albumen, which afterwards becomes the ovisac.

The organs employed in this class of animals for the proper defence of the ova, after they have been excluded from the ovaries, and during the time they are attached externally to the mother, are very interesting. After a careful analysis, they will be found throughout the whole class to be either legs not fully developed, or parts of these organs very much developed. The first of these divisions will be found to exist in the higher Crustacea, such as those belonging to the Brachyura, the Anomoura, and Macroura; this form is also found in the Araneiformes. It is in the Stomapoda we first observe these organs assuming another character, namely, that of protecting scales. We find this character gradually making its appearance, however, in the lower Macroura, as in Hippolyte and Palæmon, where we find the ova attached by means of filaments to broad scales, and in the other genera; if this is not present, the usual armature of the body will be observed enormously developed, so as in some cases almost to meet, by means of their edges, on the abdominal surface of the body.

In the Amphipoda, Læmodipoda, and Isopoda, we find these ovigerous scales bearing a considerable resemblance to those of the Stomapoda. In the Branchiopoda this character still exists, but in rather a more interesting form. In Apus we find the ova contained in a cavity formed by the expansion of the first joint of the eleventh pair of legs into two large circular-shaped and hollow plates, which, when closed together, form a safe means of defence. Through the genera Branchipus, Artemia, &c., we are led to Daphnia, Polyphemus, and Evadne, where we find this means of defence in its maximum state of developement, namely, one, or perhaps more, pairs of legs enormously developed, so as not to cover the ova only, but the whole body. The occurrence of this in these animals, is the cause of their unusual and grotesque appearance. From this form we are more suddenly brought through Saphirina and Cetochilus to the Monoculi, where we find a different means of defence altogether. Instead of great developement of the feet, we find one pair, or perhaps more, of these organs aborted altogether, and sacs or bladders are secreted each time the animal becomes loaded with spawn. This form is carried into, and continued throughout all the Siphonostoma, until we reach the Araneiformes, where we again find these organs assume almost the same appearance as those with which we first

* Edinburgh New Philosophical Journal for July 1843. Vol. 35, p. 88.

commenced, but perhaps more like ordinary feet, to which the ova are attached in small round pellets.

The metamorphosis of the larvæ are peculiar and interesting. They afford good characters for the definition of the higher divisions of the class, and even good and strong specific characters.

Meteorological Tables for 1843.

(Continued from Vol. XXXV., p. 185.)

Table VI.—June.

1843.	Ther. Max.	Ther. Min.	Ther. Mean.	Barom. ½ p. 8 A.M.	Therm. ½ p. 8 A.M	Bar. 8 P.M.	Ther. 8 P.M.	Rain.	Hail	...	Wind.	Meteors.
June 1.	49°	45°	47°	29.52	46	29.31	46°	...	...	...	E.	
... 2.	49	43	46	29.22	47	29.13	46	...	...	...	E.	
... 3.	50	42	46	29.19	49	29.47	44	...	..	...	E.	
... 4.	52	42	47	29.70	48	29.79	45	...	..	...	E.	
... 5.	48	41	44	29.79	44	29.80	43	...	...	...	E.	
... 6.	56	45	50	29.80	45	29.78	50	...	...	...	Var.	
... 7.	59	49	54	29.64	53	29.38	54	...	...	...	Var.	
... 8.	60	48	54	28.97	56	28.83	51	...	...	...	S. W.	
... 9.	57	46	51	28.82	53	29.11	51	...	...	...	W.	
... 10.	62	45	53	29.47	55	29.73	55	...	...	...	N. W.	
... 11.	60	46	53	29.90	53	30.02	53	...	...	...	W. E.	
... 12.	56	44	50	30.08	51	30.09	48	...	...	...	E.	
... 13.	55	45	50	30.08	48	30.05	49	...	...	...	E.	
... 14.	55	48	51	30.06	48	30.08	49	...	...	...	E.	
... 15.	66	47	56	30.10	56	30.07	55	...	...	...	E.	
... 16.	68	48	58	30.06	60	30.01	56	...	...	...	E.	
... 17.	57	47	52	30.00	56	29.98	50	...	...	...	E.	
... 18.	57	47	52	29.95	51	29.90	50	...	...	...	E.	
... 19.	59	48	53	29.91	51	29.96	50	...	...	...	E.	
... 20.	68	48	58	30.12	55	29.99	57	...	...	...	E. W.	
... 21.	67	46	56	29.75	56	29.82	58	...	...	...	W.	
... 22.	72	49	60	29.90	56	29.91	63	...	...	...	W.	
... 23.	68	48	58	29.98	56	29.95	59	...	...	...	E.	
... 24.	66	49	57	29.98	58	29.99	54	...	...	...	E.	
... 25.	60	49	54	30.00	56	29 96	51	...	...	...	E.	
... 26.	59	46	52	29.89	54	29.82	54	...	...	...	E.	
... 27.	55	46	50	29.78	56	29.70	52	...	...	...	E.	
... 28.	56	46	51	29.65	50	29.65	53	...	...	...	N.	
... 29.	67	50	53	29.66	52	29.61	53	...	...	...	W.	
... 30	62	47	54	29.61	54	29.62	54	...	...	...	W.	
Means,	59.16	46.33	52.33	29.752	53.65	29.763	51.76	9				

RESULTS.

BAROMETER.		THERMOMETER.	
Highest, . . .	30.12	Highest, . . .	72°
Lowest, . . .	28.82	Lowest, . . .	41°
Mean, . . .	29.757	Mean, . . .	52°.33

WINDS.

W. 5½; NW. 1; N. 1½; NE. 0; E. 19; SE. 0; S. 0; SW. 1; Var. 2.

TABLE VII.—JULY.

1843.	Ther. Max.	Ther. Min.	Ther. Mean.	Barom. ½ p. 8, A.M.	Therm. ½ p. 8, A.M.	Bar. 8, P. M.	Ther. 8, P. M.	Rain.	Hail.	—	Wind.	Meteors.
July 1.	65°	50°	57°	29.70	56	29.57	58°	...	...	...	W.	
... 2.	65	60	62	29.58	57	29.48	65	...	...	...	W.	
... 3.	66	47	56	29.47	62	29.63	58	...	...	...	W.	
... 4.	71	54	62	29.78	59	29.77	64	...	...	...	W.	Lightning.
... 5.	62	48	55	29.64	56	29.47	54	...	...	...	Var.	
... 6.	64	50	57	29.38	60	29.59	57	...	...	...	W.	
... 7.	62	53	57	29.68	59	29.70	59	...	...	...	S. W.	
... 8.	65	53	59	29.73	57	29.89	58	...	...	...	E. S. W.	
... 9.	60	50	55	29.98	55	29.98	55	...	...	...	E.	
... 10.	67	53	60	29.98	62	30.00	60	...	...	...	E.	
... 11.	67	54	60	30.10	62	30.09	61	...	...	...	E.	
... 12.	63	53	58	30.02	60	29.95	59	...	...	...	W.	
... 13.	70	48	59	29.94	63	29.95	62	...	...	...	W.	
... 14.	67	52	59	29.95	60	29.89	60	...	...	...	W.	
... 15.	68	50	59	29.83	62	29.91	57	...	...	...	W.	
... 16.	68	48	58	29.95	57	30.00	58	...	...	...	N. W.	
... 17.	66	52	59	29.96	60	29.84	58	...	...	...	W.	
... 18.	66	49	57	29.58	57	29.61	54	...	...	...	N. W.	
... 19.	58	45	51	29.55	53	29.60	54	...	...	...	N.	
... 20.	57	50	53	29.38	50	29.34	55	...	...	...	W.	
... 21.	65	50	57	29.40	58	29.58	58	...	...	...	N.	
... 22.	60	45	52	29.52	57	29.33	56	...	...	...	W.	
... 23.	60	45	52	29.40	50	29.72	54	...	...	...	N. E.	
... 24.	65	48	56	29.84	54	29.92	58	...	...	...	N.	
... 25.	65	58	61	30.00	58	30.03	62	...	...	...	W.	
... 26.	69	51	60	29.98	68	29.85	60	...	...	...	W.	
... 27.	64	52	58	29.81	60	29.82	61	...	...	...	N. W.	
... 28.	60	53	56	29.85	58	29.55	58	...	...	...	W.	
... 29.	60	49	54	29.35	57	29.23	54	...	...	...	W.	
... 30.	64	50	57	29.34	57	29.50	60	...	...	...	N.	
... 31.	62	52	57	29.65	57	29.70	58	...	...	...	W.	
Means,	64.22	50.70	57.19	29.720	58.09	29.725	58.22	19	2			

RESULTS.

BAROMETER.		THERMOMETER.	
Highest, . . .	30.10	Highest, . . .	71°
Lowest, . . .	29.23	Lowest, . . .	45°
Mean, . . .	29.722	Mean, . . .	57°.19

WINDS.

W. 17; N.W. 3; N. 4; NE. 1; E. 3½; SE. 0; S. 0; SW. 1½; Var. 1.

At Kinfauns, the extremes of pressure during June were 30.24 and 28.40; extremes of temperature 78° and 41°; mean temperature 57°.10; mean temperature of June last year at Kinfauns 55°.86, last month being warmer by 1°·24; whereas here there is a difference of nearly six degrees (viz. 5°.71) betwixt the June of the present year with the corresponding month of 1842; amount of rain 2.10 inches.

Tidal Phenomena.

Arbroath.—A singular phenomenon has lately been very frequently observed at our harbour. When the tide is making, the tide will come rushing two or three feet into the harbour, at the rate of four miles an hour, for ten minutes or so, and all at once turn and rush out again as fast, and again turn and pour into the harbour with its former violence, and continue frequently to advance and recede, when, apparently without any cause, it will resume its former quiet manner of fulfilling its destinies. At Perth, Leith, and at Valetta in Malta, the same phenomenon has occurred, and has been ascribed to earthquakes.

Campbeltown.—On the afternoon of Friday the 7th day of July, an extraordinary motion of the tide was observed here (Fort George). While some carters were employed in lading with timber a vessel on the beach about low water, the tide suddenly advanced 50 or 60 yards, surrounding men and horses with a depth of water of about 18 inches, and as rapidly retired. This rise and subsidence occurred four or five times in succession, within a short period.

The same phenomenon appears to have been observed at Valetta, in Malta, the water suddenly rising to the height of three feet, and overflowing the works of the new dry-dock. During this period, a very strong current was running out of the harbour, which the boatmen could scarcely stem. It is supposed this circumstance must have occurred through some earthquake at a remote distance.

Leith.—A very singular phenomenon was observed here on Friday the 7th July. At low-water the tide suddenly rose about six inches, rushing into the harbour, and in a few minutes again receding with equal velocity.—*Vide* Berwick and Kelso Warder for July 22 and 29, for further particulars.

Table VIII.—August.

1843.	Ther. Max.	Ther. Min.	Ther. Mean.	Barom. ½ p. 8, A.M.	Therm. ½ p. 8, A.M.	Bar. 8 P.M.	Ther. 8 P.M.	Rain.	Hail.	—	Wind.	Meteors.
Aug.. 1.	65°	55°	60°	29.58	55°	29.58	58°	...	...	...	W.	
... 2.	62	54	58	29.40	60	29.27	57	...	...	...	S. W.	
... 3.	66	54	61	29.20	62	29.22	56	...	...	...	W.	
... 4.	67	48	57	29.25	60	29.40	58	...	...	...	N.	
... 5.	62	50	56	29.62	56	29.67	58	...	...	...	W.	
... 6.	62	46	54	29.76	58	29.82	56	...	...	...	W.	
... 7.	66	54	60	29.92	58	29.83	63	...	...	...	S. W.	
... 8.	66	56	61	29.88	60	29.90	59	...	...	...	S. W.	
... 9.	71	47	59	29.93	61	29.96	59	...	...	...	W.	
... 10.	68	57	62	29.99	60	30.03	60	...	...	...	N. E. S.	
... 11.	68	57	62	30.06	60	30.10	60	...	...	...	W.	
... 12.	74	56	65	30.09	64	30.20	65	...	...	...	W.	
... 13.	72	52	62	30.13	59	30.08	63	...	...	...	N.	
... 14.	76	51	63	30.02	64	29.96	62	...	...	...	S. E.	
... 15.	62	55	58	29.97	60	29.96	59	...	...	...	E.	
... 16.	65	57	61	29.98	60	29.97	59	...	...	...	E.	
... 17.	77	60	68	29.99	64	30.00	68	...	...	...	E. N. W.	
... 18.	75	58	66	30.02	66	29.97	66	...	...	...	E. S.	
... 19.	70	54	62	29.78	62	29.64	62	...	...	...	Var.	
... 20.	65	45	55	29.65	58	29.69	54	...	...	...	N.	
... 21.	58	53	55	29.57	57	29.54	55	...	...	...	N. S. W.	
... 22.	64	48	56	29.30	59	29.17	54	...	...	...	W.	Aurora Bor.
... 23.	65	50	57	29.29	58	29.41	57	...	...	...	W.	
... 24.	69	48	58	29.58	58	29.63	56	...	...	...	S. E. W.	
... 25.	63	48	55	29.60	62	29.61	58	...	...	...	W.	
... 26.	64	46	55	29.69	59	29.76	53	...	...	...	S. W.	
... 27.	64	52	58	29.88	54	29.84	58	...	...	...	S. W.	
... 28.	64	48	56	29.72	56	29.45	59	...	...	...	S. W.	
... 29.	65	45	55	29.76	53	29.89	56	...	...	...	W.	
... 30.	64	50	57	30.00	56	30.05	55	...	...	...	W.	
... 31.	68	56	62	30.07	57	30.05	62	...	...	...	W.	
Means,	66.67	51.93	59.16	29.763	59.25	29.762	58.87	19				

RESULTS.

BAROMETER.		THERMOMETER.	
Highest, . . .	30.20	Highest, . . .	77°
Lowest, . . .	29.17	Lowest, . . .	45°
Mean, . . .	29.762	Mean, . . .	59°.16

WINDS.

W. 14; NW. 0½; N. 3; NE. 0½; E. 3; SE. 1½; S. 1; SW. 6½; Var. 1.

At Kinfauns, during last month, the extremes of pressure were 30.25 and 29.30, and of temperature 78° and 42°; mean 58.77°. Amount of rain 3.09.

Proceedings of the Royal Society of Edinburgh.

(Continued from Vol. XXXIV. p. 379.)

Monday, 6th February 1843.

Sir T. MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following communications were read:—

1. On the Law of Visible Position in Single and Binocular Vision, and on the representation of Solid Figures by the Union of dissimilar Plane Pictures on the Retina. By Sir David Brewster, K.H.,—*concluded.*
2. Papers on Glaciers. No. 1, Account of a Geometrical Survey of the Mer de Glace of Chamouni. By Professor Forbes.

Monday, 20th February 1843.

The Right Honourable Lord GREENOCK, Vice-President, in the Chair.

The following communications were read:—

1. On the Anatomy of the Human Placenta. By John Goodsir, Esq.
2. On the Mode in which Sound is produced and diffused, and on the Vibrations caused in the quality of Sound by substance and form. By Sir George S. Mackenzie, Bart.

Monday, 27th February 1843.

Dr ABERCROMBIE, Vice-President, in the Chair.

The following communication was read:—

Papers on Glaciers. No. 2, describing the Rate of Motion of the Ice of the Mer de Glace, deduced from observation. By Professor Forbes.

Monday, 6th March 1843.

The Right Honourable Lord GREENOCK, Vice-President, in the Chair.

The following communications were read :—

1. On the Nature, Locality, and Optical Phenomena of Muscæ Volitantes. By Sir D. Brewster, K.H.
2. On the Structure of the Lymphatic Glands. By John Goodsir, Esq.
3. On the Determination of Heights by the Temperature of Boiling Water. By Professor Forbes.

The investigations in this paper were made in order to reduce certain observations on the boiling point of water, made by the author in the Alps, in 1842.

He considered that it has been too generally assumed that the boiling point corresponds to a barometric pressure which expresses the elasticity of steam taken from the usual tables. He, therefore, attempted to deduce the connection of these data by a direct comparison of cases, in which both the barometer and boiling point were noticed by himself. He finds this result, that the pressures increase *rigorously* in a geometrical ratio, whilst the temperature of the boiling point rises uniformly. This law is not new, for Deluc arrived at the same result; but it appears to have been considered by all late writers as unworthy of adoption, and the scale of the elasticities of vapour by Dalton or Ure has been preferred. Now, these elasticities cannot, it is well known, be accurately represented by a geometrical proportion to the temperature; but Professor Forbes finds that the geometrical ratio represents the barometric heights exactly, whilst the tabular elasticities do not. But, farther, since the common barometric formula shews that the pressure varies geometrically, whilst the height above the sea varies uniformly, we have the same form of relation between the boiling point and the barometric pressure as between the height above the sea and the barometric pressure, namely, that each is as the logarithm of the other. Hence the boiling point falls *exactly* in proportion to the height ascended, and at the rate of 549.5 feet for 1° Fahr.

Monday, 20th March 1843.

Sir DAVID BREWSTER, Vice-President, in the Chair.

The following communication was read :—

Papers on Glaciers. No. 3, On the Structure of Glaciers, and the cause of their motion. By Professor Forbes.

Monday, 27th March 1843.

Dr ABERCROMBIE, Vice-President, in the Chair.

The following communications were read:—

1. Observations on the Temperature and Hygrometric state of the Island of Barbadoes. By R. Lawson, Esq., Assistant-Surgeon of H. M. 47th Regiment.

2. On the Growth and Migration of the Sea-Trout (Salmo Trutta). By Mr John Shaw, Drumlanrig.

The author has here pursued the same course of experimental enquiry regarding the sea-trout, as that formerly followed in relation to the salmon. Having obtained impregnated ova, from a pair of spawning fish, he conveyed these ova to his experimental ponds. This was on 1st November 1839, and the young were excluded from the egg in 75 days. They resembled salmon of the same age, but were somewhat smaller and paler. They took two years to grow about seven inches, and the majority were then converted into smolts. But about one-fourth did not assume the silvery lustre; and this peculiarity, Mr Shaw thinks, distinguishes a like proportion even in the rivers. He then experimented on the smolts in the natural streams, and found, that, after descending to the sea, they returned as *herlings* (*Salmo albus* of Dr Fleming) in July and August, with an addition to their weight of seven or eight ounces. These herlings spawn towards the end of the season of their first ascent; and, after revisiting the sea, they ascend the rivers again in the ensuing months of May and June, with an average weight of 2½ lb. This increase takes place almost entirely in the sea. After spawning for the second time, they descend for the third time to the sea, and make their appearance again in fresh water in the course of the ensuing summer, weighing 4 lbs. They are now in their fifth year, including the two seasons they had passed as fry, anterior to the assumption of the migratory dress and instinct. Descending seawards for the fourth time, they weigh about 6 lbs. when next seen in the rivers, in the course of their sixth summer. These at least were the progressive changes and ratio of increase observed by Mr Shaw, in specimens distinctively marked, and carefully noted, when retaken in the river Nith successively from year to year. The peculiar marks imposed each season are detailed in his paper, and the whole subject is illustrated by an extensive series of specimens, from the day of hatching, to the middle of the sixth year. These specimens are now in the Society's Museum.

3. Experiments with Hydro- and Thermo-Electric Currents; and an Examination of Metals long exposed to Thermo-Electric Currents. By R. Adie, Esq.

Monday, 3d April 1843.

Sir T. MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. Chemical Observations on the Flowers of the Camellia Japonica, Magnolia Grandiflora, and Chrysanthemum Leucanthemum, and on three Proximate Principles which they contain. By Dr Hope.

The author first called the attention of the meeting to the principal facts which he had established in the two memoirs read to the Society in the year 1836. 1*st*, Of these the most important are, that when the acids cause a red colour, and alkalis a green or yellow, in a blue vegetable infusion, they act on different substances;—the acids on erythrogen, and the alkalis on xanthogen. 2*dly*, That the compound of alkalis and xanthogen is of a yellow colour, and that when a blue infusion is changed to green, it is owing to the gradual intermixture of the newly generated yellow with the original blue.

Camellia Japonica.—The blossoms of the beautiful double white Camellia Japonica are remarkable for the peculiar matters which they contain, two of which appear to be distinct vegetable principles. When a parcel of the petals is infused in boiling-hot water, and digested for some time, a nearly colourless infusion is obtained, which is not visibly affected by acids. When solution of potash is added, a light orange colour appears, which gradually deepens, till in a few hours it acquires a very deep orange hue. Solution of ammonia causes a similar effect. When lime-water is employed, it produces turbidity in the infusion, and a pinkish-red colour. This colour gradually assumes a richer tint, and in about 24 hours exhibits a deep pinkish-red. The author has applied the same alkalescent agents to above 150 flowers, both white and coloured, without observing any similar effect.

To this colourable matter he gives the name of Camelline. The author tried the effects of various reagents, but no interesting facts resulted.

When any portion of the flower-leaf is bruised, whether remaining on the parent plant or removed from it, it immediately begins to assume a rusty or ochry tint, which grows deeper and deeper, till it arrives at the tint of perfect ochre.

By various experiments, it was proved that this change of colour depends upon the action of the oxygen of the air. It takes place in a very remarkable degree in oxygen gas, and does not take place at all when the contact of air is excluded by an immersion in mercury, in hydrogen, or carbonic acid gas. To this brown-growing matter, viewing it as a distinct vegetable principle, Dr Hope gave the name of Magnoline, in consequence of its occurring in a very remarkable degree in the Magnolia grandiflora and Magnolia conspicua. A third remarkable circumstance respecting the Camellia petals, is, that they contain a notable quantity of iron. This was little to be expected, seeing that iron is so common a source of colour, and the blossom is entirely colourless. The author was led to search for this metal in consequence of the strong resemblance which the colour which protoxide and protocarbonate of iron acquire by the absorption of oxygen from the air, and the colour acquired by the bruised petals of the Camellia bear to each other. Dr Hope is satisfied that the ochry tints of the Camellia are in no way dependent on, or connected with, the iron which it contains.

Magnolia Grandiflora.—When the petals of the *Magnolia Grandiflora* are boiled for some time in distilled water, a liquor of a very light yellowish brown is obtained. This colour is banished by sulphuric acid. Solution of potassa immediately causes a deep gall-stone yellow, shewing that this, like other white flowers, contains no erythrogen, but abounds in xanthogen. That matter, which becomes of an ochry hue by the contact of air, is very abundant in the Magnolia, so that, if the cuticle be scraped off, in less than a minute the place of abrasion exhibits the ochry tint. The petals of the snow-white *Magnolia conspicua* exhibit the same phenomena in a still more remarkable degree. If a petal of this beautiful flower be bruised between the finger and thumb, it immediately becomes of a very deep brown, and ere long almost black. Some other white flowers, by being crushed and exposed to the air, also acquire the ochry tint, but in a much inferior degree to those already mentioned. The ochry-growing matter is not confined to white flowers. It exists in different species of the blue flowers of the Aconitum, particularly the napellus.

The third flower of which Dr Hope gave an account, was the *Chrysanthemum leucanthemum*, the ox-eye daisy. A strong decoction of the white petals exhibits a light brown colour, which potash quickly converts into the usual gall-stone yellow. When sulphuric acid is dropped into this decoction, the colour becomes a light primrose yellow, and the liquid immediately assumes a gelatinous state. This jelly gradually becomes more consistent, and, in about a quarter of an hour, it is so firm that the capsule containing it may be inverted without the jelly falling out. The matter thus gelatinized by sulphuric acid is peculiar to this plant. The author has applied sulphuric acid to the infusion of at least 150 flowers, without any similar result taking place. The author conceives that the gelatinising matter is a distinct vegetable principle, to which he gave the name of Leucanthemine. To support this view, he drew a comparison between it and the coagulable matters, both of the animal and vegetable kingdoms, viz. animal albumen, casein, fibrin, and vegetable albumen. In drawing this comparison, the author described some remarkable properties of these albuminous matters, which have escaped the observation of all the investigators of the chemistry of organic substances, and some of which he had been in the habit of exhibiting in his lectures for nearly half a century. These are, principally, that sulphuric acid dissolves the albuminous matters in the cold, and yields a nearly colourless solution, without altering the nature of the albumen. But if the liquor be heated to 160°, it acquires an extremely beautiful rich crimson hue, during which change the nature of the albumen is totally altered. Muriatic acid acts upon albuminous matter nearly in the same manner; but the colour which the solution acquires by being heated is a very rich purple. The jelly from the Leucanthemum affords no crimson colour with sulphuric acid. After pointing out other circumstances of difference, the author gave his opinion that the Leucanthemine ought to be considered as a distinct substance *sui generis*, another vegetable principle; and concluded with expressing his hope that the Camellia, Magnolia, and Chrysanthemum furnish three to the long list of vegetable proximate principles.

2. On certain Negative Actions of Light. By Professor Moser of Kœnigsberg. In a Letter to Sir David Brewster.
3. On the Specific Gravity of certain Substances commonly considered lighter than Water. By Dr Davy.

Monday, 17*th April* 1843.

The Right Honourable Lord GREENOCK, Vice-President, in the Chair.

The following Communications were read —

1. On the presence of Organic Matter in the purest Waters from Terrestrial Sources. By Professor Connell.

It must be well known to chemists, that when solution of acetate of lead is added to the transparent and colourless water of springs, wells, and rivers, a more or less dense white cloud is almost invariably produced. This reaction has been usually attributed to the presence of inorganic salts, such as carbonates, sulphates, aud muriates; but it will be found that, generally speaking, this precipitate is formed even after the water has been boiled; that it is usually dissolved by the speedy addition of a drop or two either of acetic or of nitric acid, without visible effervescence; and that the agency of the water with nitrate of silver is commonly too small to admit of its being caused by any muriate. These facts exclude the idea that it is due in the general case to carbonates, sulphates, phosphates, or muriates; although, of course, in those particular cases where the water has enough of such constituents to affect acetate of lead, the reactions will be modified accordingly. Thus, in some cases, where the first action is as above stated, a deposition takes place after a certain interval, of sulphate of lead, no longer soluble in weak acids.

It occurred to the author that the true cause of the reaction was to be found in the presence of organic matter in the water, derived from the decomposition of vegetable matter in the strata or soil through which it had passed. To ascertain whether this view was correct, the precipitate by acetate of lead from several quarts of the town water of St Andrews was decomposed by sulphuretted hydrogen. After filtration, a liquid was obtained, which, besides sulphuric acid derived from precipitated sulphate of lead, was found to contain some organic matter apparently of an azotised nature; but its amount was too small to characterise its properties with accuracy. The salt obtained by saturating the liquid with potash, yielded by distillation empyreumatic vapour, and left a black coaly mass behind. The liquid itself, when neutralized and sufficiently diluted, had still a marked action on lead salts; and it or its potash salt produced more or less precipitate after the interval of a day or two, in acetate of copper and neutralized persulphate of iron.

The author has found this matter in the town waters of Edinburgh and Glasgow, but to a less extent than in that of St Andrews. The Glasgow water shewed the least of the three. He has also found it more or less in every instance he has hitherto tried of transparent and colourless well, spring, or river water, In rainwater it does not exist, and probably could not be found in springs above the limits of vegetation, or in snow or glacier water. It would seem that it ought to perform functions of some importance in the economy of nature, as contributing in a certain degree to the nourishment of plants and even of animals.

2. Biographical Sketch of the late Sir Charles Bell. By Sir John MacNeill.

3. Notice regarding the Bebeeru Tree of British Guiana. By Dr A. Douglas Maclagan.

The plant bearing the above Indian name, and also called Sipeeri by the Dutch colonists, furnishes the hard and heavy timber known by the name of Greenheart. The object of the present paper was to state the result of experiments made by the author on the bark and seeds of the tree, which had been found by Mr Rodie, R.N., to contain a vegetable alkali possessed of the power of checking intermittent fevers. Dr Maclagan stated that the tree was unknown to botanists. Sir William Hooker and Dr Lindley had seen the fruit and declared it to be lauraceous, but the author had been unable to find, in Rees v. Esenbeck's *Systema Laurinarum*, any genus or even sub-order of lauraceous plants, to which he could refer it. With regard to its chemical qualities, Dr M. stated that he had obtained both from the bark and seeds two distinct alkalis, both uncrystallizable, to one of which he applied Mr Rodie's name, Bebeerine; to the other he gave the name of Sipeerine. They could be separated by anhydrous ether, the bebeerine being soluble in that menstruum, whilst the sipeerine was not. Dr M. had likewise obtained, especially from the seeds, a peculiar crystallizable and deliquescent acid, which he called Bebeeric acid, and which seemed to be distinct from every vegetable acid hitherto described.

The author stated that he had instituted experiments with a view to ascertain if a soluble salt of the alkalis could be procured, which might be used as a substitute for sulphate of quinine when dear. He stated, as the result of his trials, that the produce did not amount to more than one and a-half of sulphate per cent. from the bark, but he still calculated that if the bark could be got at a moderate price, the salt of the alkalis might be prepared at a cost inferior to that of sulphate of quinine. Dr Maclagan stated that the bark appeared to be better suited for the purposes of manufacture than the seeds. The author mentioned that sulphate prepared under his directions had been sent out to Demerara, and had been tried there with marked success in intermittent fever by Dr Watt; he had likewise used it with success in a few cases of ague in Edinburgh, and also in periodic headache, so that he had no doubt of its possessing considerable power as an antiperiodic remedy. Lastly, he mentioned that a secret preparation sold under the name of "Warburg's Fever Drops," reputed a good antiperiodic, appeared to him to be a tincture of bebeeru seeds.

Monday, 1st May 1843.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. An attempt to explain the Phenomena of the Freezing Cavern at Orenburg. By Dr Hope.*

2. Observations on the Temperature of the Earth in India. By John Caldecott, Esq. Communicated in a Letter to Professor Forbes.

These thermometers, made by Mr Adie of Edinburgh, were sunk in the ground at Trevandrum, in lat. 8° 30′ 35″, to depths of 3, 6, and 12 *French* feet. Mr Caldecott says,—" I send you herewith the readings of my long thermometers, which, from various causes, I was not able to put into the ground until the 1st of last May (1842). These two months' readings, therefore, will not, of course, have the proper temperature at the respective depths, especially as it has been raining more or less nearly ever since. Still, I think they will surprise you, as being (so far as they go) entirely opposed to Kupffer's opinion, that the superficial temperature of the earth within the tropics is *below* that of the air, and to Boussingault's assertion, as to the invariability of the temperature *one foot* below the surface. The soil in which the thermometers are buried, is one which very soon becomes compact again, after having been disturbed, so that I do not think the rain can much affect the thermometers *now* * * * * . The situation is on the top of the Observatory hill; the soil, the stone called Laterite."

A subsequent letter contains the readings for four entire months, and confirms the important conclusions mentioned above. The mean annual temperature of the air at Trevandrum is 79°.24 F.

1842.	12 Feet.	6 Feet.	3 Feet.	Air.
May,......................	84.672	85.157	83.820	80.09
June,......................	...	84.562	82.062	79.32
July,..............	84.805	83.627	81.025	78.73
August,..................	84.240	82.800	80.220	77.90

The surface of the ground was grass-grown, and the thermometer stems quite exposed.

3. Researches in Hydrodynamics. Second Memoir. On Waves. By J. Scott Russell, Esq., M.A., F.R.S.Ed., &c.

* Already published in this Journal, vol. xxxv. p. 191.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *On Electrical Sounds in the Alps.*—At length we were free of the glacier, and recovered a track by no means obvious, which leads to the Châlets of Breuil, leaving upon our left hand the longer and more difficult route by the Cimes Blanches, conducting to St Giacomo d'Ayas. The atmosphere was very turbid, the ground was covered with half melted snow, and some hail began to fall. We were, perhaps, 1500 feet below the Col, or still above 9000 above the sea, when I noticed a curious sound, which seemed to proceed from the Alpine pole with which I was walking. I asked the guide next me whether he heard it, and what he thought it was. The members of that fraternity are very hard pushed indeed, when they have not an answer ready for an emergency. He therefore replied with great coolness, that the rustling of the stick no doubt proceeded from a worm eating the wood in the interior! This answer did not appear to me satisfactory, and I therefore applied *the experimentum crucis* of reversing the stick, so that the point was now uppermost. The worm was already at the other end! I next held my hand above my head, and my fingers yielded a fizzing sound. There could be but one explanation—we were so near a thunder cloud, as to be highly electrified by induction. I soon perceived that all the angular stones were hissing round us like points near a powerful electrical machine. I told my companions of our situation, and begged Damatter to lower his umbrella, which he had now resumed, and hoisted against the hail shower, and whose gay brass point was likely to become the *paratonnerre* of the party. The words were scarcely out of my mouth, when a clap of thunder, unaccompanied by lightning, justified my precaution.—*Prof. Forbes's Travels through the Alps*, p. 322.

2. *Melting of a Watch in the Pocket of a Man struck by Lightning, without his being injured.*—During a violent thunder storm, a fishing-boat belonging to Midyell, in the Shetland Islands, was struck by lightning. The electric fluid came down the mast, which it tore into shivers, and melted a watch in the pocket of a man who was sitting close by the side of the mast, without injuring him. Not only was the man altogether unhurt, but his clothes also were uninjured; and he was not aware of what had taken place, until, on taking out his watch, he found it was fused into one mass.—G. W. Spence, Esq.

GEOLOGY.

3. *Rise of the Sea by sudden rise of a body of Subterranean Water; and Flowing of Water of the Sea into the Land.*—At Samos in Cephalonia, in the summer of 1827, the sea rose about ten feet in perpendicular height, and raised on the shore some large masses of stone, brought there for

the purpose of forming a new mole on the site of the ancient mole. The weather at the time was fine and serene, indeed so calm, that the people of the place, alarmed by the rising of the water, came into the open air with lamps in their hands; it was at night. The phenomenon was without apparent cause; nothing unusual preceded or followed,—no motion of the earth,—not the slightest shock of an earthquake was perceived. It might have been occasioned by a large quantity of subterraneous water suddenly rising in the sea. As the adjoining hills abound in caverns, and the natural drainage of the hills takes place chiefly under ground, this explanation is not improbable, especially considering that the phenomenon was entirely local, and confined to the shore of Samos. However it may be accounted for, I have thought the fact deserving of notice, and worthy of being recorded. There is much that is mysterious in the physical history of these islands, especially in connection with the distribution of water, and too many facts on the subject cannot be collected; one may help to illustrate another, and ultimately some satisfactory explanation may be afforded.

The next phenomenon I have to mention is very extraordinary, and apparently contrary to the order of nature; it is the flowing by the water of the sea into the land, in currents or rivulets which descend and are lost in the bowels of the earth. This phenomenon occurs in Cephalonia, about a mile and a half from the town of Argostoli, near the entrance of the harbour, where the shore is composed of freestone, and is low and cavernous from the action of the waves.

The descending streams of salt water are four in number; they flow with such rapidity, that an enterprising gentleman, an Englishman, has erected a grist-mill on one of them, with great success. I have been informed that it produces him L.300 a-year. The flow is constant, unless the mouths, through which the water enters, are obstructed by sea-weed. No noise is produced by the descent of the sea-water, and rarely is any air disengaged; the streams have been watched during earthquakes, and have not been found affected by them. It is stated that fresh water is perpetually flowing through fissures in the rock from the land into the trench which has been dug for the reception of the mill-wheel, and that when the sea-water is prevented rushing in, then the water in the trench rises higher by several inches than usual, and is brackish to the taste. The phenomenon has been long known to the natives; familiar with it, it has excited no interest; they appear hardly to give it a thought. It is only recently that it has been brought to the knowledge of the English, within the last five or six years, and it is now become a subject of anxious inquiry and speculation. The little information I have obtained respecting these extraordinary currents, I owe to my friend Dr White, surgeon of the second battalion of the Rifle Brigade; it was collected by him when stationed in the Ionian Islands, several years after my departure from them. Probably they will soon be fully described; till then, and till they have been minutely investigated, conjectures only can be made respecting their cause.—*Dr Davy on the Ionian Islands and Malta,* vol. i. p. 164.

4. *Early Rise of the Nile in* 1843.—M. Jomard has communicated to the French Academy of Sciences, the curious fact that, in 1843, the increase of the waters of the Nile at Cairo took place two months sooner than usual. In that latitude the rise occurs generally between the 1st and 10th of July, but in this instance it commenced on the night of the 5th and 6th of May. It lasted four days, and attained a height of 0.22 metres. Bruce has instanced examples of second floods of the Nile, but these were late and not early risings; for example, that of 1737, which took place after the autumnal equinox, while the waters were falling and the country was inundated. He remarks that the same phenomenon occurred also in the time of Cleopatra; but such cases are very different from the present one; for, during the month of May, southerly winds prevail. In Abyssinia, the Nile begins to rise about the 17th or 18th of June, at the commencement of the north winds, which rarely fail to blow at the solstice (Niebuhr, Forskal). In the narrative of Abdellatif it is mentioned, that, in the year 1200, the rise began about the 25th June, but that it was preceded, *two months before,* by the appearance of a green colour in the waters of the river. This, however, is a very common occurrence, and quite distinct from the rise now noticed.

5. *Observations on the dissemination of minute Organic Bodies, still living and active, in Asia, Australia, and Africa; also on the Prevailing Formation of the Oolitic Limestones of the Jura by minute Polythalamous animals.* By M. Ehrenberg.—After a recapitulation of the investigations he has already published respecting the presence of minute living organisms in different quarters of the globe, the author mentions twenty-two new localities in Asia, which have afforded him 461 infusoria, consisting of 260 species referable to 80 distinct genera. The genus *Biblarium,* hitherto met with only in a fossil state, at Cassel, has been discovered alive under the form of *B. glans,* in Asia Minor, near Angora. The genera *Spirodiscus, Tetragramma, Discocephalus,* and *Disoma,* are peculiar to Asia, except, perhaps, the second, which is likewise observed in Lybia. The other 76 genera are common to Asia and Europe, and only the first mentioned of these quarters of the globe possesses species which are peculiar to it.

In the course of his Memoir, the author points out the advantages which arise from subjecting the faintest traces of animal organic life to a searching examination, by announcing that the oolitic limestone of the Jura formation in Germany as well as in England, wherever it is granular, appears to be composed principally of Meloniæ. The mountain limestones of the lake Onega, in Russia, are in like manner composed of Meloniae of the same species and size. In many instances, these Meloniae of the oolite limestone are so completely transformed into calcareous spar, that it is impossible even to discover their heads. In other cases, we can recognise a sparry nucleus in the centre of a great number of Meloniae, which at once leads us to suppose that there must have been a precipitation of grains of sand at the time when the transformation into calcareous spar began to take place. The Textilariae, and, as far as yet appears, the Nodosariae, which are noticed among these Meloniae in the oolitic limestone, as well as in the mountain limestone, present marks of difference from the genera still found in a living state.*

6. *New Volcano in the island of Meleda in the Adriatic.*—The *Gazzetta di Milano* announces that a new volcano was formed, about the middle of last September, in the mountainous island of Meleda, situated in the Adriatic, near Ragusa. In the night of the 14th, the crew of a Roman vessel, the *Madonna-di-san-Ciriaco,* which happened to be in the neighbourhood, and had felt successive shocks for four hours on the previous evening, saw lava issuing from the centre of the island, and flow over a space of about half a mile. The following night, while sailing in the neighbouring canal, about two miles from land, they observed that seven craters had been formed in the mountains of the island, and were throwing out burning substances. The appearance of this volcano has been considered as having an immediate relation to the very violent earthquakes which were felt at Ragusa on the evening of the 15th September. However this may be, the following are a few particulars respecting the earthquake which occurred at this date. The first shock was felt at 57 minutes past 4 o'clock. It was severe, and sensibly followed the direction from south-west. To this first shock, which lasted for four seconds, succeeded another still more violent, which continued from four to five seconds, the wind blowing from the south-west; and it was accompanied with a subterraneous noise. Other shocks occurred in the evening, nor did they cease till midnight. At 28 minutes past 1 o'clock on the morning of the 15th, a very violent oscillatory motion in the direction of south-west was experienced.

The inhabitants were beginning to become greatly alarmed; many shocks succeeded which took place during the day, and it was at last determined to quit the town. A horizontal cloud, which extended from the north-east to the south, known in the country by the name of *houtre,* and which appears to accompany every earthquake, and which was very conspicuous during the earthquake of 1669 which destroyed Ragusa, again appeared on this occasion, and it was not displaced by the wind. At Ragusa Vecchia, the shocks were less severe than at Ragusa, but they were more violent at Ombla, in the island of Guippassa, and in the neighbouring districts, particularly in Erzegovina. In the island of Curzola, the shocks took place on the 14th and 15th; as well as at Zara, Obrovazzo, and Almissa. The shocks were sometimes preceded by a detonation, and the barometer sank very sensibly. In Carniola, at Cilli, some shocks of an earthquake were likewise felt on the morning of the 15th. On the 16th, at Ragusa, two other shocks were again felt during the night, when soon after a brilliant meteor was seen to the east.

* From L'Institut., No. 517, p. 401.

Shocks, accompanied by the same phenomenon, were renewed every day up to the 23d. On the 18th and 20th they were felt nearly at all hours of the day. On the 24th, at 10 minutes past 2 o'clock in the morning, a subterranean noise was heard, and immediately afterwards the shock of an earthquake was felt so severely, that the inhabitants awoke in sudden fright, abandoned their houses in the greatest haste, and left the town. A strong wind from N.N.E. had risen immediately before the time when these shocks were felt; the sky cleared, and some bands of deep blue were distinctly seen in the neighbourhood of the Milky Way. On the 25th, about 3 o'clock in the morning, the ground again trembled for three seconds; the inhabitants of Ragusa were in the greatest consternation. On the 26th, about 27 minutes past 3 in the morning, another shock, much more violent than that of the 24th, was felt. The stars sparkled in a sky of the purest azure; but the scene changed in a few seconds. After a subterranean noise, many severe shocks succeeded each other at short intervals. At the same time a wind from the south-east enveloped the sky in clouds, the barometer sunk to 28.2, and the thermometer indicated 10° R. (54°.50 F.). All the houses of the town were shaken to such a degree, that they ran the risk of being entirely destroyed on the recurrence of a shock, even though not very severe. These shocks were also felt in the islands of Curzola and Meleda.*

Since the month of September, new shocks have taken place at Ragusa and its neighbourhood; but we have not obtained sufficiently exact information regarding them.

In consequence of the frequent shocks of earthquake which have occurred at Khalki since 17th September, this unfortunate island has been completely ruined. The island of Scarpanto, situated at a short distance from Khalki, has likewise been subject to many severe earthquakes. At Rhodes, on the 10th October, about an hour after midnight, a severe shock was likewise felt, and the undulation was prolonged beyond 30 seconds. At Khalki, a stream of boiling and sulphureous water was thrown up on the declivity of a small hill, at the same time that the shocks took place. This stream still continues to flow.

On the 3d October, a shock of an earthquake was felt, at 10 o'clock in the evening, in some parts of the department of Ille-et-Vilaine. It lasted for two seconds. Another shock occurred on the 4th, and continued for upwards of 40 seconds.

The shock of an earthquake was felt at Chateaugiron on 5th October, about 45 minutes past 9 in the morning. This was taken by many people for a peal of thunder; it did not last longer than 2 seconds. On the

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* We need hardly remind our geological readers, that in the summer of 1826 a Commission was sent by the Austrian Government to the island of Meleda, to investigate the repeated detonations which had caused much alarm to the inhabitants from March 1822 up to that period. The Commissioners were Messrs Riepl and Partsch, and an interesting report was published by the latter at Vienna in 1826, accompanied by a beautiful map.—Edit.

following morning there was another shock, about 30 mintues past 9 o'clock: it was more severe, and continued for more than 40 seconds. The noise, which might be compared to that of a heavily loaded waggon, ran from the south to the north. The same shocks were felt on the roads of Rennes, Nantes, and Jezé, at some distance from Corps-Nuds.*

ZOOLOGY.

7. *On the Structure and Development of a newly discovered Animalcule of the Human Skin, the Entozoon folliculorum.* By Erasmus Wilson, Esq., *Lecturer on Anatomy and Physiology at the Middlesex Hospital.*—While engaged in researches on the minute anatomy of the skin, and its subsidiary organs, and particularly on the microscopical composition of the sebaceous substance, the author learned that Dr Simon of Berlin had discovered an animalcule which inhabits the hair follicles of the human integument, and of which a description was published in a memoir contained in the first number of Müller's Archiv. for 1842. Of this memoir the author gives a translation at full length. He then states that, after careful search, he at length succeeded in finding the parasitic animals in question, and proceeded to investigate more fully and minutely than Dr Simon had done the details of their structure, and the circumstances of their origin and development. They exist in the sebaceous follicles of almost every individual, but are found more especially in those persons who possess a torpid skin; they increase in number during sickness, so as in general to be met with in great abundance after death. In living and healthy persons, from one to three or four of these entozoa are contained in each follicle. They are more numerous in the follicles situated in the depression by the side of the nose; but they are also found in those of the breast and abdomen, and on the back and loins. Their form changes in the progress of their growth. The perfect animal presents an elongated body, divisible into a head, thorax, and abdomen. From the front of the head proceed two moveable arms, apparently formed for prehension; and to the under side of the thorax are attached four pairs of legs, terminated by claws. The author distinguishes two principal varieties of the adult animal; the one remarkable for the great length of the abdomen and roundness of the caudal extremity; while the other is characterized by greater compactness of form, a shorter abdomen, and a more pointed tail. The first variety was found to measure in length, from 1-100th to the 45th, and the second, from the 1-160th to the 109th part of an inch.

8. *Discovery of a Mycoderm which appears to constitute the disease known by the name of Plica Polonica.* By Dr Gunsbourg of Breslau.—M. Gunsbourg's researches have been made on two different specimens, both obtained from women, and which differ only in the greater or less degree of resistance in the agglutinative mass. He found this mass formed,

1*st*, Of a great number of epidermic cellules in juxta-position, of more

* From L'Institut. No. 517, p, 404.

than ordinary size, and each containing either a very voluminous nucleus, or a multitude of very minute globules, resembling those which exist in the cells of a degenerated inflammation;

2d, Of hairs contained in their sheaths, and which, compared under the microscope with those of an individual in health, had scarcely one-third of the normal diameter; they were irregularly articulated and variously ramified; their sheath was thick, and raised in some places by the mycoderms, which were trying to pierce it in order to get to the outside.

3d, Of some cells of fat which dissolved rapidly in ether;

4th, Of Mycoderms originating in the bulb of the hairs, and still remaining attached to these hairs in the nearest part of the bulb. The cellules, variable in number, of which the trunk of this mycoderm is composed, are at first very distinct, and become less and less so, in proportion as the growth proceeds. The trunks of many neighbouring mycoderms are often united in a retiform manner.

Oval, umbilical sporules, are united to the trunk either by the umbilicus itself, or by a very short thread; they are most commonly double.

Although these mycoderms are wholly contained in the sheath, and covered with a thick layer of sporules, they usually pierce this sheath towards the base of the hairs. Some are also found, which are wholly on the outside of the sheath; it is these, particularly, which are united to each other; the net-work they form is pretty considerable.

M. Gunsbourg's note, which is merely the precursor of a more complete work, was accompanied with drawings which shew the mycoderm of the *plica* in its different states, along with specimens of the plicated mass. These latter afford the opportunity of repeating his observations.

He also gives in his note a statistical view of the Plica in the duchy of Posen, dividing the cases according to sex, age, races, and even religion.*

CHEMISTRY AND MINERALOGY.

9. *Professor Connell on the Tagua Nut or Vegetable Ivory.*†—At a meeting of the St Andrews Philosophical Society held on the 4th of December, Professor Connell exhibited specimens of the Tagua Nut or Vegetable Ivory, both in the natural condition and carved into orna-

* Comptes Rendus, 1843, No. 6, p. 250.

† There is an article now coming into general use, called *vegetable ivory*, concerning which we have had so many inquiries, that we shall probably be gratifying our readers by a more particular account of it than we can give in the notices to correspondents. The ivory-nut is the produce of a tree found on the banks of the River Magdalena, in that part of South America formerly called New Granada, but now constituting the Republic of Columbia. Humboldt and Bonpland found it at places called Barancas Vermejas and Ibague, at the foot of Mount Quindiu, and also on the Rio Opon and Caño di Chucuri. The Spanish botanists Ruiz and Pavon also met with it in the groves of Peru in the hotter parts of the Andes, and named it Phytelephas macrocarpa; the Prussian botanist

ments, and stated that he was engaged in a chemical examination of this curious substance, and expected to have an early opportunity of mentioning the results he had obtained. In the meantime, he said that he found it to contain, amongst other constituents, a few per-cents of an azotised substance, which, so far as he had hitherto examined it, had all the properties of vegetable casein. It also contained a very little albumen. These seemed to be the principal azotised constituents.

10. *Galvanic Silvering of Cast-Iron.*—Professor Connell lately exhibited to the Society of St Andrews the process of galvanic silvering of cast-iron. The liquid employed for this purpose was that suggested by M. Tewreinoff of St Petersburgh, and consists of a strong solution of moist chloride of silver and cyanide of potassium in water. The galvanic power employed by Mr Connell was two pairs of Smee's plates, the

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Wildenow changed the name, without sufficient reason, to Elephantusia macrocarpa; but his bad example is not followed. The natives of Columbia call it Tagua, or Cabeza de Negro (Negro's head), in allusion, we presume, to the figure of the nut. Almost all we know about it is contained in the following memorandum, published by the Spanish writers above mentioned:—"The Indians cover their cottages with the leaves of this most beautiful palm. The fruit at first contains a clear insipid fluid, by which travellers allay their thirst; afterwards this same liquor becomes milky and sweet, and it changes it taste by degrees as it acquires solidity, till at last it is almost as hard as ivory. The liquor contained in the young fruits becomes acid if they are cut from the tree and kept some time. From the kernels the Indians fashion the knobs of walking-sticks, the reels of spindles, and little toys, which are whiter than ivory, and as hard, if they are not put under water; and if they are, they become white and hard again when dried. Bears devour the young fruit with avidity." The tree which furnishes these nuts is a palm, although Humboldt and Kunth have referred it, for some reason with which we are unacquainted, to the order of Screw Pines (Pandanaceæ), an error which is preserved by all botanists up to the present day. Two species are known, the *Phytelephas macrocarpa*, or large-fruited, and *microcarpa*, or small-fruited. The part of the kernel which is thus similar to ivory is what is called the albumen; that is to say, the nutritious substance which surrounds the embryo, and which is destined to feed it when it begins to grow. It is of the same nature, though not of the same consistence, as the flour of corn, the spicy substance of the nutmeg, and the meat of the cocoa-nut, which in other palm-trees becomes very hard: that of the Date Palm is quite as hard, if not harder; but it is not white enough or large enough to be worth using by the turner. The Doum Palm, or Forking Palm, of Thebes, the fruits of which are called gingerbread-nuts at Alexandria, has a similar albumen, which is turned into beads for rosaries; and our correspondent Mr Murray informs us that he has a model of the Double Cocoa-nut, or Coco de Mer, beautifully carved from a portion of its own albumen, as hard as ivory, and susceptible of as fine a polish. He says he has also seen a figure, cut from the same specimen, forming the end of the shaft of a lady's parasol, not to be discriminated from one carved in ivory.—*Gardeners' Chronicle.*

polished cast-iron being connected with the negative pole and immersed in the filtered solution. One pair of the plates had hardly sufficient intensity for the purpose. The coating of silver speedily begins to be deposited, and is sufficiently thick in half an hour. It is then polished by a smooth surface of iron and subsequent friction with soft leather. There seems little reason to doubt that the process might be applied, under proper arrangements, to the coating and preserving of large objects of cast-iron, such as statues, &c., exposed to the weather; and by occasional simple cleaning, such as is usually applied to plated objects, the surface of the metal might be maintained with all the brilliancy of polished silver. Mr Connell has also found that a solution in water of the crystallised double cyanide of gold and potassium answers very well for the galvanic gilding of silver. This salt is easily prepared by precipitating a solution of gold in aqua regia by excess of ammonia; and dissolving the well-washed and still moist fulminating gold in a warm solution in water of a quantity of cyanide of potassium, equal in amount to the gold which had been deposited in the aqua regia. By concentrating the solution, the double cyanide is obtained on cooling. With a moderately strong solution of this salt, and a power of two pairs of Smee's plates, small silver objects, such as salt-spoons, were readily gilded after three-quarters of an hour's action. For larger objects, a more powerful battery would probably be required. In this way the trouble of previously preparing the cyanide of gold is avoided, which substance, as is well known, has been used for electric gilding, when dissolved in cyanide or procyanide of potassium.

11. *Extraction of Palladium in Brazil.*—The extraction of palladium from the auriferous sand of Brazil consists in fusing it with silver, and, consequently, forming a quaternary alloy of gold, palladium, silver, and copper, which is granulated by projecting it into water.

By treating this alloy with nitric acid, the gold is separated from the other metals which are soluble in the acid; the silver is precipitated by a solution of common salt in the state of insoluble chloride, which being separated, the copper and palladium are precipitated by plates of zinc. The pulverulent deposit of these metals is re-dissolved in nitric acid, and the solution precipitated by excess of ammonia, which re-dissolves the oxide of copper and of palladium. When the ammoniacal solution of these metals is saturated with hydrochloric acid, a double chloride of palladium and ammonia is deposited in the state of a crystalline yellow powder, and this, when calcined in a crucible, is readily decomposed, and leaves spongy palladium.—*Philosophical Magazine,* vol. xxiii., No. 153, p. 398.

12. *Wöhlerite, a New Mineral Species.*—Under this name, given in honour of Professor Wöhler, Scheerer of Christiania has described in Poggendorff's Annals a substance found on some islands of the Lange-

sund-Fjord, in the neighbourhood of Brevig, in Norway. It was also met with by Scheerer on the island of Lövöe, quite close to the spot where the Thorite was discovered by Pastor Esmark some years ago. Like the latter, it occurs in zircon-syenite, and, as it appears, more particularly in the variety in which the hornblende is replaced by black mica. Elaolite frequently accompanies this mineral, and, in the form of greenish grey, or reddish grains, is disseminated in the white or greyish white felspar which constitutes the principal mass of the syenite. Some of the specimens collected likewise contain embedded zircon and pyrochlore. The following are the *mineralogical characters* of Wöhlerite:—It occurs embedded in syenite, sometimes in angular grains, sometimes, though less frequently, in broad, prismatic or tabular crystals, of which, however, no exact crystallographic description can be given, as their bounding surfaces seem to be very imperfectly developed, and as it is very difficult to separate them from the surrounding mass. Indications of cleavage in one direction are perceptible, but that only in some instances. The colour is yellow, of various shades, passing into brownish, viz. light yellow, wine yellow, honey yellow, brownish yellow. These tints are not pure, but are mixed with more or less grey. Colour of the powder, yellowish white. The degree of transparency varies very much as in the zircon. The crystalline faces exhibit a vitreous lustre, and the fracture surfaces a resinous lustre. Fracture more or less conchoidal, sometimes passing into splintery and granular. Hardness between felspar and apatite. Sp. gr. 3.41. It is very difficult to determine quantitatively the exact *chemical constitution* of the Wöhlerite, and Scheerer gives the following analysis as only approximative. No. I. is the result of a complete analysis, and No. II. that of a correcting investigation:—

	I.		II.
Silica,	30.62	63.97	63.25
Tantalic acid,	14.47		
Zirconia,	15.17		
Oxide of iron,	2.12		
Protoxide of manganese,	1.55		
Lime,	26.19		25.97
Soda,	7.78		8.39
Magnesia,	0.40		0.45
Water,	0.24		
	98.54		

Possibly there may also be traces of potash and oxide of tin.

13. *Leonhardite, a new Mineral.*—Professor Blum has given this name to a substance resembling Laumonite, and hitherto included under that species. In Poggendorff's Annals we find the following description and analysis by the discoverer and M. W. Delff:—Two kinds of pseudomorphoses have for some time been known to be exhibited by the Prehnite occurring in fissures in the somewhat decomposed diorite

of the "Sattel," near Niederkirchen, not far from Wolfstein, in Rhenish Bavaria. These have lately been described by G. Leonhard. One of them is easily explained, viz. when the prehnite assumes the trapezoidal form previously belonging to Analcime, an appearance already observed and described by Haidinger. The origin of the other, however, has given rise to various opinions, and its form has been partly attributed to datolite, and partly to stilbite, mesotype, or laumonite. Professor Blum had regarded this variety of prehnite as derived from laumonite, especially as the angles presented great similarity; and it was only some months ago, when being engaged in a work on pseudomorphoses, he examined the crystals in question with greater attention, that he found the similarity existed only with respect to the size of the angles, but not to their position, which is just *reversed*, in so far as the lateral angles are concerned. As, then, it was thus evident that it could not have been laumonite which had undergone the conversion into prehnite, what substance was it? The *Mineral-comptoir* at Heidelberg, lately received a quantity of laumonite from the neighbourhood of Schemnitz, and it immediately struck Professor Blum that the crystals were the same as the pseudomorphic ones, a supposition which was confirmed by measurement. He thus found that this so-termed laumonite was to be regarded as a new mineral; and the analysis made by Dr Delff shewed that its composition was different from that of the true laumonite. There is, however, a remarkable resemblance between the two substances, both in external appearance, and in many of their properties. The great tendency to decomposition exhibited by laumonite, exists also in this new species, which Professor Blum and Dr Delff propose to term Leonhardite. The crystallographic form is klinorhombic. Primitive form: an oblique rhombic prism, M : M′ = 96° 30′ and 83° 30′; P : M = 114° and 64°. This is the only form observed. The prismatic crystals are frequently aggregated together, so that a large individual is composed of several smaller ones, which are sometimes of equal length, but on other occasions vary in this respect. The lateral planes are streaked in a direction parallel to the principal axis; two of them are often larger than the others. Sometimes crystalline-columnar and granular masses occur. Cleavage very perfect parallel to the lateral planes, imperfect in the direction of P. Fracture uneven. Hardness = 3 − 3.5 (in fresh pieces). Brittle, easily frangible. Sp. gr. = 2.25. Translucent on the edges. Lustre pearly, especially on the perfect cleavage-surfaces; on the fractured surface vitreous. Colour white, passing into yellow, and more rarely into brownish. Frequently coated with a brownish or black powder. Streak white. The Leonhardite occurs near Schemnitz, in Hungary, in the fissures and drusy cavities of a trachytic rock; sometimes in considerable quantity. It is also found imbedded, and then imparts a porphyritic structure to the trachyte. The pseudomorphic prehnite above mentioned, undoubtedly owes its form to this mineral. Analyses of the leonhardite have been performed by Dr Delff and Von Babo, and the following are the results.

Delff.		V. Babo.	
Silica, .	56.128	Silica, .	55.00
Alumina, .	22.980	Alumina,	24.36
Lime, .	9.251	Lime, .	10.50
Water and loss,	11.641	Water, .	12.30
	100		102.16

Although these two analyses do not correspond exactly with each other, yet they both lead to the conclusion that the chemical constitution of the Leonhardite and Laumonite is distinct, inasmuch as the Laumonite contains much more water and much less silica than the Leonhardite. As the analyses of Laumonite by Vogel, Gmelin, Dufrenoy, and Connell,* differ a little from one another, Dr Delff and Von Babo have also repeated the analysis of that mineral, and the following are the results.

	V. Babo.		Delff.
Silica, .	52.3		51.17
Alumina,	22.3		21.23
Lime, .	12.0		12.43
Water, .	14.2	and loss	15.17
	100.8		100

MISCELLANEOUS.

14. *Strawberries,* 1597.—Extract from the Accounts of the Lords High Treasurers of Scotland.

Junii, 1597.—Item in drink sylver to ane man that brocht stray berreis to His Majestie fra Alloway. W. C. T. V. LI.

15. *Yew and laburnum imported to England for the manufacture of bows in the 16th century.*—" Conspicite etiam nunc Anglos apud Rhetos negociantes, et in eorum montibus, Taxi et Laburni, arbores cædentes, quas certa longitudine in assulas findentes in suorum arcuum usum, in fasciculos deinde colligunt, atque lacu Valstatensi cymbis ad Rhenum usque devehunt, et deinde secundo Rheno descendentes in Angliam usque deportant, ubi arcus denum conficiunt."—Petrus Bellonius, " De neglecta stirpium, cultura," &c., Antverpiæ, 1589, p. 57. W. C. T.

16. *Capture of Whales in Faroe.*—The capture of the small Caaing-whale *(Delphinus melas)*, the Grind of the natives of the Faroe Islands, is one of the most important events which occurs in the course of the year in that country, as, besides the value of the oil, which averages about L.2 sterling for each fish, the flesh and the blubber are one of the principal articles of food, the first being dried and the latter salted, forming a wholesome and nourishing diet, though perhaps not very palatable to our more fastidious

* See Professor Connell's paper in this Journal, vol. vi. p. 262.

tastes. The usual mode of capturing these animals has hitherto been (as described in the Edinburgh Cabinet Library on Iceland, Greenland, and the Faroe Islands, and in other publications) by a number of boats collecting to seaward of the drove, and driving them into the most accessible sandy bay, where, if the fish ground in shallow water, they are speedily dispatched. In this way many hundreds are often slain; but they are sometimes chased for a considerable distance before being taken; thus 288 were captured on the 21st of June 1843, the chase of which commenced on the 18th, and extended round the islands a distance of about forty miles.

This year a new method of taking them has for the first time been tried, and with success. A net about 200 fathoms long, 14 deep, and the meshes 1 foot square, has been purchased at the cost of about L.140. The first time it was used 236 were taken, and subsequently, up to the 26th August, about 400 more,—so that the prime cost of the first net will soon be recovered, one-eighth of the capture being set apart for that purpose, and probably more nets will be purchased.

The same means might be profitably employed in taking these animals on our northern coasts, where they often occur in great numbers. The bones and flesh, if not used as food, might with advantage be employed as manure. W. C. TREVELYAN, Esq.

NEW PUBLICATIONS RECEIVED.

1. The Invisible Universe disclosed; or the real Plan and Government of the Universe. By Henry Coleman Johnstone, Esq. London, Effingham Wilson. 12mo. 1843.

2. Observations on Days of Unusual Magnetic Disturbance; made at the British Colonial Magnetic Observatories, under the Departments of the Ordnance and Admiralty. Printed by the British Government, under the Superintendence of Lieut.-Colonel Edward Sabine, of the Royal Artillery. Part 1. 1840–41. London. Published for Her Majesty's Stationery Office, by Longman & Co. 1843. 2to. *For this very important volume we are in some measure indebted to Baron Humboldt and the late Duke of Sussex. Lieut.-Colonel Sabine, with his usual ability and accuracy, has edited the work, and presented it to the scientific world in a form worthy of the British Government.*

3. Rapport sur un Memoire de M. F. de Castelnau, relatif au System Silurien de L'Amerique Septentrionale. From the rapporteurs, Elie de Beaumont, Brongniart, Milne Edwards, and Dufrenoy. 4to. 1843.

4. Rapport sur un Memoire de M. Alcide D'Orbigny, intitulé Considerations Generales sur la Geologie de L'Amerique Meridionale. 2to. From M. Elie de Beaumont.

5. Observations on the Practicability and Utility of opening a Communication between the Red Sea and the Mediterranean, by a Ship Ca-

nal, through the Isthmus of Suez; with an Outline Map of the Isthmus and Lower Egypt, shewing the Tract of the Ancient as well as of the proposed Canal. By A. Anderson. 8vo. pp. 48. Smith, Elder, & Co., London. 1843. *This pamphlet, on an important subject, was sent to us by our friend Mr Wilson Pillans, but too late for particular notice in this Number.*

6. Inquiry into the Means of establishing a Ship Navigation between the Mediterranean and Red Seas. By James Vetch, Capt. R.E., F.R.S. Illustrated by a Map. London, P. Richardson, Cornhill. 1843. 8vo. pp. 32. From the Author. *Of this memoir, by an accomplished engineer, an account appears at page* 136 *of the present number of our Journal.*

7. Popular Conchology, or the Shell Cabinet arranged; being an Introduction to the Modern System of Conchology. By Agnes Catlow. 12mo, pp. 300, with numerous woodcuts. Longman, London, 1843.

This little volume will prove a valuable aid, not only to the youthful conchologist, but even to those who have long studied this beautiful branch of Natural History. Since the Linnean classification has been to a certain extent superseded by that of Lamarck, such a work has been greatly wanted; and we rejoice to see it now so well executed. Most possessors of shells are ambitious to derive from them further interest than arises out of the mere contemplation of their beautiful forms and colours, and anxiously enquire for some book, as a guide to their classification. By the aid of Miss Catlow's Work, even beginners may arrange their shell cabinets, in strict conformity with the most advanced state of the science, so far as respects the genera; and when the genera are correctly discriminated and classed, the main difficulty of the study is overcome. The system of Lamarck has been closely followed, with a few alterations, which subsequent more accurate knowledge of the habits and physiology of the molluscous animals has rendered essential. The introductory chapter, which is written with clearness and elegance, embodies useful information respecting the inhabitants and the mode of formation and growth of shells. The woodcuts are numerous and accurate. The style of the work is perspicuous and simple; and there is throughout a gracefulness and refinement of thought and expression, that seems singularly appropriate to the description of these beautiful creations of nature.

8. Contributions to the Geology of the United States. By William Barton Rogers, Professor of Natural Philosophy in the University of Virginia, and Henry Davison Rogers, Professor of Geology in the University of Pennsylvania. Philadelphia. 1843. From the Authors. *These interesting and valuable contributions, now so well known to British geologists, ought to be reprinted for the use of geological enquirers in this country.*

9. Popular Cyclopædia of Natural Science and Animal Physiology. By W. B. Carpenter, M.D., &c. &c. Parts 1 and 2, pp. 79. *Dr Carpenter, in this Treatise on Animal Physiology, which we recommend to the cultivators of this important branch of zoology, displays his usual acuteness and skill in popular and accurate scientific writing and reasoning.*

10. The Tenth Annual Report of the Royal Cornwall Polytechnic Society for 1842. Parts 1 and 2.

11. Magnetical Investigations. By the Reverend William Scoresby, F.R.S. L. & E., and Corresponding Member of the Institute of France. Part II. London; Longmans, 1843. *We expect an article on these experiments for a future occasion.*

12. Experimental Researches; Chemical and Agricultural—shewing Carbon to be a compound body, made by Plants, and decomposed by Putrefaction. By Robert Rigg, F.R.S. 12mo, pp. 264. Smith, Elder, and Co. London. 1843. *This interesting work, received when going to press, will be noticed afterwards.*

13. Zoology of the Voyage of H.M.S. Beagle, No. II. of Part V. 1843. London, Smith, Elder, and Co. *This number contains the conclusion of Mr Bell's account of the Reptiles collected during the voyage of the Beagle.*

14. A Sermon preached in Ripon Cathedral. By the Rev. Henry Parr Hamilton, A.M., F.R.S., Late Fellow of Trinity College, Cambridge. Ripon. 1843. From the Author. *We recommend this discourse to those who may wish to obtain sound views in regard to the bearings of science—particularly geological science—on religion.*

15. The American Journal of Science and the Arts. Conducted by Professor Silliman and Benjamin Silliman jun.—up to vol. lxv., No. 5. October 1843.

16. Maps and Illustrations of the Physical Geography of the Globe. By Dr H. Berghaus, Professor of Geography, Berlin, and an Ethnographic Map of Europe, by Dr Gustaf Kombst. Folio. Edinburgh, J. and W. & A. K. Johnstons. 1843. From the Editors. *These beautiful, accurate, and admirably engraved Maps and Illustrations, are deserving of every praise and public encouragement.*

17. Annalen der Physik und Chemie herausgegeben zu Berlin Von J. C. Poggendorff. Received up to No. 7, 1843.

18. Bibliothèque Universelle de Genève, up to No. 93. September 1843.

19. Report on the Geology of Connecticut, in North America. By James G. Perceval. 8vo, pp. 495. *This work, which we consider of great importance in a geognostical point of view, has just reached us from the author.*

20. Journal of the Asiatic Society of Bengal. Edited by the Secretary. The number last received was No. 131 for the year 1842.

21. The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland. No. 2. New Series for October 1843. Edinburgh. Blackwoods.

22. General results of Microscopic Inquiries into the minute structure of the Skeletons of Mollusca, Crustacea, aud Echinodermata. By William B. Carpenter, M.D. With two Plates. 1843. From the Author.

23. Essay on the Physiognomy of Serpents. By H. Schlegel, Doctor in Philosophy, &c. Translated by T. S. Traill, M.D., F.R.S.E., Regius Professor of Medical Jurisprudence in the University of Edinburgh, &c. 12mo. pp. 254. With Plates, and a Map shewing the Geographical Distribution of Poisonous Serpents. Edinburgh, Maclachlan and Stewart,

1843. From the Author. *M. Schlegel has been long known as one of the most active and learned ophiologists in Europe, and his work, so far as it goes, is held to be classical. The creatures of the Serpent tribes, often so formidable to man, and even to the lower animals, and remarkable for the elegance of their scaly coverings, the beauty of their colours, and their many curious and important natural relations, cannot but interest the admirer of nature. Hence Ophiology has, from an early period, engaged the attention of many distinguished naturalists, and at the present day every scientific traveller and accomplished physician regard with interest the varied tribes of this great family of the animal kingdom, particularly in those countries where attention is arrested by the frightful energy of their vital powers, their beauty, and colossal magnitude. We may add, that no better guide can be put into the hands of the student of Ophiology or the traveller than this translation of Schlegel's work by Dr Traill.*

24. Illustrations of the Zoology of South Africa. By Andrew Smith, M.D. *The 17th and 18th numbers of this important work have been received.*

List of Patents granted for Scotland, from 20th September to 20th December 1843.

1. To JOHN MACINNES of Liverpool, in the county of Lancaster, manufacturing chemist, "certain improvements in funnels for conducting liquids into vessels."—2d October 1843.

2. To GOLDSWORTHY GURNEY of Great George Street, in the county of Middlesex, gentleman, "certain improvements in apparatus for producing, regulating, and dispersing light and heat."—4th October 1843.

3. To ALFRED VINCENT NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draftsman, being a communication from abroad, "certain improvements in the manufacture of cyanogen and its compounds, particularly the prussiates of potash and soda."—13th October 1843.

4. To JAMES COMBE of Leeds, in the county of York, engineer, "improvements in heckling, cleaning, preparing, and carding flax and other fibrous substances."—16th October 1843.

5. To JOHN AINSLIE, farmer, Redheugh, near Dalkeith, North Britain, "a new or improved mode of drying tiles, bricks, retorts and such like work made from clay and other plastic substances."—23d October 1843.

6. To THOMAS YOUNG of Queen Street, in the city of London, merchant, "improvements in obtaining power."—25th October 1843.

7. To MOSES POOLE of Lincoln's Inn, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in the manufacture of ornamental lace or nett."—25th October 1843.

8. To Alexander Angus Croll, superintendent of the Gas-Works, Brick Lane, in the county of Middlesex, and William Richards of the same works, mechanical inspector, "improvements in the manufacture of gas for the purpose of illumination, and in apparatus used when transmitting and measuring gas or other fluids."—27th October 1843.

9. To James Napier of Hoxton, in the county of Middlesex, dyer, "improvements in preparing or treating fabrics made of fibrous material for covering roofs, and the bottoms of ships and vessels, and other surfaces, and for other purposes."—30th October 1843.

10. To Arthur Dunn of Rotherhithe, in the county of Surrey, soap-boiler, "improvements in treating, purifying, and bleaching oils and fatty matters, and in making soap."—1st November 1843.

11. To Robert Raynsford Jackson of Blackburn, in the county of Lancaster, cotton-spinner, "certain improvements in the machinery or apparatus to be used in the preparation of cotton and other fibrous substances for spinning."—7th November 1843.

12. To James Johnston of Willow Park, Greenock, Esquire, "improvements in the construction of steam-boilers."—14th November 1843

13. To William Brockedon of Devonshire Street, Queen Square, in the county of Middlesex, gentleman, "improvements in the manufacture of wadding for fire-arms."—14th November 1843.

14. To Sarah Beadon of Hope Corner, Taunton, in the county of Somerset, "improvements in apparatus for regulating the inclination of vessels, for the purpose of drawing off liquids contained therein, in the construction of casks and such like vessels, and in the means of drawing off liquids, parts of which improvements are applicable for regulating the inclination of looking-glasses and other articles."—15th November 1843.

15. To Matthew Leach of Manchester, in the county of Lancaster, mechanic, "certain improvements in rotary steam-engines, which improvements are applicable to pumps for lifting and forcing water."—23d November 1843.

16. To Charles Tetley of Bradford, in the county of York, stuff-merchant, "improvements in the construction of boilers, part of which improvements is applicable for regulating the supply of water and other liquids."—23d November 1843.

17. To William Prosser junior, of Shaftsbury Terrace, Pimlico, gentleman, "improvements in the construction of roads, and in carriages to run thereon."—23d November 1843.

18. To Charles Brown of Woolwich, in the county of Kent, surgeon-dentist, "improvements in the manufacture of dip candles."—23d November 1843.

19. To John Kibble of Glasgow, gentleman, "improvements in apparatus for propelling vessels."—24th November 1843.

20. To Charles Brook of Meltham Mills, in the county of York,

cotton-spinner, "certain improvements in machinery for spinning and twisting cotton and other fibrous substances."—24th November 1843.

21. To JOHN WITHERS of Smethwick, in the county of Stafford, manufacturing manager, "an improvement or improvements in the manufacture of glass."—24th November 1843.

22. To HENRY AUSTIN of No. 87 Hatton Garden, in the county of Middlesex, civil-engineer, "improvements in wood-pavements, floorings, and veneers."—27th November 1843.

23. To WILLIAM BUSH of Union Street, Deptford, in the county of Kent, engineer, "improvements in rendering magnetic needles less prejudicially influenced by local attraction."—28th November 1843.

24. To HENRY RICHARDSON FANSHAWE the younger, late of Fourville le Pont, in the kingdom of France, but now of Wilds-rents, Bermondsey, in the county of Surrey, chemist, "improvements in curing hides and skins, and in tanning, washing, and cleaning hides, skins, and other matters."—28th November 1843.

25. To ARTHUR WALL of Bisterne Place, Poplar, in the county of Middlesex, surgeon, "certain improvements in the manufacture of iron." —28th November 1843.

26. To JOHN GEORGE BODMER of Manchester, in the county of Lancaster, engineer, "certain improvements in grates, furnaces, and boilers, and also in the manufacturing of iron or other metals."—29th November 1843.

27. To THOMAS DRAYTON of Brighton, in the county of Sussex, gentleman, "improvements in coating glass with silver for looking-glasses and other uses."—4th December 1843.

28. To FRANCIS HIGGINSON of the town of Rochester, in the county of Kent, lieutenant in Her Majesty's Royal Navy, "certain improvements in fastenings for parts of ships and other vessels, which improvements are also applicable to other building purposes."—14th December 1843.

29. Jo JOHN HICK of Bolton-le-Moors, in the county of Lancaster, engineer, "certain improvements in steam-engines, and in apparatus to be connected therewith for driving machinery, part of which improvements are applicable to forcing, lifting, and measuring water."—15th December 1843.

30. To LAWRENCE HARDMAN of Liverpool, in the county of Lancaster, merchant, "certain improvements in machinery, or apparatus to be employed in the manufacture of sugar."—18th December 1843.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Fifth Letter by Professor Forbes on Glaciers. Addressed to the Right Honourable Earl Cathcart. Communicated by the Author.

ROME, 29*th January* 1844.

MY LORD,—In reply to your kind letter of the 14th December last, requesting me to communicate to the Royal Society any observations upon glaciers which I was enabled to make during last summer, I may mention, that the state of my health was so indifferent during the finer months of the year, and the caution which it required so great, that I was quite unable to prosecute as I had hoped the subject of my previous inquiries in Switzerland. As, however, the journey was not quite unproductive, I will very shortly state the additional facts which I was enabled to observe, claiming from you and from the Society the indulgence which their scantiness requires.

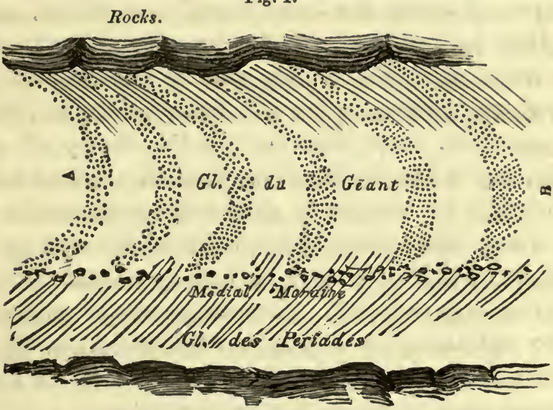
At Chamouni, the most obvious consideration was to determine the actual annual motion of the ice, the partial motions of which during the summer months had been carefully ascertained by me, as stated in my former communications. For this purpose, I had two marks of a permanently distinguishable kind, namely, blocks of stone lying on the surface of the ice; the one, formerly marked D. 7, and referred to in my *Travels* by that name, situated a little lower than the position of the Montanvert; the other, marked C, or "Pierre platte," on the Glacier de Léchaud, near its junction with the Glacier du Géant. It was the former of these masses which had been *approximately* observed in position by my guide, Auguste Balmat, during the winter of 1842–3, with great labour and fidelity—observations which first conclusively proved the fact which I had previously suspected, although opposed to the received opinions,—that the glacier moves with considerable velocity even in winter. By going to the spot with Balmat, and verifying the marks which he had from time to time made, I ascertained that his measurements, if not absolutely correct, did not admit of being materially improved, owing to the great size and repeated turning over of the block in question. His measurements between October 1842 and June 1843, have been published in the volume already cited. I had the mortification, however, to find, on the 11th September 1843, when I visited the block, that though still upon the ice, it had got shoved so near the moraine of the glacier near an angle of its course, as to be well nigh stranded; and that, in fact, since Balmat's last mark in June, its motion had been scarcely perceptible. It farther appeared, that the part of the glacier with which it had recently been moving, was so crevassed and steep, that the vast block must have rolled and tossed about, or even been precipitated occasionally forwards by the failure of the ice beneath it on the steep, in a way which amply accounts for any want of regularity in its winter progress, as indicated by Balmat's measurements. It therefore became the more interesting and important to determine with care, the motion of a point of the glacier removed from the accidental local influence of the sides and irregularities of the surface, in order to compare the *mean annual motion* with the *summer* motion of the ice. The "Pierre platte" was, in every way, an unexceptionable landmark; and I resolved to cross the Mer de Glace for the purpose of accomplishing it—an exertion which I should hardly have ventured for a less interesting result. In the course of this walk, which was fraught with interest to me, as enabling me to compare the existing condition of a glacier with the appearances which had been so familiar to me just twelve months before, I found the state of the ice just such as might be expected after a very severe and snowy winter, and a very cold and late summer. The glacier opposite the "angle" (station A), had now *a much higher level than it had at the same time in* 1842; evidently, therefore, it had, during the winter, regained its usual volume; and then, during the ensuing summer, it had wasted less than it had done during the summer before. The glacier also bore other testimony to the same circumstances; for the crevasses were far sharper and better defined, and the whole appearance of the ice less collapsed, than at the same season in 1842. The surface also at the "angle" was extensively covered with the unmelted snow of the winter, which, as I have often observed, never admits for a moment of being confounded with the matter of the glacier. The general direction and appearance of the crevasses, and of the position of the "Moulins," was the same as in 1842; as if the glacier had remained at rest, though it had really moved some hundred feet forwards. The moraines were unaltered in appearance, only perhaps less prominent (at least, this was the remark of the guides), which would naturally arise from the less superficial waste of the ice.

There was no difficulty in recognising the "Pierre platte," which, indeed, had recently slid off an ice pedestal similar to that of the preceding year (as figured in the frontispiece to my volume of Travels), but far less stupendous. As all the marks in the rock which I had at different places cut with a pick, and painted red, were as visible and fresh as on the day they were fixed, there was no difficulty in recovering, to a nicety, the exact position of the block on any day on which it was observed in 1842, and comparing it with its new position. Accordingly, referring to the starting point on the 27th June 1842, I found that it had moved, down to the 12th September 1843, or in 442 days, 320 feet, that is 8.7 inches daily. I have not now my own work to refer to; but I believe it will be there found, that the motion of the "Pierre platte" during the hottest summer months, was only between 9 and 10 inches at a mean. It is plain, therefore, that during the remainder of the year (throughout the greater part, or nearly the whole of which this part of the glacier is covered with snow), the motion, though somewhat diminished, was very far indeed from ceasing,—thus entirely confirming the observations of Balmat, near the lower end of the glacier.

Finding that my strength and the time permitted, I pursued my excursion up to the level of the Jardin, opposite the glacier of Talefre, near the Aiguille du Moine. I had a lively satisfaction in comparing my engraved map with the natural features of the country, and finding it a tolerably faithful representation; and I checked every where, with minute care, the definition I had given of the direction of the ribboned structure of the ice at different parts of the glacier. The observations formerly made I found to be rigorously exact; and especially these two facts, which at once put an end to any idea of the ribboned structure being a prolongation or *deformation* of the strata of the Névé; viz. (1.) the structure assumed by the ice of the Taléfre is *extirpated entirely* by its precipitous descent to the level of the Glacier de Léchaud, where it reappears, or rather is *reconstructed* out of the bruised fragments, according to a wholly different scheme; (2.) the veined structure often cuts the medial moraines; *i. e.*, a glacier composed of two, having originally distinct looped structures, assumes finally, after being for some time united, a single looped structure.

From the heights above the Egralets, which command a most extensive bird's-eye view of nearly the whole Mer de Glace, about 2000 feet below, I was led to make a very interesting observation,—on the whole, the newest of the season. I need not remind your Lordship, that I first observed, in 1842, the existence of certain wave-like marks on the surface of the Mer de Glace, figured in my map of that year, and represented in the models submitted to the Royal Society last winter. These waves, or "dirt bands," as I termed them, were parallel in their course to the veined or ribboned structure of the ice, and recurred at pretty regular intervals upon the surface of the glacier,—the loops pointing in the direction of its motion,—at an average distance, as I think, of between 600 and 700 feet. [The exact value is stated in my book.] I was prevented, by a premature fall of snow, from tracing these bands (which I also termed "annual rings"), higher up the glacier than the point called Trelaporte. Standing, on the 12th September last, above the precipices of the Couvercle, at the foot of the Aiguille du Moine, as above mentioned, I not only saw with admirable distinctness the "dirt bands" between Montanvert and Trelaporte delineated, as it were, upon a plan; but I was enabled to count six new ones higher up in the direction of the Glacier du Géant. Then followed a space corresponding to three intervals of dirt bands, which were, however, not perceptible. Higher up, on the Glacier du Géant, was a most striking and beautiful appearance, quite new to me. The heavy snow of the previous winter had not been entirely melted during the whole summer, and still lay in all the hollows where it could accumulate. A series of snowy bands having this origin appeared at regular intervals upon the upper part of the Glacier du Géant, corresponding in distance and form to the arrangement of the "dirt bands" in the lower part of the glacier, as I have endeavoured to represent below:—thus as-

Fig. 1.



certaining a most curious and unsuspected fact, namely, the existence of a series of curvilinear hollows on the nearly plane surface of the ice, which the eye would probably have in vain striven to detect, but for the palpable evidence of the accumulations of snow lodged in the intervals of these vast waves. In Fig. 1. the ground-plan of a part of the Glacier du Géant is shown, where it is divided longitudinally by the Medial Moraine descending from the Aiguille Noire. The left hand portion of the icy stream descending from the ridge, called "les Periades," bears that name. The masses of dots indicate the position of the snow wreaths which mark the indentations of the ice; these appeared to be confined to the ice of the proper Glacier du Géant; the *lines* indicate the direction of the most distinct veined structure in the ice, which are visible in a mass from a distance, as the finely veined structure of Cipollino marble is, even when the laminæ composing it cannot be individually seen. The conclusion from this is, that the surface of a glacier is not an inclined plane, nor an inclined convex surface, but that it is indented by furrows or wrinkles, which would give a section like figure 2., in a

Fig. 2.

direction parallel to the length of the glacier A B, fig. 1, the dots here indicating the snow wreaths as before, and the broken lines approximating to the direction of the frontal dip of the veined structure in this part of the glacier. These periodical undulations (to use a technical scientific term) are, beyond question, very important in the theory of glaciers generally, and of the dirt bands in particular. Their existence will, probably, be thought to yield some confirmation of the theory of semifluid motion of the ice, even although their precise origin be still obscure. A homely comparison, but a striking one, may be found in the *wrinkles* of the horns of many animals; and, though it may appear fanciful, there is perhaps more than a vague analogy between the facts. I hope, at some future time, to offer a better elucidation.

After some stay at Chamouni, I proceeded, by easy journeys, to Grindelwald, whose beautiful and easily accessible glaciers I had not visited for many years. I there found an exact confirmation of the views which I have published respecting the origin and structure of glaciers. I found the forms, simple and compound, of the ribboned structure, to be such as I have described, including the gradual rise of what I have called the "frontal dip," from a very low angle near the lower end of the glacier, up to 75° towards its origin, especially on the Glacier of the Strahleck, above the Zäsenberg, where it forms the Mer de Glace of Grindelwald. The other principal affluent of that icy sea, viz. the glacier descending from the Viescher hörner, exhibits the same *wrinkles* exactly as those which I have just described upon the Glacier du Géant, perhaps even better marked. As the lower glacier of Grindelwald furnished an excellent example of all the modifications which I have elsewhere shown to belong to the *canal-shaped glacier*, with branches; so the upper glacier is an exact representative, in its lower part, of the *oval glacier*, for which I have taken that of the Rhone as a type; whilst many of the tributary glaciers of Grindelwald and the Jungfrau bear ample testimony to the general fact, that the structure of glaciers is developed during their progression, and after their primitive stratification has been annihilated, by their being projected in avalanches over appalling precipices.

To these brief notes, I have only to add one interesting discovery, though of a somewhat local importance, which I made at Chamouni. The ancient lateral moraine of the Glacier des Bois is acknowledged by De Saussure, and all subsequent writers, to be found in the barrier of debris which crosses the valley of Chamouni, at Les Tines; but very feeble traces have (I believe) been observed of the corresponding lateral moraine of the left bank of the glacier, excepting those between the Châlet of Montanvert, and the descent of La Filia. I have ascertained, however, that a good part of the ascent to the Montanvert, and especially near the châlets of Planaz, passes over a vast accumulation of debris, whose nature corresponds to that of the granites of the central chain, and which lies to an immense thickness against the rocky slopes of the valley, at the foot of the Aiguille de Blaitiere. The resistance offered by this mass of debris to the progress of the torrents, which descend from the glaciers of Grepau and Blaitiere towards the Arve, has diverted their course in a direction parallel to that of the valley of Chamouni, and it was the observation of this singularity which led me to the detection of the moraine first-mentioned, which I could hardly believe had escaped me so long.

On the Comparative Value of different kinds of Parrot Coal, or Candle Coal, for yielding Gas, and on the Illuminating Powers of Gas at Different Distances from the Manufactory. By ANDREW FYFE, M.D., F.R.S.E., F.R.S.S.A. (Communicated by the Royal Scottish Society of Arts.)*

Having for some time past been extensively engaged in conducting experiments with the view of ascertaining the illuminating power of gas from different kinds of parrot coal, and,

* Read before the Society, 8th January 1844.

consequently, the value of these coals for yielding gas; and, also, in trying whether or not gas is affected by its transit in the street pipes, I have thought it proper to lay the results of these trials before the society. The subject is, in many points of view, of importance; and I trust, that what I have now to communicate will not be devoid of interest to a Society, the chief object of which is the promotion of our arts and manufactures.

I. *On the Comparative Illuminating Power of Gas from different Coals, and the consequent value of these Coals for the Manufacture of Gas.*

The coals on which the trials were made, were those from Lesmahago, of which there were two kinds, called the Duke's and Ferguson's, the Monkland, the Knightswood, and Skaterig coals, from the west of Scotland; the Wemyss and Torryburn coals from Fifeshire, and the Arniston, the Dryden, and Marquis of Lothian's coal, from the neighbourhood of Edinburgh. Of the last, there were two kinds, marked A and B, the former being that generally used by the Gas Companies here.

The above coals were first subjected to analysis, with the view of ascertaining the per centage of gas, of coke, and of ashes, and also the specific gravity. The following was the result:

Analysis of the Coals used in the Manufacture of the Gas.

COAL from	Lesmahago	Monkland	Skaterigg	Knightswood	Marquis of Lothian A.	Marquis of Lothian B.	Arniston	Torryburn	Wemyss	Dryden
Sp. Grav., . .	1233.9	1269.5	1236.4	1116.7	1427.2	1173.4		1304.7	1409.8	1244.3
Gas, ...	56.3	53.6	46.5	4.67	62.	54.3	57.5	56.3	48.8	52.3
Coke, ...	43.7	44.4	53.5	53.3	38.	45.7	42.5	43.7	51.2	47.7
	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Ashes per cent.	3.5	6.3	5.	4.	4.	4.5	1.	3.	1.9	3.5

In obtaining gas from the coals, the composition of which has been given, I employed an apparatus erected in my laboratory for the purpose, and constructed in the usual way. The retort was capable of being charged with 5 lb. of coal. The condenser consisted merely of a system of pipes, of nearly 100 feet in length, and two inches internal diameter. The purifier contained three shelves with dry slaked lime, by which the gas was thoroughly purified before going into the gasometer. In the different trials, the quantity of coal used was the same in all, viz. 5 lb.; and the temperature, to which the retort was brought, was, as nearly as could be judged of by its appearance, also the same in all. The time for taking off the gas, of course varied in the different cases; in general, it was from 50 to 60 minutes.

Different methods were had recourse to for ascertaining the illuminating power: 1*st*, The shadow. For this purpose, a wax candle, burning as nearly as possible with the same flame, was contrasted, at different distances, with a single jet of 5 inches, using the same jet in all the trials. 2*d*, The condensation by chlorine, following the method described in my former paper. These are the tests in which I place the most confidence. 3*d*, The time required for the consumpt of a given quantity. In the trials, a single jet of 5-inch flame, always under the same pressure, was used. 4*th*, The specific gravity, which was taken by Crosley's apparatus. 5*th*, The length of flame from a single jet, under a pressure of one inch. This last is what is called Aldcock's burner.

In conducting these trials, due attention was paid to every circumstance, so as to secure accuracy in the results, not only in the manufacture of the gases, but also in ascertaining the illuminating power by the methods above mentioned. For the sake of brevity, the results are given in a tabular view.

TABLE shewing the proportion of Gas and of Coke, and also the Illuminating Power of Gas got from the different kinds of Coal, compared with that of a Wax Candle taken as unity.

Coals.	*Shadow.* Jet 5 inch flame.	*Chlorine.*	*Consumpt.* 1 foot in Minutes.	*Specific Gravity.* Air 1000.	*Length of Flame, Aldcock Burner.* Pressure 1 inch.	Cubic feet of Gas from 100 lb. Coal.	Coke from 100 lb. Coal.
Arniston,...	3.57 3.69	16.5 19.5	67 70.	636 638	3.5 3.5	47. 48.	42.5 42.5
Average, ...	3.63	17.5	68.5	637	3.5	47.5	42.5
Wemyss, ...	3.36 3.67	22. 17.	80. 70.	666 657	4. 3.	45. 45.	51.25 51.25
Average, ...	3.52	19.5	75.	662	3.5	45.	51.25
Lesmahago... (Duke's)	2.98 3.62	15.75 16.50	62.5 65.	657 644	3.5 3.25	47. 50.	43.75 43.75
Average, ...	3.35	16.12	63.7	650	3.37	58.5	43.75
Monkland,...	2.64 3.9	13. 19.	60. 74.	621 713	3. 3.5	47. 44.	46.25 43.75
Average, ...	3.27	16.	67.	667	3.25	45.5	45.
Lesmahago... (Ferguson's)	3.26 3.12 2.54	16. 15. 15.	62. 61. 60.	614 641 655	3. 3. 3.	45. 45. 44.	48.75 47.5
Average, ...	2.88	15.33	61.	637	3.	44.66	48.125
Torryburn,...	2.95 2.45	13. 13.	59. 56.	633 615	3.5 3.125	52.0 48.0	42.5 42.5
Average, ...	2.7	13.	57.5	624	3.33	50.0	42.5
Marquis Lothian, A.	2.75 2.6	13. 12.5	59. 56.	633 615	3.5 3.125	46.0 46.0	47.5 47.5
Average, ...	2.67	12.75	57.5	624	3.33	46.0	47.5
Marquis Lothian, B.	2.48 2.43	14. 13.	60. 63.	556 556	3.25 3.	44.0 44.0	44.4 55.
Average, ...	2.45	13.5	61.5	556	3.125	44.0	49.7
Knightswood,	2.0 1.75	8. 10.	45. 52.	560 554	25. 25.	41. 40.	52.5 53.
Average, ...	1.87	9.	48.	557	25.	40.5	52.7
Skaterigg, ...	1.98 1.80	9. 9.	40. 51.	478 521	2.75 2.5	42. 45.	57.5 52.5
Average, ...	1.89	9.	45.5	500	2.66	46.5	55.

Tabular View of the Illuminating Power of the Gases, as proved by the Shadow, by the Chlorine Test, and by the Time for Consuming a Cubic Foot by the 5-inch jet. The gas from Skaterigg being taken as unity.

Coals.	Shadow.	Chlorine.	Consumpt.
Skaterigg, ...	1	1	1
Knightswood, ...	1	1	1.05
Marquis of Lothian, B.	1.3	1.5	1.35
Marquis of Lothian, A.	1.42	1.41	1.26
Torryburn, ...	1.44	1.44	1.26
Lesmahago, F.	1.54	1.70	1.36
Monkland, ...	1.74	1.77	1.46
Lesmahago, D. . . .	1.79	1.79	1.40
Wemyss, ...	1.88	2.16	1.64
Arniston, ...	1.9	1.94	1.5

The foregoing observations apply solely to the comparative value *of the gas* for the purposes of illumination, as found by the methods described; but it will be seen by inspecting the table, that not only is the *time* required for consuming equal volumes of the gases different, but that the quantity of gas given off, though generally nearly the same, yet in some cases is higher than in others; of course, these must be taken into account in fixing the comparative value of the gases, and consequently in enabling us to ascertain the comparative value of the coals for the manufacture of gas.

The following table shews the comparative illuminating power for equal consumpts of gas, as proved by the shadow-test. Skaterigg being taken as unity.

Coals.	
Skaterigg, . . .	1.00
Knightswood, . . .	1.00
Marquis of Lothian, B, . .	1.76
Torryburn, . . .	1.80
Marquis of Lothian, A, . .	1.90
Lesmahago, F., . .	2.00
Lesmahago, D., . .	2.48
Monkland, . . .	2.54
Arniston, . . .	2.90
Wemyss, . . .	3.08

In addition to the above, if we take into account the quantity of gas given off by the coals, then the comparative value of these coals for the manufacture of gas will be as follows:—

Knightswood, . . .	1.00
Skaterigg, . . .	1.12
Marquis of Lothian, B, .	1.81
Marquis of Lothian, A, .	2.15
Lesmahago, F., . .	2.20
Torryburn, . . .	2.22
Monkland, . . .	2.85
Wemyss, . . .	3.41
Arniston, . . .	3.43
Lesmahago, D., . .	3.58

In the above calculations, the value of the coke, and of the ammoniacal liquor and tar, is not taken into consideration.

II. *On the Effect of the Transit of Gas in the Pipes, from the Manufactory to the place where it is to be consumed.*

It is well known, that, with the view of purifying coal-gas, it is made to travel through the condenser, where it deposits its tar, volatile oil, and ammonia. In most cases, the condenser is merely a system of tubes or boxes, the length and cubic area of which vary according to the extent of the manufactory. It may naturally be supposed that after leaving the gasometer, the gas, during its passage in the street pipes, will be still further affected; and hence has arisen the idea, that the gas at a distance from the manufactory, is of inferior illuminating power to that consumed near it. A different opinion has, however, been maintained by others; indeed, many suppose, that the lighter particles are conveyed to the greater distance, and as these are conceived to be the purest and most highly illuminating, hence, it is imagined, that the gas delivered at a distance from the works, is of superior illuminating power to that consumed in their immediate neighbourhood.

Several opportunities have been afforded me for making trials on this important point; I say important, because it is a question, the settlement of which may materially influence Gas Companies in the choice of a situation for their works, besides influencing consumers, with regard to the company from which they will take their gas.

The gases on which the trials were made, and the results of which I am now to give, were those of Edinburgh and Leith, and also that of a town in the north of Scotland, to which I went solely for the purpose of experimenting on this subject, as I knew that a good opportunity there presented itself, owing to the great distance to which the gas is sent. The illuminating power was ascertained chiefly by the chlorine test, because I found it difficult, nay in some cases impossible, to carry an experimental metre with me, and hence one great advantage of this test.

Trials on the Leith Gas.—9th Dec. 1842. The gas was made from Arniston coal, which was found to yield from 510 to 539 cubic feet per cwt., that is, taking the average, 464 from the 100 lb. In my trials with my own apparatus the quantity amounted to 475 feet. By different trials with the chlorine test, the condensation at the works, on an average, amounted to 13 per cent. On the evening of the same day, the gas from the same company was tried at Niddry Street, Edinburgh, a distance of upwards of two miles from the manufactory, and the condensation there was, on an average, 12. In this instance, the flow of gas in the pipes, to supply the immediate neighbourhood, was not great, and I was, therefore, anxious to have other trials made. I accordingly selected George Street, because I knew that, from the works at Leith to this situation, the pipes are laid in a direct line; and, as the flow of gas was considerable, I was convinced that that tried at the works, and in the other place on the same day, would be the same.—16th Dec. At the works the condensation by chlorine was, on an average, 13.7. The gas was from Arniston coal, which had been in use for two days; on the evening of the same day the condensation at George Street amounted to 14.

Trials in the North of Scotland.—26th Dec. 1842. The gas was made from a mixture of two parts of Lesmahago, one of Monkland, and one of Torryburn coal. The average of all the trials at the manufactory, indicated by the chlorine, was 14.75. By the Aldcock burner, the length of flame, by one inch of pressure, was 4.1 inches. The specific gravity was 590. On the same day, the gas was tried at the distance of nearly six miles. By the chlorine the indication was 13.25. The specific gravity was 653, and the flame by the Aldcock burner, at one inch pressure, was four inches. In this trial the difference is very trifling; and as the gas on which I operated at the works was that made from Saturday evening to Monday morning, on which day the experiments were conducted, there is every reason for believing, that that tried at the works and at the distant point was the same.

Trials on the Edinburgh Gas.—15th Nov. 1842. This gas, tried at my laboratory in Surgeon Square, a distance of nearly a mile from the manufactory, indicated, by the chlorine test, 13.7. On the 9th Dec., it was 13; on the 19th, it was 13.66, giving an average of 13.45; on the 20th Dec., the gas was tried at the works, and on an average was found to be 13.16; on the evening of the same day, the gas at George Square, a distance of upwards of a mile, was 14; on Friday, the 23d Dec., at the works, it was 14; at George Square, on the evening, it was 14. The average of all the trials at the works was 13.5; at a distance, it was 14. I have again, more lately, tried this gas at a greater distance. On the 20th Dec. 1843, the gas at the works indicated 13; at the distance of upwards of three miles on the Glasgow road, the indication was 12.5.

In the above experiments, the illuminating power of the gas is, in some cases, the same at the works and at a distance; in three, it is rather lower, and in one it is rather higher, at the distant point. Taking the average of all the trials, the results are—

	At the Works.	At a Distance.
Leith, 6th December, . .	13	12
16th	13.7	14
In the North, 26th	14.75	14.25
Edinburgh, ... 1842,	13.5	14
... 1843,	13	12.5
Average,	13.59	13.35

The difference, as shewn by the above table, is so very trifling, as to be altogether unworthy of notice; consequently proving that the distance to which the gas is conveyed, after it is properly purified, makes little or no difference in the illuminating power. Of course, in making this remark, it must be understood that it does not at all bear on the loss that may be sustained, by the diminution in the quantity of gas, by leakage in the pipes, and otherwise ; a point which it is difficult to decide, where a company disposes of its gas, partly by meter, and partly by contract according to the time, but which does not affect the consumer, who has to pay for the gas that passes through his meter, or by the time his burners are in use.

On the Production of Flames in Volcanoes, and the consequences that may be drawn therefrom. By M. LEOPOLD PILLA.

THE question, whether volcanic phenomena are accompanied with flames, is, in my opinion, of so much importance in the science of the earth, that the attention of natural philosophers cannot be too strongly drawn to it; doubts are still left in the mind respecting it, which ought to be removed. The greater number of men of science who have devoted themselves to the study of volcanoes, deny that there is any manifestation of this phenomenon in volcanic eruptions; and they in general think, that what the vulgar, and even many writers, have called *flames*, is nothing else than the reflection of the light produced by the burning substances on the walls of the craters, and on the column of smoke which issues from them. I may be permitted to quote, in reference to this, the following passages from most respectable authors.

"An unanswerable proof of the insufficiency of this hypothesis (the disengagement of hydrogen gas in the eruptions of Stromboli) is the following. When the bubbles of the boiling lava burst by the escape of the enclosed gas, who does not see that if this gas consisted of hydrogen, it ought, at that moment, to become inflamed on the surface of the lava? Now, it is very certain that in no eruption do we ever see the slightest flame on the lava."*

"The different metallic combustibles and metalloids may decompose water, in proportion to the degree of affinity they possess with the oxygen of the latter, and give rise to the series of acids and oxides which appear in volcanoes. We ought, however, to observe, that the hydrogen, on leaving its state of combination, never reaches the apertures which vomit fire, and which are in communication with the atmo-

* Spallanzani, Viaggi alle Due-Sicilie, t. iii. cap. 21.

spheric air, because *we have never seen flames, either on the crater while in action, nor on the surface of the running lavas.*"*

"One of the consequences of Davy's hypothesis, and perhaps the most important, would be the disengagement by the craters of volcanoes of an enormous quantity of hydrogen, either free, or combined with other principles, if it really be the water which, by its oxygen, induces volcanic fires. It does not appear, however, that the disengagement of hydrogen is very frequent in volcanoes. Although, during my abode at Naples, in 1805, with my friends Messrs Alexander de Humboldt and Leopold de Buch, I had an opportunity of witnessing at Vesuvius frequent explosions, which threw out broken lava to a height of more than 200 yards, I never perceived any inflammation of hydrogen."†

"The brilliant light reflected by the clouds of aqueous vapour and ashes suspended over the cone, produces this appearance, which is so often described under the erroneous denomination of *flames*, in the accounts of volcanic erruptions by inexperienced observers, who have no acquaintance with science."‡

Sir Henry de la Beche, when describing the phenomena of an eruption of Vesuvius, expresses himself thus: "The solid substances thrown up by the volcano, appeared like a numerous discharge of red balls, while the light of the burning mass in the interior of the crater, reflected sometimes in a very vivid manner by the column of vapours above, produced, to the view of an observer, placed at a certain distance, the appearance of flames, *which there are strong reasons for believing to be illusory. It is at least very certain, that almost all the cases of this nature which have been cited, have no other cause than a reflection of light, which varies in intensity with the activity of the volcano.*"§

"The vapours illuminated by the incandescent substances which fill the craters, or cover their walls, have often been taken for flames. But this illusion has been combated by a great number of observers, who have affirmed that *true flames never issue from the crater of a volcano.*"||

I myself, also, drawn away by so many authorities, when I commenced to observe the phenomena of Vesuvius, said, "We ought to take care in such cases not to mistake the luminous radiation produced by the stones and incandescent scoriæ for flames, an error into which many people have fallen;"¶ and, in fact, when I wrote this sentence, I had never observed flames from Vesuvius.

* Covelli, Storia dei fenomeni del Vesuvio, § xc.

† Gay-Lussac, Reflexions sur les Volcans (Ann. de Chim. et de Phys. t. xxii.)

‡ Poulet Scrope, Considerations on Volcanoes, cap. 2, § 1.

§ Manuel de Geologie (art. Volcans en activité).

|| Brongniart, Des Volcans et des terrains volcaniques (Art. du Dict. d'Hist. Nat.).

¶ Spet. del Vesuv. fasc. 1, § xxvii.

I forbear to quote in this place passages from more ancient authors. It is true that many of them, in describing volcanic phenomena, sometimes mention *flames;* but it is evident that they paid no particular attention to this phenomenon, and did not distinguish it from the luminous reflection produced by the burning substances.

We perceive, therefore, that geologists, up to the present time, have been of opinion that volcanic eruptions have never been accompanied with flames. This opinion, however, is altogether erroneous. At least, I think that I am entitled to make this affirmation positively in reference to Vesuvius.

Let us commence by stating the facts which may support this general proposition. Of all the phenomena which I have had occasion to observe in regard to Vesuvius for twelve years, I consider those which I am about to notice, and an acquaintance with which I owe to a fortunate accident, as the most important.

On the night of the 2d June 1833, I was within the crater of Vesuvius, in order to observe the phenomena of an eruption, which was approaching its close. In the centre of the crater there was one of those cones of scoriæ which are formed and disappear with such marvellous rapidity; it was the largest cone I had ever observed, so much so, that it might have been called the little *Monte Nuovo.* There was a large funnel-shaped aperture on its summit, through which the explosions took place. At the moment of which I speak, these had become less frequent, and succeeded each other at intervals of from three to four minutes. This circumstance made me desirous to mount upon the cone, in order to observe the great phenomenon of explosions, which I never before had it in my power to do near at hand, and immediately over the opening. I had frequently, indeed, observed eruptions from the summit of *la Punta del Palo;* but the distance from the proper opening, or what may be called the air-hole of the volcano, the walls of scoriæ with which it is usually surrounded during eruptions, the smoke, the ejection of stones, and other circumstances, had always prevented me seeing distinctly what was going on in the volcanic orifice. At the moment of explosion I ascended to the edge of the cone, along with my courageous guide, who shared in my curiosity to observe the appearances. The interior of the opening was almost entirely free from smoke; a small quantity only issued from different points in the walls. This fortunate circumstance enabled me to see very distinctly all the parts of the crater, and everything that was going on there. The bottom of the funnel was open; it lay immediately under my eyes, at a depth of about eighty metres; its circumference was nearly twenty yards; the whole of its burning interior was visible. The view of the phenomena which accompanied the explosions was inexpressibly magnificent. They consisted of the following:

A loud subterranean noise, and a violent shock, announced the explosion; immediately after, and almost at the same time, the mouth opened and made a discharge, with a noise resembling that of a discharge of cannon. A column of black and fuliginous smoke issued with great violence, and there was thrown up, with the rapidity of lightning, an enormous torrent of inflamed gaseous substances and burning stones, which fell back again like hail, for the most part into the gulf, but partly without it. I was overpowered with the grandeur of the spectacle, but I did not fail to observe, in particular, the column of flames which accompanied the explosion. It was the first time that it had fallen to my lot to witness such a phenomenon. The flame rose to the height of 4 or 5 yards, and then disappeared among the volumes of smoke, so that a person whose eye was on a level with the edge of the gulf could not have seen it. I mention this, because when volcanic explosions are viewed from a distance, and from places where the crater in action is not visible, it never happens that the flames are visible; whence it is that the existence of this phenomenon in volcanic actions has been denied. The flame which I observed was of a very decided violet-red colour. It was very obvious that the gas which produced it became inflamed by contact with the air, because it burned only on the circumference of the column, and in the interior was obscure, presenting, on a large scale, what may be seen in a lamp on a small one. After the explosion and fall of stones had terminated, another very remarkable phenomenon was perceived. Insulated flames, disposed in a very picturesque manner, remained in the bottom of the gulf, moved around the mouth, and flickered very slowly about the walls of the funnel; an appearance which might be compared, in some measure, *si licet maxima comparare minimis*, to the flame of alcohol burning in a crucible. The beautiful violet colour of the flame was then easily distinguished; a faint smell of hydrogen gas accompanied these phenomena. I continued for a quarter of an hour gazing on this enchanting spectacle, and during that time I saw five explosions always accompanied by the same appearances; I would have remained longer in the same spot, had not the last of these explosions, which was much more violent than any of the preceding, compelled us hastily to retire.

I have had no opportunity, since the above noticed occasion, of observing the great opening of a volcano in a state of explosion; but I have noticed the existence of flames in circumstances nearly similar.

In the month of June, the following year, Vesuvius was in a state of eruption; on the evening of the 7th I paid a visit to the crater. The interior cone was throwing up stones with such violence that it was impossible to approach it. A current of lava was spouting out through a fissure at its base. Quite near to me, there was an elevation of a longitudinal form, which bore eight small cones, or rather eight large tubes of lava, open at the summit, and throwing out gas and steam with a whistling noise that was quite deafening, and which might be compared to that caused by opening the valves of a high-pressure steam-engine. Favoured by the darkness, we saw that their actions were accompanied with beautiful conical flames, which issued from the tubes with a violence which might be in some measure compared to a flame increased in intensity by a blow-pipe. The length of these flames was from 3 to 5 inches, and their diameter at the base about an inch and a half; they burnt with a beautiful greenish colour, like alcohol holding boracic acid in solution: such a colour would very likely be produced by the chloride of copper accompanying the gaseous substances. The smoke which escaped from the openings in the cones had an intolerable smell of muriatic acid; sulphureted hydrogen gas was not perceptible. This was the second time that I observed flames in the crater of Vesuvius, and I saw them in company with my esteemed friend M. Ravergie of Paris, who was my companion in this expedition.

I saw very beautiful flames from Vesuvius, for the third time, during the great eruption in August 1834. An opening was formed in the volcano at its eastern base, and a great current of lava was thrown out, which spread over the fertile lands of Ottajano. In the place where the lava issued, two elevations were formed, which supported twelve small cones, kinds of *hornitos*, all of which were in great activity, and produced noisy explosions. One of these cones, which appeared the most active, and which I could approach near, notwithstanding the smoke it spread on all sides, was emitting by its opening, besides quantities of stones, a bright flame of a reddish-white colour, which came forth with great violence, and rose to the height of 3 yards. The jet was continuous, like the flame from a high furnace heated by bellows. The smoke was charged with muriatic acid, and, in a few moments, it enveloped Professor Tosone of Milan and myself in such a manner that we were nearly suffocated.

I never had the good fortune to observe flames in Vesuvius in so distinct a manner as on these three occasions. I have never seen them on the surface of currents of lava far from their source. But my friend, M. Maravigna, assures me, that he observed them on a current from Etna, during the eruption of 1819.

According to all that I have said, my belief is, that volcanic explosions are constantly accompanied with flames. So convinced am I of this truth, in regard to Vesuvius, that I would engage to point them out during any eruption, provided the circumstances were at all favourable.

I again repeat, that if the existence of this phenomenon has been denied, it is owing to the great difficulty of observing explosions very near at hand; and when they are observed far from the aperture in action, as is usually the case, the flames are either concealed by the walls of scoriæ which surround them, or, if they rise, they disappear among the smoke and jets of stones.

The phenomenon of which I speak is not an accidental one in the great actions of volcanoes. It is only necessary to see it once to be convinced that it is intimately connected with the cause of these actions. It may be said that flames are the most remarkable circumstance in volcanic explosions, as the latter are the most essential phenomena of eruptions; we may perceive in them the most direct external manifestation of the origin of the internal commotion. It is for this reason that I consider my observations on the flames of Vesuvius as tending to assist in explaining the cause of volcanic phenomena.

Reviewing what has been stated, I think the following conclusions may be drawn from it:—

1. Flames never appear in Vesuvius but when the volcanic action is energetic, and is accompanied with a development of gaseous substances in a state of great tension; they do not appear when the actions are feeble.

2. Their appearance always accompanies explosions from the principal mouth; only they cannot be observed but in favourable circumstances.

3. They likewise shew themselves in the small cones in action, which are formed in the interior of the crater, or at the foot of the volcano.

4. Finally, they are not visible except in the openings which are directly in communication with the volcanic fire, and never on the moving lavas which are at a distance from their source.

After this exposition, it is natural to enquire, what is the gas which produces these flames in Vesuvius?*

Dependence of the Geographical Relations of Countries and Nations, on the extent and situation of Forests.

The rapid progress of all the material interests of nations during the general peace, which has liberated the German tribes, in particular, from the foreign yoke imposed on them from the West, was not without considerable influence upon the theoretical and practical turn of those sciences, which principally forward those interests, and are essential to their promotion. Among these sciences, Geography, which has for its object all the measurable, physical and political relations of the earth, holds no trifling, but rather a highly important place, because it offers various scientific, historical, and philosophical data, and considers them in their imposing details, in reference to the character of countries, nations, and empires. Its object is, harmoniously to connect the endless variety of the phenomena of our globe, to subordinate the accidental to what is regulated by laws, to establish an uni-

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* From Comptes Rendus, 1843, No. 17, p. 889.

versal connection between the individual facts, and to investigate the general laws, those tokens of a Divine order of things.

Its object does not consist in producing a long list of isolated facts without meaning, but it consists in representing to the senses the influence of Nature upon nations, and mankind in general; in showing the intimate connection between the general aspect of the earth and the particular physical character of the countries on the one hand, with the progress of civilization on the other, by means of giving an accurate description of the contrasts between highland and lowland countries, to be regarded as a basis and distinguishing characteristic; as also by discussing the method, according to which the knowledge of the physical character, and of the elevation or depression of countries, establishes a system of significant relations between all the different portions of continents, and gives precision and certainty to delineations and political relations. By considering the subject-matter of geography under this point of view, the method underwent a material change; the former was raised to the rank of a science, and an almost universal interest excited in its favour; for geography is now taught, not merely for the sake of its usefulness, but principally for the sake of moral and intellectual improvement. The want of a solid system of teaching geography is more and more generally felt, both as regards deeper scientific pursuits, and as regards the acquisition of knowledge bearing upon the various branches of industry; there is also a constant increase in the accumulation of geographical data, which, in spite of the great progress geography has experienced, in its new character of a science (thanks to the distinguished labours of Professor Ritter), can scarcely be mastered, whilst the interest taken in the drawing of comparisons becomes daily stronger. Abstracting entirely from mathematical geography, that is, from all the measurable terrestrial relations, and keeping in view the physical relations only, there still remains an extraordinary quantity of matter for scientific investigations, which forms a basis for political elements, and furnishes the most certain and positive evidence of the intimate connection between the earth and the human race, and of the absolute dependence of the various stages of civilization, which nations and empires are destined to pass, upon the physical characters of countries.

These physical relations are considered in reference to Continents, taken in their whole extent, viz., they embrace stereography, hydrography, and atmospherography; the distribution of products; even man himself, according to his natural elements, composed of, or resulting from, the varieties of races, and the influence of the earth upon mankind in general; finally, they embrace the changes which have occurred on our globe, together with the operating causes. Not losing sight of the principle, that nature exerts a powerful influence on nations, and mankind in general, and that the greater or smaller progress of all the geographical relations of nations depends on the development and amelioration of the physical condition of continents or countries, it is easy to perceive, that what is termed political geography entirely rests upon physical geography; for, in the first place, it has to explain the physical character of the country, its situation, elevation, and depression, as well as its condition and degree of cultivation; and, in the second place, it has to inquire into the intellectual, religious, economical, and political relations of the population, as also to give an estimate of the amount and density of the latter.

A general survey of this department of political geography shows, to the attentive philosopher, that all the relations just enumerated, are closely connected with the physical development of a country. On searching for the conditions, means, and causes of this natural development, one discovers them in the presence or absence of forests, or in their extent corresponding to the other relations; for the physical history of every country, from the remotest periods down to the present, furnishes innumerable arguments in support of the truth, that a moderate extent of forests, especially on mountains, and elevated ground, where tillage is impracticable, promotes in a high degree agriculture, trades, manufactures, and other enterprises for increasing production; also commerce, and all the interests of individuals, as well as of entire nations.

Hence it follows that forests deserve to become an object of careful examination, not only in a politico-economical and financial point of view, but also, and more particularly, in a geographical point of view, whether they be considered in reference to the one or the other section of political geography.

This subject having still failed to attract the proper attention, it is our intention to dedicate the following pages to the support of the above assertions, and to direct the thoughts of the students to whom geography and its problems form an object of interest, to the examination of a geographical element, which is of the last importance for the study of geographical relations, whether they refer to the physical aspect of countries, or to the population and all its interests. No person, at all familiar with our subject, will deny the truth of the following propositions: 1. The more simple the form which the coasts of a continent exhibit, the more simple are all its internal relations: 2. The more developed the former, the more developed are all the geographical relations of the continent, and of its individual countries and population, as regards the physical, intellectual, and moral, as well as the economical, political, and social condition of the latter: 3. The nature and the physical character of a country exert a powerful influence on nations: 4. The climate in particular controls the whole being of man, as far as he is an object of geography; in illustration of which, compare the inhabitants of the polar and equatorial regions with those of the temperate zones, observing, that the mind and body of the latter are not, as is the case with the former, arrested in their onward progress of successful development by a heat, which enervates the body, or by a cold, which causes it to shrink and to contract: 5. The capabilities of cultivation of the ground accelerate the development of the bodily and mental faculties: 6. The particular shape of a country powerfully influences the character and civilization of the people; for example, mountain plains are chiefly tenanted by nomadic tribes, who, from time to time, invade the lowlands, subjugate the natives, and inspire them with new vigour and activity; whilst the naturally favoured countries, the river and valley districts, by a more or less considerable variation in the elevations and depressions of the country, by alternate plains of more or less fertile soil, by the modified effects of light and heat, by a greater or smaller variety and quantity of natural products, &c., keep the inhabitants in a state of perpetual bodily and mental excitement: 7. and, lastly, The peculiar features of continents, and of their individual portions, impress themselves on the character of the whole population, and the peculiarities of the different portions of a continent exert a very varied and mysterious influence upon the character of their respective occupants.

No one will question the fact, that intense cold contracts the body, and checks the development of the mental faculties; as also the reverse, that intense heat provokes the human passions to the utmost, and finally reduces man to a state of indolence and apathy; so that, in either case, man cares for nothing beyond the gratification of his animal appetites; but that, on the contrary, civilization commenced and prospered under the milder clime of the temperate zones; that the life of those children of the desert, who have made but one step in advance, is very monotonous and insipid, and presents but few wants and symptoms of developed faculties, because their native land is either a barren wilderness, or unfit for cultivation; but that, on the other hand, fertile countries and those susceptible of tillage, are inhabited by agricultural nations, who, in obedience to their own laws and government, are socially and politically united, and thereby enabled to make progress in their civilization; whilst some of these countries, owing to their excessive productiveness, cause the inhabitants to relapse into a state of indolence and lethargy, and to remain stationary at a certain point in their onward course; whereas other less fruitful countries tend to rouse the bodily and mental energies of man, and to promote a more vivid display of mental activity.

These and many other data, deducible from the comparative researches of Professor Ritter, and clearly establishing the powerful influence of the physical character of countries upon all geographical relations, and all the interests of nations, are closely connected with the more or less favourable situation of forests, because it appears, from innumerable facts in the physical history of countries, that the clearing of forests produced considerable changes in the physical condition of countries, which were and still remain connected with equally momentous and influential changes in the economical, commercial, and artificial, as well as in the moral, mental, and political relations of the population: because forests, by means of their powerful effect on the condition of flowing and standing waters, on the atmospheric moisture, on the amount and more or less frequent occurrence of rain, on the local temperature, and, owing to this joint influence, on the productive powers of the soil, and on the salubrity of the air, react upon the physical condition of countries and the social relations of the population. The dependence of the temperature on geographical situation, on the direction and rapidity of the winds, on the presence and extent of flowing and standing waters, on the greater or less elevation of the ground above the level of the sea, and on the external shape as well as the internal nature and condition of the soil, is attested by a great many phenomena. Less known, however, is the influence of forests upon temperature, because it has as yet, to a certain extent, escaped examination, and because the method of comparison has not been applied to demonstrate its reality.

On considering, however, that the dark colour of forests not only prevents reflection, but favours the absorption of caloric, one may easily understand how they contribute to a decrease of temperature, just as sandy plains cause its increase. This latter case occurs, for instance, in the deserts of Africa, which are densely covered with glassy sand; whereas the immense forests of Guyana, owing to their dark green colour, considerably reduce the solar heat, and, under the same latitude, produce in the annual mean temperature a difference of 8° to 10° R. (18° to 22° F.), by which the thermometer stands lower in the former than in the latter region. From all the comparisons of the mean temperature of countries, situated under the same degree of latitude, of which the one is clear, the other full of forests, there always results a difference of 1 to 3° R. (2° to 7° F.), by which the temperature of woody countries stands lower. A comparison of France and the Netherlands with Hungary, Bohemia, Bavaria, Austria, and Prussia, regarding their climate, renders that difference more manifest; and a parallel drawn between France and western Russia, or also between France and Louisiana, Guyana, and the United States of North America, gives a balance of from 2° to 6° R. (4½° to 9½° F.).

These facts are still more strongly illustrated by the depression of the isothermal lines in woody countries, and by their elevation in those destitute of forests. In the former they descend further towards the equator, in order to attain the same mean temperature, because forests tend to diminish heat as well as cold; the isothermal lines mount, therefore, higher up in the latter. The isothermal line of 8° R. (50° F.) ascends in the Netherlands beyond lat. 53°; in Rhenish Prussia, and the two principalities of Hesse, it descends as far as 50½°; it then rises in the north of Germany a little beyond 52°, and sinks again in Saxony, Bohemia, and Gallicia, down to lat. 49°; because, these countries being well stocked with forests, less latitude is requisite in order to compensate the depression of the mean temperature which is owing to the forests, by a more southerly situation.

The isothermal curve of 10° R. (54½° F.) begins in the west with lat. 47°; rises in France, between the Loire and Seine, nearly as far as 48°, because that district is almost completely stripped of wood; but afterwards it sinks again between the Seine, the Moselle, and the Rhine, and in French Switzerland, as far as 45½°, preserves an oblique direction throughout the whole of southern Germany, Hungary, and Transylvania, until, in Wallachia, it falls as low as lat. 44°. The depressions plainly indicate the woody countries—the eastern provinces of France, Switzerland, the whole of southern Germany, and Hungary; the elevations again point out the woodless countries: so that the results obtained from our comparisons attribute a difference of 1° to 2° (2°.25 to 4°.50 F.) in the mean temperature to the intervention of forests, scattered over the different countries in Europe; whilst in America, which is densely covered with wood, that difference amounts to 4° (9° F.)

The direction of the isothermal lines proves in detail, that the influence of forests is universal, and that it affects the climate of the continents as well as of the sea-coasts. By carefully following out these comparisons, we arrive at the conclusion, that on the influence of forests depends the remarkable distinction between the Old and New World, and that it produces the great contrast between the northern and southern countries of Europe, between the north and south of Germany, between the west and east of France, and between the northern and central provinces of Spain; and that a climate, subject to the influence of forests, is not only less hot, but even much colder; whence there arises a considerable and material difference, plainly perceived by keeping in mind that the entire vegetation depends on the distribution of heat during the different seasons, and that the nearer the thermometer approaches the freezing point, and the longer it continues in that low state, the shorter will be the catalogue of indigenous and exotic plants, the more limited the number of those useful to man.

At the period when Asiatic tribes invaded Europe, its surface was covered with wood. It appears from Arthur Young's Travels, that the territory of Great Britain was densely overspread with large forests; for Ireland, at present entirely clear of woods, is called the Woody Isle, and the descriptions of Strabo and Ptolemy corroborate the existence of extensive forests in Scotland, where the Roman legions and auxiliaries were, by the command of the Emperor Severus, employed in rooting out and cutting down forests. According to the report of Tacitus, the sky of England was constantly overcast with clouds and rain; the cold, however, was not very great, and, according to Cæsar's statement, the climate was much milder, and the cold not so intense as in France.* At present the contrast is more striking, for the removal of forests wrought a considerable change in the physical condition of both countries, whence all their geographical relations became essentially modified. We recommend a more careful investigation

* Vita Agricolæ, cap. 12, and Cæsar de Bello Gallico, lib. v. cap. 10.

of this subject to those who take an interest in the study of comparative geography.

In Italy the forests occupied all the hilly districts, whilst the water accumulated and spread over a great portion of the lowlands. The history of the earliest periods informs us that huge masses of timber were exported in exchange for wine, oil, and other products,* which the natives were in want of; and, according to Vitruvius, the timber grown along the Tyrrhenian Sea was preferred to that coming from the shores of the Adriatic. Inundations of the Tiber surrounded the Mons Palatinus with a marsh; and the woody declivities of the Apennines exerted such a powerful influence on the temperature of Italy, that, according to Livy, the Roman soldiers, though hardened by the fatigues of many campaigns, had much to suffer at the siege of Veji, in the year 404 ante Ch., from violent frosts and heavy snow storms, because the hills around that city were covered with large forests.† According to the report of Columella,‡ there were, in his own lifetime, winters so severe that the frost destroyed every tree in the vicinity of Rome; and Livy states,§ that the ice interrupted the navigation of the Tiber in the year 354 after the foundation of Rome. Juvenal mentions,‖ that, in the year 128 ante Ch., the Tiber was every winter regularly covered with ice; and Horace, in his Odes,¶ states, that the streets of Rome were frozen over, that Mount Soracte was covered with snow, that the weight of the latter weighed down the woods, and that the ice obstructed the course of rivers. Upon the advice of Virgil,** the lambs born on the adjacent fields of Rome were protected against destruction by cold, and various precautions were adopted to neutralize the injurious effects of winter.

All these phenomena have long ceased, and, together with the changes in the physical condition of Italy, corresponding alterations have taken place in all the relations of the population, of which history and geography bear ample witness.

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* Vitruvius, lib. ii. cap. 10.

† Livius, lib. v. cap. 13.

‡ Colum. R. R.

§ Ut Supra.

‖ Juvenal, Sat. 6. 521. Hibernum fracta glacie, descendit in amnem.

¶ Ode 8, lib. i.

** Georg., lib. iii. v. 298.

Occasionally, but very rarely, we hear of snow-showers, but as soon as the snow has fallen, it melts away, and the refrigeration of the air, which in a warm country is termed cold, lasts for such a short period, that it produces no sensible alteration on the high temperature of Italy. The prevalence of this latter must be exclusively attributed to the clearing of forests situated on the slopes of hills and mountains. Accordingly, hoarfrost appears to have now become one of the most rare occurrences. In France, husbandry made but slow progress, and civilization dawned at a much later period than in Italy; in consequence, its physical condition changed by long gradations only, and forests continued for a considerable time to overgrow the country, and to impart an icy temperature. In Spain, according to the report of Livy,* the snow falling in the winter of the year 218 ante Ch., lay, in the vicinity of the Ebro, for 30 days, to the depth of 4 feet. In the beginning of the first century, the whole north of France was covered with forests and morasses. Tacitus states,† that in Germany the cold weather prevented fruit from thriving and ripening, and, even 50 years ante Ch., the grapes failed in the cold forests of Belgium. According to Ovid,‡ the vine plantations were frequently destroyed with frost, in countries where nothing similar is now witnessed; even the wine froze in open vessels, and the Black Sea was not only covered with ice, but the coating so strong, that heavy waggons could safely pass over.

A temperature so low, and which, for instance, in the southern districts of Germany, was lower by 8° R. (18° F.) than it is now, occurs no longer in these and other countries of the same latitude. We draw, therefore, the conclusion, that, about 1800 years ago, the forests exerted such a strong influence on the climate, that the mean temperature of the coldest month stood from 4° to 5° (9° to 11¼° F.) lower than at present; that Germany, owing to its morasses and forests, was visited by winters resembling those of western Russia. It follows, from observations, calculations, and historical traditions, that, in conse-

* Liv. lib. xxi.

† Tacit., German., p. 5, and Cæsar de Bello Gall., lib. ii. cap. 2.

‡ Eleg. 10.

quence of the clearing of forests, the temperature of countries has been on the increase.

If we now keep in mind the powerful influence of climate on the peculiarities, manners, and customs of nations, on the development of their intellectual and moral faculties, and on the cultivation and productive powers of the soil; and if we consider how much climate depends on the greater or less extent of forests, we have surely collected arguments strong enough to prove the assertion, that all the geographical relations of nations ought to be viewed as intimately connected with forests, and that it should be held a main problem of comparative geography, to estimate their influence in all its bearings, and to acknowledge them as principal agents.

Similar and still greater alterations in temperature are linked with those depending on forests, in so far as they are attributable to the formation and preservation of flowing waters. The countries and nations of antiquity, as well as of modern times, bear witness to the powerful influence of the latter upon the physical condition and fertilization of countries, as also upon trades and manufactures, agriculture and commerce, navigation, and all the other branches of industry; besides, in an indirect manner upon the intellectual and political character of the inhabitants. The ancients regarded Egypt as a gift of the Nile; the country situated between the Euphrates and Tigris, as the happy land or Paradise. Civilization made the most rapid progress along the banks of rivers, where the population is generally very numerous and in a prospering condition. The Rhine, with the adjacent districts, offers the best proof of this assertion; the Weser, Elbe, Oder, and Vistula, the Rhone and Danube, not to mention many other navigable rivers in Europe, demonstrate the same. Inundations and the retreat of flowing waters are two phenomena which considerably modify the physical aspect of countries. All the rivers of Europe testify the retreat of their contents to a lower level, as also the drying up of innumerable springs. The large masses of water they used to carry formerly, excavated the valleys and laid the foundation of the alluvial plains. The sand drifted along by the Seine and other French rivers is still found at a great distance from their banks, and leads us to form an estimate of the breadth of their original beds; the Po furnished the alluvial soil for the plains of Lombardy; the Danube formerly filled a broad valley, and the mud of the Mississippi gave origin to the boundless Savannahs of Louisiana. The inferior and central course of rivers presents, therefore, a picture very different from that which formerly existed and is still traceable in their superior course. Here we find mountains covered with snow and ice, or over-shadowed by large forests, running parallel with the superior, and generally extending as far as the central course. The Rhone, Rhine, Danube, Weser, Elbe, Maine, Neckar, and other rivers, confirm this assertion, which is still more supported by an examination of the sources of the Ganges, Dschumna, and Buramputer, fed by the snows of the Himalayas, as also of those of the Orinoco, Amazon, and La Plata, issuing from the forests of the eastern Andes.

At the same time that the forests on the northern slope of Mount Atlas convey nourishment to a number of springs and rivulets, and become thus the chief cause of fertilization, aridity prevails on the southern slope, where the Sahara, with its bleak and barren rocks of basalt, is close at hand. A similar contrast may be traced between the eastern and western slope of the Andes of South America. The former is covered with extensive forests, giving rise to the sources of the Orinoco, the Amazon, the Paraguay, and the Parana; the latter, possessing no forests, is destitute of lakes and rivers, owing to which the western regions of Peru and Chili lie barren and waste. It is impossible to attribute the cause of this to the mountains from which those rivers draw their sources; in that case, neither the Volga, the largest river in Europe, which rises at an elevation of scarcely 700 feet above its mouth, nor the Mississippi, which takes its origin on a bare mountain plain, could carry with them masses of water as large as they do in reality.

All the wild regions abound with rivers more numerous and more copiously charged than those belonging to civilized countries. This difference may be accounted for by the clearing of forests, which causes the springs to dry up, and deprives the rivulets, lakes, and marshes, of their nourishment. In consequence, the larger rivers sink below their ordinary level, their beds become narrower, their course becomes less rapid, and the process of evaporation loses in energy ; so that we may lay it down as an axiom, that, the more populous a country, and the longer inhabited, the more deficient it will be in forests, springs, and rivers ; again, the more recently and the more thinly peopled, the greater will be the amount of forests and flowing waters.

Tartary, Persia, and many other countries of Asia; also Spain, Italy, and Greece, attest the circumstance, that a scarcity of forests involves a scarcity of water, and that the bare and bleak condition of the mountains, constitutes the reason why their declivities are without springs, and their valleys without rivers. The highlands of Central Asia possess neither forests nor flowing waters ; small brooks are by heavy showers swollen into torrents, which carry the mould from the fields to the valleys, lay waste the fertile districts, and inundate their crops. The United States of North America present relations materially different. They contain immense forests and systems of vastly expanded lakes and rivers, which partly facilitate, partly impede, the geographical development of these countries ; they prove, moreover, that the extent of forests is uniformly proportional to the amount of flowing and standing waters. Comparative geography informs us, that mountainous, and at the same time woody countries, are amply provided with water ; that the clearing of forests causes the springs and brooks to dry up, and rivers to sink below their ordinary level, so that their beds and channels are finally blocked up with sand and mud, and rendered useless for navigation ; that the irrigation of fields depends on the extent of forests, covering the slopes and summits of mountains ; again, that forests contribute to the expansion of navigable waters, put a stop to the pernicious effects of aridity, and keep the ground in a state of moisture and productiveness ; finally, that the countries in the south of Europe, Spain, Italy, and Greece, in consequence of the clearing of forests, witnessed the drying up of their springs and streamlets, the almost total annihilation of their rivers, and the transformation of their soil into comparatively barren regions, so that the development of every geographical relation became seriously affected.

The United States, again, present the opposite picture. They abound with every thing promoting the physical development of the country, and enable the geographer to draw a distinct line of demarcation between the savage and the inhabitant of a civilized country. The material interests form the basis, and are amply supported by the indirect influence of forests; for these latter constitute, with regard to agriculture, commerce, trades, and manufactures, one of those elements which are indispensable to the prosperous condition of those branches of industry. The geographer, guided by the method of comparisons, has to draw his inferences respecting the relations of the population, from these agents, which so materially influence the physical development of countries.

The annual amount of rain is chiefly determined by the greater or less abundance of flowing and standing waters. Not taking in account, that it is to a certain extent dependent on the geographical condition of countries, as also on winds and mountains, by far the greater portion of it must be referred to the agency of waters and forests, which, when the latter occupy the slopes of mountains, are sure to increase the fall of rain; and, in consequence, to affect the interests of agriculture, trades, manufactures, and navigation. We might furnish an abundance of examples, taken from the physical history of our globe, from meteorological observations made to ascertain the amount of rain, &c., in order to substantiate the assertion, that the gradual reduction in the quantity of rain falling in the south and west of Europe, is by no means the result of more extended agriculture; but that the clearing of mountain forests presents itself as the most obvious and efficient reason.

The simple fact, that level countries, under nearly the same latitude, although the one contains a larger extent of forests than the other, receive about the same annual quantity of rain, tends to demonstrate that it is only the mountain forests which, by determining an increase in the average quantity of rain, exert a powerful influence on agriculture, and the other branches of industry. We may thus easily understand, why, in the woody plains of Bavaria and Prussia, the amount of rain is, on the whole, not much more considerable than in the plains of Champagne, which are destitute of wood; and why the productions of the former are not so numerous as those of the latter. This fact throws a new light on the nature of the influence which forests exert on the physical character of countries, and on the geographical relations of the population.

Atmospheric humidity is one of the most powerful agents in the formation of climate, and leads to the most remarkable changes in the physical aspect of countries. Observe, for instance, the great contrast between the savannahs of America and the sandy deserts of Africa, and between the north and south of Germany, as also between the geographical condition of the respective populations; for the highly sensible effects of moisture greatly favour the growth of plants and the breeding of insects, but they are exceedingly prejudicial to the animal functions of the mammalia, and, in particular, of the human race. A very moderate degree of humidity, or a very dry condition of the atmosphere and of the ground, produce contrary results, because they are incompatible with the life of insects, and constantly lessening the number of cultivated plants, thereby circumscribing, in the same proportion, the means of support of the human species.

Comparative geography proves to us, that, on the one hand, a damp atmosphere produces a damp country; that the latter, though unwholesome, is in a state of productiveness, and capable of increasing the products of agriculture, as also, by judicious management, capable of tendering the most precious gifts, and that it has entered the period of youthful development; of which we have a striking example in the United States of North America. On the other hand, a dry atmosphere produces a dry country, which, though wholesome, assumes a more and more barren aspect, until it refuses to supply its inhabitants with the necessary nourishment; as an instance, we may mention the deserts of Asia and Africa. These deserts are plains destitute of water, plants, and animals; they present the causes and effects of the last degree of dryness, and exhibit nature as it were in old age, a melancholy picture of its decrepitude. The geography of Asia, America, Africa, and Australia, in contrast with that of Europe, exhibits the influence of the physical character of these countries upon the population.

Whilst at an early period, the Europeans, by their perseverance, cleared the country of forests, dried up the swamps, erected dykes along the banks of flowing waters, promoted the free circulation of the air, and by all these means relieved the atmosphere of its noxious vapours; and, whilst the Americans are still engaged in the execution of this laudable object, forests have wantonly been destroyed in several countries of Europe; for instance, in the three peninsulas traversed by the Apennines, the Pyrenees, and the Balkan, as also, in many countries of Asia and Africa, which, under a long-continued civilization, have, as it were, attained to old age, and are now suffering from dryness of the ground and atmosphere, owing to which these countries are completely exhausted, misery is spreading among a scanty population, and the geographical relations present features very poorly developed. An attentive comparison between the southern and central countries of Europe, as regards their agriculture, trade, manufactures, and commerce; and, again, as regards their immaterial interests, leads us to the conviction, that it is especially the absence of mountain forests, which operates as a check upon all the material interests of the southern nations, and, in consequence, causes the immaterial interests of these nations to be depressed.

Considering the powerful influence of atmospheric humidity upon the various geographical relations, it becomes evidently a matter of importance to analyze our comparisons, and to reduce them to those elements which co-operate in the production of such a state of the atmosphere. Besides the evaporation of the sea, and of flowing and standing waters, it is chiefly the process of evaporation going on in forests, which constitutes a very efficient cause of the diffusion of moisture. Notice, for example, the agreeable coolness of forests during hot weather, attributable to the evaporation of watery particles from the surface of plants; and witness the subsequent local depression of the temperature indicated by the hygrometer, which, on exposure to the action of the forest atmosphere, rises quickly to the highest point of the scale;

so that, on account of the extreme evaporation carried on by the forests of hot countries, the instrument is unavailable for the purposes of measurement, since there the moisture considerably exceeds the highest point of the scale.

Many and often-repeated observations have proved, that the quantity of watery vapours with which the atmosphere is pregnant, increases proportionally with the extent and preservation of forests on the plains; as also that it is modified by the nature of the plants which have a particular share in determining the influence of forests on the formation of atmospheric humidity.

Comparisons between woody and woodless countries furnish many arguments in support of these assertions, which are applicable even to individual countries, for example, to Spain, France, &c. Accordingly, the province of Gallicia is indebted to the forests for its numerous population, and for many things in which it claims superiority over almost all the other provinces of Spain; the province of Estremadura, again, owes the want of agriculture and the barrenness of the soil to the absence of forests; the greater part of this latter province and of that of Murcia presents a bleak, barren, and desolate aspect, merely because the mountains have been stripped of their forests. In the latter province, there is frequently no fall of rain for eight or ten months; and the prevalence of dry sultry weather forbids agriculture, and checks the increase of population. The woody province of Valencia, on the other hand, enjoys a mild and agreeable climate; its damp and productive soil is even favourable to the growth of the nobler products, such as vines and cotton-trees, rice and palm-trees, and completely provides for all the necessaries of life. Many other provinces, deprived of their forests, for example, Old and New Castile, exhibit strong contrasts in comparison with the woody provinces of Leon, Catalonia, &c., clearly proving, that the mountain forests impart a moderate quantity of moisture to the atmosphere, which preserves the soil from becoming dry and useless, promotes the growth of plants, and is favourable to the increase of population, as well as to the thriving condition of the products of agriculture and of other branches of industry; finally, it procures the means necessary for the sustenance and comforts of human life; whilst the absence of forests carries in its train, dearth, sterility, failure of crops, and scantiness of population. Various provinces of France furnish similar proofs, and demonstrate, on a small scale, that excessive aridity leads to poverty and starvation. Many parts of the south of Asia and of America are almost completely overspread with forests; but, although an excess of moisture helps to raise vegetation and the lowest class of animals to a high state of perfection, yet the unwholesome dampness of the atmosphere is prejudicial to the growth of animals of a superior organization, undermines the constitution of man, and forms a great barrier to the development of his faculties; so that the injuries arising from this are fully equal to those created by the prevalence of excessive aridity: hence we conclude, that mountain forests, by originating a moderate quantity of moisture, contribute to the preservation of the general health, likewise to the advancement of agriculture, trades, manufactures, and commerce, as also to the prosperity of the population; and that they deserve, on that account, the greatest attention, and to be acknowledged as an essential element of comparative geography, which, though it materially influences the progress of civilization, has hitherto been too much neglected by geographers.

In regard to atmospheric humidity, woody countries differ greatly from those destitute of woods. The United States are, by one-half, more damp than the southern parts of Italy; the shores of the Caspian Sea twice as damp as those of the Bay of Biscay; the districts of Paris and of the northern provinces of France, scarcely half as damp as Bavaria and Hanover: and, again, Saxony and Prussia suffer much more from moisture, than England, Scotland, and Holland, though these latter are reckoned among the dampest countries in Europe. The evaporation of forests contributes to this increase of atmospheric humidity, as also to the well known lower state of temperature in the New World, relatively to European countries under the same latitude. These, and similar facts, procure us a positive knowledge of the former physical condition of Europe, and convince us, that Spain, Italy, and France, were, at an earlier period, as woody as the corresponding countries in North

America; that they had a temperature lower by at least 4° or 5° R. (from 9° to 11° F.) than it is now; that the banks of the Tiber were as cold as those of the River Seine, at present; that the banks of the Po, in point of temperature, resembled those of the Caspian Sea; and that, in general, the large forests of Italy and of the southern parts of France, formerly gave rise to a climate entirely different from what it is now-a-days. In accordance with these facts, Italy, owing to the quantity of moisture produced by its forests, may have resembled the United States; and France, the woody territories of the northern parts of Europe.

By careful comparisons we may gain a knowledge of the changes, which, in virtue of the contrasts visible in the physical condition of countries, have affected the various relations of the races occupying the different portions of our globe. All the material interests of nations are intimately connected with that physical condition, nay, they positively depend on it. Through the medium of this latter, they are also influenced by the variations in atmospheric moisture, which are, as we have seen, in a great measure, the direct result of the action of forests. Moreover, the immaterial interests are thus indirectly brought under its control, a circumstance deeply affecting the welfare of all the European, and, in particular, the German nations. Comparative geography furnishes the most satisfactory evidence of this peculiar and thorough connection between the physical condition of countries and all the geographical relations of the population, and it deduces these proofs especially from the influence which forests exert on the elements just alluded to; as also on the fertilization of the soil, which, on account of the importance recently attached to the seriously and broadly discussed population-question, play a very prominent part in modern science. We ought not to neglect the circumstance, that forests contribute in various ways to the production, preservation, and accumution of a fruitful soil, either by preventing the rich mould and alluvial soil from being washed and carried off the slopes of mountains, the waters of the sea from making irruptions, the sand of deserts from being scattered about, and the ground from becoming exhausted; or by influencing the condition of those atmospheric elements which assist in fertilizing the country; or, finally, by originating that rich humus, which, being so beneficial to the growth of plants, provides the people with food, the trades and manufactures with raw materials, and commerce with the articles for barter.

In mountainous but woodless countries, dislocated masses of soil carry desolation down to the valleys; torrents of rain remove and detach from the rocks the loose and fertile mould, and deprive the mountain slopes, thus exposed in all their gloomy nakedness, of the share they had in the irrigation of the plains. The forests extending along the sea-coast, by interlacing their roots with the sand and alluvial soil, help to render it more compact; they also assist in arresting the mud, deposited by rivers, and in forming the so-called deltas at the estuaries of these latter: thus, a girdle of palm-trees, stretching along the western edge of the valley of the Nile, prevents the fertile plains of the latter from being buried by clouds of Lybian sand, and protects agriculture against the masses of flying sand, drifted about by the blasts of whirlwinds. Large plantations are now acting as a check to the sands blown about on the plains of Gascony, and have laid the foundation for an increase of fertility never anticipated. In the larger countries of Europe, even in many parts of Germany, there occur instances of the beneficial influence of plantations.

Forests occasion the condensation of atmospheric vapours, convey nourishment to the springs, brooks, and rivers, and send moisture to the fields; they also render the climate more settled, and ward off from the crops whatever injury might arise from sudden and violent changes in the weather. By depressing the temperature, they prove, according to the localities, elevations, and degrees of latitude, either beneficial or injurious, and they constitute the chief cause that a woody country, the temperature of which stands, by their means, 1° or 2° R. lower than that of a corresponding woodless country, assumes the same physical aspect as if it occupied a situation 1–2° latitude farther north, or 224 toises of greater altitude; hence we conclude, that forests cannot but intensely affect the productiveness, the physical condition, and agricultural interests of a country. They establish, if we may use the expression, an artificial climate, modified in proportion as they are more or less scattered over the country.

There is not the least doubt that a reduction by 2° R. (4°.5 F.) of the mean temperature, obtained by raising plantations on the mountain slopes of Calabria, would restore to that country the delightful climate which prevailed at the time of the settling of the Greeks; and that Bavaria and Hungary might be brought to boast of the superior vegetation which characterizes the fine provinces of Flanders, Provence, Franche Comté, and that portion of Lorraine which is bounded by the Ardennes and the Vosges mountains, provided that the marshy forests on the banks of the Danube, and in other places, were cleared away, the ground properly drained, and the extent of forests rendered only commensurate to the wants of the population. Acting upon this plan, we would obtain an increase of about 2° R. in mean temperature.

Owing to their influence on temperature, the beneficial effects of forests, as regards improving the soil, increasing the quantity and variety of products, advancing the interests of trades, manufactures, commerce, and all the other branches of industry, and as regards insuring prosperity and opulence to the people, are indeed incalculable; for an unusually damp and chilly atmosphere is not only unfavourable to the growth of exotic trees and plants, but endangers their health, especially during the cold months of spring and winter, whilst in autumn it prevents them from bringing their fruit to a state of maturity. Again, excessive dryness and sterility diminish the number, and prove fatal to that class of plants which constitute the chief food for the sustenance of man and beasts. We need only allude to the breeding of silk-worms in Bavaria, which is carried on at a great expense, and is far from being in a prospering condition.

If we consider, that, in the torrid zone, the coffee-tree yields but a poor crop, unless it is planted on the slopes of hills, and screened from the heat of the sun by dense hedges of verdant galba and rose-apple; that the cocoa plantations suffer considerably, unless they are situated in the vicinity of forests, where they find shade and shelter, and a moist and deep stratum of vegetable mould; that the more delicate plants, if expected to thrive in the southern districts of Europe, require a similar situation; that the countries stripped of forests preserve scarcely a vestige indicative of their former productiveness; that the features of many countries, which have lost every trace of their former severe and unprofitable climate, are no longer recognisable; that the marshes of Burgundy have been rendered fit for the cultivation of the vine, and that Dauphiné has ceased to be placed, as it was under the dominion of the Romans, on the borders of cultivated territory; that maize yields fine crops even on this side of the Spanish and Italian frontiers; that the olive-tree is not confined to Greece and Italy; that the climate of the United States becomes milder in proportion as the settlers destroy and clear away the forests; and that the vegetables transplanted from Europe, which refused to thrive on account of the damp and cold condition of these woody countries, easily arrive at a state of perfection; that, on the other hand, the fertile plains on the slopes of Mount Atlas, deservedly called the gardens of the Hesperides, have lost all their fertility, together with their streams and forests; that the Canary Isles, extolled by the ancients for their delights and remarkable productiveness, are now despoiled of their forests and underwood, and threatened with the miseries of dearth and barrenness. If we consider all this, we are no longer in need of arguments to prove, that forests are a most essential element of comparative geography, especially as, owing to their influence upon the material interests of nations, they are likely to become an object of still greater importance; also because these interests are, by a certain party, proclaimed as the basis of intellectual interests; so that all the relations of the population are both directly and indirectly very closely connected with forests.

Moreover, if we take in consideration that forests furnish the materials for the formation of that vegetable mould from which plants derive the greater portion of their nourishment, and that every thing which falls from them is converted into a natural manure; that the plains situated beyond 68° lat., and probably not long ago abandoned by the sea, present themselves as a fertile moorland, intersected by many swamps; that the deserts extending on the south side of Mount Atlas, and which were never covered by forests, resemble large tracts of country inundated by the sea, and destined, no doubt, to remain for ever in a state of barrenness; that the isles of the Pacific, reared by volcanic action and but for a short period covered with lava, and not yet old enough for the growth of forests, or, therefore, for the formation of a fertile soil,—if we take all this in consideration, we must feel convinced, that all those countries which, since the last revolution on our planet, have either kept clear of forests, or have lost them long ago, or have acquired a new surface, cannot boast of that stratum of rich humus to which forests give rise, and which constitutes the chief cause of that remarkable productiveness which distinguishes the United States of North America; where, thanks to the forests, it unfolds to the spectator the matchless brilliancy and imposing grandeur of an ever green and ever flourishing vegetation.

Countries which, in the course of a long civilization, have lost their forests, have for more than 3000 years been subject to the fatal interference of man, and have laboured under the extremes of an unsettled climate, are now in a similar predicament, there being no prospect for the development of their geographical relations. They are lying in a state of aridity and barrenness, which can only be accounted for by the total absence of forests and of vegetable humus. The Abruzzi, Calabria, Attica, and the Morea, bear melancholy evidence, that not only the soil, but also the inhabitants, of these woodless countries are marked with the signs of old age.

The powerful influence which a productive soil, engendered by the intervention of forests, exerts on the condition of countries and nations, is proved by the historical fact, that the first communities were formed under the shelter of woods, and that agricultural pursuits attached the nomadic tribes to the banks of rivers and to the valleys, where decayed leaves and vegetables, transformed into manure, had contributed to the creation of a fertile soil, and, in consequence, laid the foundation for villages, cities, and empires; whilst, on the other hand, the inhabitants of deserts and pampas are still, and will for ever remain, barbarous and nomadic, because the ground, being destitute of vegetable humus, presents to the flocks, instead of nourishing and aromatic herbs, the withered blades of unpalatable grass.

The bad effects of scarcity of wood upon all the domestic, public, and social concerns of the people, are visible in many woodless countries, several of which are so deficient in trees, that the natives are obliged to eat their food half cooked, as also to employ the excrements of cattle (and even they are often hard to get) as a substitute for fuel. Thus the inhabitants of the Hebrides are compelled to undertake long journeys, in order to procure materials for the construction of ploughing implements, thankful if, at home, they chance to pick up what serves for the handle of a spade. Thus Greenland was deserted by Danish colonies, because the sea ceased to cast ashore the only timber to which they could have access in such a cold country; and in the western parts of France, as also in England, the scarcity of wood imposes the sacrifice of many comforts. On the other hand, Sweden has derived, for the last 1000 years, its principal revenue from the mere sale of timber; whilst England is obliged to spend immense sums for the building and preservation of its fleets; and Spain, which, owing to its extent of sea-coast, and its abundance of harbours, was formerly in the undisputed possession of the main (a claim to which nature seems to have entitled it), is now entirely blotted out from the list of naval powers. This, and a similar misfortune which presses equally heavily upon Greece and Italy, justify the assertion, that scarcity of wood is an evil, affecting every kind of national transactions, and by no means easily overcome; we are, therefore, still of opinion, that forests have from the beginning exerted, and will never cease to exert, a powerful influence upon the manners and customs of nations, as also upon their mode of living, their commerce, industry, and domestic concerns. Taking it, then, for granted, that the preceding discussions have demonstrated the intimate connection between the great historical events of the human race and the existence of forests; that mountain forests are politico-economical elements worthy of particular attention and care; and that they are as indispensable as the erection of dykes along rivers, channels, and public roads, as well as other measures intended to improve the physical condition of a country, and to adapt it to the necessities of the population; that the countries possessing a sufficient expanse of forests combine well-attested advantages, are highly favourable to the health of the people, and fully equal to the gratification of all their wants; that they are noted for their charming climate, abundance of water, fertility of the soil, as also for the number and variety of vegetable productions,—taking all this for granted, geographers ought to consider forests as an element exceedingly important, as regards the investigation of the physical condition of countries and the geographical relations of the population; moreover, as regards inferring from the excess of forests the original state of some countries, and from their scarcity, the final state of others; and, lastly, as regards the comparison of the results of these inquiries with the character of the population.

The geographer who has neglected to pay due attention to this subject, is unable to give a faithful and accurate description of the physical condition of countries, to compare the geographical relations of the people, both with the last and with one another, and thus to arrive at a system of solid geographical knowledge. Forests may be considered in many points of view, which geographers have hitherto slightly treated. They form a basis for a general outline of the geographical relations of continents, as well as of individual countries. On this basis, the geographer may build his system, derive from it his leading maxims, and be guided by it when treating of the development of the general relations of nations and countries. It is our most anxious desire that the above observations may draw the attention of geographers to this matter, which deserves, indeed, a more minute investigation, as it tends to reduce geographical data to general principles, and to render the study of geography more and more interesting and instructive.

Suggestions for the better Ventilation of Sailing and Steam-Vessels. By Robert Ritchie, Esq., F.R.S.S.A., &c., Civil-Engineer, Edinburgh. (Communicated by the Royal Scottish Society of Arts).

(*Concluded from page* 182.)

I could easily suggest other plans to attain the object, viz., a current determined to one point; for instance, the use of hot-water-tube apparatus, in which case the foul-air-extracting-trunk could pass through a coil of pipes heated by the galley-fire, and of which there might be several so heated, or the hot-water pipes could be placed within the trunk itself to raise the temperature of the air. See Richardson's work, London, 1839. A very old plan used in buildings was to have the hot smoke-pipe encircled by a tube of larger diameter, the air passing upward between.

The failure, however, of so many ingenious schemes, extending over so many years, for improving successfully the ventilation of ships, has tended very strongly to impress me with the idea, that any method to be extensively useful, especially as regards sailing-vessels, must enter into the original construction of ships. And with this view I would suggest the introduction into timber and iron-built ships, of a thorough and efficient system of spontaneous or self-acting ventilation, affording at all times an ample supply of fresh air in every part of a ship, by means of a judicious arrangement of air-flues in the former, and pipes in the latter. In a large class of vessels now afloat, by application of the openings or interstices between the timbers (presently in use for airing the frame-work) where the plan of close timbers has not yet been adopted, a free circulation of air might be effected at all times in lower decks and cabins. As regards the airing of the frame-work itself, its importance has long been a point of much interest for the preservation of the parts below the surface, though much difference of opinion among practical men is entertained on this point, one class advocating a free circulation of air about the timbers, and another the exclusion of air.* In a communication to the Royal Society of London in 1820, by Sir Robert Seppings, F.R.S., when giving suggestions for a new principle of construction of ships for the mercantile navy, he alludes to the ventilators of Dr Hales, and the utility of general ventilation, but attaches importance to the exclusion of atmospheric air for the preservation of the frame-work, though he was not inattentive to the value of admitting air to the interior of ships. Another view is taken of this subject in the able treatise on ship-building in the Encyclopædia Britannica, where the suggestion is made that the preservation of the timbers might be assisted by adopting the openings between the timbers themselves for the purpose of circulating air about them; and it is stated that, in the year 1827, the author had proposed this plan to the Admiralty. This opinion strengthens the view I entertain of the practicability of combining in a very simple way the general ventilation of the ship, with due attention to the ventilation of the frame-work.

The defect at present in airing the frame, where the interstices of timbers are made use of, arises from the difficulty of obtaining a current or circulation, from the inlet for the air being placed between decks, and no outlet being provided. But were it so contrived as to allow at all times a free current of external air by points of ingress and egress, the effect would be very different. It seems often overlooked, but there is no point more important to be attended to in spontaneous ventilation than that where openings are provided for the escape of impure air, others must also be provided for the supply of fresh air, and *vice versa*. It must not be forgotten that air, like other fluids, can only fill a given space, or, as one of the earliest writers remarks, "that unless openings are properly adapted to suffer air to pass freely through, the external air proves a stopper to the internal, and only mixes with the next in contact." The same law which regulates the effect of currents in

* Captain Symmonds, Surveyor-General of Dock Yards, has, in a man of war now ready for launching at Woolwich, carried the timbers solid about as high as the lower gun ports. Mr Lang, who is naval architect for the Prince Albert, 120 guns, now building, I am informed, does not intend carrying up the solid frame nearly so high.

natural caverns, and which has been successfully applied to the ventilation of mines, will apply with equal force here. We know that the air in a well remains stagnant and pent up; but, as has been remarked,* if two wells or shafts are sunk at a given distance from each other, and a horizontal passage cut from the bottom of one well to the other, so soon as the communication is made, there will be a tendency in the air to descend one shaft and ascend the other, whenever the temperature of the external air varied from that below. Applying the principle to the general ventilation of ships, there is nothing to prevent the converting of the open spaces between the timbers or ribs, into fresh or foul air flues or conduits. One series of these being arranged to convey down pure air—not to be taken from below, but from above the upper deck—to points of discharge at the floors of the gun and orlop decks, cabin-floors, or wherever requisite, and another series of openings, *entirely separated from the first*, to commence at the beams or ceilings of these respective places, and pass upward above decks as high as convenient, for the escape of the foul or vitiated air. The points of ingress or egress for the air between decks may be in the form of a horizontal slit covered with perforated sheet copper or zinc, to break the force of the current. The points of inlet and outlet for the air above deck might have their effect increased, by having the orifices so arranged, that, while protected from the weather, the former would open to, the latter from, the wind. A portion of the interstices of the timbers similarly arranged, communicating directly with the open air, could be made to circulate fresh air for the timbers of the ship; but the apertures for the ventilation requisite for crews and passengers, must have no communication with the former, so as to prevent the corrupt gases from the bilge entering the latter. Inconveniences may be experienced practically, in having the air openings as described, from the difficulty of constructing those on the upper deck so as to keep out the water; but were the principle adopted and carried into practice, the skill and ingenuity of ship-builders would soon overcome any such slight obstacles. Ventilation

* Letter of John Buddle, Northumberland, 1815.

cannot be attained unless fresh air is admitted from above.* When air is made to enter the openings between the timbers *below the hatches*, as is now done, it must be useless when the latter are put on, as must be obvious to the most cursory observer. Admit, however, the external air, as proposed, and whether hatches were secured down, or side-ports closed, in whatever state of weather, there would be pure air conveyed to the inmates below; and although in some cases this mode of ventilation might be imperfect, yet it possesses the advantage of being always in operation, requiring neither attention nor labour, nor incurring expense. To make it more complete in winter, the external air openings would require to be provided with means for regulation.

Were it necessary to attain a greater certainty of perfect ventilation, at all times and in all climates, recourse may then be had for increasing the circulation to the plan I have alluded to, of artificial suction by heat; and instead of allowing the foul air to escape upwards from the tubes or pipes, the air might be collected from these into one horizontal trunk, and conveyed to the galley.

In iron-built ships, and in all vessels where there are no interstices between the timbers or ribs, or where these cannot be made use of, iron, copper, lead, or zinc pipes may be substituted instead. Nor would the space these occupy form any obstruction or ground of objection, as the air-pipes could be made flat or square, keeping the line of the inner wall of the ship. By some such simple arrangements as these, I can hardly doubt very considerable improvements would be effected generally in the ventilation of ships, and the obstacles to the permanent use of any machine, however perfect, in sailing vessels, must make the view I here take of it more important. It can, however, only be brought about by shipowners and others giving encouragement to the combination or incorporation of ventilating arrangements with the construction of ships,

* The advantage of conveying air directly downwards from the upper deck, has been recently fully established in the Apollo Troop Ship from China (and one or two other instances), whose merely small openings at the gunwales, with lids to shut down in wet weather, are made use of. How easily might this principle be extended, and rendered most efficient, as above described!

such as have in a similar way been successfully done in domestic and public buildings. (See observations by me on this subject, Arch. Mag., July 1837.)

I do not wish, however, to be understood as inferring that even any such mode of spontaneous ventilation as could be incorporated with the framework of ships, would prove at all times sufficient for the ventilation of an overcrowded vessel. The immense deterioration of atmospheric air, by 600 to 800 persons crowded into a small space, where the cubical contents bear no proportion to the cubic feet of air required for each person (10 cubic feet being considered as requisite per minute to afford a wholesome atmosphere), renders such arrangement next to hopeless without mechanical agency. So long as vessels are overcrowded, hardly any plan can be devised which can afford an adequate supply of fresh air to lower-decks during night; all that can be done, without artificial means, is to prevent positive injury to health, by affording a constant and uniform supply of fresh air below decks at all times, which surely is deserving of the most serious consideration.

In very crowded ships, such as troop-ships and others, whether any arrangements, such as alluded to, are provided or not, the wind-fan, as improved, could be advantageously made use of. Two or more of these machines, worked by hand, would speedily renovate the air of a lower deck, by means of flexible pipes communicating with different parts of the vessel; in emigrant ships, the passengers would, doubtless, very gladly work these machines for the sake of fresh air in warm latitudes.

Another ventilator could likewise be advantageously applied in many cases in sailing ships, namely, an exhausting pump, with a hose or pipe, on the principle of pumping out the foul air, or a condensing or force-pump to throw in atmospheric air, worked like the pump of a ship or fire-engine. One of the earliest recommendations of a pump for ventilating purposes noticed, is by Dr Desagulier. He mentions in his Experimental Phil., that, in the year 1727, he brought before the Royal Society an attempt to shew how damps or foul air may be drawn out of every sort of mine by an engine which he contrived. "The engine consists of a triple crank with three pumps, which both suck out and force in air by means of three regulators, and are alternately applied to drive air into, or draw it out from, any place assigned, through square wooden trunks which, being made of slit deal, and ten inches wide inside, are easily portable, and joined to one another without trouble."* Dr D. illustrates his description with notices of several experiments. At every stroke, eleven cubic feet of air was driven in, or as many sucked out; if the axis of the cranks turn sixty times in a minute, one man in that time might change a whole cubic space of eight feet; and by his estimate, a man breathes a gallon, about 287 cubic inches, of air per minute, and a candle, six in the pound, will burn nearly as long in the same quantity. This agrees with modern calculations, at the lowest estimate—300 cubic inches are contaminated by a man per minute, although Tredgold and others take the quantity at 800 cubic inches, and a single candle alone at 300 cubic inches. These facts go far to prove the necessity of ventilation; and in experiments made on board ships' lower decks (Philosophical Transactions, vol. 47), it is stated that a candle burned 67 grains in thirty minutes, where there had been no ventilation for twenty-four hours; after six hours' ventilation, it burned 94½ grains in the same time. Combustion could barely be maintained in the former atmosphere.

If the utility and convenience of Dr Desagulier's hand-pumps realized the description given of them, they might still be usefully employed in the ventilation of the lower parts of ships. Many other mechanical contrivances might be noticed. For instance, the double air force-pump, worked by two or four men, on the principle now in use for diving-bells, which is worked by a lever, upon a standard, on the plan of Dr Hales' ventilator. Triewald's ventilator (page 383) was probably on this principle. It may also be noticed here that the success which attends forcing down air, into mines, by means of a fall of water, points out how the foul air, which accumulates in the well of a ship, might in a great measure be discharged by letting down to and pumping out fresh water from the well. As the use, however, of mechanical ventilators has been generally, and still may be, even when they are re-

* The machine was cheaply made—the pumps of copper, and crank of iron.

sorted to, of temporary duration in sailing vessels, no doubt, chiefly from the want of a motive power, my object in directing attention to a thorough system of spontaneous ventilation has been to shew that in my opinion it is most likely, if properly achieved, to be permanently useful.*

In steam-ships, however, there can exist no obstacle to the expelling of noxious air mechanically, or the application of a perfect system of mechanical or artificial ventilation, nor can there be any reason why they should not be properly ventilated. Yet I question much if any where an efficient system has been introduced. I have, indeed, observed of late years an attempt to introduce ventilation into the cabins of a few steamers by providing small iron pipes from the ceilings, passing upwards through the deck; but, unaccompanied as these usually are, with fresh air inlets from above, they cannot prove efficient, and only tend, perhaps, to create annoyance; however, the introduction of these acknowledges the necessity for ventilation being provided.

In the common arrangement of steam-ships conveying passengers, the sleeping berths enter from the saloon or main cabin; hence it may be said that eating, drinking, and sleeping go on in the same apartment. The atmosphere from such causes soon becomes noxious, which is generally farther increased by what Mr Dickens, in his American Notes, so strongly condemns, the red-hot sulphurous stove, the inconvenience of which is increased by passengers crowding round it. No wonder the air in such cabins and saloons is sickening and unpleasant for respiration. During the day, if the weather be fine and hatches open, matters may go on pretty well; but in bad weather, or during the night, the case is very different. If proper air conduits or pipes were provided to bring down an ample supply of *fresh* air from above, distributed at the floors or decks of every cabin and sleeping berth; and from the ceilings of the respective cabins, or vacant spaces between the beams, branch-pipes conveyed the vitiated air to one large trunk, which might be made with proper precaution to communicate with the chimney

* See "On conducting air by forced ventilation," &c. by the Marquis of Chabannes, London, 1818, and remarks on ditto, by J. Arnot.

the engine-boilers, or pass through a steam-chest, or encircle the steam-pipe,—a constant renewal of the entire air between decks would go on. The current might be checked and regulated by valves, working in a very simple manner, before entering the chimney. In winter, the comfort of the passengers might be materially increased were the air warmed before being discharged into the cabins,—cold offensive currents would thus be avoided. It is singular that the same idea had occurred to Buchanan, when he wrote, in 1810, on heating by steam. "It is worthy of the consideration," he says, "of those acquainted with nautical affairs, how far it is applicable to ships, particularly to men-of-war." There is generally in steamers very little spare steam; but a very small portion would be requisite to warm the cabins; or hot water could be even more effectively employed. In this case, the external air, before entering the cabins, might pass through boxes or cases filled with iron or copper pipes heated with hot water or spare steam from the boiler; or the air itself might pass through the interstices of iron cases similarly heated, and then enter into the cabins through numerous small apertures. Thus warmth and the supply of fresh air could in winter be combined.

I have alluded to the wind-fan having been made use of to supply fresh air or cool the furnace-room, the power being taken from the paddle-shaft. The fan admits of easy extension to the general ventilation of the steamer.*

In some recent instances, ventilators, on the principle of the Archimedian screw, have been tried for this purpose. Ventilators or revolving fans, on this plan which I have seen, are stated to have long been in use in factories. In an extensive flax-mill in Yorkshire, a very powerful fan on the principle of the screw propeller, driven by steam power, has been most successfully adopted, and the plan there in use for imparting mois-

* In September 1842, a patent was granted to Robert Hazard, of Clifton, near Bristol, for improvements in ventilating carriages and cabins of steam-boats. He proposed to remove the vitiated air within a carriage by means of a fanner fixed at a convenient place, and set in motion by the revolution of the wheel, or by other motive power. He does not specify how he intended to apply his fans to cabins, but as regards the application to the latter, there is little scope for novelty.—*Rep. Soc. of Arts,* May 1843.

ture to the air, is highly deserving of general application, and ought never to be overlooked in ventilating arrangements.

The importance of keeping the furnace-room cool is of great consequence, especially in warm climates, as the heat is injurious to the health,—the cold air rushing to the furnace, falls like lead on the heads of the stokers. To remedy the over-heating, though it cannot prevent the draught, a plan, proposed by Mr Holdsworth of Dartmouth, has recently been tried in the Victoria and Albert, Royal Steam Packet, of having the bulkheads of two plates of sheet iron, and a stream of cold water kept constantly flowing between.

Another plan of ventilating, suitable for steam-ships, which the small space it occupies recommends, is the very ingenious method adopted by Mr Oldham at the Bank of England, of forcing in fresh air, by an air-condensing pump, through the interstices of iron cases heated by steam, the power being taken from the steam-engine, as described in the Civil Engineer's Journal, March 1839. This plan gives both fresh air and a modification of its temperature.

Mr Taylor's plan, described in the Transactions of the Society of Arts, London, 1810, of pumping out impure air from mines by an air-exhausting cylinder, likewise admits of application to steam-vessels. Mr Taylor's engine discharged more than 200 gallons of air per minute.* The idea of a motive power to work ventilators is of very old standing. A plan is given in the Phil. Trans. 1758, of using the fire-engines at mines for this purpose.† Various other suggestions might be made to apply ventilation to steam-ships—even the suction from the motion of the paddle-wheels might be made subservient to this purpose; but it is superfluous to say more on what admits of so many ways of attainment.

It is unquestionable, that the same share of attention has

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* Mr Taylor's plan consisted in attaching a pump of simple construction to a small fall of water of about 12 feet. Steam-power could be substituted for water.

† It was first proposed by Erasmus King, to have ventilators worked by the fire-engines of mines; and Mr Fitzgerald, in 1758 (see Phil. Trans.), suggested an improved method of doing so. I have alluded to the similarity of mine-ventilation with that of ships; thus, by having a series of flexible pipes connected with a wind-engine, or an air-pump attached to the steam-engine, immense supplies of air might be driven in, or drawn out, where required.

not been paid to the advancement of ventilation, as to other branches of the arts and sciences. A wide field is therefore open for improvements. But to be successful, these things must not be left to chance ; they must form part of the construction of ships and steamers, and the naval architect and ventilator, as has been well observed by Dr Reid, " must work together."

While undue currents of cold air must be avoided—which are often troublesome, and must be injurious—ventilation, to be perfect, should be so arranged as to admit of being increased or diminished, according to the number of inmates. In our climate, in steamers, whether in coasting or long voyages, it would be of importance to have the power of raising the temperature of fresh air before admission to cabins ; merely giving it, however, that slight degree of warmth that will not be injurious to its hygrometric condition. This would insure a larger volume of air being admitted. The plan is so easily attainable, that it might lead to the dispensing, in a great measure, with close arid stoves, so detrimental to the health in confined situations. It is remarked, that even Celsus, amongst the ancients, recommended large rooms for the sick, or a fire in the chimney to draw off bad air. Where fire-heat is made use of in cabins, it ought, if possible, to be in open fire grates. An ample exposition of the injurious effects of close stoves will be found in the Architectural Magazine, May 1838, p. 231, by Julius Jeffreys, Esq.

It is important for nautical men to know the great value of fire-heat as a purifier of the air of lower decks and close places, in the estimation of many of the most experienced navigators and naval commanders : Cooke and Nelson may be named.* How conspicuously the importance of sanatory regulations were illustrated in the remarkable voyage in 1773-75, of Captain Cook, who, during three years, out of 118 persons on board, lost four, and of these only one by sickness.† We have likewise several similar examples in the arctic and

* An excellent paper on this subject, written nearly a century back, will be found in the Gent. Magazine, on the method of Preserving the Health of Seamen in long cruizes and voyages, where ventilation and fumigation are strongly enforced. Vol. xvii., 1747–8.

† Naval History 1773, p. 349.

antarctic voyages. See Expeditions of 1821, 1824, and later ones, where the advantages of warmth, combined with ventilation, are clearly shewn.

A very simple contrivance might be found useful for purifying lower decks when unoccupied: a grate, formed like a circular basket, hung in gimbals, which, like a pendulum, has its point of rest in the perpendicular.

The value of lime and vinegar washings and fumigations in destroying the bad effects of impure air, did not escape the older philosophers.* The knowledge of these facts was of vast utility in the days of Howard. Professor Daniel, and other chemists, have, in these times, recommended the use of chlorine gas and chloride of lime for a similar purpose. In combination with ventilating arrangements in ships, the value of such antidotes—especially where sickness prevails—should not be overlooked.

If we turn to the graphic pages of Smollett, we may at once perceive, by contrasting his description of a man-of-war with the inspection of one now-a-days, what great improvements have been made. But still, much is yet to be done in ship-ventilation generally throughout the world. The air, being invisible, deceives many a one, leading us to consider it pure, while it may be stagnant and corrupt; hence the necessity of impressing the admission of fresh air at all times, as we do light; and the absurd idea cannot be too soon exploded, of people enclosing themselves in an air-tight box or cabin. With improved means of ventilation of ships and steamers, the energies of all on board will be promoted. By in-

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* Dr Stephen Hales made many experiments recorded in his Statical Essays, London, 1731, vol. i, with a view to clear the air from noxious vapours. He found nothing so efficacious as a solution of potash. He says, page 207, "Sal Tartar should be the best preservation against noxious vapours, as being a strong imbiber of sulphurous acid and watery vapour, as is also sea-salt." A solution of caustic alkali will take up fixed air as fast as it is produced.

The rapid absorption of ammonia by water, and the avidity of fresh lime for carbonic acid, point out the utility of water, with lime recently dissolved in it, for neutralizing the effects of impure air, either by the use of frequent fresh lime-washings, or exposing, in shallow vessels, frequently stirred, solutions of fresh lime. In factories, the sulphate of lime or gypsum is in general use for the absorption of ammonia, or removing the smell of the soil-pipes.

haling pure air during night as well as day, in cold or warm climates, increased longevity will be attained, and, at all events, the general comfort improved; and as Britain has outstripped most nations in the application of steam-power to useful purposes, why should she not take the lead in cultivating those arts which the physiologist has proved to be essential to the advancement of the physical condition of mankind?

Contributions towards Establishing the General Character of the Fossil Plants of the genus Sigillaria. By WILLIAM KING, Esq., Curator of the Museum of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne. With two Plates. (Communicated by the Author.)

(Continued from page 21.)

A consideration of the ribs and furrows of *Sigillaria* will now engage our attention. From these characters, Lindley and Hutton appear to think, that the plants of this genus are allied to *Cactidæ* and *Euphorbidæ*. To these families, it is said, they seem to approach, particularly in their soft texture, in their deeply channelled stems, and especially in their scars being placed in perpendicular rows between the furrows.* Brongniart also refers to the resemblance between the external characters of the latter plants and those of *Sigillaria*. From what is stated when speaking of the internal structure of certain Cactuses, it may be inferred, that he supposes their ribs and furrows to be, in a measure, due to the same characters observable on the external surface of their ligneous cylinder.† I mention this to ward off the notion that the ribs and furrows of *Sigillaria* arise from the same cause, judging from *S. elegans*, because in this species the ligneous cylinder has no appearance of being externally fluted.

Brongniart, in his general observations on *Sigillaria*, makes a slight allusion to the resemblance between the longitudinal ribs of *S. Sillimani* and *S. contracta*, and those of some arborescent ferns.‡ Having had no opportunity of studying the latter, I have been compelled to examine more accessible plants, with the view of ascertaining if the like character occurs on any of their stems. Hitherto I have met with the most success among the Conifers. The surface of young shoots of the larch (*Larix Europœa*), and the spruce (*Abies excelsa*), are irregularly ribbed and

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* "Fossil Flora," vol. i. p. 155.

† "Archives du Musuem d'Histoire Naturelle," tome i. pp. 442, 443. 1839.

‡ "Histoire des Vegetaux Fossiles," tome i. p. 403.

furrowed, more or less in a longitudinal direction—a peculiarity which is produced by the elongation and arrangement of the leaf-bases. This is particularly obvious in a specimen of the larch, of which a sketch a little above the natural size is given in fig. 1 (Pl. IV.) In this specimen, the leaf-bases *a*, *a* are connected with each other by threads of cuticle (*b*); by this means a number of such connected leaf-bases forms, as it were, a linear series, which, when traced for any distance on the surface of the stem, will be found to coil round its axis in spirals, whose volutions are very much apart. The direction of the spirals thus traceable is, in this instance, to the left, that is, when looking upon them; but this does not appear to be constant, for in some specimens of the same plant at present before me, the spirals run in the opposite direction; irregularities of growth obviously induce this difference. From an examination of a number of specimens, I do not, however, hesitate to say, that the general direction of these spirals is to the left.*

I am not aware that the manner in which I have shewn the leaf-bases to be connected with each other has been mentioned by botanists—even by those who have written on the spiral arrangement of the leaves, and other appendages of the stem, or other axes. Can it be, that in each linear series or spiral, we have the course which the constituent vessels of a single system of leaves fall into, as they are developed, and as they strike off from the medullary sheath?

Were it not that the threads of cuticle connect the leaf-bases together, the species of larch which is figured would present a striking analogy to *Sigillaria*, as respects the ribs and furrows. Another Coniferous species, hereafter to be noticed, offers a still closer agreement.

As bearing on the subject under consideration, it may be mentioned, that, in *Lepidodendron*, the lozenges or leaf-bases, which give such a remarkable appearance to this fossil, are resolvable into spirals, whose volutions may occasionally be traced by the same means as I have pointed out in the case of the larch. In the outline given in figure 2 (Pl. IV.), two sets of spirals, corresponding to the leaf-bases *a*, *a*, and *b*, *b* may be traced, either of which might be taken for the true one, because of the absence of connecting threads of cuticle.† In the other specimen (Fig. 3, Pl. V.), however, we have not this difficulty to contend against, for the

* The volution of such spirals on the stems of *Coniferæ*, appears to follow the same direction as that of the spiral fibres in vascular tissue. According to Mohl, these fibres are in general wound to the right, that is, their volutions are such, that, to an observer placed in the axis of the cylinder around which the fibres rise, the spiral mounts from left to right.— *Vide* a paper by Mohl on the structure of annular vessels, translated in the "Annals of Natural History," vol. viii.

† This specimen exhibits, in a very decided manner, the axillary buds mentioned in a former part of this paper. I have given an enlarged outline of a lozenge in figure 2 x, in which the prominence *a* represents the axillary bud: the other parts of this lozenge I hope to be able to explain in a paper which I intend to publish on *Lepidodendron*.

connecting threads of cuticle are remarkably distinct: we are thus enabled to trace the spiral of this specimen in the direction of the leaf-bases *a, a.* Guided by this circumstance, and notwithstanding the obvious connection existing between the leaf-bases *b, b,* in the specimen represented in figure 2, I am inclined to think that the spiral of this *Lepidodendron* coils in the direction of the leaf-bases *a, a.* I am strongly supported in this view, from having observed on several specimens, that the lozenge, corresponding to the one marked *c* of the adjoining spiral on the left, is below that marked *d* of the spiral on the right, and not the converse (vide *e, f*), as would be the case were the spiral to take in the lozenges *b, b.* This will be understood by a reference to the specimen represented in figure 4 (Pl. IV.), on which, though there are no threads of cuticle visible, it is quite clear the spiral cannot have run in any other direction than in that of the lozenges *a, a* x, because the apex of that which is marked *a,* is above the inferior part of those marked *b* and *c.* We are thus enabled to see that the intervening lozenges *b* and *c,* are situated relatively to each other as just observed. By attending to this circumstance, I have been enabled to trace the direction of the spiral in specimens which offered no other means of deciding this point.* In figure 3, the lozenges (*b, c, d,* and *e*) of four spirals come in between the lozenges (*a, a*) belonging to the spiral which unites those last particularised: this is certainly an extreme case, and is evidently due to an unusual longitudinal development; this is proved by what is exhibited in the specimen given in figure 2, which has not increased so much in this direction, and which exhibits two lozenges *g* and *h,* in addition to the ordinary number, approximating to the relative position of those marked *d* and *e* in figure 3.

Having shewn a resemblance between the irregular ribs on some recent *Coniferæ,* and the more decided instances of this character so highly characteristic of *Sigillaria,* I will, in the next place, endeavour to ascertain how far the cause of those on the former can account for the production of their analogues on the latter. With this view, a representation is given in figure 5 (Pl. IV.), twice their natural size, of the leaf-bases of that beautiful Nepaul pine—*Abies Webbiana,* a fine individual of which is now growing on the grounds of Sir Charles Monck of Belsay. By following the leaf-bases *a, a,* through the connecting threads of cuticle *b, b,* it will be seen that the spiral runs to the left, and that its volutions are extremely remote from each other. But the most important feature on this specimen, is the very decided way in which it is ribbed, so much so, as to appear like a miniature *Sigillaria.* On the larch, the leaf-bases are connected with each other by means of threads of cuticle, which in

* According to this view, scales 1, 14, 27, 40, and 53, in Professor Henslow's figure of the spruce cone, follow the course of what may be termed the *true spiral.* —*Vide* "Descriptive and Physiological Botany," fig. 137, p. 127 in Lardner's Cyclopedia.

general are very distinct, and the lower part of the leaf-base of one spiral is rarely carried over more than the right half of the superior margin of the leaf-base belonging to the spiral immediately to the left; but, in the example under consideration, *the inferior part of one leaf-base is often nearly carried over the whole of the superior margin of another, and both have become so closely united, that the lines of junction are, in many instances, nearly obliterated.* As regards the threads of cuticle, they are, in many places, *confluent with the leaf-bases on the left, and their connection with these parts, that is, when they unite two different ribs, is often broken in their passage across the furrows.* These circumstances, it is evident, have given rise to the decided form which the ribs of this plant have assumed. Further, they throw considerable light, in my opinion, on the cause of the ribbed appearance of *Sigillaria;* for if the leaves of this fossil are resolvable into spirals similar to those which have been traced on *Coniferæ* and *Lepidodendra,* then nothing more is necessary for the production of its ribs, than the completion of what is only partially effected in *Abies Webbiana.*

Now, let us, in the first place, endeavour to ascertain if such a spiral arrangement of the leaves has obtained on the stems of *Sigillaria.* On the specimen outlined in figure 6 (Pl. V.), the leaf-scars, although arranged in longitudinal series, nevertheless agree with the lozenges of *Lepidodendron,* and the leaf-bases of *Coniferæ,* in following a spiral course. Judging of the means by which the direction of the spiral has been traced in these plants, the inquiry suggests itself, as to whether any specimens of *Sigillaria* shew appearances of threads of cuticle. As yet, I have not observed any positive evidence of their occurrence on any of the examples which have come under my notice. In the absence of this kind of evidence, our next way of proceeding will be, to see if the leaf-scars of one rib may be connected with those of another, so as to harmonize with the results that have already been obtained as regards other plants. In consequence of the leaf-base *c x,* being below the leaf-scar *b,* it is manifestly impossible that they can be a continuation of each other, or pertain to one series; they are precisely in the case of the lozenges *a* and *b* on the specimen of *Lepidodendron,* represented in figure 4, which lozenges, it will be perceived, were shewn to belong to two distinct spirals; but there is no difficulty in the way of the leaf-bases lettered *b x,* being consecutive to each other. By assuming that the latter are in the direction of a spiral, it will be seen that we have exactly the counterpart of what prevails in *Lepidodendron,* when the stems of this fossil have not undergone any unusual development in the direction of their axis, viz., two leaf-bases (*c x* and *a x*) of two spirals intervene those (*b x* and *b x*) which belong to the spiral, whose course has been assumed, and, confining ourselves to the former, the left one is lower than that on the right. Further, let us, in the second place, endeavour to ascertain how far the circumstances which have produced the ribs of *Abies Webbiana,* have contributed to form the like character on *Sigillaria.* Referring to the figure of the Nepaul pine, it will be seen, in consequence of the junction of the threads of cuticle with the leaf-bases, the ribs (1, 2, and 3) have been increased in width,—a circumstance which throws the leaf-scars *a x, a x* over to the left side. An examination of the figure of *Sigillaria* brings to view the same character, that is, the leaf-scars, instead of running up the median line of the ribs, are placed on the left side. This does not appear to prevail to any extent: nevertheless, I have observed it on other specimens, and Mr Hutton possesses one which shews it very decidedly. Between this last and *Abies Webbiana*, the similarity in this respect is such, as to prevent the adoption of any other conclusion than that which refers this character on both plants to one and the same cause.

It has already been remarked, that I have not succeeded in tracing any threads of cuticle on *Sigillaria*. It is necessary to mention, however, that, on the specimen which is figured, there are appearances of commissural lines between some of the leaf-scars and the immediate superimposed leaf-bases; these are represented running obliquely from the apex of some of the leaf-scars into the furrows on the left. Whether these lines are such as they appear, I am not prepared to answer in the affirmative. Their direction and position are certainly very much in favour of this being the case, as will be seen by referring to the restoration (in dotted lines) which has been attempted of the leaf-bases and threads of cuticle: the latter, it will be perceived, conform completely with the direction and position of the supposed commissural lines. If we compare the leaf-bases and threads of cuticle thus restored with the corresponding parts of *Abies Webbiana*, their resemblance to each other will appear exceedingly striking.

Thus the circumstances just mentioned connected with *Sigillaria*, go far towards shewing that its leaves are resolvable into spirals, which agree with those of *Coniferæ* and *Lepidodendra*. Associating this with the way in which it has been shewn that the rude ribs of some Conifers have been formed, we are necessarily led to conclude that the same but more regular character of *Sigillaria* has been similarly produced,—the difference as regards the latter parts in the fossil being due to the complete confluence of all the leaf-bases and other parts which belong to a rib, or, the contrary, as generally obtains in *Coniferæ*.* But, lest it should be maintained that the absence of threads of cuticle on *Sigillaria*, and the rare occurrence of these parts uniting with the leaf-bases on recent *Coniferæ*, render this conclusion inadmissible as far as the former plant is concerned; it may be stated that the solitary case which has been cited, of threads of cuticle occurring on *Lepidodendron*, opposed as it is by hundreds of specimens in which nothing of the kind is to be found, nevertheless proves that the threads of cuticle have all but universally

* The remarkable perpendicularity of the ribs of *Sigillaria*, according to this view, will have resulted from the leaves of one spiral being completely superimposed to those of the spiral which coils immediately beneath.

grown into the lozenges of this genus. Considering this, and associating it with what has been afforded by the larch and the Nepaul pine, it appears to me, that the view which has been advanced is supported by the strongest analogical evidences, and, of course, is so far placed beyond being a mere hypothesis.

Brongniart, in his "Vegetaux Fossiles," says, the width of the ribs of *Sigillaria* generally remains the same, not only on various parts of the same stem, but on the various individuals of the same species.* The three North Biddick specimens, however, strongly oppose the general application of this view, inasmuch as their ribs are clearly seen decreasing in width in the ascending direction, so much so, that they are three inches wide at the base of one of the specimens, and under an inch at its apex. This great disparity is confined to the stem; but, it must not be forgot, that there are evidences of these specimens having been furnished with branches. Now, as a considerable decrease in the width of the ribs obtains on the stem, it seems but reasonable to conclude, that a still further diminution would be continued into the branches,—that, in short, there would be exhibited on the latter all the characters of the so-called genus *Favularia*. For my part, I have little or no doubt that this was the case, and I am strongly persuaded in its favour, from having procured from a small space in one of the pits of this neighbourhood, a great number of specimens uniting *Sigillaria reniformis* with *Favularia tessellata* (?). But, if further evidence be required to destroy the general bearing of Brongniart's view, it is undoubtedly afforded by the highly interesting specimen represented in Plate 15 of Artis' "Antediluvian Phytology."

The specimens just referred to as having been obtained from this district, require some further notice, on account of their uniting such dissimilar forms as those which have been named. If this be the case, it follows, as a matter of course, that a number of species of *Sigillaria* will fall to the ground. In saying that these specimens unite *S. reniformis* and *Favularia tessellata* (?), it is to be understood, that I am fully persuaded such specimens belong to the same species, and even to the same individuals. This is strongly corroborated by the following fact:—On all the specimens, the size and form of the leaf-scars are much about the same; but there is a remarkable variation in the width of the ribs,—a variation which has been produced, not by the whole rib being thus altered, but merely those parts which are lateral to the leaf-scars. How are we to account for this fact? Simply, I would suggest, on the supposition that the attachment of the leaves to the *median line* of the ribs has checked the horizontal extension of this part, while the furrows and the lateral parts of the ribs were allowed to augment in this direction, in proportion to the development of the stem. Guided by this supposition,

* "Vegetaux Fossiles," Tome 1, p. 395. Brongniart certainly makes exceptions of dichotomous specimens; but the North Biddick fossils have undoubtedly been branched. The exceptions, therefore, cease to be such.

the ribs of the specimen of *Sigillaria lævigata,* figured in Plate 143 of the "Vegetaux Fossiles," may be considered to have been at one time no wider than the leaf-scars or bases, which form so prominent a feature in the figure, and at the same time display so obviously all that is required to confirm the above supposition. The same may be said of *S. reniformis* (Plate 142), *S. Polleriana* (Pl. 165 ?), and a number of other wide-ribbed examples which Brongniart has figured. Artis' *Euphorbites vulgaris,* shews not only the lateral parts of the ribs and the furrows to have increased considerably in the horizontal direction, but also the median line of the ribs, as is manifest by the vascular scars being very wide apart at the base of the stem. This increase of the median line of the ribs, it may be supposed, has taken place subsequently to the falling off of the leaves.

This appears a fitting moment to revert to the previously mentioned remarkable character, which is displayed on one of the North Biddick specimens. When describing this fossil, it will be recollected that mention was made of the median part of the ribs on a certain place, being below the level of the furrows. From what has just been urged in explanation of the widening of the ribs, the cause of this peculiarity, I think, will readily suggest itself; for, if the *lateral parts* of a rib offered little or no resistance to the horizontal development of the stem, compared with what would be offered by the *median part,*—owing to the attachment of the leaves, it seems a necessary consequence that the former, as well as the furrows, would occasionally grow out beyond the plane of the latter. The more general fact of the furrows becoming obsolete on the base of the stems of *Sigillaria,* is evidently attributable to the same cause.

The extreme probability of the fossils named *Favularia,*—at least some of them, constituting neither a genus, a section of a genus, nor even a distinct species, but simply the terminal portion of an individual *Sigillaria,* induces me to examine that section which Brongniart has termed *Clathraria,* and known by being reticulated as the Lepidodendrons. Of this section I have merely to suggest, that some of the species which it includes, are probably nothing more than portions of the stems of *Sigillaria,* belonging to those parts which have been subject to some irregularity of growth, arising from the setting on of the branches or otherwise. No one who has paid any attention to those species of *Conifera,* whose stems are fluted as it were, can have failed in observing, that wherever an irregularity of growth of the kind just alluded to has taken place, their surface becomes more or less reticulated. It is probable, however, that some of the specimens which are figured in the "Vegetaux Fossiles," may belong to plants *completely* so characterized; in this case, they would appear to constitute a more cycadoidal group than the true *Sigillariæ.*

Considering the great number of leaf-scars observable on the stems of *Sigillaria*, it cannot but excite our surprise that, as yet, we have no positive evidence as to the kind of foliage peculiar to this genus. When Brongniart, from external characters alone, maintained that *Sigillaria* was the ancient representative of existing arborescent Ferns, there was no difficulty in supposing that some of the numerous Fern leaves with which the coal-formation abounds, constituted its foliage; but since he has discovered that the internal structure of this plant appoximates it to the Cycadeous Gymnosperms, we have in this discovery an insuperable difficulty in the way of such a supposition, at least, if we are to consider it as utterly impossible that there could exist, during any previous organic period, a tree having leaves resembling those of a Fern, and a stem combining the structure of the latter with that of a Cycas.

The following observations, I trust, will show how far we are supported in the view that a tree of this kind vegetated under the form of *Sigillaria*.

In following up the inquiry now entered upon, it will be necessary, in the first instance, to take a brief survey of the various leaves which have been found in deposits of the carboniferous epoch, in order to ascertain which kind possesses the strongest claims to be considered as having belonged to *Sigillaria*.

Passing over such remains as *Lepidophyllum, Flabellaria, Annularia, Sphenophyllum, Nœggerathia,* and a few others, from appearing to have but a very remote connection with *Sigillaria,* or which are well known to belong to other plants, we will at once pass on to the more likely forms, as *Sphenopteris, Pecopteris, Neuropteris, Odontopteris, Schizopteris,* and *Cyclopteris.* Even this list may be considerably reduced, perhaps, to the first three genera, on account of the remainder, from their scarcity, not appearing to have belonged to stems possessing such irrefragable evidences, as to the profusion of their leaves, as the fossil under consideration. Taking this for granted, our brief survey will consequently be confined to the genera *Sphenopteris, Pecopteris,* and *Neuropteris.*

Of the first genus, it is said that about twenty-eight species occur in the coal-formation. It is generally considered to be a true Fern, although it must be confessed there are some points connected with two or more species, rather in favour of a contrary opinion. As stated in the "Vegetaux Fossiles," the recent forms which resemble *Sphenopteris* are *Gymnogramma, Asplenium, Darea, Cheilanthes, Adiantum, Lindsea, Woodsia, Dicksonia, Davallia, Trichomanes, Anemia, Hymenophyllum,* and *Botrychium.*

If it is the case that some species of *Sphenopteris* (e. g. *S. crithmifolia*) are not Ferns,—are we justified in supposing that they have belonged to *Sigillaria?* I think not; for, instead of standing in this relation, these doubtful remains appear to have belonged to an herbaceous plant. This suspicion arises from having observed some specimens appearing as if roots were attached to them. I cannot speak positively on this point;

but the appearances I have observed are such as to prevent me thinking otherwise for the present.

With respect to the other *Sphenopterides,* especially those which Gœppart has included in his genera *Steffensia, Cheilanthites, Hymenophyllites,* and *Trichomanites,* there is little doubt of their being Ferns. Therefore, on this account alone, it would appear they cannot have belonged to a plant, whose internal structure was so abnormal as *Sigillaria,* when viewed with reference to its vascular cryptogamic affinities. Another reason may be urged against *Sphenopteris* being the foliage of *Sigillaria*; it is founded on the general scarceness of individual specimens of the former over extensive areas, contrasted with the abundance of the latter when considered in the same light.

The next genus, *Pecopteris,* is stated to be the most abundant fossil of the coal-formation, no fewer than sixty species having been described: individuals are to be found in the same proportion. Brongniart says, that this group approaches to the usual structure of living Ferns, and it comprises the greatest number of species so intimately allied to kinds now existing, as to render their identity with the latter a matter beyond dispute.* This statement will be sufficient to prevent us looking amongst the various *Pecopterides* for the object of our present researches, especially if any other fossil will afford us a character less related to the existing *Filicidæ.*

This appears to be the case with the next and allied genus *Neuropteris,* which, as regards "fructification, the form and structure of the leaves, seems to differ completely from all living Ferns."†

The genera *Pecopteris* and *Neuropteris,* as they stand at present, evidently comprise species which may serve as the types of other genera: further, it is a debateable point, whether many of the so-called *Pecopterides* may not belong to the genus *Neuropteris*: and, on the other hand, it is disputable whether some which have been described as *Neuropterides* may not belong to *Pecopteris.* The following species seem to be in this predicament, *Pecopteris sinuata, P. obliqua, P. Deforanci, P. nervosa, Neuropteris elegans, N. lanceolata* (Steininger), and several others.

Twenty-four species of *Neuropteris* have been enumerated, as belonging to deposits of the carboniferous epoch. In Felling Pit, where we find *Sigillaria,* this genus and *Pecopteris* are the commonest fossils.

We have thus hastily glanced at three kinds of foliaceous remains, whose connexion with *Sigillaria* has the most probability in their favour. But of this number, from what has been said of *Sphenopteris,* two alone, viz., *Pecopteris* and *Neuropteris,* appear to merit our attention in this respect; and, though we have not as yet been able to decide which of the latter constituted the foliage of *Sigillaria,* we have nevertheless, it will shortly be seen, made some progress in our enquiry.

The internal structure of *Sigillaria elegans* indicates clearly that this

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* "Vegetaux Fossiles," tome i., p. 267. † Ibid. p. 227.

fossil not only approaches to the Cycadeous Gymnosperms in some respects, but in others it manifests a leaning to the Vascular Cryptogams. I am even inclined to grant it a closer resemblance to the latter, than is admitted by Brongniart. The consideration of this point, however, must be waived until it is educed in its proper place; and, for the present, I will content myself with the admission that the genus *Sigillaria* comes in between the two groups just named. I presume this is not going beyond what Brongniart will agree to. This admission granted, it follows, that the foliage of this fossil would neither be truly Cycadeous, nor truly Vascular Cryptogamic, but rather of a character departing from the Ferns, and approaching the Cycases.

Now, whether are we to consider *Pecopteris or Neuropteris* in this light? What has already been stated with reference to these fossils, will have somewhat biassed the mind in favour of an answer to this question; but, in order that we may be further supported, let us examine some other fossils, which, from their affinity to the last-mentioned genus, require our next consideration. According to Brongniart, the genus *Neuropteris* is characterized by having the leaflets adhering to the rachis only by the median part of their base, and their veins, which are dichotomous, diverging from this part.*

It is evident, however, if we are to consider such forms as *Neuropteris conferta, N. decurrens, N. elegans*, and some others, as belonging to this genus, the above *diagnosis* will require some modification. Sternberg has already done this, by placing the foregoing in his third section, which he describes as having the leaflets attached by their entire base, and sometimes decurrent.†

This section appears to form the link which connects *Neuropteris* with *Odontopteris*—the genus requiring our next consideration. A glance at *Neuropteris Villiersi* (Brogn.), *Odontopteris crenulata* (Brong.), and *O. Braardi* (Brong.), will shew how difficult it is to distinguish the one genus from the other.

We have now arrived at a group of fossils, which, to use the language of Brongniart, "is completely different from all living Ferns with which we are acquainted." Their leaflets are very thin, and adhere by the whole of their base to the rachis; they have no, or almost no, midrib; their veins are equal, simple, or forked, very fine, and most of them spring from the rachis. No known living Fern possesses leaflets and veins of this character.‡ Species of this genus are exceedingly rare; only seven or eight having been described. *Odontopteris Lindleyana* (Sternb.)§

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* "Végétaux Fossiles," tome i. p. 226, and "Prodrome d'une Histoire des Végétaux Fossiles," p. 52.

† Essai d'un Tableau Geognostico-Botanique de la Flore du Monde primitif, Par Comte G. Sternberg, 1835, p. 74.—In this section of *Neuropteris*, several so-called *Pecopterides* ought, I think, to be included.

‡ "Vegetaux Fossiles," tome i. p. 250.

§ *Odontopteris dubia* (Lindley and Hutton), *vide* "Fossil Flora," pl. 40.

is the only one figured in the "Fossil Flora," that has been found in England.* Lardin, Terrasson†, St Etienne, Manebach, and Saarbrücken, are the principal localities on the continent which yield the few species that have been described by Schlotheim, Brongniart, and others.

I cannot but anticipate, what will hereafter be sufficiently evident, that *Odontopteris* is leading us gradually from the Ferns to the Cycases, inasmuch as from the last genus we pass insensibly into that of *Otopteris*,‡ which merges completely into the Cycadeous type.

Hitherto, the genera which have passed under our notice are essentially characteristic of the carboniferous epoch; but the genus (*Otopteris*) which is now to engage our attention, not only does it approximate us to *Cycadidæ* by its characters, but by its position in the geological series we are brought up to a later age, in which there is positive evidence of this class of plants having existed.

Without attending to the doubtful forms of *Otopteris? dubia*, and *Otopteris? Dufresnoyi*,—a Triassic fossil,§ we may, by means of such species as *O. Beani*,|| *O. Bechei*, and *O. latifolia*, pass insensibly into *Otopteris pectiniformis*, which places us at once in immediate proximity to fossils which have been, by general consent, placed in the family *Cycadidæ*. As *Otopteris pectiniformis* may be said to osculate two great classes of the vegetable kingdom, I have represented in figure 7, Pl. V. the veining and form of its leaflets twice the natural size. Their base, at its superior side, possesses a very slight auricle, into which a few veins are seen to curve; below these, a set, although still having an upward tendency, runs more directly from the base to the termination, and the remainder, or the lowest veins, run out perfectly parallel with the inferior margin: occasionally the veins are divided. If we compare the veining thus represented with that of the leaflets of *Otopteris latifolia* as figured by Phillips, and by the authors of the "Fossil Flora," it will

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* Mr Morris, in his "Catalogue of British Fossils," mentions *Odontopteris Britannica* as occurring in Yorkshire.

† In this locality occurs the singular *Sigillaria Braardi*. It would be a most important point gained, in support of the view I am now endeavouring to establish, were it discovered, that this plant, which agrees with the Cycases, by its external characters, possessed *Odontopteris* for its foliage.

‡ The leaflets of *Odontopteris* being attached by their entire base, while those of *Otopteris* are constricted (e. g. *O. Beani*, &c.) at this part, would seem to be a serious objection to the one passing into the other; but the interesting coal-measure fossil figured in the "Fossil Flora" as *Otopteris? dubia*, whether we consider it a species of the last genus, or of *Odontopteris*, appears to break the force of this objection.

§ *Neuropteris Dufresnoyi* of Brongniart; *Vide* "Vegetaux Fossiles," pl. 74. fig. 4.

|| The veining and general form of the leaflets of this fossil agree so much with the same characters of *Otopteris latifolia*, as to prevent my considering it as belonging to the genus *Cyclopteris*, in which some are disposed to place it. Looking upon *O. latifolia* as the type of *Otopteris*, it would appear that in *O. Beani* the genus had assumed the form of *Cyclopteris*. For the specific names which are adopted in the text, of the genus *Otopteris*, *vide* Supplementary Note.

be seen, that, in the latter species, we have but a slight modification of this character, and that we are, to all appearance, among the Ferns:* on the other hand, if we compare it with the veining of the leaflets of *Palæozamia pecten*,† as represented twice the natural size in figure 8, Pl. V., it cannot be concealed, that, in the latter, we have a modification equally slight, and that we have before us a form essentially that of a Cycas.

Respecting the allied genera *Pterophyllum*, *Ctenis*, and some others, it is unnecessary to enter into any particulars, as their situation in the same division of the vegetable kingdom is not improbable.

Notwithstanding the view involved in the foregoing observations, I wish it to be understood, that it is not my decided opinion that the genus *Otopteris* is the form by means of which the passage from the Ferns to the Cycases was effected. The presence of *Pterophyllum* in rocks belonging to an earlier age than is usually assigned to the true *Otopterides*, would almost confirm the notion, that the Cycadeous type was directly related with the Odontopteroid Ferns. With the exception of its probably being bipinnate,‡ *Odontopteris obtusa* (Brong.), a coal-measure plant, does not appear to differ much from some species of *Pterophyllum*. Nor would I be in the least surprised, if true Cycadeous remains were hereafter to be found in rocks of the same age as those of the North of England coal-field; even in this case, the view that has been taken would not be disproved, unless it were shewn that remains of this kind occurred in formations older than our coal-formation, and deposited during a period in which the Ferns had not been called into existence. In the preceding arguments, I have not been so anxious to establish the true connecting link between two great classes of the vegetable kingdom, but rather to shew, as far as leaves may be taken as guides, that certain fossils of this kind possess characters which insensibly lead us from the Ferns to the Cycases. Having, I trust, established this point, there still remains for

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* Although Otopteris is here called a Fern, I am equally disposed to look upon it as a Cycas.

† *Vide* Supplementary Note.

‡ As regards the difference between Ferns and Cycases in the divisions of their leaves, I may state, that this difference does not appear to be of much importance. I can offer no example of a cycadeous leaf being bipinnate, nor do I know of any shewing a tendency to this character; nevertheless, suppose a leaf were discovered bipinnate, and Cycadeous in other respects, I think we ought to pause before placing it among the Ferns. The same may be urged with reference to finding a simple leaf displaying a Cycadeous character: and as an instance in point of the unwillingness (as it may be termed) of some Cycadeous leaves to leave the simple form, I may refer to *Pterophyllum minus*, which shews its terminal portion undivided to a considerable extent,—often a great irregularity in its divisions,—and occasionally the latter extending only half-way to the rachis. Mr Bean possesses a specimen of an entire leaf, about four inches in length, which he considers of this species: it has only one division on one side of the rachis, or mid-rib, and two on the other.

our consideration the principal question, as to whether *Pecopteris* or *Neuropteris* constituted the foliage of *Sigillaria.*

When detailing the observations of Brongniart on the internal structure of *Sigillaria elegans,* it was stated, that the ligneous cylinder, being formed of scalariform tissue, allied this fossil to the Vascular Cryptogams, and that the form of this cylinder, and the radial arrangement of its tissue, approximated it to the Cycadeous Gymnosperms; the affinities thus clearly indicated, it is obvious, ought to influence us in our researches as to what kind of leaves constituted the foliage of this genus. Accordingly, to arrive at a solution of this question, no other method appeared so legitimate as to ascertain, in the first place, which of the coal-measure leaves afforded the most evidence of their correlation with *Sigillaria;* this point gained, the next was to see if the remains thus fixed upon indicated a Cycadeous tendency.

This is precisely the method that has been adopted. Through various considerations, it was made out, in the first instance, that the weight of evidence was in favour of *Pecopteris* or *Neuropteris:* in the next place, and in order to ascertain which of these two forms displayed the strongest tendency to depart from the Ferns and run into the Cycases, two independent modes of argument were pursued; the one as founded on the essential characters of either *Pecopteris* or *Neuropteris,* graduating into those of a Cycadeous leaf; and the other, as founded on the subsequent appearance of *Cycadidæ,* to whichever of these genera might be decided upon. By these means, it was ascertained that *Neuropteris* possessed the strongest claims in its favour; in fact, there was traced an unbroken chain of affinity from this *Fern* to a decidedly *Cycadeous type,* and, at the same time, the required appearance of the latter, subsequently to the former, was made as conclusive as available data would admit. Thus the evidence is in favour of *Neuropteris* having constituted the foliage of *Sigillaria.*

Before concluding this part of our subject, it will be necessary to observe, that, although the previous reasoning has led to the above conclusion, it must not be understood that all the fossils usually considered *Neuropterides* were thus correlated. The genus, as at present constituted —even confining ourselves to those of the carboniferous epoch—may embrace species belonging to widely different plants: it is probable some may have belonged to Tree Ferns; others to herbaceous forms of the same class; while some may have belonged to plants which cannot be included with the latter,—for instance, *Sigillaria.* Thus, *Neuropteris obovata* (Sternb.), fragments of which occur in immense numbers in the coal-deposits of Mireschau, and Swina in Bohemia, are found associated with what appear to be specimens of a compound *rachis* of a gigantic frond, having, in some instances, a length of eight feet.* Drifting might certainly

* Sternberg, in "Flore du Monde Primitif," parts 5 and 6, p. 74. The divisions of this *rachis* are alternate, sometimes forming an acute angle with the axis, and occa-

bring together portions of plants having no connection with each other; and, on the contrary, the same means might widely separate different portions of the same plant; but, under the circumstances which these remains occur,—being unmixed with any other fossils, it seems extremely probable that they are portions of one and the same Fern. *Neuropteris conferta*, which belongs to that section which our reasoning would lead to the belief constitute the foliage of some species of *Sigillaria*, occurs in the Zechstein of Ottendorf in Silesia; but I have not heard of any portions of the latter fossil having been found associated with it. And *Neuropteris cordata*, Mr Binney, one of the secretaries of the Manchester Geological Society, informs me, is most abundant in Lancashire, where *Sigillaria* is not to be found. I mention these facts to shew, that, however strongly a certain view may appear to be supported by certain processes of reasoning, it is necessary, previously to forming any positive conclusion, to be in possession of evidences more to be depended upon. Let it be understood, that I am perfectly agreeable that this observation should be applied to the *conclusion* which has just been arrived at: this being allowed, it behoves us, then, to adopt some means by which it may be tested. Now, the most effective means with which I am at present acquainted, consists of the leaves which are usually found imbedded with *Sigillaria*, especially when the latter is *in situ*. When the North Biddick specimens came under my notice, it occurred to me, that the leaves which might be found so associated were probably those which had belonged to them. I was thus led to examine the rock in which they were imbedded; and although other foliaceous remains were observed, for example, *Asterophyllites*, yet the prevailing one was a form allied to *Neuropteris heterophylla*, but with larger leaflets than those of the specimen figured by Brongniart. A similar result has attended my examination of some large masses of shale, containing impressions of *Sigillaria*, which were sent to the Newcastle Museum from Derwent Vale Colliery on the Tyne: and in Felling Pit, where the roof and floor are in some places crowded for a considerable extent with *Sigillaria reniformis*, the leaves generally found are *Pecopteris* and a variety of forms of *Neuropteris*, the principal of which have been figured in the "Fossil Flora," under the names of *N. Loshi*, *N. Sorett*, *N. heterophylla*, *N. ingens*, *N. gigantea*, and *N. acuminata*. Hence, if some species of this genus really belonged to *Sigillaria*, it would appear a matter of some difficulty to say which stood in this relation. I am firmly persuaded, however, if we leave out of view *Neuropteris gigantea*, we will not be far from the truth, by including the remainder of the above in a single species, say *N. heterophylla*. So far, then, the Felling case confirms the

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sionally running out horizontally. The main *rachis* is somewhat semicircular, and, at the thickest part, is from two to three inches in thickness. Sternberg does not mention any specimens which may be considered Rhizomes occurring with this *rachis*; if nothing of the kind has been found, it would appear that *Neuropteris obovata* was an herbaceous Fern.

others: and the probability is, that that section of *Neuropteris* having the leaflets attached by the median part of their basal margin, pertained to *Sigillaria reniformis* and its allied species.

Mr Binney, who has paid especial attention to this subject, has been so kind as to inform me, that the leaves which he finds commonly associated with *Sigillaria* are *Neuropterides,* apparently the same as those which occur in Felling; along with these are also to be found *Pecopteris lonchitica* and *P. nervosa** (Lindley and Hutton); the latter is particularly abundant. It ought to have been mentioned, that *Pecopteris nervosa* occurs in Felling Pit. If the latter should eventually be proved to have belonged to *Sigillaria,* it would appear to follow that the truth was at direct variance with our conclusion. This variance, however, I have little doubt, is more apparent than real. When stating in a former part of these "Contributions," that several of the so-called *Pecopterides* ought to be included in Sternberg's third section of *Neuropteris,* that is, those which have the entire basal margin of the leaflets attached to the rachis, *Pecopteris nervosa* was one which I had in view, because, in its veining, it possesses a striking resemblance to *Neuropteris alpina,* which is figured by Sternberg.† In figure 9, Plate V, I have represented a portion of the Felling *Pecopteris nervosa,* by which it will be seen, that, although the leaflets are confluent with each other at the rachis, and therefore so far in agreement with the genus in which it has been placed—the absence of a distinct midrib,‡ and several of the veins springing from the rachis, utterly forbid us placing it in *Pecopteris.* In short, taking the whole of its characters into consideration, it appears to have a strong relationship to *Odontopteris*;§ but whether it be considered as belonging to this genus, or to *Neuropteris,* it is precisely the form to which the previous train of argument has conducted us. It is therefore possible, that the so-called *Pecopteris nervosa* may have belonged to some species of *Sigillaria.*

The description which Brongniart has given of *Sigillaria lepidodendrifolia,*‖ is clearly opposed to the conclusion at which we have arrived. This celebrated botanist, from certain appearances afforded by this species, is inclined to think that it was furnished with leaves, similar to those observable on *Lepidodendron,* and termed *Lepidophyllum.* I strongly suspect, however, that what have been taken for leaves in this specimen, are merely *ramenta,* similar to those found upon Ferns.** Two speci-

* Brongniart's species under this name appears to be different.

† "Flore du Monde Primitif," parts 5 and 6, tab. xxii. fig. 2.

‡ In Lindley and Hutton's figure of this species, the leaflets are represented with what might be considered a midrib; but the veining has not been properly attended to.

§ *Otopteris Braardi* (Brong. pl. 76,), which possesses a striking resemblance, in some points, to the fossil that is figured, has the leaflets partly confluent with each other at the base.

‖ "Vegetaux Fossiles," Tome i, pl. 161, pp. 426—428.

** In his general remarks on *Sigillaria,* Brongniart expresses himself in favour mens of *Sigillaria*, in the Newcastle Museum, strongly corroborate this supposition. On breaking one of those specimens, which is in the state of the division A, Fig. 3, Plate 1, and consequently a portion of the rock which surrounded the plant, there were clearly displayed a great many ramentum-like appendages, about an inch in length, and an eighth of an inch in width. They were visible only in those breaks corresponding to the furrows; and, owing to their number, the specimen was most easily broken in those parts. That they were not leaves, is abundantly proved by their size and situation: in this respect they agree with *ramenta*, which I have seen on the channellings of the larch, and some other conifers. The base of the leaf-like appendages which Brongniart observed on the specimens of *Sigillaria lepidodendrifolia*, already referred to, appears to have embraced the entire width of a rib; in this case, these appendages may have been broad *ramenta*, similar to what are found on Ferns: if they are really attached to the leaf-scars, as is stated, may they not be compressed petioles? their being slightly channelled would almost induce one to answer this question in the affirmative.

(*To be continued.*)

SUPPLEMENTARY NOTE.

Several of the foliaceous remains of the Jurassic period, although evidently belonging to *Cycadidæ*, are nevertheless possessed of characters which prevent them being placed in any genus of existing plants of this family. This is the case with *Pterophyllum, Ctenis,* and *Nilssonia.* Besides these, there are others which have generally been considered as Cycadeous; they have their leaflets more or less auriculated at the base, and are furnished with veins which diverge from the rachis, and which are often forked. Brongniart, in his "Prodrome" (1828), made a genus for them under the name of *Zamites.* The plant figured in the "Transactions of the Geological Society," 2d S., vol. i., pl. 7, fig. 3, appears to have been considered as typical of the genus. Previously to the publication of the "Prodrome," its author had named the same fossil *Filicites Bechei* in the "Annales des Sciences Naturelles," for 1825.

In 1834, Lindley and Hutton received the same species from Dr Buckland, and, being unacquainted with the circumstances just referred to, they published it under the name of *Otopteris obtusa*,* expressing at the same time their doubts as to its being a Fern. Subsequently, they included in the same genus a fossil which they had previously published under the name of *Cyclopteris Beanii,* and also another which they called *Otopteris acuminata,* but which had been previously named *Cycadites latifolius* by Phillips in his "Geology of the Yorkshire Coast."

Mr Morris, in the "Transactions of the Geological Society," 2d S.,

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of Fern leaves having been its foliage, in support of which he adduces the supposed remains of petioles, on a specimen from Anzin (Pl. 164, fig. 7). I presume the discovery of *S. lepidodendrifolia* was made subsequently to the general remarks being written.

* "Fossil Flora," Pl. 128.

vol. v., and in the "Annals of Natural History," for April 1841, proposed the genus *Ptilophyllum* for some fossil plants which Colonel Grant had brought from Cutch, which genus it was proposed should embrace the fossils named *Zamites*, by Brongniart. In the "Annals," Mr Morris observes, that "this name ought to be restricted to those fossils agreeing more closely with the recent genus." On this point I fully agree with him; but, as the same fossils which he refers to had been already named *Otopteris*, they cannot, on any account, be placed in *Ptilophyllum*. It remains to be decided, whether or not the Cutch fossils can be retained in this genus.

The genus *Otopteris*, it has already been remarked, includes *Cyclopteris Beanii* (L. and H.), *Zamites Bechei* (Br.), and *Cycadites latifolius* (Ph.) In the plate in which Professor Phillips represents the latter, there are figured other two fossils, which, if they are not varieties of the same species, evidently belong to the same genus. In addition to these, there are other two species which Mr Bean of Scarborough has named *Otopteris graphica*, and *O. lanceolata*; specimens of which this gentleman has obligingly added to my cabinet. *Otopteris graphica* appears at first sight to be a variety (with the leaflets more elongated) of *O. Bechei*; but, on a closer examination, it is seen that the veins of the former are finer and more numerous, judging from a specimen, apparently of the latter species, in the Newcastle Museum, and procured at Cloughton Wyke, on the Yorkshire coast. *Otopteris lanceolata* is evidently a more developed form of Sternberg's *Polypodiolithes pectiniformis* (*Zamia pectinata*, Brong.) I am supported in this view by several specimens at present before me, which it is utterly impossible to divide into two genera, or even into two species, so variable are the leaflets in their mode of attachment to the rachis, and in their form.

In his "Prodrome," Brongniart instituted the genus *Nilssonia* for some plants, which he describes as having the leaflets adhering to the rachis by their entire base, and traversed with parallel veins, some of which are more strongly marked than others. The fossil which Phillips figures under the name of *Cycadites pectinoides*,* appears to answer to this character. I speak, not only from the enlarged figure which is given of one of its leaflets, but from an examination of a specimen lent me in the kindest manner by Mr W. K. Loftus of Caius College, Cambridge, and which exhibits portions of two or three fronds, clearly of the same species, under forms altogether different. In one form, that is, where the leaflets are large, they are attached to the rachis by their entire base, which is even dilated a little beyond the superior and the inferior margin of the leaflet; in the first character this fossil agrees with *Nilssonia brevis*: where the leaflets are smaller, the superior part of the bases appears to be free, and slightly auriculated; in this form the specimen agrees with *Palæozamia pecten*† : and at the termination of the frond, the leaflets are constricted at their base, somewhat as in *Zamia lanceolata*‡. I do not say that *Cycadites pectinoides*, and the two last-mentioned fossils, are different parts of the frond of one species; but, notwithstanding their leaflet bases exhibiting such a marked difference in each, I am certainly disposed to look upon them as belonging to the same genus, from the circumstances of their agreeing in having the leaflets furnished with strong veins, rather wide apart, and with *apparently* smaller ones intervening the latter.

As respects the veins being of different sizes in *Nilssonia*—is it perfectly

* "Geology of the Yorkshire Coast," pl. 10, fig. 4. † Vide Plate V., fig. 8.
‡ "Fossil Flora," pl. 194.

made out that the smallest are true veins? This question suggests itself in consequence of what I have observed on a specimen of *Zamia gigas*.* In this fossil the veins are strong, and rather closely approximated, and the cellular tissue of the leaflet is arranged in linear series, corresponding with the direction of the veins. Hence it has occurred to me, that the so-called small veins of the Swedish *Nilssoniæ*, may be nothing more than the cells of parenchymatous tissue intervening the true veins, arranged in the same manner. How far I am correct in this view, I have no means at present of ascertaining; but from what I have observed in *Cycadites pectinoides*, *Zamia lanceolata*, and *Palæozamia pecten*, the appearance of small veins intervening the large kinds in those plants, may certainly be explained by the fact which has been noticed in *Zamia gigas*. Future observations will, I trust, enable us to decide as to whether these fossils might not be included in the genus *Nilssonia*; at any rate they cannot, I am of opinion, be placed in *Otopteris*. If they are not *Nilssonia*, it necessarily follows, from their being furnished with characters which are not to be found in species of the existing genus *Zamia* and its allies, that they will have to be placed in some other genus. As Endlicher evidently had fossils of this kind in view, (*e.g. Zamia taxina*, L. & H.) when he proposed his *Palæozamia*, I think it would be the most judicious plan to include them in this genus for the present.

The Cutch fossils, which Mr Morris considers as the type of his genus *Ptilophyllum*, appear to be as variable as *Cycadites pectinoides*. When young, the leaflets are, as Mr Morris describes, closely approximated; but, in a more advanced state, they are separated from each other nearly a quarter of an inch, and their basal margin becomes dilated on the inferior as well as the superior side of the leaflet. In the last state they offer a striking general resemblance to *Cycadites sulcicaulis* of Phillips (*Ctenis falcata*, Lind. and Hutt.). The Indian and Yorkshire fossils, however, differ in this—in the former the veins are simply forked, but in the latter they anastomose. This brief account is from specimens in my cabinet, brought from the Behar Hills, in India.

It is very much to be regretted, that Brongniart, in naming the Yorkshire Cycadeous plants in his "Prodrome," did not give, at the same time, such a description of each as would have enabled others to have identified his species: and it is equally to be regretted, that Professor Phillips, when he discarded, in his excellent treatise on Geology, in the "Encyclopædia Metropolitana," the names which he had previously published, of probably the same fossils, in his "Geology of the Yorkshire Coast," did not give us the synonyms of those which are adopted in the former work. It is true, Mr Morris has done this to a certain extent in his "Remarks upon the recent and fossil Cycadeæ," which appeared in the Annals of Natural History for April 1841; but the synonyms therein mentioned do not appear to agree with the catalogue of Jurassic plants which Professor Phillips has published in the treatise just referred to. I am, therefore, unable to give the synonyms of the Otopteroid and other allied fossils, which have been mentioned in this paper, so completely as I should have wished, in the following synopsis.

Synopsis of the species of the genus Otopteris.

Otopteris ? dubia, (Lindley and Hutton), "Fossil Flora," pl. 150.

——— ? Dufresnoyi, (*Neuropteris Dufresnoyi*), Brong., "Vegetaux Fossiles," pl. 74, fig. 4.

——— Beani, *Cyclopteris Beanii*, (Lindley and Hutton), "Fossil Flora," pl. 44.

* "Fossil Flora," Plate 165.

* Otopteris Bechei, "Transactions of the Geological Society." 2d S. vol. i. pl. 7, fig. 3.—*Filicites Bechei,* (Brong. 1825). —*Zamites Bechei* (Brong. 1828).—*Otopteris obtusa* (Lindley and Hutton), "Fossil Flora," pl. 128.

——— graphica, (Bean), M.S.

——— latifolia, *Cycadites latifolius,* (Phillips) "Geology of the Yorkshire Coast," plate 10, fig. 1—(*Cycadites gramineus,* and *C. lanceolatus* of this author are, I strongly suspect, the same species). — *Otopteris acuminata* (Lindley and Hutton), "Fossil Flora," pl. 132.—*Otopteris brevifolia* (Lindley and Hutton), "Fossil Flora," pl. 208.—*Odontopteris undulata* (Sternberg), "Flore du Monde primitif," parts 5 and 6, pl. 25, fig. 1, p. 77.

——— pectiniformis, *Polypodiolithes pectiniformis* (Sternberg), "Flore du Monde primitif," part 3, pl. 33, fig. 1, p. 44. —*Zamia pectinata,* (Brong.), "Prodrome," p. 23 and 94. *Filicites Scolopendroides,* (Lindley and Hutton, not Brongniart), "Fossil Flora," pl. 229. —*Otopteris lanceolata* (Bean), M.S.

Synonyms of Palæozamia pecten.

Cycadites pecten (Phillips), Geology of Yorkshire, pl. 7, fig. 22.—*? Cycadites pectinoides* (Phillips), pl. 10, fig. 4.—*Pterophyllum pecten* (Lindley and Hutton), "Fossil Flora," pl. 102.—*? Zamia taxina* (Lindley and Hutton), "Fossil Flora," pl. 175.

Observations on South American and African Guano. By JOHN DAVY, M.D., F.R.S. Lond. and Ed. Communicated by the Author.

Modern enterprise and intelligence are well displayed, and in a very characteristic manner, in the importation from such distant regions as the coast of Peru and the south-western coast of Africa, of the excrement of a bird, and in its varied application, founded on scientific principles, to the purposes of agriculture and of horticulture.

There is something very agreeable in the idea, that our merchants, instead of sending vessels out for cargoes of slaves to the latter coast, keeping up a cruel and barbarous traffic, are commissioning them to the same sea in quest of a valuable

* I have not seen specimens of *Otopteris Bechei* from Axminster. If it possesses finer veins than those which are displayed in the fossil at present before me, and procured from Cloughton Wyke, on the Yorkshire coast—a character which appears to obtain, judging from the figure in the "Transactions of the Geological Society,"—it is probable that the Yorkshire and the Axminster fossils may belong to two distinct species.

Fig. 1.

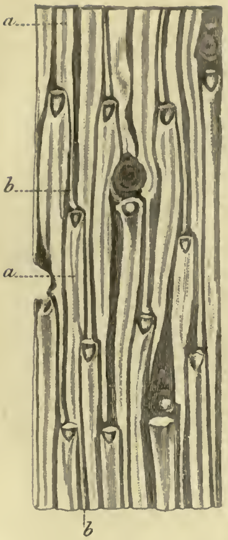

Fig. 5.

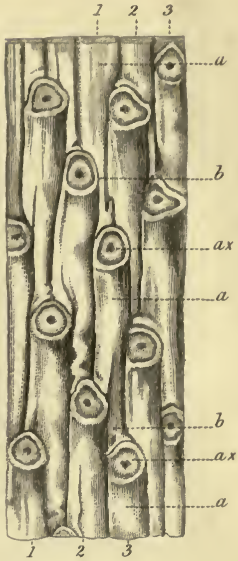

Fig. 2.

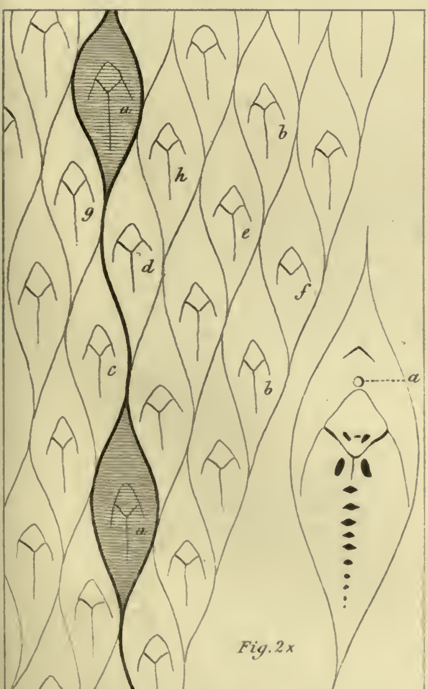

Fig. 4.

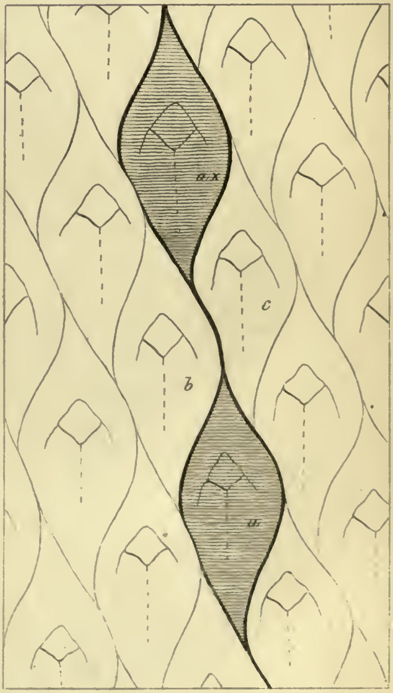

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Fig. 3.

Fig. 6.

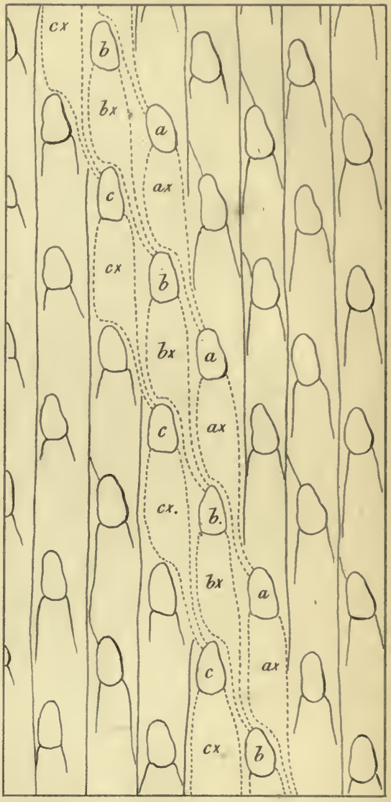

Fig. 7.

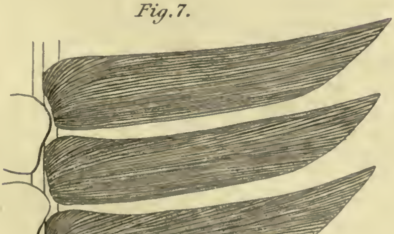

Fig. 8.

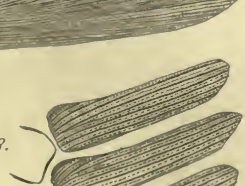

Fig. 9.

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manure, and a manure that appears to be admirably fitted to enrich the exhausted soil of the sugar-growing islands of the West Indies, and to lighten and reward the free labour of the liberated African.

At the present time, when the attention is so much given to agricultural improvement, and such great exertions are making to increase the productiveness of our own soil, to meet, as it is to be hoped, foreign competition, the discovery of great deposits of concentrated manure, such as the guano is, may be considered peculiarly fortunate and encouraging.

As we have few or no good accounts of the localities, and as they are very curious and peculiar, I shall insert a description of one instance in particular,—viz. that of an islet from which some African guano was taken, the composition of which I shall have to notice farther on.

" The island from whence the guano is taken, is about three miles from the shore, on the south-west coast of Africa. It is a barren rock, about a mile in circumference; has no soil, or the least sign of vegetation. The guano lies to the depth of about twenty feet, and without any variation in quality. The continent is very sandy, and in high winds (hurricanes, for instance) will cover a ship's deck nearly 100 miles from the land. The birds on the island are a kind of penguin, and cannot fly to any distance, if at all, their wings being a kind of fin. It is believed, that the captain of the vessel who brought the guano, was the first human being who set foot on the island, which is very difficult to approach, there being no harbour and a heavy surf. On walking on it, he could scarcely set his foot without treading on the birds, and they took no notice whatever of him, except pecking at his feet, he being barefoot; and, on a gun being fired, they merely fluttered a good deal and made much noise. There is no fresh water, it is believed, for some hundreds of miles along the coast, and no rain."

For this interesting and simple account, I am indebted to a friend, who obtained it from the merchant, the importer of the guano.*

As in consequence of the increasing demand for guano,† and

* Mr John Rae, South Castle Street, Liverpool.

† The price of the Peruvian is about L.12, and the African is offered for L.9 a ton.

its high price as a manure, there is great temptation to adulterate it, or impose a spurious compost in imitation of it; and which, indeed, is said to be practised already to a considerable extent, any precise information respecting the genuine article can hardly fail to be useful. With the hope of contributing something of this kind, I have examined both the American and African guano, comparing them together; and I shall now briefly state the results, premising a slight notice of their appearance.

Both, when moist or damp, as when imported, and offered for sale, are of a pretty dark reddish brown colour, very like that of dark moist snuff. In drying, both become of a lighter hue, and the African kind, on exposure to the air, soon exhibits a white efflorescence. Both when moist exhale a strong ammoniacal odour (the African the strongest), mixed with a different and peculiar smell, somewhat offensive, which, with the ammoniacal, they in a great measure lose in drying.

Under the microscope, using a high power, both appear to consist chiefly of very minute granules, many of them smaller than the blood corpuscles, and of slender prismatic crystals of oxalate of ammonia, in which the African kind is most abundant.

Subjected to chemical analysis, the two kinds (No. 1 the American, No. 2 the African) have appeared to consist of—

No. 1.	No. 2.	
41.2	40.2	Matter soluble in water, destructible by fire or volatile, such as oxalate of ammonia, diphosphate and muriate of ammonia, and animal matter.
29.0	28.2	Matter not destroyed by fire, nor soluble in water, or very slightly so, chiefly phosphate of lime and magnesia, with a little sulphate of lime and a very little siliceous sand.
2.8	6.4	Matter not destroyed by fire, but soluble in water, chiefly common salt, with a little sulphate and sesquicarbonate of potash.
19.0	...	Matter destructible by fire, little soluble in water, chiefly lithate of ammonia.
8.0	25.2	Matter expelled in drying on a steam bath, chiefly water and sesquicarbonate of ammonia.
100.0	100.0	

As regards the American guano, the results of this coarse analysis do not disagree with those of the more minute one of Völckel, excepting in one particular; he obtained 7 per cent. of oxalate of lime, a salt which certainly did not exist in the specimen which I examined; and this I say, after having carefully sought for it.

Comparing the constituents as they are placed side by side of the American and African guano, the chief difference is seen to be, that while the American kind contains a large proportion of lithate or urate of ammonia, the African kind is totally destitute of it. This I little expected, considering its origin, the excrement of birds, their fæces and urine, the latter of which commonly consists chiefly of lithate of ammonia. The obvious explanation of the circumstance is, that the lithic acid, which formed a part of the urine, has in a long period of time suffered decomposition, and has given rise to oxalate of ammonia. And, that this guano is very old, was indicated by the partially decomposed state of some feathers, not excepting the quills, which were included in it. I have said, that the African guano is totally destitute of lithic acid; and I believe I am warranted in coming to this conclusion, having carefully sought for it in vain. It may be mentioned that search also was made in both kinds of guano for urea, but without well-marked success. The brown animal matter, soluble in water, yielded a small portion to alcohol, which had some of the properties of urea, and formed a compound with nitric acid, but less distinctly crystalline than the nitrate of urea.

On account of the origin of guano, that already referred to, and the questions involved in the difference as to composition which exists, or is supposed to exist, between it and the matter from which it is derived, it appeared to me desirable to examine with care the excrements of birds, and especially the urinary portion,—thinking it not improbable, that besides lithate of ammonia, [which it would appear, as far as experiment has hitherto gone, is the principal constituent of the urine of birds, whatever their food may be,] oxalate of ammonia might form also a part.

The specimens which I have hitherto examined have been chiefly the following, viz., from the common goose, after feeding on grass, from the pigeon, the common fowl, the gull, the pelican, and the white-headed sea eagle,—the three latter in the garden of the Zoological Society of London, where they are fed chiefly on fish, and the gulls entirely so, as I am informed by my friend Mr Gulliver, to whose kindness I am indebted for the specimens.

The urine from each of these birds, in its purest state, or least mixed, was very similar; of an opaque white, sometimes with a stain of brown. Under the microscope, it appeared to be composed chiefly of granular matter, each particle seldom exceeding in size a blood corpuscle, viz., that of man, and commonly less. In the instance of the urine of the sea-eagle, delicate tabular crystals were mixed with the molecules. Submitted to chemical examination, each kind was found to consist chiefly of lithate of ammonia, with a little phosphate of lime and magnesia. In that of the goose, a trace of urea was detected, with a little carbonate of lime and magnesia; and in that of the sea-eagle, a small portion of oxalate of lime, constituting the tabular crystals seen under the microscope. In each instance, search was made, but in vain, for oxalate of ammonia,—not a trace of it could be detected; and thus tending to confirm the conclusion of M. Liebig, that this salt results in guano, from the decomposition of lithic acid, and the new arrangement of its elements, aided, I would add, by the absorption of oxygen from the atmosphere.

That atmospheric oxygen is concerned in this change, I am induced to infer from some experiments which I have made. I shall briefly notice two. Lithate of ammonia (the urine of the sea-eagle) in a moist state, was subjected in a close vessel to the temperature of boiling water, for about twenty-four hours, when it was tried for oxalate of ammonia; but not a trace of this salt could be detected. It was next exposed to the same temperature, mixed with black oxide of manganese, and for about the same time. Now, the presence of oxalate of ammonia was clearly indicated, for the mixture, after digestion, with a little water and filtered, yielded a solution, which, after having been made slightly acid, was rendered turbid by muriate of lime, and the precipitate had the properties of oxalate of lime. I may mention further, that the aqueous solution was coloured slightly brown, seeming to show, that besides the formation of some oxalate of ammonia, in this instance, a soluble matter also was produced, which, it may be conjectured, is analogous to that which exists in the guano, imparting colour to it.

Before concluding, I would wish to say a few words relative to the tests of guano,—the means of distinguishing between the genuine and the spurious kinds. Taking into account its origin, and that deposits of it can be found only where no rain falls, the inference seems now to be obvious, either, if old, that it must abound in oxalate of ammonia; or if, comparatively of little age, in lithate of ammonia; or if not of great age, it must contain a notable quantity of both these substances. These salts are easily detected, and the first mentioned, very readily by the microscope.

The adulteration of the genuine kind, to which the temptation is great, cannot be so easily detected. I fear it cannot be accomplished, excepting by means of chemical analysis; and that the farmer must hold himself dependent on the integrity of the merchant; and if he would wish to avoid the chances of imposition, he must purchase only of the merchant of established name, and at the regular price.

As guano appears to be constantly exhaling ammonia on exposure to the atmosphere, to prevent loss and deterioration, it cannot be, before use, too carefully excluded from the air; and on the same account, it ought not be applied as a manure, whilst vegetation is inactive, but rather at the moment of its coming into activity, and when in progress, according to the Peruvian manner of bestowing it on the plant, rather than on the soil.

I have alluded, in the beginning of this notice, to the spirit of enterprise displayed in the importation of guano. I have since learnt, that, in procuring the African kind, that which I have examined, no ordinary degree of this quality has been exerted, accompanied by a boldness of daring, and a perseverance worthy of record. The importer, Mr Rae, informs me, in a letter with which he has just favoured me, that his son is the discoverer of the guano-islet or islets, for it would appear that there are several of them, so remarkably situated.

That he was led to go in search of them in the beginning of last year, from remembering having, when a boy at school, looked into the sea-journal of an American whaler, in which mention was made of such spots. And that his first attempt was unsuccessful, and nearly proved fatal to himself and all concerned,—he and his boat's crew, in exploring the islets, having almost perished from want of water, before they could rejoin their little "surveying vessel;" and then (she, too, being in want of water), having had to sail 1500 miles before they could obtain a fresh supply.

This is a meagre outline of a hazardous and most important enterprise. The details of it, it is to be hoped, will be published; they can hardly fail exciting interest; and they may convey valuable information, either directly or indirectly, on many important points connected with the physical history of a region of which at present so little is known. The result of the voyage, the director of it may well be proud of, contemplating, as he writes to me, by the introduction of some thousands of tons of productive manure, increase of fertility to our soil, to the extent of "producing three bushels of corn where only two were previously grown."

THE OAKS, AMBLESIDE,
February 29. 1844.

On the Physical Characters of the Esquimaux. By RICHARD KING, M.D. (Communicated to this Journal by the Society.)

Read before the Ethnological Society, London.

Of the circumpolar family, the Esquimaux branch is the most remarkable, owing to the vast extent of country over which it is spread. This race of fishermen inhabit the whole of the northern coast of North America, and as far as Prince William's Sound in the Pacific, and the coast of Labrador in the Atlantic, their western boundary being St Lawrence Island, and their eastern Greenland. Although, for a considerable period, Labrador has been the most southern limit of the Esquimaux, in the direction of the Atlantic, it would appear that, in early times, their migration extended further south, including the occupation, as a summer residence, of the Island of Newfoundland. The Abbé Raynal expressly states, "that no savages have ever been seen on that island, except some Esquimaux from the continent in the hunting season," an opinion I had formed, before this passage in the Abbé Raynal's work came under my observation, from a careful study of the narratives of the early travellers in search of a north-west passage. "Sebastian Cabot," writes Purchas, "brought to England three savages taken in Newfoundland,* who were clothed in beast's skins, and did eat raw flesh, and, in their demeanour, like to brute beasts, whom the king kept a time after; of the which, upon two years after, I saw two apparelled, after the manner of Englishmen, in Westminster Palace, which, at that time, I could not discover from Englishmen till I learned what they were; but, as for speech, I heard none of them utter one word." The account given by Whitbourne of the natives of Newfoundland,† is in accordance with the known habits of the Esquimaux, but has no reference to the Red man of America. Whitbourne has thus expressed himself:—"The naturall inhabitants of the country, as they are but few in number, so are they something rude and savage; having neither knowledge of God, nor living under any kind of civill government. They live altogether in the north and west part of the country, which is seldom frequented by the English. But the French and Biscayans, who resort thither for the whale-fishery, and also for the cod fish, report them to be an ingenious and tractable people (being well used). They are ready to assist them, with great labour and patience, in the killing, cutting, and boyling of whale, and making the traine oyle, without expectation of other reward, than a little bread or some such small hire." Lieutenant Roger Curtis, in a paper addressed to the Royal Society,‡ informs us that the Esquimaux were settled at different places upon the sea-coast down to the river St John's; but for many years past, whether owing to the quarrels with the mountaineers, or the encroachments of the Europeans, they have taken up their residence far to the north. O'Reilly

* 1497. † 1612. ‡ 1773.

has made the same remark,* probably a quotation from Lieut. Roger Curtis. Thorsin, the Icelander, describes the people of Winland† as of low stature, having boats covered with leather. Now, it has been contended, and very ably, that Winland was south of the Gulph of St Lawrence; and since we have been able to trace the Esquimaux thus far, it is by no means improbable that they took advantage of the mighty St Lawrence and penetrated inland; for near the Falls of Niagara have been discovered numerous tumuli, attributed by the Red man, who does not adopt this mode of sepulture, to an extinct race that inhabited the country before him. Dr Hodgkin, with his usual intelligence and research, examined a skull exhumed from one of these tumuli, and pointed out its strong resemblance to the skull of a known Esquimaux. A cast of the former and the original of the latter, is to be seen in the Museum of Guy's Hospital, arranged by Dr Hodgkin, a museum deservedly the pride of this country and the admiration of foreigners.

Of that all-important branch of science—statistics, in relation to the Esquimaux, our knowledge is but slight. The population of the north-west coast of America, from Prince William's Sound to Point Barrow, was estimated, in 1822, at 1200; of Regent's Inlet, in 1830, at 160; of Melville Peninsula, in 1823, at 219; and of Labrador, in 1773, at 1623. It appears evident, then, as far as our information extends, that the north-west corner of America, Labrador, and Greenland, are better inhabited than the American boundary of the Polar Sea, and that the population gradually increases in the direction from east to west, except in the neighbourhood of the Mackenzie, where perhaps from the resources of the country, compared with any other given area, it is most abundantly peopled.

The Esquimaux all speak radically the same language, and even where there exist dialectrical differences, they are so slight, that a native, whether he is located on the shores of the Pacific or of the Atlantic, is able to make himself understood throughout all the various and widely spread communities.

The author of "Researches into the Physical History of

* 1818. † 10th century.

Mankind"* has made no allusion to the form of the skull of the Esquimaux in his first edition; but in that of 1826, there is a description agreeing with a skull which he has figured, and which figure and description appear in his recent work, entitled the "Natural History of Man." The letter-press runs as follows: "The face is of a lozenge shape, rising like one of the faces of a pyramid almost to a point,"—and so it is represented. But a comparison with four skulls figured in Blumenbach's Decades, four in Morton's Crania Americana, one in the collection of the Hunterian Museum of the Royal College of Surgeons, one in the Museum of Guy's Hospital, and twelve in the possession of the phrenologist Deville, entirely contradicts Dr Prichard's remarks. It is true that O'Reilly has given a similar description to that of Dr Prichard.† "The forehead," in the opinion of that traveller, in allusion to the Esquimaux of the island of Disco, "and the side of the head above the temples are greatly depressed; the crown is elevated considerably; and the back of the head is depressed as the forehead. The smaller end of a hen's egg presents a familiar resemblance to their cranium." But after inspecting fourteen skulls, and representations of eight more, I am led to believe that O'Reilly and Dr Prichard have been describing the same skull, which owes its peculiarity to some accidental cause; and since O'Reilly wrote in 1818, and Dr Prichard in 1826, it is probable that the Doctor sought for, and obtained, the skull which O'Reilly has described. However this may be, it is certain that the skull figured by Dr Prichard cannot be taken as a type of this Arctic family. It would be as incorrect, with the materials before us, to agree with Dr Prichard, as, with our knowledge of the custom of artificially modifying the form of the head practised by the natives of North-western America, to fall in with the views of Professor Tiedemann, and Mr Pentland, that the skulls found in the ancient graves, called huacas, in the great alpine valley of Titicaca, were moulded so by nature. The Esquimaux, in my opinion, strongly exemplify the broad-faced and moderately-vaulted character of the skull classified as Mongolian. The most striking characteristic is the outward projection of

* Dr Prichard.

† O'Reilly's Greenland, and the North-West Passage, p. 62.

the cheek bones, which, turning backwards, meet a corresponding projection of the temporal bone, forming with it a large rounded sweep. The upper part of the face is remarkably flat, and the nasal bones are nearly on a plane with the cheek bones.

As we find the skull, so do the features correspond. The face is round and full, and the nose is apparently sunk, owing to the prominences of the cheeks, which sometimes exists to such a degree, that a ruler placed across them will leave the nose untouched.* Besides, there are peculiarities regarding the eyes which do not depend upon the skeleton head. They are small, and placed obliquely within the orbits, the inner part being depressed, while the outer part is proportionably raised; and Mr Edwards, the surgeon to Sir Edward Parry's second expedition, has pointed out a peculiarity common to many individuals of Melville Peninsula, consisting in the inner corner of the eye being covered by a duplication of the adjacent loose skin. This fold is lightly stretched over the edges of the eye-lids, covering the caruncculus lachrymalis, which in Europeans is exposed, and forms, as it were, a third lid of a crescentic shape. This singularity was ascertained to be very remarkable in childhood, less so toward the adult age, and then frequently disappearing altogether, the proportion in which it existed in grown up persons being small compared with that observed among the young. Although these are the general features, they differ with individuals as is in all other nations; yet, like the Jews, they have, even when their countenances are shaped like Europeans, an expression altogether peculiar.† Oval faces and high Roman noses are occasionally to be met with, and when this is the case with both parents, the children always resemble them.

Notwithstanding the general features of the Esquimaux do not convey the general idea of beauty according to the European standard, still they have found more or less favour in the eyes of every traveller who has visited them. Captain Cook thought few were of the handsome sort, though their countenances commonly indicated a considerable share of vivacity,

* Lyon. † Ibid.

good nature, and frankness, and that some of the women had agreeable and delicate features. Sir John Franklin and Dr Richardson considered the young women and children handsome, some of them having a considerable share of beauty, and one in particular, who would have been deemed pretty even in Europe; and, like our own belles, they spared no pains to shew themselves off to advantage. Sir Edward Parry makes the same observations of the natives of the river Clyde, and of Melville Peninsula, and then adds, that "one of the prettiest women of the latter people had a face more oval than that of Esquimaux in general, very pretty eyes and mouth, teeth remarkably white and regular, and possessing in her carriage and manners a degree of natural gracefulness which could not be hid even under the disguise of an Esquimaux woman's dress." Two young men, about twenty years of age, and standing five feet seven inches, were both "handsome and prepossessing, and their limbs well formed and muscular; qualities which, combined with their activity and manliness, rendered them, to speak like a naturalist, perhaps as fine specimens of the human race as almost any country could produce. A man named Téä, his brother, his wife, and two daughters had good Roman noses, and one of the latter was an extremely pretty young woman." The natives of Prince William's Sound are described as having generally broad flat faces, small eyes, and white and regular teeth, though there is considerable variety in this respect. Those of Kotzebue's Sound have forbidding countenances, and an expression of wantonness, but not of stupidity, their features being characterized by small eyes and very high cheek-bones;* while, according to Captain Beechy, they are a good looking race, although, at a comparatively early age, they (the women in particular) soon lose their comeliness, and old age is attended with a haggard and care-worn appearance, rendered more unbecoming by sore eyes, and by teeth worn to the gums by frequent mastication of hard substances. The same state of the teeth has been observed in the aged of all the different communities. The natives of Mackenzie River, to Sir John Franklin's western limit, were found to have the

* Kotzebue.

cheek-bones less projecting than usual, but "having the small eyes and broad nose which ever distinguish that people."* Of the natives of Regent's Inlet, Sir John Ross expresses an opinion that the females were certainly not beautiful, but their features were mild, and their cheeks, like those of the men, ruddy, and one girl of thirteen years of age was considered to have a pretty face. Lieutenant Roger Curtis considered the natives of Labrador as, "in general, not very disagreeably featured, though there were some among them who were extremely ugly. They were flat visaged, and had short noses."

In complexion, Sir Edward Parry† considers the Esquimaux not darker than the Portuguese, and such parts of the body as are constantly covered, not to fall short in fairness to the generality of the inhabitants of the Mediterranean, a very fine healthy blush tinges the cheeks of females and young children, and frequently they have complexions nearly as fair as that of Europeans; but the men are more inclined to be sallow. Sir Martin Frobisher states, "their colour is not much unlike the sun-burnt countryman." The natives of St Lawrence Island are somewhat fairer, and at Prince William's Sound "the complexion of some of the women and of the children is white, but without any mixture of red."‡

The beard is scanty, but few instances occurring of the chin being entirely covered. The moustaches are more thick. The hair is straight, coarse, and of a raven black, but it has, for a few years during infancy, a shade of brown. On their bodies there is but little hair; in fact, some are totally destitute of it;§ and at St Lawrence Island there is a deficiency even of beard.|| The hair becomes blanched in persons advanced in years, which was common at Regent's Inlet; while at Herschel Island, to the westward of the Mackenzie River, an old woman, whose hair was silvered with age, was a conspicuous object.¶ A solitary case of bald head is recorded in a native of Regent's Inlet, aged fifty-six years.**

The Esquimaux are of a robust make and healthy appearance, with small hands and feet, well turned at the ankles,

* Sir John Franklin. † Also Lyon, Hearne, Curtis. ‡ Captain Cook.
§ Cook, Curtis, Parry, Lyon. || Beechy. ¶ Franklin. ** Ross.

and without blemishes, and their gait is erect and free.* The neck and shoulders of the women are generally in good, though large, proportion, and the arm and wrist are sometimes handsome. Though not inclined to corpulence, they are generally plump, flesh loose, and without firmness; and even in the young and strong men, the muscles are but ill-defined; and though, when dressed, they appear a stout well-set people, their figures, when uncovered, are rather weak than otherwise.† We are naturally led to believe, therefore, that their bodily strength is not great, which was the conclusion Captain Lyon arrived at, after matching some of his men with Esquimaux, of equal sizes, to lift weights, when it invariably happened that burthens which were raised with facility by the British, could scarcely be lifted by the natives. Sir Edward Parry is of opinion that the two races are equal, and Crantz, that the Esquimaux of Greenland far surpass us. Crantz states, "that a man that hath eat nothing for three days, at least nothing but sea-grass, can manage his caiak in the most furious waves; and the women will carry whole a rein-deer the space of four leagues, or a piece of timber or stone near double the weight of what an European could lift." As the Esquimaux of Greenland practise athletic games, there can be no doubt that they are a strong people; but Crantz has most assuredly overrated them. I am inclined to believe, from my own observations, that the Esquimaux equal in strength the generality of Europeans. The subject, however, has not been sufficiently investigated to justify a conclusion.

Stature is another point that requires attention; the various travellers having written in general terms. These are, nevertheless, sufficient data to afford an average. The tallest man of Kotzebue's Sound, seen by Beechy, was 5 feet 9 inches and the average of all the tribes he had seen, from Kotzebue's Sound to Point Barrow, including St Lawrence Island, 5 feet 7½ inches; of the inhabitants of Boothia Gulf, the range of the men was from 5 feet 3¾ in. to 5 ft. 10 in.,

* Cook, Kotzebue, Franklin, Hudson, Frobisher, Curtis.

† Hearne, Lyon.

the average being 5 ft. $6\frac{1}{2}$ in. ; and that of the women from 4 ft. $9\frac{1}{2}$ in. to 5 ft. 5^{1} in., the average being 5 ft. $1\frac{7}{8}$ in. Of the natives of the River Clyde, the range of the men was from 5 ft. $4\frac{1}{2}$ in. to 5 ft. 6 in. ; and that of the women from 4 ft. 10 in. to 4 ft. 11 in. Of twenty individuals of each sex of Melville Peninsula, the range of the men was from 4 ft. 11 in. to 5 ft. 10 in. ; the average being 5 ft. $5\frac{1}{3}$ in., and that of the women from 4 ft. $8\frac{3}{4}$ in. to 5 ft $3\frac{1}{2}$ in. ; the average being 5 ft. $0\frac{1}{2}$ in. Of the natives of the Savages Island, Lieut. Edward Chappel considers a fair average standard might determine their height to be between 5 ft. 5 in. and 5 ft. 8 in. The average height of the men, therefore, is 5 ft. 6 in., and that of the women 5 ft. 1 in. ; and the people evidently increase in stature in the direction from east to west.

The Esquimaux have various modes of arranging their hair according to the locality. As regards the men, some wear it long and allow it to hang about their heads in a slovenly manner.* Some cut it short before,† so as not to incommode their face, and others both before and behind ;‡ while at Kotzebue's Sound § and Schismareff Inlet,|| the only part that was cut was a circular patch upon the crown of the head like the tonsure of the Roman Catholic clergy. This fashion was adopted by a few of the natives of Melville Peninsula¶ and of the Mackenzie River,** while at St Lawrence Island many had that part shaved.†† At Southampton Island, the hair is worn in one large mass as large as the head of a child rolled into the form of a ball, and projecting from the rise of the forehead. One of these bundles Captain Lyon found to consist of six long strings of his own locks originally plaited, but then so matted with dirt and deers' fur as to resemble a rough hair tether. These extraordinary tresses were bound tightly together at their base and measured about 4 feet.‡‡ Hair is pretty universally worn by the Esquimaux on the upper lip and chin from 1 inch to $1\frac{1}{2}$ inch in length, and some cultivate a little tuft between the chin and lower lip.

The women of Prince William's Sound tie a small lock of

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* O'Reilly, Franklin, Parry. † Parry, Franklin, Frobisher.
‡ Kotzbue, Cook, Ross, Beechy. § Beechy and Kotzebue. || Beechy.
¶ Parry. ** Franklin. †† Beechy. ‡‡ Lyon.

the hair on the crown, and a few club it behind after our own manner, leaving the rest of the hair to hang down.* Those of the Mackenzie River wear it very tastefully turned up from behind to the top of the head, and tied by strings of white and blue beads, or cords of white deer-skin. It is divided in front, so as to form on each side a thick tail, to which are appended strings of beads that reach to the waist.† At Kotzebue's Sound,‡ Boothia,§ and Melville Peninsula,‖ they separate their hair into two equal parts, one of which hangs on each side of their heads and in front of their shoulders. To stiffen and bind these, they use a narrow strip of deer-skin, attached at one end to a round piece of bone 14 inches long, tapered to a point, and covered over with leather. This looks like a little whip, the handle of which is placed up and down the hair, and the strap wound round it in a number of spiral turns, making the tail, thus equipped, very much resemble one of those formerly worn by our seamen. The strap of this article of dress, which altogether is called a togleega, is so made from the deer-skin as to shew, when bound round the hair, alternate turns of white and dark fur, which give it a very neat and ornamental appearance. On ordinary occasions, it is considered slovenly not to have the hair thus dressed, and the neatest of the women never visited the ships without it. Those who are less nice, dispose the hair into a loose plait on each side, or have one togleega and one plait; and others, again, wholly disregarding the business of the toilette, merely tuck the hair in under the breast of the jackets.

This slovenly disposal of the hair was found to be the case with the natives of the River Clyde,¶ and Hudson Straits.** The Esquimaux of Labrador, Hudson's Straits,†† and the Great Fish River,‡‡ wear the hair parted in front into two festoons, secured by a fillet of white deer skin twined around the head, whilst the remainder flows gracefully over the neck and shoulders, or is tied up into a knot behind. At Southampton Island, the same style is adopted, but instead of using a band for the purpose, it is twisted into its position. The natives of Green-

* Cook. † Franklin, Richardson. ‡ Beechy. § Ross.
‖ Parry, Lyon. ¶ Frobisher. ** Chappell. †† King. ‡‡ Lyon.

land braid it, and tie it up in a bunch on the top of the head.*

The circular band of white reindeer fur worn usefully by the natives of the Great Fish River and Labrador, is occasionally used by some of the communities as a mere ornament, and is frequently formed of brass, either plain or serrated at the upper edge,† or of brass-buttons strung together.‡ In some cases, the men thus ornament themselves, which, among the inhabitants of Kotzebue's Sound, consists of alternate blue and white shells; and at Melville Peninsula, of several strips of skins sewn together, alternately black and yellow. Near the upper edge, some hair is artfully interwoven, forming, with the skin, a very pretty chequer-work. Along the lower edge is suspended more than a hundred small teeth, principally of the deer, neatly fastened by small double tags of sinew, and forming a very appropriate fringe.§ Necklaces, bracelets, and ear ornaments, are seldom worn. But one instance of the former is on record, that of a native woman of Kotzebue's Sound, recorded by Captain Beechy. This necklace consisted of pieces of amber strung together. Bracelets and ear ornaments are generally worn by the natives of Prince William's Sound,|| and but rarely by the inhabitants of Kotzebue Inlet.¶ The bracelets are formed of iron or copper;** pieces of amber or shells, or beads strung together.†† The ear ornaments consist of blue glass beads at Kotzebue Inlet, and at Prince William's Sound, of pieces of tubulose shells in small bunches. These people were, moreover, singular in both sexes, wearing ear-drops.‡‡ The ears were not pierced after our fashion, but with several holes about the outer and lower part of the edges. Neither Kotzebue nor Beechy have stated whether the same kind of perforations were adopted by the natives of Kotzebue's Sound. At the Great Fish River, the ermine skin was the favourite ear ornament;§§ and Sir John Ross obtained from a native of Regent's Inlet, a small bar of iron ore, suspended by sinew for the same purpose. Lieutenant

* O'Reilly, Egede. † Parry, Curtis. ‡ Frobisher. § Parry. || Cook. ¶ Kotzebue, Beechy. ** Kotzebue, Beechy. †† Cook. ‡‡ Cook. §§ King.

Roger Curtis informs us, that at Labrador the natives load their heads with large strings of beads, which they fasten to the hair above the ears; and Captain Lyon found the natives of Southampton Island adopting the same fashion; but instead of beads, having at one time little bone ornaments, and at another time small irregularly shaped pieces of lead, strung alternately with square cut pieces of the claw of a bird. Although but one of the women of Kotzebue Inlet was observed to wear a necklace, there were several who had a mode of ornamenting themselves quite peculiar. They had suspended to their hips, under their clothes, three or four bells, and one even lower down, which was of the size of a dustman's bell. For what purposes they were placed there, was not within the reach of conjecture; but "by their polished surface, and the manner in which they were suspended, they appeared to have long occupied those places. They were certainly not hung there for convenience, as the large one, in particular, must have materially incommoded them in walking."*

Among the personal ornaments of the Esquimaux must be reckoned that of tattooing, which is of indispensable importance to the women. The parts of the body thus adorned are the face, arms, hands, thighs, occasionally the breasts, and, in Greenland, the feet. Tatooing is rarely practised by the men; a few only of the natives of Melville Peninsula were thus marked on the back of the hands, considered by them as a *souvenir* of some distant or deceased person who had performed it; and Sir John Franklin heard from one tribe, that another tribe were accustomed to tattoo their faces. The art is most abundantly practised by the women of Melville Peninsula, of Regent's Inlet, and of the Great Fish River, the pattern being the same, though varying in the number of lines. The pattern consists of from three to six lines drawn horizontally across each cheek, from three to eighteen vertically across the chin, and from three to eight from the forehead to the centre of the nose (between the eyebrows), a double line round the neck and breast, above the shoulder, another below the shoulder, and a third above the elbow. Between each of

* Beechy.

these which encircle the arms, and parallel to each other, there are ornamental devices, but without any meaning.* Between the Mackenzie and the Coppermine Rivers, and at St Lawrence Island, the women are tattoed across the cheeks only.† At St Lawrence Island, however, there is one peculiarity in the outer line, extending from the lower jaw over the temple and eyebrow.‡ Westward of the Mackenzie, five or six blue lines merely are drawn perpendicularly from under the lip to the chin ;§ and at Norton and Kotzebue Sounds,‖ the same fashion is adopted, but at the latter Sound the lines are reduced to three.¶ Captain Cook has not stated the number of lines which marked the natives of Norton Sound ; he has only mentioned the fact of their being tattoed from the under lip to the chin. At Labrador and Southampton Island, instead of lines, small dots are substituted.**

The operation of tattooing is performed about the age of ten, and is very expeditiously managed by passing a needle and thread, the latter covered with lamp-black and oil, under the skin according to a pattern previously marked out. Several stitches being thus taken at once, the thumb is pressed upon the part, while the thread is drawn through, by which means the colouring matter is retained, and a permanent dye of a blue tinge imparted to the skin. It is a painful as well as tedious process, especially as the needles are made of strips of whalebone. For those parts where a needle cannot conveniently be passed under the skin, the method by puncture is used.††

Although the Esquimaux men do not practise tattooing, many of them pierce the lower part of the face for the purpose of introducing various kinds of ornaments. From Prince William's Sound to the Mackenzie, this custom is universally adopted ; but, as far as our knowledge extends, it is confined within those limits. The lower lip, each corner of the mouth, and the septum of the nose, are the parts selected for the purpose ; but it is more generally the fashion to pierce only the

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* Parry, Ross, King. † Richardson, Beechy. ‡ Beechy.
§ Franklin. ‖ Cook. ¶ Beechy.
** Frobisher, Lyon. †† Parry.

corners of the mouth, in which are placed labrets, formed with a double head like a stud, either made of ivory and blue beads, of ivory alone, or of different kinds of stone, as steatite, porphyry, or greenstone.* The men of the Mackenzie†, and the women of Chamisso Island,‡ in addition, pierce the septum of the nose, through which they thrust the quill feathers of birds, or pieces of bone, or tubulose shells strung on stiff pieces of sinew. Both sexes at Prince William's Sound thus pierce the septum of the nose, but prefer the lower lip to the cheeks, and adopt two modes. The one consists in the under lip being slit or cut quite through, in the direction of the mouth, a little below the thick part. This incision, which is made even in children at the breast, is often above two inches long, and either by its natural retraction when the wound is fresh, or by the repetition of some artificial management, assumes the true shape of lips, and becomes so large as to admit of the passing of the tongue. This happened to be the case when the first person having this incision was seen by one of Captain Cook's party, who called out that the native had two mouths, which the immortal traveller observes it very much resembled. In this artificial mouth is placed a flat narrow ornament, made chiefly of a solid shell or bone, cut into little narrow pieces like small teeth, almost down to the bone or thickest part, which has a small projecting portion at each end to support it in the divided lips, the cut part then appearing outward. The other mode is merely to perforate the lower lip in several places, when the ornaments consist of as many distinct shelly studs, whose points are pushed through the perforation; the heads appear within the lip, as another row of teeth immediately under their own. Attached to the studs from below are suspended small strings of beads which hang down to the point of the chin. These are not removed so easily as the lip ornaments, which are at pleasure displaced and replaced with the tongue. The Esquimaux of the Mackenzie valued the labrets so highly, as to decline parting with them;§ while those of Prince William's and Kotzebue's

* Cook, Kotzebue, Franklin, Beechy. † Franklin.
‡ Beechy. § Franklin.

Sound, gave them freely,* regardless of the inconvenience of the saliva that flowed through the badly cicatrized orifice over the chin, but rather laughed when one revolted at the sight, and delighted in thrusting the tongue through the opening, at the same time, that they winked the eyes. Nor are they particular what they substitute for the labrets. One man, we are informed by Captain Cook, appeared before him with two iron nails projecting from them, like prongs; and another endeavoured to make a large brass button answer the purpose of a labret.† Through the septum of the nose, awls, and large cod-fish hooks are thrust, and the women appropriate ear-rings and thimbles as decorations to their dress.‡ The perforations are made at the age of manhood, by an incision sufficiently large to admit a quill, about half an inch below the corners of the mouth, which has the effect of depressing the under lip, and keeping the mouth open.§ The orifice is enlarged from year to year, until it reaches half an inch in diameter; and in more advanced years, is not unfrequently of a much larger size. Captain Beechy obtained from a native of Schismareff Inlet a finely polished jade that was three inches in length by an inch and a half in width. For some time after the operation has been performed, it is necessary to turn the cylindrical pieces of ivory frequently, that they may not adhere to the festering flesh. In time, this action becomes as habitual to them, as that of turning the mustaches is to a Mussulman.‖

On the Theory of Parallel Lines. By Henry Meikle, Esq. A.M. Communicated by the Author.

During the long succession of ages which have elapsed since the origin of geometry, many attempts have been made and treatises written, though with little success, to demonstrate the important theorem which Euclid, having failed to prove, has styled his 12th Axiom, and which is nearly equivalent to assuming that the three angles of every triangle amount to two right angles. Among the more distinguished and ex-

* Cook, Beechy. † Cook. ‡ Franklin. § Franklin. ‖ Beechy.

tensive writers on the subject in our own times, may be reckoned M. Legendre in his *Elements*, and in the *Memoires de l'Academie*, as after noticed; and Colonel T. Perronet Thompson, in his "Geometry without Axioms," and in his more recent tract proposing a proof by help of the logarithmic spiral. To the former of his treatises, Colonel Thompson has appended critical notices of thirty of the more plausible methods which have, at very different times, been given by other authors as demonstrations. His criticisms, though neither always the first which have been made on these methods,* nor yet all new, are generally just, and even fatal to them. He strongly objects, though rather metaphysically, to employing infinite quantities, as is done by M. Bertrand, who reasons upon a numerous set of areas, each of which is of infinite magnitude, and such that, while on one side it has no limit of any kind, it is, on other sides, bounded by infinite lines stretching immeasurably beyond the fixed stars. But, from other and more elementary considerations, I shall afterwards briefly shew Bertrand's method to be a complete failure. Colonel Thompson himself, however, does not seem to have been aware that, in his own attempt at a sort of mechanical demonstration, in which he supposes a straight line to preserve its parallelism whilst "travelling" laterally, no finite line could suffice for such "travelling line." Again, in calling in the aid of the logarithmic spiral, he has assumed that triangles formed partly of unequally curved arcs of that spiral, and partly of arcs of unequal circles, are equal to one another, and identical with rectilinear triangles. This, like most of the attempts on Euclid's axiom, evidently assumes far more than the whole thing to be proved. Nay, supposing the curved sides of those triangles were of the same lengths as if formed of straight lines, it would still be necessary to prove, what is not even true, that their angles are the same. But these are by no means the only objections to which his demonstrations are liable.

Much of the form of the present essay has been adopted with a view to brevity; various minute and commonplace details are omitted which every tyro can readily supply, and occasionally some abbreviations and modes of reasoning are adopted, which, though not very common at that stage of geometry, are well known, and have no dependence on what is to be proved. But independently of this, the question is here treated in a very different manner from anything I had previously met with on

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* For instance, in the Philosophical Magazine for December 1822, I had greatly anticipated him in refuting the 30th, or Mr Ivory's, method; for I have there shewn that it depends on the very liberal assumption that four straight lines may always be made to meet round any given polygon, so as completely to inclose it, let it have ever so many sides: which is far more complicated and less evident than Euclid's axiom.

this subject. For, so far as I am aware, it had not till now been shewn that, if, in so much as one triangle, the sum of the angles differed from 180°, a definite relation behoved to subsist between the areas and angles of all triangles. It is to be hoped that such a relation, which I have here deduced from that supposition, and endeavoured to follow it up to an absurdity, may yet lead to other and preferable modes of demonstration.

PROP. I. Triangles, whose areas are equal, have the sums of their angles equal.

Fig. 1.

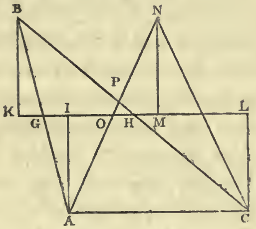
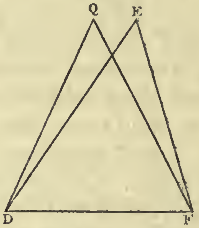


Fig. 2.



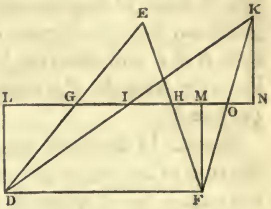
Case 1. To prove this of any two triangles, A B C, D E F, which have their bases equal, as well as their areas; bisect A B, B C in G and H, and to G H or its extension draw the perpendiculars A I, B K, C L. Then the triangles A I G, B K G, having two angles and a corresponding side respectively equal, are (Eucl. i. 26) every way equal; and for the like reason, the triangles B K H, C L H, are equal. Hence, the triangle A B C is equal to the quadrilateral A I L C, and its three angles are equal to the two angles I A C, A C L.

The three perpendiculars drawn to I L have in effect been proved to be equal; and in the same way as it has been shewn of A B C, every other triangle which could have A C for a base, and whose sides would be bisected by G H produced, if necessary, must have its perpendiculars which are drawn to this bisecting line, as well as its area, and sum of angles respectively, equal to those of A B C. Bisect, therefore, I L in M, and on it erect the perpendicular M N = A I; join A N, N C, cutting I L in O and P. Then, nearly in the same way as above, the triangles A I O, N M O, N M P, C L P, being found equal, first in pairs, and then all four; the triangle A N C is isosceles, and has its area equal to A I L C = A B C, and its three angles equal to the same two angles as before, or to the three angles of A B C.

In like manner, construct an isosceles triangle D Q F = D E F. Then the areas and bases of the two isosceles triangles A N C, D Q F, being respectively equal, there is no alternative but their angles must agree; for otherwise, a copy of the one triangle being constructed on the base of the other, their areas behoved to differ. Consequently, the triangles A B C, D E F, have the sums of their angles equal.

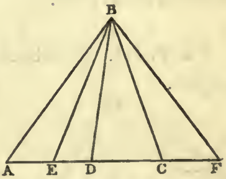
Case 2. When A B C, D E F (figs. 1 & 3) are any two equal triangles which have no equal bases; bisect D E, F E in G, H; and let A B C be the triangle which has the greatest side; then an arc having D as a centre, and a radius equal to half that side of A B C, will meet G H or its extension in some point I. Produce D I to double its length in K, join F K meeting G H in O, and to G H draw the perpendiculars DL, FM, KN. Then, as remarked above, D L = F M, and therefore, the triangles D I L, K I N, having two angles and a corresponding side respectively equal, we have also K N = D L = F M; and since, therefore, the triangles K N O, F M O, have likewise two angles and a corresponding side equal, L M bisects F K in O. Hence, as noticed under the first case, the triangle D K F has its area and the sum of its angles equal to those of D E F, since their sides are bisected by the same line. Again, by the first case, the triangles D K F, A B C, have the sums of their angles equal; for D K is equal to one side of A B C, and their areas are equal. Consequently, any two triangles A B C, D E F, whose areas are equal, have the sums of their angles equal.

Fig. 3.



Prop. II. If, in one triangle, the sum of the angles differed from 180°, so it would in every triangle; the difference would always have the same sign, and be proportional to the area.

Fig. 4.



Case 1. Let A B C be a triangle whose angles, if possible, fall short of 180°, and let its area be bisected by B D. Then each of these halves will (Prop. 1.) have the same amount of angles; but whether they were halves or unequal parts, their six angles would evidently be equal to the three angles of A B C, together with 180° at the point D: so that, the six angles of the two parts will always be less than 360° by the same quantity that the three angles of A B C are less than 180°. Consequently, the defect of half the sum of the six from 180° will just be half the defect of the three angles of A B C from 180°. In the same way, if B E bisect the area A B D, the angular defect of each part from 180° will be half of the defect for A B D, or one-fourth of that for A B C; and so on, for the continual bisection of the whole till each part be less than any given area.

But however unequal the two parts may be into which any one of such bisecting lines divides the whole or a part of the area A B C, the same relation will still subsist. For when the several parts of the areas and of the angular defects are proportional, so must any corresponding sums of such parts. Thus area E B C will be proportional to its angular defect.

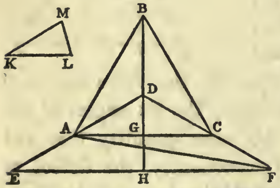
The like evidently follows, when, on the extension of any side of A B C, as, for instance, on that of A C produced to F, an addition B C F equal to one or more of the foresaid parts of A B C (which have their areas proportional to their angular defects) is made to the area A B C : so that, area A B F will likewise be proportional to its angular defect. Hence, in every triangle, the area and the angular defect follow the same proportion.*

Case 2. In the same way, it may be shewn that, if one triangle had the sum of its angles greater than 180°, so would every other, and that the excess (as in spherics) would be proportional to the area.

SCHOLIUM. Since it could still more readily be shewn, either as above or in several other ways, that if one triangle had the sum equal to 180°, so would every other; it is obvious that, in respect of 180°, every triangle has the sum of its angles of the same kind, whatever that may be. But there are several known methods of proving that the sum can never exceed 180°. The grand difficulty has always been equivalent to proving that it can never be less. Thus, since, however small might be the amount of some two angles of a triangle, we may always increase those two to be ever so little short of 180°, and yet, according to Euclid's 12th axiom, the sides which have been thereby altered, if produced, will continue to meet, forming a still greater triangle; it is evident that his axiom is equivalent to assuming that the three angles of even the greatest of triangles cannot be sensibly less than 180°. From this second proposition, too, would follow the converse of the first one, if ever the sum differed from 180°.

PROP. III. The three angles of every triangle are equal to two right angles.

Fig. 5.



Let A B C be any equilateral triangle; bisect its angles, which obviously will divide it into three equal triangles, each having an angle of 120° at the point D. Produce D A, D C to E and F, making A E = C F = ½ A C; join A F. Then each of the angles A C F, E A F being exterior in respect of A D C, exceeds this obtuse angle. Hence A F exceeds A C, whilst E F being greater than A F, is still greater than A C or twice A E. Produce B D to bisect A C in G, and E F in H. Let the lines K L, K M, each of which is equal A E, form an angle equal to half the least angle, which any equilateral triangle can ever have: for as long as no two sides of an equilateral triangle can coincide in one straight line, its angle must have some magnitude, and therefore so must the area

* Although, strictly speaking, such reasoning is only applicable to commensurable areas, it is more than sufficiently general and exact for the present purpose.

K L M. Now angle A E H being obviously half the angle of an equilateral triangle whose base would be E F, it is at least as great as angle L K M; and, therefore also, because E H exceeds E A, while the angle E A G being obtuse, and E H G a right angle, every straight line drawn from E to meet A G or G H, will exceed E A, since it will be the greatest side of a triangle having either E A or E H for another side; so that the quadrilateral A E H G will be more than capable of containing the triangle K L M, and so will the equal quadrilateral C F H G. It is also evident that by continually increasing D E, D F by parts each equal A E, and drawing other lines to join their extremities, the triangle E D F may be increased to exceed any given area, for it would thereby acquire an unlimited number of increments each greater than area K L M. But if one triangle had the sum of its angles less than 180° by the nth part of that quantity, another triangle whose area is n times as great, would (Prop. II.) have no angles at all, which is absurd, and more especially in the present case, where angle A D C is considerable.

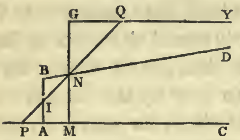
Such being a deduction from the premises in Prop. II., shews that the angles of a triangle can never be less than 180°, which is equivalent to proving Euclid's 12th Axiom, the one being so easily deduced from the other. But if one triangle had its angles greater than 180° by an nth part, another whose area is half of n times as great, would (Prop. II.) have its angles equal to 270°; and, consequently, some two of them would amount to at least 180°, which (Euclid I. 17) is impossible. Hence, there is no triangle whose angles exceed 180°; and, therefore, the three angles of every triangle are just equal to 180°.

The equilateral triangle in Fig. 5 might have been omitted, and a right angle used for A D C, but that would scarcely have conduced to greater brevity or simplicity. If anything could be reckoned to have been assumed above, it would be that the sum of the angles of a triangle can never be inappreciable or so small that it could not be multiplied to exceed any given angle. But if this cannot be regarded as already self-evident, yet the assumption absolutely necessary here to complete the proof would be smaller than any that could be assigned; and, therefore, Euclid's assumption of the angles of a triangle amounting to 180° would exceed the amount required here, in a ratio greater than any that could be assigned. Thus, whatever part of the 180° were supposed to be necessary here as the least sum of the angles; for example, though it were only the millionth, billionth, or any other assigned part, a still smaller would obviously suffice. Under the anticipated objection, therefore, the question would still be reduced within limits incomparably narrower than, so far as I am aware, it had ever been before; especially since most of the authors who have attempted it, assume more than even Euclid assumes.

I shall now endeavour to examine the professedly direct demonstrations depending on infinite quantities, as employed by Bertrand, in his *Developpement de la Partie Elementaire des Mathematiques*, and by Legendre, *Memoires de l'Academie, tome* xii. *p.* 367, (an elaborate essay, but seems to have escaped the notice of Col. Thompson). It will be found that these illustrious geometers have assumed the whole that was to be proved. The following is a theorem of Legendre:—If the indefinite straight lines A C, B D, (Fig. 6) be both perpendicular to A B, and if from any point N in B D we draw N M perpendicular to A C, then N M will be equal A B and perpendicular to B D.

Draw N I bisecting A B in I and meeting A C in P, and produce it till N Q = N P. Draw also the indefinite line Q Y, making angle N Q Y = P N D, and produce M N to meet Y Q in G. From these premises, M. Legendre readily proves that the triangles P I A, B N I are equal, and, consequently, that area D B A C = D N P C; and that angle C P N + P N D = D N Q + N Q Y = C P Q + P Q Y = 180° As readily does he prove the triangles P N M, N G Q to be equal, and therefore area Y Q P C = Y G M C.

Fig. 6.



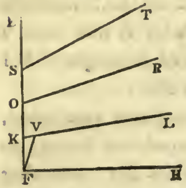
When any two indefinite straight lines—such as N D, P C—make, with the same side of a third straight line, P N, the two interior angles C P N, P N D equal to 180°, Legendre calls the figure D N P C a *biangle.* Now it is evident that in the above construction we have other four biangles. But, unfortunately, through some sad oversight, M. Legendre in effect makes it to contain one or two besides. Thus, without having proved anything whatever regarding the value of angle M N D, he not only calls C M N D a biangle, but reasons upon it as such, or as having two right angles, which obviously is the same as just at once assuming M N D = 90°; and it is on this ground he concludes that M N = A B. Then, by *reasoning in a circle* from this conclusion, he proves what, as just stated, he had already assumed, namely, that M N D = 90°. The direct demonstration of Legendre is, therefore, a total failure.

But since area C M N D is less than D N Q Y by the sum of the triangles P N M, G N Q, the angle G N D cannot be acute; because then C M N D, instead of being less than D N Q Y, would exceed it by the infinite area of an angle equal to the excess of angle M N D over G N D, which is absurd. Were reasoning upon infinite quantities liable to no objection, this absurdity obviously would amount to an indirect proof that the angles of the quadrilateral A B N M cannot be less than 360°, or that those of a triangle cannot be less than 180°.

MM. Bertrand and Legendre regard a biangle as an infinite area of the first order, and maintain that it bears no proportion to, and so could never by repeated subtraction exhaust the area of an angle, which they reckon an infinite of the second order. But I shall now shew that this doctrine, which is the foundation of Bertrand's demonstration, can only be maintained on the assumption that the angles of a triangle amount to

180°, or those of a quadrilateral to 360°, which is just the whole affair to be proved. For let E F H (Fig. 7) be a right angle, and H F K L a biangle every way equal to C A B D (Fig. 6) and which will also be equal to area C M N D, if, as Legendre assumes, the angle M N D = 90°. In that case, too, the area E K L would be just as great as the original area E F H, and the rest of the process would seem equally satisfactory.

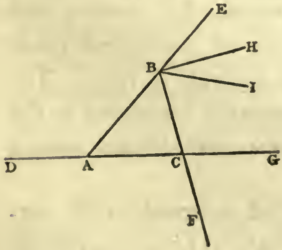
Fig. 7.



But if angle B N M be acute, and of course M N D obtuse, it is easy to shew how an area equal to C M N D, or one much less, may be taken from E F H, so as to diminish that infinite area, and, by repetition, exhaust it altogether. Thus, whether F K be taken as great as M N, or in any ratio less, if we only make angle E K L equal to the acute angle G N D, the area H F K L may be equal to or much less than C M N D, and yet the area E K L will evidently be less than area E F H in the same ratio that the acute angle E K L is less than E F H. Draw F V, making angle H F V = L K E; then the sum of the angles H F V, F V L will evidently exceed the sum of H F K, F K L by whatever the three angles of the triangle F V K want of 180°; for if in one case the angles are deficient, so (Prop. II.) they must in every other. If, therefore, K O be taken of any magnitude not exceeding F V, and angle K O R be made equal F V L, the difference between angle E O R and E K L will exceed the difference between E K L and E F H by whatever the three angles of F V K want of 180°. Hence, by continuing the like construction, the residuary angles which the upper sides of the successive lines make with E F, will continually decrease with increasing differences, till they vanish altogether, and thereby exhaust the whole area E F H. Thus, Bertrand's famous demonstration fails entirely, unless we first assume that the angles of every triangle amount to at least 180°.

Another demonstration by Bertrand, and which, like the two just noticed, is highly commended by Legendre, amounts to this: Since the entire indefinite area around a point A, only differs from the sum of the infinite areas of the exterior angles D A E, E B F, F C D of any triangle A B C, by the finite area of that triangle itself; which is supposed to bear no proportion to the indefinite area of the plane; it is, therefore, concluded that the sum of the angles around the point A is just equal to the sum of these exterior angles. But such reasoning, in this case, is as faulty as in the two preceding, where the doctrine that a finite quantity has no effect on an infinite, was found to fail, unless we first assumed the very thing to be proved, that the angles of a triangle cannot be less than 180°. For if they may be less, produce A C

Fig. 8.



towards G, and make angle E B H = E A G. Then the two angles G C B, C B H will exceed 180° by whatever the three angles of the triangle A B C fall short of 180°; and, consequently, after the finite area A B C has been taken from the infinite E A G, instead of the remainder E B C G being still equal to or less than area E A G, it will exceed area E B H = E A G, by more than the infinite area of an angle H B I equal to the defect of the three angles of A B C from 180°.

This absurdity, while it shews the fallacy of the direct reasoning, would amount, as in the former case, to an indirect proof that the angles of a triangle cannot be less than 180°, were it not that the reasoning still involves infinite quantities, which we have just seen are apt to mislead the greatest of geometers.

On the Light thrown on Geology by Submarine Researches; being the Substance of a Communication made to the Royal Institution of Great Britain, Friday Evening, the 23d February 1844. By EDWARD FORBES, F.L.S., M.W.S., &c. Prof. Bot. King's College, London. Communicated by the Author.

About the middle of the last century, certain Italian naturalists* sought to explain the arrangement and disposition of organic remains in the strata of their country, by an examination of the distribution of living beings on the bed of the Adriatic Sea. They sought in the bed of the present sea for an explanation of the phenomena presented by the upheaved beds of former seas. The instrument, by means of which they conducted their researches, was the common oyster-dredge. The results they obtained bore importantly on Geology; but since their time, little has been done in the same line of research,—the geologist has been fully occupied above water, and the naturalist has pursued his studies with far too little reference to their bearing on geological questions, and on the history of animals and plants *in time.* The dredge, when used, has been almost entirely restricted to the search after rare animals, by the more adventurous among zoologists.

Convinced that inquiries of the kind referred to, if conducted with equal reference to all the natural history sciences,

* Marsili and Donati, and after them Soldani.

and to their mutual connection, must lead to results still more important than those which have been obtained, I have, for several years, conducted submarine researches by means of the dredge. In the present communication, I shall give a brief account of some of the more remarkable facts and conclusions to which they have led, and as briefly point out their bearings on the science of geology.

I. *Living beings are not distributed indifferently on the bed of the sea, but certain species live in certain parts, according to the depth, so that the sea-bed presents a series of zones or regions, each peopled by its peculiar inhabitants.*—Every person who has walked between high and low water-marks on the British coasts, when the tide was out, must have observed, that the animals and plants which inhabit that space, do not live on all parts of it alike, but that particular kinds reach only to certain distances from its extremities. Thus the species of *Auricula* are met with only at the very margin of high water mark, along with *Littorina cœrulescens*, and *saxatilis*, *Velutina otis*, *Kellia rubra*, *Balani*, &c.; and among the plants, the yellow *Chondrus crispus* (*Carrigeen*, or Iceland moss of the shops), and *Corallina officinalis*. These are succeeded by other forms of animals and plants, such as *Littorina littorea*, *Purpura lapillus*, *Trochi*, *Actineæ*, *Porphyra laciniata* (Laver, Sloke), and *Ulvæ*. Towards the margin of low water, *Lottia testudinaria*, *Solen siliqua*, and the Dulse, *Rhodomenia palmata*, with numerous Zoophytes and Ascidian molluscs, indicate a third belt of life, connected, however, with the two others, by certain species common to all three, such as *Patella vulgata*, and *Mytilus edulis*. These sub-divisions of the sea-bed, exposed at ebb-tide, have long attracted attention on the coasts of our own country, and on those of France, where they have been observed by Audouin and Milne Edwards, and of Norway, where that admirable observer Sars has defined them with great accuracy.

Now this subdivision of the tract between tide-marks into zones of animal life, is a representation in miniature of the entire bed of the sea. The result of my observations, first-in the British seas,* and more lately in the Ægean, has been to define a series of zones or regions in depth, and to ascertain *specifically* the animal and vegetable inhabitants of each. Regarding the tract between tide-marks as one region, which I have termed the *Littoral Zone,* we find a series of equivalent regions, succeeding it in depth. In the British seas, the littoral zone is succeeded by the region of Laminariæ, filled by forests of broad-leaved Fuci, among which live some of the most brilliantly coloured and elegant inhabitants of the ocean. This is the chosen habitat of *Lacunæ*, of *Rissoæ*, and of *Nudibranchous mollusca.* A belt generally of mud or gravel, in which numerous bivalve mollusca live, intervenes between the laminarian zone (in which the Flora of the sea appears to have its maximum), and the region of Corallines, which, ranging from a depth of from 20 to 40 fathoms, abounds in beautiful flexible zoophytes and in numerous species of Mollusca and Crustacea, to be procured only by means of the dredge. The great banks of Monomyarious Mollusca, which occur in many districts of the Northern Seas, are for the most part included in this region, and afford the zoologist his richest treasures. Deeper still is a region as yet but little explored, from which we draw up the more massy corals found on our shores, accompanied by shellfish of the class *Brachiopoda.* In the Eastern Mediterranean (where, through the invaluable assistance afforded by Captain Graves, and the Mediterranean Survey, I have been enabled to define the regions in depth, to an extent, and with a precision which, without similar aid, cannot be hoped for in the British seas), between the surface and the depth of 230 fathoms, the lowest point I had an opportunity of examining, there are eight well-defined zones, corresponding in part, and presenting similar characters with those which I have enumerated as presented by the sea-bed in the North. The details of these will be given in the forthcoming volume of the Transactions of the

* The first notice of these was published in the Edinburgh Academic Annual for 1840.

British Association, to which body I had the honour of presenting a report on the subject, at the last meeting.

When we examine the distribution and association of organic remains, in the upheaved beds of tertiary seas, we find the zones of depth as evident as they are in the present ocean. I have proved this to my own satisfaction, by a minute comparison of the newer Pliocene strata of Rhodes, where that formation attains a great thickness, with the present state of the neighbouring sea, and carrying on the comparison through the more recent tertiaries with the more ancient, have found indubitable evidences of the same phenomena. The strata of the cretaceous system yield similar evidences, and doubtless, in all time, the element of depth exercised a most important influence in regulating the distribution of animal life in the sea. If so, as our researches extend, we may hope eventually to ascertain the probable depth, or, at any rate, the region of depth, in which a given stratum containing organic remains was deposited. Every geologist will at once admit, that such a result would contribute materially to the history of sedimentary formations, and to the progress of geological science.

II. *The number of species is much less in the lower zones than in the upper. Vegetables disappear below a certain depth, and the diminution in the number of animal species indicates a zero not far distant.*—This conclusion is founded on my Ægean researches. Vegetables become fewer and fewer in the lower zones; and dwindle to a single species,—a *nullipora*, at the depth of 100 fathoms. Although the lower zones have a much greater vertical range than the higher, the number of animal species is infinitely greater in the latter. The lowest region (the 8th) in the Mediterranean, exceeds in extent all the other regions together; yet its fauna is comparatively small, and at the lowest portion explored, the number of species of testacea found was only eight. In the littoral zone, there were above 150 species. We may fairly infer, then, that as there is a zero of vegetable life, so is there one of animal life. In the sea, the vertical range of animals is greater than that of vegetables;—on the land, the reverse is the case. The geological application of this fact, of a zero of life in the ocean, is evident. All deposits formed below that zero, will be void, or almost void, of organic contents. The greater part of the sea is far deeper than the point zero; consequently, the greater part of deposits forming, will be void of organic remains. Hence we have no right to infer that any sedimentary formation, in which we find few or no traces of animal life, was formed either before animals were created, or at a time when the sea was less prolific in life than it now is. *It might have been formed in a very deep sea.* And that such was the case in regard to some of our older rocks, such as the great slates, is rendered the more probable, seeing that the few fossils we find in them, belong to tribes which, at present, have their maximum in the lowest regions of animal life, such as the Brachiopoda, and Pteropoda, of which, though free swimmers in the ocean, the remains accumulate only in very deep deposits. The uppermost deposits, those in which organic remains would be most abundant, are those most liable to disappear, in consequence of the destroying action of denudation. The great and almost nonfossiliferous strata of Scaglia, which form so large a part of the south of Europe and of Western Asia, were probably, for the most part, formed below the zero of life. The few fossils they contain, chiefly nummulites, correspond to the foraminifera which now abound mostly in the lowest region of animals. There is no occasion to attribute to metamorphic action the absence of traces of living beings in such rocks.

III. *The number of northern forms of animals and plants is not the same in all the zones of depth, but increases either positively, or by representation, as we descend.*—The association of species in the littoral zone is that most characteristic of the geographical region we are exploring; but the lower zones have their faunas and floras modified by the presence of species which, in more northern seas, are characteristic of the littoral zones. Of course, this remark applies only to the northern hemisphere; though, from analogy, we may expect to find such *inversely* the case also in the southern. The law, put in the abstract, appears to be, that *parallels in* *depth are equivalent to parallels in latitude*, corresponding to a well-known law in the distribution of terrestrial organic beings, viz. that *parallels in elevation are equivalent to parallels in latitude :* for example, as we ascend mountains in tropical countries, we find the successive belts of vegetation more and more northern or southern (according to the hemisphere) in character, either by identity of species, or by representation of forms by similar forms ; so in the sea, as we descend, we find a similar representation of climates in parallels of latitude in depth. The possibility of such a representation has been hypothetically anticipated in regard to marine animals by Sir Henry De La Beche,* and to marine plants by Lamouroux. To me it has been a great pleasure to confirm the felicitous speculations of those distinguished observers. The fact of such a representation has an important geological application. It warns us that all climatal inferences drawn from the number of northern forms in strata containing assemblages of organic remains, are fallacious, unless the element of depth be taken into consideration. But the influence of that element once ascertained (and I have already shewn the possibility of doing so), our inferences assume a value to which they could not otherwise pretend. In this way, I have no doubt, the per-centage test of Mr Lyell will become one of the most important aids in geology and natural history generally ; and, in fact, the most valuable conclusions to which I arrived by the reduction of my observations in the Ægean, were attained through the employment of Mr Lyell's method.

IV. *All varieties of sea-bottom are not equally capable of sustaining animal and vegetable life.*—In all the zones of depth there are occasionally more or less desert tracts, usually of sand or mud. The few animals which frequent such tracts are mostly soft and unpreservable. In some muddy and sandy districts, however, worms are very numerous, and to such places many fishes resort for food. The scarcity of remains of testacea in sandstones, the tracks of worms on ripple-marked sandstones, which had evidently been deposited in a

* Ten years ago, in his "Researches in Theoretical Geology."

shallow sea, and the fish remains often found in such rocks, are explained, in a great measure, by these facts.

V. *Beds of marine animals do not increase to an indefinite extent. Each species is adapted to live on certain sorts of sea-bottom only. It may die out in consequence of its own increase changing the ground.*—Thus, a bed of scallops, *Pecten opercularis*, for example, or of oysters having increased to such an extent that the ground is completely changed, in consequence of the accumulation of the remains of dead scallops or oysters, becomes unfitted for the further sustenance of the tribe. The young cease to be developed there, and the race dies out, and becomes silted up or imbedded in sediment, when the ground being renewed, it may be succeeded either by a fresh colony of scallops, or by some other species or assemblage of species. This "rotation of crops," as it were, is continually going on in the bed of the sea, and affords a very simple explanation of the alternation of fossiliferous and nonfossiliferous strata; organic remains in rocks being very rarely scattered through their substance, but arranged in layers of various thickness, interstratified with layers containing few or no fossils. Such interstratification may, in certain cases, be caused in another way, to-wit, by the elevation or subsidence of the sea-bottom, and the consequent destruction of the inhabitants of one region of depth, and the substitution of those of another. It is by such effects of oscillation of level, we may account for the repetition, at intervals, in certain formations of strata indicating the same region of depth.

VI. *Animals having the greatest ranges in depth have usually a great geographical, or else a great geological range, or both.*—I found that such of the Mediterranean testacea as occur both in the existing sea, and in the neighbouring tertiaries, were such as had the power of living in several of the zones in depth, or else had a wide geographical distribution, frequently both. The same holds true of the testacea in the tertiary strata of Great Britain. The cause is obvious: such species as had the widest horizontal and vertical ranges in space, are exactly such as would live longest in time, since they would be much more likely to be independent of catastrophes and destroying influences, than such as had a more limited distribution. In the cretaceous system, also, we find that such species as lived through several epochs of that era, are the few which are common to the cretaceous rocks of Europe, Asia, and America. Count D'Archiac and M. De Verneuil, in their excellent remarks on the fauna of the Palæozoic rocks, appended to Mr Murchison and Professor Sedgwick's valuable memoir on the Rhenish Provinces, have come to the conclusion that the fossils common to the most distant localities, are such as have the greatest vertical range. My observations on the existing testacea and their fossil analogues, lead to the same inference. It is very interesting thus to find a general truth coming out, as it were, in the same shape, from independent inquiries at the two ends of time.

VII. *Mollusca migrate in their larva state, but cease to exist at a certain period of their metamorphosis, if they do not meet with favourable conditions for their development;* i. e. *if they do not reach the particular zone of depth in which they are adapted to live as perfect animals.*

This proposition, which, as far as I am aware, is now put forward for the first time, includes two or three assertions which require explanation and proof, before I can expect the whole to be received. First, *that mollusca migrate.* In the fourth volume of the Annals of Natural History (1840), I gave a zoo-geological account of a shell-bank in the Irish Sea, being a brief summary of the results of seven years' observations at a particular season of the year. In that paper, I made known the appearance, after a time, of certain mollusca on the coasts of the Isle of Man, which had not previously inhabited those shores. They were species of limpet, about which there could be no mistake, and one was a littoral species. At that time, I could not account for their appearance. Many similar facts have since come to my knowledge, and fishermen are familiar with what they call " shifting" of shell-beds, which they erroneously attribute to the moving away and swimming off of a whole body of shell-fish, such as mussels and oysters. Even the *Pectens*, much less the testacea just named, have very little power of progressing to any distance, when fully developed. The " shifting" or migration is accomplished by the young animals when in a larva state. This brings me to a second point, which needs explanation. *All mollusca undergo a metamorphosis* either in the egg, or out of the egg, but, for the most part, among the marine species out of the egg. The relations of the metamorphoses of the several tribes are not yet fully made out; but sufficient is now known to warrant the generalization. In one great class of mollusca, the *Gasteropoda*, all appear to commence life under the same form, both of shell and animal, viz. a very simple, spiral, helicoid shell, and an animal furnished with two ciliated wings or lobes, by which it can swim freely through the fluid in which it is contained. *At this stage of the animal's existence, it is in a state corresponding to the permanent state of a Pteropod*,* and the form is alike whether it be afterwards a shelled or shell-less species. (This the observations of Dalyell, Sars, Alder and Hancock, Allman, and others prove, and I have seen it myself.) It is in this form that most species migrate, swimming with ease through the sea. Part of the journey may be performed sometimes by the strings of eggs which fill the sea at certain seasons, and are wafted by currents. My friend, Lieut. Spratt, R.N., has lately forwarded me a drawing of a chain of eggs of mollusca, taken eighty miles from shore, and which, on being hatched, produced shelled larvæ of the forms which I have described. If they reach the region and ground, of which the perfect animal is a member, then they develope and flourish; but if the period of their development arrives before they have reached their destination, they perish, and their fragile shells sink into the depths of the sea. Millions and millions must thus perish, and every handful of the fine mud brought up from the eighth zone of depth in the Mediterranean, is literally filled with hundreds of these curious exuviæ of the larvæ of mollusca.†

* It is not improbable that the form of the larva of the Pteropod, when it shall be known, will be found to be that of an Ascidian polype, even as the larva of the Tunicata presents us with the representation of a hydroid polype.

† The nucleus of the shells of the Cephalopoda is a spiral-univalve

Were it not for the law which permits of the development of these larvæ only in the region of which the adult is a true native, the zones of depth would long ago have been confounded with each other, and the very existence of the zones of depth is the strongest proof of the existence of the law. Our confidence in their fixity, which the knowledge of the fact *that mollusca migrate* might at first shake, is thus restored, and with it our confidence in the inferences applicable to geology which we draw from submarine researches.

Some of the facts advanced in this communication are new, some of them have been stated before: but all, for which no authority is given, whether new or old, are put forth as the results of personal observation.

The Geological Arrangement of ancient Strata deduced from the condition of the present Oceanic Beds. By William Rhind, Esq.

While the conditions of the atmosphere and of the earth's strata have received their full share of the attention of naturalists, comparatively little has been done to investigate the state of the ocean. We still require much information regarding its depth, temperature, currents, and the localization of its plants and animals.

With the exception of the continents and islands scattered over its surface, the ocean forms a continuous zone of fluid encompassing the globe, and varying in depth from a few hundred feet to perhaps four or five miles. Like the atmosphere, its particles are in continual motion; some portions being rarefied by heat, and others condensed by cold; while regular currents, produced chiefly by the unequal temperatures of high and low latitudes, continually cause a movement of its waters, and a tendency to an equalization of its general temperature. From the laws which regulate fluids, however, its mean temperature is more uniform and steady than that of the air, and there is less difference between its polar and equatorial temperature than that which exists in the atmosphere.

Accurate experiments on the temperature of the ocean in different la-

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similar in form to the undeveloped shells above alluded to, and it is yet to be seen whether all Cephalopoda do not commence their existence under a *spiral-shelled Pteropodous form.*

titudes, and at different depths, are still wanting. We give the following results:

Lat.	*Temp. of Surface.*	*Depth.*	*Temp.*	
3° 26′ S.	73°	1000 fath.	42°	Wauchope.
20° 30′ N.	83°	1000 ...	45°5	Sabine.
9° 21′ N.	83°	250 ...	77	Kotzebue.
0 Equat.	83°	300 ...	55	Kotzebue.

Mean reduction of temperature in Tropics, 1° in 25 fathoms.

Lat.	*Temp. of Surface.*	*Depth.*	*Temp.*	
36° 9′ N.	71°9	100	52°8	Kotzebue.
30° 39′ S.	67	300	44°	Kotzebue.
44° 17′ S.	54°9	196	38°8	Kotzebue.

Mean reduction of temperature in Temperate Zone, 1° in 28 fathoms.

It thus appears that, within the range of the tropical and temperate zones, there is a decrease of the temperature from the surface downwards of one degree in every 25 to 28 fathoms. In colder regions, when the temperature is at or below 38°, the cold surface water, being heavier, sinks, while a warmer stratum rises from below to supply its place.*

Peron found the temperature of the ocean to decrease gradually from the surface downwards, and Ellis found it stationary at the depth of a mile.

On the whole, then, it may be assumed as a general rule, that, in tropical and temperate regions, the ocean decreases in temperature from the surface downwards, that this decrease is greatest in the upper portions, while, at certain depths, the temperature may remain almost stationary. In temperate and tropical latitudes the shallowest parts of the ocean are the warmest; in this way, the waters of estuaries and long ranges of littoral shallow seas will be of higher temperature than the deeper portions, or even than shallow reefs far out at sea. Inland seas, where there are no tides, will also vary in temperature according to latitude; in low latitudes, such seas as the Baltic will be colder than the ocean. It will thus appear that the ocean, like the atmosphere, has its zones of temperature, but in the reverse order, the upper portions of the waters of the ocean being warmest; while those next the earth are of a lower temperature. The upper zones of the ocean are full of life, both vegetable and animal; but, like the atmosphere, there is a limit, beyond which, neither plants nor animals will be found. This limit is caused not alone by diminished temperature, but also by diminished light, and probably by other causes, such as increased density, and the deficiency of atmospheric air and influence. We do not, as yet, exactly know the depth at which marine plants and animals totally disappear; but the fact is established beyond all doubt, as soundings made in deep water, and at considerable distances from the shore, bring up mud or comminuted rocks, but no traces of organic beings. It is probable that this depth varies according to latitude.

* Scoresby, Ross.

It seems to be now also established, that marine animals take up their localities at depths, varying according to their classes and genera, and even their species. Every one who has dredged for mollusks must have remarked that particular species are only to be found in particular localities, and Professor E. Forbes, in a paper read before the last meeting of the British Association, has shewn, from his researches in the Ægean Sea, that there are about eight regions in that inland gulph, varying from a depth of 2 to 230 fathoms, tenanted by its particular groups of mollusca and radiata. These zones or regions are, in many respects, Mr Forbes remarks, equivalent to parallels of latitude in regard to the distribution of species.

There is every reason to believe, that fishes also observe a similar arrangement with regard to their localities; for, in a general way, these inhabitants of the ocean have for a long time been divided into littoral and pelasgic, as well as the testaceous molluscs.

We know, indeed, that many families of fishes are strictly confined to particular localities. Some inhabit the deep seas, as the cod and haddock; some the flat shallow sands, as the rays and other flat fish; while numerous species are confined to the shallow shores of bays and estuaries. On the whole, then, we shall have, perhaps, a not inaccurate idea of the condition of the ocean, if we compare a descending section of it to a tropical mountain, which we find divided into zones of temperature, each zone being characterized by distinct groups of plants and animals.

Now, such an arrangement in our existing seas appears to us to furnish a very simple and adequate explanation of the condition of our ancient geological strata. For when we find a series of such strata containing distinct beds of fossiliferous remains, grouped in regular and successive order, we at once perceive that they are thus arranged according to the localities which the living organisms maintained in their contemporaneous ocean, and that this localization is entirely due to the respective depths of water in which each species of animal was accustomed and adapted to live.

On this view of the subject, we can readily explain why the upper series of strata are so full of organic life,—why, as we descend, that organic beings become more and more rare, till at last we arrive at a line where all traces of plants and animals cease.

Suppose that in the present day some great eruption of plutonic rocks were to elevate a portion of our seas—the basin of the Forth, for instance, extending in a line outwards into the deep sea, we should then have in the upheaved strata an illustration of the successive localization of its marine inhabitants. In what would be called the upper or newer strata, we should find fluviatile fishes and mollusks imbedded in calcareous mud or shale; to these would succeed beds swarming with marine testacea; farther on, amidst the sandstone debris and conglomerate, the various species of rays and other flat fishes would present themselves;

still farther out into the deep sea, the cod and haddock would prevail, till at last, in the schistose strata formed in the still depths of the ocean, we would in vain look for any traces of animal existence.

We can thus explain, too, why strata of considerable thickness, having the same mineral character throughout, apparently formed under similar circumstances, and within one epoch, should yet differ materially in the specific characters of the inhabitants of its upper and lower beds. The difference of a few hundred feet in the depth of water; and, consequently, in the temperature, and other circumstances, being sufficient to influence the instincts of the animals by which the beds are tenanted. Agassiz found no species of fish common to two formations, although the same genera are distributed among several; this is also the case with molluscous testacea. In general, the species are confined to distinct beds, although several genera have a wide range over different strata. Thus, trilobites, goniatites, and pennatulæ, are found only in the oldest or deepest fossiliferous strata; productæ and spiriferæ range through the middle strata; terebratulæ have a still wider range, up to the chalk, and the family of ammonites seem also to have been much dispersed, although they center most numerously in the oolitic series.

Although the same arrangement of fossil animals, in regard to super-position, appears to be very uniformly the same in every part of the earth hitherto explored, yet the mineral characters of the equivalent fossiliferous strata are not always similar; so that the presence of particular fossils is no test of the mineral characters of rocks. This character of sedimentary strata is dependent upon the type of the older rocks, out of which they have been formed, and hence may vary in different localities. In the same way, strata may be mineralogically alike, and yet differ essentially in their fossils. Thus, we have various denominations of sandstone, according to the depths at which the deposits were made, which, although mineralogically identical, are tenanted by different genera of fossils. The same occurs in calcareous and aluminous deposits.

Neither can the opinion any longer be tenable that fossils are the key to the relative ages of strata, unless in those cases where there is an actual super-position of one series of strata on another, and even in those cases there may have been a contemporaneous formation of strata by a gradual and nearly equable extension of the upper and lower beds from the shores out into the deep sea. In cases where there is no super-position, it is sufficiently evident that the schistose depositions, called primary, may have been going on accumulating in the depths of the ocean, at the same period at which the lias and oolites were forming in the shallower seas.

By the same process of reasoning, it will appear evident, that it was possible for the whole range of fossil animals to have been contemporaries in the same primeval ocean; and yet, that not even a single species should have obtruded upon the appropriate domains of another.

By a reference to the present condition of the ocean, too, some light may be thrown on the mode of formation of the ancient strata.

Thus, the upper portions of the ocean are much agitated by currents and winds, while the lower portions remain comparatively calm and undisturbed. Gneiss, and the so-called primary schists, have evidently been deposited at great depths where the water was still; hence their regular lamination of structure, the minute and uniform comminution of their particles, and the still and placid manner in which these particles have been deposited.

The transition series again marks a period of commotion and turbulence, of rocks violently broken up into fragments, and of water-worn pebbles, transported by currents, and accumulated in frequent layers of varied conglomerates. The same observations will apply to the older sandstones, while the calcareous mud of the lias points out the action of currents, carrying the lighter detritus of rivers a considerable way into the deeper sea. The oolites, again, are still more littoral; and being formed nearer the genial surface of the water, afford a suitable bed for those numerous species of melaniæ or infusory tribes, of which the spherical portions of this rock are, according to Ehrenberg, supposed to be chiefly composed, and which impart to it its peculiar character.

The mountain limestone-beds were most probably reefs of corals and encrinites rising nearly to the surface, but constituting a locality different in temperature and other respects, from the more littoral oolite. Over these reefs, probably after they have suffered depression, or some other change, the coal-measures appear to have been deposited. These carboniferous beds, extending in thickness from 2000 to 3000 feet, appear to be an exception to the other marine strata in this respect, that there is a uniformity of fossils throughout, the same organic remains appearing in the lower beds as in the upper. This is to be accounted for from the circumstance that the fossils are almost all vegetables which have grown on the earth's surface, and must have been successively transported into their position by fluviatile currents from the land; or, that part, if not the whole, may have successively grown on the surface of the same locality on which they are now found, and that this surface has repeatedly been submerged by the gradual or periodical sinking of the strata below. A few fluviatile shells and fishes are occasionally interspersed, as also layers of marine limestone, with fossils; but the different species do not assume that successive position which the true marine-beds uniformly do, even in strata of the thickness of a few hundred fathoms. This exception of the coal strata to the general law appears to afford an interesting test of the mode of arrangement which is universally prevalent in all the other series. The presence or absence of any one of the series of geological formations may also be readily accounted for from the depth or shallowness of the ancient ocean on any given locality. Thus, the prevalence of the oolitic and tertiary formations in the region of the Alps, and, indeed, in the whole centre of the continent of Europe, implies a shallow state of the ancient seas there. The same appears to have been the case in the south of England, while the sea would appear to have gradually deepened to the north-west of England, and continued deepening onwards throughout Scotland. In South America, according to Elie de Beaumont, there is an entire absence of the oolitic series, while the gneiss, schistose, and silurian systems are extensively diffused. The chalk and tertiary strata are also extensive, thus shewing a sea with deep and shallow bottoms, but a deficiency of a middle level. It frequently occurs that some of the higher formations, as the chalk or oolite, repose directly upon gneiss or schistose strata; this may arise from an elevation of these deeper beds at once to the natural level of the higher strata, without affording an opportunity for the formation of intermediate beds, or, rather, a fit locality for the peculiar animals which inhabit such.

When any of the strata are thus raised above their appropriate levels, it may be supposed that their inhabitants, or those of them that have escaped destruction, immediately retire to lower levels.

It is a circumstance frequently remarked by geologists, that fossil remains, especially of fishes, are found only at particular points, as, for instance, in a seam of shale of a few inches thick, while above this shale many hundred feet of the same strata may exist without a trace of any organism. This may indicate a convulsion of the oceanic bottom by which myriads of fishes were entombed at once; while, at the same time, the stratum was depressed far below its proper level, by which means the detritus afterwards deposited was at too great a depth to be tenanted by living beings. Or the super-imposed detritus may have been suddenly drifted and accumulated over the shale, without any great change of level. This drifting by currents or convulsions of the ocean appears to have been the cause of many of the vast accumulations of marine sandstones which are not unfrequently visible; and which, for many hundred feet in depth, exhibit few or no traces of organised bodies; and even such as are found, consist only of the detached scales or bones of fishes, as if the bodies to which they had belonged had been broken up and destroyed by the violent action of the waters.

Although depressions of strata may occasionally take place, yet it is evident that such are of much less frequent occurrence than elevations, because we very rarely, indeed, meet with a reversal of the order of position which the labours of modern geologists have so successfully established as generally, we may say almost universally, existing. Indeed, it is astonishing to find with what accuracy fossil remains preserve their respective positions in the earth's strata in every region of the globe which has yet been explored by the geologist. Thus, the equivalents of the British strata, containing almost identically the same fossils, have been found on the continent of Europe, in Asia, in America, in the remotest lands of the Pacific Ocean; nay, even in the now frigid climates of the Arctic and Antarctic circles.

This uniformity of its inhabitants would seem to indicate a uniformity of temperature, and other conditions of the primeval ocean, differing from those which prevail in the present seas, and a difference in distribution of animal life over the various regions of the earth.

We shall not here enter into the changes which marine animals, and especially testaceous molluscs, undergo, in consequence of differences of locality, temperature, and other external circumstances, because this is a subject which belongs more properly to considerations regarding the past and present temperature of the general surface of the globe. It may be remarked, however, that such external circumstances give rise to changes, in many cases amounting even to specific differences, in the appearance of the animals. Thus, the testaceous inhabitants of inland seas and gulphs are pigmies compared to their congeners of the open ocean;* and not only size, but colour, and even form of shell, may be changed. In those raised beaches which are so common in Scotland, and on many of the coasts of the European continent, testaceæ are found apparently with specific differences from those existing in the contiguous seas. Thus, on the banks of the Clyde, species have been found having more of an arctic character than those of the same family at present existing in that estuary.† Now, this may have arisen simply from a change of level in the locality, altering, in some slight degree, the temperature, such as a rise of the channel of the Frith, and a consequent shallowing of the water. In this way, the depth of the waters of the Clyde may have been formerly such as to permit of localities more approaching in temperature to that of the friths of arctic regions.

If the above conclusions are found to be based on sound deductions, our geological systems will, at least, require somewhat of revisal.

In the first place, the mineralogical character of rocks will be that on which their proper arrangement and classification must depend. Thus, if calcareous mud, brought down by currents, is deposited at a certain depth in the ocean, it becomes tenanted by producti and ganoid fishes. The same mud, if deposited at a higher level, is taken possession of by belemnites and ammonites; in the one case, it is called carboniferous limestone, in the other lias.

The present nomenclature, too, of primary, secondary, and tertiary, can, in many cases, only convey erroneous impressions. The same may be said of many other terms pointing out a precedency of formation or relative age.

Organic remains will henceforth indicate the respective, and, perhaps, by analogy with living species, the actual depth at which the strata in which they are contained were situated in the primeval ocean. They are

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* E. Forbes, Esq. † James Smith, Esq., Jordanhall, Edin. Phil. Journal.

not the true tests of the relative ages of strata; nor has the Trilobite, inhabiting the profound depths of the ocean, any title to priority of birthright over the humble Cardium, which burrows among the superficial sands that lie barely within tide-mark. Strata of high and low levels may be found in immediate contact, without the aid of supposititious denudation of intermediate strata.

Any one or more of the geological formations may be entirely awanting in certain localities to a great extent, their places being supplied by formations of a higher or lower level.

The whole series of geological formations could never be present in every portion of the earth's surface as long as there existed an ocean varying in depth.

Remarks on the Entomology of Angola with reference to the Geographical Distribution of Insects in Africa. By Professor Erichson of Berlin.

A careful and attentive examination of the manner in which the natural products of Africa are distributed, appears to us the only safe method by which we may establish our inferences regarding the condition of the unknown interior of this colossal Continent, on a broader and more sold basis than can be expected from comparisons with other parts of the world, similarly formed and situated. The comparatively small quantity of water transmitted to the sea from such an extent of country, and which, contrasted with the numerous and gigantic streams of Asia and South America, seems very inconsiderable, has long been a matter of surprise. Nevertheless, some geographers imagine that the desert-girt interior of Africa is traversed by immense mountain chains. It becomes of course necessary to suppose, that the greater portion of the atmospheric vapours, condensed by the action of these great mountains, is carried to inland-lakes, the size of which must be so considerable, that their surface of vaporization will correspond with the quantity of water received, since they have no communication with the sea. We readily confess that this view presents many interesting points, but it is impossible to reconcile it with the manner in which the various species of animals are distributed over Africa. The circumstance, that several species, for instance the ostrich, have spread from the deserts of Arabia, and from the foot of the Atlas Mountains, as far as the Cape of Good Hope, is strongly in favour of the hypothesis, that the whole of Central Africa, so far from being traversed by chains of lofty mountains, presents nothing but a series of sandy plains and deserts. The existence of the former would certainly prove an insurmountable barrier to the ostrich, though we are less positive with regard to the similarly distributed lion and elephant.

Our argument derives, however, the most solid and the most substantial support from an examination of the manner in which the various Faunas are distributed over the Continent of Africa. Generally speaking, the want of variety amongst them is very striking. In Asia the towering mountains of the Himalayah distinctly separate the Indian Fauna from that of Central and Northern Asia; and their respective Faunas are so unlike each other, that they scarcely admit of comparison. In a similar manner, the Andes of South America divide the Faunas of the eastern districts from those of the western provinces; and it is in vain to look for points of resemblance between the two. This is certainly not the case in Africa. The Faunas of Nubia and of Senegambia are materially the same, even as regards the identity of a vast number of species. So close a relationship between the Faunas of opposite coasts could hardly be accounted for, on the supposition that immense masses of mountain ranges, like those of the Himalayah, of the Andes, or even of our Alps, are interposed. Accordingly, if it can be proved that there exist similar relations between the various districts of Africa, which lie remote from each other; and that their Faunas are intimately connected with one another, not only in the directions of geographical longitude or latitude, but likewise in that of the diagonal; the well known hypothesis of Lacepede, according to which the interior of Africa is said to consist of a vast collection of mountain chains, is then completely refuted: on the other hand, the opinion of Professor Ritter becomes more firmly established, according to whom the interior of Africa is a large table-land encompassed by ranges of hills, and no doubt interspersed with mountains, but which are so loosely united, and of such secondary importance, that the Faunas may easily traverse them in every direction. The researches published in Mr Wagner's Travels through the States of Algiers, vol. iii. p. 140, clearly demonstrate that there is a striking resemblance between the Fauna of those districts of Africa which extend along the Mediterranean, and the Fauna distributed over the opposite coast of Europe, with the exception, however, of a few species, which are confined to the former. The States of Barbary are distinctly separated from the southern countries of Africa, not so much, as one might fancy, by the chains of Mount Atlas, as by the desert of Sahara. The Fauna of Egypt is more closely related to that of the other districts of Africa, than to that of the States of Barbary; and when single forms are distributed from thence to other parts of Africa, it is only through Egypt. We have already alluded to the great affinity traceable between the Egypto-Nubian Fauna, with which that of Sennaar and Kordofans forms the connecting link, and the Fauna of Senegambia. Guinea, which, among the magnificent Fauna of its lowlands, certainly claims some species as entirely its own, for instance the genuine Goliaths, agrees on the whole with the countries of the Senegal, because a great many species are found to be, to a certain extent, common to both. The Fauna of South Africa is apparently more isolated. This is owing partly to the circumstance that it embraces several species of its own, partly to the peculiar mode of distribution by which the general relations of the Fauna are more or less affected. We allude, in the present instance, to the predominance of the Melasomas, to the frequent occurrence of small Melolonthidæ, &c. Hence we have ample reason to expect here an independent Fauna system. The fact, that several species, common to the tropics of Africa, to Guinea and Senegambia, are found again in great abundance at Christmas Bay, as also in the vicinity of Cape Town, might confirm us in the belief that there are no mountains intervening, at least none large enough to effect a division between the Faunas. So long, however, as we are left in ignorance with regard to the condition of the countries extending from the Gulf of Guinea to the Cape of Good Hope, or even with regard to particular localities, so long shall we be prevented from determining the exact relation which the Fauna of the Cape bears to that of other countries of Africa.

Under these circumstances, a small collection of insects, formed in the Portuguese possessions of Angola, and presented to the Royal Entomological Museum by the Privy Councillor of Medicine, Dr Schönlein, has created considerable interest, as it promises to enlarge our knowledge of Africa. A careful examination has led to the conclusion, that the Fauna of Angola stands between that of the Senegal and of Guinea, on the one hand, and that of the Cape on the other, since it embraces not only a great number of species, but likewise several characteristic genera, which are common to those countries, and since it forms the connecting line between the Fauna of South Africa, and that of its central regions. We are indebted for this collection to Mr Edward Grossbendtner. This gentleman, honoured by the support and patronage of Dr. Schönlein, accompanied the Consul-General, Dos Santos, on a commercial expedition, consisting of six vessels, and had thus an opportunity of visiting the Portuguese settlements of Angola, Benguela, of St Paolo de Loanda, and of the independent negro empire of Ambriz.

This enterprise turned out to be unsuccessful, more especially as the Commander himself was carried off by the effects of the climate. The expedition arrived in Benguela on the 10th October, the weather being then very favourable. The sight of a luxuriant vegetation, and a temperature of about 90°, seemed to hold out the most flattering prospects; but the naturalist, Mr Grossbendtner, and the botanist, Mr Wrede, were unexpectedly detained on board, and afterwards towards the end of October, when many Europeans perished from diseases caused by the rainy season, both gentlemen fell victims to their enthusiasm. According to the catalogue published by the Privy Councillor of Medicine, Dr Schönlein, the collection of Grossbendtner, who, during his few days of good health, spared no trouble in bringing it together, consists of 2140 specimens of Coleoptera belonging to 173 species, and of 20 butterflies. Part of it has probably been lost, because, from the time of his death, no one on board of the ship took an interest in the preservation of these curiosities.

Considering how little we are acquainted with the Fauna of that portion of Africa, which is situated between the equator and the tropic of Capricorn, a closer examination of this small collection cannot but furnish matter for very interesting observations.* A few species only have hitherto been published, in particular by Messrs Olivier and Laporte (Hist. Nat. d. Ins. Suit. à Buff., where Dongola seems sometimes to have, by mistake, been substituted for Angola.)

It is impossible to give a detailed account of the various relations of the Fauna, until a collection has been subjected to a thorough and minute investigation. Obscure species, as well as those generally known and widely diffused, are in that case of equal importance with those newly discovered or looked upon as great rarities.†

On the Incipient Disengagement of Elastic Fluids. By JOHN THOMAS WOODHOUSE, M.D., Senior Fellow of Caius College, Cambridge. Communicated by the Author.

I have never seen or heard a satisfactory explanation of the well-known fact, that when a tea-kettle with boiling water in it, is removed from the fire, the bottom is only moderately warm. It has been referred to like causes (substituting steam for vapour), as when spirits are thrown upon the skin, and a sensation of cold is produced, in which case heat is first given to the fluid, succeeded by a change in the state of the fluid.

This explanation appears to me defective and unsatisfactory; and I will now endeavour to shew where it is defective, and supply the defect.

When the kettle boils, the water in it will raise the thermo-

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* The few butterflies are not worth considering, because, being chiefly species of Antocharis and Lyosena, they exhibit nothing characteristic of the Fauna of Angola.

† From Erichson's *Archiv für Naturgeschichte*, 1843, part 3d, p. 199. We hope, on a future occasion, to lay before our readers Professor Erichson's particular details.—EDIT.

meter to 212° Fahrenheit; the fire is much hotter, and yet the hand which soon after touches it, feels only a moderate warmth —in a short time the heat becomes intolerable, *i. e.*, of the same heat as the superincumbent fluid.

Now, admitting when the heat of the bottom is becoming greater than 212°, that the water undergoes a change by its conversion into steam, and that the heat of the contents of the kettle is thus partly latent,—admitting, that this would account for the bottom not indicating a greater heat than 212°, I contend, it is unequal to explain, why the bottom should be less than 212°: for the water is 212°, the steam under the ordinary pressure of the atmosphere is supposed to be 212°, and the fire which was under it more than 212°. The object of this paper is to explain, why the bottom immediately on its removal from the fire, should indicate a heat less than 212°, and soon after, a heat equal to that of the water in it.

Fig. 1.

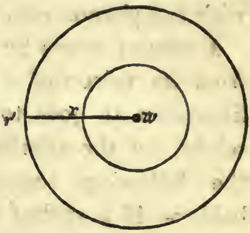

Fig. 1. Let w represent a portion of water. Let the sphere, whose radius is $w\,r$, represent the space occupied by the steam, into which this portion is converted by the communication of heat. The heat of the steam filling this sphere would be 212°; but, in explaining the object of this paper, I suggest that the heat of the steam may be less than 212°, and to establish this, I propose the following theory:—

I assume, that when a portion of water is converted from its fluid into its gaseous state, a sudden expansion, or what may be termed, an explosion happens, *i. e.*, supposing the steam in its quiescent state, and under the ordinary pressure of the atmosphere, would occupy the sphere $w\,r$, at the instant of its conversion—by its elasticity, or momentum of its particles, it proceeds to fill a sphere whose radius is $w\,r'$, which is greater than $w\,r$.

Now, according to the acknowledged doctrine of latent heat, when water receives heat, which converts it into steam, the steam under atmospheric pressure would occupy a space varying with the quantity of caloric imparted to it. By the same doctrine of latent heat, if the same quantity of steam under the same pressure be made to occupy the greater space $w\,r'$, it would require a greater quantity of caloric; and supposing the change from its filling the sphere $w\,r$, to its filling the sphere $w\,r'$, to be effected mechanically by its elasticity, it would be covetous of caloric, and would take it from any substance which touched it.

This theory will (I conceive) explain all the phenomena. A certain portion of water is converted into steam at the internal bottom of the kettle, which, in its quiescent state, under atmospheric pressure, would occupy the space $w\,r$, but by its elasticity or momentum of its particles, at the instant of its conversion, it occupies the space $w\,r'$, becomes colder than 212°, and thus takes heat from the bottom, reducing it below 212°, after the supply of heat from the fire has been removed. This reduction of heat can only happen whilst the water is boiling; after the water has ceased to boil, it soon communicates its own heat to the bottom, which explanation accords with the phenomena.

I cannot prove by experiments, that when gas is liberated from its prison of a fluid, or a solid, at the instant of its liberation it goes to occupy more space than it would do solely, by the admitted laws of latent heat; but I suggest the following consideration, which may make this probable:—If a spring be fixed in a table, be bent towards the right, and afterwards released, it does not merely go back to the place where it will ultimately rest, but by its elastic property, it would go considerably to the left, and would pass its resting-place several times before it be still. May not the spring held down by the finger on the right side represent, or bear an analogy to, gas confined in a fluid or solid; and may not its proceeding to the left of its resting-place represent its expanded condition immediately after it has gained its freedom?

I must now mention another circumstance, which is closely connected with, and comes in aid of, the present subject.

It has been observed, that on the first removal of any metallic vessel from the fire containing boiling water, the ebullition is increased. The solution may be this:—The cold air then surrounding, and coming in contact with, the outside of the vessel, by the subtraction of heat, may cause its external surface to contract, and this may mechanically contract it internally, and so heat may be evolved. This explanation is nearly the converse of the previous one of the steam which has been given. There, a chemical expansion first happens, followed by a mechanical expansion, by which heat is involved. Here, in the metal, a chemical contraction first happens, succeeded by a mechanical contraction, by which heat is evolved.

On the Mode of Formation of Crystalline Limestone, Contact Products, Crystalline Silicide-Slates, and Unstratified Crystalline Silicide-Rocks; with Preliminary Observations on the present state of Geology, and on the Methods of Investigation pursued in that science. By B. M. Keilhau, Professor of Geology in the University of Christiania. Communicated by the Author.*

It would be a happy state of things if, by means of direct observation, we could every where acquire a perfect knowledge of the structure of that portion of our globe which consists of the known mountain-rocks, and if we could obtain a clear view of the part performed by each mountain-rock, or generally by each mineral mass, as an architectural element, in a construction so complicated. We should thus create an indestructible basis for geology, and could provide for it a foundation of data really belonging to itself, which would confer on it an independence that is at present awanting. It is an acknowledged truth, that we cannot be very doubtful as to the origin of a mountain-rock when the relations of its masses are clearly placed before us; at least, in such a case, altogether erroneous hypotheses would scarcely be possible. If, for example, we see that an entirely irregular mass lies between sedimentary strata, and branches out into these, we cannot, in refer-

* This literal translation of Professor Keilhau's manuscript has had the advantage of his revision.—Edit.

ence to its mode of formation, place it in the same class with the strata; and if we see it bounded on all sides by the sedimentary rocks, we cannot attribute to it an eruptive origin; in short, by means of this simple but correct mode of examining the subject, both the hypotheses which are most frequently brought forward would be rejected. In the same manner undoubtedly, every hastily adopted opinion as to the origin of rocks would, by a complete knowledge of the facts of the structure of our globe, be at once reduced to its true value. But this is not all, for this perfect, this, so to speak, anatomical knowledge, would also afford many positive contributions to an insight into the actual nature of the object,—to an understanding of the origin and cause of the phenomena presented.

It can admit of no doubt that geology would undergo very essential changes, were we able in such a manner to have an unobstructed view of the structure of the earth. So long as, owing to the impossibility, in most cases, of obtaining a knowledge of facts by direct examination alone, observers always endeavour, in a greater or less degree, to assist their observations by supposing, by means of considerations as to causes and actions, how the phenomena under investigation must be, this great evil must result, that the science does not obtain possession of perfectly unmixed data. What are termed facts frequently very little deserve the appellation. Such data are partly the work of the understanding of the observer, and, as they are more or less the result of a preconceived explanation, the science becomes in the same degree illusory, being founded on such a basis; nay, the whole procedure remains a mere movement in a circle, inasmuch as the observer explains what the explanatory reasoning itself has just silently advanced. This great evil could not exist in the case previously supposed. Imaginary relations would then never take the place of those really observed, partly because there would be no necessity for it, and partly because what was inaccurate would be much too easily pointed out, and in this way the mania for making hypotheses would be suppressed. On the contrary, the opportunity afforded of furnishing the science with that pure foundation of unfalsified data which it now wants, would be eagerly embraced, and the first endeavour of every investigator would be directed to the distinct exposition of the really existing phenomena. When this was entirely finished, but not sooner, the observer would proceed to the task of treating of the causes of the geological phenomena, and of their mutual connection. In such labours, therefore, the actually existing geological relations themselves would always constitute, as the very nature of the subject requires, the first and most important point, and that not merely because these are precisely the object of investigation, but also because they themselves can afford such essential aid in the elucidation of their true nature. In the next place, what we learn by the observation of geological processes going forward before our eyes would become available; and, thirdly, then, for the first time, would those elucidations come into consideration, which can be borrowed from other sciences, viz. from chemistry, natural philosophy, and astronomy.

We know sufficiently well that this natural order of things is at present not the existing one. As we cannot obtain such irrefragable knowledge of geological facts as would be possible on the supposition made above, geologists are so far from assigning to results obtained by geognostical examination the just rank in their theories, that they often consider themselves authorized to *modify* the exposition of geological phenomena, in order to accommodate it to the explanation created out of those sciences which only stand in more or less remote connection with geology. It is sometimes even the case that they go still farther, and from time to time believe themselves justified in putting observation entirely out of sight, and supplanting it by ideal phenomena according to views derived from the most varied sources, which, however, are beyond the proper territory of geology. In this manner, the proper order is exactly reversed; the science comes to rest on a foreign basis; and that which ought to be the result of pure geognostical observation becomes, at least in a greater or less degree, a mere *construction*, for which *opinions* derived principally from chemistry have solely furnished the materials.

Thus, if geological relations were freely and openly exposed to observation, so that no difference of opinion could be possible, with regard to the real nature of the facts; if, on this account, nothing were borrowed from ideas derived from other sources; and if no one were to replace the real by imaginary phenomena, then, undoubtedly, in theorizing, these relations would with justice attain to the first rank under all circumstances—a state of matters which does not at all exist at the present day. Besides this principal change which, upon our supposition, would take place in the science, it may be worth while to mention another, which would also be very important.

Although, under the circumstances supposed, much that is now problematical would be cleared up, yet, on the other hand, the very possession of geognostical data which could not be impugned, and which could admit of no accommodation, would very often cause us to meet with inexplicable phenomena. This, however, would not be at all remarkable, and still less would it be a stumbling-block. That the human mind, which in no instance is able to understand nature to the very foundation, should here find an exception, would occur to no one; and we should here likewise have to encounter darkness beyond the nearer or farther limits which explanation could reach. It is worthy of attention, that at the present time matters are in a very different position; of phenomena which cannot be explained we now hear very little in geology! precisely in that science in which so much must be obscure, it seems as though every thing were perfectly understood. The method adopted goes the length of requiring that every phenomenon shall be placed in such a light that its cause can be ascertained, otherwise no attention will be paid to it, or its description will be regarded as inaccurate. To the uninitiated this must appear in the highest degree absurd. When, indeed, we reflect how much is still obscure respecting the origin of mineral bodies, we must be astonished at such a state of matters in geology, and such a mode of proceeding among geologists. But in this, we only see a direct consequence of the existing circumstances. Instead of geological phenomena, which are adduced as facts, being considered as quite certain by and for themselves, and only by and for themselves, we cannot now, as it is so difficult to observe with perfect accuracy, have full confidence in the apprehension of the observed facts, until we find that they harmonize with what, according to our *theory*, we assume *ought* to exist in the case under consideration. In order to be able to believe, we must also be able to understand. As the objects cannot here be made palpable, we are not in such a situation that belief must exist under all circumstances, whether we understand or not. It is even considered as a very correct principle of investigation, when the question regards any geognostical appearance, not to recognise it as a pure fact until counsel is taken from sciences to which in reality only a secondary voice should be assigned in such matters. Thus it has become quite usual in geology not to tolerate phenomena which seem to be in discordance with the present position of the sciences alluded to. These are rejected on the pretext either that they are imperfectly observed, or that they constitute isolated abnormal phenomena, to which no weight can be given. An ample field is thus thrown open for caprice, and the science is exposed to this disadvantage, that a multitude of important facts are not introduced into its archives, facts belonging precisely to that class on which might be founded principles, that are not at all, or at least not easily, to be obtained from any other science but geology itself. As the key to such facts cannot be found in those other sciences, these very facts are thrown aside: but this is done to the irreparable detriment of geology; for, as already remarked, it may be the case that it is only by the study of these facts themselves, and their analogies, that the most important information can be obtained. I hope afterwards to illustrate this more fully by distinct examples.

What I have already said, is sufficient to shew that geology is by no means in the best possible situation. However, inasmuch as the conditions for a more desirable position, such as that mentioned at first, with which the actual one was compared, depend on an impossibility, of what use can it be, it may be asked, to think of a change of that kind? But on considering the matter more attentively, it will be found that very much can be done in this respect. We have it completely in our power to approach much more nearly to such a position than we are at present; it is possible in very many cases at least to substitute direct observations for ideas.

The constructive character which predominates in much too high a degree in the present method, can be limited to a great extent. It is possible to observe with much greater accuracy and certainty than has hitherto generally been done, and in this way to procure attention even for facts which cannot at once be explained so as to harmonize with the results obtained from other sciences, but which must nevertheless be studied in order to advance the science. It is possible to procure for geological facts more than they have hitherto possessed of the influence to which they are entitled, in the explanation of many of the most important problems of which the only solutions hitherto attempted have been derived from sources foreign to geology. It is possible, especially, to employ the assistance of chemistry in geology in a more judicious manner than heretofore.

There are, indeed, not a few mountain-rocks which, either themselves, or their complete analogues, are formed before our eyes by processes whose general nature cannot admit of a doubt. A multitude of other mineral masses, however, and, among them, precisely those which play the most important part in the structure of the earth, viz. the greater part of the crystalline rocks, have an entirely hidden origin and development. The direct observation of the mode of formation of these is so difficult, that it has not yet been definitively determined (for I must be allowed this assertion) to which principal class the agent belongs that has here specially been in operation. It is more particularly with regard to these mountain rocks that the method of investigation hitherto pursued must be changed in the manner already indicated. More diligence must here be employed than has hitherto been bestowed in the discovery of all the geognostical relations presented by these rocks; and, at the same time, in the decision of debateable subjects, greater influence must be given than heretofore to the results obtained by means of unprejudiced observation. It is especially requisite to beware of the principle, that chemistry alone can and must decide in such matters; for, although the laying down of this maxim as a fundamental truth has been believed to be the perfectly philosophical mode of going to work, it may nevertheless turn out to be incorrect. Inasmuch as chemistry has artificially produced the analogues of some of those minerals of which the problematic mountain rocks are composed, it can without doubt point out with certainty particular modes by which nature may have operated in the formation of such minerals; but can we depend on chemistry being able to point out all the modes of operation it is possible for Nature to have employed in such cases? And if this were taken for granted, how can chemistry decide as to which of these was actually employed in the instances in question?

Chemistry already points out more than one way in which such minerals as are now under consideration may have been produced, so that there may really be more than one mode of formation; and, as that science is itself only in a state of progression, it is possible that afterwards it may be able to indicate other processes besides those already discovered. But perhaps those very modes of formation* to which the rocks in question owe their origin and development may, even in time to come, remain undiscovered; for chemical knowledge of this kind rests on experiment, and it cannot well be assumed that art will ever have it in her power to apply all the means by which Nature herself has operated in her great laboratory.

But let us assume that every mode is known by which the minerals composing granite, basalt, crystalline limestone, &c. could have been formed. How, I would ask, can chemistry determine which of the processes Nature has followed, when, in the production of these rocks, she effected the formation of the minerals of which they are constituted? We remarked that the rocks of which we are now speaking had an obscure origin and development; the process by which they became what we now find them is concealed from direct observation;

* Perhaps if we saw the subject in a proper point of view, we should recognise only *one* mode of formation for crystals. But so long as the ultimate causes are unknown to us, much, in our estimation, seems different, which is really not so. When we find a compact mass gradually becoming crystalline at the ordinary temperature, we naturally regard the crystals produced as formed in a different manner from those crystals which result from fusion. If, again, as is credible, the process of cementation can be caused by different influences, so here also, according to our limited observation, several apparently distinct modes of operation may be supposed to have been in action.

there is, therefore, no other method of determining which of the various possible modes of formation was in such cases the true one, except the investigation of the geognostical phenomena, viz. of the forms of the masses, and of the whole circumstances of their occurrence. But this is just a work which does not at all belong to the chemist as such. When we really see chemists employed in answering the question I have mentioned, this takes place, inasmuch as they then make their appearance in the character of geologists. That people should be deceived in this matter is really very extraordinary. That, for example, in the discussion on crystalline limestone, the chemists have declared themselves in favour of the opinion that this rock generally owes its formation to heat, has hardly been caused by a single purely chemical consideration. Is it at all more probable, for chemical reasons, that the calcareous spar (which undoubtedly has in many instances been produced without the action of heat), composing, with its small individual crystalline portions, the granular limestone, should have been formed in what is termed the dry way than in the moist? Certainly not. It is known, that in the last mentioned way such a mass can even be produced artificially.* As already remarked, it is not chemical reasons which have determined that opinion, but it is speculations belonging entirely and alone to the peculiar province of geology which have called it forth from chemists. It is only a dilettantism in this interesting science to which we owe this and similar judgments, before which even geologists, according to the philosophical maxim already quoted, bow with a loyalty that is even regarded by them as a matter of no small pride.

The volcanists should reflect well on the following: that in so far as chemists adhere at present, as they will perhaps do for some time to come, to the hypothesis of the pyrogenic origin of granite, the reason of this can be no other, than that they have declared for this opinion *in the character of amateurs of geology;* for there assuredly exists no necessity arising out of chemistry itself for adopting this view. By the side of the

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* The medallions which are made at San Filippo, in Tuscany, exhibit in their interior a structure precisely similar to that of fine grained natural marble.

celebrated fact, which shewed the possibility of the formation of felspar "by heat," that fact, by means of whose discovery volcanism was enabled to boast of its greatest triumph over its old opponents, chemistry itself now places experiments, which prove the possibility of producing felspar in the moist way.* That notwithstanding this, chemists who are perfectly impartial with regard to geological disputes, when they have only to choose between the Wernerian doctrine as to granite, and that which prevails at present, would have no hesitation in declaring for the latter, is apparent; but that they will not assert that the latter view is *absolutely* the right one, may well be concluded from the advances which have lately been made.

We may hope that these very advances in chemistry itself, will contribute to shew, that the relation which must exist between that science and geology, has hitherto not been properly understood. In investigations respecting these problematical crystalline rocks, and other mineral masses of still uncertain origin, chemistry can at most only afford geology suggestions or considerations. It is for geology itself, with the assistance of geognostical investigations, to examine, to what extent the explanation proposed and the theories advanced, are correct or not. But then it is chiefly requisite, in reference to the phenomena existing in nature, to acquire that kind of knowledge which is pure matter of fact; for, should this knowledge of natural phenomena really become the touchstone of theories, good care must be taken that the former is not by anticipation modified by the latter.

In another point of view also, it is of consequence, as regards the true bearings of the facts, to elicit information which no preconceived theory has influenced. This information is not merely to be employed in testing suggestions made from extraneous sources for the explanation of phenomena, but also, as has already been repeatedly stated, to call forth available ideas, which hitherto could not have been obtained by any other means. I hope to prove this by the following observations on some of the mineral masses, whose mode of formation is involved in greater obscurity than that of any others. I hope thus to shew the correctness of all the

* Becquerel's *Traité de l'Electricité*, vol. v. p. 144, &c.

opinions I have now offered on the mode of investigation at present pursued in geology; and at the same time, to justify the method which has been adopted by myself, and which has been so strongly blamed. With this view, I intend to treat, first, of crystalline granular limestone, or, to make use of a shorter expression, of marble; next, of several of those peculiar mineral masses, which are most easily designated by the name of "contact-formations;" and, lastly, to say something on the crystalline silicide-slates, and the unstratified crystalline silicide-rocks. I should very willingly have embraced this opportunity of taking also into consideration the history of gypsum and dolomite, but the fear of being thought much too digressive, causes me to refrain. The subject of the crystalline silicide-rocks is, on the other hand, suitable for an additional reason, viz., that these constitute the most important component parts of the country, to whose description I am about to offer contributions;* and I may remark, that marble also is an essential member of the geological formations of Norway.

Crystalline Limestone or Marble.

The various modes of occurrence of crystalline limestone in groups of fossiliferous rocks, are the most instructive. It sometimes also itself contains organic remains. Not unfrequently it presents itself in these groups in the four following ways:—1. As larger or smaller spheroidal or kidney-shaped masses, lying included in strata, whose sedimentary origin cannot be denied. 2. In the form of entire strata, interposed between equally distinctly sedimentary beds, and situated quite in the midst of their undisturbed order of succession. 3. As a middle portion of such strata, and gradually passing on both sides into uncrystalline limestone. 4. As a terminal portion of strata, which are elsewhere composed of uncrystalline limestone, and which, at their crystalline termination, are in contact with some rock entirely different from the limestone.

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* Professor Keilhau here alludes to a geognostical description of Norway, which he is preparing for publication, and which we feel assured will prove a very valuable contribution to Geological Literature.—Edit.

At present, we hardly know of a more remarkable example of the first mode of occurrence, than that presented by the Wenlock limestone of the West of England. This member of the upper division of the Silurian system, is both underlaid and over-laid by shale, whose nature is well indicated by the name of mudstone, which has been assigned to it. The Wenlock formation consists partly of irregular beds of impure argillaceous limestone, and partly of "ball-stones," as they are termed; and it is of the latter that we have here to speak. Murchison, in his "Silurian System" (vol. i. chap. 17.), describes them as "concretionary lumps," generally varying in diameter from a few inches to several feet, but occasionally attaining an immense size, consisting of a pure crystalline limestone, which is sometimes quite a white marble, full of petrifactions, and surrounded by beds of shale and impure limestone. The strata generally terminate suddenly, when they encounter these masses; but in some places the stratification seems gradually to cease at the place of meeting, so that the concretions imperceptibly unite with the strata. The latter are, for the most part, much contorted, where they surround the concretions; and Murchison's observation on this point is remarkable. He says, that these contortions have, for the most part, been formed during that process of solidification or crystallization, which gave rise to the concretions of the limestone, and cannot be considered as resulting from the dislocations by which this tract has been agitated, for such concretions enter intimately into the structure of the Wenlock limestone.

Concretions, which must be regarded as quite analogous to the true ball-stones, occur also in the underlying shale. These are of a spheroidal form, consist of argillaceous limestone, but also sometimes of pure crystalline limestone, and have in part a structure similar to the well-known "cone in cone" of the lias formation. Such masses have been found containing crystals of quartz, crystals of calcareous spar, flakes of anthracite, and organic remains. In the Wenlock limestone itself, veins of calcareous spar and copper pyrites frequently occur. Murchison, when he first examined them, thought that they might communicate downwards with fissures, connected with deeply-seated subterranean agency; but subsequent examination convinced him, that many of them are veins of segregation; and he observed some of them fairly terminating at both ends in the limestone. They are most abundant in the proximity of the large concretionary masses. Other larger and vertical rents have their walls lined with crystals of calcareous spar, the surfaces of which are coated over with black bitumen.

If we inquire as to the origin of marble occurring in such a manner, and under such circumstances, it is very evident that neither of the theories generally received can explain the problem. "Fire" is entirely out of the question, and "water" does not afford us much assistance. The latter has plainly given its aid so far; the formation can, and must, be pronounced to be hydrogenic, and we even know, that it was the water of the sea which thus far was in operation. But if it was water which *deposited* the carbonate of lime, still it could not, even if its power of solution had been rendered ever so great by means of some accidental component part, have produced directly the peculiar calc-spar aggregates, which, in this case, constitute the marble. These masses did not crystallize in the sea; and as assuredly no one, who pays sufficient attention to the geognostical relations just detailed, can advance such an opinion, I shall not stop to refute it.

But how is the new path to be found, which is here to be trodden? Chemistry does not assist us, for, in the question as to the formation of calcareous spar, it gives no other explanation, but either the previous fusion of the carbonate of lime or aqueous solution, in which latter alternative, nothing else is spoken of but water, excess of carbonic acid, and the shooting out of crystals in the liquid.

Here, then, is one of the instances where, inasmuch as chemistry, so far at least as has yet happened, firmly refuses to come forward with other propositions, suggestions for a new theory can spring from the geognostical phenomena themselves. When these are attentively considered, the idea occurs of actions that have operated powerfully in masses, which at first were principally mere rough mechanical mixtures, nay, which, perhaps, were not precipitated in a crystalline state at any one point. The view presses itself irresistibly upon us, that the crystal-forming agents produced the result which we have before us, *after* the deposition of the formation at the bottom of the sea. Should it even be necessary to assume, that solution did not take place at all during the process, and that these agents have had to work in a perfectly solid material, we can easily recall to our remembrance some facts which, notwithstanding an old chemical doctrine, render it incontrovertible, that a substance can crystallize without having previously been in a liquid state. Inasmuch as it is only from geognostical considerations that our attention is directed to these facts, which hitherto have not been applied in geology (probably, because they were placed in the shade by the axiom alluded to), we perceive that, in such a way also, these considerations are not without their fruits. From the study of the subject, in this point of view, the following events in the history of the Wenlock formation present themselves, and these, though perhaps not so well established as the experimental chemist requires, must, nevertheless, be regarded as certain, viz.: The deposition of masses of mud containing clay and lime, and of other mechanical products, in which the marine organisms of the period became enveloped; the concentration and crystallization at certain places of the carbonate of lime, from which the clay was then removed; the bringing together, and crystallization of the silica and carbon, for the formation of rock-crystal and leaflets of anthracite in the nodules in which these minerals are found; all by processes slowly operating at the usual temperature. To these may be further added the production of the veins by means of similar slow movements of the substances in the hardened masses.

It is no small proof of the irresistible power with which the matter-of-fact circumstances of the case speak to the observer, that these very circumstances have called forth such ideas as those already stated, from a geologist, who is one of the most faithful in following the rule, that the theoretical geologist must, at all times, unconditionally place himself under the authority of chemistry. Murchison, who is a keen volcanist, does not, however, venture in the case of the Wenlock formation, to call in the aid of any "subterranean agency;" although, as we have seen, he would also here have very willingly given a volcanic explanation. His opinion regarding the metalliferous veins, and the contortions of the stratified masses occurring along with the concretions, has been already noticed. In another part of his work (p. 245), he gives an ideal profile, representing some of these concretions, and advances the opinion that, as the concretionary structure interferes with the laminæ of deposit, and truncates them, " there can be no doubt the concretions were formed by some chemical or electric action, after the first aggregation of the surrounding strata." Murchison thinks, that the sequence of the operations was : *First*, the successive deposition of the materials composing the beds ; and, *secondly*, the arrangement of these materials, so as to occasion particles of similar matter to unite and form concretions.*

The second, and still more the third, of the already enumerated modes of occurrence (p. 10), of granular limestone in groups of fossiliferous rocks (the first also containing organic remains), are likewise very instructive as regards the problem of the origin of that rock. I shall here, however, content myself with merely mentioning them, and at once proceed to the consideration of the fourth case.

It has been observed in many localities, that beds of completely uncrystalline, often argillaceous, and bituminous limestone, exhibit, when in the vicinity, or, most frequently, when in direct contact with certain other rocks, an entirely different constitution, inasmuch as they there consist of pure perfectly saccharine granular marble. This phenomenon shews distinctly, that, in this instance also, the marble is a later product developed from the already solid rough mass, and this has been universally understood to be the case.

But what is to be said farther in regard to this subject ?

It is clear, that the contact with the dissimilar rock must stand in causal connection with the alteration met with in the uncrystalline limestone ; and we certainly do not err, when we

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* In this part of his work, Mr Murchison is treating more particularly of another remarkable process, which has been in operation in the solid masses, viz. of that by which, what are termed " joints," have been produced. These are the parallel fissures, or *ablösungen*," which, in slates, so frequently cut through the true slaty structure.

at once admit, that the change has been partly or wholly produced by this contact. But how did this take place? The knowledge, that burning liquid masses sometimes burst forth from the interior of the earth; and, in addition, Hall's experiments, naturally led to the idea, that these contact rocks were at one time in a melted condition, and thus produced the conversion by means of heat. Besides, indeed, we find, that the rocks in question are, in most cases, those respecting which it is, at least at present, assumed, that they were at one time in a melted condition; nay, we perhaps find even examples of the masses in contact being undoubtedly pyrogenic, and having thus been able to act upon the limestone when in a hot state. When, however, we pursue our examination still farther; when, from the love of truth, we do not fear to encounter facts, which may, perhaps, overturn the result thus obtained, it may really happen, in consequence of what observation has shewn, that we at last meet with such facts, and that we are actually placed in the position of seeing our first conclusion overthrown. Thus, it is not without example, that the conversion of uncrystalline limestone into marble is met with near masses, that either have never possessed the high temperature that is usually presumed to have existed in such instances, or, at least, not since they were in contact with the limestone. Such is the case with a mass of the well-known bone-breccia observed by H. Bronn, on the coast of the Mediterranean; and F. Hoffman saw, in Sicily, limestone reposing *on* basaltic *tuff*, which was altered in the same manner as happens when it is in contact with solid basalt.

The cautious and reflecting observer will, however, not require facts of this description, in order to find it necessary at this point in the investigation, to defer deciding, in the mean time, in favour of the explanation of the contact-marble by means of heat. He will feel the importance of the circumstance, that the phenomenon of conversion, in the greater number of instances, occurs in proximity to rocks, whose former melted condition is not undoubted, but is only for the moment *assumed*. He will become so much the more circumspect at this stage, when he discovers that the theorists, from whom this opinion emanated, have, in fact, founded it chiefly on the very occurrence of the marble, in place of compact limestone, in contact with the rocks regarded as pyrogenic! As, indeed, uncrystalline limestone deposits, similarly constituted to those which exhibit the conversion into marble in the vicinity of these other rocks, present the same phenomena of conversion in cases where heat has undoubtedly not produced the change (as in the instances mentioned at first, p. 10, &c.), it follows, that there is no existing necessity for our being obliged to regard the contact-marble as a product of heat. Such a mode of formation would only become *probable*, if the said marble were *without exception* found in contact with undoubted pyrogenic rocks. As, therefore, this is not the fact, the cautious geologist must find himself far removed from being able positively to declare, that contact-marble receives a full explanation from Hall's experiment.

It is frequently asserted that the ordinary compact limestone, presenting a crystalline structure at its contact with the bounding rock, is no longer stratified where it has become crystalline, and that there the fossils, which elsewhere are distinct, have become obscure, or have even completely disappeared. This is certainly true in *very many instances*, and both phenomena are also very natural. As, however, these phenomena *must* exist in such positions according to the prevailing theory, because that theory presupposes that the limestone was at least rendered soft by the heat in order to become crystalline, a deficiency is apparent, inasmuch as it is by no means the fact, that such phenomena occur, without exception, in contact-marble. In the district of Christiania, fossils, with perfectly distinct outlines, are found in marble next the granite.* Here the mass of limestone evidently did not lose its solid condition during its conversion into marble. The necessity is thus removed, as far as I can judge, of presupposing an excessive temperature; and thus the process of conversion is placed in the same class with those crystallizations, so completely proved by observation, of solid amorphous bodies, where

* The Museum of the University of Christiania possesses an extremely beautiful specimen of this description, in which the *Catenipora labyrinthica* is fully as distinct as in the completely uncrystalline limestone of the same district.

heat may sometimes, indeed, operate as an assisting, and, as it would seem, as an accelerating, agent, but which can also go forward at the usual temperature. Although we not unfrequently hear from volcanists (for the facts often drive them to this), that fusion was not necessary in order that the change in question might take place, yet it is not to be denied, that, in their mode of viewing the matter, they must think that the limestone was reduced to a state of softness ere it could become crystalline ; and, according to their whole line of argument, they are more especially compelled to adopt this supposition in the cases where various accidental minerals, such as silicates, are crystallized in the mass of limestone. Now, where the distinct petrifactions are met with in the marble, there were likewise found various foreign crystallizations which had plainly been produced simultaneously with the calcareous spar of the marble, between whose crystals they are embedded. Naumann attests this fact in his "*Beiträge zur Kenntniss Norwegens*" (vol. i. p. 12); for he says,—"We found a very distinct specimen of Favosites (*Calamopora*) embedded between fibres of tremolite." In the marble quarry of Gjellebök, between Christiania and Drammen, garnet, zinc-blende, and large masses of grammatite occur, and amongst these distinct petrifactions are met with.

If those who explain contact marble by a melting heat, flatter themselves that they have struck out the only safe and really legitimate path of investigation, such is, undeniably, a great deception. If we consider *all* the circumstances of its production—if we do not select certain of them, which, as regards a particular view, seem to suit, and if we do not suppress the others,—we do not arrive at the result, that the substance in question has been melted either partially or completely. It is by no means a physical or logical necessity which calls forth this result, but it is the desire to possess a principle which is so urgently requisite in the system adopted. It sounds well, and is perfectly adapted to deceive the less skilful, when it is said,—we found our opinion on the old chemical axiom, that it is only bodies in a liquid state which can crystallize; we have on our side the fact confirmed by experiment, that melted limestone consolidates into just such a crystalline mass as the product in question, under precisely the same circumstances as those under which the said product must have had its origin; in short, if there be theorists who endeavour to tread on safe ground, and who wish only to ascend from what is known to what is unknown, then assuredly we are those theorists! And it is so much the more easy to be thus misled, because the doctrine of these theorists is so simple, and can be understood with so much facility. All difficult details of the subject are necessarily kept at a distance, for if they were introduced, the whole theory would be destroyed.

At present, all that can be legitimately asserted (by making full use of "what is known"* connected with the question) regarding the marble which occurs so frequently at the contact of another rock, but which, in other respects, belongs to some uncrystalline deposit of limestone, is limited to the following: *a*. That it must be considered as a product of that probably very slow process by which crystallizations and chemical results generally, are effected by modes of operation which either cannot be at all imitated by art, or can be so in but a very imperfect manner; and, *b*. That the actions which took place were either caused, or, at least, greatly assisted by the contact of the limestone with the other rock of an entirely different description.

We may conjecture that it was electricity which here performed a principal part; we may willingly consider the phenomenon as the result of a "molecular action" that has taken place in the solid mass; we may speak of "concretionary actions" (Murchison, i. 360), of "corpuscular forces," &c.; in short, we may proceed as if we knew something of the processes which were in operation: but it must still be allowed

* Hitherto no one has brought all this together. Among the circumstances which belong to those that bear on this matter, we may also include what is known respecting the distance from the line of contact at which the limestone begins to be granular, and also the power of conducting heat possessed by melted rocks. As to the first point, I can instance that the dark compact limestone of the Christiania district begins to become light-coloured and crystalline at a distance of 4000 or 5000 feet from the granite; and, regarding the second, that, on Mount Etna, an old stream of lava has been seen reposing on a mass of ice, over which it had been poured, just as in Iceland lava-streams have been found resting on still existing glaciers.

that we do not, in fact, at all understand their nature, and that, as these are so little adapted for investigation by the assistance of experiment, there is but little prospect of afterwards acquiring more than a conjectural knowledge of the subject. The prospect is certainly not enticing. We have, however, acknowledged that it is quite natural that a complete and perfectly candid study of facts must often leave us with nothing but what is inexplicable. If it be true that, when every thing is fully explained, there is room for doubting that the strictest attention has been paid to what is matter of fact, the friend of truth will readily be satisfied with his less distinct result.

We now proceed, so far as that can here be done, to treat of the marble which occurs in the oldest rocks, or in formations which contain no organic remains. I say, in so far as that can here be done, because our opinion of the origin of this marble is, after all, entirely dependent on the view we deem it necessary to take of the mode of formation of the principal rocks among which it presents itself.

Here in the North—and, no doubt, the same is the case in other places—what is termed primitive limestone occurs most frequently in gneiss, mica-slate, and hornblende-slate, in masses which, at the first glance, might be thought to have completely the nature of beds, but which, on closer inspection, do not at all correspond entirely with the idea of beds, in so far as we understand by the term bed a particular stratum constituting a member of a series of layers parallel to one another, which were gradually deposited the one upon the other, each for itself in its own period of time. The relations exhibited by these masses of marble are by no means so simple as they would have been, had, for example, a stratum of mica-slate been first formed, then over it a stratum of marble, and again over the last mica-slate. It is not only the case that, at the junction of the limestone and the including rock, we find both masses, as it were, mixed with one another, inasmuch as frequently grains of calc-spar are disseminated through the slate, and in the same way the constituents of the latter disseminated in the marble, but both rocks frequently invade the boundaries of each other in a bifurcate manner; nay, portions of slate, entirely isolated, are often met with embedded in the marble, while the latter likewise forms isolated masses in its hanging and lying including strata; the limestone in this instance being usually in very thin stripes between the strata, and the portions of slate likewise being parallel to the planes of stratification of the great slate masses. The individual plates of mica, too, which occur in the manner just stated in the marble near its junction, are parallel to the planes of stratification; and the same is the case with the hornblende crystals which are met with in the marble, when the latter is in contact with hornblende-slate. All this plainly shews that, in fact, no such entirely exclusive process of formation can have produced such a marble; whereas it is clear that the origin of the marble and that of the slate, both as regards the time and the mode of production, have had very much in common with each other. If now we must, in so far as the slates composed of siliceous compounds are concerned, reject the Neptunian theory (and this I regard as absolutely necessary), this must also be done in reference to the marble.* And those who are of opinion that the calc-spar constituting the marble has been formed by carbonate of lime in a state of fusion, or, at least, softened by heat, having been slowly solidified under sufficient pressure, will, in consequence of the above-mentioned relations of the whole mineral combination, doubtless not fail to admit that the silicates and the quartz of the slate are equally the products of fusion; while, on the other side, it will be conceded, that if the non-pyrogenic origin of the including slate can be proved, a pyrogenic origin cannot be assigned to the marble. This result will, I hope, be kept in remembrance afterwards, when we consider the origin of the primitive slates. Those who already, perhaps, think that this marble has been formed by "*actions lentes*," without heat, will doubtless refer to the analogy of its geognostical relations with those of the masses of marble occurring between uncrystalline strata, which were already discussed.

With respect to the crystalline limestone, an esteemed

* I shall not pause here to adduce arguments against the Neptunian hypothesis. The many silicates occurring in primitive limestone contradict it, as was shewn by Berzelius in his *Jahresbericht* for 1839.

geologist expresses himself in the following terms:—"This rock occurs in not a few localities under circumstances which are in favour of its having burst forth from the interior of the earth in a melted condition, and that subsequently to the formation of the gneiss and mica-slate. The limestone masses bear completely the stamp of great veins, or of the upfillings of fissures. There are appearances indicative of a violent pushing of the limestone into the surrounding rock. Where the limestone is in contact with the bounding rocks, we find, sometimes on the former, sometimes on the latter, the consequences of powerful rubbings, exhibited in specular or friction surfaces. Farther, the granular limestone includes fragments and large masses of the bounding rock; and lastly, where the former meets other rocks, we find contact products of various descriptions, according to the nature of the different rocks which are in proximity to the limestone."

Regarding the preceding quotation I must remark, that what I have said of the mode of occurrence of the marble, may very easily be made analogous to the appearances described by the author. When the strata of the slate are much undulated and contorted (as may every where be observed, both when the marble is present and when it is absent), the whole phenomenon acquires a complicated aspect, and the isolated portions of slate embedded in the marble may easily be thought to resemble actual fragments, which, it may be imagined, were violently torn from the hanging or lying sides. The minerals disseminated in the limestone at its junction with the slate, and which are the same as those of which the slate is composed, may readily be taken for "contact products;" that is, for products of quite a different act of formation from that which gave rise to the mineral species of the slates. As to the marks of rubbing and sliding, they prove nothing for the view expressed, as they can scarcely have been produced in any other way than by the action of perfectly compact masses on one another; and they are met with in all places and in all kinds of rocks where separations have occurred, and where the separated part has been subjected to a sliding or other movement.

That, however, granular limestone can occur as the upfilling of fissures, that sometimes it contains true fragments of the including rock, and in the vicinity of the latter is accompanied by peculiar mineral products, is willingly conceded; but, as I hope to make apparent afterwards, all this is no proof of the pyrogenic origin of marble. The chief object I had in view in quoting the passage, was to direct attention to the pernicious consequences of the mode of proceeding at present followed by so many geologists. I may be forgiven for not being able to help thinking that we really have here an example of the manner in which the science may be deprived of that impartial observation and delineation of facts so perfectly indispensable for its advancement; and this either because the observer had previously a particular opinion as to what must and should be found, and thus was led quite involuntarily to construct rather than to observe and remark directly, or because the describer was unwilling to represent facts which, perhaps, he could not at once satisfactorily account for to himself and others. As, in the description which has been quoted, the occurrence of the limestone as the upfilling of fissures, is alone mentioned, we are thus only informed of the very rarest case met with; while the usual mode of occurrence of the marble in the crystalline slates, and precisely that which constitutes the general rule, is altogether omitted. As I speak here from my own observation, I believe that my distrust will be thought natural, and that, therefore, no disapprobation is to be expected even from the respected author himself, on account of my candid expression of opinion.

(*To be continued.*)

Notices of Earthquake-shocks felt in Foreign countries, in Britain, &c. By David Milne, Esq., F.R.S.E., M.W.S., F.G.S., &c. Communicated by the Author.

(Continued from page 86.)

This comparison is facilitated in regard to one portion of the continent, by there having been a regular register kept of the shocks from the 19th December 1838 to the 8th March 1840. The shocks so registered, were felt at St Jean de Maurienne, in Savoy, a place where the sun comes to the meridian about 40 earlier than at Comrie. As the time of each shock is given in hours and minutes in this register, means are at once afforded, for judging whether the shocks at St Jean de Maurienne and at Comrie corresponded in point of time.

The following are the results of this comparison :—(1.) The two Registers run together for five months, viz. from October 1839 to March 1840. (2.) During that period, 130 shocks were felt at Comrie, and 58 at St Jean de Maurienne. (3.) At Comrie, the most violent and the greatest number of shocks occurred in October 1839, whilst at St Jean de Maurienne, December 1839 was the month, during the above period, in which they were most frequent and severe. (4.) Whilst at Comrie there were, during the above period, about 61 days on which shocks occurred, at St Jean de Maurienne there were only 22 days. (5.) During the above five months, when shocks were occurring so frequently at both places, there were only eleven days on which shocks were felt at both, within the same space of twenty-four hours. (6.) So far from any of the shocks, on these days, having happened at St Jean de Maurienne about 40′ before they were felt at Comrie, no shocks occurred at the two places nearer one another than 3½ hours ; and no regularity of intervals between the shocks of the two places, is discoverable.

These results shew conclusively, that the shocks of earthquake at St Jean de Maurienne, have no correspondence or connection, in regard to the period of their occurrence, with those at Comrie. Indeed, the circumstance stated in the report on these earthquakes, that the shocks caused by them were not in general felt to extend into Piedmont, prove that, just as at Comrie, the origin of them cannot be very deeply seated.

But what is thus proved of earthquakes in Savoy, viz. that they have no connection with those occurring in Great Britain, will be found to hold equally good of earthquakes occurring in other parts of the world. Some trouble has been taken to collect notices of suc, and a number have been collected applicable to the three last years, which will now be given chronologically.

23d March 1840, at night.—*Ammerapoora*, almost destroyed by an earthquake. The shock lasted 2′ or 3′, and killed about 300 persons. The cities of Ava and Tarquin are also said to have been destroyed by an earthquake, with many neighbouring villages.

20th June 1840 (old style.)—*Mount Ararat* convulsed by a series of earthquakes, which "occurred at intervals, till the 28th July. They afterwards diminished in force, but they did not entirely cease in the district of Sharar, until the 1st September; and though very feeble towards the close of that period, they were still accompanied by a slight subterranean noise." The severest shock was on the 20th June; and it was such as to crack the walls, in a number of towns and villages. "The shocks which occurred between the 21st and 28th June, overthrew the buildings which the first earthquake had shaken. The heaviest shocks afterwards observed, were on the 14th July, at 3 A.M., and on the 25th July, at 3 and 10 A.M., and 5 P.M. The shock on the 20th June occurred at 6^h 45′ P.M., and it was such as to change, "in a few moments, the entire aspect of the country in the neighbourhood of Mount Ararat. Repeated but intermittent shocks, which seemed to come from the mountain, gave to the earth a movement resembling waves, which continued for about two minutes. The first four and most formidable shocks were accompanied by a subterranean sound, and have left traces on the summits of hills, and bottoms of valleys, which the eye of the scientific observer will recognise after many ages shall have passed away."

It was, at the same time observed, that numerous rents or fissures took place—all parallel to the course of the rivers Araxes and Arpatchai, and which ploughed the earth to the distance of a verst from the beds of the rivers; and in accordance with the movement given to the soil by the shocks, they were seen every moment to open and shut. There also occurred a great number of vertical explosions from the bottoms of holes, like little craters, which, opening and shutting in the same way as fissures, spouted out torrents of water, and cast up immense quantities of pebbles and gravel. The influence on the wells was remarkable. In one canton, upwards of thirty springs were dried up for some time. Some continued for several days after the catastrophe, to yield only thick and whitish-coloured water; others became more abundant than they had been.*

30*th October* 1840.—" At ZANTE, an awful earthquake took place, which was followed, in the course of one week, by about one hundred shocks. It occurred in the middle of the day. The Lord High Commissioner, who was in a steamboat at the time, and within six miles of land, imagined that the boiler had burst. The island of the Trente-Nova has sunk into the sea. The buildings which had their foundation on limestone, escaped pretty well, except one village, which was turned topsy-turvy;—the strata underneath shewed itself betwixt the lime formation, to be full of large veins of mud."

11*th November* 1840.—" At PHILADELPHIA, a severe shock occurred at night. It was accompanied by a great and unusually sudden swell in the Delaware."

22*d November* 1840.—" In JAVA, there was an eruption of Mount Gede. Subsequently, other eruptions took place,—viz. on 1st December, at between five and six o'clock in the morning; on the 2d, at half-past eight; and on the 3d, at six in the evening. The following are some particulars of the explosion of the 1st, which was more violent than the two others. After the first violent explosion, accompanied with a slight motion of the earth, the fire rose from the crater to the height of four hundred or five hundred feet, at the same time a thick column of smoke rose to the height of fifteen thousand feet. (These estimates are founded on the ascertained fact that the top of Gede is 7500 feet above Tjunjir.) The noise resembled the report of several pieces of artillery, accompanied by flashes of lightning. The sight was the more magnificent, as the sky was perfectly cloudless and serene. An eye-witness

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* This is abbreviated from an official account published by the Russian Government.

gives the details of the preceding eruptions. On the 11th he ascended to the top of Mount Gede, and stopped there to pass the night. He was awakened, about four o'clock in the morning, by an explosion of the crater; the fire rose to the height of one hundred and fifty feet. On the same morning stones were cast up from one to four feet in diameter, and many of smaller size; the largest fell the distance of two pals from the crater; the smaller ones, with pieces of brimstone an inch in diameter, four pals; and ashes sixteen pals from the crater. In the eruption of the 2d, the noise of which was louder than the preceding, many stones, about five feet in diameter, fell."

February 26, 1841.—" At Zante, after three days and nights of incessant rain, attended by a violent gale of wind, a most alarming shock of earthquake was felt about seven in the evening. It was not so disastrous as that of last October, but the duration was much longer, the vibration being continued from thirty to thirty-five seconds, while the former lasted only eight or nine. The streets were in an instant filled with the terrified people, eager to fly, but not knowing where to seek safety. In their houses they dreaded being buried in ruins; in the streets they were drenched with rain. Only a few houses fell, either in the town or the surrounding country; others were more or less shaken; and as the shocks recur daily, we have every reason to fear that, in the end, the town will become a heap of ruins. After the earthquake of October, which destroyed nearly all the houses in the island, Zante experienced successive shocks, more or less strong, during forty days, making the number amount to no fewer than 259, and during the remainder of 1840, vibrations of the earth, more or less perceptible, nearly every day. All the violent shocks were attended with dull rumbling sounds and subterranean explosions. Sometimes these noises were heard without being succeeded by any vibration, and sometimes the shocks were silent. Since 1514, Zante has experienced twenty-one earthquakes. That in 1514 divided the hill on which the fortress stood, and buried part of the ancient town in the ruins. In 1767, the shocks were repeated for three months, during which an epidemic disease prevailed. In 1791, the great shock lasted several minutes, caused immense damage, and was followed by minor shocks for six weeks. In 1820, the earthquake, which once more desolated the island, was preceded by a single flash of lightning. That of 1837 lasted with great intensity for twenty seconds; and that of 1480 was the most disastrous of all. In fine, the unfortunate island of Zante has suffered during the sixteenth century, two earthquakes; during the seventeenth, three; during the eighteenth, ten; and during the first portion of the nineteenth century, six."

March 22, 1841.—" The shock of an earthquake was felt in the morning, along the MOSELLE, between Treves and Coblentz, and up the Rhine as far as Camp, in the Duchy of Nassau, a line which coincides with the region of extinct volcanoes in that neighbourhood. The steersman of one of the steamers declares he saw a blue flame rise from a hill at a distance, in the direction mentioned, which, after remaining suspended in the air for some time, descended and disappeared upon the spot it rose from. Letters from Naples bring intelligence of the destruction, by an earthquake, at Reggio, of the Cathedral, four churches, three chapels, the palace of the Provincial Government, the Police Office, and a great number of houses; ten or twelve persons were killed, and three hundred injured. The same shock was felt at Messina."

June 1, 1841.—" An earthquake was felt at Kingston (in JAMAICA) on this day. Several fatal accidents had taken place, in consequence of the rains having flooded many of the roads."

June 12, 1841.—" At 4 P. M., in the island of TERCEIRA, an earthquake occurred, followed by a more severe shock, at 5^h 25′ P. M. On the 13th, tremblings were felt at short intervals. On the 14th, at 4 A. M., a perfectly perceptible undulation destroyed a number of buildings; and the Villa da Praia de Victoria was reduced to a complete ruin at half-past three in the morning of the 15th, by a vibrating and distinctly visible rocking motion. Not a single house or edifice escaped. The ground then remained comparatively quiet till 2^h 40′ A. M.

on the 16th, when a violent shock did farther damage. Every convulsion was preceded by a loud subterranean or submarine noise, which exactly varied in intensity with the force of the shocks. A rent, a mile in length, was formed in the ground extending from the shore. Only some of the severer shocks were felt in the adjoining islands." The soundings round Terceira were not altered."*

On the 12*th June*, there was an earthquake also at St Louis, situated near the junction of the Missouri and Mississippi.

July 4 and 5, 1841.—During the night, between these two days, two shocks of earthquake were felt in the central parts of France, which presented phenomena in many respects extremely interesting.

The following notices are taken from the Journal des Debats of 9th and 15th July:—

"On night between Sunday (July 4, 1841) and Monday at 12^{h} 25′, three shocks of earthquake were felt at Blois, in the suburbs of Vienna and of Foix. The subterranean current seemed to have run along the side of the Grovets as far as Chouzy, and to have shaken specially the alluvial districts.

"The timbers cracked in a way which threatened the safety of the houses. The furniture was displaced and even upset. In the preceding evening, at 8 P. M., a violent storm passed over Blois, but the clouds soon dispersed.

"At 10 P. M., the clouds again accumulated, and after the earthquake shocks, the rain fell in torrents.

"In the course of the same night, between midnight and 1 A. M., several persons observed a globe of fire which burst (eclaté) in the air, and struck against the willow-trees at Sanitas. This circumstance has sometimes made us doubt, whether the violent commotion felt during the same night was the effect of an earthquake. We attribute it to the explosion of this electrical meteor; but since it has been re-

* This last fact was communicated to me, by Captain Vidal, R.N., commanding the surveying vessel sent to Terceira by the British Government, soon after the earthquake, to make fresh soundings.

ported, that the same effects had been felt at St Cloud, Menars, Cellette, Chaumont, Chauzy, and the surrounding places, it is impossible to see in them only an electrical commotion. Besides, the most conclusive phenomena have been remarked: *here*, plates violently set in motion, *there*, beds changed in position; farther off, empty barrels upset. What is most singular is, that even in the oldest buildings, no cracks have been noticed.

"At Moulins, Engilbert (Nièvre), the shock was very severe, and lasted several seconds. People rushed out of bed, and out of their houses, believing that the end of the world was at hand. Large articles of furniture were moved out of their place.

"The earthquake was felt at $12^h\ 45'$, at Paris, Tours, and Nevers. From this town they write, that two detonations, deep and hollow, preceded by several seconds, the principal commotion, which was such that a crowd of persons felt themselves roughly shaken in their beds. The oscillatory movement manifested itself from W. to E., and lasted about $2''$. In this country, where such phenomena are probably without example, there has been no proper explanation yet given of this unusual shock. The atmosphere was all the time in a profound calm, only the air was heavy (lourde), the moon obscured, and one could remark on the horizon great clouds of a red colour charged with electricity.

"At Orleans, in the Hôtel Dieu, the servants and patients distinctly saw the beds move. The terror experienced by certain persons has been so great, that they mention the case of ladies who are still (7th July) ill. A second shock, but much less severe, was felt towards 4 A. M. On the Saturday night, an atmospherical phenomenon sufficiently remarkable occurred, as a precursor. On Sunday evening, about 7½ P. M., a choking heat, which had prevailed all day, gave place suddenly, and without transition, to a violent gust, such as generally precedes storms, and yet there were few clouds in the sky. At Chateau-neuf (near Orleans), the shock was sufficiently strong to crack the plaster of houses, and to shut doors standing open.

"At Pontlevoy (Cher), after a warm and calm day, a wind suddenly and violently got up towards 8 P. M., on the 4th July 1841. The sky was charged with electrical clouds. Vast lightnings illuminated the horizon. At last, at $12^h\ 30'$, a deep and hollow sound was heard, and it was followed by a marked oscillation that lasted $3''$ in the direction from W. to E. At $3^h\ 40'$ A. M., a second movement was repeated. Then slighter oscillations were renewed at a later hour. The weather was calm at 4 A. M."

The same paper, under date Montargis, the 5th instant, states, that "a noise was heard in that town similar to that produced by the explosion of cannon, but as there was no artillery there, every one was inquiring what could have produced it. It was afterwards ascertained that a stone had fallen from the clouds at Chateau Renard, half a league from Montargis. This stone was round and of a dark colour, and weighed 95 lb."

The following paragraph appeared about the same time in Galignani's Messenger:—"The shocks of the earthquake felt at Paris on Sunday night, were perceived with greater force in the centre of France. The same storm that fell over the capital visited a considerable part of France. A rumbling noise is said to have been heard in most spots, attending the oscillations of the ground. At Orleans, on the same evening about eight o'clock, a violent whirlwind passed over the town, and the evening was dark and stormy until midnight, when the horizon began to clear up, with occasional lightning. At this time there was not the slightest wind. At a quarter before one on Monday morning, three shocks of an earthquake roused the inhabitants from their sleep, and occasioned great alarm. They succeeded each other at intervals of from 30 to 35 seconds. The first, which was the most violent, had the effect of a violent motion from east to west; the two others were from north to south. The third, in particular, lasted a pretty long time, and many persons who were lying in their beds, placed in the direction of east and west, felt them move five or six times. Those, on the contrary, whose beds were placed in the direction of north to south, felt their heads and feet rise and fall with great rapidity. This last shock had a noise like that of a heavily-laden carriage passing over a pavement at a distance. A short time afterwards a violent wind rose, and was followed by torrents of rain. At three in the morning the storm was over, but another shock of earthquake was experienced. This was in the direction of north to south, but was less violent. The shocks were felt in all the communes near Orleans, but, with the exception of the derangement of small articles of furniture, and some slight cracking, no evidence remained of them. It is a remarkable fact, that all the insane patients of the asylum at Orleans passed the night in a state of extreme agitation and irritation."

14*th July* 1841.—" On this day a remarkable phenomenon was witnessed at Marseilles, where, between eleven and one in the day, a strong tide set in at the mouth of the harbour and caused the level of the water to rise a foot and a half, knocking several vessels against each other, and causing some alarm. At this port no tides are ever felt, and the cause of this sudden rise of the water is supposed to be connected with a volcanic phenomenon on the other side of the Mediterranean, accounts of which will probably be received at a future period."

16*th August* 1841.—" At Antigua, in the morning of this day, a shock was felt. It was a sudden and severe jerk, with a short subsequent tremor."

17*th August* 1841.—" At St Lucia, on the evening of this day, a most appalling shock of an earthquake was experienced here. Though it did not last for more than twelve seconds of time, the agitation of the earth was of the most violent nature, preceded by a hoarse rumbling sound. We are happy to add, that the shock has not been attended with any deplorable effects here ; only some slight cracks have appeared on a few stone buildings in town."

Martinique.—Three shocks of an earthquake had likewise been experienced in this island.

2*d September* 1841.—At San Jose, Gosta Rica, on the Isthmus of Darien, an earthquake occurred a little after 6 a.m.—" The houses, though much shaken, had not fallen, and we were every instant expecting another shock. The next was less violent, and fortunately did no damage. But for nine days, we were kept in continual alarm, by slighter tremblings of the earth. The houses here are only from 12 to 15 feet in height, as some precaution against the danger of earthquakes. In Cartago, five leagues hence, the effects were dreadful. In less time than I can write, a city of 10,000 souls was laid in ruins. The cause of these calamities has been the eruption of a volcano three leagues beyond Cartago. The last earthquake took place in the year 1822."

25*th September* 1841.—This was the day on which a remarkable disturbance took place in the magnetic equilibrium of the earth, as indicated by observations on the diurnal motion of the magnets made simultaneously at Greenwich, Toronto (in Canada), St Helena, Cape of Good Hope, and at Trevandrum (in India).*

It is stated in the Toronto report of this disburbance, that one had occurred there on the very same day in the previous year; and that "the months of September and October appear to be those of greatest disturbance."

No earthquake-shocks are noticed as having occurred on this occasion.

End of October 1841.—The town of Komorn, on the island of Schutt, in Hungary, at the confluence of the Waag and the Danube, was visited by an earthquake, which shook down all the timber houses, and seriously damaged other buildings. In consequence of this disaster, upwards of five thousand people are said to be ruined, and left without shelter. The general loss is estimated at an immense amount.

27*th December* 1841.—At $6^{h}\ 30'$ A.M., in Calabria, there was a strong shock of earthquake, which lasted $15''$.

From the foregoing notices of shocks in various parts of the world during the years 1840 and 1841, it appears—

(1.) That there were only three occasions on which shocks occurred on the same day in different parts of the world,—

* Reports of British Association for the Advancement of Science, 1841, p. 346.

viz. on 22d March 1841 (at Comrie and in Germany), on 12th June 1841 (at Terceira and St Louis), and on 4th July 1841 (in Argyleshire and in France). But in these coincidences, there is nothing to indicate that they were not purely accidental.

(2.) That on the occasion of the most violent shocks, violent atmospherical disturbance was, as usual, exhibited.

Thus, the one under date 26th February 1841, took place "after three days and nights of incessant rain, attended by a violent gale of wind."

In the account of that shock, reference is made to one of older date, which "was preceded by a single flash of lightning;" a phenomenon, common enough in all volcanic eruptions, as exemplified under date 22d November 1840.

In those parts of France affected by the earthquake of 4th July 1841, it will be seen, that at *one* place "a violent whirlwind" occurred, succeeded first by a calm, "with occasional lightning," and afterwards by "a violent wind" and "torrents of rain;"—at *another* place, "a globe of fire burst in the air, and struck against the willow trees;"—at a *third* place, a noise like "the explosion of cannon" was heard, near to which place a stone fell "from the clouds"—weighing 95 lbs."

Having mentioned the shocks of earthquake in Great Britain as well as abroad during the years 1839, 1840, and 1841, we proceed with an account of such as have since occurred, in so far as accounts of them have been obtained.

I. *Notices of British Earthquakes during years* 1842 *and* 1843.

We begin with an extract from the Comrie Register.

LIST OF EARTHQUAKE SHOCKS FELT AT COMRIE IN THE YEARS 1842 AND 1843.

Year, Month, and Day.		Time of Day.	Strength of Shock.	Barometer at 9 A.M.	Barometer at 9 P.M.	Average of Bar. for Month.	Quantity of Rain for Month.	
1842.								
January	2.	A. M.	1					
~	7.	A. M.	1				2.21	for Jan.
~	~	A. M.	1					
March	10.	1 P. M.	1				4.15	for Feb.
April	21.	3 P. M.	1				4.95	for March.
~	22.	10 30′ P. M.	1				0.02	for April.
~	~	11 P. M.	2					
June	1.	12 A. M.	1				3.14	for May.
~	2.	1 A. M.	2					
~	8.	1h 30′ A. M.	..	...	...	30.11	3.83	for June.
~	~	1h 45′ A. M.	..					
July	1.	2h 30′ P. M.	1					
~	~	4h 30′ P. M.	1	...	...	29.78	2.59	for July.
~	?	9 A. M.	1					
~	10.	3h 30′ P. M.	2					
August	?	?	..					
~	?	?	..	...	...	29.93	1.56	for Aug.
~	?	?	..					
~	27.	11h 45′ P. M.	1					
Sept.	2.	3 A. M.	1					
~	24.	5h 53′ A. M.	3	...	...	29.75	2.81	for Sept.
~	~	6h 59′ P. M.	2					
~	25.	A. M.	1					
~	~	A. M.	1	...	...	29.87	0.90	for Oct.
Novem.	18.	12 P. M.	1					
~	29.	12h 11′ P. M.	1	29.38	29.67	29.62	2.83	for Nov.
Decem.	4.	2 A. M.	..	29.15	...			
~	17.	8 A. M.	1	29.42	29.52	29.75	6.27	for Dec.
~	~	A. M.	1	...	...	29.43	2.91	for Jan.
~	~	A. M.	1	...	...	29.69	1.04	for Feb.
1843.								
March	23.	8h 30′ P. M.	2	29.17	29,35	29.72	3.22	for March.
~	~	11 P. M.	1					
May	14.	1 A. M.	1	...	...	29.59	5.17	for April.
~	~	2 A. M.	1					
~	28.	12h 20′ P. M.	1	...	...	29.60	3.56	for May.
June	4.	12h 15′ P. M.	2	29.82				
~	~	3 P. M.	..	...				
~	10.	2h 30′ A. M.	..	29.46	29.78	29.90	2.22	for June.
~	~	3 A. M.	..					
~	15.	?	1	30.1				
~	17.	4 A. M.	2					
July	2.	11 P. M.	1					
~	6.	10h 25′ A.M.	1	29.29	29.64			for July.
~	16.	8h 30′ P.M.	1	...	30.00			
August	25.	10h 40′ A. M.	5					for Aug.
~	~	11 A. M.	1					
~	~	2 P. M.	1					
Sept.	2.	12 A. M.	2					for Sept.
~	~	4 A. M.	1					for Oct.
~	~	6 A. M.	1					
Novem.	26.	4 A. M.	0					for Nov.

(The third column in this table, indicates the relative intensity of the shocks, ten being taken as marking that felt on 23d October 1839, which is the severest felt at Comrie during the present century.)

This list, like the one previously given, indicates only the shocks attended by movement, and not those indicated by sound only. There is no shock producing a movement which is not accompanied by noise of one kind or other, but occasionally the peculiar noise is heard, without either heave or tremor.

In regard to several of the shocks recorded in the foregoing list, some observations were made, not stated in the table, and which will now be specially noticed.

8th June 1842.—The horizontal pendulum in Mr M'Farlane's house was moved, indicating a vertical upheave of the ground, rather more than half an inch.

29th November 1842.—Cloudy, showery day, but fine.

4th December 1842.—Both morning and evening cloudy; a red sunset.

17th December 1842.—Cloudy and showery; towards evening, rain and wind. The shocks here recorded were felt only at Tomperran, half a mile east of Comrie.

23d March 1843.—Day very foggy, dark and damp, but fair till 3½ P.M.; very heavy shower at 4 P.M.; rest of day showery; the wind was gentle from east; At the instant of first shock, and for five minutes after, the barometer stood at 29.12, at the instant of the second shock, it stood at 29.1.

14th May 1843.—Showery and cloudy; a blink at noon, and at 1 P.M.; afterwards chill and cloudy; a breeze from SE.

4th June 1843.—The first shock registered is said to have been felt with about equal severity at Comrie, St Fillan's (six miles west), Clatheck (three miles east), and at Invergeldy (six miles N. W.) At Comrie, the vertical pendulums were moved E. and W., about one-eighth of an inch. The horizontal pendulum sunk $\frac{1}{16}$th of an inch. The second shock was felt at Clatheck, but not at Comrie. The day cloudy, with a cold wind and slight skiffs of rain; after 5 P.M. very cold.

10th June 1843.—Cloudy day with gleams of sunshine; rather cold; windy in evening.

15th June 1843.—Two other very slight shocks thought to have been felt to-day; clear sunshine and very warm.

17th June 1843.—Clear sunshine and very warm.

2d July 1843.—Cloudy; slight drizzle on hills; fine Scotch mist after 3 P.M.; rain in evening.

6th July 1843.—Dull day; cloudy, with showers; rather cold.

25th August 1843.—The day in succession, cloudy, cold, showery, and close. There was rain from 2 to 4½ P.M.

Mr M'Farlane reports, that the index of the long spiral pendulum in the steeple at Comrie, made a rut in the powder ¾ of an inch in a direction W. by N. There was no disturbance of the powder, on the opposite side of the central point. At Kingarth (about two miles north of Comrie), the inverted pendulum vibrated in a S.W. and N.E. direction. The horizontal pendulum intended to be affected by an upward movement of the ground, sunk fully half an inch. At Clatheck, the shocks produced no undulation, but a considerable tremor.

At the instant of the shock, the barometer and thermometer were respectively 29,5 inches and 56°; five minutes after the shock 29,5 inches and 55°.

In examining the list of shocks just given, it will be perceived that the height of the barometer was, with scarcely one exception, on the day of their occurrence, lower than its average height during the month. Farther, it would appear that, on the 23d March 1843, the barometer, at the *instant* of the two shocks, was lower than the average of the day on which they occurred. It is matter of regret, that the height of the barometer, at the instant of all the shocks, was not marked.

Abstract of Meteorological Observations for 1843, *made at Edinburgh, in North Latitude* 55° 57′ 20″, 3° 10′ 30′, *W. L., distant from the Sea two miles.* By ROBERT D. PAUL, Esq.

I.—TEMPERATURE.

1843.	High.	Low.	Mean.	Highest 8½ A.M.	Lowest 8½ A.M.	Mean.	Highest 8 P.M.	Lowest 8 P.M.	Mean.	Mean of Morn. and Even.
Jan. ...	51°	23°	36.67°	48°	27°	36.41°	52°	24°	36.80°	36.60°
Feb. ...	42	16	32.21	38	22	32.30	38	21	32.39	32.34
March	60	24	40.45	47	27	38.64	49	31	39.67	39.15
April...	64	27	45.30	52	35	44.76	50	34	43.93	44.34
May ...	63	37	47.54	54	42	47.38	52	41	45.83	46.60
June ...	72	41	52.33	60	44	53.65	63	43	51.76	52.70
July ...	71	45	57.19	68	50	58.09	65	54	58.22	58.15
Aug. ...	77	45	59.16	66	53	59.25	68	53	58.87	59 01
Sept....	75	34	57.66	66	43	56.60	66	44	57.46	57.03
Oct. ...	62	27	43.16	58	33	43.32	60	32	42.77	43.04
Nov. ...	52	29	40.53	47	32	40.13	48	33	40.13	40.13
Dec. ...	54	30	45.29	53	36	45.41	53	32	45.58	45.49
Average	61.91	31.50	46.44	54.75	37.00	46.32	53.33	36.83	46.11	46.21

REMARKS.

Thermometer.		Highest, 17th Aug. 77°.	Wind,	E. N. W.
...		Lowest, 14th Feb. 16°.	...	N.
...	Morning.	Highest, 68° on 26th July.	...	W.
...	...	Lowest, 22° on 6th Feb.	...	N. E.
...	Evening.	Highest, 68° on 17th Aug.	...	E. N. W.
...	...	Lowest, 21° on 14th Feb.	...	N.

Mean temperature of the year 1843, 46″.44. Mean of 1842, 48°.06,—difference 1 .62 Coldest month, February—Warmest, August. The December of this year, again, is worthy of particular notice.

II.—PRESSURE.

1843.	Highest 8½ A.M.	Lowest 8½ A.M.	Mean.	Highest 8 P.M.	Lowest 8 P.M.	Mean.	Highest 11 P.M.	Lowest 11 P.M.	Mean.
Jan.	30.21	28.32	29.403	30.20	28.23	29.357	...	...	...
Feb.	30.11	28.89	29.605	30.15	29.09	29.608	...	...	...
Mar.	30.20	29.08	29.752	30.20	29.16	29.750	...	...	...
Apr.	30.07	29.07	29.563	30.19	29.02	29.609	...	...	...
May	30.29	29.28	29.710	30.29	29.27	29.696	...	...	...
June	30.12	28.82	29.752	30.08	28.83	29.763	...	...	...
July	30.10	29.34	29.720	30.09	29.23	29.725	...	...	...
Aug.	30.13	29.20	29.763	30.20	29.17	29.762	...	...	...
Sept.	30.44	29 39	30.265	30.41	29.40	30.278	30.43	29.40	29.991
Oct.	30.17	28.85	29.544	30.12	28.71	29 507	30.18	28.74	29.476
Nov.	30.10	28.80	29.527	30.21	29.01	29.528	30 23	28.94	29.536
Dec.	30.28	29.51	30.021	30.29	29.45	30.026	30.30	29.40	30.021
Aver.	30.44	28.32	29.718	30.41	28.23	29.709	30.43	28.74	29.756

REMARKS.

Barometer.		Highest, 30.44 inch.	on 23d Sept.	Wind,	W.
...		Lowest, 28.23 ...	on 13th Jan.	...	S. E.
...	Morning.	Highest, 30.44 ...	on 23d Sept.	...	W.
...	...	Lowest, 28.32 ...	on 13th Jan.	...	S. E.
...	Evening.	Highest, 30.43 ...	on 23d Sept.	...	W.
...	...	Lowest, 28.23 ...	on 13th Jan.	...	S. E.

Mean height of barometer for the last four months, at 11 P. M., 29.756.

During January, the barometer fluctuated for one week at a mean of 28.73 inches. The depression in question had been coming on for eight days previously, and terminated on the 16th with the wind at north. Six days after the storm of the 13th, the barometer had risen two inches; the northerly wind suddenly supervening on the southerly being the cause and acting as a counterpoise.

January had the least mean pressure, and September the greatest; the latter month being 00.535 inches above the mean of the same month in 1842. Lastly, although the December of this year had again a very high temperature, it is also remarkable as being, in point of pressure 00.329 inches above the mean of the corresponding month of last year.

III.—PREVAILING WINDS AND THEIR MEAN TEMPERATURE.

1843.	West.		N. West.		North.		N. East.		East.		S. East.		South.		S. West.		Prevailing Wind.
	D.	°	D.	°	D.	°	D.	°	D.	°	D.	°	D.	°	D.	°	
Jan.	7	40.4	7	31.4	4	29.7	...		...		2	34.0	1	43.0	10	40.4	W. & NW.
Feb.	3	29.6	2	28.5	5	25.6	8	33·8	9	35.5	1	33.0	...		...		E. & NE.
Mar.	7	39.5	...		4	33.5	1	39.0	7	38.4	6	42.5	2	50.0	4	45.0	E. & W.
Apr.	7	45.1	2	40.0	4	38.0	1	41.0	3	45.0	1	49.0	2	49.5	8	49.3	W. & SW.
May	4	49.2	...		3	48·3	2	44.0	17	46.5	1½	47.5	...		3½	51.0	E.
June	5½	54.5	1	53.0	1½	52.0	...		19	51.5	...		...		1	54.0	E.
July	17	57.9	3	57.6	4	55.2	1	52.0	3½	58.5	...		...		1½	58.0	W.
Aug.	14	58.7	0½	...	3	58.3	0½	...	3	62.0	1½	63.0	1	64.0	6½	58.0	W.
Sept.	10	58.9	1	54.0	5½	46.8	2	62.5	6	60.0	1½	57.0	...		4	60.0	W.
Oct.	15	46.8	3½	40.3	5	33.7	2	44.0	1½	41.0	1	35.0	1	39.0	2	46.0	W.
Nov.	7	38.0	2	42.0	1	36.0	2	41.5	...		5	39.6	2	38.5	11	42.3	SW.
Dec.	16	46.1	2	42.0	1½	35.0	...		...		1	45·0	3	45.5	7½	46.8	W.
Aver.	112½=47.0		24=43.2		41½=41.0		19½=44.7		69=48.7		21½=44.5		12=47.0		59=50.0		

Winds,	W. & NW.	136½	times,	temperature	45°.1
...	N. & NE.	61	...	...	42.8
...	E. & SE.	89½	...	...	46.5
...	S. & SW.	71	...	...	48.5
		358			

During the year, the variable winds prevailed twice in April, twice in June, and once in August. The prevailing winds this year, as in the last, are the W. & NW.

Rain	fell on 193	days.
Hail ...	30	...
Snow ...	21	...
	244	

Aurora Borealis, &c. observed.

On 28th January, 5th, 6th, and 23d April, 22d August, and 6th December; Lunar Halos were frequent during the year; but the only solar one observed by me, which was colourless, occurred on the 12th January.

Abstract of Meteorological Observations for 1843, *made at Applegarth Manse, Dumfriesshire.* By the Rev. Wm. Dunbar.

Long. 3° 12′ W. Lat. 55° 13′ N. Height above the Sea, 180 feet; Distance from the Sea, 10 miles. Rain Gauge 5 feet from the ground.
The observations made at 9 A.M. and 9 P.M.

BAROMETER.

1843. Monthly.	Atmospheric Pressure, Morning.	Atmospheric Pressure, Evening.	Mean of Morning and Evening.	Red. to 32° Fahr. and corrected to sea-level.	Mean Daily Range.	Mean Nightly Range.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest range in 24 hours.	Least range in 24 hours.
January,	29.46	29.47	29.46	29.61	0.160	0.118	0.278	30.28	28.03	0.84	0.04
February,	29.58	29.58	29.58	29.74	0.094	0.094	0.188	30.08	28.85	0.65	0.01
March,	29.75	29.74	29.74	29.88	0.096	0.100	0.196	30.24	29.15	0.45	0.04
April,	29.59	29.36	29.47	29.60	0.100	0.131	0.231	30.13	29.08	0.61	0.03
May,	29.72	29.68	29.70	29.81	0.073	0.035	0.108	30.25	29.10	0.40	0.02
June,	29.73	29.76	29.74	29.85	0.092	0.077	0.169	30.08	28.85	0.71	0.01
July,	30.09	29.80	29.94	30.08	0.112	0.078	0.190	30.14	29.32	0.57	0.00
August,	29.80	29.82	29.81	29.90	0.080	0.084	0.164	30.18	29.23	0.60	0.01
September,	30.09	30.08	30.08	30.17	0.064	0.076	0.140	30.47	29.73	0.43	0.02
October,	29.51	29.53	29.52	29.63	0.150	0.120	0.270	30.24	28.61	0.87	0.02
November,	29.59	29.63	29.61	29.72	0.169	0.159	0.328	30.28	28.91	0.76	0.08
December,	30.08	30.08	30,08	30.29	0.077	0.184	0.181	30.33	29.47	0.59	0.03
Means,	29.75	29.71	29.73	29.85	0.105	0.104	0.204				
1842,	29.77	29.74	29.75	29.89	0.097	0.103	0.200				

THERMOMETER.

Months.	Mean of greatest Heat.	Mean of greatest Cold.	Mean Temp. of Morning.	Mean Temp. of Evening.	Mean of Extreme.	Mean of Morning and Evening.	Mean of both.	Mean range of 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest range of 24 hours.	Least range of 24 hours.	Mean Temp. of Spring water.
January,	43.3	33.3	36.3	38.3	38.3	37.3	37.8	10.0	53.0	18.0	24.0	3.0	43.2
February,	38.8	30.8	33.6	34.0	34.8	33.8	34.3	8.1	46.0	17.0	17.0	0.0	41.6
March,	47.6	34.8	39.5	40.7	41.2	40.1	40.7	12.7	59.0	20.5	25.5	3.0	46.1
April,	53.7	38.6	45.2	48.3	46.1	46.7	46.4	14.8	65.5	26.0	23.0	8.0	47.0
May,	57.7	41.3	50.2	48.5	49.5	49.3	49.4	16.4	69.0	35.0	31.0	4.0	48.0
June,	63.6	45.2	55.1	54.9	54.4	55.0	54.7	18.2	76.0	41.0	33.5	5.5	50.7
July,	64.1	50.8	57.0	57.4	57.4	57.2	57.3	13.8	72.5	44.5	20.0	7.5	54.6
August,	66.3	49.2	58.0	58.6	57.7	58.3	58.0	17.1	79.0	39.0	26.0	5.0	53.5
September,	65.5	46.7	56.1	56.2	56.6	56.1	56.3	18.7	77.0	30.5	33.5	8.0	62.0
October,	50.8	38.0	42.8	43.8	44.4	43.3	43.8	12.7	58.0	29.0	21.0	1.0	45.5
November,	47.7	36.6	39.7	43.1	42.1	41.4	41.7	11.0	55.0	25.0	18.5	2.5	44.2
December,	50.0	44.1	45.8	45.8	47.0	45.8	46.4	5.9	57.0	33.0	16.5	2.0	46.1
Means,	54.1	40.7	46.6	47.4	47.4	47.0	47.2	13.2					
1842,	54.3	41.1	47.5	47.7	47.7	47.6	47.6	13.7					

Dr Dunbar's *Meteorological Observations for* 1843.—(Continued).

WINDS—THEIR DIRECTION AND FORCE, AND WEATHER, STATED IN THE NUMBER OF DAYS IN WHICH EACH PREVAILED.

Months.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	Moderate.	Brisk.	Strong Breeze.	Stormy.	Sun shone out.	Rain fell.	Snow and Hail.	Frost.	Thunder.	Rain in Inches.
January,	...	...	4	2	3	8½	7½	6	10	5	3	7	6	19	19	5	11	...	3.02
February,	4	12	6	...	...	...	...	6	8½	5	4½	7	3	24	5	5	13	...	0.95
March,	½	4½	5½	10	4½	4	2	...	11½	8	1	10½	...	25	10	...	12	1	0.98
April,	2½	2	1½	1	6	9½	4½	3	11½	10½	1	7	...	28	13	H 2	6	1	4.54
May,	...	5	4	9	7	1	4	1	12	14	3	2	...	29	14	...	2	1	2.12
June,	2	10	5½	1	3½	1	3	4	5	7½	6	11½	...	25	7	...	...	2	2.45
July,	...	1	...	3	5	8	5	9	10	13	2	6	...	26	21	H 1	...	3	5.31
August,	...	...	2	3½	8½	9	7	1	16	8	3	4	...	28	16	...	...	2	3.75
September,	4½	2	5	1	3½	9	3	2	14	9	4	3	...	28	7	...	2	2	0.19
October,	...	...	3	...	1	11	8	8	9	8	7	7	..	25	13	H 1	5	...	4.16
November,	1	3	2½	...	3½	12	6	2	11	8	1	8	2	24	19	...	3	...	3.41
December,	...	...	...	4	2	20	4	1	14	9	...	6	2	21	20	...	1	...	1.81
Total,	14½	39½	39	34½	47½	93	54	43	132½	105	35½	79	13	302	164	14	55	12	32.69
1842,	9	40	42	50	41	94	44½	44½	126	100½	45	80½	13	306	147	22	55	9	25.82

REMARKS.

The maximum of atmospherical pressure was on the 23d of September, when the Mercury stood at 30.47 inches. The minimum on the 19th of January, viz. 27.99. Extreme range, 2.48 inches.

The maximum of temperature was on the 19th of August 79°. In 1842 it was 79° on the 17th of August. The minimum on the 16th February, viz. 17°. Difference of extremes 62°. The mean of extremes exceeds the mean of Morning and Evening by 4–10ths of a degree. The mean temperature of 1843 was 47.2 degrees, less than that of 1842 by 4–10ths of a degree, but exceeds that of the last 20 years by 1 and 2–10ths of a degree. As in 1842, the high temperature of December is remarkable, being 10 degrees above the mean of that month for the last 20 years, and equal to the mean annual temperature of that period.

The Rain in 1843 exceeded that of the preceding year by 7 inches, but was less than the average of the last 15 years by 2 inches.

SCIENTIFIC INTELLIGENCE.

GEOLOGY AND MINERALOGY.

1. *Sandstone Pillars and Caves of North-Western Australia.*—We here remarked a very curious circumstance; several acres of land on this elevated position were nearly covered with lofty isolated sandstone pillars of the most grotesque and fantastic shapes, from which the imagination might easily have pictured to itself forms equally singular and amusing. In one place was a regular unroofed aisle, with a row of massive pillars on each side; and in another there stood upon a pedestal what appeared to be the legs of an ancient statue, from which the body had been knocked away. Some of these time-worn columns were covered with sweet-smelling creepers; while their bases were concealed by a dense vegetation, which added much to their very singular appearance. The height of two or three which I measured was upwards of forty feet; and as the tops of all of them were nearly upon the same level, that of the surrounding country must at one period have been as high as their present summits, probably much higher. From the top of one of these pillars I surveyed the surrounding country, and saw on every side proofs of the same extensive degradation; so extensive, indeed, that I found it very difficult to account for; but the gurgling of water, which I heard beneath me, soon put an end to the state of perplexity in which I was involved, for I ascertained that streams were running in the earth beneath my feet; and on descending and creeping into a fissure in the rock, I found beneath the surface a cavern precisely resembling the remains that existed above ground, only that this was roofed, whilst through it ran a small stream, which in the rainy season must become a perfect torrent. It was now evident to me, that ere many years had elapsed the roof would give way, and what now were the buttresses of dark and gloomy caverns would emerge into day, and become columns clad in green, and resplendent in the bright sunshine. In this state they would gradually waste away beneath the ever during influence of atmospheric causes; and the material being then carried down by the streams through a series of caverns resembling those of which they once formed a portion, would be swept out into the ocean, and deposited on sand-banks, to be raised again at some remote epoch a new continent, built up with the ruins of an ancient world. I subsequently, during the season of heavy rains, remarked the

usual character of the mountain-streams to be, that they rose at the foot of some little elevation, which stood upon a lofty table-land, composed of sandstone, then flowed in a sandy bed for a short distance, and afterwards mysteriously sank in the cracks and crevices made in the rocks from atmospheric influences, and did not again reappear until they had reached the foot of the precipice which terminated the table-land whence they sprang; here they came foaming out in a rapid stream, which had undoubtedly worked strange havoc in the porous sandstone-rocks, among which it held its subterraneous course. What the amount of sand annually carried down from the north-western portion of Australia into the ocean may be, we have no means whatever of ascertaining: that it is sufficient to form beds of sand of very great magnitude, is attested by the existence of numerous and extensive sand-banks all along the coast. One single heavy tropical shower of only a few hours' duration, washed down, over a plot of ground which was planted with barley, a bed of sand nearly five inches deep, which the succeeding showers again swept off, carrying it farther upon its way towards the sea.—*Grey's Journals of his Expeditions of Discovery.*

3. *On the Chemical Constitution of Wolfram.* By M. Marguerite. —Chemists are agreed as to the nature of the elements which are contained in Wolfram, but they appear to be ignorant of the degree of oxidizement in which the tungsten exists. In a late sitting of the Institute, M. Pelouze stated the results of the experiments which M. Marguerite had performed to determine this point.

Admitting, of which, indeed, no doubt can be entertained, that the analyses of Vauquelin, Berzelius, and Ebelmen, are correct, the following formulæ may be assigned to Wolfram:—

1st, $3\,Wo^3\,FeO.\,MnO\,Wo^3$.
2d, $3\,W^2o^5\,FeO^3\,MnO\,Wo^3$.
3d, $4\,(W^2\,O^5)\,3\,(Fe^2\,O^3)\,Mn^2\,O^3$.

The author decides in favour of the last formula, which represents Wolfram as a compound of blue oxide of tungsten, and of two-thirds oxide, of either iron or manganese. These oxides being isomorphous, if R represent iron or manganese, or a mixture of these two metals, we arrive at the formula

$$W^2\,O^5\,R^2\,O^3,$$

which will represent all the varieties of Wolfram.

The first formula, which represents the tungsten as in the state of tungstic acid, and the iron in that of protoxide, is not admissible;

for, on the one hand, cold hydrochloric acid separates peroxide of iron, from the mineral, and leaves blue oxide of tungsten; and, on the other hand, the free protoxide of iron reacts on the tungstic acid isolated, and reduces it to the state of the blue oxide, and itself becomes peroxide.

When heated to ebullition in hydrochloric acid, the blue oxide, in its turn, reduces the salts of iron, and this reaction explains how Berzelius and Ebelmen adopted the first formula.

The superior oxides of manganese convert the blue oxide into tungstic acid, under the same circumstances as the peroxide of iron, even when cold. This circumstance induced the author to suppose that the manganese might exist as the oxide $Mn^2 O^3$, isomorphous with the sesquioxide of iron, although found only in the state of protoxide in the solution of the mineral.

M. Marguerite further observes, that it is easy to explain the causes of the different opinions which have been given on the chemical constitution of Wolfram; that it had not been deduced from the very exact analyses already alluded to, is, because the last phase of an operation was considered instead of its commencement.

M. Marguerite concludes that,—

1*st*, The tungsten in Wolfram is in the state of blue oxide.

2*d*, The iron is in the state of peroxide.

3*d*, The tungstic acid and protoxide of iron obtained are the results of the analytic means employed, and that these two products are eventually formed from each other.

4*th*, Of the three formulæ which have been given, the second and third only agree with experiments, and the second may be reduced to $W^2 O^5 R^2 O^3$.—*Journ. de Ph. et de Ch.*, Octobre 1843.—*Philosophical Magazine*, Vol. XXIV., No. 157, p. 153.

3. *Analysis of Ancient and Fossil Bones.* By MM. Girardin and Preisser.—The authors found that human bones taken from various ancient tombs contained from less than 1 up to 8 per cent. of phosphate of magnesia; and in one case the bones of an infant taken from a Gallo-Roman tomb at Rouen, were found to be of a fine chrome-green colour, and contained 3.1 per cent. of carbonate of copper, for the existence of which no sufficient cause appeared.

In ancient buried bones, as well as in the fossil bones of animals, the authors always found a much greater quantity of phosphate of lime than in recent bones. Under certain unknown circumstances, this salt suffers some curious modifications, by which it is converted into, for the most part, sesquiphosphate of lime, which crystallizes in small hexagonal prisms on the surface of the bones. This transformation is effected without either gain or loss of elements, and solely by a simple change in the relation or the position of the elementary atoms of the salt, so that the sub-phosphate of lime, the original composition of which is 8 C *a* O, 3 P^2 O^5, is divided into two more permanent compounds, neutral phosphate and sub-sesquiphosphate, the production of which is explained by the following equation :—

$$8\ Ca\ O,\ 3\ P^2\ O^5 = (2\ Ca\ O,\ P^2\ O^5) + 2\ (3\ Ca\ O,\ P^2\ O^5).$$

It is very probable that it is the tendency of the sub-sesquiphosphate of lime to crystallize, which occasions its formation. Many facts prove the mobility of the elements of phosphate of lime, and the property which it possesses of undergoing slight changes in its constitution; without these two circumstances, it could not, as observed by Berzelius, perform the functions which render it so important in the animal and vegetable economy. The crystals which form on the surface of burnt bones are identical with the apatite of mineralogists.

The authors were unable to detect the slightest trace of fluoride of calcium in ancient human bones, whereas they always met with it in fossil animal bones ; the existence of this salt in recent human and animal bones is more than doubtful. MM. Berzelius and Morichini are the only chemists who have stated its existence in recent bones. Fourcroy and Vauquelin, Klaproth, Dr Rees, and the authors of this paper, were not able to detect it in fresh bones.

It follows from these introductory statements, that, at any rate, the presence of fluoride of calcium, even if it ever exist in recent bones, is accidental, and not constant, and that as this salt exists in all fossil bones, it must necessarily have arisen by infiltration from without, for neither mineralization nor fossilization has the power of creating mineral substances.

When, therefore, fluoride of calcium is found in notable quantity in any unknown bone, it may be considered as a fossil bone of an antediluvial animal, and not as a human bone.—*Ann. de Ch. et de Phys., November* 1843. *Philosophical Magazine*, Vol. 24. No. 157, p. 154.

ZOOLOGY.

4. *The Geographical Distribution of the Articulated Animals.* By M. E. Blanchard.—The distribution of animals over the surface of the globe has long attracted the attention of naturalists ; and that of insects in particular has been made, by the celebrated Latreille, the subject of a special work. At the period when this work was undertaken, the native country of the greater part of the species was

too imperfectly known to enable any one to give a precise definition of the different entomological regions, and to trace their respective limits on the surface of the globe. In the absence of a natural division, Latreille proposed a systematic one, dividing each hemisphere into zones, containing 12° of latitude, and subdividing each of these zones by meridians distant 24°. We can now substitute for these arbitrary limits those which result from the configuration of the ground, and accomplish for the branch of natural history with which we are at present concerned, what M. Milne Edwards has successfully done in regard to the Crustacea. Proceeding as he has done, we find a certain number of points, which may be considered, in regard to insects, as centres of creation, whence the species have radiated, and the mountains, in many cases, present us with the natural limits of these regions, in the same manner as is done by the extent of seas in the instance of the other articulata considered by M. Milne Edwards.

We therefore divide the surface of the globe into 55 regions, each characterized by the most typical genera and species which are peculiar to it, and the limits of which are fixed at the point where the greatest number of these genera and species cease to exist.

The geographical distribution of animals, considered in relation to their classification, may be of great importance in appreciating the value of the characters on which generic groups are established. Indeed, when we examine each family, and each genus by itself, we are surprised to perceive that there are some of them which inhabit certain countries of the globe of very limited extent, while others are scattered over a much more considerable space; and that others, finally, inhabit the greater part, and sometimes almost the entire surface of the globe. Moreover, we perceive that tribes and genera exist sometimes comprehending a very great number of species which have all the same peculiar *facies*, and the strongest resemblance to each other in form and colours; in such a case we are certain that these are very natural genera. Coming after these, we find a considerable number of genera which are still with propriety regarded as natural, but which, at the same time, are so in an inferior degree to the preceding. All the species of which these are composed have a great analogy to each other, but we may observe minute differences in form and the greatest dissimilarity in colours. Lastly, when we pass to a third sort, we find genera, tribes, and entire families, which possess, it is true, the common organs which furnish the most important zoological characters, but which present very obvious modifications in the form of the body, and particularly in the colours. We are soon, however, enabled to ascertain that the differences between certain species and the generic type, to which we are obliged to refer them, are always in relation with the homogeneousness, or the reverse, of the places which these animals inhabit. The more natural a genus is, the more restricted will be found to be the portion of the globe to which it is confined, and in it the circumstances of the soil, humidity, and temperature, will be found identical; the less natural a genus is, the more extensively will it be found distributed over different parts of the globe, under the influence of different circumstances.

Species are found to stand in the same relation to the places which they inhabit, as genera, tribes, and families. Certain species are spread over a wide extent of country, and others inhabit only certain very limited localities; those which are distributed over a great extent present individual varieties without number, while no varieties appear among those which live in the same places.

This leads us to the consideration of a question which has already been often treated of, but has never yet been sufficiently investigated; we refer to the influence which climate exercises on forms and colours. This influence is particularly manifest among insects and the arachnides. It is very true that a multitude of small species are spread from the equator to the poles, without presenting any thing remarkable at these two extremes; but it is demonstrated, at the same time, that species of large size, or of singular forms, or brilliant colours, always require the conditions of high temperature and much humidity. In proportion as these conditions are diminished, the species become smaller and less deeply coloured. In Africa, where the soil is generally dry and arid, and the heat considerable, we find fewer species of large size than in inter-tropical America, or in the East Indies, and the greater proportion are black, or of not very bright colours. In cold countries the large species disappear; scarcely any of brilliant colours are found; the species of an intense black also cease to be met with, till at last we find none but of a shining black, greyish or brownish.

M. Milne Edwards has observed that the species of crustacea were much more numerous towards the equator than in northern regions, but that the individuals considered collectively were not less numerous in northern regions. The same thing certainly does not take place with insects and arachnides; the species, it is true, are generally much more multiplied towards the equator than towards the poles; but nothing indicates that the species of the north are more numerous in individuals than those of the tropics; we are assured, indeed, that the contrary is the case.—From *Comptes Rendus*, vol. xii. page 1216.

5. *Wingless Birds of New Zealand.*—Professor Owen lately gave a communication to the Royal Institution of London, on the Wingless Birds of New Zealand. In the year 1839 there arrived in this country, from New Zealand, a fragment of the shaft of a bone of some unknown animal, supposed to have existed in those islands during the historical period. From this single relic, deficient in those terminal processes to which the zoologist looks for a clue to his researches into the probable forms and habits of extinct animals, Professor Owen inferred that this bone must have belonged to a struthious bird, about the size of an ostrich, but resembling the extinct Dodo of the Mauritius. Since that time, other bones, belonging to birds of the same family, but of different species, have reached England, and established, beyond all doubt, the justice of Mr Owen's inference, made four years ago, on such scanty data. The great point of Mr Owen's present communication, was to explain the process of reasoning which led him to this result. Looking into the interior of the piece of bone he had to examine, he observed, that its cancellous structure was less fine and fibrous than that of any of the long bones of a mammiferous animal,—that it was still less like the bone of a reptile, which is generally solid throughout,—that with respect to the remaining order of the animal kingdom, the birds, the structure of this bone, its density and size, proved that, though the bone of a bird, it could not belong to any that were organized for flight. Mr Owen also remarked, that although a sufficient supply of various bones of the leg and foot of this bird had subsequently been received by him, to enable him to characterize several species, there had not appeared any bones of wings. Hence, he concluded, that this bird must have resembled—only on a gigantic scale—the Apteryx (the wingless bird) of Australia. Mr Owen called attention to a specimen of Apteryx, lent by the Council of the Zoological Society. He noticed the long beak of this bird, resembling the bill of a woodcock, its legs, like those of a fowl, attached to a trunk like that of a cassowary; and then appealed against the reasoning which disputed the reality of the Dodo's existence, because the same sort of body and legs was found on that bird, united with a beak resembling that of a vulture. Mr Owen stated, that, on visiting the Hague, he saw there a picture, painted soon after the Dutch had become possessed of the island of Mauritius, and in a corner of this picture was a figure of the Dodo, extremely small, but so elaborately finished, as to enable a zoologist to characterize its species. Mr Owen then offered some speculations as to the extensive distribution of the struthious birds over the surface of the earth

in remote ages. He referred to the recently-discovered foot-prints of a bird, similar to this gigantic wingless bird of New Zealand (to which he has given the name Dinornis), in the sandstone of Connecticut. With respect to the country from which these bones have been received, it appears to abound with ferns, whose roots are rich in farinaceous substance, well calculated for the support of the kind of bird to which they are ascribed. When it is remembered that the only animal found in New Zealand, at the time of its discovery by the Europeans, was a small species of rat, it seems extremely probable that this vast bird, having inhabited these islands, as it inhabited other remote countries, before they were occupied by man, was destroyed by the first settlers, who then, as may be conjectured, having acquired a taste for animal food, and finding no other, took to eating one another. Mr Owen illustrated his discourse by a conjectural diagram of the figure of the Dinornis. Its height (which he supposes fourteen or fifteen feet from head to foot) was contrasted with that of the birds most nearly resembling it—the cassowary and the ostrich.—*Athenæum*, No. 850, p. 138.

BOTANY.

6. *Flowers and Fruits of Australia.*—Many fruits grow and flourish in these colonies which can be reared in England only when they are housed, when means are taken to temper the keenness of the winter's blast, and when the temperature of the air is increased by artificial contrivances. It is a matter of doubt, however, whether anything is gained by the inhabitants of New Holland in this particular; for many fruits which are admirably adapted to the temperature and moist climate of Great Britain, either do not come to perfection, or will not grow at all, in the dry hot atmosphere of New Holland. A decision on the relative advantages and disadvantages will depend, in this instance, on the tastes of the individual; and, in arriving at a conclusion on this point, the native of Great Britain must not forget to bear in mind, that every one is apt to attach somewhat more than its intrinsic value to that which is beyond his reach. For example, the Englishman will be in danger of forming a highly favourable opinion of the capabilities of that country for the growth of fruit, where the orange and the grape flourish and yield abundantly in the open air; but it will do him no harm to remember, that if the Australian colonist gain the orange and the grape, they lose the apple, the currant, the gooseberry, and that most delicious of all fruits, the strawberry. As it is with fruits, so it is with flowers. The native flowers are many of them exceed-

ingly beautiful, and the geranium is almost a weed; but still very many of the sweetest and most beautiful English flowers will not grow in the climate of New Holland. The native flowers are, with very few exceptions, perfectly inodorous, and they gladden the eye with their grateful presence but for a short period. The dreary wastes of New Holland are relieved by the varied tints of the native flowers in the spring time only. But few persons, I apprehend, would estimate the beautiful, but scentless, native flowers of New Holland, beyond the more quiet-tinted, but sweet-smelling flowers of Great Britain. Even were they on a par in point of beauty and fragrance, the English flowers continue blooming a great part of the year, whilst the dull monotony of the arid shrubs of Australia is relieved only for a short time by beautifully-formed and exquisitely-tinted, but inodorous flowers. With all the charm of form, the Australian flowers must yield to the delicious fragrance and simple colouring of the flowers of the charming hedgerows of "merry England."—*Bartlett's New Holland.*

NEW PUBLICATIONS RECEIVED.

1. Geological Observations on the Volcanic Islands visited during the voyage of H.M.S. Beagle; by Charles Darwin, Esq., M.A., F.R.S.L. 8vo, pp. 175. Small, Elder, and Co., London. 1844. *The Geologist will find in these pages much to instruct and amuse him.*

2. The Year-Book of Facts in Science and Art; exhibiting the most important discoveries and improvements of the past year (1843). 12mo, pp. 288. David Bogue (late Tilt and Bogue), Fleet Street, London. 1844.

3. A Manual of Electricity, Magnetism, and Meteorology; by Dionysius Lardner, F.R.S., &c. &c. Vol. ii., pp. 550. Longman and Co., London: 1844. *The best English work on Electrical Meteorology.*

4. An Inquiry into the nature of the Simple Bodies of Chemistry; by David Low, F.R.S.E., Professor of Agriculture in the University of Edinburgh: 8vo, pp. 160. Longman and Co. 1844. *This learned and interesting work, from the pen of the celebrated Professor of Agriculture in our (Edinburgh) University, is well deserving the attention of chemists.*

5. Lessons on Chemistry; by William H. Balmain. Duodecimo, pp. 208. Longman and Co. 1844. *We particularly recommend this judicious treatise to students of chemistry attending university lectures.*

6. Anatomical Manipulation; or the methods of pursuing practical Investigations in Comparative Anatomy and Physiology. Also an Introduction to the Use of the Microscope, &c.; and an Appendix; by Alfred Tulk, M.R.C.S., M.E.S.; and Arthur Henfrey, A.L.S.M., Mic. S. With Illustrative Diagrams. 12mo, pp. 413. John Van Voorst, Lon-

don. 1844. *Comparative anatomists and naturalists will find this volume indispensably necessary in their investigations. No museum of comparative anatomy should be without it.*

7. A History of British Fossil Mammalia and Birds. By Richard Owen, F.R.S., F.G.S., &c. &c., Hunterian Professor and Conservator of the Museum of the Royal College of Surgeons, London. With numerous Illustrative Engravings. 8vo., No. 1. J. Von Voorst, London. 1844. *The high name of Professor Owen is an ample guarantee for the character of this work.*

8. Geology: Introductory, Descriptive, and Practical. By David Thomas Ansted, M.A. F.R.S., F.G.S.; Fellow of Jesus' College, Cambridge; Professor of Geology in King's College, London. 8vo., pp. 128, No. 1. John Van Voorst, London. *We can venture no judgment on a work as yet but in embryo—one number only having appeared.*

9. Prize Essay on the Nature and Objects of Medical Science, and the principles upon which its Study and Practice ought to be founded. By P. H. Williams, M.D., Edinburgh. 8vo., pp. 52. Churchill, London; and Maclachlan, Stewart, & Co., Edinburgh. 1844.

10. Bibliotheque Universelle de Geneve. Nos. 95 and 96.

11. The Encyclopædia of Chemistry, Theoretical and Practical. By James C. Booth, and Martin H. Boys. With numerous Engravings. Publishing in Octavo Parts, and printing in double columns. Philadelphia. Caryd and Hart. 1844.

12. An Address to the Members of the Berwickshire Naturalists' Club, delivered at the Anniversary Meeting held at Ford, September 20. 1843. By George Johnston, M.D., President. 8vo. Pp. 80. With plates.

13. Proceedings of the Academy of Natural Science of Philadelphia.

14. Ueber den Glasigen Feldspath im Basalte des Hohenhagens Zwischen Göttingen and Münden. Von J. Fr. Hausmann.

15. Geologischen Bemerkungen ueber die Gegend von Baden bei Rastadt. Von Hofrath Hausmann.

16. Glossology; or the Additional Means of Diagnosis of Disease to be derived from Indications and Appearances of the Tongue. By Benjamin Ridge, M.D., M.R.C.S.L. 8vo. Pp. 84. With Plates. John Churchill, Prince's Street, Soho, London. 1844.

17. Annalen der Physik und Chemie. Von J. C. Poggendorff. Up to Numbers 10 and 11. 1843.

18. Sur le Phenomene Erratique du Nord de l'Europe, et sur les mouvements recents du Sol Scandinave. Par M. A. Daubree, Professeur à la Faculte des Sciences de Strasbourg. 8vo. With a map of Norway. 1844.

19. The American Journal of Science and Arts, by Professors Silliman. No. 1. January 1844.

20. Catalogue of the Museum attached to the Class of Military Surgery in the University of Edinburgh. 8vo. 1844.

21. L'Institut, Journal Universel des Sciences et des Societés Savantes en France et à L'Etranger. Up to No. 530. 22 Fevrier 1844.

22. The Journal of Agriculture and the Transactions of the Highland and Agricultural Society of Scotland. For March 1844.

23. On the Killas group of Cornwall and South Devon;—its relations to the Subordinate formations of Central and North Devon and West

Somerset;—its natural Subdivisions;—and its true position to the Scale of British Strata. By the Rev. David Williams.

N.B.—The sections required for the proper understanding of this Memoir did not accompany it—it was, consequently, in an unfit state for republication.

List of Patents granted for Scotland from 18*th December* 1843 *to* 22*d March* 1844.

1. To Henry Austin, of 87 Hatton Garden, in the county of Middlesex, civil-engineer, "a new method of gluing or cementing certain materials for building and other purposes."—26th December 1843.

2. To Francis L'Estrange, of Dawson Street, in the city of Dublin, surgeon, "improvements in hernial trusses, to prevent the descent of hernia through the internal as well as the external ring."—26th December 1843.

3. To Charlton James Wollaston, of Welling, in the county of Kent, gentleman, being a communication from abroad, "improvements in machinery for cutting marble and stone."—26th December 1843.

4. To George Gwynne, of Regent Street, in the county of Middlesex, and George Fergusson Wilson, of Belmont, Vauxhall, in the county of Surrey, gentleman, "improvements in the manufacture of candles, and in apparatus for and processes of treating fatty and oily matters to obtain products for the manufacture of candles and other uses."—29th December 1843.

5. To Margaret Henrietta Marshall, of Manchester, in the county of Lancaster, "a certain improved plastic composition, applicable to the fine arts and to useful and ornamental purposes."—5th January 1844.

7. To James Champion, of Salford, in the county of Lancaster, and Thomas Marsden of Salford, in the county of Lancaster, machine-makers, "improvements in drawing, winding, and spinning cotton and other fibrous substances."—9th January 1844.

8. To James Overend, of Liverpool, in the county of Lancaster, gentleman, being a communication from abroad, "improvements in printing fabrics with metallic matters, and in finishing silks and other fabrics."—9th January 1844.

9. To Charles Townsend Christian, of St Martin's Place, St Martin's Lane, in the county of Middlesex, East India Army Agent, being a communication from abroad, "improvements in the construction of steam-engines."—12th January 1844.

10. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, being a communication from abroad, "improvements in machinery or apparatus for facilitating the tracing or copying of designs, drawings, and etchings of all kinds, either of the original size, or upon an enlarged or reduced scale."—15th January 1844.

11. To William Nichol, lithographer and printer in Edinburgh,

"improvements in lithographic and other printing presses."—18th January 1844.

12. To CHARLES TAYLEUR and JAMES FREDERICK DUPRE, of the Vulcan Foundry, Warrington, in the county of Lancaster, and HENRY DUBS, also of the Vulcan Foundry, engineer, "certain improvements in boilers."—18th January 1844.

13. To ALEXANDER BAIN, of No. 320 Oxford Street, in the county of Middlesex, merchant, "certain improvements in producing and regulating electric currents, and improvements in electric time-pieces, and in electric printing and signal telegraphs."—19th January 1844.

14. To WILLIAM EDWARD NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, being a communication from abroad, "certain improvements in machinery for preparing and combing wool, hair, and other fibrous substances."—19th January 1844.

15. To THOMAS ASPINWALL, of Bishopgate Churchyard, in the city of London, esquire, being a communication from abroad, "an improved cannon formed either of wrought iron or steel, or wrought iron and steel combined, and also instruments and machinery used in making, and a method of making said cannon."—25th January 1844.

16. To THOMAS SOUTHALL, of Kidderminster, in the county of Worcester, druggist, and CHARLES CRUDGINGTON, of the same place, banker, "improvements in the manufacture of iron and steel."—25th January 1844.

17. To ALEXANDER SPEARS of Glasgow, merchant, being a communication from abroad, "certain improvements on, or appertaining to, glass-bottles proper for wines and other liquids."—31st January 1844.

19. To WILLIAM EDWARD NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, being a communication from abroad, "a new or improved system of machinery or apparatus for obtaining and applying motive power for propelling on railways or water, and for raising heavy bodies, applicable also to various other purposes where power is required."—5th February 1844.

19. To PHILIP WALTHER, of Angel Court, Throgmorton Street, in the city of London, merchant, being a communication from abroad, "certain improvements in the construction of steam-engines."—5th February 1844.

20. To JOHN KIBBLE, of Glasgow, in the kingdom of Scotland, gentleman, "improvements in transmitting power in working machinery where endless belts, chains, or straps, are, or may be used."—12th February 1844.

21. To HUGH INGLIS, of Kilmarnock, in the county of Ayr, Scotland, mechanic, "improvements upon locomotive steam-engines, whereby a saving of fuel will be effected, which improvements are applicable to steam-vessels and other purposes, and to the increasing the adhesion of the wheels of railway engines, carriages, and tenders, upon the lines of rail, when the same are in a moist state."—13th February 1844.

22. To EZRA JENKS COATES, of Bread Street, Cheapside, in the city of London, merchant, being a communication from abroad, "improvements in the forging of bolts, spikes and nails."—15th February 1844.

23. To William Rowan, of the firm of John Rowan and Sons, of Doagh Foundry, in the county of Antrim, engineer, "certain improvements in axles."—26th February 1844.

24. To Gottlieb Boccius, of New Road, Shepherd's Bush, in the county of Middlesex, gentleman, "certain improved arrangements and apparatus for the production and diffusion of light."—5th March 1844.

25. To Thomas Murray Gladstone, of the Swan Garden Iron-works, Wolverhampton, in the county of Stafford, iron-master, "certain improvements in machinery for cutting or shearing iron or other metals."—5th March 1844.

26. To George Bennetts, of Gunnis Lake, in the county of Cornwall, civil-engineer, "certain improvements in steam-engines and boilers, and in generating steam."—8th March 1844.

27. To Edward Eyre, of Poole's Hotel, in the city of London, gentleman, being a communication from abroad, "certain improvements in railways, and in the machinery or apparatus to be employed thereon."—11th March 1841.

28. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, being a communication from abroad, "improvements in the preparation of caoutchouc or India-rubber, and in manufacturing various fabrics of which caoutchouc forms a component part."—12th March 1844.

29. To Joseph Crawhall of Newcastle-upon-Tyne, rope manufacturer, "improvements in machinery for manufacturing ropes and cordage."—20th March 1844.

30. To Richard Prosser, of Birmingham, in the county of Warwick, civil-engineer, and Job Cutler, of Lady Pool Lane, near Birmingham aforesaid, gentleman, "improvements in the machinery to be used in manufacturing pipes and bars, and in the application of such pipes and bars to various purposes."—21st March 1844.

31. To Isabella Larbalestier, of Noble Street, Falcon Square, in the city of London, furrier, "improvements in making certain skins resemble the sable fur."—22d March 1844.

32. To Henry Brown, of Selkirk, in the county of Selkirk, manufacturer, "improvements in carding silk, cotton, and other fibres."—22d March 1844.

33. To Moses Poole, of Lincoln's Inn, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in steam-engines, steam-boilers, and furnaces or fire-places."—22d March 1844.

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