THE

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PHILOSOPHICAL JOURNAL.

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PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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

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

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On Columnar Crystallization of Ground-Ice. By the Rev. WILLIAM SCORESBY, D.D., F.R.SS. Lond. and Edin., Member of the Institute of France, &c., &c. (With a Plate.) Communicated by the Author.

The configurations assumed by the crystallization of water, and condensed aqueous vapour, present, within the limits of the specific angle of crystallization, an endless variety both in structure and beauty. Opportunity for examining the singularly elegant forms exhibited in snow-crystals, was abundantly afforded me whilst engaged in Arctic enterprise and adventures,—so that I was enabled to sketch a large number of these configurations, out of which nearly a hundred varieties were selected for publication in the "Account of the Arctic Regions." Other crystalline forms, derived from the consolidation of dew, in the form of hoar-frost, have subsequently engaged my attention, and many delicately beautiful specimens have from time to time been figured. From the feathery ice, too, formed, by the deposition of moisture in rooms, on the inside of the windows, when microscopically examined, many examples of singular beauty of the dendritical class of figures have been obtained. But a curious species of aqueous crystallization, which I had not previously thought of examining, engaged my attention during a recent visit to Gateshaw, Roxburghshire, the residence of my friend and near connection, William Ker, Esq., which, from the variety and beauty of configuration, it may not be uninteresting to describe.

The species of crystals referred to occurs, generally, just *within* the surface of the ground, so as, except incidentally, to be hidden from observation. There they form a thin stratum, or sort of platform, of clusters, prisms, or needles, vertically arranged, which, springing out of the earth beneath, adventitiously push upwards on their summits a pellicle of the earth, or gravelly surface of the ground.

Instead of attempting to generalize the phenomena, however, which the limited nature of my observations would scarcely justify, it may be safer, for the sake of accuracy, rather to give the results of what I observed on the particular case referred to.

It was in the morning of the 6th of October, of the present year 1849, after a clear, frosty night,—leaving the ground thickly covered with hoar-frost, and the general surface hard frozen,—when my attention was directed to an unusual disturbance of some parts of the surface of a gravel-walk, on which I was in the habit of taking exercise. Ofttimes, indeed, I had noticed the loosening of the surface of gravel-walks, or gravel-covered roads, by the operation of a night's frost; but I had never observed the disintegration so complete, or the general surface so crisply spungy as on this occasion. The first portion examined was on a wide terrace-like walk, covered with fine gravel mixed with earth, which did not usually bind with much compactness. Being extremely pervious to water, and heavy rains having recently prevailed, the walk, though pleasant enough for exercise, contained a large quantity of moisture, and more particularly on the side next the grass descending into the lawn, which was little trod upon.

Here I observed the surface of the gravel to be raised an inch or two above the proper level, appearing rough, and the elements of which it was composed disjoined; in some places the appearance was cauliflower-like; in others more evenly rough, but separated by parallel curvilinear striæ into corresponding ridges. Occasionally, where the gravel had not been raised, there were little patches of exposed ice-crystals, from an inch to an inch-and-a-half high, all standing, closely associated but separate, in prisms of clustered needles, perpendicularly to the general surface of the ground. On examining into the cause of the elevation of surface, I found similar prismoidal or columnar ice-crystals beneath, bearing upon their points, or upper ends, a portion of earth or fine gravel. In many cases this earthy superficies was crisp, and, in small patches, adhesive ; though the adhesion was generally due rather to a crusty, frozen portion of earth, on which the base of the crystals rested.

The lower part of a sloping walk, into which much moisture had flowed from above, was, on examination, found to present the like interesting configurations of crystallization ; as also, a part of the lower side of the carriage-drive, from the entrance-gate to the house.

Numerous portions, from these three localities of the raised surface of the ground, were examined during the forenoon ; for which examination, the continuance of frost almost throughout the day, in the shade, afforded favourable opportunity. Assisted by one of the young ladies of the family, who dug up a large number of frozen bits with her penknife, and held them on the back of her hand whilst I examined them by magnifiers, I was enabled to sketch about thirty or more varieties.

Of these, seventeen in number are figured in the annexed plate (see Plate I.), generally of the natural size. It is not presumed, however, that all these, sketched in so much haste, whilst often falling down by the melting of the ice, are strictly accurate ; strict accuracy, as to any minute particular, being neither possible nor necessary. But every figure herein preserved is, as far as I am aware, *strictly characteristic* of the specimen from which it was drawn ; whilst all the peculiar forms, such as the honeycomb and curvilinear crystals, the pinnacles, arches, and other remarkable architectural-looking figures, exhibit, I believe, accurate outlines of the configurations which fell under my examination.

What may be considered as the general elementary form of the crystals examined, was a delicate and transparent needle, or prism of ice, about the thickness of moderate-sized pin-wire ; these were usually combined in the form of a clustered column, of the diameter, perhaps, of the sixth or the eighth of an inch, and varying ordinarily in height from half-an-inch to an inch-and-a-half. Generally, these columnar fasciculæ were, to appearance, *solid*, but some were obviously hollow; and one specimen more particularly (17), was quite of a hollow prismatic, or honeycomb, formation.

A not unfrequent variety in the form was that of a tapering or pointed needle, of which several examples are given in the plate. Another variety, often presenting singularly beautiful architectural forms, was that of a curvilinear prism, of which examples occur in figures 6, 7, 9, 10, 11, 12, 17, and 18. All these were usually *transparent*, and cast more into separate prisms than clustered. Fig. 11, however, was an exception, which was a clustered column, about the sixth of an inch thick, *opaque*, and almost opalescent in character and colour. Some, of the arch-like form, and series of arches, appeared to be perfectly symmetrical, especially the clustered arches in fig. 9, and the single staple-form of Fig. 12.

The dark rough portion of the figures at the base represents, rudely, the frozen platform of earthy or fine gravelly substance to which the crystals were attached, and out of which they seemed to have sprung. The corresponding appearance, at the summits of many of the columnar fasciculæ, represents minute portions of earth or gravel which had been thrust upward, and permanently sustained, by the growth of the icy crystal.

Most usually the columnar fasciculæ were detached, of which one of the most ordinary examples is represented in fig. 1. Other instances, in which the columns were more widely separated, are shewn in the figures 8, 13, 15, &c. Of crystals acuminated to a point, examples appear in Nos. 5, 14, 16, &c. Fig. 22 is a rude and miniature representation of a remarkable portion of the surface of the carriage-drive, in which various parallel and curvilinear ridges occurred, the dark grooved lines representing the unraised portions of the surface. The width of these ridges might be about four inches.

The examination of these manifold configurations was attended with singular interest, from the novelty, variety, and suprising beauty, of many of the incidental portions. The general columnar structure, combined out of clustered needles or spiculæ, and these standing perpendicularly on a base of frozen soil or gravel, and very commonly capped with portions of earthy matter or particles of gravel, necessarily presented the miniature forms of architectural remains. In many portions the resemblance was equal to that of miniature models of diversified series of columns, sometimes bearing a rough sort of architrave, sometimes standing separate or naked. Frequently the eye could penetrate betwixt a double row of numerous columns, resembling (except as to deviations from a perfect line in position) the vista of a cathedral nave or aisle.

Many portions of these crystalline formations afforded striking representations of the columnar remains of some Grecian temple, or relics of eastern architectural structures. In other species of formation were found many resemblances of ruins of edifices, ornamented with pinnacles or minarets.

Sometimes, by reason of the higher position of the base of a central portion of an aggregation of columns, a resemblance to a doorway was produced, exposing a deep recess within, as in figure 8; and sometimes, as will be seen in figures 9 and 12, a happy resemblance of a fine, round-arched doorway. Nor was tracery or other ornament altogether wanting; for some of the thin needles were crossed by narrow lines or marks singularly approaching the effect of architectural ornament.

Some cavernous arrangements of columns were met with, which reminded each of our party, who examined them, of basaltic formations, suggesting a likeness, in one instance, to Fingal's Cave.

An humbler resemblance of the general body of these crystallizations,—as, without particular examination, they appeared sustaining, in large extents of surface, rough masses of earth and gravel on their summits,—was that of the cauliflower. But, whenever examined in detail, this apparent characteristic was immediately resolved into the more beautiful forms of architectural models.

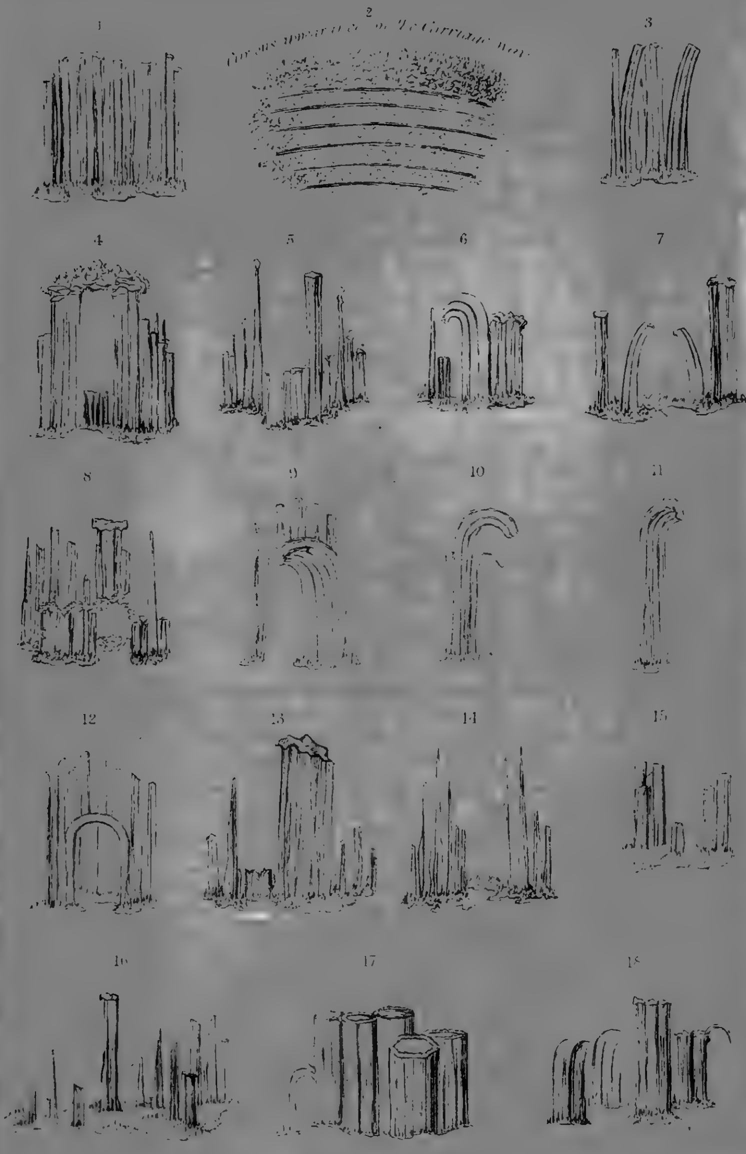
The continuance of the frost during the day gave, as I have intimated, every opportunity for examining these varied and beautiful formations; for, though the sun shone brightly, considerable portions of the walks in which these occurred were, sufficiently for our purpose, protected by the shade of the contiguous shrubs and trees.

Of the theory of these beautiful crystallizations, I have but little to submit. An idea which was intuitively suggested by observing the upraised surface of gravel and earth, and the interior stratum of columnar crystals, was of *growth or expansion upward.* It was hardly possible, indeed, to conceive of any other mode of progress. For in one region to which my observation was directed—the side of the terrace-walk—there was an almost continuous range of these formations, extending for, perhaps, 15 or 20 yards along the walk, and of a varying breadth of half-a-yard to a yard, in which the thin superficies of earth and gravel had been disrupted and elevated by means of the hidden stratum of diversified and beautiful crystallizations.

The surface of the ground, as it seemed, had, by draining and evaporation, been dried probably to the depth of two or three tenths of an inch; whilst the materials beneath were in the condition of a somewhat compressed sponge, having the pores pretty well, not entirely, filled with water. The frost of the night acting upon this wetted portion, had frozen it to no inconsiderable depth; and the expansion of the water on freezing was, apparently, a chief cause of the phenomena. But whilst this might account for the general fact of the disruption of the gravelly surface, it could hardly account for the formation and exudation of a vertically-arranged bed of crystals. Was it that the resistance above being the least, the crystallization assumed this direction, and was thrust progressively upward as the consolidation and expansion advanced below? Or was it that, whilst this least resistance might facilitate an upward growth of the columnar fasciculæ, that certain electric relations betwixt the earth and the air might dispose to this direction of enlargement and form of crystalline development?

To the impression that the latter principle had to do with the origination of the phenomena described, I much incline; for it is hardly to be doubted, but that, if after a long period of damp weather, the electricities of the air and earth should

COLUMNAR CRYSTALS OF GROUND-ICE.



tend, as I suppose, to become assimilated; so, during a continuous night of clear atmosphere and dry frosty air, the ordinary diversity of electricity, it may be imagined, would be not only restored but increased. The engagement of electricity in the phenomena of chemical operations, and in every change in the constitution of natural substances, or of combinations in physical elements, necessarily suggests a reference to this principle for an explanation of the crystallizations described; whilst the ascertained fact of the existence of a prevalent diversity of electrical condition betwixt the air and the earth, especially during the night, renders such reference at least plausible, whilst considering a reason for the *vertical* position and elevation of these interesting crystallizations arising out of the ground.

On the Tides and Dew-Point. By WILLIAM GALBRAITH, M.A., F.R.A.S., Teacher of Mathematics. Communicated by the Author.

I. *On the Tides.*

The phenomena of the tides have been long known; and it was frequently inferred, by repeated observations, that they had some relation to the motions of the sun and moon. It was not, however, till the time of Newton that any approach was made to their true theory.

The investigations of Euler, Bernoulli, and Maclaurin, contributed much to perfect Newton's views. Of late years renewed attention has been paid to their phenomena by Laplace, Lubbock, Whewell, Airy, and Daussy. Improved methods of observation and registration have been introduced, though these are still capable of greater perfection. Tide-gauges of a superior construction have been erected at several important points, but not so numerous as could be desired.

A very able marine surveyor has observed to me, that he could have wished it had come within my scope, in the new edition of Ainslie's Surveying, edited by me, to recommend the general adoption of self-registering tide-gauges, where I gave the requisite formulæ of reduction hitherto omitted in all works on the subject in this country.

These tide-gauges, he observes, can be manufactured for about £30, complete, including a clock, and along with it such a register of the tides by night as well as by day. He remarks that Hewitson of Newcastle possesses a beautiful instrument of this kind; and my friend, Mr John Adie, assures me he could furnish an excellent one at the same price, or even lower, though these not quite of a superior quality.

Our friends, Messrs Brysons, have already put up apparently a most complete one at Glasgow, giving a register of both tides and winds. From its position near the Broomielaw Bridge, I am afraid the register of the winds cannot be very accurate, by reason of the adjacent buildings; which is the fault of the proprietors, not that of the constructors. I have been informed that they could furnish a pretty good and complete tide-gauge for about £25, or £5 less than what has hitherto been charged in England.

I shall here, then, endeavour to supply the omission formerly alluded to, by urging the propriety of erecting self-registering tide-gauges, and wind-gauges at all our sea-ports and lighthouses hitherto unfurnished with them, wherever there is free access to the flowing and ebbing of the tide, and the unbiassed direction of the wind.*

It requires great caution and perseverance to record properly the rise and fall of the tide, as well as the precise time of high and low water, without these properly constructed gauges, though this may be sometimes indispensable in certain localities, or where they cannot be conveniently placed. If a calm bay be selected, to which there is free access of the tide, these may be recorded with tolerable accuracy in moderate weather. On rocky shores there are frequently *narrow openings* among the rocks, where a suitable position may be selected. The irregularities may be often obviated, or greatly modified, by laying a quantity of sea-weed or wreck across the entrance, through which the tide must percolate; and a little care and ingenuity will generally (except in storms) so regulate the flow as to enable an observer to estimate both the time and the height with considerable accuracy.

According to the theory of Newton and Laplace, the sun and moon, by their attraction, exercised upon the waters of the ocean, produce the tides which we observe. The tide may therefore either be produced by the sum of their attractions or by their difference, according to their relative positions in reference to one another and to the earth. The compound tide is very great towards the *syzygies*, that is, about new and full moon, for then it is the *sum* of the partial tides caused by the sun and moon respectively, and are commonly called *spring tides*, while those at the quarters are called *neap tides*, and are the smallest, because they are caused by the *excess* of the lunar tide above the solar.

The spring tides are not all equally great, because the partial tides which concur to produce them vary with the declination of the

* Without barometers and wind-gauges no accurate conclusions, in particular cases, can be deduced from tide-gauges.

sun and moon, and the distance of these bodies from the earth. They are, in fact, proportionally more considerable when the sun and moon are nearer the earth, and in the plane of its equator. It may be remarked that, in general, on our coasts, the highest tides follow the times of new and full moon by about a day and a half or thirty-six hours, except perhaps in some peculiar localities in rivers and deep bays or salt water lochs, intersected by shallows and narrows, in which there are several tides at the same time.

The time of high water at *new* and *full* moon is therefore a very important element in the computation of the time of high water on a given day, and is generally called the *establishment* of the port when the moon is at her mean distance, or when her horizontal parallax is 57′ nearly.

On observing attentively the height of the tides, which happen thirty-six hours after the syzygies at the equinoxes in any port, when the sun and moon are at their mean distances from the earth, we find, from a mean of all the heights, a certain quantity u, the unit of height, which varies with each port or place, and its application is shewn in page 398, &c., of my edition of Ainslie's Surveying, published by Messrs Blackwood in 1849.

I shall now proceed to shew the method of determining the value of u, the *unit* of height, and of the height of a given point above mean tide, though for want of the necessary apparatus of the best kind, and my limited time, at a period of the year when I could not have the sun and moon both on the equator, the final result will not be that required correctly. If the method indicated be followed nearly by others who are more favourably situated, considerable advances may be made to greater precision. The requisite allowance for the effect of the height of the barometer and thermometer has never, in this country, to my knowledge, been at all applied, though when accuracy is required it is indispensable. The merit of the discovery of the constancy of the *mean level* of the sea has been recently claimed by the writer of a book on Marine Surveying, for his father, who made it about the year 1830. Why, the investigations of Laplace depend upon this constancy; and it is stated *annually* in the *Connaissance des Tempes*, that the *unit* of height, u, at Brest, is known with great accuracy, because it was determined from a series of *sixteen* years' observations, from 1806 to 1823 ! ! !

To allow for the effect of the height of the barometer, it must be recollected that the specific gravity of mercury at 30 inches of the barometer, and 50° of Fahrenheit's thermometer, is 13·574, while that of sea-water varies, in different parts of the ocean, from about to 1·026 to 1·028, and 1·027 may be taken as the mean. Whence, $\frac{13 \cdot 574}{1 \cdot 027} = 13 \cdot 2$, nearly, the ratio of the specific gravity of mercury to that of sea-water, at the above pressure and temperature. Whence the formula for the effect of the pressure of the atmosphere, on the rise of the tide, assuming 30 inches for the mean standard height at the level of the sea, will be

$$-13{\cdot}2\ (b-30\text{ in.})\text{ in inches; or}$$
$$-\ 1{\cdot}1\ (b-30\text{ in.})\text{ in feet.} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Since it is the more convenient method to record the rise and fall of the tide in feet and decimals, the last form will be the more appropriate, though the former is that given in Ainslie's Surveying.

To shew the use of this formula, we shall give one or two examples.

1. Suppose the tide, when the barometer stands at 30 inches, to be 24 feet, what would it be if the barometer stood at 28 inches?

Here, $-1{\cdot}1\,(b-30)=-1{\cdot}1\times(28-30)=-1{\cdot}1\times-2=+2{\cdot}2$ feet.

Hence, $24+2{\cdot}2=26{\cdot}2$ feet, the real height.

2. Suppose the tide under 30 inches rises 24 feet as before, what would it rise under 31 inches?

Here, $-1{\cdot}1\ (b-30)=-1{\cdot}1\times1=-1{\cdot}1$ feet.

Hence, $24{\cdot}0-1{\cdot}1=22{\cdot}9$, the actual rise.

The difference of these two, or $26{\cdot}2-22{\cdot}9=3{\cdot}3$ feet. Hence the difference of the predicted heights given in our almanacs must be liable to an uncertainty in this case of 3·3 feet, independent of the effects of the wind; and this should always be allowed for when the exact height is of importance.

When the rise of the tide is observed under a given barometric pressure, and it is required to reduce it to the *standard*,* which we have assumed at 30 English inches, the correction from formula (1.) must be applied with a contrary sign, that is, it becomes

$$+1{\cdot}1\ (b-30\text{ in.}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

3. Suppose the barometer stood at 28 inches when the tide rose 26·2 feet, what would be the rise at the standard pressure of 30 inches?

From formula (2.) we have

$+1{\cdot}1\ (b-30\text{ in.})\ =\ +1{\cdot}1\times(28-30)\ =\ +1{\cdot}1\times-2=-2{\cdot}2$ feet.

Hence, $26{\cdot}2$ feet $-\,2{\cdot}2$ feet $=24{\cdot}0$ feet at 30 inches.

4. Suppose the barometer stood at 31 inches when the tide rose 22·9 feet, what would be the rise at the standard pressure of 30 inches?

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* In the valuable article on the Tides, in the Admiralty Manual, by Dr Whewell, Master of Trinity College, Cambridge, edited by Sir John F. W. Herschel, he says, p. 123, article 28, that "$\frac{1}{20}$ of an inch of mercury is equivalent to 1 inch of salt water," and the coefficients would, instead of 1·1, be 1·67, or about one-half too great, while he gives no standard to which they ought to be reduced.

From formula (2.) there will be found

$$+1{\cdot}1\,(b-30\text{ in.}) = +1{\cdot}1\,(31\text{ in.}-30\text{ in.}) = +1{\cdot}1\times 1 = +1{\cdot}1\text{ foot.}$$

Hence, 22·9 feet + 1·1 foot = 24 feet, at 30 inches.

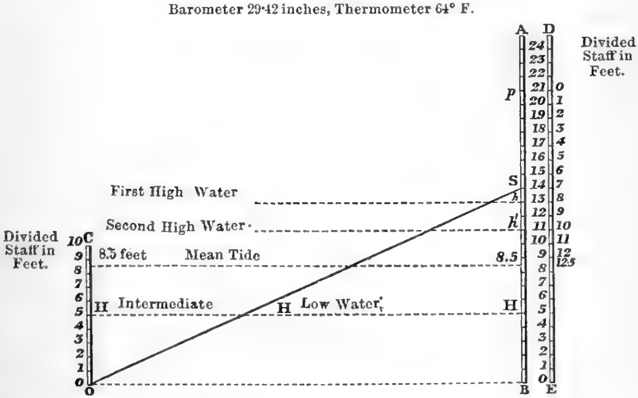
In this manner, all registers of tides should be uniformly reduced, otherwise considerable irregularities would be unaccounted for.

There is another irregularity to which the rise of the tides is subjected, and that is the force and direction of the wind, which, in some localities is very considerable, and is more difficult to be estimated correctly. But if *tide* and *wind* gauges were always combined with *barometers*, as they certainly ought to be, the effects of the barometer and force of the wind could be eliminated, and each effect accurately known and determined.

I shall now shew the method of determining the height of the mean tide, or the mean level of the sea, as if there were no tide, and of referring a given point to it, from which point a series of levels may be carried over a country, and referred to the mean level of the sea. (See Ainslie's Surveying, new edition, 1849, p. 397, &c., Broddick, Arran, 30th July 1849.

Method of Determining the Height of the Mean Tide, and of referring a given point to it.

Barometer 29·42 inches, Thermometer 64° F.



If the observations be made at a pier or quay-wall, and the height of the tide can be read at both high and low water; one divided gauge, as A B, will be sufficient. Also the height of the point p, may, by the usual spirit-level, be transferred to any rock or public building in the vicinity which may be convenient, and there permanently marked for future reference.

In the diagram, A B is the divided staff, like the common levelling staff, into feet, tenths, &c., fixed on the shore, near high water mark in spring tides; C O, another, near low water mark, or such other convenient position as may be thought necessary, with such others, intermediate if need be, and S H O, the sloping shore or beach. Then the first high water, marked h in the diagram and formulæ of reduction in Ainslie, rises on the staff A B to 13 feet, the next observation or intermediate low water, marked H, falls to 5 feet on the staff C O, and the next or second high water rises to 11 feet on the staff A B. From these it is required to find the mean height of the level of the sea, 8·5 feet on the staff A B, or 12·5 on D E, the height of the given point p, above the mean tide, the barometer being at 29·42 inches, and the barometer at 64° Fahrenheit. Likewise the height of the point p above mean tide, when the barometer stands at 30 inches, and Fahrenheit's thermometer at 50°.

Barometer observed, .	29·42 inches.	Thermometer 64°.
Reduction from 64° to 50°	−·04	
Barometer . . .	=29·38 inches at 50° Fahr.	

Whence by the formula, Ainslie, page 397,

$$\begin{array}{rl} h = 13 & \text{feet on the scale of the divided staff.} \\ h' = 11, & \text{and} \\ 2\,H = 10 & \\ \hline 4)34 & \end{array}$$

Height of mean tide $= 8{\cdot}5$ feet above zero of the scale A B.
Height of $p = 21{\cdot}0$ feet above zero.
Height of $p = 12{\cdot}5$ feet above mean tide on D E.

By formula (2.) $+1{\cdot}1\ (29{\cdot}38-30) = +1{\cdot}1 \times -0{\cdot}62 = -0{\cdot}682$ feet.

Hence $8{\cdot}500$ feet $- 0{\cdot}682$ feet $= 7{\cdot}818$ feet on AB, the mean height of the tide reduced for the barometer and thermometer, and 12·500 feet $+ 0{\cdot}682 = 13{\cdot}182$ feet, the true height of p above the reduced mean tide.

In this manner both the position of the mean level of the sea and the height of a given point above it, may be readily obtained, due allowance being made for the state of the barometer and thermometer.

It occasionally happens that an observer cannot command the assistance of the best apparatus to conduct the observations in the preceding manner, as was the case with myself at Broddick this year; yet I was anxious then, as well as on former occasions, to determine the position of the mean level of the sea as nearly correct as possible, in the circumstances in which I was placed. For this purpose I selected the upper surface of Broddick Quay as my point p, and on a kind of rude, inclined jetty, at right angles to the sea-wall, I chose a large stone exactly, by levelling, eight feet lower than the pier. I then determined, by a level, the height of the high water below the top of the pier, and the fall of the low water by a level and divided staff below the stone; whence, by adding 8 feet, I got the measure of the low water below the pier. By this means I could get the number of feet and decimals the high and low water was each below the pier in the usual manner, as before indicated by the divided rods, and the following table contains the results, though the mean *Greenwich* time cannot claim any very great precision.

Remark.—Without precise operations of this kind, carefully executed, no confidence can be placed in the relative positions of land and water, or whether there be any gradual change of the land and sea levels so much insisted upon by popular writers, giving themselves little trouble about the proper means of investigation.

Register of the observed Heights of the Tides, as previously explained, under the top of Broddick Quay in 1849.

Date.	Greenwich Time.	High water.	Low water.	Barometer.	Thermometer.	Weather.
	H. M.	Feet.	Feet.	Inches.	°	
Aug. 2,	11·0 A.M.	6·40		30·20	60 F.	Calm.
2,	5·5 P.M.		12·60	30·20	60 ...	Do.
2,	11·20 P.M.	5·67		30·20	60 ...	Do.
3,	5·25 A.M.		12·80	30·20	60 ...	Do.
3,	11·40 A.M.	6·41		30·20	60 ...	Do.
3,	5·50 P.M.		13·00	30·20	60 ...	Do.
3,	12·0 P.M.	5·45		30·20	60 ...	Do.
4,	6·20 A.M.		14·10	30·20	60 ...	Do.
4,	0·30 P.M.	5·83		30·20	60 ...	Do.
4,	6·45 P.M.		13·00	30·10	61 ...	Do.
5,	1·0 A.M.	4·30		30·10	62 ...	Do.
5,	7·10 P.M.		13·70	30·00	60 ...	Do.
6,	1·20 P.M.	4·70		30·00	60 ...	Do.
6,	7·35 P.M.		13·00	30·00	60 ...	Do.
7,	1·45 A.M.	4·50		30·00	60 ...	Do.
7,	7·55 A.M.		13·50	30·00	60 ...	Do.
7,	2·10 P.M.	5·20		30·00	60 ...	Do.
8,	2·35 P.M.	5·20		30·00	60 ...	Do.
9,	3·10 P.M.	5·25		30·00	60 ...	Do.
10,	9·45 A.M.		13·80	29·80	65 ...	Do.
10,	3·55 P.M.	5·33		29·80	64 ...	Do.
12,	5·35 P.M.	3·50		29·35	64 ...	Fresh Breeze.
13,	6·20 A.M.	4·10		29·25	62 ...	Do.
13,	0·45 P.M.		12·00	29·20	62 ...	Do.

Date.	Greenwich Time.	High water.	Low water.	Baro-meter.	Thermo-meter.	Weather.
	H. M.	Feet.	Feet.	Inches.	°	
Aug. 13,	6·55 P.M.	4·40		29·05	62 F.	Fresh Breeze.
14,	7·40 A.M.	5·40		29·50	62 ...	Do.
14,	2·0 P.M.		12·80	29·40	62 ...	Very fresh from W.
14,	8·15 P.M.	5·50		29·60	61 ...	Do.
15,	9·30 A.M.	5·50		29·70	60 ...	Do.
15,	3·40 P.M.		12·80	29·80	61 ...	Do.
16,	10·30 A.M.	5·40		29·60	60 ...	Do.
17,	11·45 A.M.	5·40		30·10	61 ...	Moderate.
17,	5·15 P.M.		13·60	30·10	60 ...	Do.
18,	11·50 A.M.	5·50		30·20	59 ...	Do.
18,	5·55 P.M.		13·80	30·20	60 ...	Do.
20,	7·10 A.M.		14·20	30·30	61 ...	Do.
20,	1·20 P.M.	5·40		30·30	62 ...	Do.
20,	7·30 P.M.		14·05	30·30	61 ...	Do.
21,	7·50 A.M.		14·40	30·00	64 ...	Do.
21,	2·10 P.M.	5·50		30·32	64 ...	Do.
21,	8·20 P.M.		14·00	30·30	65 ...	Do.

The method of reduction is as follows:—

	Feet.			Feet.
First high water, .	6·40	or,	First low water, .	12·60
Second high water, .	5·67		Second low water, .	12·80
Sum, ...	12·07		Sum, ...	25·40
Half Sum, . . .	6·03		Half Sum, . . .	12·70
Intermediate low water,	12·60		Intermediate high water,	5·67
Sum, ...	18·63		Sum, ...	18·37
Half sum, . . .	9·32		Half sum, . . .	9·18
Barom. cor. for 30·2 in.,	0·22		Barometric cor. .	0·22
Mean tide, . . .	9·54		Mean tide, . . .	9·40

	Inches.
Barometer, . . .	30·20
Standard, . . .	30·00
Difference, . .	+0·20

Hence by formula (2.), $+1·1 \times 0·2 = 0·22$ foot, the correction for the barometer, as stated above.

Thermometer about 60°.

It is hardly necessary to correct the barometer for this temperature, except in cases of great accuracy, when the observations are made with the best apparatus.

Whence the position of mean tide or level of the sea below the top of Broddick Pier, can be determined by two different methods, of which the results are given in the following table, from 2d August to 5th August inclusive.

The whole has not been reduced, because I had not an apparatus to record the times and heights of the tides at those hours which I have found it either inconvenient or dangerous, such as at 11^h, 12^h, at night, or 1^h, 2^h, 3^h, &c., in the morning.

No.	Date.	Time.	Barometer.	Ther. Fahr.	High water.	Low water.	Rise.	Bar. Cor.	First Rise.	Second Rise.	First Half Tide.	Second Half Tide.
	1849.	H. M.	In.	°	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
1	Aug. 2,	11·0 A.M.	30·20	60	6·40	...	...	...	...	...	...	...
2	2,	5·5 P.M.	30·20	60	...	12·60	6·57	+0·22	6·79	...	9·54	...
3	2,	11·20 P.M.	30·20	59	5·67	...	...	...	...	7·25	...	9·40
4	3,	5·25 A.M.	30·20	60	...	12·80	6·76	+0·22	6·98	...	9·64	...
5	3,	11·40 A.M.	30·20	60	6·41	...	...	...	...	6·71	...	9·88
6	3,	5·50 P.M.	30·20	60	...	13·00	7·07	+0·22	7·29	...	9·69	...
7	3,	12·0 P.M.	30·20	60	5·45	...	...	...	...	8·32	...	9·72
8	4,	6·20 A.M.	30·20	60	...	14·10	8·46	+0·22	8·68	...	10·09	...
9	4,	0·30 P.M.	30·20	60	5·83	...	...	...	...	7·94	...	9·91
10	4,	6·45 P.M.	30·10	61	...	13·00	...	...	...	...	...	...
											4)38·96	4)38·91
	Mean position of half tide under the Pier,										9·74	9·73

The rise of the tide each day is also given, of which that on the morning of the 4th is the greatest, being 8·68 feet near the time of full moon, though the highest is generally later than this.

The highest by far, that I observed, was that on 12th August 1849, the day on which, I believe, the Queen left Belfast Loch, and run for safety over to Loch Ryan.* By the table it will be seen that the barometer stood very low, 29·25 in., or, corrected for temperature, 29·20 inches, while the wind was blowing fresh, and increasing almost to a gale, nearly in the direction of the coming tide. That is, two circumstances combined to produce the very high tide, which rose to within 3·50 feet, or 3 feet 6 inches of the top of the pier. To allow for the effects of the low barometer we have

* I first observed the Royal squadron come in sight of Broddick at about 8 o'clock of the morning of 13th August 1849.

Barometrical observations,	29·25 inches.
Reduction from 64° to 50° Fahr.,	−·05
Barometer at 50° Fahr.	29·20
Standard barometer,	30·00
Deficit,	0·80

Hence the correction $= +1{\cdot}1 \times 0{\cdot}8 = 0{\cdot}88$ foot and $3{\cdot}50 + 0{\cdot}88 = 4{\cdot}38$ feet, the depression of the tide under the pier after allowing for the state of the barometer and thermometer. But, according to the best estimation I can deduce from a consideration of the circumstances of the case, the depression would have been about 5·40 or 5·50 feet. Hence $5{\cdot}45 - 4{\cdot}38 = 1{\cdot}07$ foot, the effect of the wind upon the tide. If, however, experiments were made with a register tide-gauge, combined with a wind-gauge, the exact quantity of rise caused by the state of the barometer and winds respectively, could be accurately determined. Till these machines are erected in all eligible places round our coasts, there will always be much uncertainty in the time and height of the tides.

What I have here said is, I fear, far from satisfactory to the public, who take an interest in these matters ; and I freely admit, from the inadequacy of my means, it is not much so to myself. It may, however, be the means of drawing attention to this most interesting subject, especially to a maritime country like this, whose interests are so much involved in trade and commerce.

It may also be inferred that the greatest rise of the tide by our observations was 8·68 feet on 4th August 1849, near full moon; and one-half of this, or 4·34 feet is the unit u of the height of the tide above the mean level of the sea, as understood by Laplace, &c., and explained in the new edition of Ainslie's Surveying, page 398, while that of the neaps is about 3·36 feet, and the establishment is near *noon*, Greenwich, *mean time*, or $11^h\ 34^m$, Broddick, *apparent* time.

II. On the Dew-Point.

Another interesting subject is the determination of the dewing point, that is, the temperature at which, in a given atmosphere, *dew* will be deposited from it on material objects. Daniell's Hygrometer is a very convenient, though rather an expensive, instrument for this purpose. It has therefore been an object with many to determine this with simpler and less expensive instruments. Accordingly, two thermometers have been repeatedly proposed for this purpose, one whose bulb is covered with wet cloth, and another dry and uncovered. After a good many experiments and attempts with various success, I have found the following formula to give results pretty conformable to the best hygrometric instruments. The thermometers used were made by my friend, Mr John Adie, optician.

New Formula.

Let f' represent the force of aqueous vapour at the dew-point.

f, the force of aqueous vapour, at the temperature of evaporation.

τ, the temperature by the dry thermometer.

t, that by the thermometer with the wet bulb.

$\tau - t = \delta t$, the difference of these two ;

And b, the height of the barometer, then

$$f' = f - b\,(0{\cdot}000356 + 0{\cdot}0000005\, t)\, \delta t \quad . \quad . \quad . \quad (3.)$$

Where the value of f' will give, by a table of the force of aqueous vapour at different temperatures, such as that in my Mathematical Tables, page 64, Table IV., or that of Dalton, the corresponding dew-point.

1*st*, $f' = 0{\cdot}4817 - 30{\cdot}2\,(0{\cdot}000356 + 0{\cdot}0000005 \times 58{\cdot}1^\circ)\, 5{\cdot}7^\circ$.

Or $f' = 0{\cdot}4817 - 0{\cdot}0663 = 0{\cdot}4154$, which, by the table, gives *d* the dew-point at 53°·8 Fah., and so on with the others as in the following table :—

Table of Results from the preceding Formula in August 1849.

No.	Date.	Time.	Dry Thermometer.	Wet Thermometer.	Barometer.	Dew-Point.
		H. M.			In.	
1	Aug. 6,	10·0 A.M.	63°·8	58°·1	30·20	53°·8
2	6,	6·50 P.M.	64·0	56·0	30·20	48·6
3	7,	11·40 A.M.	60·0	57·6	30·10	55·2
4	8,	0·45 P.M.	65·0	62·0	30·00	60·1
5	8,	4·45 P.M.	68·0	64·0	29·80	61·6
6	10,	11·0 A.M.	65·0	62·0	29·80	60·1
7	10,	7·0 P.M.	64·0	61·0	29·75	59·1
8	10,	9·0 P.M.	66·5	63·0	20·75	61·7
9	11,	Great rain from 5 A.M. till late.				
10	13,		59·0	57·4	29·50	56·3
11	17,		62·0	54·0	30·10	51·3
12	18,	5·30 P.M.	56·5	50·0	30·20	44·0
13	20,	8·0 A.M.	59·0	56·0	30·30	55·3
		Weather wet.				

In this way it is hoped I have shewn how a number of interesting results may be obtained by a little diligence and attention, with very moderate means. It is clear, from a comparison of the dew-point temperature with that of the atmosphere, as they approached to equality, rain certainly followed.

Note 1.—It has often struck me how much useful matter might be obtained by gentlemen possessing pleasure yachts, provided they had a little taste for, and knowledge of branches of useful science, instead of wasting their time in indolence, or at most, pleasant amusement.

Note 2.—*On the Height of Mont-Blanc.*—In the preceding paper I have shewn the method I pursued in determining the height of a given point above the mean level of the sea, whence my heights in Arran were derived trigonometrically, and a like plan must be followed in all similar cases claiming the requisite accuracy. In this way, too, a series of levels was carried from the Atlantic Ocean, through France towards Switzerland, from which the height of Mont Blanc was determined, as shewn in the *Description Geometrique de la France*, by the late M. Puissant.

From data contained in these works, I gave a computation in the new edition of Ainslie's Surveying, in which I regret there are one or two typographical errors, but not in the final results, which may be considered correct. Since then I have revised the whole, and find the mean from Plana's observations to be 15817·86* feet above the mean level of the sea; Colonel Coraboeuf's give 15788·05 feet, forming a mean of 15802·96 feet, nearly what I formerly gave, with a difference of 29·81 feet, and the half of which, about 15 feet, may be reckoned the probable error in the final result, as in Johnston's Physical Atlas.

I may remark, however, that Mrs Somerville, in her Physical Geography, vol. i., p. 67, edition of 1849, gives 15759·8 feet. Again, in her Appendix, vol. ii., p. 418, she gives 15739 feet, on the authority of the Piedmontese Surveys, published in 1845, and Eichman's Swiss Surveys in 1846.

This shews the difficulty of determining accurately the heights of mountains of great altitude.

Professor Forbes gives, in his book of Travels through the Alps, 15744 from the French engineers.

The mean of all these will give about 15760 feet for the culminating point of central Europe, though I prefer 15803, the mean of Plana and Coraboeuf's results.

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* This differs from Plana's own results, because the heights of Mont Colombier and Mont Granier had not been then definitely fixed, and were 8·81 metres, or 29 feet, too small, as has been latterly found by the French engineers.

Description of New Optical Instruments, viz.: 1. Polarizing Spectacles. 2. Picture Polariscope. 3. Polarizing Diaphragm for the Microscope. 4. Surgical Polariscope. By ALEXANDER BRYSON, Esq. Communicated by the Author.

1. *Polarizing Spectacles.*

By the aid of this instrument, the naturalist, engineer, or salmon-fisher, is enabled to distinguish objects beneath the surface of the water. It consists of a pair of Nicol's prisms, so adapted as to prevent the transmission to the eye of the horizontally polarized ray reflected from the surface of the water, and thus to destroy the glare which prevents the light from penetrating below the surface. When the surface of the water is smooth, and the instrument so arranged as to form an angle with the water of 52°, the effect is complete; at other angles, half of the incident ray passes through the prisms. The prisms are fitted into common spectacle frames, or used in the hand like a double opera-glass. When used by the engineer, for examining the bottoms of rivers or canals, where the water is sufficiently clear to admit the reflected rays from the bottom to permeate, and where the depth is known, the terminal planes of the prisms can be modified so as to destroy the effect of the refraction of the water, and exhibit the objects at the bottom in their true places.

The prisms used are those last invented by Mr William Nicol, as they give a larger field of view than those formerly described in this Journal. The angles being 71° and 93°.

2. *Picture Polariscope.**

This instrument is the same in construction as the preceding, except that the prisms are placed so as to prevent the admission of a perpendicular instead of a horizontal polarized

* Since this instrument was constructed, I find I have been anticipated in this application of Nicol's prism by Professor Dove of Berlin.

ray. By means of an endless screw, by which the prisms may be rotated equally on their axis, through an angle of 90°, the polarizing spectacles may be used as a picture polariscope. By the aid of this instrument, a picture hung in a bad light, or too highly varnished, appears *flat*, and can be perfectly seen.

3. *Polarizing Diaphragm for the Microscope.*

This instrument is attached to the microscope in the same manner as Messrs Smith and Beck, of London, adapt their polarizer to the stage. It consists of two Nicol's prisms, one of which is fixed, the other being allowed rotation through an angle of 90°, on the same axis as the fixed prism. By this arrangement, the light can be modified from its greatest brilliancy to total darkness.

4. *Surgical Polariscope.*

This instrument is intended to aid the oculist when examining the cornea of the eye; it is a Nicol's prism placed in a tube behind a lens of long focus, which rotates freely on its own axis, to suit the varying plane of the polarized ray from the cornea. It thus enables an oculist to observe any minute foreign body on the cornea with ease, as the glare from the surface is entirely removed.

Notice of a Shooting-Star; and on a Method of Cooling the Atmosphere of Rooms in a Tropical Climate. By Professor C. Piazzi Smyth, F.R.S.E., &c.

1. *Notice of a Shooting-Star.*

The object of this notice was merely to call attention to the importance of observing the phenomena of shooting-stars more carefully and rigidly, and of applying to them, more correctly than has generally been the case hitherto, the measurement of time and of space, and to exemplify what may be done in this way by the calculation of a recent instance. This instance, the rare one of an *ascending* shooting-star, was furnished by Captain W. S. Jacob, Bombay Engineers; and he having given the place where the body first appeared, that where it disappeared, and the time, the author of the paper, who had great faith in his friend's exactitude, considered the opportunity favourable for trying what results would be given by the application of Sir J. Lubbock's theory.

Some dissatisfaction has been felt about theories of shooting-stars, inasmuch as no one of them will explain *all* the observed phenomena. But though this is undoubtedly a necessary characteristic of a true theory, still great allowances are necessary here, where so many different classes of cosmical and atmospherical objects may be confounded even by practised observers; and where the greater number of observers are utterly unpractised, and their senses wholly uneducated for scientific observation. Allowing that some electrical and magnetical effects have been mistaken for shooting-stars, but excluding the baseless electrical, chemical, and lunar hypotheses, a great proportion are undoubtedly of a cosmical nature, and belong properly to astronomy; and these may be divided into two classes of small bodies. 1*st*, Those which are circulating round the sun as a primary; and, 2*dly*, Those which are revolving round the earth as such. The first we may occasionally see when passing near them in their orbits, but are not likely to come within sight of the same again, unless, indeed, they approach so near the earth as to gravitate towards it instead of the sun, and so become satellites or shooting-stars of the second class.

Sir J. Lubbock's theory is, that the shooting-stars shine by reflected light, and are extinguished by entering the earth's shadow; and he has given formulæ on this supposition for computing the distance of the body from the spectator, by noting the place in the sky where, and the time when, the extinction occurs.

These formulæ have been rendered more convenient for computation by Mr Archibald Smith, *Phil. Mag.*, March 1849, and, computed according to them, Captain Jacob's observation gives, for the distance of the body from the observer, 1721 miles; and that entry into the earth's shadow was the true cause of the disappearance, is borne out by the fact that the direction of motion was *towards* the axis of the earth's shadow. And, on account of the extremely small distance of the body, its change of place during flight would sufficiently account for its gradually appearing in the lower part of the sky when coming out of conjunction, increasing in brilliancy during its flight (reaching, at its maximum, the brightness of Venus), and then slowly vanishing as it entered first the penumbra and then the umbra of the earth's shadow in a slanting direction; and lastly, the body can hardly fail of being a satellite, as its distance is so much less than that of a shooting-star, which M. Petit of Toulouse has pretty well identified as revolving about the earth in $3^h\ 20^m$, or at about 3000 miles from the surface.—(*Proceedings of the Royal Society of Edinburgh*, 1849.)

2. *Method of Cooling the Atmosphere of Rooms in a Tropical Climate.*

After stating the case distinctly, and dwelling emphatically on its importance, as shewn by individual instances in private life, and by the statistics of the world at large, the author proceeded to describe the various methods adopted at present in India, and shewed their incapacity to meet the end proposed, as they merely agitated the air already in a room, or perniciously overloaded it with moisture.

To take the most difficult case that could occur, he chose that of a country where the mean temperature of day and night, and summer and winter, is never below 80°, and where there could, consequently, be no coolness in springs or rivers, or in the night air; where also the atmosphere being saturated with moisture, no cold could be produced by evaporation; and under such circumstances proposed that a method should be found of lowering the temperature of the air in a room; doing, in fact, there, the reverse of what is effected in a cold room by lighting a fire.

The principle of the plan which he brought forward was dependent on the property of air to increase in temperature on compression, and to diminish on expansion; the air was to be compressed by a forcing-pump into a close vessel, then cooled or rather deprived merely of its acquired heat of compression, and then being allowed to escape into the room desired to be cooled, would issue at a temperature as much below that of the atmosphere as it had risen above on compression.

That this was a *vera causa* there was no doubt; the *sufficiency* and the *practicability* were the only matters of doubt. These the author attempted to solve, by shewing the quantity of increase of heat due to a certain amount of compression; and by devising the most convenient form of the necessary apparatus, and concluded that a one-horse power should supply a room with 30 cubic feet of air per minute, cooled 20° below the surrounding atmosphere. The various sources of mechanical power likely to be met with in warm countries, were then described; and particularly a new and simple, and at the same time, a remarkably compact and effective form of windmill; as the wind is everywhere so cheap and abundant, and in the tropics so certain a species of moving power. Methods also of ventilating the cooled room, *i. e.*, of keeping it constantly supplied with cooled fresh air, and removing the vitiated, were explained, as well as a natural principle for meeting the residual difficulty that might be expected to arise in some cases, viz., the too great moisture of the cooled air.—(*Proceedings of the Royal Society of Edinburgh*, 1849.)

Examination of Professor E. Forbes's Views on the Geographical Distribution of British Plants. By A. D'Archiac.*

Mr Edward Forbes, whose important researches on the distribution of animals in the depth of the sea we have already referred to,† has considered the English flora in a point of view which connects it with geology, and more particularly

* Histoire des progrès de la Géologie de 1834 à 1845. Paris, 1848. T. 2, p. 128.
† Vol. i., p. 397.

with the quaternary epoch. This important work*, which has greatly interested botanists by the originality of its views, likewise deserves our attention for a short time. In analysing it, we shall follow the order of the author, carefully distinguishing between the results of direct observation, which we are quite disposed to admit, and certain explanations which appear to us to give rise to serious objections.

Supposing that beings have been distributed from certain primitive centres, Mr Forbes is of opinion that the ordinary agents of transportation, such as land and marine currents, winds, animals, and, lastly, the influence of man, are not sufficient, in the majority of cases, to account for the resemblance of certain local floras at present very remote from each other; he therefore endeavours to shew that there were formerly communications between these different regions, occasioned by oscillations of the surface of the earth, which subsequently ceased. This notion, it may be remarked, is only the development of an idea advanced by Mr Hewat Watson.

The vegetables of the British Islands admit of being grouped into five distinct floras, four of which are concentrated in well-defined provinces, and the fifth, which alone occupies a large surface, is likewise further extended by mingling with the four others.

The *first* of these floras is the most restricted, and is confined to the mountainous districts of the west and south-west of Ireland. It is characterized by species not very prolific, and the nearest point of Europe, from which it seems to be derived, is the north of Spain. There appears to be no fauna or assemblage of animals corresponding to this flora.

The *second flora*, that of the south-east of Ireland and south-west of England, comprehends a certain number of species not found elsewhere in the British Islands; but it has a close connection with the Channel Islands and the neighbouring parts of France. Some land-shells appear to be distributed in a similar way.

In the south-east of England, where the chalk is particularly developed, the vegetables of the *third* flora exhibit a

* *Vide* Memoirs of the Geological Survey of Great Britain, vol. i., pp. 336 to 432.

great number of species common to this district and the opposite coasts of France. The characters of the entomological fauna bear a relation to this flora ; and such is likewise the case with the land-shells either confined to this district, or very rarely extending beyond it.

The plants of the Scottish mountains, which compose the *fourth flora*, are few in number to the south, in Northumberland and Wales, but they are all identical with those of the northern chains, such as the Scandinavian Alps, where we likewise find associated with them certain species not occurring in the British Islands. The Alpine forms diminish progressively from the north to the south, and the same distribution seems to exist in regard to the fauna of the mountainous region.

Lastly, the *fifth flora*, whether viewed alone, or associated with the rest, is identical with that of Central and Western Europe, or the German flora, and the accompanying fauna diminishes as we advance northwards and westwards.

It was not till after the deposition of the London clay, or lower tertiary formation, that the migrations of the plants and animals in question could have commenced, the temperature before that period having favoured the development of organized beings of a very different kind. These migrations must likewise have taken place before the appearance of man, for the peat deposits, composed of the remains of vast forests, which occupied a great part of the existing surface of the British Islands during the most remote historical times, contain fresh water marls with *Cervus megaceros, &c.*, which, in their turn, lie above the tertiary pleistocene deposits, forming the elevated bed of the sea at the time of the glacial period.*

During the quaternary (*post-pliocene*) period, the greater part of the flora and fauna of the British Islands migrated from the Continent on this elevated bed of the glacial sea. The animals, as well as vegetables of Germanic type, shew, by their distribution in the east of England, not less than by their rarity in proportion as we advance westward, and

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* Throughout his whole work, Mr E. Forbes regards the existence of the glacial period as a fact that has been demonstrated.

their absence from Ireland and Scotland, the reality of the point of departure assigned to them.

The fourth fauna migrated from the north during the *glacial era*, when Scotland, Wales, part of Ireland, and certain groups of islands, were surrounded with ice. The sea was then much more extended, and the present mountains were only islets, on whose sides plants of a subarctic character flourished. When the bottom of the sea was raised upwards, these islets became mountains, a new population of vegetables and animals occupied this newly-emerged surface, and the plants of the *glacial period* maintained themselves on the upper parts of the mountains.

In this statement, the skilful English zoologist has not taken into account one of the most certain facts relating to the quarternary period, or rather he seems to have taken the period of ice for that of the arctic fauna; but the phenomenon of striæ must have been produced at the time of the extensive supposititious ice, and it is anterior to the arctic flora and fauna. The lands were then more elevated than during the existence of arctic shells, or a subsidence brought *beneath* the water the striæ and polished surface which had been produced *above*. It is not easy to conceive how vegetables could propagate themselves to a distance, either when the whole country was under ice or snow, or when a great part of it was covered with water. In either case the circumstances must have been little favourable to such migrations. Mr Forbes's hypothesis is therefore in contradiction of the most probable deductions, namely, that the lands were more elevated during the formation of striæ than during the deposition of arctic shells, whose elevation results from a third phenomenon, posterior to the two others; and there is nothing to prove that, since then, the bottom of the sea has been more exposed than it is now.

As the south of Ireland and England, continues the author, were not submerged during the glacial period, the three other floras might come to these places before, during, or after this epoch. The third, which is the most extensive, occupies the chalky surface of Kent, a circumstance in other respects fortuitous, with regard to the nature of the ground, for it is not essential to the existence of the species. These vegetables came from the north-west of France, and the formation of the strait will indicate the period of their isolation. If, as is probable, the rupture of the strata took place before the destruction of *the great Germanic plain* which favoured the migration of the fifth flora, we may, says Mr Forbes (page 346) regard the flora of Kent as very ancient, perhaps even anterior to the second, that of Cornwall, Devonshire, the south-east of Ireland, the Channel Islands, and the west of France, which has a more southern character than the third.

We have already seen that geological and zoological data unite in placing the separation of England from the Continent at the epoch of the destruction of the fauna of the large mammifera, that is to say, at the end of the phenomenon which accumulated the *drift;* which ill agrees with the antiquity which Mr Forbes ascribes to this rupture, relatively to an emerged plain, whose existence nothing geological, hydrographical, or orographical tends, in any way, to confirm.

The geological characters of the districts occupied by the second flora are connected with the remains of a great destroyed barrier, which likewise marked the southern limit of the Icy Sea. But what is this great barrier evoked by the author? Is it the North-Down chain of hills of which we have already spoken? Besides, the northern limit of the second flora, represented by a rose-coloured tint on the chart (Plate VI. of Professor Forbes's Memoir), certainly does not coincide with any character, either physical or geological, in the soil of France and England. The shore of the *Icy Sea* is not in unison with this supposed limit; for, with the exception we have pointed out, the erratic phenomenon of the north is not seen to the south of a line drawn from the mouth of the Thames to Dusseldorf.* The comparative examination of the relief, of the disposition and relative thickness of the tertiary and

* The limit is, in fact, likewise indicated in Plate VII. of the Memoir. Notwithstanding this assertion, true in general, we have seen that M. Mantell mentions fragments of primary rocks in the second diluvial bed of the steep shores at Brighton.

more recent deposits, such as we have already referred to, and which we shall afterwards explain more fully, does not therefore appear to us to warrant Mr Forbes' suppositions.

The first flora, that of the west of Ireland, comprehends plants peculiar to the great peninsula of Spain and Portugal, and principally to the Asturias, or which are very widely distributed there; and, as its presence cannot be explained either by marine or aërial currents,—the first on account of their direction, the second on account of the kinds of seeds transported,—the ingenious naturalist contends, that, at a period more ancient than that of the preceding floras, there was a geological union, or a very close neighbourhood, between the west of Ireland and the north of Spain, and that the flora of the intermediate lands was an extension of that of the peninsula. Finally, the destruction of these lands was anterior to the glacial period.

After enumerating the characters of the miocene fauna (p. 348), by supposing the communication of the Mediterranean with the ocean between Montpellier and Bordeaux—of which we have not yet any proof, while there exist negative reasons in disproof,—Mr Forbes says, that it is not at this period that he places the junction of the Asturias and Ireland; but that having observed in Lycia medium tertiary deposits at an altitude of 1800 metres, he thinks that this great *miocene sea* may have been uniformly elevated in the centre of the Mediterranean and west of Europe; and this, according to all probability, must have been the period of the approximation of the Asturias and Ireland. Here, again, we regret that we can find nothing, either in the present orography of this part of Europe, nor in the stratigraphical characters of the deposits, nor yet in the forms which may be ascribed to the ancient tertiary basins, from the directions of the beds, which confirms the existence of this emerged surface. The possible prolongation of certain portions of land to the west, such as the points of Cornwall and Bretagne, gives no probability to so general an emersion as that supposed. The instance mentioned on the declivities of Taurus is purely local, and not applicable to the west of Europe, where the medium marine tertiary beds do not exceed 150 metres in height, and that from Norfolk, as far as the foot of the Pyrenees, as in Spain, Portugal, and the Azores. It would be necessary, besides, to admit of a subsequent sinking or depression, of which Mr Forbes makes no mention, nor of the period when it took place. With regard to the very ingenious argument drawn from the great bank of seaweed in the Atlantic, it rests on a knowledge of the fact itself far too incomplete to be of much value.

In a botanical point of view, it would, perhaps, be desirable to determine whether the external circumstances under which the five floras of Great Britain now live, such as latitude, altitude, temperature, winds, humidity, or dryness, exposure, nature of the soil, greater or less distance from the coast, &c., are altogether insufficient to explain their different characters. Now this important part of the question appears not to have been entered upon by the author. The geography of plants, as it has been founded by its illustrious author, and as it is studied by his followers, among others, M. Ch. Martins and M. Alph. De Candolle, is not an abstract speculation ; it is the consequence of a multitude of physical circumstances, the relative importance of which must be duly appreciated. We know, moreover, that plants have very different geographical limits : thus, there are some which we meet with over an extent of 25° in latitude, and much more in longitude, while others occupy only zones extremely restricted in both senses ; it would, therefore, be useful to study the five English floras in this point of view. The radiation of plants from a centre, is by no means satisfactorily proved, and it may be asked, for example, What is the original centre from which the species common to North America and Southern Europe could have radiated ? This idea appears to us to have been presented in a more philosophical manner by M. A. Richard, when he says, " Perhaps a more attentive examination may prove that these points of departure, the number of which, though considerable, is still limited, correspond to the different epochs of the elevation of different points of the surface of the ground."*

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* Nouveaux Éléments de Botanique, p. 523, 8vo. Paris, 1846.

Mr Forbes shews farther on (p. 350), that the specific identity, over a certain extent, of the flora and fauna of one country with those of another, depends on this, that the countries form, or have formed, a part of the same specific centre; or else on this, that they have derived their animals and vegetables by transmission, by means of migration over a continuous or very nearly approaching country—a migration favoured, in the case of Alpine floras, by transportation on floating ice. The identity of the Alpine flora of the centre of Europe with that of Central Asia is likewise attributed to the glacial epoch, and the phenomena which it occasioned; but for this we have no geological proofs more positive than for many of the preceding assertions; and there is nothing to shew that the sea of the glacial epoch extended to Central Asia. We know that erratic blocks and striæ have not yet been found, either in the Ural or Altai mountains, and still less are they to be expected to the south of these chains, and in the vast plains which separate them.

The argillaceous deposits, with blocks and beds of arctic shells, should be, according to the author (p. 352), contemporary with a flora which came from the north, a circumstance which justifies our former observation, for these deposits were formed after the extensive ice, when less land had emerged than now. He then inquires into the distribution of the molluscs now living on the coasts of the British Islands, and follows them into remote seas, where they have representatives. He shews that the radiated animals have a distribution analogous to that of the molluscs; and, with regard to this fauna, considered as a whole, he is induced to think, that it may have had some representatives from the cretaceous period, and in the inferior tertiary period; but it is not till the medium tertiary period, that the analogies become really remarkable.

In speaking of the tertiary formation, we mentioned Mr Forbes's memoir referring to it; and we shall here continue to speak only of what relates to the quaternary formation. The author has collected in it, in England, Scotland, and Ireland, 124 species of shells, which, with few exception, live in the neighbouring seas.* But this fauna is by no means rich in species and individuals when compared with that of the Crag which preceded it, or that of the present coasts which followed it. This difference must arise from the climatological conditions in which it lived, conditions not favourable in consequence of a lower temperature. The fauna of the quaternary molluscs of the British Islands has been placed numerically between the present fauna of Greenland and that of the coasts of Massachusetts, although nearer the former, and probably very nearly allied to that of the coast of Labrador. It is composed of species living in the seas of Great Britain, and original natives of the north; of others now confined to the more northern latitudes: some appear to be extinct, and one or two may have had a southern origin, or rather are known only in the Crag of the south of Ireland. The species most abundant, and most widely distributed throughout the *drift*, are essentially northern.

Reverting to the distribution of the molluscs on the coasts of England (p. 371), a subject which he had discussed in a previous memoir,† Mr Forbes distinguishes four zones or regions: the littoral zone; that of the laminariæ; that of the corallines; and that of the polypi of the deep seas. Among a multitude of interesting details, he shews that the first zone is comprised between the sea when at the highest and when at the lowest; the second between the low water and a depth of 27 metres; the third extends from 27 to 90 metres; and the fourth from 90 to 180 metres, and beyond it. With these data, he first determines the existence of the first zone in the quaternary deposits, and then proves that the latter are in no respect deposited under a depth of water which could reach the fourth. The greater number of the fossils must have lived in cold waters, not of great depth, and belonging to the three first zones. The arctic characters of this fauna are not, therefore, the result of it having lived at

* See likewise Agassiz, Coquilles fossiles d'Angleterre identiques avec des especes vivantes, Verh. d. Schweitz naturf Ges in Zwich. 1841, p. 63.

† Edinburgh Academic Annual for 1840.

a great depth, but under a colder climate than that now prevailing in the same latitude.

(P. 379.) One-third of the quaternary species still live both on the coasts of America and those of Europe. In the present day, sixty-six species of testaceous molluscs are common to the coasts of the United States, situate to the north of Cape Cod, and to those of Europe. None of these species has its northern European limit to the south of England, and ten only extend to the seas of the south of Europe. On the other hand, not less than forty-five of them inhabit the arctic seas. Of the sixty-six preceding species, fifty-one are again found in the quaternary deposits, and it follows from the table drawn up by the author, that the identity between the northern American fauna and that of Europe has been established, at least, during the quaternary period, not by pelagic species, but rather by littoral shells.

The elevation of the bottom of the sea (p. 385) would be gradual; and what M. Forchhammer has said of the quaternary formation of Denmark will be applicable to the British Islands. In the Isle of Man, the marls contain bivalve shells of the second and third region, and they are covered to a great thickness with sand and gravel, sometimes with rolled littoral shells. The largest blocks rest upon these sands. The elevation of the land, which appears to coincide with the end of the period of cold, has determined the contours of the present coasts, and the organization we now behold was then developed. Part of the ancient species has become extinct, another has retired to the arctic seas, and a small number have disappeared from the coasts of Europe, and continue to live on those of America. Many species remain in these new waters along with those which have come thither in great numbers, whether appearing for the first time in creation, or brought from warmer seas by the intervention of currents. Among the latter, we must include such as already existed on the spot at the period of the crag, of which upwards of fifty are enumerated, which the low temperature speedily expelled. Besides, cavities of greater depth allow certain species to continue to live at the same points, where they now form, in certain parts of the British seas, true oases of the arctic fauna, at from 150 to 180 metres in depth.

Again, considering (p. 386) the quaternary formation of the north (the *newer pliocene*), Mr Forbes, with reason, compares it with certain strata in Sicily; but he still insists upon a communication of the *Icy Sea* with the Mediterranean, and that to explain the presence of five or six species of shells in the Mediterranean deposits, a fact previously made known by M. J. Smith. No geological data, however, confirm this hypothesis, in which, moreover, we perceive, as in the foregoing, an enormous disproportion between the grandeur of the means invoked, and the smallness of the effect produced. Neither can we agree with the author, that the Norwich Crag, or that containing mammifera (p. 391), and the beds of Bridlington in Yorkshire (p. 393), are of the same age as the quaternary deposits, and still less that the fresh water deposits of Grays are in part contemporary with the red crag; for these are assertions completely opposed to all that has been written by geologists best acquainted with the east of England.

In the latter part of this Memoir there are many subjects of which we shall have occasion to speak, in treating of the tertiary series; and we shall only say here, without entering upon the discussion of the opinions of the geologists who have preceded him, and whom he does not mention, that Mr Forbes seems to confound the fauna of the crag (*miocene*) with the quaternary fauna (*newer pliocene*), thus completely cutting off the superior tertiary fauna (*pliocene*), which is represented in England by the Norwich Crag, as has been shewn by Mr Lyell. The fauna of the quaternary mammifera in England, as on the Continent, is perfectly distinct from that of the superior tertiary formation, and from that of the *moyen* tertiary formation. The beautiful works of Professor Owen, as well as those of the zoologists of France, Germany, and Italy, in this respect perfectly agree with the result of researches exclusively geological. The succession of phenomena, such as it appears to us to result from the numerous investigations made in all parts of the British Islands, does not warrant us to admit, with Mr

Forbes (p. 403), that the white crag represents of itself the *moyen* tertiary formation,—the red crag, the superior tertiary formation,—the deposits of the glacial period, the quaternary formation (*newer pliocene*), while the fresh water marls, and elevated regions would constitute two *post-tertiary* epochs.

We have thought it right to discuss some of the hypotheses advanced by this skilful English naturalist, because it appears to us necessary to shew the inconveniences arising from an attempt to give an account of facts hitherto inexplicable in one science (botanical geography), by drawing from another science (geology) suppositions, made, as it appears, with the sole view to these explanations, and for which there is no sufficient authority. We are far from thinking that these ingenious conjectures may not pave the way to some interesting discoveries, and that even many of them may be well-founded; but it is necessary to repeat, that proofs drawn from geology must rest on more certain data than those which, in this instance, have been adduced.

On the Action of Lime on Animal and Vegetable Substances.

By JOHN DAVY, M.D., F.R.S. Lond. & Edin., Inspector-General of Army Hospitals.

LESKETH HOW, AMBLESIDE, *Nov.* 14, 1849.

MY DEAR SIR,—If you think the following observations on the action of lime on animal and vegetable substances likely to be useful at the present time, you will oblige me by inserting them in the Philosophical Journal. They were first published in a collection of essays* ten years ago. Notwithstanding, judging from instructions recently given on the subject of interments, and from the remarks of more than one writer on agriculture, the results of them, with the practical conclusions to which they lead, seem to be little known, if at all.—I am, dear Sir, yours very truly,

J. DAVY.

To Professor JAMESON.

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* Researches Physiological and Anatomical, 2 vols. 8vo. London, 1839.

On the Action of Lime on the Textures of the Human Body.

It is commonly asserted and believed, that lime exercises a corroding, destructive influence on animal matter in general, and that animal bodies, exposed to its action, rapidly decompose and disappear. Accordingly, it has been almost invariably recommended to add this earth to graves, in instances in which a rapid decay is considered desirable, as on the occasion of the crowding of grave-pits with dead bodies, during the prevalency of pestilential diseases.* From the results of many experiments which I have made with lime on animal substances, I have been compelled to come to the conclusion that this opinion is not well founded in fact,—indeed, that it is altogether erroneous.

The experiments were commenced in Malta, in the summer of 1829, and they were carried on during the following year. The method observed was, to immerse the animal matter for trial in cream of lime, or rather a paste of lime contained in a wide-mouthed bottle, well corked and covered with cerate cloth, to exclude the ingress of atmospheric air, and to preserve the lime in its caustic state.

One of the first experiments tried was commenced on the 27th

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* The following instance, extracted from the ninth volume of the Philosophical Transactions, abridged, may be given as an example of the vague, and, as I believe, erroneous manner of considering the operation of lime. In 1746, when means for preventing the infection of an epidemic disease, which then prevailed among the cattle, were under consideration, burying them was thought the most effectual method, and the introduction of lime was recommended "for the more speedy destruction of the distempered carcasses." But doubts arising whether the lime might not exalt the putrid particles, and help to spread the infection, it was the opinion of several of the learned, that it was most safe on that account to bury them without it.

Dr Parsons, the author of the paper, adds, "that the question will probably be decided by a fact that had come to the knowledge of one of the Justices, John Milner, Esq., appointed to inspect into the affair, and will serve to prevent the practice of burying them with lime for the future, as it makes it more than probable that malignant particles by the operation of the lime may be sent up, and spread through the air." The fact referred to was the following:—"Mr Stallwood, a farmer at Hackney, informs the Justices to whom the case of the distempered cattle was committed, that he had buried thirteen cows very deep, with the quantity of lime appointed by the Justices; and observing his dogs to scratch and tear up the ground with their feet to get at the cow's flesh (the lime fermenting, and causing a foam, as he called it, or strong scent of meat to arise, which made the dogs so eager to come at it), he beat them off several times, but the dogs always returning as soon as he was gone, for some time he hired a boy to keep them off; but that he had buried several other cows in another place, with their hides cut and slashed, without any lime, and the dogs never attempted to scratch or tear up the ground there." Two bushels of lime were allowed to each cow. With lime the bodies were buried ten feet deep, without lime eight feet deep.

Relative to the explanation of the fact,—was not the difference observed in the two instances owing to this—that, in the one the dogs were attracted by the smell of the meat preserved by the lime, and not in the other, where it was not so preserved, and where it was undergoing putrefaction?

August. Portions of various textures were immersed, as mentioned above. They were taken from a subject in a state of incipient putrefaction, and they inhaled a fetid smell. On immersion in the lime and water, as might be expected, they gave off a strong ammoniacal odour. They were first examined on the 24th of September. They were then all in excellent preservation, swollen but not corroded, nor their delicate tissue injured. They were next examined seven months after, viz. on the 5th of May of the year following. The report was equally favourable : it is stated that they were much in the same state as before, the texture of each part distinct, and the part, as a whole, easily distinguishable. They were left undisturbed nearly two years, until the 6th April 1832, when, on examination, they were found to have undergone material change. The cuticle had become soft and transparent, as had also the dura mater, admitting of being torn with the greatest ease. The muscle appeared to be converted into adipocire, which was quite white,—had no unpleasant smell,—was friable when dried, and burned with a bright flame, without any unpleasant smell. The other parts were not distinguishable. Most of the lime was converted into carbonate of lime,—atmospheric air not having been entirely excluded.

The second experiment recorded was commenced in the beginning of October. Portions of aorta, dura mater, intestine, skin, cellular tissue, muscle and tendon, were similarly treated. The results were examined on the 5th May following. Then, on opening the bottle, an ammoniacal, but no putrid smell was perceptible. The parts were found well preserved, excepting the fatty matter contained in the cellular tissue, which had become of an opaque white, and friable, from combination with the alkaline earth, and conversion into soap. The tendon, it is mentioned, was somewhat distended, and rendered more transparent, but not gelatinized; and so, also, in a less degree, were the dura mater and cutis; and the last was deprived of its cuticle and hair.

Some other experiments were made, but, as the results were very similar, it would be tedious to describe them. I may state, generally, that with the exception of cuticle, nail, and perhaps hair, lime exerted, on the different textures on which it was tried, no destructive power, but a contrary influence,—and more particularly a well-marked antiseptic one. It has been stated how certain parts, in the first experiment, lost the putrid odour which they had acquired when immersed in lime and water. Moreover, it appears from notes of experiments, that after animal substances have been fully subjected to the action of lime, they ceased to be putrescent; they resisted putrefaction, whether placed in air or plunged and kept in common water. I shall mention one instance. On the 13th of May 1830, a portion of ileum, with mesentery attached, and a portion of muscular part of heart, with chordæ tendinæ, were placed in a large jar of transparent lime-water, and covered with cerate cloth. Examined nine months after, on the 16th February, they were found in good preservation, and without any putrid or unpleasant odour. The only change perceptible, was, that the portion of heart and intestine had acquired a light greenish hue, and the tendon an opalescent hue; and all were a little softened. A crust of carbonate of lime had formed on the water, which still retained some caustic lime. They were then transferred to a jar of common water, where, after four days, they continued unaltered. I may add, that a portion of cutis, similarly treated, placed in confined air in a bottle, after a whole month, emitted no unpleasant odour, and appeared to be unchanged.

I have observed that cuticle, nail, and perhaps hair, are to be excluded from the list of animal substances, not materially altered by the action of lime. On the cuticle its action is powerful, and, I apprehend, in consequence of a chemical combination between them being formed. It is well known how lime has the property of rendering the cuticle easily separable from the cutis vera, and how, in the art of tanning, it is applied to this purpose. The human cuticle, that, for instance, of the sole of the foot, I find becomes soft and gelatinous from immersion in lime and water. After drying, a portion thus tried (well washed previous to drying), was white, semitransparent and brittle; incinerated, it yielded 17 per cent. of ash, which consisted principally of lime and carbonate of lime.

The effect of lime on nail is similar to that which it exercises on cuticle, but not so strongly marked. A portion of nail of great toe, macerated in lime and water, from the 7th of June to the 18th August, was rendered soft and friable,—a little swollen, and disposed to separate or break up in layers. Dried, it exhibited the same character as cuticle, and when incinerated, burned in a similar manner, and left a considerable ash, consisting of a small proportion of phosphate of lime, which pre-existed in the nail, and a large proportion of lime, with which, during the change from maceration, it may be inferred it combined.

On hair, the effect of lime appears to be more destructive, but, in what manner it acts, I have not attempted to ascertain. A portion of human hair of the head, which had been kept in lime and water about three months, was partially decomposed. At the bottom of the vessel, there was a little black sediment. The hair, which was black, had acquired a just perceptible reddish shade, and had become much finer as if wasted, and more friable, so as to be easily broken.

Relative to the results of the experiments generally, they appear to me to bear me out in the remark with which I prefaced them, viz., that lime does not exercise a destructive corroding power on animal substances generally, nor one promoting their decomposition; but, on the contrary, a preservative, and decidedly antiseptic power, arresting putrefaction, even when commenced, and retarding decomposition. What new arrangements of the elements of animal matter may take place under the influence of lime is a subject for further inquiry. Probably the effects of lime on cuticle, nail, and hair, on which, in the arts, its operation has been best known, led to the ideas of its agency on animal substances generally, which I have been under the necessity of combating.

On the action of Lime on Vegetable Substances.

Reasoning from analogy, from what I had witnessed of the effects of lime on animal substances, I was induced to question the views which are commonly entertained of the operations of this earth on vegetable matters, as its supposed power of facilitating their decomposition, and promoting their fermentation and solution. And the few experiments which I have made, have more than confirmed me in my doubts.

As the subject is of very great importance in relation to agriculture, I shall describe the results which were obtained, using small quantities as most manageable, and best accordant with my limited means, hoping that my statements may induce others to repeat the experiments on a large scale, and extend them in a manner befitting the consequences involved.

The experiments were commenced in June 1836, and they were concluded in November 1838. I shall describe them individually.

On the 27th of June 1836, one portion of sawdust, I believe of Norwegian fir, was put into a bottle, with distilled water and quick lime (the bottle was about half filled with the mixture) and corked.

Another portion of sawdust was put into a bottle with water and corked, but without the addition of lime.

Examined on the 1st November 1838, the sawdust, with the lime, had no appearance of any material change; its colour, perhaps, was a little heightened; the water only just perceptibly coloured; it had a strong taste of lime; evaporated to dryness, it afforded a light yellow residue, consisting chiefly of lime; the proportion of vegetable matter was hardly appreciable.

The other portion examined after the same interval, also exhibited very little change; a mucilaginous film had formed over the submerged stratum of sawdust, too delicate and small in quantity to be collected and examined in a satisfactory manner; the sawdust retained its colour, and the water was colourless. The water, evaporated to dryness, yielded a very minute brownish residue, slightly bitter, which had no effect either on litmus or turmeric paper. No smell was perceived on opening the cork of either bottle.

On the 17th June 1836, some clover leaf and flower, and some leaf of the common mallow, were put into a bottle with quicklime and water; the bottle was corked, and the cork was covered with sealing-wax. The quicklime used was tested for carbonic acid, and found to be perfectly free from it,—not effervescing in the slightest degree in dissolving in an acid. For comparison, two other mixtures were at the same time made of the same vegetables and water and bottled, one of which was corked, being about half full of air, the other was not corked ; a little cotton wool merely was put into the mouth of the bottle to exclude dust and prevent evaporation.

Examined on the 1st November 1838 the mixture with the lime appeared to be but little altered ; the leaves and the flower retained their form ; the former were of a bright fresh green ; the latter had become brown ; the water was colourless ; the lime had acquired a light greenish hue ; it dissolved in dilute muriatic acid without giving off a particle of carbonic acid gas. The water evaporated yielded but a small residue, consisting chiefly of lime. The leaves and flower, though they retained their form, yet when shaken in the bottle, were broken into small pieces, and the leaflets detached. Farther, it may be remarked, that the smell perceived on drawing the cork was similar to that of bruised clover.

The other mixtures, to which lime had not been added, exhibit different results. That which was corked, a portion of air included, examined on the 4th November 1838, appeared much altered ; there was at bottom a light greenish sediment ; at top an almost black mass, and suspended in the water intermediately were the small flower-leaves, almost colourless ; the water was greenish, and when evaporated to dryness yielded a small brownish extract ; the leaves were in a pulpy state, and disorganized ; the smell from the mixture was offensive, not unlike that of clover fermenting.

The mixture from which air was not excluded, fermented soon after it was put by ; ten days after, namely, on the 27th June, it was noted that the vegetable matter was rapidly decomposing with a very offensive odour, approaching to that of the putrid ; that much gas had been disengaged, and a good deal of sediment had collected.

Examined on the 2d November 1838, it was found in a state very similar to that last described, bearing marks of advanced decomposition.

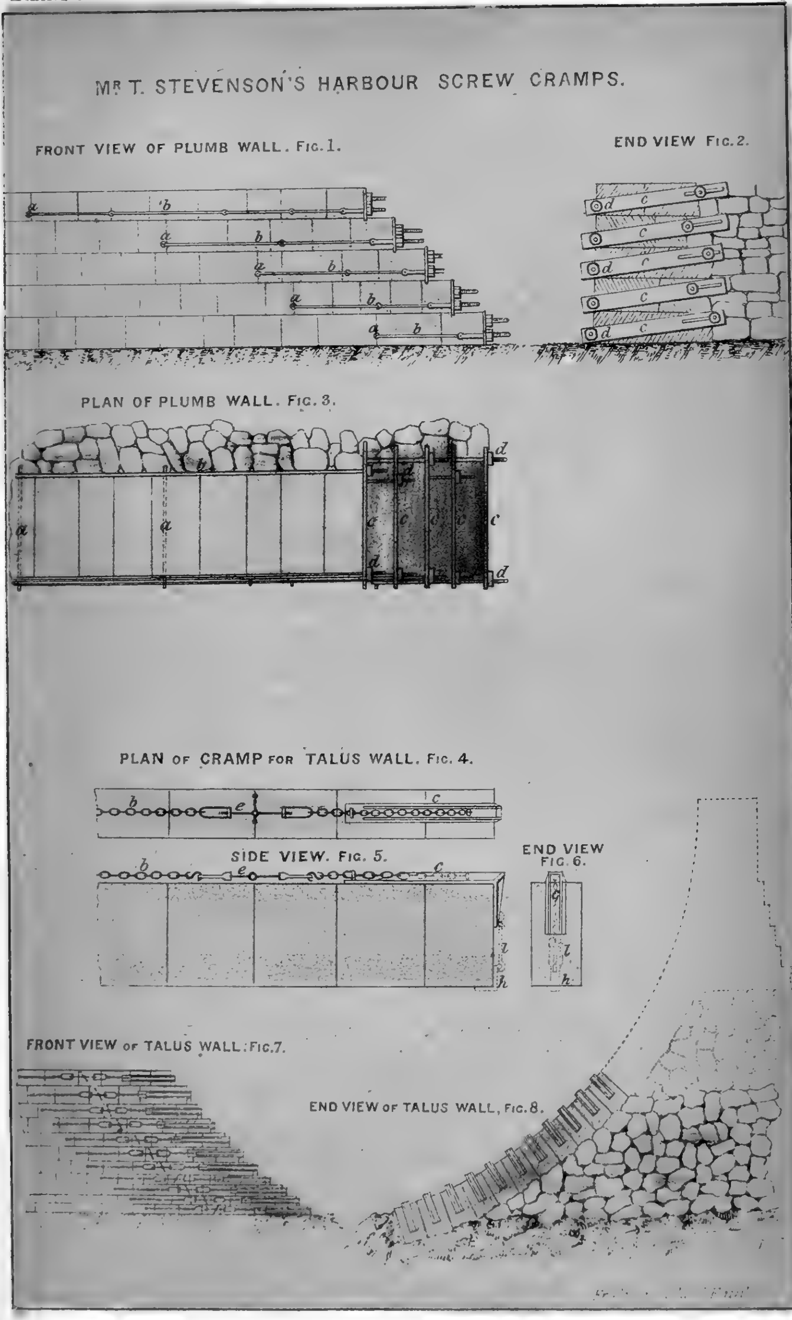
An experiment similar to that on the mallow leaf and clover was made with lime and water, on moss and lichen, and with a very similar result. It was commenced on the 23d June 1836, and terminated on the 1st November 1838. The moss and lichen retained their form, and bore being shaken in the bottle without falling to pieces. The water had acquired a light greenish hue ; the lime a brownish hue ; the water evaporated afforded a small brownish residue consisting chiefly of lime.

I shall mention one experiment more of the same duration, in which clover leaf and flower and mallow-leaf were put into a bottle covered with hydrate of lime, and the access of air excluded by a cork and sealing-wax. Examined on the 2d November 1838, the flower was found brown ; the clover-leaves of a very light green ; the mallow of a dark green, and all very friable, falling to pieces when touched. The lime was examined both before and after for carbonic acid, and it was quite free from it at the commencement of the experiment, as it was also at the conclusion; it dissolved in an acid without the slightest effervescence, and appeared to be quite unaltered.

These results appear to me conclusive that lime does not promote the decomposition of vegetable matter; and, as I have before mentioned, that, instead of promoting, it arrests its fermentation. The circumstance that no carbonic acid could be detected in the lime after having been in contact with vegetable matter—both with and without water—I apprehend may be considered as demonstrative on this point.

Whether lime has any solvent power on vegetable matter, apart from the supposed one of exciting fermentation, is a distinct question. From what I have witnessed in carrying on these experiments, I infer that it has, in a slight degree at least, in combination with water. The extract obtained by evaporating the lime-water which was in contact with the vegetable matter, was perhaps indicative of this, especially in the instance of the sawdust, as was also the softened state of the leaves and flowers, falling to pieces on being shaken; and confirmation, perhaps, is afforded in the results of two comparative experiments made on the same leaves and flower, with magnesia and water and a solution of carbonate of potash (the old subcarbonate). Both mixtures were made on the 23d June 1836, and they were both examined on the 4th November 1838. There was a marked contrast between them. The leaves and flower, with the magnesia water, retained their form unaltered, and their texture did not appear to be materially weakened; they bore being shaken in the bottle without falling to pieces, and the water was only just perceptibly coloured greenish, and the magnesia brownish, the leaves retaining their colour unimpaired. The leaves and flowers, on the contrary, in the alkaline solution, were reduced to small pieces, and seemed to be wasted and deprived very much of their colouring matter, and in a pultaceous state; the solution was of a dark olive green; evaporated, it yielded a residue abounding in colouring matter. Lime in its solvent power, is probably intermediate in degree between magnesia and the more active alkali, more active even in combination, with one proportion of carbonic acid, than the magnesia, or even lime in a caustic state.

The application of the preceding results to agriculture, in relation to manures, I must decline discussing; the subject is one of too much importance and magnitude and difficulty to be lightly entered on.


Mr. T. STEVENSON'S HARBOUR SCREW CRAMPS.
FRONT VIEW OF PLUMB WALL. FIG. 1.
END VIEW FIG. 2.
PLAN OF PLUMB WALL. FIG. 3.
PLAN OF CRAMP FOR TALUS WALL. FIG. 4.
SIDE VIEW. FIG. 5.
END VIEW FIG. 6.
FRONT VIEW OF TALUS WALL: FIG. 7.
END VIEW OF TALUS WALL, FIG. 8.

Description of Harbour Screw-Cramps, for temporary use in binding together the Stones in the Construction of Harbour or other Marine Works. By THOMAS STEVENSON, Esq., Civil Engineer, F.R.S.E., F.R.S.S.A. (With a Plate.) Communicated by the Royal Scottish Society of Arts.*

Of all engineering operations, the construction of marine works in exposed situations may be regarded as the most precarious and difficult. The great force to which such erections are subjected, has recently been put to the test of direct experiment, by means of an instrument designed by me for the purpose, which has been named the Marine Dynamometer. The observations which were made with this instrument I lately communicated to the Royal Society of Edinburgh, and as the facts which are there stated are intimately connected with the subject of the following observations, I beg leave to give a brief digest from that Society's Transactions.

"In the Atlantic Ocean, according to the observations made at the Skerryvore Rocks, the average result (of the force of the sea) for five of the summer months, during the years 1843 and 1844, is 611 lb. per square foot. The average results for six of the winter months, during the same years, is 2086 lb. per square foot, or *thrice* as great as in the summer months.

"The greatest result yet obtained at Skerryvore was during the heavy westerly gale of 29th March 1845, when a pressure of 6083 lb. per square foot was registered. The next highest is 5323 lb.

"In the German Ocean, according to the observations made at the Bell Rock, the greatest result yet obtained is 3013 lb. per square foot.

"It thus appears, that the greatest effect of the sea which has been observed by the instrument, is that of the Atlantic at Skerryvore, which is nearly equal to three tons per square foot."†

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* Read before the Royal Scottish Society of Arts on 11th December 1848.

† Trans. Roy. Soc. Edin., vol. xvi.

Though the force to which marine buildings may be in some cases exposed is therefore very great, it must nevertheless be borne in mind that the surface of such works which is simultaneously subjected to this force is very limited, and perhaps rarely embraces more than one or two courses of masonry. Every harbour pier of proper construction is of greatly more than sufficient strength, when viewed as a *whole*, to resist the shock of the assailing waves. The friction to be overcome in causing relative motion between even two or three courses, coupled with the resistance offered by the backing, is so great as, in most piers, to ensure their stability. Although when viewed as a whole, therefore, almost every pier is more than sufficient to resist being overturned *en masse;* and the friction and resistance of the backing, consequent on relative motion, is so great as to prevent disunion on the large scale; yet there is hardly any building whose constituent blocks are in themselves weighty enough to resist successfully the dislocating action of the sea. Accordingly, if we inquire into the history of the failure of harbour works, we shall generally find that those have occurred during their construction, and can be traced to the want of due protection while the works have been in progress. And thus it appears that blocks of stone which are insufficient of themselves, when first laid, to resist the sea, are rendered perfectly secure when kept down by the superincumbent weight of the courses which are afterwards built upon the top of them. The last stones of unfinished courses, also, though equally insecure while in such a state, are found, after the works are completed, and the courses "*closed in*" at both ends, to resist for ages the greatest assaults of winter storms.

Many instances may be adduced of harbours suffering great damage in the course of construction, which have, when finished, successfully withstood the efforts of after gales; and of destruction to a whole work resulting from neglect in speedily repairing some trifling breach in the sea-walls. The north harbour of Peterhead, for example, during its construction in 1819, received damage from a winter gale, which (as estimated at the time) would require an additional expenditure of about £3400; and this pier has, since it was finished, remained quite secure for the last thirty years. At the construction of Pulteneytown harbour it is recorded, that in 1827 damage was done to the extent of £5000 in the course of two tides, though it seems now quite secure, after a lapse of nearly twenty years. At Ardglass harbour, which, I understand, cost about £20,000, a breach of 20 feet in extent was, from want of funds neglected, and the whole of the structure which was above low water was either utterly demolished, or greatly shaken, and has never been repaired. In 1844, I was informed that some of the stones at Dysart pier required, from decay, to be replaced by others, and a hole about four feet square had been made for the purpose, when a gale unfortunately came on, and completely carried away the whole of the outer part of the pier, extending to about thirty yards, with the exception of a fragment of the quay-wall, where many longitudinal and vertical fenders or stretchers of wood had been attached. On visiting the ruins about a week after the accident, I was surprised to find that this part of the quay-wall, though wholly deprived of backing, should have so successfully resisted the action of the sea, owing to the support which it received from the fenders. On making inquiries at an eye-witness of the scene, I was told that the sea-wall and hearting were very speedily scattered before the destroying element, and also *that* part of the quay-wall which had not been protected, or so securely protected by fenders. On the other hand, wherever the wall was strongly bound by fenders, the sea encroached very slowly; and that part which was most exposed to injury from vessels rounding their course into the harbour, and which was, in consequence, more strongly and closely bound with horizontal and vertical fenders, resisted successfully the force of the waves.

From the foregoing statement of facts, which shew us the almost total dependence of one stone on another, and the havoc which may ensue from even a single stone near the bottom of the structure being left insecure, we may readily see the advantage which is to be derived from connecting them together by iron bars and wooden fenders, or wrapping them round with chains, &c.; and hence, in many lighthouses and harbour works, great expense is incurred in joggling the stones, or in connecting them together with oaken tree-nails. The great advantage, therefore, of any contrivance which could be easily, and, above all, quickly applied for this purpose, without involving the expense of jumping holes or cutting the stones, appears very evident. The adoption of such means might confidently be expected, to a great extent, to prevent those great losses to which almost all such works are more or less liable during construction, and of which I have given a few examples. The importance of the subject, more especially at this time when the English refuge harbours, involving the expenditure of so many hundreds of thousands of pounds of the public money, have been at length commenced in good earnest, must be my excuse for bringing an untried project before this Society.

The accompanying drawings and models (Plate II.) represent two simple implements, which, from their resemblance to others commonly used in carpentry, may be termed *Harbour Screw-Cramps.* They are intended to be employed for temporary use in the construction of harbour and other sea works in exposed situations. The principle on which these have been designed, is that of coupling stones together in such a manner that the outermost cannot be removed without dragging the adjoining stones along with it. That represented by the diagrams, marked figs. 1, 2, and 3, is a screw-cramp adapted for a vertical or nearly vertical sea-wall. It consists of a cross rod of iron *a*, inserted diagonally between two of the stones of the work, and on either end of this rod, iron rods or chains *b*, are slipped, one set being at the front of the wall, and the other at the back. To prevent the backing of the wall from pressing upon the chains or rods, a small void like a drain, three inches square, is to be formed at the back of the wall as the work proceeds. At the conclusion of each tide's work, the abutment-plate *c*, is placed diagonally across the last stone, and the chains or bars are tightened up by means of the capstan-headed screws *d*, when the whole is thus bound together, and rendered secure. When the work is again commenced, the abutment-plate *c*, is removed, and more stones are set, which are, in like manner, connected with the others at the end of the tide's work by lengthening the chains or rods. After a stretch of sufficient length has been completed, the bars or chains *b* being slackened, the cross rod *a* is removed, and the bars or chains, being now free, are withdrawn. A new cross rod having been previously inserted at some convenient joint, the chains are now attached, and drawn tight as before. In this way, the open end of the work is at each course strongly connected to the stones farther back, without any loss of iron, or expenses for boring or cutting the stone.

The *cramp*, represented in figs. 4, 5, 6, 7, and 8, is designed more particularly for a talus or sea-wall. In this case, one end of the chain is made fast to a ring bolt or lewis *a*, in any stone which is at a sufficient distance from the open end, while the kneed abutment or anchor-plate *c*, is placed upon the last or outermost stone of each course, when the chains *b* are tightened by means of the draw-screw *e*.* This apparatus, in the event of a gale coming on suddenly, could be applied in the course of a few minutes. Instead of the ring-bolt or lewis, a bar or pinch could, in such an emergency, be easily driven down between one of the joints of the talus wall. This kind of screw-cramp could, in cases of great exposure, as a farther security, be also applied transversely or across the courses, so as to extend from low to high water mark.

It is proper, perhaps, in conclusion, to mention, that, in order to render the last or outermost stone as steady as the others in the same course, no more strain need be applied to the chains than is sufficient to make up for the defect of friction, caused by its not being in contact on all its four sides with other stones. The amount of the strain required for the chains should therefore, in each case, be proportioned to the number of the sides of the last or outermost stones which are exposed. It must be borne in mind, however, that, although the chains were slack, the last stone could hardly be carried away unless the chains were broken.

EDINBURGH, *November* 25, 1848.

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* As an additional security, another kneed plate *h*, may be inserted below the stone, and screwed up tight by the draw-screw *l*.

On the Motion of a Lava-Stream, observed on the Side of Mount Vesuvius, 27th April 1849. By A. MILWARD, Esq. Communicated by the Author.

Some observations on a Mud-Slide in Malta, published in the Philosophical Journal at the commencement of the present year, having been read with some interest, it has occurred to me that an account of some peculiarities connected with the motion of lava-streams, which have since come under my notice, may not be altogether unacceptable.

Professor Forbes has already drawn attention to this subject; and were it not that accident appears to have presented me with a more favourable opportunity than he seems to have enjoyed, these remarks would be superfluous from *me*. The chief interest they can possess, will consist probably in the light they may throw on the phenomena of the motion of viscous fluids;—and here I have the opportunity of presenting some interesting evidence of the tearing action produced by the difference of motion in the various parts which make up the breadth of the stream. Before, however, I proceed to this description, I would wish to make a few remarks on Professor Forbes's Fifteenth Letter on Glaciers. In a paper on Glacier Dirt-Bands, read before the British Association at Swansea, and afterwards published in this Journal, I ventured to put forth three inquiries, all of which seem to be answered by Professor Forbes in the affirmative. It appears, 1*st*, That, to a slight extent, structural bands of porous and compact ice are, if I mistake not, supposed to exist in the upper part of the glacier, consequent upon the mechanical pressure and compression which forms the ridges; but, nevertheless, this seems to be only a secondary phenomenon. 2*dly*, That there are waves or ridges, arising from similar causes to those which originate the ridges in a mud-slide; and, 3*dly*, That the saturation of the névé at the foot of the upper slopes, resulting in gushes of wet ice and snow, is the cause of these simultaneous phenomena.

I did not attempt to explain the origin of the ridges, as I was not then aware that they had been observed.*

It is curious, yet at the same time perfectly consistent, that, while in the mud-slide, the intervals between the ridges are most compact in structure, in the glacier the contrary is the case. In the *mud-slide*, the water drains away from the ridges the finer particles, and deposits them—a sort of alluvium—in the hollows. The difference of consistency is thus an *after* phenomenon. In the glacier, there can be no such *after* phenomenon. In this case, the cause is I think dependent upon the varying action of the phenomena concerned in the formation of the glacier, rather than upon a *mechanical* action.

Professor Forbes illustrates in an admirable manner, the way in which pressure from behind causes a wave or ridge in advance, in consequence of the posterior parts finding less obstruction in rising and over-riding what is in front of them, than the anterior parts do in forcing their way forwards. But I think, in the instance of the glacier, the ridge owes its origin *chiefly* to the accumulation at the foot of the steeps of névé, as I ventured to suggest in my former paper. My reason for this opinion is as follows:

Upon the ground which Professor Forbes so well explains, we may expect in all cases to find a ridge, formed by pressure, at a certain distance from the foot of the névé slopes. So long as these slopes remain unchanged in character, the *form* of the ridge will be permanent, although the névé or ice of which it is composed constantly moves on. On the occurrence of summer gushes, this ridge will necessarily be *enlarged*, because the weight or force which causes it, is increased ; and while it remains enlarged, the part of the gla-

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* I owe an apology to Professor Forbes, for not being aware that he had described the ridges in his Fifth Letter. Ill health, of long continuance, obliges me to be absent from England a great part of the year ; so that, withdrawn as I am from the means of reference, I cannot pretend to keep pace with the information published on any subject, and can only be excused for intruding on public attention when the "accident of travel" enables me to impart a knowledge of facts which would otherwise pass unnoticed. In the present instance, the edition of Professor Forbes's Travels which I used was the *first*, which does not contain the Fifth Letter, reprinted from this Journal.

cier which moves forward from it will have to that extent a higher level than the part which moved forward from the lower ridge; and at the same time that the ridge is enlarged, it is advanced further down the glacier, because the accumulation behind is also advanced. It must be remembered, however, that the ridge even when thus enlarged is small, compared with the mass of fallen névé resulting from the gush itself. The effects of adding tea-cups full of plaster in one of Professor Forbes experiments will illustrate this. The addition of each tea-cupful causes a crease, the direct result of the accumulation at the head of the slope, and the weight of this crease or accumulation may cause a ridge a little beyond it;* but this is so small as not to be remarked. The same *relative* insignificance must be true in the glacier. For a short time after a gush the glacier advances from the new

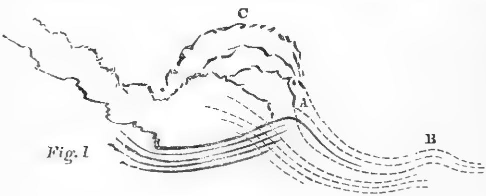

Fig. 1

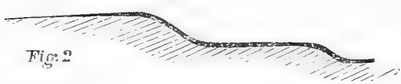

Fig. 2

ridge at a higher level B, but soon the advance is made from the accumulation resulting from the gush itself, and is at a very much *higher* level C, and forms the main summer ridge. Thus each ridge is preceded by a smaller rise, as in fig. 2, dependent on the difference of height of the two ridges, and is doubtless very small. The distance between the greater and lesser rise is represented by A B C in fig. 1, and yet, small as it is, this is all that results from mechanical pressure, the main ridge is the direct result of the accumulation produced by the gush, as was suggested in my former

* This will depend on the consistency of the plastic mass.

paper. In fig. 1, I have attempted to shew, by black and dotted lines, the first and second state of things at the foot of the névé steeps.

I am disposed therefore to conclude that the difference of mechanical pressure in connection with the greater and and smaller ridges is not sufficiently effective to warrant us in supposing that the compression caused in throwing up the summer ridge is the cause of the ice being then more compact. So that we cannot from this suppose structural bands of porous and compact ice to have any existence worthy of consideration. The difference of consistency will depend solely on the different degree of saturation in the summer and winter névé.

But to return to our observations on the lava-stream. As there are in the motion of a glacier peculiarities not common to other viscous bodies, and resulting from the varying action of heat at points *below* ordinary temperatures, so in a lava-stream are these characteristics no less striking, derived from the action of heat at *higher* temperatures. Indeed, so remarkable are the effects of this action, that few persons would be prepared at first sight to identify the masses of cinders which terminate one of these streams with the glowing current that issues so smoothly and rapidly from the interior of the mountain. The chief characteristics of lava appear to be the intense heat of the mass as it issues from the interior, and its very low conducting power, which causes it to retain for a long period its viscous nature. It is said also, that the cohesion of its particles is not very great.

The heat of the mass on its first appearance, allows the opportunity of rapid atmospheric action, favoured by the escape of gas, and the slowness with which the cooling action passes into the interior. The surface is in this way encumbered by a scum or slag which gradually passes into scoriæ, until at the lower part of the stream a complete chaos of cinders is presented to view. At no great distance from the source it appears to be a mass of broken fragments and pinnacles when seen at hand; but from a more distant and elevated point, a general impression of diverging streams is clearly produced. Occasionally a fragment at the termination of the stream will be tolerably smooth, and exhibit distinct curves and loops. These fragments have much the appearance of dried pitch, and the loops rise in masses, and have the peculiarity of curling over in some degree. This smooth appearance appears to be owing to the thinness of the lava at these points—(and perhaps also to some other cause), by which it cools more quickly without allowing time for the formation of thick scoriæ, and the consequent breaking up of the surface. Such examples are to be seen very frequently in the lava which in 1819 encroached on the Atrio di Cavalli, on the north-west side of the cone of Vesuvius.

The altered character and appearance of the surface of the streams extend to a very varied distance into the interior of the mass. Near Torre Annunziata, there is a very good section on the railway which shews the remarkable difference between the surface and the interior. I find the following note of it in my pocket-book. "Near Torre Annunziata, the railway cuts through a hard and thick stream of lava, apparently not very ancient, and extending to the sea-shore. It is curious to observe the difference between the compact grey interior and the brown decomposed surface, which the section displays most beautifully. The decomposed part varies in thickness from 2 to 6 feet: it is brown and friable, and looks like some kinds of hard brown mud crumbling into dust. It is partially cultivated, but does not appear old enough to be very fertile. The compact lava has cracks and cleavage planes passing down vertically or nearly so, more than in any inclined direction.

The lava streams which I had the opportunity of observing, formed small portions of a large and broad stream, the result of an eruption which occurred in the autumn of last year. The lava extends about two miles down the south slope of the mountain towards Torre Annunziata, and was still advancing a short time since. The greater part of the surface at the period of my visit was comparatively cold, and presenting a rough black surface, but many parts were still in such a state as to give off much smoke by day, and exhibit long luminous bands at night. In these places, the hot lava rising from below was nearer the surface than elsewhere, and was seen through the cracks of the superficial scoriæ. I particularly examined two of these luminous places, but shall chiefly confine my description to one of them, and refer to any difference worthy of notice in the other. The most striking characteristic of the great mass of lava on the south side of the mountain, was that of irregular heaps, fantastic projections; but from an elevated point, longitudinal ridges, and something like transverse ones, were suficiently apparent. The hot lava presented the curious phenomenon of flowing along the tops of these longitudinal ridges, so as to occupy the highest ground. I have reason to think that the greater part of the lava was still red hot, and plastic within little more than a foot or two of the surface. Where it pursued its course in a plastic state along the tops of the ridges, it evidently rose up from below like a spring, in consequence of the pressure from behind. A change in the inclination of the mountain side, or some obstruction below, was probably the immediate occasion of this rise. I examined particularly two of these streams which were not far from the east boundary of the lava, and where the black mass was still advancing slowly, and causing the small trees of a wood already partially destroyed, to bend before it from day to day.

The hottest of the two streams was about a dozen or twenty yards distant from the margin of the lava, and rose through a small hole in the top of a ridge, assuming a breadth of three or four feet, and following the line of the ridge from which it had emerged. It gradually concealed itself, as it advanced, beneath black scoriæ, which rapidly increased in thickness, and then broke away in fragments, as I shall presently describe. The lava, as it rose in an unbroken curve from the hole, was red hot, and perfectly homogeneous, free from scoriæ or surface-markings of any kind. Its consistency was so great that the sides where the ridge fell away did not, for the space of six or seven yards, spread out over the cold lava, but advanced with a boundary nearly vertical, and about a foot in thickness,—apparently preserving so far the form which it had been forced to assume at its appearance. In consequence of this exposure, I could distinctly see the upper part moving in layers over the under, which was in contact with the cold, or *old* lava, as we may call it. These layers were by no means parallel with the course of the stream, or with the surface, but assumed undulating forms evidently caused by the variation of declivity, and the obstacles at the bottom. Little portions of scoriæ often described a curved path as they rolled over with the layers of the stream. The fact of the layers moving at different rates was made evident to the eye by the existence of incipient lines of scoriæ partially covering the red lava. On the surface a similar differential motion was still more apparent near the origin. It has been already remarked, that the lava was perfectly red and glowing, as it emerged from the hole in the cold lava,* but it gradually assumed a darker appearance as its surface became encumbered with slag. This did not come on uniformly, and much sooner at the sides than towards the centre of the stream. Through a length of about five or six feet, and a breadth of nine or ten inches, the central part exhibited only slight parallel longitudinal markings, consisting of ribbons of slag alternating with the hot lava; and thus appeared to indicate the movement of bands or laminæ of lava passing one another at different rates, and so tearing as it were these shreds of slag. Towards the sides the slag became more and more continuous, but was honeycombed, as it were, with openings, caused perhaps by the escape of gas; and here the red lava was exposed. These openings also served to make manifest the fact, that the central parts moved with greater rapidity; for by noticing the relative positions of certain of them when they approximated to a line pointing obliquely up the stream, this line of direction would gradually be seen to become transverse, and then to point down the stream at the same time that it discontinued to be a *straight* line.†

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* I have spoken of the lava as cold, but this is only relatively correct, for near the rising stream, and particularly behind it, the black surface was too hot to be stood upon with any comfort, and everywhere the lava surface was more or less warm.

† I very much regret that the fact of my forming one of a large, though pleasant, party, and the near approach of evening, prevented my making such exact observations as to the peculiar appearance of the surface of the lava as I

In order to test the consistency of the lava, I threw several pieces of cold lava upon the surface of the stream, near the origin, where it was free from slag, and where, by the bye, it curved down slightly at the sides, being convex in form. These pieces were only a few pounds in weight, and if thrown with any force, it was very difficult to make them rest where they were cast; they bounded off just as if thrown upon a smooth sloping rock. By approaching as near the stream as the intense heat would allow me, I succeeded in causing some to remain on the surface, but generally close to the side. At length, however, I deposited a piece of a few pounds in weight just where the lava curved over from below, and in the middle of the narrow stream. After waiting until it had advanced curving upwards about a foot-and-a-half from the origin, I timed its rate of progress, and estimated that it passed over a foot in about twenty seconds. The heat was of course very great, as I stood within a yard of the red-hot stream, and the air for some distance around all these streams was in a state of constant agitation, just as it is seen over a heated stove, only to a much greater extent.

At the distance of ten or a dozen yards from the origin, this stream, like all the others, and like those which had already grown comparatively cold, became covered over with scoriæ, and gradually presented a broken surface of cinders.

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could have wished. Although I have a coloured drawing before me, in which I have endeavoured to give a general view of what I saw, yet, as it was not executed on the spot, but from memory, two days after, it cannot be anything more than a general resemblance. The lines of scoriæ were, as far as I can remember, from $\frac{1}{16}$th to $\frac{1}{4}$th inch wide, and separated by narrower intervals; they were a little broken and honeycombed. The great heat rendered it almost impossible to approach sufficiently near to see how these lines moved past one another. I did not at the time remark any loops, or any tendency to convergence. The latter may have existed, but it must have been slight. The centre was less marked with scoriæ, but they were not, I think, absent; and the fainter indication I ascribed at the time to the greater heat.

It has been suggested to me, that the differential motion is not sufficient to account for these appearances, and that they may be the result of some peculiarity in the manner of cooling. I would observe that if the differential motion caused only a tendency in the covering to crack in a slight degree, the *continuance* of the motion and the radiation of heat through the crack, would gradually enlarge it.

When the coating of slag—for some time tolerably smooth—was no longer sufficiently plastic to yield to the motion of the viscous fluid beneath, it was torn up in fragments, which increased in size as the fracture occurred further down. The first stream which I examined illustrated this characteristic phenomenon much better than that which I have just described. It was a much larger stream, and presented a well-marked front, rising five or six feet above the cold ridge along which it advanced. It was rounded at its extremity, and advancing very slowly. Its surface presented a covering of black irregular masses, cracked in all directions ; but the vertical and horizontal divisions were the most numerous. The red interior was thus very freely exposed, and the alteration in the position of the fragments, occasioned by the motion of the stream, could be readily observed. In places the heat had so far gained upon the coating of fragments, as to cause the surface to become red hot. As the plastic interior advanced, the surface and sides cracked away and rolled over, especially in front, where large vertical slices were frequently detached ; these would be more or less remelted as the lava flowed on, but many of the smaller fragments roll out of the way, and line the side of the stream with cinders. Where the surface has remained smooth for a short distance, as in the small stream first described, the fissures caused by the motion of the interior are more clearly seen to depend on the tearing force exerted. A rattling noise was constantly audible,* but now and then a louder crash marked the more considerable falls.

KEYNSHAM, SOMERSETSHIRE,
September 1849.

To Professor JAMESON, Edinburgh.

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* In Professor Forbes's paper in the Phil. Trans. for 1846, p. 150, allusion is made to this "jostling of parts ;" but it is not described as a superficial phenomenon caused by the cooling of the outer parts.

Synopsis of Meteorological Observations made at Whitehaven, Cumberland, in the Year 1848.
By J. F. Miller, Esq., F.R.A.S., &c. Communicated by the Author.

1848.	BAROMETER. Max.	BAROMETER. Min.	BAROMETER. Mean at 11 A.M.	BAROMETER. Mean at 3 P.M.	BAROMETER. Mean at 11 P.M.	BAROMETER. Mean of the three.	BAROMETER. Range.	THERMOMETER. Absolute. Max.	THERMOMETER. Absolute. Min.	THERMOMETER. Mean of Max.	THERMOMETER. Mean of Min.	THERMOMETER. Approximate Mean Temperature.	THERMOMETER. Range.	PLUVIOMETER. Rain.	PLUVIOMETER. Snow.	Wet Days.	Evaporation Gauge.	Prevailing Winds.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	°	°	°	°	°	°	°	Inches.	Inches.		Inches.	
Jan.	30·39	29·01	29·772	29·737	29·728	29·745	1·38	53·5	15·	38·39	29·60	33·995	38·5	3·745	·355	13	·743	W., Var.
Feb.	30·27	28·34	29·370	29·365	29·375	29·370	1·93	50·	21·	46·00	38·42	42·210	29·	7·815		23	·792	SW. & W.
March	30·00	28·47	29·417	29·426	29·450	29·431	1·53	56·	31·5	46·82	36·74	41·780	24·5	4·588		23	1·397	NE.
April	30·01	29·00	29·605	29·596	29·605	29·602	1·01	63·5	30·5	52·01	39·46	45·735	33·	·495		16	2·728	NE.
May	30·22	29·15	29·901	29·904	29·909	29·904	1·07	73·5	36·5	63·45	48·06	55·755	37·	1·798		6	4·580	SW.
June	30·05	29·13	29·587	29·590	29·590	29·589	·92	75·	42·	64·24	51·34	57·790	33·	3·867		19	3·749	SW.
July	30·33	28·89	29·780	29·778	29·786	29·781	1·44	73	47·	65·59	54·56	60·075	26·	3·630		18	3·935	W.
Aug.	30·03	29·05	29·631	29·648	29·662	29·647	·98	68·	45·5	63·11	52·19	57·650	22·5	5·054		19	3·686	W.
Sept.	30·21	29·16	29·814	29·806	29·805	29·808	1·05	72·	38·	60·91	51·31	56·110	34·	2·266		13	2·896	SW.
Oct.	30·15	29·03	29·614	29·619	29·634	29·622	1·12	63·	31·5	53·19	46·18	49·685	31·5	5·772		22	1·549	SW.
Nov.	30·41	28·82	29·710	29·697	29·700	29·702	1·59	53·5	27·	46·46	38·95	42·705	26·5	3·507	·208	21	1·129	NW., Var.
Dec.	30·29	28·50	29·641	29·631	29·640	29·637	1·79	54·	25·5	45·12	38·74	41·930	28·5	4·805		18	1·019	SW.
Means	30·19	28·88	29·653	29·649	29·657	29·653	{1·32 / 2·07}	62·9	32·6	53·77	43·79	48·785	{30·3 / 60·0}	47·342	·563	211	28·203	SW.

1848.	Absolute Minima.			Mean Nocturnal Temperature.				Terrestrial Radiation.							
								Maximum.			Minimum.			Mean	
	Six's Thermometer, 4 feet above Ground.	On Cork on Grass.	On Wool on Grass.	Six's Thermometer at 4 feet above Ground.	Naked, on Cork on Grass.	Naked, on Wool on Grass.	Difference between Temperature on Cork and on Wool.	On Cork on Grass.	On Wool on Grass.	Day.	On Cork on Grass.	On Wool on Grass.	Day.	On Cork on Grass.	On Wool on Grass.
January,	15°·	9°·2	8°·5	29°·60	23°·03	21°·76	1°·27	12°·5	13°·	13th.	0°·5	0°·5	30th.	6°·57	7°·84
February,	21·	14·5	12·2	38·42	33·18	32·65	0·53	13·5	12·7	17	1·	0·8	5	5·24	5·77
March, .	31·5	22·7	21·	36·74	31·24	29·76	1·48	9·7	13·	4	1·2	2·2	5	5·50	6·98
April, .	30·5	24·	19·7	39·46	33·55	31·13	2·42	17·	20·	27	2·	4·	23	5·91	8·33
May, .	36·5	25·	20·2	48·06	38·68	36·22	2·46	19·	21·	4	4·5	6·5	28	9·38	11·84
June, .	42·	31·	27·	51·34	45·36	43·53	1·83	12·3	16·7	16 & 17	0·7	1·	18	5·98	7·81
July, .	47·	37·	35·	54·56	49·15	47·48	1·67	10·7	11·2	3	1·	1·	18	5·41	7·08
August,	45·5	35·	34·5	52·19	45·21	43·93	1·28	11·	13·5	6	1·	1·	22	6·98	8·26
September,	38·	28·7	27·	51·31	44·86	43·57	1·29	13·	15·7	1	0·8	1·	16	6·45	7·74
October,	31·5	22·2	21·	46·18	39·20	38·25	0·95	12·5	16·5	11	1·5	1·5	1	6·98	7·93
November,	27·	14·	10·	38·95	32·15	30·73	1·42	14·	18·	5	1·5	1·5	16 & 26	6·80	8·22
December,	25·5	11·2	7·	38·74	31·30	29·81	1·49	15·5	19·8	22	2·3	2·3	13	7·44	8·93
1848, .	32·58	22·87	25·	43·79	37·24	35·73	1·51	13·39	15·92		1·50	1·94		6·55	8·06
1847, .	33·70	24·01						12·60			0·70			5·94	
1846, .	36·12	23·12						14·62			1·35			7·20	
1845, .	32·87	22·06						12·75							

Hygrometers.

1848.	Mean Dry Bulb.	Mean Wet Bulb.	Mean Dew-Point, deduced*.	Mean Dew-Point, observed.	Mean Complement of Dew-Point.
January,	36°·04	34°·31	31°·62	31°·73	4°·30
February,	43·61	42·22	40·47	40·38	3·22
March, .	44·64	42·18	39·35	39·32	5·32
April, .	49·96	45·96	41·72	41·66	8·40
May, .	58·67	53·54	49·87	49·84	8·82
June, .	60·73	55·44	51·74	51·73	8·99
July, .	63·00	58·05	54·63	54·81	8·19
August,	60·36	55·00	51·25	51·06	9·30
September,	59·14	54·89	51·91	51·54	7·59
October,	51·49	48·57	45·65	45·60	5·89
November,	44·37	42·37	40·05	40·04	4·33
December,	42·90	41·17	39·03	38·71	4·19
Means, .	51·24	47·81	44·77	44·70	6·54
1847, .	51·20			44·03	7·17

* In determining the deduced point, Mr Glaisher's valuable Hygrometrical Tables have been used, and the above results, as also those for the year 1847, shew, in a striking manner, the extreme accuracy of those tables, as well as the correctness of the factors deduced from the six hourly hygrometrical observations made at the Greenwich Observatory, on which Mr Glaisher's tables are founded. The above observations were taken twice daily, viz., at 11^h A.M. and at 3^h P.M.

Remarks on the Climate of 1848.

On reviewing the various elements of the weather in the past year, I find that the fall of rain is half-an-inch above, and the mean temperature a quarter of a degree *under*, the average of eleven years.

The dew-point is 0°·87 above, and its complement is nearly identical with the average.

The evaporation is 2·17 inches under the usual quantity, and 19·13 inches under the deposit of rain; it exceeds the rain in the months of April, May, and September, and in June and July the two processes very nearly balance each other.*

The greatest depth evaporated in 24 hours, is 0·243 inch, on the 17th of July, temperature 70°, dew-point 62°·5, with a serene and cloudless sky; the *least* is 0·002 on the 5th of February, with an exceedingly thick atmosphere within 1° of saturation, and rain.

The winds in 1848 have been distributed as under :—

N. 30; NE. 40; E. 32½; SE. 40½; S. 65½; SW. 50½; W. 59½;

* The gauge receives a fair proportion of wind and sunshine, and it is always exposed in the open air during the day, except when rain is falling; at night, it is placed under a capacious shed supported by iron pillars.

NW. 38½, and dead calms, 9; the westerly exceeding the easterly winds on 71 days, or by one-half nearly.

The Forces are divided into the following classes: calm, 39; light, 63; moderate, 107; fresh, 69; strong, 59; and gales, 29 days; the calms being 11, and the stormy days 7, under the average number in the 6 previous years.

The Weather.—In 1848, we have had 18 perfectly clear days; 137 more or less cloudy, but without rain; 211 rainy; 289 on which the sun shone out; 45 days of frost (the thermometer below 32°, at 4 feet from the ground); 4 of snow; 18 of hail (of which one occurred in July, and one in August); 14 of thunder and lightning; and 3 days of lightning without thunder. There have also been 1 solar and 7 lunar halos, and 18 appearances of the aurora borealis, of which 15 were registered in the last 4 months of the year. The cloudless days are 10 less, and those of thunder and lightning are 6 less, whilst the days of sunshine are 18 *more*, than the usual number.

The exhibitions of the aurora borealis during the winter of 1848–9 were so numerous, and such was the diversity of form, the richness and variety of the tints, and the extraordinary beauty and brilliancy of the meteor on many occasions, that its more prominent features must have arrested the attention even of those who do not ordinarily notice either atmospheric or celestial phenomena.

The most notable of these appearances occurred on the nights of the 18th of October and the 17th of November; but as the writer published a minute description of the phenomena at the time, it will be unnecessary to do more than briefly notice them in this place. Between the 18th of October and the end of the month, the sky was lit up by auroræ almost every clear night. During this period there were frequent sudden torrents of rain, attended with thunder, lightning, and large quantities of hail and snow in many places; likewise brilliant meteors, and other indications of violent electrical disturbances in the air. During a thunder-storm on the morning of the 23d, the electric telegraph on the Whitehaven Junction Railway was so affected by the atmospheric electricity, that the signal bell several times rung spontaneously, during the continuance of the storm.

The magnets at the Greenwich Observatory underwent unusual oscillations, (as also on the 16th, 17th, and 18th, of November), and at Liverpool, the electric telegraph was, for a time, rendered useless.

Suspended in the library of the Greenwich Observatory is a photographic tracing, exhibiting the strangely abnormal curves formed by the magnets during the aurora of the 17th of November. The aurora of 18th October, was seen all over Great Britain, except in the South of England, where heavy rain was falling at the time. That of 17th November extended to Naples, Madrid, Oporto, Montreal in North America, and to the Azores, where the phenomenon had never before been witnessed. The portion of the sky reflecting the red light was greater than in any aurora I have seen, not excepting the gorgeous displays which occurred on the nights of the 25th and 26th of January, 1837.

Radiation.—The surface of the earth and all bodies upon it, have a constant tendency to throw off at night, the thermal rays which they receive from the sun and other sources during the day. Indeed, it is certain that this process takes place to a certain extent by day as well as by night, in situations shaded from the direct rays of the sun, as I have frequently found by direct experiment. And, with a clear sky, dew may often be observed on vegetation in the shade, when the sun is a considerable height above the horizon; in such cases, the temperature of the surface must not only be considerably under the temperature of the air, but also below that of the dew-point, as from several experiments which I made during the past winter, it appears that dew is rarely deposited on grass, until its temperature has fallen *several degrees below* the point of saturation, at the ordinary height of a thermometer in the air.

The radiating powers of the various substances in nature are probably as various as the bodies themselves, in all their numerous combinations and modifications of condition; and this property appears to obtain in the direct ratio of their absorbing, and in the inverse ratio of their conducting and reflecting powers. The leaves of plants are excellent radiators, and from their sharp angular form, are well calculated to throw off superabundant heat. Hence, the natural grassy surface of the globe plays an important part in modifying the temperature of the air, and in effecting the formation of dew,—a material highly conducive to the healthy growth and sustentation of the vegetable kingdom. As there is a constant tendency towards an equilibrium of temperature in all bodies, relatively warm substances will radiate latterly a portion of their heat, which will be absorbed by neighbouring bodies at a lower temperature. Moreover, the freely radiating surface of grass will receive by conduction, an accession of heat from the subjacent soil; and the quantity of caloric which it would naturally part with under any given circumstances, will be apparently diminished by that amount. As it is necessary to keep the radiation thermometers level with the points of the grass, for several years I made use of a flat piece of cork (about 1½ inch in thickness), which is a very slow conductor, for this purpose; but latterly I have employed raw white wool, which is perhaps the most perfect non-conducting material hitherto discovered; and it is my intention to reduce all past and future results to this standard. The mean monthly difference in the results between the temperature on cork and on wool, varies from 0°·95 to 2°·46, the average difference being about 1½°.

I am now (1849) making daily observations of the temperature on the surface of grass, with a naked thermometer suspended on *Ys*, and so far as the experiments have proceeded, the mean difference of the readings from those on wool is about 3°.* When this series is complete, the observations taken on cork and on wool, can at any time be reduced to the standard of short grass.

From the above table, it will be seen that the temperature has been below 32° in every month of 1848, except in July and August, when the minima were 35° and 34°·5 respectively; but vegetation is liable to be

* For the results of experiments (made at Whitehaven) on the radiating powers of various substances, also on the amount of radiation as indicated by a thermometer placed in the focus of the parabolic mirror, *vide* Mr Lowe's Treatise on Atmospheric Phenomena.

subjected to a temperature at or below the freezing point of water at *any* period of the year; and on the open heaths and meadow lands of England, where the radiation is not diminished by fortuitous accessions of heat from surrounding objects, the surface must often fall far below the point of congelation, even in the hottest months.

In January 1848, a naked thermometer on the grass, placed on wool, was at or below 32° on 29 nights; in February on 12 nights; in March on 23 nights; in April on 20; in May on 6; in June on 2; in July, none; in August, none; in September on 3; in October on 7; in November on 13; and in December on 19 nights.

The greatest difference I have ever found between the minimum of a thermometer placed on raw wool and fully exposed to the sky, and that of a six's thermometer at 4 feet above the ground, and protected from radiation, was 21°; and this great disparity has occurred on two occasions, viz., on the night between the 4th and 5th February 1847, and during the night between the 3d and 4th May 1848; and on one or two other occasions it has amounted to 20°. Mr Glaisher, F.R.S., of the Royal Observatory, who has thrown more light on this interesting and important subject than any other person, in the remarks accompanying his extensive and elaborate tables printed in the Philosophical Transactions, states that the greatest difference he ever observed between the *simultaneous readings* of two thermometers so circumstanced, was 25°, and this extraordinary difference occurred once only in a period of several years.

But the differences which obtain between the minimum readings of self-registering thermometers placed on substances exposed on the surface, and in air, are not the maximum differences, unless the two minima occur at the same time, which is seldom the case.

I have generally found the lowest temperature on the ground to occur some hours before midnight, and rarely after it; whereas the greatest degree of atmospheric cold takes place about the time of sunrise, as is well known to those whose occupations cause them to pass the greater part of the night in the open air.

The difference between the two minima should be increased by the difference between the readings of the air-thermometer at those times, which, according to Mr Glaisher, may amount to 10°. Hence, it is probable, that when a difference of 20° or 21° obtained between the minima at Whitehaven, an *absolute* difference of 30° may have occurred during some part of the preceding evening or night. The greatest differences recorded by Mr Glaisher (with S.R. thermometers) were, on raw wool 20°·4, and with flax 21°·8, which are nearly identical with the extreme differences found at Whitehaven.

From the large amount of heat absorbed by the earth during the summer, it might be supposed that the effect of terrestrial radiation would be greatest in the autumn and early part of winter, and least in the spring months. But a careful examination of my observations does not countenance this opinion; on the contrary, the radiation is occasionally small in the winter, and large in the summer months; but such excess or deficiency is caused wholly by accidental atmospheric conditions, which favour or retard the process. On the whole, it appears that under equal and similar circumstances, the amount of radiant heat thrown off by the earth's crust at night, is pretty equal at all seasons, and at all temperatures of the air.

The following brief notes will convey a general idea of the character of each month in the bygone year.

January.—A seasonable frosty month. The temperature, rain, and evaporation, are all below an average; the first by 4°·70, the second by 0·338 inch, and the evaporation by 0·164 inch. The thermometer in air has been below 32° on 23 nights, and on the night between the 28th and 29th, it fell to 15°, a point which it rarely reaches at this place.

February.—An excessively wet month, with an unusually high temperature, and an extremely *low* atmospheric pressure. The average fall of rain for the second month is 3·611 inches; but this year we have had 7·815, or 4·204 above the average quantity. This is the wettest February on record at this place. The barometer was below 28 inches from the morning of the 22d February to the night of the 1st of March, a period of nine days. The mean temperature is 2°·95 above the average of 10 years, and the evaporation is 0·298 inch *below* the mean amount for the month.

Bees began to bear burdens on the 20th.

March.—Similar to February; a wet month, with high temperature and low atmospheric pressure. The temperature and depth of rain are both above the average, the former by 0°·75, the latter by 0·90 inch. The evaporation is nearly half an inch below the usual quantity. The tortoiseshell butterfly (*Vanessa Urtica*) was seen on the 23d. The zodiacal light was visible on several evenings during the month.

The temperature of the quarter ending 31st March, is 0°·33 below the average. The average fall of rain in the first quarter of the year is 11·179 inches; in 1848, we have had 16·148 inches, or 4·967 inches above an average quantity. The evaporation is 0·909, or nearly an inch below the usual amount.

The deaths throughout the Union during the quarter, are 276, being 43·9, or 19 per cent. above the corrected quarterly average (232·1); for the town only, they are 151, being 45·3, or 43 per cent. above the average number, which is 105·7. The numbers, both for the town and Union, are greater than in any corresponding quarter (except in 1847) since the register was begun in 1839. The deaths exceed the births, in the town by 56, and in the whole Union by 52. It appears by the Registrar-General's report, that the same remark applies to the mortality all over the kingdom. The deaths are 6,755 above the calculated average for the quarter.

April.—A fine dry month, with frequent very slight showers. Less than half-an-inch fell in 17 days, and there was only one day on which the sun did not shine out more or less. The temperature is $\frac{3}{10}$ths of a degree under the average. The depth of rain is 1·968 inch below, and the evaporation is 0·232 inch *above*, the mean quantity. The Io butterfly appeared on the 1st. Swallows made their appearance on the 21st; and the cuckoo was heard towards the end of the month. The cuckoo was heard, and three swallows were seen at Wastdale Head on the evening of the 19th.

Between the 2d and the 9th, the maximum temperature had fallen 21°, and the minimum 15 degrees.

May.—A month of most delightful summer weather, with an unusual proportion of clear sky. The sun shone out every day in the month, and during a considerable part of it, the sky was almost free from clouds. The atmospheric pressure is unusually high; and, except from the 15th to the 20th, it has been very steady and equable. The temperature is 2°·62, and the evaporation is 0·521 in. above the average for the month. The rain, although only 0·17 in. below a mean quantity, all fell in six days; and more than ¾ths of an inch of the whole depth (1·798 inch) was measured on the morning of the 31st day. Corncrake heard on the 21st.

June.—A rather cold month. The temperature is 1°·00 below the average. The rain is a mean quantity, and nearly the whole of it has been thrown off by evaporation. The evaporation is nearly an inch less than usual.

The mean temperature of the quarter ending June 30th is 0°·43 above the average. The average depth of rain in this quarter is 8·333 inches; in 1848 there has fallen 6·16 inches, or 2·17 inches less than usual. The evaporation, which is nearly an average quantity, exceeds the fall of rain in the summer quarter of 1848, by 4·897 inches. The deaths in the town of Whitehaven, during the past quarter, are 117, being 33·3, or 40 per cent. nearly; and, for the entire Union they are 225, being 26·4, or 13½ per cent. above the calculated average numbers in the corresponding quarters of the nine previous years (from 1839 to 1847, inclusive), which are 83·7 and 198·6 respectively. The mortality is greater than in any previous summer quarter since the register was commenced. Yet, notwithstanding this great excess, the births in the town exceed the deaths by 32; and in the entire Union the births exceed the deaths by 105. The deaths throughout England are only 927 above the average. The Registrar-General, in his report for June, remarks: "It is gratifying to observe a very remarkable improvement in the state of the public health. The mortality of the country, after being excessively high during the latter half of the year 1846, the whole of 1847, and the first quarter of 1848, is now little above the average of the nine years ending with 1847."

July.—A rather cold but fine month, and on the whole favourable for the hay harvest. Strong winds and heavy showers prevailed from the 20th to the 27th. The temperature is 0°·63 under the average, and 3°·13 under that of July 1847, which was a remarkably hot and dry month.

The rain and evaporation are both under a mean quantity,—the former by 1·46 inch, and the latter by 0·315 inch.

Hay was mostly under cover in this neighbourhood by the 19th; the crop was good, and it was secured in excellent condition.

August.—A cold, wet month. Temperature 2°·14 under the mean. The rain and evaporation are above the average respectively, by 1·33 inch and 0·34 inch. The grain harvest in this neighbourhood commenced on the 11th instant.

September.—A fine, mild, and rather dry month. The mean temperature is 0°·38 above the average. The evaporation and rain are both *under* a mean quantity; the former by 0·33, and the latter by 1·10 inch.

The high maximum temperature of the 5th (71°·5) and 23d (70°·5), is worthy of notice.

Early in the morning of the 13th there was a total eclipse of the moon; but it was not seen here, the sky being overcast throughout the night.

About the time of the eclipse, the temperature at Greenwich fell to 32°, being 15° lower than the minimum at Whitehaven. Between the 8th and 13th, the mean temperature at Greenwich had declined 16°; at Whitehaven it only fell 8° in the same period. The grain harvest was completed in this neighbourhood by the 19th.

The temperature of the quarter ending 30th September, is 0°·79 under the average of the season, and the evaporation is $\frac{1}{10}$th of an inch under the mean amount. The average depth of rain in the autumn quarter is 12·652 inches; this year we have had 10·95 inches, or 1·70 inch below the ordinary quantity.

The deaths throughout the Union during the quarter, are 199, being 13·3, or 7·5 per cent above the corrected average (185·7) of the previous nine years.

In the town, the deaths are 113, being 22½, or 25 per cent. above the calculated average number for the quarter, which is 90½.

The births exceed the deaths in the town by 13, and in the entire Union by 107. The deaths throughout England are 809 *under* the corrected quarterly average, and less by 6034 than were registered in the corresponding quarter of 1847.

October.—The temperature is 0°·49 under, the fall of rain 0·51 inch *above*, and the evaporation 0·45 in. under the respective averages for the month. Between the 4th and the 6th the extremes of temperature only varied 3°, and on the 6th the fluctuation was but 1°·8. First appearance of ice on the morning of the 18th. The cabbage-butterfly was seen on the 6th, and the tortoiseshell variety on the 10th and 12th. The swallow was not seen in this neighbourhood after the 15th September.

November.—The mean temperature, rain, and evaporation, are 1°·53, 1·35 inch, and 0·23 inch respectively, under the monthly averages. Snow fell on the evening and night of the 8th, and the ground was thinly covered with it throughout the following day. On the 5th, between 2 A.M. and 11 A.M., the temperature rose 19°, and the dew-point 23°: the dew-point had fluctuated no less than 44° in the 48 hours preceding 11 A.M.; having fallen 19° on the 4th, and risen 25° on the 5th. The temperature had fluctuated 31°·5 in the same period.

December.—The mean temperature is 1°·04, and the rain 0·23 inch above, whilst the evaporation is coincident with the monthly average.

The mean temperature, fall of rain, and amount of evaporation, for the last quarter of the year 1848, are all below the averages for the period,—the first by 0°·32, the second by 0·60 inch, and the evaporation by 0·77 inch.

The deaths throughout the union during this quarter, are 283, being 84·4, or 43 per cent. nearly; and for the borough they are 124, being 22·6 in number, or 22½ per cent. above the respective calculated averages, which are 198·6, and 101·4.

The births exceed the deaths, in the town by 41, and in the whole Union by 33. Whilst the mortality in this neighbourhood was considerably *above*, the deaths throughout England during this quarter were 2571 *under* the average number in the nine previous years, making an allowance of 1·75 per cent. annually for increase of population.

The total number of deaths in the year 1848, in the borough of Whitehaven, is 505; and for the whole Union, comprising 23 districts, they are 981, the calculated average numbers, from 1839–47 inclusive, being 381·1, and 823·7, respectively. Consequently, the deaths in the town are 123·9, or $32\frac{1}{2}$ per cent., and for the entire Union they are 157·3, or 19 per cent. nearly above the average number. The absolute number of deaths in this borough, during the last 10 years, are as under: 1839, 313; 1840, 260; 1841, 316; 1842, 303; 1843, 337; 1844, 309; 1845, 287; 1846, 522; 1847, 534; and 1848, 505. In the whole Union, they are as follow: 1839, 753; 1840, 607; 1841, 646; 1842, 630; 1843, 695; 1844, 657; 1845, 664; 1846, 1038; 1847, 1175; and in 1848, 1174.

In 1848, the deaths exceed the births, in the town by 30, and in the whole Union by 193. The births in 1848 exceed the average number, in the town by 100, and in the Union by 270. The average annual number of births in the borough and Union are 435 and 904; and the numbers in 1848 are 535 and 1174 respectively. By the census of 1831, the population of this borough was 13,193, and of the suburb of Preston quarter, 4,323. By the census of 1841, the population of the town was 12,107, and of Preston quarter, 4,528. In consequence of an application from the Local Sanitary Board and other parties in 1848, to have this town placed under the "Health of Towns Bill," and it being the general opinion that the population had greatly increased since the census of 1841, and, consequently, that the per-centage of deaths was considerably augmented by such increase, a staff of upwards of 40 gentlemen, residents in the town, undertook to take the census anew, and, on the 18th December 1848, it was taken accordingly; the results for the town being 14,070, and for the suburb, 4721,—total, 18,791. Hence, it appears that the population of this borough has increased by 1963 persons in the last 7 years, being at the rate of 2·3 per cent. annually. In the above calculations on the mortality, I have assumed the mean population in the 9 years from 1839 to 1847, to be the mean between the census of 1841 and the census of 1848, say 13,088 persons, which is an increase of very nearly $\frac{1}{12}$th on the population in 1841. Hence, to the *absolute* number of deaths in any quarter, I have added 8·1 per cent. (which is equivalent to an increase of 2·31 per cent. annually) to obtain the mean quarterly average, corrected for increase of population. It is presumed that, on these data, the excess or diminution of the mortality, over or under the mean quarterly averages, as given above, must be very near the truth indeed. The Registrar-General of births, deaths, &c., allows 1·75 per cent. annually, for increase in the number of deaths consequent on increase of population. Assuming the mean population of the borough in the 10 years between 1839 and 1848 inclusive, to be a mean between the census of 1841 and that of 1848, viz., 13,088, the mean annual number of deaths in that period is 368·6, being one in every 35·5 persons, or 28·1 deaths in every thousand persons. By adding $\frac{4}{7}$ths of the increase of population, between the years 1841 and 1848, to the population in the former year, we have a mean of 13,228 inhabitants for the 7 years from 1839 to 1845, and the mean annual number of deaths in that period is 303·5, which gives 1 death annually in every 43·5 persons, or 22·8 deaths per 1000. Assuming the mean population in the 3 years between 1846–48, to be 13,649, (the mean of 13,228, and 14,070), the mean annual number of deaths is 520·3, being 1 in every 26·2 persons, or 38·1 deaths per 1000 inhabitants in those three unhealthy years.

In the past year 1848, with a population of 14,070, the deaths are 505, being 1 in every 27·8 persons, or 35·8 deaths in every 1000 inhabitants. The average annual rate of mortality throughout England, in towns, is 26, and in the country districts 18 in 1000; hence the deaths at Whitehaven, in the years 1846, 1847, and 1848, have been in excess to an extent unprecedented in former years.

THE OBSERVATORY, WHITEHAVEN,
13*th November* 1849.

Account of a Halo observed at Pictou, Nova Scotia, August 23, 1849. By J. W. DAWSON, Esq. Communicated by the Author.

Complex halos, consisting of several circles and arcs, have not recently been so rare as formerly in this province. Within the last six years we have witnessed three of these appearances. Of the two first, I believe no very accurate notes or figures have been preserved, at least in this part of the province. Of the last and most beautiful, I now send you figures and descriptions.

In the town of Pictou, the halo was attentively observed by a number of persons. Several sketches of it were taken, and the diameter of the principal circle was measured. At the Albion Mines, nine miles southward of Pictou, and at an elevation of 120 feet above Pictou harbour, the different parts of the halo were measured, and its whole progress watched by Henry Poole, Esq., an experienced meteorological observer.

Fig. 1 represents the halo, as seen by Mr Poole at the Albion Mines. The following are his notes of its appearance, with a copy of which he has kindly furnished me for insertion in this communication.

"*Albion Mines, Pictou, Nova Scotia, lat.* 45° 34′ 30″ *north; lon.* 62° 42′ *west; Thursday, Aug.* 23, 1849.—The ring A was visible at noon; rings B and C were visible at $3\frac{1}{2}$ P.M.; the arcs D and E directly afterward, and F shewed as far as the points *y* and *s*. The sun's position was $63\frac{1}{3}$° west of true south, at an altitude of $29\frac{1}{2}$°, and the junction of rings A and B, was at an altitude of $51\frac{1}{4}$°.* The

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* Measured with a theodolite.

ring B appeared to be in proportion to A as 3 to 2; ring C had the same diameter with A; rings D and E the same diameter with B, and the ring F double the diameter of B. The rings A and C, and the rings A and B, at the points where they intersected, and the arcs D and E, were prismatically coloured; the other parts, and the ring F, were white. No mock suns were visible at the points of junction of the rings; but the rings were at these points somewhat flattened.

"At 3¾ P.M., the rings B, C, D, and E, had nearly disappeared, and the ring F became perfect; and, at 4 P.M., all the rings had gone off, except A, which continued until near sunset.

"Barometer 29·84 in.; thermometer 75° in the shade. Wind gentle, from north-east; cirro-stratous clouds from north-east, and cumuli rising from south-west near the horizon."

Fig 1.

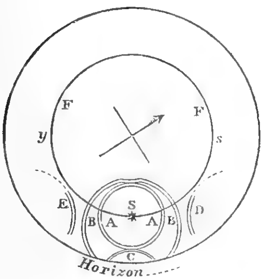
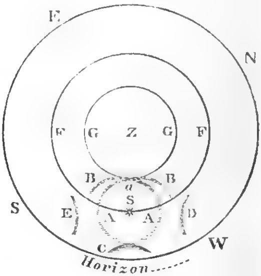


Fig. 2.



Halo observed at Pictou, Nova Scotia, August 23, 1849.

Fig. 2 represents the halo, as drawn by the writer at Pictou, with the aid of sextant measurements, made by Mr James Yorston of this place, and the following are the notes taken at the time:—

"The altitude of the sun, at 33 minutes past 3, was 33° 40′ 30″. The wind was light, and from the north-west; and the sky was covered by a thin cirro-stratus, curdled into small patches, and distinctly, though irregularly, fibrous. This cloud appeared to be the seat of the luminous rings.

"The circle A had a diameter of 45°. Its upper part was bright, and shewed the prismatic colours, and it became gradually fainter towards its lower or western side. This I have endeavoured to represent in the figure by the shading, the brightest parts being most deeply shaded. It was crowned by the two arcs B D, which cut the circle A, and each other in the point (*a*), producing an appearance of flattening or depression at that point. These arcs were prismatically coloured, and faded toward their extremities.

"The arc C was short and broad, well defined at its convex margin, but fading towards its concave margin and extremities. It was coloured, the yellow ray predominating.

"The arcs D E were brightly coloured, and appeared to have their convex sides towards the sun. They had much the aspect of fragments of rainbows.

"The large horizontal circle F, passing through the sun's centre, was of a milky whiteness. This circle retained its perfect form for a very short time, and the portion nearest the sun was always the most clearly defined. The smaller horizontal circle *g* was the last part of the halo in order of development, and was very transient. It was white, and extremely faint."

It will be observed, on comparing these descriptions, that the arcs B B were seen at the Albion Mines as parts of one circle, somewhat flattened at the point where it touched the halo A. At Pictou, they seemed to be arcs of two circles, each of about the same diameter with A. At Pictou, however, their apparent length was much less than at the Mines. The circle *g* was not seen at the Albion Mines. These discrepancies may possibly have been due to the small difference of elevation and latitude between the stations.

After the disappearance of the other circles and arcs, the circle A, crowned with portions of the arcs B B, still remained, though it gradually became less distinct. After the disappearance of the horizontal circles, the stratum of cirrus lost its curdled appearance, and was replaced by a more dense and uniform veil, with straight fibres, the wind at the same time changing to north-east. Towards evening a few ragged cumuli formed beneath the cirrous veil. On the following day the wind was still north-east, and the sky overcast, threatening rain; but only a very few drops fell. For some time before the 23d, the weather had been dry and warm. The northerly wind, which accompanied and followed the halo, caused a slight fall of temperature.

*Personal Observations on Terraces, and other Proofs of Changes in the Relative Level of Sea and Land, in Scandinavia.** By ROBERT CHAMBERS, Esq., F.R.S.E. and V. P. S. A. Sc. (With a Plate and Map.) Communicated by the Author.

The remarkable proofs which Scandinavia affords, of changes in the relative level of sea and land, have for many years attracted the attention, not only of the native, but of several eminent foreign geologists. The observations made by these inquirers are more or less generally known. They refer to beds of shells, identical in species with those of the present coasts, in parts of the country far inland, and elevated considerably above the present sea level; remains of serpulæ, balani, and other marine animals, adhering to the rocks in certain inland situations; terraces at different heights above the sea, which have evidently been formed by that element, whether appearing as detrital deposits, charged with shells or otherwise, or as indentations in rocky coasts, a result of the wearing agency of the waves. Attention has also been drawn, as is well known, to an apparent rise of parts of the Scandinavian Peninsula towards the south, at a slow but steady rate; a phenomenon in which we seem to have presented to our living eyes some remains of that force, whatever it is, by which the greater changes of ancient times were effected.

In a tour of Norway and Sweden, during the summer of the present year, I had an opportunity of making some personal observations on the phenomena connected with the changes of the relative level of sea and land; and of these I shall lay an account before the Society, along with a few illustrations which may be of service in helping out description.

I shall first allude briefly to a few elevated alluvial formations in the southern part of Norway.

At the head of one of the branches of the Christiania fiord, where the busy mercantile town of Drammen is situated, a river, named, from its clayey banks, Lir, enters the sea.

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* Read before the Royal Society of Edinburgh, Dec. 3, 1849.

M^r Chambers delt. Fr. Schenck lith^r Edinburgh

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These banks, for several miles from the embrochure of the stream, are composed of terraces of clayey alluvium, rising above each other to the height of several hundred feet. In some instances, I succeeded in ascertaining that these were of corresponding heights on the different sides of the river. Some of them persevere for several miles along the valley at one uniform height. I therefore considered them as roughly indicative of stages or pauses in the change of the relative level of sea and land. Taking as a basis the surface of the sea, which is here at its mean level, I found, by careful measurement with the level and the staff, that two of the most distinct and persistent of these terraces were respectively of these heights, namely, 77 and 98 feet; taking in both instances that point in the sectional outline of the terrace, where the moderate inclination of its surface gives place to a new rise. A very distinct portion of this latter terrace runs along under a country-house called Nystad, and towards one called Rudd House.

The valleys of those affluents of the Glommen River, which are crossed on the road from Christiania to Trondheim, are full of alluvial terraces of various materials. That of the Nytte River presents terraces of sand; that of the Leer River (a different river from the Lir above mentioned) affords terraces of clay; and hence, no doubt, the name given to the stream. The appearance of these terraces, broken into short spaces by side streams, and often having farmsteads perched on the detached pieces, while the banks in front descend at a deep inclination to the bottom of the valley, produces scenery of a peculiar and striking kind. The pass from this valley to the next, in which runs the river issuing from the Miösen Lake, is a broad flat space composed of a bed of water-worn gravel, with pieces as large as a man's head; and this flat is several hundred feet above the level of the sea.

The valley, containing the river issuing from the Miösen Lake, is several miles broad, between hills of no great elevation; yet it is filled from side to side with a formation, at least topped with pure sand, generally flat, and extending with a slight rise up to the lower extremity of the lake, which it embraces in a beautiful curve, rising in a steep bank about 240 feet above its waters. The outlet of the lake is through a deep trench in this formation. In the neighbourhood of the lake, the sandy plain rises towards the hills on each side, at a gentle inclination, and with a remarkably equable surface, like a sea-beach. What adds not a little to this resemblance is a fringe of gravel at a greater inclination, abutting against the hill-side. Taking the height of the Miösen Lake on the day of observation at 420 feet above the sea, the utmost elevation of this ancient sea-margin above the present sea-level appears to be about 656 feet. In the inner valley or trench, cut by the river, there are minor terraces, at respectively 522, 533, and 598 feet above the sea. A few miles down the valley, at the Trygstad post-station, a great terrace is seen passing for several miles at one level along the hill-side, rendered the more conspicuous by the bright line of green formed by its grassy turf, in contrast with the dark hue of the woods which rise immediately from it to the very summits of the hills. With the spirit-level, I found the line of this terrace to be about the same height with Trygstad station, which is given as 590 Rhenish feet above the sea, in Professor Keilhau's *Goea Norvegica.* As this is an unusually distinct example of the ancient beach, it is very desirable that its elevation were more exactly ascertained. Meanwhile, we may be tolerably satisfied, when we allow for the difference between Rhenish and English measure, that it is nearly, if not quite, identical in height with the terrace just spoken of as 598 feet. It may also be remarked, that the Scandinavian geologists report upon an ancient beach of 597 feet at Lake Oyeren, a lower portion of this group of waters, distant only a few miles from Trygstad. It might be worth while to inquire if any connection can be established between these terraces.

These ancient sea-markings will be the less liable to challenge on the part of my present audience, when I remind them of the conclusion arrived at several years ago by the geologists of Scandinavia, that there are proofs, in terraces and shell-deposits, of that peninsula having been upraised from 600 to 700 feet, at a period immediately preceding the historical era. There are not wanting, however, evidences of a similar nature, that the relative level of sea and land has, in Scandinavia, undergone a much greater change.

The valley of the Rauma, which opens upon the west coast, and that of the Logan, an affluent of the Glommen, which pours itself into the Baltic, meet in a trough of country in the Dovre field, the summit of which is occupied by the *Lässöverks vand*, 2045 feet above the sea. By an extraordinary natural arrangement, the lake emits the Rauma at one end and the Logan at the other; so that a portion of Norway is completely enclosed by natural water. The valley of the Logan, for several miles down, contains great masses of pure sand, in the form of terraces and isolated mounts. On one of the latter, Dovre Church is situated, at an elevation of 1543 feet. In this portion of the valley, there is a terrace unlike the rest, in as far as it is a narrow ledge of detrital matter, running continuously along the hill-side for fully 14 miles, however much more, while the terraces resting on the skirts of the hills lower down are great projecting masses, seldom extending far on one level. This remarkable terrace is most conspicuous on the right or south-west side of the valley. It begins on that side at Oue, between the Hougen and Tofte post-stations. It is there seen truncating the prominent ancient delta of a side stream, called, in Professor Munch's map, the Jondal's Elv, several hundred feet above the bottom of the valley. As we ascend the valley, it becomes nearer to our eye; but this is only because we rise to it, for, when examined with a correct instrument from its own elevation on the opposite side, it is proved to be for a great way truly horizontal. On the left or north-east side of the valley, the corresponding mark is a line composed of slight projecting banks of water-laid sand. Though not continuous, this line is sufficient to have determined that of a long mountain-path connecting a series of farms. Beyond Lie post-station, the road to Molde passes along it, and it here affords positions for a close series of hamlets, which make a conspicuous appearance in the map above cited. I believe it is nearly, if not exactly, of the same elevation with the little *hof*, called

Dombaas, of which the height is given by Prof. Naumann as 2162 (English) feet. In its relation to the lakes in the summit between the two valleys, it precisely resembles the lowest of the Inverness-shire *parallel roads*, as exemplified in Glen Spean, where advancing to the basin of Loch Laggan, between the Spean and Spey valleys. The terrace in every other respect bears a strong resemblance to the Inverness-shire *roads;* while in some important respects, as already noted, it differs from other terraces. I should much desire to see it obtain the attention of local observers, by whom its internal constitution and other features could be more particularly ascertained. Meanwhile, it is not unworthy of remark, that on a neighbouring portion of the plateau of the Dovre field, between two and three thousand feet above the sea, there are peat mosses containing remains of much larger trees than now grow in the district, the vegetation of which does not ascend above a dwarf birch. If the terrace were at one time upon the level of the sea, this plateau would of course enjoy a climate equal to that of districts of a few hundred feet of elevation, and it might then be well able to raise such pieces of timber as now lie ruined in the mosses.

The city of Trondhiem lies at the opening of the valley of the Nid, with high grounds on the east side, and a bold cliffy hill overlooking the sea on the west. Close to the town, and along the valley for several miles, there are terraces of clayey material, none of which persevere for a great way. From indeterminateness of form, and partly of level also, it is impossible to state their elevations with great distinctness; but I may mention, for the sake of general description, that, on sighting them with the telescope-level across the country, they exhibited lines, more or less definite, at about 60, 111, 145, 253, and 435 feet. The most interesting object of the kind is a terrace of erosion, on the face of the cliffy hill to the west of the city. This is an extraordinary and most impressive example. It extends for miles along the face of the hill, at one uniform elevation, which I ascertained with the level and staff to be 522 feet above the sea. Seen from the opposite side of the valley, or from the streets of Trondheim, it appears as a dark band across the hill-face. On near inspection, we find a deep cut into the almost horizontally disposed slate-rocks, with a ledge, flat though rough, at some places as much as twenty paces broad, while overhead rises a cliff more or less bold, formed of the angular edges of the broken strata, with here and there a modern talus descending upon the terrace. Not the least doubt can exist, that it is the effect of the working of the sea, when this part of the hill was on a level with the waves. On the opposite coasts of the Trondheim fiord there are marks of a similar terrace at apparently about the same elevation. The drawing here exhibited presents the appearance of the terrace above the city of Trondheim, at a place where its floor is well defined and flat, and the cliff nearly vertical, and certainly not less than forty feet high. Directing our eyes to the southward, we here see a hill about a mile off, called Sverrosborg, because King Sverro, a distinguished Norwegian monarch of the twelfth century, had a fort upon the top of it. This top is composed of a mass of bare rock about thirty feet high, starting up out of the green-sided hill. The terrace of erosion is marked all round under the mass of bare rock, producing a curious and quaint appearance. The sea has manifestly worn out this terrace at the time when it produced the line of erosion on the neighbouring hill-face, for it is precisely of the same height. Some of the neighbouring grounds come to about the same level, as if produced by a contemporaneous silting-up of these spaces to the surface of the sea.

Connected with this terrace of erosion there are some remarkable alluvial terraces in the interior of the country. A few miles to the south-east of Trondheim, the road leaves the Nid valley, and passes into that of the Gula, a powerful river which discharges itself into a neighbouring branch of the fiord. The country over which the road passes between the two valleys, is a spacious moor, composed of detrital matter, and very flat. Its general elevation is about the same height with the great terrace of erosion. When we advance into the Gula valley near the post-station of Oust, we see a terrace commencing along the east side of the valley, and persevering at one height for a considerable way, being just about the same elevation with the aforesaid moor. This terrace is clearly of the same kind with that in the Logan valley, not a product of fluviatile deposits, as so many terraces are, but of the long-continued washing of the sea against a mountain-side. Descending into the Gula valley, we find vast alluvial deposits, generally of a muddy character, and sometimes terrassiform. Their composition changes as we advance to a fine sand; and this again begins to shew a gravelly admixture, the light materials having, as usual, been carried farther than those of a heavier nature. Near the Meelhuus post-station, which is 123 feet above the sea, there is a sand terrace fully 200 feet high, and from the face of which the material rises in a cloud with every gust of wind. The comparatively low terraces, resting in huge masses on the skirts of the hills, continue for several miles to be very conspicuous, while the higher line on the hill-faces is no longer traceable. At length, between the Leer and Vollan post-stations, and about 25 miles from Trondheim, a highly-remarkable alluvial formation is observed upon the left or west side of the valley. It has a surface perfectly flat, and perhaps an English mile broad, abutting against the hills behind, and in front descending in a steep grassy bank to the river's brink. It extends for miles along the valley, always preserving one elevation, while a terrace of the same height, but less persevering, is seen on the opposite side. The termination in the downward direction of the valley is abrupt, as if the terrace had been broken down at a certain point by the retiring sea; and here there are seen, on the face of the bank below, five several minor terraces, extending only a short way. The accompanying sketch will convey a more lively, though still imperfect, idea of these objects. I am unable to speak with precision of the height of this grand alluvial terrace; but, from my observation of its elevation above the Vollan station, which is set down as 310 feet by Mr Keilhau, I deem it not unlikely that it will prove, on examination, to be coincident with the aforesaid line at Oust, and the terrace of erosion at

Trondhiem. This is the more likely, from what may be observed at Soknaes, the next post-station in the valley. There is here a wide space formed by the junction of a branch valley. The whole space within sight might be described as a *nest* of alluvial terraces, reaching to a considerable height above the two rivers. At one spot, near Soknaes, as many as six are seen rising above each other, the inn being placed on a promontory formed by the fourth of the series. A connection between this group of terraces and that at Vollan is obscurely traceable along the valley. By M. Von Buch, the elevation of the Soknaes station is set down at 487 feet; and hence, I presume, that the sixth or highest terrace at that place may be about the same elevation with the Trondheim line of erosion, or 522 feet. It is desirable that a careful examination of the whole of the Gula markings should be made, and their levels ascertained, in order to ascertain how far they observe uniformity, and if any of them be truly identical in elevation with the Trondheim terrace of erosion, as here surmised from observations which I am sorry to find so much more vague than was to be desired.

On the Sokna, the branch of the Gula here spoken of, there is a similar system of alluvial terraces, on one of which the church of Soknadalen and post-station of Hof are situated. It may be remarked, however, that such terraces, though evidencing a shift of the relative level of sea and land, are not always exact marks of the point at which the sea and land formerly met. Where found sloping in the line of the valley, as is the case with several at Hof, they may be regarded as only the ancient haughs of the river before the withdrawal of the sea (so to speak) allowed it to cut down its alluvial deposit, and seek a lower channel. Where, on the other hand, an alluvial terrace is of the character of that at Vollan, not only broad and flat, but extending a long way upon one level, experience teaches me to expect such relations of measurement as indicate its being the true mark of an ancient line of coast. Among the Soknadalen terraces, I find that I have noted that on which the post-station and church are situated as alone answering the requirements of an ancient sea-level, making the additional remark, that the exact site of Hof is probably a few feet lower, as not being at the highest point of the terrace. By M. Von Buch the elevation of Hof is given as 945 Paris feet above the sea; by another observer it is placed somewhat lower. The mean, given by Professor Keilhau, is 960 Rhenish feet. It appears probable, that the former relative position of the sea is, in this instance, elevated between 990 and 1000 English feet.

I have now to lead attention to the shores of the provinces of Nordlands and Finmark, only previously remarking, that in the intermediate coast there are no terraces of any kind visible from the open sea, there being, in reality, scarcely any detrital formations there, while the rocks are so smoothed by glacial action, as to have afforded little inlet to the erosive power of the waves. It is not till we reach the Island of Hindöe, one of the Lofoden group, that any such markings are presented. In Raft Sund, on the south-west side of that island, about latitude 68° 20′, two faint terraces of erosion are traceable. They are also seen on both sides of the strait between the island and the mainland. At Trondinaes, the northern point of the island, where there is a recess of comparatively soft ground in the iron-bound coast, these two lines are more conspicuous, forming indentations in the grassy slopes; while, in the rocky cliffs, they appear as strongly-marked terraces of erosion. A rough little island, called Magöe, at this place, is cinctured with these terraces of erosion, exactly like the hill of Sverrosborg, but in a more marked manner, for here the waves of the ancient sea have had to deal with strata of unequal hardness; therefore, some masses are left starting up in sharp ridges and rude columns above the general floor of the terrace, which is nevertheless sufficiently well-defined. In all circumstances, the two lines seem to preserve their respective heights undeviatingly, the one being apparently about 50 feet high, and the second 100 feet higher.

In an inlet of the Island of Anderiöe, a few miles from Hindöe, I observed three terraces at a place called Ibbestad, all apparently under 100 feet, and therefore, presumably, a different system from those hitherto noticed.

Farther north, after passing the great inlet of Balsfiord, we find in Trom Sund, the two former terraces resumed. I have been informed, however, by a gentleman of Tromsöe, who has given some attention to the terraces of the district, that there are several distinctly traceable in Balsfiord, one of them at the height of perhaps 400 feet above the sea.

In Trom Sund, the faces of the hills are soft and green, and the two terraces appear as slight, but distinct indentations in the grassy slope, never failing to preserve to all appearance, one relation of levels. It is remarkable, that, while clear and conspicuous on both sides of this narrow sound, they are scarcely to be traced on either side of the interjected island of Tromsöe. On the north-west side of the sound, behind the island, there is a faint appearance of a third terrace upwards of 100 feet above the second. Of the two distinct terraces I took a measurement with the level and staff, and found them to be respectively 57 and 143 feet above the highest tide-mark. Another observer, M. Siljestrom, has given measurements of them slightly different, namely, 56 and 149 feet, and has added the elevation of a third, which I did not succeed in seeing, at 220 feet.

In the range of sounds through which the post-steamer passes, in the sixty-ninth degree of latitude, I observed the two lines well marked on the green skirts of the hills, with scarcely any interruption. The continuity from island to island is very remarkable. Sometimes there is an escarpment or a line of exposed rock, to render the ancient sea-mark the more distinguishable. All along there seems to be not the slightest departure from one set of levels. In recesses, where perhaps little rills have brought down some detritus, not only these two terraces are marked, but several intermediate ones besides. At a promontory of soft matter, called Skätoren, forming the eastern extremity of the island of Ringvatsöe, there are at least four terraces below the higher of the two already so often alluded to, besides some minute and less distinct markings. On the island of Vorteröe, at the south point, which consists of a narrow lofty rock, there is an object of an instructive character, namely, a series of terraces on one side, with a series of greater elevation on the other, shewing how it may depend on local circumstances, as currents and perhaps prevailing winds, that any such impressions are to be made upon a coast, while the relative level is in the course of being changed.

The farthest north point to which I traced the remarkable couple of terraces is Mour Sund, fully ten hours' sail north of Tromsöe. The mountains then begin to be rough with debris, so as perhaps to have presented an unsuitable surface for such markings. M. Keilhau appears to have found the lower of the two still farther along to the north-east, namely, in Langfiord, one of the branches of the Altenfiord. The terrace which he observed in that place is set down by him at 52½ feet above the sea. The gentleman who told me of the terraces in Balsfiord, assured me of there being similar objects in Lyngenfiord, a great inlet which receives several considerable streams. A careful examination of these recesses would probably afford a rich harvest of results, and help materially to solve the problems connected with this subject.

We now approach a portion of the coast, presenting a group of terraces which has already attained some celebrity. The district in question may be said to extend from the Altenfiord, with its branch Kaafiord, into the strait called Varg Sund, which is formed by the mainland on the one side, and the Island of Seiland on the other; being afterwards prolonged into the two sounds surrounding the Island of Qualöe. Altogether it is a range of estuaries and straits extending about fifty miles in a direction generally north and south, and mostly comprised in the 70th degree of latitude. This portion of the Norwegian coast was examined in 1839, by M. Bravais of the French Scientific Expedition of the North; and the facts pointed out by him were briefly these:—

At the mouth of the river Alten, there is a terrace of sand, 223 feet above the sea, and this extends up the river, always on the same level, till, at a village five or six leagues in the interior, it is only about 91 feet above the general level of the district. At the mouth of the smaller stream, commonly called the Kaafiord Elv, a few miles from the mouth of the Alten, there is a similar sandy terrace at the same elevation above the sea, besides a narrow shelf, "like the towing-path of a canal," about 90 feet above the sea.

Proceeding in a northerly direction, M. Bravais found at Krognaes and Talvig an alluvial terrace at 185·5, and another whose mean height was 80·5; and these he considered as representing the two others, though at lower levels.

Advancing in the same direction, he found at Komagfiord two lines of erosion on the faces of the mountains, respectively 169·6 and 67·3 feet above the sea.

Still further advancing, he found the same two lines on the precipices of Quaenklubb, at 162·3 and 60 feet; at a place on the islands of Seiland and Qualöe at 139·3 and 54 feet. Finally, at Hammerfest, the two lines appeared at respectively 92·3 and 46 feet. Thus it appeared that there was a constant decline in the elevation of these markings from south to north. M. Bravais likewise observed some less distinct tracings of the same kind,—a dark-coloured band on Kongshavensfield, near Bossikop, at 128 feet; and one further on, upon the same side of the fiord, at Sortbierg, at 81 feet; a line of erosion between Storvignaes and Krognaes, at 126 feet, and one at Talvig of 141 feet. He regarded these as indications of an intermediate line which had failed to be expressed throughout the intermediate space, but which reappeared at Hammerfest in a terrace of about 69 feet.

It may be remarked that M. Bravais used a barometer for his measurements, adopting usually the mean of several observations, and that he took as his basis a point $0^{m}{\cdot}6$ above the line formed by the sea-weed on the rocks, having found that line 64 centimètres above mean height of the sea.

M. Bravais inferred that there had been at least two distinct angular movements of elevation in the region comprehended by the terraces, the first being measurable by the difference of heights between the upper and lower terrace, and the second by the height of the lower terrace at various points above the sea. The following table indicates the amount of the first elevation:—

Points of Observation,	1.	2.	3.	4.	5.	6.
Upper Line, .	223	185·5	169·6	162·3	139·3	92·3
Lower Line, .	91	80·5	67·3	60·	54·	46·
Amount of 1st Elevation,	132	105·0	102·3	102·3	85·3	46·3

He remarked, however, that the intermediate terrace considerably alters the relative amount of the earlier and later elevations along the coast, reducing the first or most ancient to about 24 feet, making the second or intermediate movement of 22 feet, and leaving the third or most recent of 46 feet, and therefore considerably greater than the other two.

In a work published by me on *Ancient Sea-Margins*, I expressed some doubts as to the alleged inclination of the Finmark terraces, being partly led thereto by the discovery, in other parts of the earth, of uniform levels for such markings; while it likewise appeared to me that, for perfect proof of M. Bravais's positions, we should have required either evidence of greater continuity in the appearances, or measurements taken at a greater number of points. I deemed it not unlikely that the fragments of terrace which he saw at different places might be, not representative of two great lines, as he supposed, but representative of a number of lines nearly if not quite equal to the number of the points of observation. I was therefore glad when I was able to pay a visit to Finmark, with a view to making a rigid personal examination of the terraces, and with the means of measuring their elevations more accurately than had yet been done. The result I am now to bring before this Society.

The general fact of the existence of two continuous lines of erosion on the rocky coast between Kortsfiord and Hammerfest I found to be true. They appear as part of the same system of terraces with those seen farther south, or as a prolongation of them; but, unlike those terraces, they do not observe a level, for the upper line is at one end not much less than a hundred feet higher than it is at the other. It is about the middle that they bear the best resemblance to the couple of terraces in Trom Sund and elsewhere. The resemblance is chiefly in likeness of elevation. As to the material, there is a difference throughout a great part of the space; for while the terraces towards the south are chiefly indentations in soft matter, those of Varg Sund are chiefly sections made in the rocky cliffs,—true terraces of erosion. In these northern terraces, however, the lines are in some places continued from a rocky hill-face to a soft grassy slope, changing from a cut in the rocks to a mere impression on the detrital surface, without any change in their direction or inclination. The appearance of two such markings along about twenty-five miles of coast is calculated to arrest the attention of every intelligent stranger; and even the natives, who are chiefly Laplanders, have not failed to remark them, and to attribute them to the agency of the sea, though not doubting that the sea has simply retired from two several heights at which it had formerly stood. No one on the spot seems to have ever thought it worth while to inquire whether they observe one level, or at what height they stand in any place above the sea.

When I speak of the two lines as distinct, I mean that they are so strongly marked as to be visible from a considerable distance. For example, a person standing on one side of Varg Sund, though it is fully three miles broad, can, in tolerably clear weather, easily see the lines passing along the opposite coast. (See Plate III., *Upper View.*) When narrowly inspected, those designated as terraces of erosion are found to be produced by a true mechanical incision of the rock, sometimes leaving a ledge with a precipitous cliff overhanging it, sometimes consisting only of a rough groove across the mountain-side, without any distinct ledge being left,—sometimes, again, under a form intermediate to these two in all imaginable degrees. Very frequently, when wishing to examine a portion of the line, though it may at a little distance have appeared sufficiently palpable, it is found on near inspection to be obscure and indeterminate, the eye in that case not taking in at once a sufficient amount of the line to produce a distinct impression, and the immediate objects appearing rough and confused. In other places a well-defined section is offered for inspection. For example, at one part of the prominent rock called Quaenklubb (see above view), the upper line is a terrace thirty paces broad, a flooring flat and smooth, produced by a power which has been sufficiently strong to cut sharply through the hard slaty strata, and leave scarcely any inequalities. At the same place, in strong contrast with this marking, the lower line shews, on near inspection, only a shattering of the cliff, and a wearing of it out in vertical hollows; so that it would be impossible to say, within eight or ten feet, what is the height of that line. At a place on the south side of Qualöe island, I found appearances respecting which the following entry was made in my note-book:—"Nothing could well be more perfect than the *ledge* formed by this terrace, there being only such irregularities as were unavoidable from the various hardness of the strata; some having been so very hard as to leave a slight ridge above the line, while, in other instances, a mass was left like a gross short column standing up as a monument of what it had been originally connected with. One of these surviving masses looked much like the ruins of some old castle." At a place on the mainland, opposite to the above, I found a similar ledge, but with an irregular row of short columnar masses in front, somewhat like the obelisks designed to support a chain along the skirts of an artificial terrace, while in a vacant space arose a rough rock, round the top of which the sea had cut a circular flat, causing the upper prominence to appear like a human head rising above a broad pair of shoulders. (The picture here exhibited of the line of erosion at Trondheim gives a good idea of the section of a terrace which has a flat, well-defined floor and cliff rising above, as well as of an isolated mass cinctured by a terrace, like the object last spoken of.)

It may be remarked that, according to M. Bravais, probably reporting the observations of Professor Keilhau, "the mountains of Altenfiord and of all this part of the coast belong to the group of metamorphic rocks; but the nature of the rock differs widely, since calcareous beds are found at Storvignaes, and at Talvig, between Kortsnaes and Skillifiord, amphibolic rocks on the island of Seiland and near Hammerfest; while diallage, quartzose sandstones, and argillaceous schists, are not rare." M. Bravais adds, that he was not able to judge of the influence of these rocks on the different phenomena of the neighbourhood. I may further remark that the only general feature that seems likely to have told in making the mountains along the sea susceptible of such impressions is the fact of its being an inland sea. The direction of the various sounds and estuaries can scarcely be supposed influential, as, in fact, the markings are made upon coasts in a great variety of directions.

With the assistance of Mr Paddison, civil engineer, who was so kind as to associate himself with me in my examination of these terraces, I executed a series of accurate levellings at about eighteen points along the space between Hammerfest and Kortsfiord, and completely convinced myself of the reality of the inclination, which I conceived M. Bravais to have left in some doubt.

At Hammerfest, M. Bravais adopted as his point of observation an almost horizontal bank surrounding the small lake behind the town, together with a terrace, probably of transported matter, near by. On examining these objects I was at first of opinion that they represented the upper line, which was farther continued as a true line of erosion along the cliffs. I afterwards became convinced that they belong to a different system of markings, inconsistent in level with the true line of erosion as it exists at this place. That line is 84·73 feet above the highest tide-mark of the neighbouring shore, a point probably about six feet above the mean level of the sea.

(In what follows, I am to be understood as using, as a base in levelling operations, the same mark, as far as it could be ascertained, and as indicating distances in geographical miles.)

On Hoiöe or Hoy, an island much resembling the Bass in shape, four-and-a-half miles, a little to the south of west from Hammerfest, the same upper line is strongly marked at 85·29 feet, being nearly the same as the last-mentioned elevation.

About a mile to the west of Hammerfest, the upper line is strongly but roughly marked on the cliffy coast; and here I found the elevation to be 87·84. A little farther along the same line of coast, and a mile and a half from Hammerfest, the elevation, at a very distinctly-marked place, was 89·49 feet, indicating a decided rise in this direction. Within Rypfiord, a bay a very little way onward, at a place one mile and a half of direct distance from Hammerfest, the elevation is 91·58, being nearly a rise of seven feet from the first point of observation.

At Saragamma, another place in Rypfiord, 2¾ miles from Hammerfest, the elevation is 96·69 feet, thus evidently observing a certain proportion of rise in this direction.

Passing round a promontory, along which, on both sides, the line is well marked, we found in Akkerfiord a broad alluvial marking in a green recess beside the discharge of a small river. This spot is 3⅓ miles of direct distance from Hammerfest. The elevation of the terrace is 104·69 feet.

Hitherto no tolerably distinct trace of the lower line has appeared; but at length at Molstrand, about a mile onward from the last place, it becomes visible, as a rough impression on the precipitous coast, while a distinct terrace of blocks runs along for a short way at a still lower level. The elevation of the upper or grand line, as it may be called, here appeared at 106·11. The point which I assumed as that of the lower line is 43·75 feet, and the terrace of blocks 23·65.

From this point the upper line continues to be well marked, along the whole of the west coast of Qualoë, though for several miles mostly as an indentation in the soft face of the hill, there being only a few rough places where it appears as a terrace of erosion.

At Indre Sioholmen there is a green recess watered by a rill; and here, as might be expected, are some alluvial deposits. The lower line here appears as a green terrace at 44·97 feet; and the upper one in the same character at 114·32. There are two terraces in the alluvial formation at intermediate heights, namely about 97 and 106 feet.

The upper grand line is clearly traceable along the coast of Seiland, from a point opposite to Hoiöe; but I made no measurements there till we reached a place called Quisnaes, nearly opposite to Indre Sioholmen. Here the line is deeply impressed in the cliffs, at 106·87, being very nearly the same as at Molstrand, but decidedly below the terrace at the more directly opposite place of Indre Sioholmen. I made two rough measurements with the mirror level, at parts of the coast of Qualöe further to the south, and found unquestionable proof of a continuous rise for the upper line, which is there very clearly marked, especially on the south coast.

Directly opposite to the southern angle of Qualöe, divided from it only by a narrow strait, is a promontory of the mainland called Beritsmol. The upper line is here presented as a broad flat terrace of rock, encumbered with blocks, but not so much as to prevent its being selected as a road by the reindeer, in their passage from one pasture to another. I took two measurements here, within half a mile of each other, and found a slight difference, the one being 129·22, and the other 130·66. These two points are not in the same line as the points of observation along Qualöe island; they cross that line of rise at a considerable angle. Turning a promontory to the eastward, we enter a small branch of the sea terminating in a valley, in which are situated the Kiopman's house of Qualsund, a chapel, and some lonely farms. The two lines here become broad green terraces, the upper of which is seen on both sides running for miles along, till it terminates in a morass near the head of the valley. This grand terrace, measured a mile along the valley, is 137 feet, shewing still a rise in the southerly direction. There is, however, something anomalous in the measurement of the lower line, for, while it appears at 53 feet as a line of erosion in the sound, it becomes a detrital terrace of only 44 feet on turning into the valley. Such at least is its elevation at the Kiopman's house, which is situated upon it.

The two terraces are well marked along the sounds skirting the east side of Qualöe, as far as the eye can see from Qualsund. At a place close to the entrance of Reppefiord, there is a recess or *sinus* in the line of the mountainous coast, where a little rill enters. The neighbouring hill-faces are covered with long irregular ridges of detrital matter, probably the relics of ancient moraines. In consequence, apparently, of this abundance of transported matter, the rivulet has formed a large delta, which projects like an immense spoil-bank from the hill-face into the sea. The upper terrace is continued across this formation, as a broad flat plain of several acres in extent, and slightly ridged in front; shewing that the formation of the line has been an event subsequent to the subaqueous discharge and formation of the delta. It may be remarked that, 25 feet above this plain, is another of smaller extent, bearing curious curvilinear ridges in a direction from front to rear.

I continued my measurements along Varg Sund on both sides, and found a constant rise, though manifestly in a less rapid ratio. At a place a little east of Nœverfiord, the upper line passes distinctly across the almost perpendicular cliffs at 143 feet. At Rabastynaes, nearly opposite, I found, with the mirror level, the same line broadly marked at 144 feet, with a block of a different rock as large as a good-sized house reposing upon it. The great bold promontory called Quaenklubb (Plate III., *Upper View*) exhibits the lower line as a rough horizontal breach of the cliff, but the upper as a flat rocky floor of fifty paces broad, formed by a section of the almost vertical slaty strata, and 154 feet above the sea. In the adjacent green recess, the two lines form distinct indentations across the soft ground, the lower being here 57 feet. The face of Quaenklubb exhibits platforms at greater heights, namely, 176, 216, 302, and 318 feet, none of them of any great extent, and therefore dubious as indications of an ancient working of the sea, but remarkable for the burden which they bear of gneiss blocks and gravel, and other transported materials. On the platform at 302 feet, there is a block of gneiss, perfectly unworn, and measuring fully ten feet each way.

At Olderfiord in Seiland, a little farther along the sound, the upper line appears at 154 feet, besides two terraces in a green recess at 56 and 64 feet. At a point in the mainland opposite Storbeckarfiord, the lower line of erosion appeared at 64 feet, and the upper at 161. The latter is here a very rude flat, where not three yards are free of irregularities, produced by ridges of the strata, or loose blocks,—the cliff rising above it not less irregular,—the whole rendered still more rough by masses of moss, stumps of trees, and the living vegetation, from the wild flower to the birch; yet is the terrace nevertheless so far definite, that it has been adopted at the line of a path marked by the feet of men and wild animals. The range of vertical space involved by the terrace floor and the cliff is from 20 to 25 feet.

The next station of observation was on the south side of the entrance to Leerisfiord on the mainland. The upper terrace is there about 170 feet. It now becomes comparatively obscure, while the lower entirely escapes observation. In the next recess, called Komagfiord, the latter forms a green mound or terrace of fertile meadow-ground all the way round at from 57 to 64 feet. The upper line is very faint, and it is difficult to say in what manner it is produced. It was not till after many examinations and sightings of the one side of the valley from the other, that I determined its elevation at 179 feet. M. Bravais, who seems to have been under the same difficulty with it, states its elevation at a few feet less.

It will have been observed, that the rise of at least the upper terrace continues without any interruption throughout the whole of the twenty-five miles within which it is so distinct. From 85 feet at Hammerfest and Hoïöe, it has become 179, or 94 feet higher, at Komagfiord. The lower line, though more obscure, and less liable to exact measurement, also manifestly rises within a more limited space, namely, from about 44 at Molstrand to 64 at Komagfiord.

That the markings were made by the sea is, I presume, admitted on all hands. That the land involved in the case has made two angular movements, first, one subsequent to the time when the higher shelf was formed, and then another subsequent to the time when the lower line was impressed, seems also beyond question. These two positions are laid down by M. Bravais, and I believe they are not to be shaken. There remain, however, some interesting points of inquiry, as whether the movement has been regular, in what direction it has been made, and where was the axis of rest?

That the movement has been regular does, I think, appear from the measurements now presented; but this point can only be fully settled in connection with the next question, which regards the direction in which the movement has taken place. It will be observed, that the series of elevations from

Hammerfest to Beritsmol—that is, in a line south-south-east —is tolerably equable in proportion to the space passed over; but when we turn off into Varg Sund, in a line south-south-west, the rise is much slower. To give particulars: Between Hammerfest and Beritsmol, a space of 11½ geographical miles, the rise is 46 feet; but from Beritsmol to Saraby, 9¼ miles, it is only 24 feet; while from this to Komagfiord, 9 miles more, it is 25 feet. If, however, we form a line nearly coincident with that of the first line of observation, as in the accompanying map, and, prolonging this into the mainland, raise upon it vertical lines touching the various positions in Varg Sund, we shall find that *equal spaces are passed through in this portion of the affected district within equal spaces of the rise.* It therefore appears that the rise is not slower in Varg Sund than in the sound between Qualöe and Seiland, but only the terrace is there not so coincident with the line of rise. The rise observably becomes accelerated as we pass on to Komagfiord, because the trend of the coast in that quarter gets more into a conformity with the line of rise. The heights taken on opposite coasts, moreover, as at Molstrand and Quisnaes, near Nœverfiord and at Rabastynaes, at Saraby and Olderfiord, correspond pretty well with this theory of the direction of the movement. In short, if a fair allowance be made for inaccuracy of maps, and the indeterminateness of the base line of the measurement (for a high water mark is in some places difficult to hit), it will appear that there is a remarkable approach to equability of rise throughout the whole of the space between Hammerfest and Komagfiord, the rate being pretty uniformly about 4 feet in a geographical mile.

M. Bravais regarded the possibility of the terraces having either risen at the one end or fallen at the other, so as to produce the slope; but he had not probably observed the two terraces which run along the coast to the southward, with little interruptions, for a space of about 180 miles, observing to all appearance, one uniform relation of heights, at 55 and 143 feet. I consider the two sloping terraces of the northern sounds, as a disturbed portion of the system represented by the two southern terraces. If this be a just view, the disturbed land has moved on an axis of rest, rising at the one end and falling at the other. It has been a see-saw movement, the centre or point of which must be looked for at the place where the two inclined terraces are at the normal height of the system of which they presumably form a part. Keeping in view the upper one only, this place is in the line near the point of observation at Nœverfiord (see Map, Plate IV.), for the upper terrace is there 143 feet high, being the elevation of the upper line as measured at Tromsöe. From that place, the terrace falls 58 feet to the transverse line at Hoiöe and Hammerfest. How much farther it may have fallen in that direction, has not been ascertained. From the same central point it rises proportionately in the opposite direction.

Does the rise continue farther to the southward than the line of Komagfiord? I was at first of opinion, that it stopped here, instead of going on, as supposed by M. Bravais, to the embouchures of the Alten and Kaafiord rivers, because I found in Kortsfiord, the next opening to the southward of Komagfiord, what I believed to be the upper line, at nine or ten feet below its Komagfiord level. Hence it appeared to me as if that line had here attained a culmination point, and was beginning to descend towards the south. I have since, however, discovered the remarkable fact, that if the line of movement be prolonged till it comes abreast of the mouth of the Alten, the perpendicular line then raised upon it will pass through the whole range of terraces between that point and the mouth of the Kaafiord river, which are all of them about 220 feet high in front, but rise inland to an entire height of 239 feet; and this elevation will be just about what might be expected of the upper line prolonged to that point. (See Plate III., *Lower View, representing the alluvial terrace of Quaenvig, a good example of its kind.*) The uniformity of the elevation of these terraces over a range of 10 miles is in itself remarkable; their being traversed by the perpendicular line from the line of movement at the point where we might expect the elevation of the terraces to be attained (see Map, Plate IV.), is very striking. Finally, on considering the somewhat extraordinary character of these alluvial formations, I became inclined rather to regard them as belonging to a disturbed than to a steady district. The objection from the terrace at a lower level in Kortsfiord may be allowed to give way, for there are so many anomalous markings in that valley, and in Komagfiord (to be presently adverted to), that a mistake may probably have happened. What adds in some degree to the probability of the Alten terraces being part of the district of disturbance, there are similar sandy formations connected with other rivers at Melsvig and Talvig; and these, being five or six miles less advanced upon the presumed line of movement, are proportionately lower. If we assume that the district of disturbance extends thus far, the vertical movement connected with that disturbance at Alten must be regarded as equal to the difference between 143 and 239, or 96 feet. Does the movement extend farther southward? We should have to answer in the negative if M. Bravais is right in saying that the great Alten terrace goes for four or five leagues along the valley without change of height; but this point may be worthy of future inquiry.

M. Bravais indicated the existence of a faint intermediate line, which he observed both at Hammerfest and Alten, and which he conceived to imply an intermediate movement. But the fact is, there are, at many stations throughout the disturbed district, terraces over and above, though mixed up with, the noted two which are here discussed. At Hammerfest, besides the intermediate line alluded to by M. Bravais, which is formed by a grand terrace of blocks fallen from the schistous mountain behind the town, there is, on a hill-face near by, a series of shingle terraces, scantily covered with vegetation, and precisely resembling shingle beaches of the present day, at 87, 123, and 144 feet. There is also a ring terrace of transported matter, topped with water-laid sand, round the Lake of Hammerfest, at 97 feet above the sea. Here, it will be observed, are traces of the sea at not less than three elevations superior to the higher of the two lines. In Rypfiord and at Indre Sioholmen there are also terraces above the higher line. Such is likewise the case at Qualsund. In Komagfiord, at the entrance of its little river, there is a series of terraces both below and above the lower line, besides a broad flat terrace of soft materials on the hill-face at 161 feet, being 18 below the upper line. In Kortsfiord there are similar objects, particularly the terrace already alluded to at 169 feet, and one connected with a mountain streamlet at 241. The distinctest markings of this kind, however, are seen on the faces of the Alten and Kaafiord formations. At Bossikop, Quaenvig (see Plate III., *Lower View*), and Kaafiord, there is one of marked importance at between 80 and 90 feet, being probably that which M. Bravais set down as the lower line in that district. It appears as a cincture round the singular sandy promontory of Oskarnaes, at a few feet lower; which is what might be expected, as that is a point some way advanced on the assumed line of dip. There are, besides, however, at Quaenvig and Kaafiord, markings equally or even more decided, at 52, 123, 144, and 167 feet, indicating no fewer than three movements between the dates of the upper and lower lines, and at least one subsequent to that of the lower. To establish connections among these markings, would obviously require no small amount of additional observation.

The general fact may now be considered as tolerably certain, that there is a district in Finmark, of 40 geographical miles in extent, which has sunk 58 feet at one extremity, and risen 96 at the other. Its line of dip and of rise is pretty well ascertained. It is not greatly different from that of the magnetic meridian for the district, which is about 11° west of north. The movement has been surprisingly equable over relative proportions of the space. A shift of the relative level, after the manner of the Alten and Hammerfest terraces, is, however, exceptional, for there is a much larger district to the south, which has evidently been involved in this process of shift at the same time, but where that shift has taken place without being attended with a change of the plane originally observed by the land. In the central and southern districts of Norway, there are other ancient sea-markings, which appear to preserve horizontality, and even awaken the surmise, that they coincide with similar levels in other countries.

There is, however, a large tract in the south and east of

Scandinavia which is ascertained to be undergoing an elevatory movement, even at the present day. I was able to visit the celebrated stone at Löfsgrund, near Gefle, on the Gulf of Bothnia, which has been marked with the height of the water at various periods; lastly, by Sir Charles Lyell in 1834. I found his mark 2 feet 7 inches below that of 1731; and the sea, on the day of my visit (2d September), was about 6 inches below Sir Charles's mark, or rather more than 3 feet below that made 118 years before. It occurred to me as unfortunate to have selected for this kind of test a loose block lying near the shore; for we cannot exclude the suspicion, that the ice may carry it a little way up the beach every winter. Nevertheless, as there are other marks presenting similar results, where no such liability to fallacy exists, the probability is, that no movement has actually taken place. Two days afterwards, I visited the mark made by Flumen upon the cliff of Grasöe, near Oregrund, in 1820. The sea was so calm as not to wet more than an inch of the cliff above its ordinary level of the day; and the seamen informed me, that the water was at a very fair average for the season. I found the surface of the water 11 inches below Flumen's mark, which had been made only nine days later in the year. Thus, if the sea at the two periods was in similar circumstances, or at its mean level for the season, there appeared to have been a rise of the land in this district to the extent of 11 inches in twenty-eight years.

A Conjecture as to the Forces which produce the Tails of Comets. By WILLIAM JOHN MACQUORN RANKINE, Esq., C.E. Communicated by the Author.

The immense velocity with which the tails of comets, on the approach of those bodies to their perihelia, are projected in a direction opposite to that of the sun, has always been held to indicate that the particles of those nebulous envelopes are acted upon by some powerful force directed from the sun. Various hypotheses have been proposed as to the nature and origin of this force; but they are all more or less inconsistent with known facts.

One mode of accounting for the existence of such a force has, so far as I am aware, been hitherto overlooked.

The nebulous substance of a comet appears to consist of matter in the state of smoke or mist: that is to say, of minute solid or liquid visible particles, suspended in invisible vapour. If we suppose that, on the approach of the comet to its perihelion, each of these visible particles is partially volatilized by the sun's heat, it follows that it will emit invisible vapour, chiefly on the side *next the sun*, and that, by the reaction of the vapour so emitted, the portion of the particle which remains in the visible state will be propelled *away from the sun*, with a force depending on the rapidity of the evaporation. This force will be greatest in the superficial portion of the nebulous envelope, the internal parts being more or less protected from the sun's rays. The particles thus propelled, after describing an orbit more or less elongated, will return towards the nucleus under the action of gravity.

Such will be the ordinary action of the solar heat, according to the supposition now proposed, as displayed in the usual form and position of the tails of comets; but it may be modified by other forces, such as the action of jets of vapour issuing from the nucleus, so as to give rise to those forked and oblique tails which have occasionally been observed.

I do not propose this conjecture as a theory sufficient to account for all the phenomena of the tails of comets, but merely as a speculation, somewhat less visionary, and more in accordance with the known properties of matter, than those which have hitherto appeared on the same subject.

EDINBURGH, *September* 1849.

On Volcanic and Metalliferous Eruptions. By M. Elie de Beaumont. (With a Table.)

Volcanic eruptions bring to the surface of the globe, on the one hand, rocks in a state of fusion, lavas, and all their accompaniments ; these are *volcanic products after the manner of lavas ;* and on the other, substances volatilized or carried along in their molecular state, such as steam, gas, salts, &c. ; these are *volcanic products after the manner of sulphur.* On going backwards in the course of geological periods, we observe that volcanic substances after the manner of lavas become more and more rich in silica ; and that volcanic substances after the manner of sulphur become more and more varied. The latter are the produce of the humid way, in the same manner as the products of thermal springs are those of heat. The greater part of metallic veins appear referable to this.

The following is a brief summary which M. E. de Beaumont gives of his memoir. The numbers refer to those in the Table at the end of the article.

1. Bodies most generally spread over the surface of the globe. These are sixteen in number.* We may add titanium, bromium, iodium, selenium, which are generally diffused in small quantities, which would raise the number of these bodies to twenty ; but of these not above twelve are found frequently and in abundance.

2. Fourteen simple bodies, which enter into the composition of various species of lavas produced by existing volcanoes. Although sulphur is found in sulphuric acid, hydrogen in the water of haüyne, chlore in sodalite, fluor in mica, yet these four bodies occur in lavas only in an exceptional way, and the number ought, therefore, to be reduced to ten.

3. Fifteen simple bodies, which compose the ancient volcanic rocks.

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* See Researches on the Theoretic Portion of Geology. By Sir Henry de la Beche.

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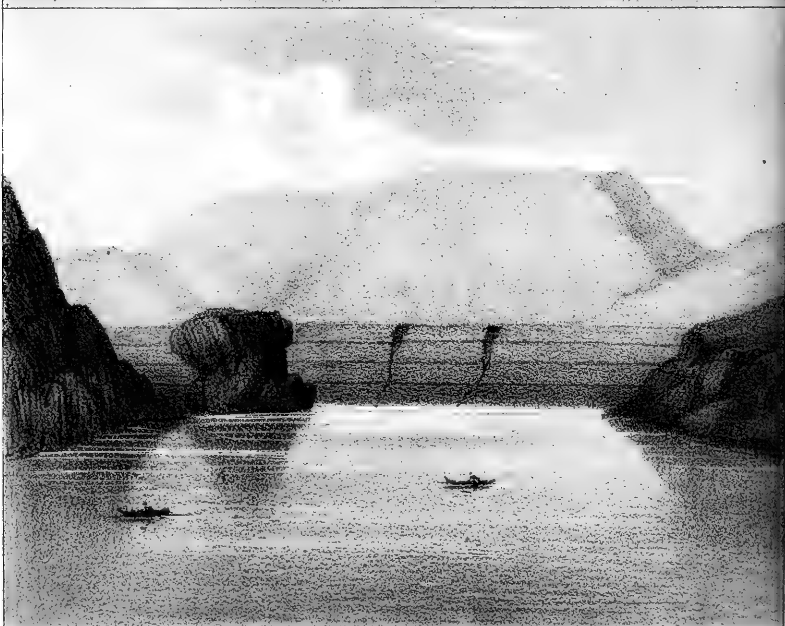

R. Chambers del^t Fr. Schenck, Lith^r Edin^r

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4. Simple bodies which enter into the composition of the *basic* rocks, or those whose mode of eruption has differed from that of volcanic rocks, especially in the rarity of scoriæ; such are the serpentines, traps, &c.

5. Simple bodies, composing granitic or acidiferous rocks; that is to say, rocks in which the bases are saturated with silica, and in which the silica is in excess; such are quartziferous porphyry, diorite, syenite, protogine, granite, pegmatite.

6. Simple bodies, which enter into the composition of stanniferous veins, or veins of substances which accompany tin.

7. Simple bodies of ordinary or plumbiferous veins and others, to which have been added the bodies which enter into the composition of the crystallised masses contained in the *geodes* of amygdaloids, in the fissures of septaria, &c.

8. The elements met with in mineral waters. It is to be remarked that this list is, so to speak, only an extract of the list of bodies which are found in ordinary veins.

9. Simple bodies found in the emanations of existing volcanoes. This is a synopsis of such as are found in mineral springs; yet cobalt, lead, and selenium, which are found here in very inconsiderable quantities, are wanting in the list of simple bodies occurring in mineral waters. By comparing columns 2 and 9, with columns 5 and 6, we infer that the foci of active volcanoes are the poorest in simple bodies of those which have acted at the earth's surface. A great part of the simple bodies have been set apart in the first geological phenomena, so that they do not reappear elsewhere; there was among simple bodies a gradual selection, constituting a great phenomenon, which was going on during the whole time the earth's crust was forming, but the effects of it have varied in proportion as the earth's crust became thicker.

10. Simple bodies found in a native state on the surface of the globe. Some of them (palladium, rhodium, ruthenium, iridium, platina), do not form lasting combinations but among themselves; they appear to constitute a department by themselves in the midst of the mineralogical world.

11. Bodies found in ærolites, to the number of twenty-one.

All of them are bodies already known at the surface of the globe, and fifteen of them are included in the list of the sixteen simple bodies which are most widely diffused.

12. Bodies which enter most generally into the composition of organised bodies; they are the same as those occupying the first column in the table. "This identity shews," says the author, "that the surface of the globe contains in almost all its parts everything essential to the existence of organised beings; it furnishes a new and striking example of the harmony which exists in all the departments of nature. The sixteen simple bodies in question being all found either in volcanic productions or in mineral waters, we perceive that Nature has provided not only for the establishment but the maintenance of this indispensable harmony. The globe, as it becomes older, will never fail to furnish organised beings with all the elements necessary for their existence."

In the remaining portion of his memoir, the author presents a series of interesting and ingenious reflexions, derived from examining the distribution of simple bodies in the different columns of his table. The following struck us most. The position of a great number of veins which is the same as that of the mineral waters, ought to convince us that these veins are nothing else than deposits produced by mineral waters in the fissures which they traverse. This theory differs from that of Werner, only in so far that the latter supposed the waters to come from the exterior or surface of the globe. The principal difference between the position of mineral waters and that of veins, consists in this, that the former are co-ordinate with modern eruptive rocks, while the second are co-ordinate with the most ancient eruptive rocks. Veins are rents afterwards filled; but we ought to distinguish two classes of them, *concretionary veins*, composed of *terrigenous* substances (quartz, sulphate of barytes, carbonate of lime), and *metalliferous* substances (galena, pyrites, &c.), arranged in symmetrical bands; and *injected veins*, in which we do not find this latter arrangement (basalt, melaphyre, porphyry).

The author then considers the connection between the emanations of existing volcanoes, mineral waters, and veins, with the object of explaining the mode in which the latter are formed, and of shewing that the substances which compose them are *volcanic, in the manner of sulphur.* Then, passing to the consideration of granite, M. E. de Beaumont proves that the metallic accompaniments of granitic rocks become poorer in proportion as their eruptions are more modern; and thus their modes of eruption and crystallisation become modified, till they are reduced to the existing state. He endeavours to explain the crystallisation of granite, and is of opinion, that a suffusion of quartz, a small quantity of water, certain volatile salts (chlorure of sodium, chlorure of iron, hydrochlorate of ammonia), as well as other substances equally volatile, sulphur, fluor, phosphorus, borium, and electricity, all take a part in this phenomenon. These agents have been produced by phenomena which may be compared to the *rochage* of silver, and the spheroidal state of bodies. But we give here only a very general notion of the author's dissertation; it must be read entire, in order to understand the skilful manner in which the facts are discussed, with the view of throwing some light on this singular phenomenon, which has so long engaged the attention of those who study the origin of rocks. "Although the action of heat has predominated," says the author, "water would appear to have acted a considerable part, so that the formation of granites very probably is connected on the one hand, by the silicates which enter into their composition with that of the lavas; and on the other, by the free silica which abounds in it, with the formation of deposits of silica, which constitute quartzose veins." Yet the conclusion of this learned dissertation is not a very clear explanation of the crystallisation of granite.

"However unsatisfactory this explanation may be," says the author, "we may assert that it is, to a certain point, on a level with the present state of science; for we are prevented from developing it further, only by the imperfection of our present knowledge on the intimate nature of the physical phenomena to which we are led to appeal."*

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* From Biblioth. Universelle de Genève, Aug. 1849, p. 319.

Table of the Distribution of Simple Bodies in Nature.

	1. Bodies distributed most generally over surface of the Globe.	2. Modern Volcanic Rocks.	3. Ancient Volcanic Rocks.	4. Basic Rocks.	5. Granites.	6. Stanniferous Veins.	7. Ordinary Veins and Geodes.	8. Mineral Springs.	9. Volcanic Emanations.	10. Native Radicals.	11. Aërolites.	12. Organic Bodies.
1. Potassium	*	*	*	*	*	*	*	*	*	...	*	*
2. Sodium	*	*	*	*	*	*	*	*	*	...	*	*
3. Lithium	...	...	...	...	*	*	...	*				
4. Barium	...	...	...	...	...	*	*	*				
5. Strontium	...	...	...	...	...	...	*	*				
6. Calcium	*	*	*	*	*	*	*	*	*	...	*	*
7. Magnesium	*	*	*	*	*	*	*	*	...	...	*	*
8. Yttrium	...	...	...	...	*	*						
9. Glucinium	...	...	...	...	*	*	*					
10. Aluminium	*	*	*	*	*	*	*	*	*	...	*	*
11. Zirconium	...	...	...	...	*	*						
12. Thorium	...	...	...	...	*							
13. Cerium	...	...	...	...	*	*						
14. Lanthanum	...	...	...	...	*	*						
15. Didymium	...	...	...	...	*	*						
16. Uranium	...	...	...	...	*	*	*					
17. Manganese	*	*	*	*	*	*	*	*	*	...	*	*
18. Iron	*	*	*	*	*	*	*	*	*	...	*	*
19. Nickel	...	...	...	...	...	*	*	...	...	...	*	
20. Cobalt	...	...	...	*	*	*	*	...	*	...	*	
21. Zinc	...	...	...	*	*	*	*					
22. Cadmium	...	...	...	...	...	*	*					
23. Tin	...	...	...	...	*	*	*					
24. Lead	...	...	...	*	*	*	*	...	*	*	*	
25. Bismuth	...	...	...	*	*	*	*	...	...	*		
26. Copper	...	...	...	*	*	*	*	* ?	*	*	*	
27. Mercury	...	...	...	...	...	...	*	...	...	*		
28. Silver	...	...	...	*	*	*	*	...	...	*		
29. Palladium	...	...	...	*	* ?	*	*	...	...	*		
30. Rhodium	...	...	...	*	...	...	...	...	...	*		
31. Ruthenium	...	...	...	*	...	...	...	...	...	*		
32. Iridium	...	...	...	*	...	...	...	...	...	*		
33. Platinum	...	...	...	*	...	...	*	...	...	*		
34. Osmium	...	...	...	*	...	...	...	...	...	*		
35. Gold	...	...	...	*	*	*	*	...	...	*		
36. Hydrogen	*	*	*	*	*	*	*	*	*	...	*	*
37. Silicium	*	*	*	*	*	*	*	*	*	...	*	*
38. Carbon	*	...	...	...	*	*	*	*	*	*	*	*
39. Boron	...	...	...	...	*	*	*	*	*			
40. Titanium	...	*	*	*	*	*	*					
41. Tantalum	...	...	...	...	*	*						
42. Niobium	...	...	...	...	*	*						
43. Pelopium	...	...	...	...	*	*						
44. Tungsten	...	...	...	...	*	*						
45. Molybdanium	...	...	...	...	*	*	*					
46. Vanadium	...	...	...	...	...	*	*					
47. Chrome	...	...	...	*	*	*	*	...	...	...	*	
48. Tellurium	...	...	...	...	...	*	*	...	...	*		
49. Antimony	...	...	...	...	...	*	*	...	...	*		
50. Arsenic	...	...	...	*	*	*	*	*	*	*		
51. Phosphorus	*	...	*	*	*	*	*	*	...	...	*	*
52. Azote	*	...	...	...	...	...	...	*	*	*	*	*
53. Selenium	...	...	...	...	...	*	*	...	*	*	*	
54. Sulphur	*	*	*	*	*	*	*	*	*	*	*	*
55. Oxygen	*	*	*	*	*	*	*	*	*	*	*	*
56. Iodine	...	...	...	...	...	...	*	*				
57. Bromine	...	...	...	...	...	...	*	*				
58. Chlorine	*	*	*	*	*	*	*	*	*	...	*	*
59. Fluorine	*	*	*	*	*	*	*	*	...	...	...	*
	16	14	15	30	42	48	43	24	19	20	21	16

On the Different States in which Fossil Vegetables are found.

The vegetables which we find in a fossil state are scarcely ever (I believe it may be said that they are never) complete ; they are only portions or fragments of vegetables, stalks, branches, leaves, fruits, or rarely flowers, separated from the other organs of the plant. In this respect, we are in the same condition as in regard to actually existing vegetables, when we receive insulated and incomplete portions of an exotic plant, which we often find great difficulty in determining. But besides this, fossil vegetables, thus reduced to some of their insulated organs, scarcely ever present them in such a state of preservation, as enables us to study them in all their constituent parts. Thus the stalks often retain nothing more than their external form, or, in other cases, nothing more than their internal structure, frequently altered in many respects. The leaves, in many instances, retain nothing but the imperfect net-work of their nervures, and rarely can the epidermis and its details of structure be conveniently studied. As to the fruits, most frequently the external form alone is left to enable us to judge of their affinities, their internal structure being destroyed or greatly altered by compression or by petrifaction.

The different modes in which vegetables are preserved in a fossil state, may, however, be referred to two principal classes.

The impression or cast of the plant, accompanied with the complete destruction of the vegetable tissue, or the preservation of few of its constituent parts ; petrifaction or carbonisation, which preserves, more or less completely, the structure of the tissues of the vegetable organs, by changing completely, or only modifying, their nature.

The impression or cast, in a strict sense, that is, without the preservation of any portion of the organs of the vegetable more or less altered, is rather rare ; yet it is the habitual state of fossil vegetables in the variegated sandstone (*the Gris-bigarré*) and tertiary limestones.

The place once occupied by the vegetable is empty, or the vegetable is replaced only by a substance usually ferruginous, sometimes calcareous or earthy, which presents no organisation, and which, consequently, is not the vegetable petrified.

In this case, we can judge only of the exterior forms of the vegetable, and often the best means of doing it with accuracy is, after carefully removing the amorphous matter which fills the hollow left by the vegetable, to pour into the cavity either wax, sulphur, or any other matter, which will represent exactly the forms of the destroyed vegetable.

The impression with some preserved portions of the vegetable tissue is very frequent with the stems found in the coal-formation. This is their ordinary mode of preservation; and here the exact determination of the different forms of the vegetable requires much attention.

In the greater part of these stalks or stems, the superficial portion, a kind of thick and woody epidermis, has passed into the state of compact and anthraciteous coal; all the rest of the plant has been destroyed and replaced by clay, micaceous sandstone, often even by a coarse sandstone, without any appearance of organisation. Sometimes, however, this destruction of the internal tissues is less complete; the most resistant are preserved and turned into coal; these are the ligneous or vascular parts, the place of which and even the structure is indicated by coal lineaments. This has been long observed in *Stigmaria ficoides*, and M. Corda has often seen the same thing in many stems found in the coal-pits of Bohemia. Sometimes, besides the axis and woody cylinder properly so called, there is an internal cortical zone, then an external bark, which are likewise preserved, while the intermediate cellular tissue is destroyed. These different zones of denser tissue which, separated by large beds of destroyed cellular tissue, envelope one another like so many cylinders inclosed one within another, and are often preserved insulated, have each their special form, and often a different one on their external and internal surface. The same stem may thus produce very different forms, each cylindrical, and resembling so many different stems.

I have long since pointed out this fact in regard to the stems of a Sigillaria, whose stem, deprived of its coal bark, had constituted the genus Syringodendron.

In the *Lomatophloios crassicaule* of M. Corda, the vascular axis forms a cylinder finely striated, which may be taken for the stem of a peculiar genus, and the medullary cylinder which this vascular cylinder surrounds, presents peculiar transverse furrows, which, according to this author, have been made to characterise the genus Artisia. I may add that, in specimens of this stem, or of a closely analogous species from the mines of Saarbruck, I found an intermediate zone between the external surface and vascular axis, which appeared to correspond to the origin of the bases of the leaves, and which affords all the characters of the stem figured by M. de Sternberg, under the name of *Knorria Sellowii.*

In stems with tissues imperfectly preserved, we must therefore carefully distinguish the different zones of tissue in the same stem, and their external and internal surfaces which produce so many different appearances.

What I have said of stems applies equally to fruits, in which the thickness of the pericarp often gives rise to two very different forms, and in which the cavities, in other instances, are not real cavities, but, on the contrary, spaces occupied by a different destroyed tissue, and even sometimes by all the solid parts.

Carbonised vegetables, or such as have passed into the state of lignites, give rise to fewer observations ; yet it must be remarked that, in this alteration, their tissues have often undergone modifications which render it difficult to understand them rightly. Lastly, it not unfrequently happens, that a portion of the organs of vegetables passed into the state of lignites, is transformed into pyrites, or else pyrites of a globular shape are formed in the middle of this tissue, and may, at first sight, be taken for a character of organisation. The section of certain dicotyledonous fossil woods often in that case resembles a monocotyledonous stem.

Petrifaction often gives rise to apparent changes in the tissues, the origin of which must be carefully attended to.

1. In certain cases all the tissues are not equally preserved during petrifaction ; and it is particularly in silicified woods that we see frequent examples of this. Most frequently the soft tissues, more easily altered, are destroyed, as in maceration, while the stem being placed in circumstances suited to silicification, and the more resisting tissues, have preserved their character while becoming silicified. Often in such a

case, the cellular tissue is replaced by amorphous chalcedony and the ligneous and vascular tissues are alone petrified, so as to preserve the forms which characterise them. Sometimes, though more rarely, the reverse of this takes place; the cellular tissue is silicified and preserves its organisation, and the denser tissues have disappeared during petrifaction, leaving cavities in their place; whether it be that these tissues had never been silicified, or that, transformed into a more alterable substance, they were destroyed at a later period. Thus, I have seen many examples of the wood of silicified palms, in which the place of the fibrous bundles was, at least in great part, represented by empty cavities, the remains of the tissue being silicified.

2. Sometimes tissues of the same nature are differently preserved in different parts of the same specimen. In some cases, a kind of partial maceration has destroyed the structure in certain places, while it is well preserved in the neighbouring parts; but there are other instances where the tissue is petrified in a distinct and regular manner at one point, and destroyed at the side. This is particularly obvious in a remarkable fossil wood, described by Mr Witham under the name of *Anabathra pulcherrima;* and I have likewise seen it in some other specimens. The siliceous petrifaction appears to have taken place at first in certain zones very distinctly defined, and most frequently in the form of insulated spheres. In all these parts the tissue is perfectly preserved; but around it, in the intermediate spaces, this tissue is entirely destroyed, and has been replaced by amorphous silica. At first sight, and in a transverse section, the silicified parts would seem so many distinct ligneous fascicles, and give to these stems a very anomalous structure; but an attentive examination shews that the medullary rays and ligneous zones are continued from one part to another, and we may reestablish, so to speak, the tissue throughout. Besides, we perceive that these fascicles are not continued in length; they are insulated spheres, results of a partial petrifaction, enveloped in an amorphous siliceous mass.

3. It very often happens that, during silicification, the vegetable has been compressed, broken, and deformed, fissures filled with crystallised or amorphous silica run across

it, and the tissues no longer continue to be regular; but it is almost always easy to make allowance for these alterations, and do away with their effect.

We perceive that, before endeavouring to compare a fossil vegetable with living vegetables, it is necessary,—

1st, To reconstruct, as completely as possible, according to the parts preserved, and the general data of vegetable anatomy and organography, the portions of the plant under examination.

2dly, To endeavour to determine what may have been the relations of these portions of the plant with the other organs of the same plant, searching more especially for their points of attachment, their forms and vascular connections; trying, in general, to be guided by the traces of structure rather than by the exterior forms.

3dly, To use every exertion to recomplete a vegetable, by seeing whether—among the fossils of the same formation, and particularly of the same beds and the same locality—there may not be some which belong to the same plant. As long as we have not positively ascertained the connection of these different organs, we ought not to consider their reunion to form the same plant as anything more than a simple probability, which positive facts may either overthrow or confirm

This connection of the different parts of the same plant is one of the most important problems to be solved in vegetable palæontology, and which must be recommended to the special attention of those observers who can engage in the inquiry on the spots where these fossils are found.

M. Brongniart's *Tableaux des Vegetaux Fossiles*, from which the preceding observations are taken, is divided into two principal parts. In the first he explains systematically the division of fossils into families and genera. He divides them into many branches, comprehending,—

1st, Amphigenous cryptogamous vegetables, or cellular Cryptogams, which he subdivides into two classes,—the Fungi and Algæ.

2d, Acrogenous cryptogamous vegetables, comprehending two classes, the Musci a numerous class of Filices, itself subdivided into five families,—the Ferns, Marsileaceæ, Characeæ, Lycopodiaceæ, and Equisetaceæ.

3*d*, Dicotyledonous phanerogamous vegetables, of which he enumerates the numerous families, indicating the characters more or less certain, more or less doubtful, which enable us to apapproximate them to the same families of vegetables now living.

4*th*, Lastly, Monocotyledonous vegetables.

The second part is devoted to a chronological examination of the periods of vegetation, and of the different floras which have succeeded each other on the surface of the earth.

Some Particulars respecting the Spheroidal State of Bodies; Proof by Fire; Man incombustible, &c. By M. Boutigny (d'Evreux.) Read before the French Academy of Sciences, 24th May 1849.

Towards the close of the third century of our era, the religion of Zoroaster having suffered many desertions, a council of wise men was convoked to revive the declining faith of his followers. What was said with this view would now be of little interest to us. We may only state that eighty thousand dissentients continued to persist in their incredulity.

In the year 241, Sapor or Chapour ordered the magi to do everything in their power to persuade them to return to the faith of their ancestors. It was on this occasion that one of the pontiffs of the prevailing religion, named Adurabad Mabrasphand, offered to undergo the proof of fire. "He proposed that they should pour on his naked body eighteen pounds weight of melted copper, issuing from a furnace, and at a red heat, on condition, that if he was uninjured, the infidels should give way at so great a prodigy. It is said that the proof by fire was undergone so successfully, that they were all converted." The historian adds, with an air of doubt, which may assuredly be readily permitted in such a case; "We see that the religion of Zoroaster likewise has its miracles and its legends."*

Now, this proof by fire, so successfully submitted to by

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* Dictionnaire Historique, Critique et Bibliographique, t. xxvii., p. 417.

Adurabad Mabrasphand, is plainly an experiment of primitive facility and simplicity, and which is nothing less than miraculous.

But here I pause a little; for I think I observe a smile of incredulity on the lips of those who now do me the honour to listen to me,—a smile so discouraging to one destitute of sincerity, but which only kindles the ardour of one who wishes to deceive nobody, and who uses all his efforts that he may not be deceived himself. Let such persons then allow me to reassure them; the little I have to relate is not like the truth, but it is true, and that is enough. With these remarks I continue.

We know that the followers of Zoroaster were worshippers of fire, which they regarded as the principle of all things.

It is to this celebrated philosopher that this salutary precept is ascribed: When in doubt, refrain.

Zoroaster, according to many thinkers, is one of the greatest moralists of antiquity. According to Voltaire, he was only a quack who would make a poor figure among the least skilful philosophers of our day.

But the French philosopher mentions a fact in relation to Zoroaster, too intimately connected with my subject not to be related here. "The chief of the magicians caused the infant (Zoroaster) to be brought, and wished to cut him in two; but his hand instantly withered. *He was thrown into the fire, which became to him a bath of rose-water.*"*

It is unnecessary to say that the illustrious sceptic considered this statement as eminently fabulous. With regard to myself, I humbly ask pardon of his memory; for I consider it, if not true, at least possible and probable. I speak here only of fire which does not burn.

Whether in France, Italy, or England, wherever I have had occasion to speak of bodies in a spheroidal state, I met with persons who asked me this question: Is there not some connection between these phenomena and that of men running with naked feet over red-hot melted metal, or plunging the

* Dictionnaire Philosophique, t. xiv., p. 179.

hand into melted lead, &c. ?* To such I have always replied, Yes; I believe that an intimate relation subsists between all these facts and the spheroidal state. And then, in my turn, I put this question, Have you witnessed the fact you refer to ? The reply was invariably in the negative.

I confess that all these reports and the wonderful legends which I had read in different works† respecting proofs by fire and incombustible men, admitted without reserve by some, and as stoutly denied by others, had greatly excited my curiosity, and made me eagerly desire to verify all those phenomena, and to recall them to the recollection of my contemporaries; for all this, alas! is as old as the world. *Nil sub sole novum.*

I first wrote to my friend Dr Roché, who spends his life among the furnaces of Eure, and is physician to a part of the Cyclopean population who work there. I asked him to give me precise information.

All that he could learn was, "that one named Laforge, a man between thirty-five and thirty-six years of age, and very corpulent, walked barefooted over the hot iron from the furnace," but he had not seen him. This was not enough to dispel my doubts.

I then applied to the foundry at Paris, when they laughed at me, and shewed me to the door. I did not insist, and retired somewhat discouraged, reflecting on the difficulties of verifying a single fact, even of the simplest kind.

I was afterwards fortunate enough to meet with M. Alph. Michel, who resides among the forges of Franche-Comté. M. Michel obligingly promised to make inquiry into these facts, and solve my doubts.

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* I have said something respecting these facts, in a work entitled *Nouvelle Branche de Physique, ou Etudes sur les corps a l'etat spheroidal,* p. 36.

† Des erreurs et des préjugés repandus dans les diverses classes de la Société, t. xi., p. 183.

Dr Montagne, so well known in the learned world by his numerous works on botany and micography, has translated, upwards of forty years ago, a memoir of Professor Sementini of Naples, on the alleged phenomenon of incombustibility. In this memoir a description will be found of a great number of experiments on fire, and which Dr Montagne had often witnessed (*Bulletin des Sciences Medicales, Juillet,* 1809, p. 5.

The following is an extract from a letter which he wrote to me on the 26th of March last: "On my return I did not fail to inquire among the workmen what truth there was in the statement (as to the immersion of the finger in red-hot melted iron), and generally I was laughed at, but that did not deter me. At last, when at the foundry of Magny, near Lure, I again put the question to a workman, who replied that nothing was more simple; and, in order to prove it, at the moment when a mass of melted metal was pouring out from a Wilkinson, he thrust his finger into the burning jet; another individual in the same employment repeated the experiment with impunity; and I myself, emboldened by what I had seen, did the same thing. I must observe that, in making this trial, none of us moistened the finger."

"I hasten, Sir, to make you acquainted with this fact, which seems to support your ideas on the globular state of liquids; for, the fingers being naturally more or less humid, it is, I conceive, owing to this humidity passing into a spheroidal state, that we must ascribe the momentary incombustibility."

I entirely adopt M. Michel's opinion, and shall afterwards give the theory of it. To my own mind the fact was no longer doubtful; but still I could not allow myself to communicate it to the Academy, making it an invariable rule to submit nothing to its judgment but facts which I have often witnessed *de visu*.

I again made application to different foundries; but they, unfortunately, had not been working for a long time.

I despaired of finding an opportunity of verifying this fact, so curious in appearance, yet so simple in reality, when a particular circumstance brought me into daily connection with forges and foundries, which enabled me to experiment as I pleased on the red-hot burning metal.

The following are the experiments I made:—

I divided or cut asunder with my hand a jet of melted metal of five or six centimetres in diameter, which was escaping from the outlet; then, at the same moment, plunged the other hand into a reservoir full of the incandescent metal, which was truly fearful to look at. I trembled involuntarily ; both hands came out victoriously from the proof. And now, if anything surprise me, it is that such experiments are not quite common.

I will surely be asked what precautions must be taken to preserve one's self from the disorganising action of the incandescent matter. I answer, None. Have no fear ; make the experiment with confidence, pass the hand rapidly, but not too rapidly, through the stream of fully melted matter.

Otherwise, if the trial be made timidly, or with too great haste, we may overcome the repulsive force which exists in the incandescent bodies, and thus bring them in contact with the skin, which will then unquestionably be reduced to a state that may be easily imagined.

To perceive the danger that must arise from passing the hand too rapidly through the melted metal, we have only to recollect, that the resistance is in proportion to the square of the quickness, and in a compact fluid, such as melted iron, this resistance certainly increases in a greater ratio.

The experiment succeeds more especially when the skin is moist ; and the involuntary alarm we feel beside these masses of fire almost always brings the body into that state of moisture so necessary to success. But by taking a few precautions, we truly become invulnerable.

The following method succeeded best with me : I rubbed my hands with soap, so as to give them a polished surface, then at the moment of making the experiment, I plunged the hand into a cold solution of sal ammoniac, saturated with sulphuric acid, or simply into water containing sal ammoniac, and when that was not at hand into cold water.

Regnault, who has investigated this subject says : "Those who make a trade of handling fire, and holding it in their mouth, sometimes use an equal mixture of spirit of sulphur, sal ammoniac, essence of rosemary, and onion juice," all volatile substances, it will be observed, which, while evaporating, render a certain portion of heat latent.*

A common experiment in glass-houses, with which I was made acquainted by M. Dumas, may be mentioned here. It

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* Le P. Regnault, Entretiens sur la Physique, t. xi., p. 101, edit. of 1737.

consists of pouring a mass of melted glass into a pail of water, and kneading it, though red-hot, with the hands.

In this experiment there are two periods well marked,—in the first, the mass of glass is insulated in the midst of the water; in the second, it is covered with a solid and transparent layer, which allows us to see the incandescent mass. The duration of the first period is very short, and it is only during the second that the melted glass can be kneaded with impunity. This experiment, M. Dumas added, has been known from time immemorial; it has been noticed by Bellani, who remarks, that the mass of glass produces no hissing or sign of ebullition in the water, *la quale acqua rimane tranquilla come ponendovi un pezzo di ghiaccio.**

Let us now try to find the rationale of these facts.

We have the formula $m\ c\ t$, which gives the quantity of heat contained in any body.

Let m be the mass, expressed in kilogrammes;

c the specific heat of the body;

t its temperature.

But here we must abstract the *facteur* m, because there was no contact between the hand and the melted metal, and because the experiment presents no difference whether made with 10 kilogrammes or 1000. The sensation experienced is the same in either case, *and it may be easily understood, knowing the repulsive force of the red-hot surfaces which are opposed to the contact of any body.*

The finger or hand is, therefore, insulated in the middle of the melted mass, and thus preserved from the disorganising action of the burning matter. I repeat, that we must abstract the mass.

Let the two *facteurs* $c\ t$ remain. I shall suppose, and it is a sufficient approximation, that the value of $c = 0{\cdot}15$, and that of $t = 1500^\circ$, the temperature of the melted matter; now the product of $1500^\circ \times 0{\cdot}15 = 225$.

It thus appears that the skin of the experimenter will be exposed only to 225 *calorics.* This certainly is a consider-

* Giornale di fisica di Pavia, anno 1816. P. 225.

able degree of heat for the skin of the experimenter; but it is too high, as we shall see.

There is no contact between the hand and the metal; this is a fact which appears to me positively established. If there be no contact, heating can take place only by radiation; and that is enormous, it must be admitted. But if the radiation be neutralised by reflection, and such is the case, it is the same as if it did not exist; and, in short, it is in these normal conditions, so to speak, that the operator is placed.

I think that I have long since proved that water, in a spheroidal state, has the singular property of reflecting radiated caloric,* and that its temperature never reaches that of ebullition; hence it follows that the finger, when moist, cannot be raised to the temperature of + 100° C., the experiments not being continued sufficiently long to allow the humidity entirely to evaporate.

To state the case briefly: On passing the hand through melted metal, it is insulated, the moisture which covers it passes into the spheroidal state, reflects the radiated caloric, and does not become heated to the boiling point; that is the whole.

I was, therefore, justified in saying, at the outset, that this experiment, dangerous in appearance, is almost insignificant in reality.

I have repeated it often with lead, bronze, &c., and always with the same success.†

Such individuals as remember the experiment which consists of plunging a mass of silver or incandescent platina in water, will easily conceive the mechanism of the latter. In the first, the water recedes from the metal, which then seems

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* A new Branch of Physics, or Studies on Bodies in a Spheroidal State, p. 24, &c., 132, &c. See also my two Letters to the Academy of Sciences, dated the 14th and 21st of July 1845. An explanation of this phenomenon will be found in the places referred to.

† The experiments on melted metal were made in M. Davidson's foundry, at Villette, and on bronze in that of M. Nerat, Pierre-Levée Street. I am happy to have the opportunity of publicly thanking these gentlemen for their kind assistance.

to be inclosed in a crystal envelope; in the second, it is the liquid metal which retires from the moist hand. Again, in the first, the metal is active, the water passive; in the second, on the contrary, the moist hand is active, and the melted metal passive. It is the same experiment reversed, and the two form only one; it is the reaction equal to the action; it is, in short, one of the simplest of equations, namely, $a\,b = b\,a$.

I do not speak here of putting a lighted candle into the mouth, and many other experiments of the same kind, which are childish feats, unworthy of the attention of the Academy.

Thus, at an interval of ten years, it has been in my power to form ice in a furnace at a white heat, and to bathe myself, with impunity, in red-hot melted metal, and that in virtue of the laws which regulate matter in the spheroidal state.

Let individuals deny, if they please, the great importance that attaches to the thorough study of matter in the spheroidal state; let them deny, if they choose, the part which this molecular state must necessary fulfil, sooner or later, in science; of this I do not complain, it is only a question of time, and of the future; but this future, which belongs not to us, will perhaps judge with severity those of my countrymen who suppress in the *Memoirs des Savants étrangers*, before printing them in France, passages which are friendly to my favourite pursuits.* This is one of those actions which are sufficient to tarnish the lustre of the most brilliant scientific reputations.

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* See the Memoir which I had the honour to address to the Academy on the 2d of last October (1848).

It is M. Grove's Memoir that is here treated of, entitled, "On certain phenomena of Voltaic Ignition, and the Decomposition of Water into its Constituent Gases by Heat." This memoir has been translated by M. Louyet, and the following is the passage which has been struck out in France:—"However, to return to more important considerations, the spheroidal state, which has, of late, attracted the attention of philosophers, would appear to have the most intimate connection with these phenomena, and, in consequence, the interest which attaches to it is much increased." (*Bulletin du Musée de l'Industrie Belge*, 4th liv. 1847.)

I have stated elsewhere* that we find traces of the spheroidal state in the Bible. Does not the fact I have related respecting Adurabad Mabrasphand (and I could have added many others) seem to prove that the knowledge of the ancients with regard to heat, was much greater than we suppose? They were unacquainted, perhaps, with minute matters respecting heat, as, for example, the thousands of centigrade degrees, but they certainly knew its grand effects.

It may be gathered from this note that a certain number of historical facts considered fabulous may be true, and that the ancient philosophers probably knew many things of which we are ignorant. A little more respect for them, a little less admiration of ourselves, would not be misplaced.

I shall conclude this note by reminding the Academy, whose kind indulgence I again solicit, of this remarkable and unexpected analogy existing between the living molecule, and the molecule in the spheroidal state; it is the fixity of the temperature, whatever, in other respects, may be the variation of that of the surrounding medium.

Thus man may live in media which vary from $-30°$ C. to $+40°$ C., without his own temperature being affected. We know that man may even endure for some time the extreme temperatures of $-60°$ C. and $+150°$ C., his own remaining uniform. We know that the inhabitants near the poles, those of the fortunate climates of the tropics, and the burning regions of the line, are of the same temperature, or that, if it vary, it is only within very narrow limits.†

This being admitted, let us take a drop of water and throw it into a vessel heated to 142° C.; this water will immediately assume the temperature of $+98°$ C., and will continue there, the vessel being changed to all imaginable temperatures from the above minimum I have mentioned ($+142°$).

This, the stable equilibrium of bodies in the spheroidal state, with regard to heat, will one day enter, at least

* Nouvelle Branche de Physique, ou Etudes sur les corps a l'etat spheroidal, p. 1.

† Ibid., p. 94 and 192.

I hope so, into the explanation of one of the greatest mysteries of creation—the creation itself.*

It will at once be understood that a fluid whose temperature is invariable, whatever may be the variations of the temperature in the bodies which surround it, is a fluid eminently adapted for incubation. This latter word expresses the whole of my idea without developing it.

On the Geology of the Valley of Reposoir in Savoy, and on the Rocks containing Ammonites and Belemnites lying above the Nummulitic Formation. By M. A. FAVRE, Professor in the Academy of Geneva.†

This paper is not intended to bring forward a theory ; it is simply to describe a fact which has appeared to me important and difficult to explain, although it presents itself in a manner apparently simple. It is the superposition of great calcareous masses containing belemnites and ammonites on rocks filled with nummulites. This order of superposition is contrary to what palæontology and geognosy have hitherto made known to us ; consequently we have reason to doubt whether the position of the rocks in question be normal. On the other hand, this singular fact presents itself in a manner so simple ; the structure of the mountains where it occurs is so regular, that it is deserving of the attention of observers ; this is the object of the present notice. But I recommend them, before judging, to take the trouble to traverse these high mountains in different directions. Although I have done so many times, it is not without extreme diffidence that I bring forward my observations.

Can the valley of Reposoir be one of those localities where examination reveals to us exceptions to the general rules which science has established for the Alps ? Must the posi-

* If the Deity has permitted the human mind to penetrate into this hitherto inexplicable mystery.

† From a copy sent by the Author. *Vide Bibliothèque Universelle de Genève*, June 1849.

tion of the rocks of this valley be added to the exceptions already known, presented by the geology of the Alps, when compared with that of other countries? This is what I do not undertake to decide; for it is necessary that exceptions of this kind should be established by more than one observer. But I may refer, as an example of these anomalies, to the localities of St Cassian and Hallstadt in Austria, where we find a mixture of orthocera and ammonites; Petit-Cœur, in the Tarentaise, where we see belemnites associated with plants of the coal-formation; the apparent superposition of jurassic limestone over the tertiary molasse, which is observed throughout a great part of the northern acclivity of the Alps, from Savoy nearly to Vienna in Austria; and, finally, the fan-shaped structure which, in a great number of localities, places the crystalline slates above limestones containing belemnites.

The valley of Reposoir is situate in Savoy, on the left bank of the Arve, between the towns of Cluses and Thones; it is inclosed between two chains of elevated mountains. That on the north is the chain of the Vergys mountains; that on the south is the chain of Meiry, or La Pointe-Percée, which separates the valley of Reposoir from that of Mégève, the prolongation of which occupies the right bank of the Isère between Albertville and Montmelian.

The beds which constitute the chain of Vergys dip very nearly to the south-east, while those of the chain La Pointe-Percée dip to the north-west. It is the same beds which form these two chains, so that the valley of Reposoir, which is situate between these two, presents the geological structure named *structure en fond de bateau*.

The highest peaks of the chain of Vergys reach 2388 metres above the level of the sea, according to M. Chaix. Pointe-Percée, which is the most elevated part of the chain to which it gives the name, has never been measured; but I calculate that it rises to 2500 or 2600 metres above the sea. Between these two chains of mountains, and consequently in the centre of the valley of Reposoir, rises a large mountain, known under the name of *Montagne des Anes*. Its base at Reposoir is 981 metres (barometric observations made at the inn); I estimate its peak at about 2300 metres. It divides the valley of Reposoir into two parts, which unite at the north-east and south-west extremities of the valley. The Montagne des Anes is connected with the chain of the Vergys to the north by the Col de la Touvière or des Ferrands, and on the south to the chain of Pointe-Percée by the Col des Anes. These two cols are very interesting in respect to their geology.

It is therefore evident, that the Montagne des Anes rests wholly on the beds which form the chain of the Vergys and Pointe-Percée, or, what is the same thing, these beds run beneath this mountain. Such is the position and structure of this valley. I now proceed to the geological part of this notice.

The two chains of mountains just spoken of are composed of neocomian beds; the greatest mass belongs to the limestone of the first zone of rudistes or limestone with Chama Ammonia. In some of its most elevated parts we perceive the inferior neocomian, which has pierced through the upper stage of the neocomian. It is characterised by the *Toxaster complanatus*, Ag., which is found in great abundance in the Col du Balafras (2303 metres, barometric observations, chain of the Vergys), and at the *Cheminée* du Meiry (chain of Pointe-Percée.) The jurassic formation is seen below the neocomian formation on the southern reverse of this latter chain; while it cannot be seen in the chain of the Vergys.

The neocomian formation of which we have spoken is covered to a great thickness with white limestone, with Chama Ammonia, on which lies green sandstone, or the albian formation in beds, or in portions of beds, lying here and there on the surface. This formation is rich in fossils on the southern reverse of the chain of the Vergys, on the stairs of Sommiers, and at Roselletaz, in the chain of Pointe-Percée. According to the observations which Murchison communicated to the meeting of the Helvetic Society of Natural Sciences, met last year at Soleure, this formation ought to be covered in some localities by a limestone which appears to be equivalent to the limestones of Seewen and the white chalk. This rock is covered by a blackish calcareous sandstone, filled with small nummulites. This nummulitic limestone is surmounted by the Alpine macigno formed by limestone rocks, more or less marly, associated with some sandstones. This is a formation identical with that which the Geological Society of France studied some years ago in the Deserts near Chambery. The beds of this macigno, which form the bottom of the valley of Reposoir and the base of the Montagne des Anes, alternate in very great numbers at a time with beds, more or less thick, of Taviglianaz sandstone, which, as I have said elsewhere,* appears to be a kind of old volcanic tufa. This rock is associated with *cargneules*, with red limestones, and near the Col de la Touvière, we find a quartz rock in a mass, which is subordinate to it. It is below all these rocks that the great limestone mass which forms the Montagne des Anes is situated. It is composed of a greyish or yellowish limestone, which encloses pantacrines, pectens, terebratulæ, fragments of ammonites and belemnites, very easily recognised as to the genus, but not to be determined as to the species.

I do not in general believe in anomalies and exceptions in geology; because the phenomena have been too general to produce what may be called geological monstrosities. Yet, although I have often visited this singular locality, I have always come to the same result, and I have always seen the superposition of the ammonitic and belemnitic limestones on the nummulitic limestones. The observations are very easily made, for the Montagne des Anes, as I have said, is insulated in the middle of the valley, and we see, on the north as well as on the south side, the beds of the Vergys and Pointe-Percée dip below it.

I do not know to what age the formation of this mountain should be referred; but I may say, that, in its aspect, it presents more relation to the jurassic formation than to any of the stages of the cretaceous formation of our country.

I may state, in conclusion, that it is not the first time that more ancient formations, resting on nummulitic formations,

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* Notice of the Geology of the German Tyrol, and on the Origin of Dolomite.—*Bib Univ. (Archives)*, tom. x., p. 205.

have been mentioned. M. Prof. Studer* found gneiss, in the Bernese Oberland, lying over the nummulitic formation.

Perhaps the most extraordinary fact is that stated by M. Escher, in his Geological Account of the Canton of Glaris. At Ortstock, we find the following section proceeding from above downwards: 1. Superior and middle jurassic limestone; 2. Inferior jurassic limestone; 3. Sernfconglomerat, which is a puddingstone analogous to that of Valorsine, whose normal position is between the crystalline rocks and the jurassic formation; 4. The middle jurassic limestone again occurs; 5. The nummulitic limestone below all these beds.

The mountain of Glarnish presents the same section, only we find the neocomian limestone and nummulitic limestone below the preceding beds. In this mountain, the latter is found, therefore, at the summit and the base. These examples might be easily multiplied, but I think that we cannot find any others more extraordinary.†

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* *Bulletin de la Sociéte Geologique de France,* 2d Series, t. iv., p. 213.

† The following are a few facts analogous to those mentioned in this note, and may serve as a point of comparison.

M. Studer says, that, in some localities in Switzerland, the nummulitic rocks are covered by a formation, *a fucoides,* which incloses belemnites. (*Actes de la Sociéte Helvetique des Sciences Naturelles,* p. 104. Basil, 1838.)

M. Coquand assures us that M. Savi found a hamites (perhaps Ancyloceras), in the macigno in the neighbourhood of Florence; M. Pentland discovered an ammonite there, and M. Pareto likewise obtained an ammonite in the macigno of the mountains of Génoa. According to M. Coquand, this macigno likewise contains nummulites, and ought to be ranked in the cretaceous formation. (*Bulletin de la Sociéte Geologique de France.* 2d Series, t. ii., p. 194.) According to M. Murchison, these fossils should have been found in the rocks inferior to the nummulitic formation. (*On the Geological Structure of the Alps, Carpathians, and Apennines, from the London, Edinr., and Dublin Phil. Mag., March* 1849.)

M. Gaillardot points out, in the environs of Cairo, beds with ammonites covering nummulitic beds at the foot of Mokatam. (*Ann. de la Sociéte d'Emulation des Vosges,* 1845, t. v. 3d Part.)

According to the account given in Ferussac's Bulletin Geologie, 1829, t. xvii, p. 322, it would appear that M. Partsch has found an ammonite in the fucoidal sandstone rocks of Kahlenberg near Vienna.

Notice of an Intestinal Concretion from a Snake. By JOHN DAVY, M.D., F.R.S. Lond. and Ed., &c. Communicated by the Author.

Whilst in Barbadoes, I received from assistant staff-surgeon Dr Webb, one of several concretions, which had been voided by a Boa constrictor, that he kept in confinement, and which from their character it may be inferred were intestinal, for they had none of the properties of urinary concretions ; they contained no lithic acid.

The several masses voided were all very similar, in form approaching the oval, of the colour nearly of unbleached wax ; of a greasy feel, of about the hardness of wax, but more brittle and easily reducible to powder. In size, they were nearly equal to a pullet's egg. That which I have examined, when broken, exhibited in the fracture a feeble resinous lustre, and a structure somewhat concentrically lamellar.

Exposed to the action of heat, it burnt with a bright flame, first with a smell of animal matter, such as horn gives when burning, afterwards with a strong ammoniacal odour. The coal it left was bulky, and easily incinerated. It yielded (three different portions of it), from 6·6 to 8·4 and 9·8 per cent. of white ash ; which was found to consist of phosphate of lime, with a minute proportion [little more than a trace] of lime, magnesia, and potash.

Digested in boiling alcohol, and that repeatedly, it lost 32·4 per cent. The alcoholic solution on cooling, became turbid from the separation of oil particles, and when evaporated, yielded more of the same oil, which had the properties of oleine. It was colourless and liquid at ordinary temperatures. No cholesterine could be detected mixed with it. What remained after the separation of the oil, nearly resembled in its qualities the matter of epithelium or cuticle.

At first inspection of the concretion, from its resemblance to the indurated yolk of egg, I thought it probable that it might be this substance somewhat altered, but the facility with which its ash is obtained, and the composition of its ash, so different from that of the yolk (containing so much phosphate of lime which is absent from the yolk, and no pure phosphoric acid which abounds in the yolk), is opposed to this conjecture. Considering what is the ordinary food of snakes, a more probable conjecture is, that it is formed of the undigested portions of the animals on which the snake that voided it fed, such as cuticle, and the horny or scaly parts with fatty matter, mixed with epithelial scales, derived from its own alimentary canal.

A concretion of this kind, is well adapted by its composition to resist decay, and to become consequently a coprolite. And it is in relation to such an occurrence, I apprehend that the one I have described is chiefly interesting.

French Scientific Mission to the Pampa del Sacramento. By M. F. De Castelnau.

It was remarked in this country, some years ago, that the French had almost ceased to add to the general stock of geographical knowledge,—a fact which it is not easy to explain in a people, so very large a proportion of whom receive the elements of a scientific education, and who might, therefore, be expected to furnish a larger proportional number of scientific explorers than the plainly-educated manufacturers, shopkeepers, and farmers of Great Britain. Certain it is, too, that little has been wanting on our side to excite a spirit of rivalry in this most legitimate field of national emulation. Caillé's three volumes of Travels in Central Africa is a very poor affair,—the work rather of an editor than of the traveller himself, who was miserably wanting in the accomplishments necessary for such an expedition.

The South American enterprise, of which a short narrative follows, was planned and conducted very differently The region visited is abundantly interesting. Let the reader turn to a map of South America ; he will perceive that the great chain of the Andes, for many degrees to the north and south of the equator, runs close to the Pacific, leaving a vast interval betwixt its ridges and the Atlantic. This interval is extremely fertile, abounding in those three grand elements of fertility, heat, moisture, and a deep alluvial soil. The vast volumes of water drawn up by the tropical heats from both oceans, and the rarefaction of the atmosphere over the heated plains, landward, produce rainy seasons of extraordinary intensity, and to this must be added the floods caused by the melting of the snows. Of the vast basins formed by the streams that flow into the Atlantic, those to the north and south of that of the Amazon and its tributaries, receive a less amount of rain, and consist of the richest prairies as well as of wood; but that of the Amazon, being the most extensive of all, is so charged with moisture and heat, as to be covered throughout its whole extent with stupendous trees, and a thick undergrowth of shrubs and parasitic plants, so as to be in many places almost impenetrable, and extremely unhealthy. No wonder that the upper parts of this, the largest forest in the world, separated from the Spanish colonies on the Pacific by the tremendous barrier of the Andes, and remote from the chain of European settlements on the Atlantic sea-board, should be still very imperfectly known. This is the region which M. Castelnau and his companions were commissioned to explore.

Thinly scattered as is the present Indian population of this extensive basin, it may be expected one day to be as prolific in human beings as it is now in the noblest productions of vegetable life, and in wild beasts, birds, and fishes. Indeed, on comparing the descriptions given us by Humboldt of the three great basins drained by the Oroonoko, the Amazon, and the Rio Plata, it would seem as if the first and the last were best suited to herbivorous animals, the horse, the ox, and the sheep; while the central one may ultimately prove the most abundant in providing for the sustenance and convenience of man. Humboldt's description forms an interesting preparative for the story of this French expedition, and we therefore give it entire.

"These three transverse chains, or rather the three groups of mountains, stretching from west to east,—that is, from the great chain of the Andes to the Atlantic, within the limits of the torrid zone, are separated by tracts entirely level, the plains of the Caraccas, or the Lower Oroonoko; the plains of the Amazon and the Rio Negro; and the plains of Buenos Ayres or the La Plata. I do not use the word valley, because the Lower Oroonoko and the Amazon, far from flowing in a valley, form but a slight furrow in a vast plain. The two basins placed at the extremities of South America are savannahs or steppes, pasturage without trees; the intermediate basin which receives the equatorial rains during the whole year, is almost throughout one vast forest, in which the rivers form the only roads. The same vigorous vegetation that conceals the soil, renders the uniformity of its level less perceptible; and *the plains* of Caraccas and La Plata alone bear this name. The three basins just described are called by the colonists the Llanos of Varinas and Caraccas, the *bosques* or *selvas* (forests) of the Amazon, and the Pampas of Buenos Ayres. As the region of forests comprises at once the plains and the mountains, it extends from 18° south to 7° and 8° north, and stretches over nearly 120,000 square leagues. This forest of South America,—for in fact it is only one,—is six times larger than France."

Let it not be supposed, however, that this vast forest, in comparison with which those of Europe sink into perfect insignificance, even in its wild state, produces no better food for man than the acorns and chestnuts of our climate. Be it remembered, that so large a part of the human family live on the produce of different sorts of the palm-tree; that man has been not inappropriately called a palmivorous animal. Now, the great traveller we have quoted speaks of ninety different species of the palm, all adapted for human support, and all to be found in this region! Nor are they the only trees in that huge territory that provide abundant and nutritious food for our race, and to such trees must be added the various edible roots, and the fish that teem in the rivers, all contributing to the supply of a population which might be immensely multiplied without having to dread starvation, unless from the improvidence and idleness of savage life.

It is true that the Selva, though so abounding in food for man, is at present so unhealthy, that even the Indians scattered over it are said seldom to live beyond fifty; but they are in a wretchedly-low state of civilization, and their habits are probably as unfavourable to longevity as any other cause. Science and industry, too, may do much to counteract the morbific effects of a hot and humid atmosphere. The remains of pottery found in the woods, inscriptions left on the face of the hardest rocks, traces of causeways laid across swampy plains, attest the former existence in those regions of a more numerous, civilized, and energetic race than is now to be found there. May not another such race yet inhabit this vast region and make it another China, teeming with inhabitants, uniting the enterprise and inventive powers of the European with the plodding and methodical industry of some of the Asiatic races, Should those low ledges of granite, which Humboldt found traversing the Llanos of the Oroonoko, extend through the wooded and, as yet, little known basin of the Amazon, abundant materials may be found for facilitating communications by means of causeways and bridges, and for aiding the natural drainage of the country with canals.

Our interest in such a territory, and our hopes of its becoming an extensive seat of civilization and useful industry, are modified of course by the accounts we receive of the Indians who now inhabit it, and who are known chiefly through the Roman Catholic missions that have been established amongst them; but we must not judge of the capacity of a savage people for receiving Christianity and civilization by the experience of such missions. According to what Humboldt was told and believed, some of the Selva tribes are cannibals,—no unlikely thing, since the New Zealanders, who seem to have come originally from South America, were notoriously such down to our own days. But are we to despair of the cannibals of South America, after the complete success of our Protestant missions in New Zealand, because the Roman Catholic missions have failed to Christianize them? This would be preposterous, considering how totally different the whole policy of these respective missions has been. Previous, indeed, to Humboldt's disclosures, the most extravagant ideas were propagated even in Britain as to the enlightened efforts of the Spanish Romanist missionaries among the Indians; and when these Indians have turned on their alleged benefactors and killed them, it has been regarded as the most conclusive evidence of a nature so incurably savage as to be almost past hope of improvement. It appears from M. de Castelnau's report, that in the Pampa del Sacramento alone, comprising but a small portion of the Selva, no fewer than sixty-seven missionaries had been murdered; but we apprehend that the zeal of which they were the victims was evangelical only in name. "The conquest of souls," as understood by the missionary fathers, was very far indeed from being that of the Gospel. To prevent their missions from dwindling away, they made hostile incursions, called *intrœdos*, into the villages of the independent Indians; the natives were usually attacked in the night, and their children seized and carried off, to be distributed among the Indians of the missions as serfs. Humboldt relates a heart-rending story of the sufferings undergone by a poor heathen Indian mother, in her almost superhuman efforts to recover a child thus mercilessly kidnapped from her. In these circumstances, it can be no matter of surprise that the Indians have been slow to adopt the Christianity of the religious orders that have been labouring so long, and with so little success among them; or, that they look upon the missionaries with abhorrence, and are looked upon by the latter in return as ferocious savages; in short, that while the Protestant missions at Otaheite, the Sandwich Islands, and New Zealand, have, within the last twenty or thirty years, produced the happiest results, far otherwise have been the state of things resulting from the labours of three centuries spent by Roman Catholics in South America.

M. Castelnau's Report.

You are aware that we went to Cuzco, with the view of penetrating into the Pampa del Sacramento, and surveying the river Ucayali (one of the higher branches of the Amazon.) That whole region is now regarded by the Peruvians, as it formerly was by the Spaniards, as one of wonder and terror. Sixty-seven missionaries had successively found a martyrdom in it; and but a few years ago, some deserters who had fled thither, all fell in one night under the clubs of its savage inhabitants. Only one man had ever been known to enter those wild solitudes with impunity, M. Palacios, a citizen of Cuzco, who, after being sentenced to death,—in the course of one of those political revolutions which are so common in that quarter,—effected his escape, and, having nothing to lose, surrendered himself to the wild Indians. After many adventures among them he reached the river Amazon, and thus regained his liberty.

From the prefect of Cuzco I had a most cordial reception, his Excellency the President of the Republic having recommended the expedition to his attention in the most formal manner. That functionary, himself a most intelligent person, was fully aware of what importance to his own country a knowledge of the tract lying to the east of the Cordilleras might prove; he knew that the deep valleys that traverse it are almost the only fertile parts of Peru, and that their rich produce is at present of no use from want of the means of transport. Accordingly, he organized the expedition in the most ample manner. Sixty mules laden with provisions, cordage, and other articles required for passing torrents and precipices, soon, thanks to his care, were on the road to the valley of Santa Anna. Our party consisted of Messrs D'Osery, Deville, and myself. To these the Peruvian government had added the frigate-captain Carasco, and three other officers. Sixteen soldiers were to escort the expedition as far as the point of embarkation. As these soldiers were known to hold the very idea of entering the Pampa in the greatest horror, some officers were sent along with them to keep up their spirits, as well as command them. Finally, I must not forget my faithful Malay servant, Florentino, who has rendered me such valuable services during this severe campaign.

The prefect of Cuzco, accompanied by a numerous staff, and many of the city's inhabitants, among whom there was a fellow-countryman of my own, convoyed us so far on the 21st of July 1846. They parted from us with unaffected grief, thinking our doom was sealed. We slept that night at Urumbamba, and arrived next day at Olianty-Tambo, the ancient seat of a warrior famous in that district for having raised the standard of revolt against the powerful Inca of Cuzco. Ruins that called forth our wonder were seen stretching afar into the valley. The fortress, which is constructed of stones of prodigious size, crowns a hill-top overlooking the town, right in front of which there rises a very steep hill; and, on its side, an ancient building is seen overhanging a tremendous precipice, over which, as chronicles relate, the tyrant used to toss his enemies,—a tale rendered probable by the numerous skeletons that lie scattered at the foot.

The route we now followed was singularly picturesque, lying among the spurs of the great chain of the Andes. Here we traversed primæval forests, and found the soil everywhere rent by water-courses, along which the mountain torrents swept the snows they had received from the neighbouring peaks. These valleys are peopled with colobri or humming-birds of the most brilliant hues, and remains of the ancient Indian civilization could often be recognized among the noble trees of the forests. Ere long, we had to ascend a chain of mountains, whose summits seemed lost in the clouds. The path wound along the mountain-side until it reached the limit of perpetual snow; here we were enveloped in thick mist, and our limbs felt benumbed with cold. The sole inhabitant of those lofty regions is the condor, which keeps constantly swooping above the head of the traveller.

We descended the eastern side of the Andes, by paths extremely narrow, and running along the edge of tremendous precipices, and reached at last the lovely valley of Santa Anna, abounding in sugar-canes, coffee, cocoa, and coca. Here, this last production is the most valuable of all; for whereas all the rest perish under foot, owing to the impossibility of transporting them to the coast, the coca, on the contrary, being an indispensable article of food to the Indians, always commands a ready sale. With no farther provision than a few handfuls of the leaves of this shrub, these will undertake an eight days' journey, and even more. I doubt not, that ere long, this production of the soil will be in request in Europe; it strikes me, it might be serviceable above all to sailors, whom it might secure against the horrors of famine, so common in very long voyages. We were hospitably received at the fine farms that cover the valley, which are cultivated by Indians, paid at the rate of from a shilling to fifteen pence. But it is often difficult to procure labourers, and the proprietors complain much of their capricious tempers.

We reached the small village of Echarate on the 29th of July, and remained there for some time, for the purpose of organizing the expedition, this being the last Peruvian settlement in that direction.

Notwithstanding the minutest precautions, several of the soldiers had already deserted, and among these was the sergeant, who went off while relieving guard. This escort proved a most ridiculous affair. While on the march, there was always one officer at the head of the column and another brought up the rear; on arriving at a village or *hacienda*, the soldiers were locked up and guarded, by men belonging to the place, during the night, in spite of all which precautions, there was not one left on our arrival at Echarate. One of the officers even had left us on the road.

Here, with much ado, we succeeded in engaging the services of eight men belonging to the district, and Fray Raymond Bousquet, an old French Franciscan missionary, who had penetrated into the Pampa del Sacramento forty years before, expressed a desire to accompany the expedition. This monk, who had been born in Spain, although now verging on eighty, had resolved to give us the benefit of his experience among the tribes of the wilderness, having been requested to do so by the Bishop of Cuzco. Ever in high spirits himself and full of kindness, he kept up our spirits by his example and his discourses, until the day on which he fell a victim to his evangelical zeal, as we shall see by-and-bye. A French artist whom we found at Echarate, likewise joined our party.

We had now to embark on the river Urubamba, which takes the name of Ucayali, after its junction with the river Jambo or Apurimac, the point of embarkation being six leagues from Echarate. Thither we accordingly repaired, passing on our way the mission of Cocabambilla, which had been founded by Father Bousquet. At the quay we found six canoes and two rafts, that had been prepared for us by an officer sent forward for that purpose by the prefect. Twelve Antês Indians also had been engaged as guides. These Antês form a numerous nation, occupying the whole tract of country between the rivers Urubamba and Apurimac. They still wear the ancient costume of the Incas, consisting of a long robe, and an opening on each side for the arms. The Indians paint themselves of a red colour, with the pigment called rocou. They never congregate in villages, but live in detached families along the banks of the rivers.

The expedition embarked at last, on the afternoon of the 14th of August, starting from the small port of Chouaris, where there is but one solitary uninhabited hut, being that set apart for storing quinquina, abundance of which is collected in that quarter.

Here, M. Minister, I am compelled to say that there was but little harmony between M. Carrasco (the Peruvian naval officer), and the French part of the expedition. I abstain from entering into the painful details, which I communicated to the Chargè d'affairs at Lima. All I shall here say is, that that officer ill answered to the kind and generous intentions of the government of the republic.

Not many minutes after our setting off, we passed the first rapid. The very small size of our canoes, rendered these rapids highly dangerous to us, and one of them narrowly escaped being swamped. We found it necessary to remove the slight coverings of palm leaves which we had put up to protect us from the broiling rays of the sun. On the 15th we passed several rapids, and the Falls of Illiampani. Next day, one of the boats made a narrow escape; and a box, containing our astronomical instruments, was carried down the stream. I offered a high reward to whoever should recover this precious part of our baggage, and we were fortunate enough to do so. We were about six leagues only distant from the port of our embarkation, and already, owing to circumstances to me unaccountable, our provisions began to fall short.

During the night of the 17th, four men deserted, and on the day following, looking at the difficulties now before us, the question of the possibility of our continuing the expedition was discussed. I then considered it as my duty to shew that the French never abandon an enterprise, however hazardous, as long as there remains the slightest chance of success. I called my fellow-travellers together, accordingly, and placing myself at their head, asked each to state his opinion, in the order of their ages, beginning with the youngest. All were agreed as to the gravity of our circumstances. I then proposed the following questions: 1*st*, Is there any possibility of continuing the expedition, without sacrificing the baggage? 2*d*, Can it be prosecuted at the expense of separation from the baggage?

The answer to the former of these questions was given with one voice in the negative, and as for the latter, two voices only declared the impossibility of prosecuting the expedition under any circumstances; one being that of M. D'Osery, who, aware that I had fully resolved to send back the baggage to Lima under his safeguard, could not think of encouraging his companions to subject themselves to risks which he knew he would not have to share with them. I refrain from saying who the other was, and who exclaimed that it would be running into certain death, and would fain have had us recall the resolution we had taken, alleging that the junior officers had misapprehended the question; and, finally, declaring that it meant that a part of the expedition might continue, but that nothing bore that it ought to be continued.

I immediately broke up the council, and it was resolved, that leaving all our instruments, and almost all our baggage under charge of M. D'Osery and a Peruvian officer, we should forthwith proceed with our voyage in the canoes. It was agreed that our comrade should rejoin us by the land route, on the Amazon. Our separation in such circumstances caused one of the saddest moments of my life, and I stept into the canoe, hardly able to suppress my tears.

No sooner had we set off than we enterd a succession of dangerous rapids in which three of our canoes were upset; the men contrived to escape with their lives, but a bag of rice,—our main supply of food,—was lost, and we found ourselves reduced to forty pounds of chocolate for the support of so many people in unknown wilds. Almost all our powder, too, had been swept away by the water. In the evening we arrived at the mouth of the river Sirialo; here we had for supper only some green bananas and a little wet and mouldy biscuit. An extraordinary rise of the river took place that night. The water rushed upon us instantaneously, and a furious storm following next morning, drenched us thoroughly. This storm lasted till noon of the 19th, and the rest of the day was spent in drying our clothes and other articles. The bank of the river presented a very odd sight for the time. Here were spread out coats and uniforms; there lay priest's ornaments and sacramental cups. We did our utmost to dry the chocolate, but already it had become sour. But this day I met with a much worse loss,—that, namely, of my chronometer, into which the water had found its way and stopped its movements.

Among our Indians there was one that had particularly distinguished himself by his activity and willingness to be of use. This evening he told us that he was a runaway, in consequence of his having murdered a family of ten persons in order to get possession of an axe. Such were the men at whose mercy we were about to find ourselves placed.

We had eaten nothing during the day, and hunger, which had already extremely weakened us, now affected us with a constant giddiness. We still had a mouldy ham, filled with maggots, and a council being held in order to decide whether it should be sacrificed, although prudence urged our preserving it for some worse extremity, gluttony prevailed, and we thought it delicious.

Two men deserted on the night of the 20th. So reduced were we in strength that it required all our efforts to float the canoes that had been left high and dry by the subsiding flood. We this day passed the falls of Sirialo, forming four successive descents of the water from a great height. We proceeded along the banks, assisting the old priest with much difficulty over the rocks, which were often perpendicular, while the men passed the canoes over the falls. This was an infinitely laborious operation; for the crews were now reduced for the whole tour, to two of the men who had been hired, and four Indians, together with my faithful servant Florentino.

Having passed the falls, we re-entered the canoes, two of which were almost immediately swamped in a rapid, by which we lost nearly all that remained of our chocolate, our last resource against the horrors of hunger.

We slept at the mouth of the little river Sangobatea, whence two of our men went to an Indian hut, situate about a league off in the interior, and returned with some roots of manioo. One may readily imagine our delight at getting these. Next day I lost one of my canoes in a rapid, and was happy in being able to purchase from the Indians, for an axe, a small raft in which to stow what articles we had contrived to save. We then passed many rapids, and put up for the night at an Indian hut, the owner of which possessed a beautiful canoe. This I vainly tried to purchase. "I am happy," said he, "and there is nothing on earth I need wish for: I have a hut, a canoe, bow and arrows, three wives, and two dogs, and what more would you have me have?"

Vegetation now became more and more beautiful; ferns as tall as trees, together with thousands of palm trees, imparted to the scene a decidedly tropical character, and this was not belied by the presence of numerous Indians. Here Father Bousquet baptized an infant, whose father insisted on having an axe given to him. It is not likely that Maria Francisca, such was the child's baptismal name, will ever know in this world that the waters of baptism have regenerated her forehead.

In the course of the night I had a sharp attack of fever, which I attributed not only to our horribly bad food, but, alas, to the water finding its way into our canoe, and the wetness of my clothes. The rapids followed each other in such close succession as to admit of no intervals long enough for the drying of our clothes in the sun.

Another boat was swamped on the 22d. Some time after, a large Howling monkey was killed; but, hungry as I was, I could not prevail on myself to eat of an animal so much resembling a child. Already we were without shoes, and when proceeding by land, the pebbles and rocks, heated intensely by the sun, scorched our feet, and caused acute suffering; in addition to which, we were frightfully cut by the sharp stones over which we had to walk. In the evening we halted at an Indian hut: there was nobody in it, but we found a few green bananas. I had suspended my hammock from the posts which supported the frail edifice; in the course of the night down came the whole with a crash, and I barely escaped being crushed by the roof, from which I found it no easy matter to disengage myself. Here we remained all next day for the purpose of drying our powder and other articles that had escaped our successive disasters. Having killed a hocco and some red aras, this secured us a dinner. The latter we found very common here, and the bright plumage of these magnificent birds forms a charming contrast with the deep green of the palm trees on which they perch in large flocks.

On the 24th my canoe was swamped in a rapid, and with it perished a large part of my effects. Such accidents were now matters of daily occurrence, and are mentioned merely to give an idea of the miseries we had to encounter.

Next day, having nothing left but chocolate, we tried to swallow a little, but found it as sour as vinegar. We then, with great difficulty, passed the Fall of Montalo. Here the whole formation is schistous, but among the boulders on the banks of the river we found much of the debris of granite and red sandstone. We were compelled to go more than half a league by land, over rocks shelving so rapidly that we had often to support ourselves by holding by the branches. During the whole afternoon we were exposed to a violent storm.

Our supper consisted of a coati, an animal that lives in the woods, and much resembles in appearance a large rat, and of roots gathered from among the rocks. At nightfall some Indian women brought us bananas.

The 26th was a sad day to our party. The canoe that conveyed Father Bousquet was carried over a fall, and the poor missionary was drowned; a prayer to God being the last words he was heard to utter. The old man had adopted a child, who accompanied him; and it is impossible to describe the despair of poor Franchito on seeing his benefactor perish. He implored us to allow him to search for his dead body, a favour which the pressure of hunger compelled us, alas! to refuse. Such was the death of a man who had spent fifty years in the wilds of America. On the same occasion we sus tained a loss, little thought of at the time, but keenly felt afterwards, that of our supply of salt.

We now followed the right bank of the stream, proceeding by land, and clinging with infinite difficulty to the rocks. The canoes made vain efforts to rejoin us; they were prevented by the force of the current. Ere long they were struggling with the Maperontani Fall, where the river, hemmed in betwixt perpendicular cliffs rushes furiously against the enormous rocks that obstruct its course. The fall presents three successive steps, and is about three metres and a half in total height. We held the right bank, whereas the canoes ran along the left. We could see everything, but the noise effectually prevented our making ourselves heard. Such of the canoes as contained a few articles succeeded in passing, but it was soon found impossible to get the others over with their cargoes. The Indians and the men who had been hired, quite spent with fatigue, refused to take the baggage in their arms; and then it was that the faithful Florentino threw it over his shoulder, and, quite unassisted, carried the entire lading of the two canoes, in successive trips, over frightful rocks. I anxiously followed him with my eye, fearing lest, sinking with fatigue, he might leave behind what was the most precious article for me, yet whose value he could not appreciate, my only barometer, brought with so much difficulty from Lima. Great was my delight at seeing him take up the instrument and carry it with a sort of respect to the point where it could be re-embarked. The Indians then attached stout ropes of bindweed to the canoes, and thus succeeded in passing them over the cataract. It was not until nightfall, and about half a league farther on, that we could rejoin our fellow-travellers on board the canoes. Several of them had lost their clothes that day; and it gave M. Deville and myself great pleasure to share with them the few articles that remained.

Weak as we were from the effects of bad food, and harassed with fatigue, our courage was sustained by the certainty of our having nearly reached the termination of the falls. Of this we were made aware, both by the thermometer and by the notably diminished elevation of the spurs of the Cordillera. In fact, on the 27th, we passed the two last falls, but these are the most terrible of all. The former of these is called Chalioncani; the latter, Chilbucani; the latter is an object of dread even to the Antés Indians, habituated though they be to that kind of danger.

At this point the river enters a narrow channel, and on each side perpendicular rocks tower as high as the eye can reach. The waters rush furiously down this pass, which, towards its lower extremity, becomes a gorge only from six to eight metres wide. While the savages passed the canoes along by means of cordage attached to both ends, we followed a narrow ledge running along the left bank. This we found exceedingly difficult to pass, helping each other the best way we could. At last we suddenly came to the end of this parapet, and were horrified to find we should have to embark from the top of a very high rock, worn perpendicular by the action of the water, which was boiling with incredible fury at its base. The Indians passed the canoes along with great skill; but the cordage that attached one of these to the rest, having snapt, it shot down a great way with the speed of an arrow. The whole being brought at last to one place, the embarkation proceeded.

By this time I was so reduced in strength, that I was obliged to have myself bound to a rope before I durst venture to slide down from the top of the rock. An Indian tribe, very little known as yet, the Pauca Pacouris, often lie in ambush among these rocks, with the view of attacking the Antés when engaged in surmounting the natural difficulties of this passage.

Hardly had we emerged from this dangerous pass, when we entered a narrow channel, differing much, however, from the last. Here the water lay perfectly still and dead, as if the river had been exhausted with its long struggle, and felt the need of repose. This formed one of the most picturesque points I ever saw; on each side immense perpendicular schistous rocks, taking the forms of vast towers and ramparts. They impend high over the river, and from their summits descend innumerable cascades, which, ere they reach the surface of the stream below, are dissipated into mere mist and rain, which the rays of the sun tinge with all the hues of the rainbow. In the interstices of the rocks, there is a vigorous growth of tropical vegetation, in which elegant palms form the finest object. No words can give an adequate idea of the beauty of this magnificent landscape.

Here the falls of the Urubamba terminate, and we had henceforth nothing worse to encounter than rapids attended with little danger; and now there lay stretched out before us, the vast wooded plains known as the Pampa del Sacramento. This point is about sixty leagues from Echarate; and should trade ever take possession of the course of the Ucayali, here there should be a post, and a land route should be opened as far as the settlements in the valley of Santa Anna.

The Indians left us on the following night, and we found the utmost difficulty in pursuing our voyage without them. The whole morning was spent without food, when, at last, pressed by hunger, we entered a small stream, the Rio Sabeti, into which we threw a poisonous root used by the Indians for the fish, and shortly a number of small ones, about the size of gudgeons, rose to the surface in a torpid state. Among these there was one of a new kind which I wanted to add to our collections, but its owner refused to part with it until I had relinquished to him all my own share of the capture. This I mention only to shew how far hunger had led us. Not long afterwards some Indians sold us some green bananas which we devoured when hardly cooked in the ashes. One more of the canoes was this day swamped and lost, and with it our last remaining musket.

In the course of the afternoon, we came to a cabin belonging to the Antés Indians, who gave us some lamentin (sea-cow) beef. It was served in a calabash, and all of us, black and white alike, eagerly thrust in our hands for a share. Two of these Indians engaged for axes, sabres, &c., to conduct us as far as the country of the Chataquiros or Piros, called by the Antés Simirinchis.

On the 20th we passed the mouth of a large river flowing from the east ; the Antés call it Camisca.

Although we started before day, we made but slow progress, for the Indians stopped at every step to fish, or to hunt the hoccos or pecaris, a sort of wild boar ; but as they generally consented to sell us part of the proceeds of their chase, we had thenceforward far less to suffer from hunger. Still the preparation of our food without salt gave us great disrelish for it. The Indians wanted to leave us in the evening, although paid for a ten days' journey ; and told us with incredible coolness that they could not go as far as the Choutaquiros, because they had kidnapped several of them, and were they to fall in with them, would most certainly be killed.

We were in this singular perplexity when we saw two canoes coming towards us, which were recognized as belonging to that nation. They contained two families, every individual among whom was painted black all over, so that they looked like negroes. They are thought to be the most thieving tribe of all that inhabit those regions, and it is they that have committed most of the murders that have made the Pampa del Sacramento an object of terror ; but, on the other hand, they are the most fearless of all that navigate the Ucayali, and their expeditions extend from the Urubamba to the Amazon. They accosted us with shouts of satisfaction, and were delighted at receiving knives, mirrors, small bells, and necklaces of glass beads. They undertook to conduct us as far as one of their villages, which they called Santa Rosa, and which they told us lay at the distance of a ten days' journey. I omit the details of our voyage beyond saying that we suffered cruelly one day in consequence of having eaten some poisonous kidney beans which we had found in a forsaken hut.

We were always careful at night to encamp on the left bank of the stream, the right being exposed to the attacks of the Impeteneres or Amahuacas, a barbarous nation at war with the Choutaquiros, and several individuals belonging to which we saw reduced to a state of slavery in the cabins of the latter. Our preparations for the night were of the simplest kind ; the sand sometimes broiling hot, sometimes soaked with rain, formed our only bed, and there we lay till morning exposed to the tortures inflicted on us by the musquitoes. As soon as the canoes were brought to the bank, aware that we had no service to look for from any one, M. Deville and I set off to collect wood for a fire; and severe work it was, in the weak state we were in, to drag to our encampment heavy branches or entire trunks of trees. While we were thus occupied, Florentino drew the canoes ashore, baled out the water, lifted out the stowage, and then cooked our food. During these proceedings, we often met with crocodiles, and were constantly awakened at night by the howlings of the jaguars.

At last, on the 6th September, after having passed the mouth of the Rio Tambo or Apurimac, we reached the small Choutaquiro village of Santa Rosa, which has nothing Christian about it but its name. There we were received with sufficient hospitality, and the Indians agreed to conduct us as far as the Tachytea. On the 8th we came to the large village of Consaya, inhabited also by that tribe, and where we had to submit to numerous thefts without a thought of complaining, to avoid being massacred.

On the 11th of September we reached the Connibos nation, with whom it is the practice to compress the heads of their infant children between two wooden boards, so as to deform them, in order, they say, to give them the shape of the moon, while that of the whites is like the head of a monkey. One of these Indians gave me a small bit of salt.

On the 16th we reached the small village of Connibos of the Pachytea, which stands opposite the mouth of the river of the same name, and is inhabited by the cannibal tribe of the Cashibos. The houses of the Cashibos are remarkable for their immense size; they can generally afford shelter to above two hundred men.

On the 1st of October we left this village, and the same evening the Indians, who had been engaged and paid to go as far as Sarayacu, would proceed no farther. After this, we began daily to experience such embarrassments; sometimes on making fresh payments of articles from our store, or on distributing pieces of money among them, they would agree to accompany us a day or two longer; at other times, they would leave us in the wilderness. It is impossible to put any confidence in what they say: their bad faith exceeds belief.

On the 21st we reached the first huts of the tribe of the Sapibos, whom we fouud more civilized than the preceding tribes, and who conducted us on the 27th to the mission of Sarayacu, situate at some distance in the interior. Such was the state of weakness to which I was reduced, that I could not proceed thither by land, whereupon the Padre Plaza, prefect of the mission of Ucayali, was kind enough to send me a canoe, which was dragged along a brook that led as far as the village. We were received with the utmost hospitality by the worthy old man, who threw himself on my neck, and told me, that on receiving a notice of my journey from the government, he had immediately written in reply, dissuading from the attempt, under the idea of its impossibility. He lost no time in celebrating a thanksgiving mass, and all the Indians of the mission, about a thousand in number, betook themselves to their national dances, which they accompanied with discharges of musketry, in testimony of their joy at our safe arrival. These Indians belong to the Pani tribe.

After recruiting our strength, we set off with M. de Ville for the Amazon, where I shall wait for M. D'Osery till the 1st of January.

The loss of our scientific instruments has deprived us of those highly interesting observations which we might have made upon the river; yet we shall give a report on a series of barometrical observations, pursued even during our most disastrous days, and which will present curious enough results, demonstrating the extraordinary rapidity of the fall of the rivers that descend from the Cordillera, an itinerary of the river, much geographical information and beautiful collections for the museum of natural history.

Be pleased, Mr Minister, to accept the assurance of profound respect with which I am, &c. F. DE CASTELNAU.

MISSION OF SARAYACU, 10*th October* 1846.

Remarks on the Level of the Molasse in the Eastern Alps, and other Geological Topics. Communicated in a Letter to ROBERT CHAMBERS, Esq. F.R.S.E., &c. &c.

VIENNA, 25*th November* 1849.
K. K. Neues Munzgebäude.

DEAR SIR,—I take the liberty of answering your interesting letter of the 8th of June, having, in the mean time, conceived a theory, which, if correct, may become of some importance in your researches. I have here translated a first short notice, published in the Journal of the Friends of Science at Vienna (vol. vi., August 1849.) Perhaps you may think it proper to appear in one of your Journals.

I am glad to hear an Englishman complaining of the exclusiveness with which palæontology is cultivated, to the detriment of real geology, for I myself am devoting my particular attention to the latter, leaving shells to others; and this summer, in particular, I flatter myself to have made a great step in metamorphism, and to have found the general key to it. I have now read your paper on the Rhone Valley, and was sorry to see the question stated in that light, after having been elucidated in its chief points by Necker, De Saussure, and Alphonse Favre, who have shewn that the diluvial terraces of Geneva are genuine river formations, without the shadow of anything like traces of the sea.* The tertiary sea had entirely disappeared when the diluvial period began. The heights I give you are taken from Dufour's splendid military map; being trigonometrically measured, they deserve full confidence as to their correctness. I am much obliged for the notice on your till and drift; but I feel that I must see it with my own eyes to get a clear notion of it. I hope, in course of time, to make an application of your theory on the changes of the relative level of sea and land. The theory of *soulèvemens* of Elie de Beaumont, although true in many cases, will have to be modified. You will easily perceive that my researches on the molasse are not at all unfavourable to you; when combined with what we know of the molasse in the rest of the world, it even leads us to suspect an immense change of 3000 feet over the whole globe, and of which your diluvial changes of a few hundred feet would be the last tip end of the tail. An immense and powerful continent must have been submerged somewhere, even before the subsidence of the Pacific continent. I am glad to be able to furnish you with some data on the west coast of North America, from a letter of my friend, William Fraser Tolmie of Glasgow, surgeon to the Hudson's Bay Company in Oregon. In laying it before the Friends of Science at Vienna, I again recalled to mind your theory, which is strongly supported by the fact of these terraces being now known all round North and South America. My friend writes from Nisqually on Puget's Sound. As he is in hopes of coming over in 1850, you may perhaps see him. I answered his letter, and mentioned your views.

Scotland being such a beautiful *fiord region* might, perhaps, furnish good data to complete the proposed theory. I take

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* Allusion is here made to a paper by Mr Chambers, which appeared in this Journal (January 1849), and in which it was maintained, that a recipient body of water, such as the sea, is required, besides a river, to account for the formation of valley terraces, placed high above the present beds of the river. The author thinks that M. Morlot, from prepossession of mind, may have given too hasty a glance at this paper, and failed to observe the force of its arguments.

the liberty of calling your attention to the subject. You must surely possess most extensive soundings for the purpose of navigation. Believe me to be, dear Sir, your faithful and obedient servant, A. MORLOT.

On the Level of the Molasse in the Eastern Alps.

The younger tertiary formation (miocene), or molasse, forms the low hilly country surrounding the Alps; it shews very distinctly in Lower Styria, a system of equally elevated ridges, which, looked at from a distance, constitute a horizontal level, distinctly cut off by the transition rocks, which arise abruptly out of it to a much greater height. It is thus clear, that the molasse once formed a continuous plain, the subsequent furrowing of which produced the undulated country described. At first sight, one is tempted to consider this clearly-defined level as that of the *tertiary sea* itself, in which it was formed, and which would then, in the neighbourhood of *Gratz*, for example, have stood at about 500 feet above the *Mur*, or 1500 feet above the present level of the sea.

The interior of the Eastern Alps presents the same phenomena. We see there as well as in such wider depressions as Lower Carinthia, as also in the larger valleys, like that of the Mur and Mürz, of the Drava and Sava, more or less horizontal and continuous deposits of the miocene formation, but which reach here much higher than round the outskirts of the Alps, for they attain in Lower Carinthia, and in the wider parts of the chief valleys, as for example, at Tudenburg in Upper Styria, a level of 2500 feet above the sea, whilst they even rise in some more remote and gently ascending side-valleys to an extreme height of 3000 feet; for example, at the Pass of *Obdach*, between *Tudenburg* and *Wolfsberg*, in Carinthia, the very top of which is miocene, and contains the brown coal and impressions of leaves characterizing the oldest strata of the system in the country. Another, and even more singular instance of the same kind is to be seen at Tarvis, when, after following the miocene deposits from Lower Carniola all along up the valley of Sava, you arrive at the highest point of the pass, between Carinthia and Italy, and find here the same formation again, nearly 3000 feet above the level of the sea, and shewing that the miocene *Italian* sea stood in uninterrupted communication with the gulfs or mediterranean districts of Carinthia and Carniola, by a long and narrow arm, crossing the precipitous chain of the Southern Alps, which rise in the Terglou,* to a height of 9000 feet. A narrow side arm of this long channel must have branched off from the valley of the Sava into the *Wochein,* a most picturesque site, representing a deep chasm in the stupendous limestone masses of the Terglou. For here, again, the same deposits are found at about 2500 feet above the sea, and what is of particular import, they contain here not only the usual impressions of land plants but also sea shells (*Cerithium and Natica*) ; so that it is quite certain that even here, in this most retired corner, the water was salt and in communication with the open sea.

Everywhere, in all the known localities, one finds the same subdivisions of the formation. At the bottom, brown coal and marly schists, with fossil plants, mostly dicotyledonous ; as for example, at the celebrated locality of Parschlug in Upper Styria (Mürz Valley), where Professor Unger has found 142 different species, all extinct, but very similar to such as grow now in the southern parts of North America. Next follow grey micaceous sandstones, true molasse, generally passing into and covered by more or less coarse conglomerates. It is thus clear, that we have everywhere the same parallel strata deposited contemporaneously in one and the same sea, not only skirting the outer flanks of the Alps, but reaching far inland into all their depressions and even crossing the chain by those passes, which at present are not higher than 3000 feet, thus forming a most complete *fiord region* (or frith region), as is more clearly illustrated by a map coloured to the purpose.

Now comes the question. Whence does the considerable difference in the known levels of the formation arise, a difference attaining not only as before seen 1500 feet, but which

* *Terglou* or *Triglou,* Slavonian *tri-glava,* three heads.

reaches even near 3000 feet, if we compare the brown coal bed on the Pass of Obdach and the deepest Artesian borings at Vienna, where they have not yet sunk through the miocene formation, although they reach the present level of the sea ?*

If the level of the formation indicates, as at first supposed, the level of the *tertiary sea* itself, it follows that, at the close of the tertiary period, the different tracts of the country must have been upheaved in a very different measure. But whilst there is nowhere in the whole country a single appearance of such unequal local upheavings, the intimate and regular connexion between the height of the level and its situation, more or less inland and remote from the open sea, points to a totally different cause, upon the nature of which Mr Simony's very accurate and complete soundings of the Lake of Hallstadt, near Salzburg, appear to have thrown some light. This irregularly-shaped lake, surrounded by precipitous cliffs, naturally also shews beneath *water-mark* (water-level) steep, rocky banks, which, however, are suddenly cut off at a certain depth by a tolerably even surface, evidently the *plane of deposition* of the nearly horizontal recent strata. Where the lake is narrowed this plane rises nearer and nearer to the surface of the water, so as almost to reach it, and yet without standing in any relation to deltas, which of course are here set aside, and are not considered as being foreign to the subject.

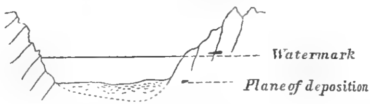
Now, considering the known tertiary deposits, it appears highly probable that their level does not indicate the former level of the sea, but only marks the *plane of deposition*, which being more or less deep, owing to the sea being more or less open, so that the more you advance along the fiords into the interior of the eastern Alps, and the more of course, the bottom of the ancient sea was shallowing, the more you see the level of the tertiary deposits arising, until they attain an extreme height of 3000 feet, which will have been but little below the *water-mark*.

To avoid all misunderstanding, let it be remarked, that there has been no question here of diluvial terraces, which

* Vienna lies 545 English feet above the sea.

regularly following the present water-courses, and being formed by them *after* the tertiary sea had disappeared from the country, really mark the former higher *river-level* itself. We are thus led to distinguish two sorts of levels of deposition: *First*, In *standing* water, in seas or lakes, at a certain depth below the surface, dependent on the configuration of the water reservoir; *secondly*, as produced by *running water*, rivers, or torrents, and, in this case, marking the highest level attained by the water. Sea-beaches, not forming strata of much importance, and only tracing a line along the shore when this is not steep, have not been considered here. If they existed in the tertiary seas of the eastern Alps, they must have been easily obliterated during the diluvial period.

Lake of Hallstadt.



Letter from William Fraser Tolmie, Esq., Surgeon in the Service of the Hudson Bay Company.

The superficial formations along the shores of Puget's Sound, Admiralty Inlet, and by the southern side of the Straits of Juan de Fuca, to within a few miles of Cape Flattery, is in some places, as here, a gravel bed of from 200 to 400 feet in thickness above the level of the sea; in more numerous localities it is a whitish-yellow, loamy earth, this being the character of the cliffs and promontories of the western coast of Whidbey's Island, and, as Captain Wood, of H. B. M's. surveying vessel, informed me, from Port Discovery to the neighbourhood of Cape Flattery, which portion of the coast he surveyed in 1846. The prairie in general, from Nisqually to Puyallip, is elevated from 200 to 350 feet above the sea-level, and its ascent is sudden and steep. All the clumps or belts of pines not bordering the river-banks, the sea, or lakes, grow on elevated knolls or table-lands, many of which have only a covering of grass and scattered oaks; some are raised 50 or 60 feet above the level of the prairie, and it is remarkable that the soil of all is more free of gravel and of better quality than that of the plains. Springs burst from the sides of some of them, and small lakes are found on the summits of some of the principal. In proceeding from Nisqually to Puyallip, five terraces, running across the prairie in an easterly and westerly direction, are descended, and only one inclining in a contrary direction is to be met with.* The occurrence of terraces is observable in the prairies south of Nisqually, but isolated mounds are less frequent than hereabouts.

NISQUALLY, 2*d December* 1848.

On the Limits of the Chalk-Formation. By M. DE BUCH.

The low latitude to which the chalk strata have attained, when compared with the jurassic formations, and still more with the palæozoic rocks, has been considered by M. Boue, with much appearance of truth, as the most ancient action of climateric influence on the fauna of the ancient world. The most northern point of the world at which chalk has hitherto been found, is, according to M. Forchhammer's observations, the neighbourhood of Thistedt, in Jutland, that is to say, near the 57° north latitude, nearly on a line with Aberdeen, in Scotland, Calmar Mittan, Twer, and Casan. Chalk does not reach so high a latitude in England ;† the last point where it is observed is the southern side of Rathlin Island, at the Giant's Causeway, in the latitude of Apewiade, Bornholm, and Tilsit. Flamborough Head, at 54° north latitude, is its extreme limit in England. In Russia, its limits become lower towards the south. From Grondo, where the

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* Judging from a rough topographical sketch, M. Tolmie means one of the five terraces, not a sixth ; but I am not sure.

† The occurrence in Buchan, in Aberdeenshire, of tracts of country with chalk flints containing the usual animal remains, may be held as an intimation of the probable existence there of portions of the chalk-formation in some of the hollows at present covered up by diluvium. Many years ago we examined Buchan. We trust that ere long the existence or non-existence of deposits of chalk will be determined. Mr William Ferguson, in his amusing published Notices on Buchan, says he had not met with chalk.

chalk still appears at 54° north latitude, it descends, as may be seen by M. Murchison's beautiful geographical map, by Mohilew and Orel, half a degree to the south of Moscow, then to Simbirsk and the Volga, as far as the Caucasus. It was altogether an unexpected event that Messrs Murchison, Verneuil, and Keyserling again discovered this chalk on the banks of the River Ural, 20 German miles below Orenbourg, at 51½° north latitude. The Muchodjar mountain appears to be the limit of these formations towards the east. The prodigious extent of Siberia, from the Ural as far as Ochotzk, and from the Altai to the Icy Sea, has been so well and carefully examined by mining engineers, naturalists, and searchers for gold, that we have reason to doubt the existence of chalk strata over this vast extent of country.

Everywhere within these limits, the upper chalk appears to be the bed distinguished principally by *Gryphæa vesicularis, Belemnites mucronatus* and *mamillaris, Inoceramus Cuvieri* and *Cripsii, Ostrea diluvii, Terebratula carnea* and *semiglobosa, Ananchytes ovata, Galerites vulgaris* and *albogalera*, and their analogues. The old chalk strata appear as we descend to the south; and in the Caucasus and Daghestan, these ancient (neocomian) beds seem to reach, according to the excellent researches of M. Abich, a thickness of nearly 5000 feet. It is as if a vast wave had descended from the summit of the Caucasus, extended itself forwards, and expired by degrees on the plain, at the limits of the ancient rock formations.

On the other side of the ocean, the chalk-formations terminate in the Atlantic portion of the United States, before reaching the city of New York, so that their limit scarcely rises to 40° of north latitude, 16° less than in Europe. In Kentucky and Tenessee, this limit is below 37° north latitude. But it is otherwise in the Missouri, where the great river of that name flows without interruption from the foot of the Rocky Mountains, over an extent of 1400 miles, through formations of chalk, at least as far as the mouth of the River Sioux. This, at all events, is what we learn from the Memoirs and Collections of Prince Neuwied, and the Report of Nicollet, the astronomer. It appears, then, that, in the western part of America, the chalk strata reach to 50° of north latitude, that is to say, 10° higher than the east coast. Their development, also, in this place, is greater than that of any other formation known on the surface of the globe. Captain Fremont has seen, in the neighbourhood of the River Plate, chalk strata with scattered specimens of *Inoceramus Gripsii*. They have likewise been found in the Arkansas, and as far as Santa Fe, in New Mexico, near Monterey and Laredo, by Dr Vislicenus, as appears from reports made to the Congress of Washington in 1848. In the Rocky Mountains and their prolongations to the east of Santa Fe, in New Mexico, this chalk-sea appears completely interrupted. Captain Fremont could discover no traces of it towards the River Columbia, nor towards the River Humboldt, in the singular great basin which extends as far as the Western Ocean.

It is further observable, that here this vast formation of chalk consists only of the superior beds. According to very extensive and correct researches, Sir Charles Lyell is of opinion, that throughout the whole of North America, only strata of chalk are met with, which extend from the chalk of Maestrich to the *gault;* and M. Ferdinand Römer, in consequence of his accurate researches in the Texas, goes so far as to affirm that all the formations of that country, which is already sufficiently remote from the Atlantic coast, ought to be considered only as superior beds, which do not even belong to the *gault*.

These surprising phenomena are, however, limited to North America. Even in Mexico we see deeper beds make their appearance.

M. Galeotti has brought back from Tehuacan, towards the limits of the province of Oaxaca, trigoniæ, which he has described as the *Trigonia plicatocostata* (Bulletin de Bruxelles, iii., No. 10). This trigonia belongs to the subdivision of the *Trigoniæ scabræ* of Agassiz, and differs little from the *Trigonia aliformis*, Sow. It is characteristic for the middle chalk, chloriteous chalk, and even the gault. According to M. Galeotti, it is found in the middle of the great cordillera of Anahuac, 12 leagues to the north-west of Tehuacan, in such great abundance that it may be considered as characteristic in regard to the whole of this formation. One is surprised, he says, to meet with such great masses of fossil shells, so many fragments of ammonites of many feet in diameter, or stalks of gigantic corals in these localities to such a degree, that there is not perhaps, in the whole surface of the earth, a point where, over so many leagues, we find such a mass of organic remains. This trigonia reappears in South America, in the mountains of Santa Fe de Bogota, whence M. de Humboldt brought it to Europe for the first time. These mountains of Santa Fe present, in the most evident manner, judging from the products found in the formations, the middle stage of the chalk. This I have endeavoured to shew in the description of M. de Humboldt's Collection of American Fossils (Berlin, 1849); and the proof has been given at still greater length by M. Alcide d'Orbigny, in his learned and well-considered work on M. Boussingault's collections. As all the formations of chalk in New Granada reach a thickness of 5000 feet, it ought not to appear surprising that we there meet with organic remains of the inferior stage of the chalk, the neocomian formation. M. d'Orbigny has described an *Exogyra* from Socorra, which differs in nothing from the *Exogyra Couloni* of the neocomian formation. The same exogyra has been collected in abundance by the late Meyen, on the declivity of the volcanoes of Maypo, in Chili, at a height of 13,000 feet, but it has been only imperfectly figured (*Acta der Leopold Acad.* xvii., p. ii., 649, t. 27. f. 5). Darwin (*Geolog. Observ. on South America,* 1846) has likewise found it not only at a little distance from Maypo, in the pass leading to Portillo, in the Penquene chain, but also 60 English miles beyond, towards the north, in the pass of Uspaletta. The *Exogyra Couloni* or *aquila* is nevertheless a shell, truly characteristic of the neocomian formation.

All that Darwin has collected among the mountains above Copiapo and Coquimbo, in the north of Chili,—all that M. Domeyko, professor of mineralogy at Coquimbo, has sent to Paris, belong to the recent chalk-formations, and are met with even at a great distance from these localities, beyond the great transition nucleus of Titicaca, which penetrates with the ancient rocks into the chain of the Andes. Among these forms, the most remarkable is the beautiful *univalve* which M. de Humboldt first brought from San Felipe, to the south of Quito, near the Amazon river, and to which I have given the name of *Pleurotomaria Humboldtii* ; M. d'Orbigny, and Darwin after him, calls it *Turritella Andii*, although it is still doubtful whether this is done with propriety. It seems to be quite peculiar to South America. Darwin likewise met with it in abundance in the formations which extend from Coquimbo to Rio Claro and Arqueros, and even beyond Guasco and the Amolanas, the principal valley of Copiapo. This Pleurotomaria is always associated with the pectens, which are likewise found in the northern portion, between the Montar and Guancavelica, in such great abundance as to form fields of fossils and entire mountains, which have been long and generally known under the name of *Choropampas* (*Pecten alatus* Dufresnoyi, d'Orb.). It is the same which Ulloa in 1761, places, to his great astonishment, at so great a height above the level of the sea, where it seems to compose mountains of shells. This astonishment was expressed in all books till it was perceived that it was not absolutely necessary that the shells should have lived at these heights, but that they may have been raised upwards from the bottom of the sea. As the *Hyppurites organisans* (d'Orbig., p. 107, t. 22) is met with in the middle of the beds of Pectens, it follows that all these beds in Perou as well as at Coquimbo and Copiapo, belong at least to the gault, an opinion which appears clearly justified by an Exogyra which M. Domeyko has sent to Paris. It is, in fact, in every respect like that which has been described and figured by Morton, *Gryphæa* (*Exogyra*) *Pitscheri* of the Texas, the position of which M. Ferdinand Römer has placed above the gault, at Friedrichsberg.

The deeper chalk strata, analogous to those of Aconcagua, are not, however, unknown in the Andes of Lima. The celebrated zoologist, M. de Tschudi, has found on the eastern acclivity of the mountains, between Oroja and Yaui, near Tarma, and after him many other travellers have likewise found neocomian shells well characterised. *Pterocera Eme-*

rici (d'Orb., Pl. 216), *Conoidea Glifs, Holaster dilatatus* and *H. complanatus* or *Spatangus retusus,* both recognised by M. Agassiz, *Diadema Bourgeti,* likewise met with at Neufchatel, *Pecten cretosus,* Brong., and *Pecten quinquecostatus.*

It would appear, therefore, that the chalk-formation in South America is developed in a different manner; that it is thicker and more varied than to the north of the Gulf of Mexico, and that its resemblance to the chalk-formations of Europe is much more complete among the Andes. But it is a very remarkable circumstance that, in North America, the chalk strata, altogether horizontal, occupy a considerable surface, and consist in a great measure of clay and sand, and other masses by no means compact. In South America, on the contrary, we only meet with black limestones or compact sandstones, the cohesion of which is such that this may be often taken for true quartz, such as those found between the Maranon and Lima, where these beds, far from affecting a horizontal position, are all, more or less, inclined; a violent situation which can only be the result of the action of powerful forces. Throughout the whole of Brazil, in the vast extent of La Plata, Paraguay, and Bolivia, we no longer find the chalk, and none exists.

Darwin has followed the chalk-formations to the extreme point of the continent. He has seen chalk-shells in quantity at the summit of Mount Tarn, 2000 feet in height; at Port Famine, in the Straits of Magellan, and at 53° of south latitude, consequently three degrees of latitude higher than at Missouri. The *Ancyloceras simplex,* d'Orb., and the *Hamites elatior,* Low, leave no doubt as to the nature of this chalk. The Hamites is even, says M. E. Forbes, one of the largest shells I have ever seen; it is 2½ inches at its greatest diameter. Darwin's discovery probably indicates the southern limits of the chalk-formation; and the polar influences, consequently, seem thus to be opposed, at this point, to a great development towards the pole.*

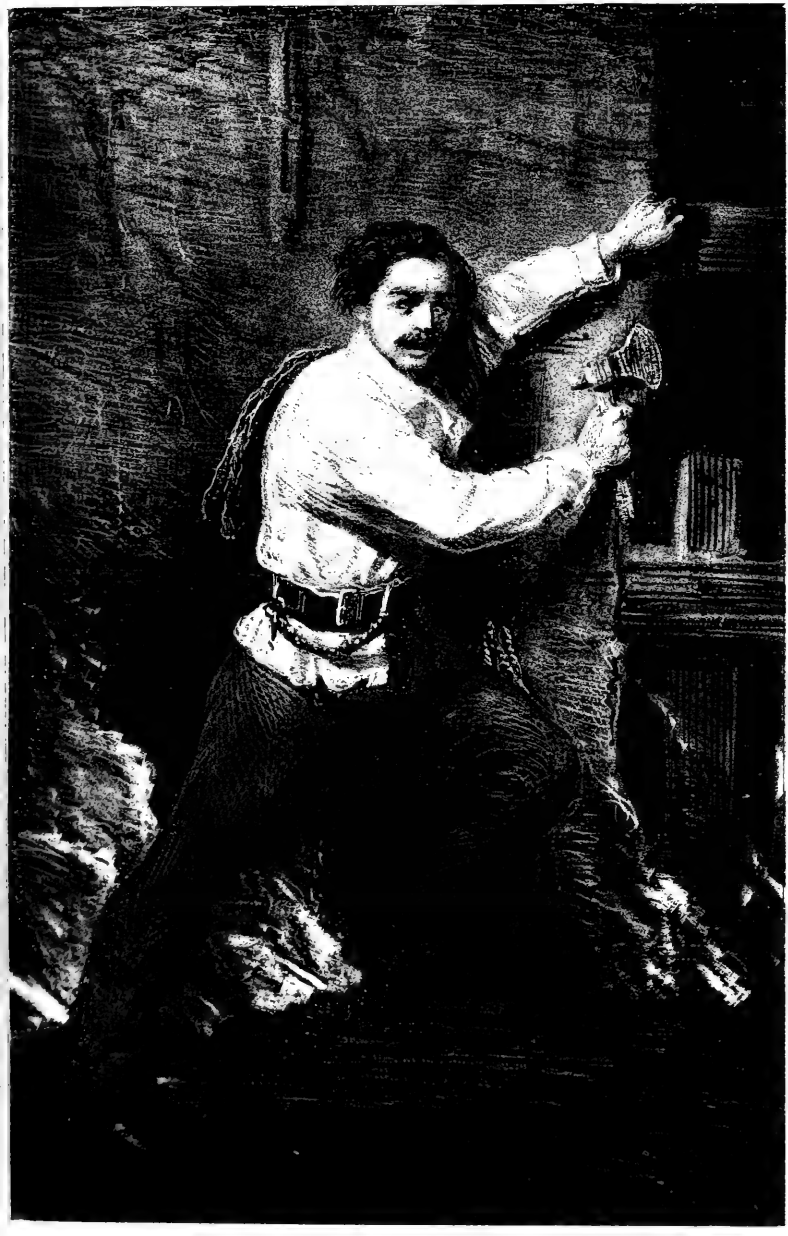
* Monatsbericht der Konigl. Preuss. Academiæ der Wissenschaften zu Berlin, March 1849; also L'Institute, No. 821, p. 306.

On the Geological Signification of the word "Flysch." By M. Studer.

The name *flysch*, proposed for the first time in 1827 by M. Studer, has since given rise to much confusion. M. Studer, going back to its origin, thus gives the history and explanation of it.

The unfortunate name *flysch* appeared for the first time in 1827, in two Memoirs on the valley of Simme, inserted in Leonhard's Journal, and in the Annalles des Sciences Nat. It was a local denomination which I proposed to designate,—a somewhat complex calcareo-slaty formation in the Simmenthal, covered with Portland limestone. After this, M. Alexander Brongniart, to whom I had sent the Portland fossils of Simmenthal, made the mistake of referring these fossils to the *flysch* formation, which was thus arranged among the highest of the jurassic formations. The following year, M. Keferstein (Teutschland, v. 559) availed himself of this name to designate, by a single expression, almost the whole limestones of the Alps, arenaceous and slaty, which he thought proper to consider as constituting a single formation, corresponding in the geological scale to the inferior cretaceous formation of the north of Europe, but including all the series of fossils from the carboniferous limestone to the tertiary formations (*Naturgeschichte des Erdkörpers*, i. 276.) In my work on the Western Alps of Switzerland, which appeared in 1834, I described, as lying between the lakes of Thun and Geneva, three zones of marly-schistose formations, composed of rocks almost identical, and containing the same Fucoides, but whose parallelism did not appear to me evident. That I might prejudge nothing, I indicated these three zones by different names; calling the formation which composes this chain, and which appears to dip under the Portland chain of Spielgarten, *Niesen slates and sandstones.* I reserved the name *flysch* for the formation of Simmenthal superior to this chain; and, in applying the name of *Gurnigel* sandstone to the formation superior to the Châtel or Oxford limestone, which forms the exterior limit of the Alpine country, —observing, at the same time, that nothing prevents us regarding these two latter formations as identical. About this same time, in the Autumn of 1833, I made my first excursion with M. Escher among the mountains of Entlibuch, on which I made a report, inserted in Leonhard's Journal for 1834. We ascertained that a thick formation of marly slates and fucoidal sandstones, differing in nothing, as regarded the rocks, from the *flysch* of Simmenthal, covered the nummulitic formation of the cretaceous chain of the Neiderhorn, Schratten, and Mont-Pilate; and, dating from this period, confusion, hitherto unknown in Swiss Alpine geology, began to be introduced into our own publications.

M. Escher gave a precise geological meaning to the name *flysch*,



L. Ghémar, comp. & del.

Specimen of Lithography newly discovered by Mr. Schenck & Mr. Ghemar Edinburgh.

by restricting it to the slaty arenaceous fucoidal formation, which covers the nummulitic formation among the Alps and Apennines. For my own part, I felt the need of a petrographic name to designate the whole of the slaty and arenaceous rocks which, among the Alps, entered between the different limestone chains and the masses of gneiss and protogine, and whose geological position remains uncertain; because the fossils found there are insufficient to determine their age,—as in Maurienne, Tarentasia, in the Valais, in the Grisons, and other parts of the Alps. Finding the formation superior to the nummulites described under the name of Macigno and Albérèse, by M. Pareto and other Italian geologists, I proposed to adopt this name with the epithet "Alpine;" and I named "Alpine Macigno" what M. Escher called *Flysch*, while I thought we should reserve the latter name to designate systems of rocks very similar to the true macigno, but whose age and geological position remain undetermined.

I adopted this latter nomenclature in all that I have written since 1840, while, in the Memoir on the Alps of Lucerne, inserted in the Memoirs of the Geological Society of 1838, I conformed to the nomenclature adopted by M. Escher. According to my manner of speaking, there may be *flysches* of all ages; the name will be allowed to drop, in relation to each group whose geological position is definitively fixed by agreement of fossils and position; and, if it be possible for us to attain this object, in regard to all the Alpine groups, the name *Flysch* will be at last discarded from our geological terminology.

Explanation of the Treatment of an Invention in Lithography made by Mr Schenck and Mr Ghemar of Edinburgh, in August 1849. (With a Lithographic Plate.) Communicated by the Royal Scottish Society of Arts.*

A grained lithographic stone is a little warmed, then the composition used for rubbing in tint-stones, known to the generality of lithographers, mixed with an addition of white wax and a little copal varnish, is rubbed down with a piece of coarse short-haired flannel, or coarse cloth, until the colour becomes an equal brown grey. After this the drawing is either sketched upon the stone with soft lithographic chalk, or traced in the ordinary way with red paper. The lighter

* Read before the Society, 10th December 1849.

parts may be rubbed lighter in colour; the highest lights are taken out with a scraper, which is also used to blend the finer tints carefully together; darker parts can be rubbed in darker, and finished with softer or harder lithographic chalks. The darkest parts are laid in with liquid ink with the brush or pen, after which the stone is strongly prepared with acid, and thus in a short time a very powerful design can be produced, as exemplified in the heads of Cranmer, Buchanan, &c. (*Vide* the volume of Leading Reformers, published by Oliver and Boyd, Edinburgh; and Ackermann and Co., London.) Drawings executed in this manner are easily printed, and stand large numbers of impressions.

The important merit which this invention possesses, consists in its taking advantage of the chemical composition of the lithographic stone, and the chemical nature of its printing. Upon this basis we have made trials, and have succeeded beyond our most sanguine expectations, that is, to produce almost instantly the middle tints of any surface, *and which do print*,—a result which is precisely that which, in every other mode of printing, requires a considerable time, and to finish with ease a drawing in a spirited and artistic manner in a brief space of time, and with comparatively little previous practice.

The reproach which has often been justly made against lithography, relative to its gray tone and want of colour, need no longer be advanced, as those defects are from henceforth happily overcome; and it may now be safely averred, that the chief feature in that beautiful art will, in future, be the remarkable ease with which great power, depth, and brilliancy of tone, together with a variety of texture, can be attained; features so important in drawings, and which can now be produced in an incredibly short time (about one-tenth, often one-twentieth part of the time required to do *finished* lithographs in the ordinary chalk method.)

Painters will, in future, have it in their power, with comparatively little practice, to immortalize their names in their works, even as Rembrandt, not with the etching needle, but by the simple process of rubbing, the use of the scraper, stump, chalk, the pen and brush; nay, even by rubbing with their fingers on the stone, and will produce lithographs combining a richness of colour, with variety of tint, of which no Rembrandt can boast, and an artistical merit of touch the etching needle could in vain attempt.

A Michael Angelo or a David Wilkie could exercise their talents in producing original drawings of large size on stone within a day or two, possessing a strength of which no mode of printing has hitherto shewn proof, and producing an effect equal to a painting.

Publications on an extensive and frequent scale will now become possible things. The rapidity of execution and consequent cheapness, together with the pleasing and novel effect, must give an impulse to the diffusion of works of art and genius, which a mere question of time and expense has hitherto rendered almost impossible.

I do not desire that lithography should be either imitative of, or pretend it will ever supersede, copper, steel, or wood engraving. I claim for it an entirely independent position among the Fine Arts, which it will now easily obtain from the assistance of great painters, sculptors, and architects.

From the simplicity of its treatment, the beautiful results obtained, the immense improvements it will effect on lithography, and its useful influence on art in general, I trust I have in some measure been able to convince the public of the value of the invention I have been endeavouring to explain.

FR. SCHENCK, F.R.S.S.A.

[The Messrs Schenck and Ghemar's valuable discovery, explained in the above communication, promises to do more for the general spread of lithographs in this country, than any improvement hitherto known to us; indeed, from what we have heard, it appears to be one of the most striking improvements in lithography since the first discovery of the art in Germany. The wonderfully short time occupied in producing lithographs of a high standard, by Messrs Schenck and Ghemar, has astonished every one. The small lithograph, Plate V. of the present number of the Journal, was drawn, lithographed, and cast off in three hours. The elegant work in 4to, entitled "Portraits of the Leading Reformers," now in the course of publication by the inventors of the new process, shews the powerful effect produced by characteristic lithographs.—*Edit.*]

Notice of a Chromatic Stereoscope. By Sir DAVID BREWSTER, K.H., F.R.S., V.P., R.S. Edin. Communicated by the Royal Scottish Society of Arts.*

In the year 1848, I communicated to the British Association, at Swansea, a brief notice of the principle of this instrument.

If we look with both eyes through a lens, about 2½ inches in diameter or upwards, at an object having colours of different refrangibilities, such as the coloured lines on a map, a red rose among green leaves, or any scarlet object upon a blue ground, or, in general, any two simple colours not of the same degree of refrangibility, the *two* colours will appear at different distances from the eye of the observer.

In this experiment, we are looking through the margin of two semilenses or virtual prisms, by which the more refrangible rays are more refracted than the less refrangible rays. The doubly-coloured object is thus divided into two as it were, and the distance between the two blue portions is as much greater than the distance between the two red portions (red and blue being supposed to be the colours), as *twice* the deviation produced by the virtual prism, if we use a large lens or two semilenses, or by the real prisms, if we use prisms.

The images of different colours being thus separated, the eyes unite them as in the stereoscope, and the *red* image takes its place nearer the observer than the *blue* one, in the very same manner as the two nearest portions of the dissimilar stereoscopic figures stand up in relief at a distance from their more remote portions. The reverse of this will take

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* Read before the Society, 10th December 1849.

place if we use a concave lens, or if we turn the refracting angles of the two prisms inwards.

Hence, it follows, and experiment confirms the inference, that we give solidity and relief to plane figures by a suitable application of colour to parts that are placed at different distances from the eye.

These effects are greatly increased by using lenses of highly-dispersing flint glass, oil of cassia, and other fluids, and avoiding the use of compound colours in the objects placed in the stereoscope.

On the California Gold Region. By Rev. G. S. LYMAN.*

From the western base to the summit of the range of the Sierra Nevada, is a distance generally of a hundred miles, or more. The western slope is broken and precipitous, and through the deep ravines that abound, flow the numerous mountain-streams that form the tributaries of the Sacramento and San Joaquin rivers. The gold region is a longitudinal strip or tract, from ten to forty miles in width, lying about midway, or a little lower, between the base and summit of the range, and extending in length a distance of many hundred miles; active operations being already carried on through an extent of four or five hundred miles at least. The gold mines near San Fernando in a spur of the same range, and which have been known and worked to some extent for many years, are doubtless a part of the same great deposit.

On approaching the gold region from the valley of the Sacramento or San Joaquin, soon after leaving the plain, the attention is arrested by immense quantities of quartz pebbles, slightly rounded, and of the size of walnuts, scattered over the gentle elevations which form the western base of the Snowy Mountains.† There is here but little soil; the earth is of a yellowish-red colour, and nearly destitute of vegetation. Nearer to the gold deposits the quartz pebbles

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* In a letter to one of the Editors of the American Journal of Sciences and Arts, vol. viii., No. 24, Second Series, p. 415.

† See Observations by J. Dana (2), vii., 257, 261.

become larger, and not unfrequently boulders are noticed of considerable size. The quartz is so uniformly associated with the gold, that even the most unscientific explorer would not think of looking for the metal where quartz did not abound. Passing up the mountains, it is easy to tell when you leave the region of gold from the sudden disappearance of the quartz. In August of last year, in company with Mr Douglas and others, I ascended from the " dry diggings" near the Rio de los Americanos, to within a few miles of the snow, enjoying in the highest degree the sublime scenery presented by lofty and precipitous mountains, separated from each other by dark, deep ravines, and wooded with primæval forests of towering firs and pines. The back bone of this mountain range is granite, the several varieties of which constituted almost the only rock visible in the last few miles of our journey. In descending, we passed successively several forms of gneiss and other primitive and transition rocks, till we reached the slate-formation which prevails in this part of the gold district. We penetrated on this occasion some forty or forty-five miles beyond the " dry diggings ;" and after leaving the quartz twelve or fifteen miles up, scarcely a particle of gold was discovered.

As I have mentioned, the prevailing rock of the gold region, near the Rio de los Americanos, is slate. There are many varieties of it ; some shaly and friable, others hard and massive, somewhat resembling greenstone. The laminæ of the slate-beds are nearly perpendicular, and their direction about NNW. and SSE , or nearly the same as the direction of the range. These slate-beds often include dykes and beds of quartz rock several feet in thickness. At the dry diggings above named, I passed at right angles over the upturned edge of continuous strata of slate, a distance of four or five miles ; and in the same direction, slate-beds occur several miles farther on, but I had not the means of knowing that they were part of the same great deposit.

In some of the richest explorations yet made, this slate-formation immediately underlies the stratum of drift or diluvium which contains the gold, and much of the gold is found in the crevices of the slate, the rough edges of the upturned strata forming innumerable receptacles or "*pockets*," as they are called, into which the metal has originally found its way from its own gravity assisted by aqueous agency. It is this accidental association of the gold with the slate-rocks which has caused the statement to be frequently made, even by persons of much general intelligence, that the gold exists in the body of the rock itself, and forms a component part of it, in the same sense that iron pyrites forms a part of the rocks in which it occurs. But I have no where seen gold among the slate, except in circumstances where its presence could be accounted for by its introduction from without, a close scrutiny readily discovering some cleft or opening through which it might have entered. The richest of these "pockets" are in the bottoms of sharp ravines, which seem to have been notched into the body of the slate, and generally in situations where the bottom of the ravine, after descending at a considerable inclination for some distance, becomes more nearly horizontal. Just below a sudden descent or precipice, in the bottom of a dry ravine, gold is often found in the cavities in great abundance. From such a spot Mr Douglas extracted a pound of gold in a few hours, even after the place had been previously "dug out," as was supposed, and abandoned.

I have noticed, in published accounts, many erroneous statements respecting the geological position of the gold. Some have said there is no porticular formation in which the gold occurs, but that, in different places, it is found in different kinds of earth or rock. You will not need to be informed that this is without foundation. So far as I have been able to examine, or can learn from competent witnesses, there is but one geological formation with which the gold of the Sierra Novada is associated, and in which it uniformly occurs. This is the stratum of *drift or diluvium*, composed of a heterogeneous mixture of clay, sand, gravel, and pebbles, and varying in thickness from a few inches to several feet. Here, as elsewhere, this stratum is neither horizontal nor of uniform slope, but conformed to the varying inclination of the earth's surface, covering the declivities, and even the summits of the hills, as well as the bottoms of the ravines and valleys. Out of this stratum I have nowhere found gold, except where a stream has cut its way and made its contents a part of some alluvial formation of comparatively modern date. The sand-bars of some of the mountain torrents, and the gravelly projections formed at the bendings of the streams, are often extremely rich in metal. A bar in the Rio de los Americanos (at high water an *island*), about 23 miles above New Helvetia (now called Sacramento), and on which some of the earliest explorations were made, is of this character. But where the diluvium has remained undisturbed since the period of its deposition, I am confident no "alluvial" or "stream" gold has been, or will be discovered, except in connection with it. It is evidently as much a part of this formation, as associated quartz, greenstone, hornblende, and other pebbles; and whoever will explain the origin of the one, will at the same time elucidate the origin of the other,—for one and the same agency unquestionably spread both of them over the surface of the district. How the latest theory of geologists is to account for the dispersion of drift, I am too isolated from the scientific world to know. Quartz is the only substance with which I have seen the gold intimately united, and these compound lumps seem to shew clearly that the original *matrix* or veinstone of the metal was a dyke or bed of quartz rock. And we have only to suppose, that when the quartz, with its accompanying rocky strata, was broken up by natural agencies at some former geological epoch, the interspersed or included veins of gold were at the same time reduced to fragments, and these rough and angular fragments subsequently broken, and further comminuted and rounded by mutual attrition, to account for the present form and appearance of the gold, and for its constituting a portion of the materials of the drift. But whether these materials, with their golden treasure, now occupy the precise geographical position of their parent rocks, or whether they have been transported by aqueous or glacial agencies, or both, from some neighbouring or perhaps far distant locality, is a question which future investigations into the geology and physical geography of the region will better elucidate than the imperfect data at present in my possession. I cannot avoid the fancy, however, in connection with the glacio-aqueous theory, that when the continent was wholly or partially submerged, the materials of the diluvium, including the gold, were transported by icebergs from their present locality, and when at length set free, left to assume their present position on what was then the rocky and uneven bottom of the superincumbent ocean. And we have only to imagine these freighted icebergs stranded by oceanic currents against the partially-emerged range of the Sierra Nevada, to account for the great longitudinal extension of the gold region along the western slope of the mountains, while laterally it appears to extend neither above nor below certain definite limits.

The gold of different localities varies very much in size. That from the banks and sand-bars of the rivers is generally in the form of small flattened scales, and commonly it is found to be finer the lower down you descend the stream. That taken from the bottoms of the dry ravines, which everywhere abound in these mountains, and furnish outlets for the torrents of the rainy season into the principal streams, is mostly of larger size, and occurs both in small particles and also in small lumps and irregular water-worn masses, from the size of wheat-kernels to pieces of several ounces or even pounds in weight. The fine gold of these ravines is commonly less worn and flattened than that in the alluvion of the rivers; and the flattened scale-like form of the gold in these latter deposits, would seem to be owing to the great malleability of the metal,—the stones and pebbles, among which the minuter particles and fragments of the original vein of native metal chanced to lie, and by which they were rudely hammered, having performed very effectually the goldbeater's office, and gradually reduced the rough angular particles on their granite anvils, to the flattened spangles which we now observe. Some of these flakes are often an inch or more in diameter, and scarcely thicker than paper. Many specimens bear the distinct impression of the crystalline structure of granite and other rocks; and I have seen several pieces deeply stamped, as with a die, by crystals of quartz, the form of the crystal being as distinctly apparent as the device on a gold eagle fresh from the United States' mint.

The black, ferruginous sand, which everywhere accompanies the gold, and which, from its great specific gravity, remains with it in the bowl or machine after the other earthy materials have been removed, varies in fineness with the size of the accompanying gold,—that obtained in connection with the fine river gold being of the fineness of writing sand; while that associated with the coarse gold of the ravines is often as large as wheat-kernels, or peas, and sometimes of the size of hazelnuts or walnuts. These coarser pieces are fragments of crystals very hard and heavy. I found no specimens with the faces complete, and have not the means of knowing to what species they belong, but suppose them to be magnetic iron. That the fine sand is composed of fragments of the same crystals greatly comminuted, I infer from the regular gradation of the one into the other.

I am not aware that gold has yet been discovered *in place*, or imbedded in its native matrix. The slates, however, of the gold region, as I have before observed, are often traversed by dykes or beds of quartz rock; and I have examined these in many places for indications of the presence of the metal, but could detect no traces of it. Individuals have asserted that they have found veins of it in the rocks, but they have refused to divulge the place where, inasmuch as they intended to work the veins themselves as soon as the season would permit. Though these statements are, of course, not impossible, nor indeed improbable, I do not consider the fact as established by testimony, since the witnesses are men in whom I place but little confidence.

The amount of gold taken from these mines it is impossible to estimate; but it has been immense, and the coming season it will doubtless be greater. New and rich deposits are developing every day. Accounts from various points in the mining district, represent the gold as very abundant, more so, if possible, than last year,—individuals, even that early in the season, obtaining often from three to ten or even twenty ounces a-day. The diggings on the several forks of the Rio de los Americanos, the Stanislaus, the Tuwalumnes, the Merced, the Mariposa, King's River (Lake Fork, on Fremont's new map), and in many other places, are represented as peculiarly rich.

There was one specimen of gold, mingled with quartz, found near Stanislaus last autumn, which I had resolved to procure, if possible, for the cabinet of Yale. It was irregular in form, about 4 inches in diameter, and weighed $5\frac{1}{2}$ pounds avoirdupois. The metal was interspersed in irregular masses through the stone, and, as near as I could judge without special investigation, was equivalent to about 2 pounds troy, perhaps a little more. Other specimens, much larger, are said to have been found, and one of 20 pounds weight pure, near the Stanislaus; but these I have not seen.—(*American Journal of Science and Arts*, vol. viii., No. 24, 2d Series, p. 415.)

On the Identity of Sillimanite, Fibrolite, and Bucholzite, with Kyanite.

Sillimanite was originally described by Bowen,* from an analysis made in Yale College Laboratory, in 1825, which, shewed it to be a silicate of alumina with a proportion of silica too high to allow it to come within the formula of Kyanite. It was subsequently analysed by Dr Thomas Muir, in the laboratory of Dr Thomson, who found in it a large quantity of zirconia, an observation which all subsequent researches have failed to confirm. Since that time, it has been analysed by various chemists, viz., by Connel, Norton, Staff, Hayes, and Thomson. The most recent of these analyses which has been published, is that by Thomson, who reports it to contain 45·65 per cent. of silica. We have, then, the following discordant results in the amount of silica found in Sillimanite by different chemists, in the order of their publication:—

	Bowen.	Muir.	Connel.	Norton.	Staff.	Hayes.	Thomson.
Per cent.	42·67	38·67	36·75	37·40	37·36	42·60	46·65

The cause of this disagreement will undoubtedly be found in the difficulty of effecting a complete decomposition of anhydrous silicates of almunia, which contain a high per-centage of alumina. This decomposition can be completely effected only by the aid of caustic potash, applied to the mixture of carbonates and the mineral during the fusion, as first recommended by Berzelius, or by fluo-hydric acid.

* Journal Acad. Nat. Sci. Phil., iii. p. 375.

Select crystals of this mineral were taken from the original locality at Chester, Conn, and their analyses afforded the following results. Quantity taken, 775·5 grammes. Found—

Silica, ...	0·292	= 37·653	per cent.
Alumina, ...	0·484	= 62·411	...
	0·776	100·064	

			Required.
2 Atoms Silica, . .	1154·62	= $Si\ O^3$	37·47
3 Atoms Alumina, .	1927·00	= $Al^2\ O_3$	62·53
	3081·62		100·00

These results give, then, exactly the formula of Kyanite, viz., $2\ Al\ O_3$, $3\ Si\ O^3$. The analyses of Staff and Norton give also the same results.*

We can, therefore, have no longer any hesitation in referring Sillimanite to Kyanite, as originally suggested by Haidinger.†

Bucholzite, is a name given by Brandes to a silicate of alumina from Tyrol, which occurs in compact masses, of a finely fibrous structure and hardness, equal to Kyanite. Thomson has also analysed a mineral from Chester County, Pennsylvania, well known to collectors, and has referred it to Bucholzite.‡ Being in possession of authentic specimens of the Chester mineral, I have analysed it with the following result. Quantity taken, 0·561 gr. Found—

				Another Sample.
Silica, . .	0·1925	= 34·31	per cent.	35·96
Alumina, .	0·3615	= 64·43	...	
Magnesia, .	0·0028	= 0·52	...	
Manganese, .	trace	trace		
	0·5568	99·26		

This, also, will give us the same formula as Kyanite. The mineral being less pure than Sillimanite, cannot be expected to furnish results as accurate as the former analysis. Professor Shepard in his System, expresses the opinion, that Bucholzite and Sillimanite were the same species.

* In Professor Norton's analysis, which was made in Yale College Laboratory, the excess of 2·73 was owing, undoubtedly, to aluminate of potash, which remained with the alumina, after separating the peroxide of iron by caustic potash. Subtracting this sum from the sum of alumina and peroxide of iron, which is almost exactly the quantity required by theory, and I have corrected the analysis accordingly, with the consent of Professor Norton. That analysis was made on the Sillimanite from Fairfield, New York.

† In his Translation of Mohs, vol. iii. 154.

‡ Erdmann appears also to have made his analysis on the mineral from the same locality.

There is also found at Brandywine Spring, Delaware, a mineral which has been extensively circulated under the name both of Bucholzite and Fibrolite. A specimen from this locality furnished me the following results, viz., quantity taken, 1·0675 gr. Found—

Silica, . .	0·386	= 36·159	per cent.
Alumina, . .	0·679	= 63·525	...
	1·065	99·684	

This is evidently identical with Kyanite. Minute traces of iron and manganese, which are found in both the above, are regarded as of no importance in the result, being mere impurities.*

Fibrolite of Bournon.—This mineral was first distinguished by Count Bournon, who detected it among the associated minerals of corundum from India and from China. The name has reference to its fibrous character. It was analysed by Chevenex, who found—

Silica, . . .	38·00
Alumina, . . .	58·25
	96·25

Even upon so imperfect an analysis, there has been no hesitation with most writers in referring it to Kyanite. Having a specimen of this mineral from Count Bournon at my disposal, I have analysed it.† It yielded, on 0·427 gr. taken,—

Silica, . .	0·1551	= 36·309	per cent.
Alumina, . .	0·2665	= 62·415	...
Magnesia, . .	0·0030	= 0·702	...
	0·4246	99·426	

The results just given leave it no longer possible for us to separate Sillimanite, Bucholzite, and Fibrolite, from Kyanite. The hardness of Sillimanite proves also to possess the same inequality, on different faces, which is found on Kyanite. The cleavage face is much softer than the angle or side of the prism, so as to be easily scratched with a sharp point of hard steel. The crystalline forms of Sillimanite

* It may be objected to the conclusion, that Bucholzite is identical with Kyanite that I have not analysed a specimen of the original mineral. This I should have done could I have procured one in time for my present purpose. The Chester mineral here analysed was received by Baron Lederer from Dr Nuttall; and, so far as I can learn, no one questions that the mineral from that locality corresponds entirely with the Bucholzite of Brandes. I am convinced that those chemists who have obtained so high a per-centage of silica in their analyses of *disthene* minerals, had not taken the precaution to employ the aid of caustic potash, *added to the assay during fusion*, as recommended by Berzelius; and that if they had re-analyzed their silica, they would invariably, in cases where the amount exceeded 38 per cent., have found in it a portion of alumina.

† The specimen referred to was taken from the collection of Col. Gibbs (now in Yale College), and was received by him from Count Bournon in a large collection of gems which this gentleman furnished to Col. Gibbs.

and Kyanite are also identical; the one being derived by the simplest modification from the other. The cleavage in both is in the orthodiagonal.

It may be worthy of remark, that "Andalusite" has the same chemical constitution as Kyanite, but belongs to the right rhombic form, while Kyanite is oblique, doubtless a case of dimorphism, and, perhaps, the same may be said with truth of stanrotide. My pupil, Mr George J. Brush, afforded me essential aid in the foregoing investigation.—(*Silliman's Journal*, vol. viii., No. 24. 2d series, p. 386.)

Theory of Marine Currents. By M. Babinet.

The theory of the movement of the waters in the different oceans which cover the greater part of our globe, does not hitherto appear to have been placed, like that of the trade-winds and their counter-currents, on the rigorous principles of mechanics and physics. In order to compare theory with facts, I shall confine myself exclusively to M. Duperrey's Map of the general and permanent currents of seas, independently of the superficial and temporary currents produced every season by the prevailing winds in a great number of maritime localities. This map has been constructed from facts observed by the author himself, and by other navigators engaged in scientific investigations, without regard to any theory. It presents us, therefore, with the laws to which every theory ought to conform; and, on the other hand, every theory which shall reproduce these facts in all their details, will, to a certain extent, derive support from it.

It is a notion of the French school of Laplace, already in possession of the public, that the semidiurnal swellings of the sea, called tides, which are produced *successively* from east to west (affording us the measure of the mean depth of the different oceanic basins), cannot give rise to any current in the fluid masses of the earth's surface.*

The current of the Gulf Stream, or, to speak more accurately, the circulation of the waters of which the Gulf Stream forms a part, has been recently ascribed by M. Maury to a

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* Not a circulating *current.* John Herschel, 1833 and 1849.

cause analogous to that of the trade-winds and their counter-currents, while he, at the same time, has pointed out the influence of these winds on the current of the northern basin of the Atlantic. It will afterwards be soon enough to trace the history of it, when the theory of marine currents shall be generally known and adopted.

Let us consider an oceanic basin, such as the northern part of the Atlantic, comprised between the equator on the south, the polar circle on the north, the Old and New World on the east and west; it is evident that, in this vast liquid plain, the tropical part, dilated by the heat, would be thereby elevated, and form a layer whose upper portion would exceed the level of more northern seas, and would tend to direct itself towards the waters of the north; while, at the same time, the latter, in consequence of the excess of pressure resulting from the new superincumbent mass, and from the deficiency produced by this same transportation in the pressure of the tropical columns, would tend to flow southwards in an under current, so that if the earth had no rotatory motion, we would observe, on the one hand, a kind of current or cascade from south to north throughout the whole breadth of the Atlantic, which south-north and superior flow of waters would be compensated by a similar one below, but running from north to south. An analogous effect would be produced in the four other similar basins which physical geography presents us with, namely, the southern part of the Atlantic, the northern part of the Pacific, the southern part of the same ocean, and, lastly, the Indian Sea, bounded on the north by Asia, on the west by Africa, on the east by the Islands of Sunda and New Holland, and on the south by the Antarctic Ocean. To complete the division of the terrestrial waters, we may add to these five great basins two circular basins, the one between the icy regions of the South Pole and the southern limits of the current of the Indian Sea, the current of the Pacific, and that of the Atlantic; the other, between the arctic ice and the northern limits of the Old and New Continent.

Let us return to our five great basins, of which the two northern ones carry their warm superficial waters to the north, while the three others convey the tropical waters to the south.

It is a common notion, that the quickness of rotation towards the east of a mass situate at the surface of the earth, is so much greater in proportion as this mass is situate nearer the equator; so that a mass of whatsoever kind transported towards the poles, maintains in its passage an excess of quickness towards the east, while a mass conveyed towards the equator, on leaving the medium latitudes, and having only a smaller rapidity towards the east, is precisely in the same condition as if it had a movement towards the west, in virtue of the quantity with which it has been advanced towards the east by the more southern masses in the midst of which it is transported.

According to this view, if we consider what happens with the warm superficial waters diffused over those of mean latitudes, in the northern basin of the Atlantic, for example, it is evident that these tropical waters, preserving a greater quickness towards the east than the quickness towards the east of the waters which occupy mean latitudes, must not only advance towards the north, but also towards the east. Of this nature is the phenomenon presented by the upper part of the great circuit of which the Gulf Stream forms a part. A contrary movement, that is to say, towards the south and west, would be taken by the waters which flow towards the equator on leaving the mean latitudes to replace the preceding; for their movement, being less considerable towards the east, will produce a real transport towards the west. Such, indeed, is the direction of the ocean's movement in the equatorial part of the great circuit, which, after its waters have travelled from the west to the east by the mean latitudes, turns towards the south in the latitudes of Europe and Africa, again to repair to the coast of tropical America, by crossing the Atlantic at its greatest breadth. If we keep in mind that a very small difference in latitude produces very great differences of quickness towards the east or west, we will perceive that it is more especially towards the limits of the circuit that the movements must be most perceptible. If we observe the motion of water in a vessel heated on the one side, we will notice, in like manner, that the current which ascends along the heated side, and descends along the opposite side, forms a circuit, the interior part of which scarcely partakes in the agitation of the current which surrounds it. Casting our eyes on M. Duperrey's chart, we immediately perceive, in the five circuitory oceanic basins, that the greatest intensity of the currents is principally towards their limits, and that the five intermediate spaces are left undisturbed, as they would have led us to suppose *a priori.*

The following, then, are the five great circuits ; the first, in the Northern Atlantic, runs from Africa to the Gulf of Mexico by the equator, and returns by the Gulf Stream, and its derivatives to Europe and Africa, completing its return to the point of departure in about three years. The second, in the Southern Atlantic, is bounded by the west coast of Southern Africa by the equator, by the eastern side of South America, and, lastly, by a line running from the southern point of America to the southern point of Africa. The third circuit occupies the northern part of the Pacific Ocean, and even involves a considerable part of the waters lying between the equator and the southern tropic. The fourth circuit, situate in the southern part of the same ocean, takes its departure from the west coast of South America, and is bounded by the southern tropic, New Holland, below which it descends to the south in such a manner as to turn round New Zealand, and then reverts towards America. The fifth and last circuit occupies almost the whole of the Indian Sea, with the exception of the part nearest to Asia, which, from this want of circulation, is found to have the highest temperature of all the intertropical seas. This circuit, bounded on the south by the parallel of the Cape of Good Hope, appears to be restricted from north to south within narrow limits, and the continual predominance of the current produced by the blowing of the monsoons on the permanent currents, admitted by M. Duperrey, indicates a small degree of activity in the circulation of the waters of the fifth circuit.

We do not speak of the currents which must necessarily establish themselves between these different basins, independently of the circuitory movement which constitutes the principal and permanent flow of waters. It appears that a small secondary circuit exists in the North Sea, around Iceland. But we may mention more particularly the current derived from the Gulf Stream, which M. Duperrey directs towards the Icy Sea, along the coasts of Northern Europe. This current, flowing rapidly towards the north, must enter the sea bordering the north of Sibera with considerable rapidity towards the east, that is to say, towards Behring Straits. If the currents which descend from the west of the North Sea, indicate a similar movement in the waters on the north of America, we may consider the whole of the Icy Sea, comprised within the polar ice and the northern limits of the Old and New Continent, as having a circulatory motion from west to east, and supported by the impulsion of masses of water reaching this sea by means of currents originating in lower latitudes.

It only remains for us to examine the seventh division of the terrestrial waters, namely, the circular portion of the antarctic seas comprised between the icy regions of the south pole, and the southern limits of the three southern circuits of the Indian, Pacific, and Atlantic Oceans. It is to be supposed that the influence of the movements of these three circuits, which all convey their waters from the west to the east in the neighbourhood of the antarctic seas, produces, by communicating movement to the mass of waters composing this sea, a movement likewise directed to the east; this communication of rotatory motion being moreover subjected to all the circumstances of depth, breadth, friction, and obstruction, which in general leave nothing more, in all regular and permanent movements, whether primitive or communicated, than one sole law, the law of equal *depense*, which determines and regulates alike the local speed of all permanent fluviatile currents.

M. Duperrey's chart does not appear to us to present any thing opposed to this mode of considering the subject.

By joining, therefore, this current or southern circumpolar circuit to the northern circumpolar circuit, and both of them to the five great circuits which extend to the equator in one of their directions, we shall have descriptively and theoretically a view of all the movements of the different seas. The influence of the trade-winds, which, between the tropics, have the tendency to carry the sea towards the west, and that of the north and south counter currents of the trade-winds, which tend, on the contrary, to convey the extratropical seas towards the east, must be added to the predominating influences of the forces which arise from the displacement of the liquid masses of the equator towards the poles, and reciprocally. This direction of these regular winds must likewise cool the eastern sides of continents in the mean latitudes, and, on the contrary, warm the western sides; for the west winds, which prevail in these regions, carry far from land the heated air of the warm seas which lie along the eastern coasts; while in the same latitudes, they convey this warm air along the western coasts.

Without attempting here to follow all the consequences of these movements in the seas, we cannot avoid noticing the important result that may be deduced relatively to the excess of temperature prevailing in the northern compared with the southern hemisphere. We have only to cast our eyes on M. Duperrey's chart to perceive that the mass of tropical waters conveyed towards the north by the two northern circuits of the Atlantic and Pacific Oceans, is much more considerable than that which the three other circuits convey towards the south, and that the advance in latitude of these three last circuits is at the same time much less. The eastern point of South America, and the eastern point of New Holland, which partake of the tropical waters of the Atlantic and Pacific Oceans, are situated geographically in such a manner as to determine the caloriferous circuits altogether in favour of the northern hemisphere.

The following is a synoptical view of what has been stated:—

1*st*, The waters of intertropical regions, dilated by the heat, rise above the level of the extratropical waters, and pour themselves on the top of the latter, conveying to them an excess of quickness towards the east, which produces, in these extratropical regions, a current from the west to the east; while, by a contrary effect, the waters which are conveyed between the tropics to replace those that have been removed, assume a movement towards the west. From this arises a circular progress towards the west, in the portion of it next the equator, and towards the east in its extratropical portion.

Theory and observation point out to us five great circumscribed currents or circuits, in the five great oceanic basins which reach the equator by one of their limits.

2d, We may admit two other circumpolar circuits, which surround, the one the north pole, the other the south pole, in their progress from the west to the east.

3d, Independently of numerous consequences in relation to climates, the watering of the globe, the distribution of thermal lines on the earth, on the sea, &c., the excess of temperature in the northern hemisphere over the southern hemisphere results from the preponderance of the two northern circuits over the three southern circuits, a preponderance resulting from their more extensive surface, and the greater heat of their tropical waters, and lastly, from their greater extension in latitude.

4th, Finally, it is evident, that on turning with a uniform movement a metallic vase warmed on one of its vertical faces, we may produce exactly the case of the masses of liquids in the sea, transported to different latitudes where the rotatory movement is different. We shall afterwards revert to the various consequences of this theory of the currents of the sea, as well as to the experiment which ought to produce the principal results of it. These results, moreover, appear to us fully realised in nature, according to the laws followed by the currents marked out by M. Duperrey.*

On the Porosity and Colouring of Agates, Calcedonies, &c. By M. Nöggerath.

In the last century many experiments were made to colour agate, calcedony, carnelian, &c., by solutions of metals, &c.,

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* From Comptes Rendus, t. xxviii., p. 749.

applied to the surface, and sometimes made to penetrate slightly into them. The processes have been frequently described, but it remained unknown how to render the various quartzes included among the gems of the ancients penetrable to colouring fluids.

Within the last twenty or twenty-five years the processes of the agate-cutters of Oberstein and Idar have reached such perfection that they are able not only to bring out and heighten the natural colours of calcedony, onyx, carnelian, &c., which are sometimes faint, but also to render them entirely penetrable to colouring fluids by which the beauty and variety of the stones is much increased.

This colouring process was at first a secret, known only to a few agate-dealers in Idar. It was eagerly sought after by Roman stone-cutters (as the lapidaries of Oberstein say), who bought up all the onyxes. The secret seems at length to have been discovered by some of the foreigners, or been bought up.

This art arises out of the property which the fine layers of calcedony, although exhibiting only faint differences of colour, possess of becoming variously coloured by the application of colouring fluids. By this process very mean-looking slightly-coloured stones can be turned into very fine onyxes, &c., which by their various bands of colour afford materials for cameos; and at least the beauty and designs of the agates intended for other purposes are much increased.

There is a method by which the agate-dealers of Oberstein and Idar determine the fitness of the crude minerals for the colouring process; at least to value them before purchasing them from the diggers. They break off a thin portion of a seemingly useful mass, and after moistening it with the tongue, observe whether the absorption of the moisture by the alternate bands takes place at regular intervals; if so, it is then deemed fit to be coloured as an onyx. This proof is not always decisive of the value of the minerals, yet it affords a fair criterion to the dealers to go by before they buy valuable pieces from the diggers.

Large balls of calcedony, in which many fine bands occur, especially if the rest be of a red colour, are much prized.

Weisselberg, near Oberkirchen, in the district of Wendel, produces fine specimens, but in small quantity. Bamstedt relates that one was found in the year 1844, and weighed 100 lb. It was sold in its crude state for 700 Rh. guilders.

The purchases between the diggers and dealers are made by mutual understanding, and generally without any previous trials being made or desired, the price being agreed on by the weight.

That the different varieties of quartz which form agate balls and amygdaloidal-shaped pieces, vary in their porosity has been proved by an interesting experiment of Von Kobell, who applied fluoric acid to polished agate where the different streaks were not regularly developed and only slightly visible.

Still more direct proof of the porosity of calcedony has been brought forward by Gautieri.* Near Vincenza there occur balls of calcedony, which contain, in their interior, water or air, and sometimes both, so that, through the translucent balls, we can always observe the upper part of the contained bubble by giving them a slight motion. These stones are called Enhydri. Gautieri placed one of these calcedonies, which contained no water, but only air, for some weeks in water, and observed the result. Some had, and some had not, absorbed or taken in the water into their hollow cavities; those which had not had turned clearer and harder. Such masses, when kept in a dry place, lose their contained water, and yet no opening or fissure can be observed. This proof of their porosity was thus shewn by Gautieri. Fuchs† lately repeated these experiments with similar masses of calcedony from Schio, obtained from the Zuggiano and Lago Mountains; he did not succeed so well as Gautieri, but yet his experiments are convincing of the fact in question. A longer immersion in water, accompanied by strong pressure, is not sufficient to bring the water back into the empty cavities. It may, however, be done by gradually heating the

* Untersuchung über die Entstehung, Bildung und den Ban des Chalcedons. Jena, 1800, S. 157.

† Beitrag zur lehre von den Erzlagerstatten. Wien, 1846, S. 41.

stones in water till it boils, and then rapidly cooling them (the stones must not be taken out of the water during the process). The heating expels a part of the expanded air from the cavities, through the pores, through which again the water is pressed on cooling, while the size of the air-bubble is dependent on the difference of temperature.

In many transparent calcedonies, the little cavities which the stone contains may be recognised by the naked eye. They are seen to be small bubbles often round, often long, frequently running into each other, and forming tuberculous cavities. In others, however, they cannot be observed by the naked eye, but are easily seen by the aid of a microscope, under which they appear to be filled with small cavities, especially the Brazilian Carnelian, which is particularly well suited for colouring. In a species of agate, which is called Rainbow Agate, when exposed to the sun, many well-known beautiful iris colours are produced. This optical phenomenon is explained by an examination of the mineral, when a great many small bubbles are discovered lying over one another longitudinally.

The colouring of onyx and calcedonyx (if we are to understand that the white and black, or dark-brown, stones are to be called onyx, and the white-and-grey streaked varieties are to be called calcedonyx) is performed at Oberstein and Idar in the following way. The best stones are first well washed and dried without any raising of the temperature; after this they are placed in honey diluted with water (one half pound of honey to a chopin of water). The pot in which they are then to be placed must be clean and free from grease. It must then be put into hot ashes, or into a hot oven, and the stones covered with the fluid, which must not be allowed to boil. The minerals must, indeed, always be covered with the fluid, which must be added from time to time. The minerals are to be treated in this way for a fortnight or three weeks. They are then taken out of the honey, washed, and placed in another vessel along with as much oil of vitriol as will cover them. The vessel is then to be covered with a lid, and placed in ashes in which hot coals are placed. The porous or soft stones are coloured in an hour, others in a day, and some take on no colour at all. The stones are taken out, washed, and placed in an oven. After which they are ground, and kept a day in oil, by which some fine cracks are made to disappear, and a better polish obtained; the oil is then rubbed off with bran. By this process light grey streaks are brought out on some; and others, according as their porosity was greater or less, indicate grey, brown, or black streaks. The white impenetrable masses become whiter through the loss of their transparency, and many red streaks become heightened.

The so-called Carneole from Brazil, which is wrought in Oberstein and Idar in great quantity, costs, on an average, about 50 guilders the 100 lb. Those selected with straight streaks, as being suitable for cameos, often cost as high as 2500 guilders per cwt., receive sometimes the same treatment as the native stones, and sometimes the process employed in colouring carnelian and sardonyx, as I will shortly relate.

They are originally either one-coloured, muddy yellow, grey, or contain a variety of shades of such colours, and can scarcely be called carnelian in their natural state, which name is only given to such as are of a red colour. These carnelians, when found with streaks, after they have received the above-mentioned treatment, form the finest onyx.

The chemical changes induced by the above-related processes require no detailed explanation. By the placing of the stones in hot honey, the latter penetrates into the fine pores of the stone; the vitriolic acid then causes carbonization of the animal substance,—and the more the honey in the stone is carbonized the darker its colour becomes; and while the slightly porous portions become only grey or brown, the more porous ones become black. The white and red bands appear not to be penetrable to the honey, and it is to the treatment alone that we can attribute the increased intensity of their colours. Brazilian carnelian contains the oxyhydrate of iron, and is generally penetrable in its bands; the red tints are destroyed by the carbon, and appear of the colour of a mixture of grey and black, or most commonly of a dark brown. These Brazilian carnelians afford the finest onyxes.

Calcedony can be coloured a very fine citron-yellow, either generally diffused or streaked (if this condition is already indicated in the stone). The process is as follows: they are first dried two days in an oven, care being taken not to let the oven become too warm; the stones are then to be placed in a clean vessel, and covered with spirit of salt. A cover must be firmly cemented on the vessel with clay; they must then remain from fourteen days to three weeks in the oven, and then the yellow colouring process is complete.

It deserves further inquiry, whether this yellow colour is occasioned by the formation of a salt,—by the mixture of the hydrochloric acid with some previously existing matter in the stone itself, or whether the colouring principle is entirely contained in the acid. I know no natural calcedony having a colour similar to that produced in this way. There occurs, however, in opals such a citron-yellow colour, but it is rather more of the appearance of wax.

In the coloured stones, however, this shade shews itself here and there, and seems to be inherent in them, as the colouring matter always remains the same.

Of late years a very fine blue colour has been produced in calcedony, shewing all the different shades of the torquoise. The process for this is yet a secret, known only to a few of the cutters.

Many minerals are also burned,—such as agate, calcedony, and Brazilian carnelian. This is done partly to increase the beauty of their natural colours, and partly, as it is said, to give the natural colours more durability. Many calcedonies become, through this process, almost white, the red colours more intense, and the pale yellow a very fine red. This is also the case with the Brazilian carnelian. By which process the streaked stones of this kind become transformed into fine sardonyxes, and those with one colour take on the true colour of the carnelian. The process is as follows: the stones are rendered perfectly dry, by being placed for a fortnight or three weeks in a hot oven; they are then placed in a shallow dish and moistened with vitriolic acid, but not covered. The polishers usually dip the stones in the acid, and then place them beside each other in a vessel, which is then covered and placed in a hot fire until they are red hot. The fire is slowly extinguished, and they are taken out when cool; by this roasting the oxyhydrate of iron* which the stones contained is freed from its moisture, and the colour of the oxide assumes a more lively hue, and is seen in the translucent mass in the proper colour of the carnelian. The smaller stones are burned before being polished; the larger ones are first cut into various shapes, *e. g.*, dessert plates, bowls, vases, &c. Small pieces do not easily fly to pieces in the roasting, but large ones very easily; so it is necessary to make them as thin as possible by grinding.

There are many other dexterous manipulations necessary, which are known only to the polishers themselves, but I have collected the above processes from many sources; and my esteemed friend, Herr Tischbein of Herstein, in the Palatinate of Birkenfeld, has given me many particulars which assisted my studies on the agate very much. (I acknowledge them here with much thankfulness.)

When once, however, the properties which these minerals (to which I have given the collective name of agate) possess of being quite penetrable to colouring fluids, in consequence of their porosity, are better known, it is probable that other colours may be given them; and, also, that many antique stones, presenting unusual colours, may have been coloured so artificially. This seems to me very likely, as many of the antique cameos and intaglios which I have seen in collections seem to be so.

Why should we not find the ancient coloured stones so good as we know the ancient pastes were?

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* That iron is the colouring principle of carnelian cannot be doubted after the experiments of M. Heintz (Poggendorff's Annalen. Band 60, s. 579). Gaultier de Claubry (in Poggendorff's Annalen, Band 26, s. 562) has attempted to shew that the colouring principle is in the nature of the carnelian itself; but the critique of his experiments, and Heintz' opposite conclusions, have shown the untenableness of his views.

An Account of the Mineral-Field between Airdrie and Bathgate, and from Bathgate to Edinburgh and Leith. By ROBERT BALD, Esq., F.R.S.E., M.W.S., Mining-Engineer. (Communicated by the Author.)*

The mineral-fields of which I am now to give an account, extend from the town of Airdrie, situate about eleven miles east from Glasgow, to the town of Bathgate, in the county of Linlithgow, and from Bathgate to Edinburgh and Leith.

Until a late investigation was made by Mr William M'Creath and myself, the minerals of Bathgate were regarded as of little value, as the collieries west from the town of Bathgate were upon a very small scale, and the whole output of coals very limited.

The ironstone and other useful minerals were reckoned of no value, and were thrown aside as rubbish; but since the investigation made last year (1846), the Bathgate mineral-field has risen in value to a very great degree, and the systematic view of the minerals obtained, which had not been the case formerly, has effected this change.

The collieries upon this mineral-field were isolated as to sales; but now that a railway is in course of being completed, with its branches, the value of this mineral-field will be realised, and the minerals brought abundantly to market by their transit, both to the west and east, to Edinburgh and Leith.

In describing this extensive mineral-field, it may properly be divided into five distinct sections, viz.:

1*st*, From Airdrie to West Craigs Inn.

2*d*, From West Craigs Inn to the outcrop or commencement of a lower bed of coal of the Bathgate coal-field.

3*d*, The Bathgate coal-field.

4*th*, The hill ground immediately east of Bathgate.

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* Read before the Wernerian Natural History Society, 13th March 1847.

5th, The mineral-field from the Bathgate Hills to Edinburgh and Leith.

At Airdrie, and all around, there are very many extensive collieries established, not only for the general sale of coals, but for supplying the blast-furnaces, for the production of cast-iron in the vicinity, which are no less than sixty in number.

The coals of the Airdrie district are the same as in what is termed the "Glasgow coal-field," with certain changes in the arrangement; in particular, where the intermediate strata betwixt two coals in the Glasgow district have disappeared, and the two coals being brought into juxtaposition, form one very thick bed of coal.

The common clay or argillaceous ironstone abounds here, and the Mushet ironstone band, discovered by Mr David Mushet, who established the Calder Iron-Works, is very abundant, and is of more than double the value of the common argillaceous ironstone. It has a portion of carbonaceous matter combined with it, which greatly aids its calcination, as very few coals are required for that purpose.

East from the town of Airdrie, the Glasgow coal-field continues, until it reaches the estate of Auchingrey, which once belonged to the late Rev. Mr Haldane, where, immediately by the south side of the turnpike road leading from Glasgow to Edinburgh, appears a very great extent of "trap rocks," commonly named Greenstone, or the "Blue Whinstone" of Scotland. These rocks are not much elevated, and no doubt they pass under the Auchingrey estate, and produce those changes which are commonly connected with such rocks.

These trap rocks are nearly a mile in breadth, and extend eastward to the West Craigs Inn. They have evidently been forced up through the regular coal-field, and have made great derangements; for the open burning coals of the Airdrie district are no longer to be found here, but the anthracite (the Blind coal of Scotland), and the caking or smithy coal abound; and there is no doubt that much of the coal in contact with these rocks is so much changed, that it will not ignite, as is always the case in such districts.

These great changes, produced by the trap rocks, in the condition of the coals, are universal through the coal-fields of Scotland, and not only so, but the ironstones are affected; for they are, by natural calcination, brought to yield a higher per-centage than in their ordinary state. From this we conclude, there can be no doubt that these rocks are igneous in their origin.

From West Craigs Inn, and farther east, the effects of the trap rocks are less observable, but they produce changes in the minerals; and on leaving this district, we come to the outcrop of the lowest ascertained coal in the western part of the Bathgate coal-field.

The first series of coals consists of four beds of coal, of from two to four feet in thickness, inclining to the quality of caking or smithy coals, and as such are used. These coals dip to the eastward at a moderate rate, until interrupted by a slip or dislocation of the strata, which could not be seen. It is said to throw down the coals to the east; but of this I have great doubts, for we can trace no analogy whatever betwixt those coals on the west side of the dislocation, and those on the east side of it; but the conclusions drawn regarding this dislocation will be particularly noticed, after describing the coals from it to Bathgate.

The coals, from the east side of the dislocation dip eastward to the town of Bathgate, at a moderate rate. These consist of seven beds of coal, of from one foot four inches to four feet thick, but the third and fourth coals from the surface can only be wrought as one coal, along with the intervening stratum. Besides these coals there are several very thin coals, termed "unworkable."

This term "unworkable," requires to be clearly explained, as it is much used in describing coal-fields. No coal, however thin, is, physically, unworkable; for by cutting away either the roof or the pavement upon which the coal rests, room can be made for the miner to work the coal; but thin coals, and even coals of one foot three inches thick, may be workable in a district where coals are scarce, and sell at a high price; when in another district, where there is abundance of coal, and thick beds of it, those of from two to three feet are reckoned unworkable ; that is, they cannot be sold with profit in that district.

The coals dip regularly until near the town of Bathgate, when they flatten and are formed into what is called the "trough" of the coal, and then deflecting from the regular line of dip, rise to the east upon the western face of the Bathgate Hills, at a great angle of elevation with the horizon, and are termed Edge coals. The common coal strata, and the accompanying mountain-limestone, below the thick coals, partake of the counter-rise.

In general, thin coals are found in the coal strata below the mountain-limestone ; but they are very seldom workable to profit.

The coals amount, in their aggregate thickness, to twenty feet four inches ; but we have reason to infer that there are thick coals in the "trough," or lower part of the coal-field, both above the upper coal, and below the lowest coal.

All these coals are of good quality, generally cubical, and a portion of them splint, suitable for all furnace purposes ; and at the town of Bathgate there is a thick coal, with "Parrot," or gas-coal about one foot thick, connected with the ironstone band.

Benhar Coal-Field.

This coal-field falls now to be described. It is situate to the south of the trap rocks before mentioned, and contains several beds of coal ; but is chiefly valued, in having one bed of coal, of about four feet thick, of very superior quality to any in all the surrounding district. It is easily wrought, as the natural backs, cutters, and horizontal fissures, are numerous ; hence the coal, when wrought, is in pieces of regular rhomboidal figure, and in the olden days was termed "Bible coal," being similar to a large folio Bible.

We therefore infer, from the geology of the district around, that this main coal of Benhar, is nearer to the trap rocks below than any of the coals of the Bathgate coal-field ; and this propinquity may have improved its quality, as is the case in Stirlingshire, where the Bannockburn coal, which is undoubtedly an open burning coal, towards the Carron Works, is there converted into one of the most valuable caking coals in Scotland; and this change to all appearance is caused by the immense bed of trap rock which passes under this coal-field. Thin beds of coal which lie nearer this trap rock are converted into anthracite, or glance coal. The same change, apparently from the same cause, is to be seen in the Dollar coal-field in Clackmannanshire.

These instances, and many others in Scotland, leave no doubt of the effects produced in coals by this class of rocks, and hence I am led to infer, that the slip does throw up the coals and their accompanying strata, to the west; but this view is only hypothetical.

Mountain-Limestone.

This mass of limestone passes under all the workable beds of coal which have been ascertained in the Bathgate coal-field. It is no less than forty feet thick, and of very superior quality, suitable for all the purposes to which lime is applied, more particularly when used as a flux in the blast-furnaces, where four tons of this limestone produces the same effect as six tons of the limestone at present generally used in the Airdrie blast-furnaces. The expense of carriage of this limestone to the furnaces prevents it being used; but the railway now making will open up a very great sale for it, and be very advantageous to the agricultural interests of the country, as well as for the iron-works and buildings.

Ironstones.

Ironstones abound in this mineral-field, but the distance from blast-furnaces renders them of no value; and no account was taken of them when sinking the pits.

The ironstones which have been found are of three kinds, viz:—

1*st*, The Mushet ironstone band, found in the roof of a coal at the town of Bathgate. It is from six to eight inches thick, is accompanied with a parrot or gas coal, about a foot thick, and also with bands and balls of clay ironstone.

2*d*, The common argillaceous or clay ironstone, yielding from thirty to thirty-three per cent. of iron.

3*d*, The curly band ironstone, which is very irregular in its form, is of the best quality for producing grey cast-iron, strong in its texture, and suitable for artillery. It was lately analysed by Dr Thomson junior, of Glasgow University, and found to contain no less than forty per cent. of iron,—a produce equal, if not superior, to any ironstone in Scotland.

In future the ironstones found in sinking will be carefully registered, as well as those found in broken ground, by the side of brooks and in ditches.

Light-coloured Argillaceous Rock, termed Fire-Clay.

This mineral is found in great abundance in the Bathgate coal-field, from a few inches to several feet in thickness. It is of good quality, fit for all furnace purposes, and for some kinds of pottery; it is, however, of no value at present, nor can be so until the railway now forming is in operation.

It is a remarkable fact in natural history that, in general the beds of coal rest upon this kind of rock, although at times but very thin.

Sandstone.

There are several quarries of sandstone in this mineral-field, suitable for general purposes, but none for elegant architectural buildings, Binney quarry excepted, which is of a very superior kind, and presently used in the chief buildings of Edinburgh.

Sandstones have been searched for by boring; and one bed, of a white colour, thirty feet thick, was found; but no quarries have as yet been opened up in consequence.

Millstone and Flagstone.

These varieties of sandstone have been opened up at a place named Bogend; the millstones are of a very dark and singular appearance, differing from any that we have seen, and are used for grinding all kinds of grain, wheat excepted.

Connected with the millstone bed there are good flagstones, some of which are of excellent quality for grindstones.

Mineral-Field from Bathgate Hills to Edinburgh and Leith.

There is a sudden rise of the strata to the east; and this elevated range, with the mountain-limestone, extends northward to a considerable distance, where many limestone quarries have been opened; but it is remarkable that this range is suddenly cut off upon the south, near Bathgate, and here the mountain-limestone disappears; but limestones are found from this district, extending to the Blackburn and Whitburn districts, and contain coals, and their accompanying strata, such as sandstone, fire-clay, and ironstone. The coals hitherto found are few in number, being, in general, from three to four feet thick, and the beds of limestone from six to eight feet thick.

It is worthy of remark, that at Blackburn there is a coal of four feet thick, found a few feet below the limestone, from which we infer that this coal field to the south is a formation altogether different from that of Bathgate coal-field.

Upon the east of the Bathgate Hills, a trap range of rocks commences, and we infer that they are the cause of having elevated the strata at the town of Bathgate; as is precisely the case upon the south side of the Ochil Mountains in Clackmannanshire, and upon the south side of the Pentland Hills, near Edinburgh.

From some distance east from Bathgate, and towards Broxburn, the coals which had been wrought are all of the smithy or caking kind, and so disturbed by the trap rocks, that no coal-field of any extent has hitherto been discovered; and the coals which have been wrought are in patches, and much disjoined.

After passing Broxburn, no coals have been found; but there are valuable beds of sandstone, which evidently belong to the series under the coal-formation: in this range, also, limestones are wrought of very good quality.

Connected with these limestones there are many beds of bituminous shale, which in burning gives out much flame, but leaves a residuum nearly as large as when in its raw state. In Scotland this kind of slate has been turned to no use; whereas in Sweden limestones are calcined with this kind of schistus, and it is the only fuel used in the furnaces of the alum-works there, where this mineral abounds.

From this limestone district to the city of Edinburgh and to Leith, the lower series of the coal-formation abounds, with very few traces of coal. Under and immediately around the city of Edinburgh, the strata are upheaved and disordered, in an uncommon degree, by trap rocks, which are found both in beds and dykes; whereas, on the other hand, towards Leith, where bores have been put down, the series of strata under the coal-formation, consisting chiefly of sandstones and slate-clay and bituminous slate, are found lying very regular and undisturbed, and extend to the limestone at Duddingstone, viz., the mountain-limestone which lies under the great coal-field of the Lothians.

The same series, under the coal-formation, extends to Wardie, where thin and sulphurous coals are found, and many bands and balls of argillaceous ironstone.

As a railroad is now constructing from Airdrie to Leith, the chief consumption of the very valuable minerals of these districts will be in Edinburgh and at the town and port of Leith; and there is no doubt that coal, ironstone, and limestone, will be in great request for the iron-works at and around the town of Airdrie, where there are sixty blast-furnaces, and the annual consumption of these is as follows, viz.:—

Coals, ...	837,070 tons.
Ironstone, ...	1,157,650 ...
Limestone, ...	115,765 ...
	2,110,485 tons.

The production of cast-iron from these furnaces amounts to 356,200 tons.

The above statement shews in a very strong light what immense excavations are made in the mineral-fields around Airdrie; and that, ere long, minerals for these furnaces must be brought from a distance. There is, therefore, a necessity of husbanding with the greatest economy our mineral-fields, and to leave no pillars below ground, so far as this system can be pursued with safety, as regards the workmen and mineral-fields; for in working with pillars, from a fourth to a third of the whole area of coal is left behind and lost, and if there is ironstone in the roof of the coal, what is immediately above the pillars is lost also.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Destructive Effects of a Water-Spout on the Bredon Hill, North Gloucestershire, on Thursday, 3d May* 1849.—About half-past five in the afternoon of Thursday, the 3d of May 1849, during a storm of thunder, lightning, and hail, an enormous body of water was seen to rush down a gully in the Bredon Hill, and direct its course to the village of Kemerton. The stream was broad and impetuous, carrying everything before it. Its extraordinary force and body of water may be judged of from the fact, that, on reaching the residence of the Rev. W. H. Bellairs, of Kemerton, it broke down a stone wall which surrounded the garden, burst through the foundation of another, made a way for itself through the dwelling-house, and then carried off a third wall of brick, six feet high. The garden soil was washed away, and "enormous blocks of stone," and debris from the hill left in its place. By this time the current was considerably broken; nevertheless, it flowed through the house, to the depth of nearly three feet, for the space of an hour and forty minutes. The neighbouring railway was so deeply flooded as to delay the express train, by extinguishing the fire of the engine.

Upon the Saturday morning, as soon as possible after the occurrence of this remarkable phenomenon, Mr Bellairs rode up the Bredon Hill to ascertain its cause. For more than a mile the course of the torrent could be easily traced, from twenty to thirty feet in breadth, every wall being broken down, and the whole, or greater part, of the soil removed. On arriving at the north-west shoulder of the hill, the place where the mass of water had fallen was discovered. It was a barley-field of about five acres in extent, the greater part of which was beaten down flat and hard, as if an enormous body of water had been suddenly poured out upon it. Beyond this field and on higher ground, there were no signs of the fall of water to any great amount. The part of the hill where this waterspout had emptied itself was thus fully ascertained. In the vicinity heavy rain had descended, for minor tributaries had left marks of union with the main current; but in these there was nothing remarkable.

As the water rushed down the hill towards Kemerton, it did not spread itself out as under ordinary circumstances it would have done, but flowed in a body, as if kept together by the velocity of the current, the physical features of the place aiding it in this respect; for there the hill is steep, and the course of the water was in a gully. The general depth of the torrent was from six to seven feet, though in one instance marks upon a tree were met with *sixteen* feet above the ground. We must not overlook, however, the bending of the tree under the power of the stream; consequently, though the mark would lead to the belief that the water had risen sixteen feet, it does not follow that it actually did so.

The rain ceased immediately after the fall of water; and it is said that a strong sulphurous odour was perceived.—(*Communicated by David P. Thomson, M.D., Liverpool.*)

2. *Fall of Rain in the Lake and Mountain Districts of Cumberland and Westmoreland, in the year* 1848. *By J. F. Miller, Esq.*

No.			Rain, in Inches.	Wet Days.
1.	High Street,	Whitehaven,	47·34	210
2.	Round Close,	Whitehaven,	46·70	
3.	St James's Church Steeple,	Whitehaven,	36·34	
4.	The Flosh,		60·82	207
5.	Cockermouth,		52·37	228
6.	Bassenthwaite Halls,		47·06	196
7.	Keswick,		66·40	229
8.	Gillerthwaite, Ennerdale,		97·73	...
9.	Loweswater Lake,		76·66	217
10.	Foot of Crummock Lake,		98·07	207
11.	Gatesgarth, Buttermere,		133·55	...
12.	Eskdale Head,		70·38	...
13.	Do., centre of Vale,		86·78	205
14.	Wastdale Head,		115·32	243
15.	The How, Troutbeck,	Westmoreland,	91·34	201
16.	Ambleside,	Westmoreland,	76·82	...
17.	Langdale Head,	Westmoreland,	130·38	212
18.	Seathwaite, 6 inches above surface,	Borrowdale,	160·89	232
19.	Do., 18 inches above do.,	Borrowdale,	157·22	
20.	Stonethwaite,	Borrowdale,	130·24	242

The Mountain Gauges.

No.	Feet above Sea.	In 13 Months, 1847. 1848. 1st Dec. to 31st Dec. Inches.	Summer Months. 1st May to 31st October. Inches.	Winter Months. Dec. 1847 to April 1848, and Nov. & Dec. 1848. Inches.
21. Sca Fell Pike,	3166	*64·73	49·46	...
		From 1st May.		
22. Great Gable,	2928	91·32	46·81	44·51
23. Sprinkling Tarn,	1900	148·59	70·95	77·64
24. Stye Head,	1290	138·72	60·35	78·37
25. Brunt Rigg,	500	109·19	43·18	66·01
14. Valley to the west, Wastdale,		127·47	50·16	77·81
13. Do. to the S.E., Eskdale,		95·71	37·69	58·02
26. Seatollar Common, } Borrowdale,	1334	139·48	57·97	81·51
19. Valley, Seathwaite, } Borrowdale,	1334	177·55	68·96	108·59

From the table for the summer months, it appears that between the 1st of May and the 31st of October, the gauge at 1290 feet has received 20½ per cent. more rain than the valley; at 1334 feet, 15½ per cent. more; at 1900 feet, 41½ per cent. more; at 2928 feet, 6 per cent. less; and at 3166 feet, 1 per cent. less than the valley.

In the winter months, the gauge at 1290 feet has collected 0·5 per cent. more; at 1344 feet, 5½ per cent. more; at 1900 feet, 1 per cent. more; and at 2928 feet, 42½ per cent. less than the adjacent valley.†—*Observatory, Whitehaven, June* 7, 1849.

MINERALOGY.

3. *Black Oxide of Copper of Lake Superior.*—Mr Whitney made some remarks on the remarkable vein of black oxide of copper which was formerly worked at Copper Harbour, Lake Superior, but which was abandoned after some forty or fifty thousand pounds of this very valuable ore had been raised. It was the only vein of this substance, and perhaps the only locality known in the world, and specimens will be highly prized by the mineralogist hereafter. The substance called copper black, and sometimes black oxide of copper, which occurs in an earthy, pulverulent form, is not to be confounded with the pure oxide of copper found at Copper Harbour. Copper black is a mixture of various hydrated oxides, especially of iron, manganese, and copper, of which the latter forms but a small portion; it occurs in an incrustation on other ores of copper, and is

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* The fall of rain on Sca Fell during the winter of 1847–8 was lost, in consequence of injury sustained by the receiver from the frost. The receivers at the mountain stations have since been renewed. They are made of extra-heavy sheet copper, double lapped at the seams, and with the bottoms convex inwards, the better to enable them to resist the expansive force of the water during its conversion into ice. Such an accident is therefore not likely to occur soon again.

† The above details were given in an interesting memoir read before the Royal Society, which will soon appear.—*Edit.*

evidently the result of their decomposition. Semmola, however, has described a substance occurring in small tubular crystals belonging to the hexagonal system, which, according to him, are pure oxide of copper, Cu. To this substance he has given the name of Tenorite. The oxide of copper found at Copper Harbour is generally compact, though the purer specimens have a crystalline structure. Mr Teschemacher has, however, two specimens, which he has kindly allowed me to examine, in which this substance is distinctly crystallised in cubes, with their solid angles truncated. The question arises, was the substance described by Semmola as crystallised in the hexagonal system, really Cu, or is this substance dimorphous?

Some portions of the oxide of copper from Copper Harbour are almost chemically pure, though it is generally mixed with a little silicate of copper. One of the purest specimens contained only 1·2 per cent. of impurities, mostly silica, with traces of lime and iron.

As the oxide of copper of this remarkable vein has not been mineralogically described, the following description is added:

Crystallised in cubes, with their solid angles occasionally truncated; generally, however, massive, with crystalline structure, sometimes earthy; no traces of cleavage. H. = 3; G. = 6·25; colour, steel grey to black; lustre metallic, the earthy varieties acquire a metallic lustre on being scratched or cut with a knife; opaque.

Chemical composition Cu, almost pure; containing copper 79·86, oxygen, 20·13.—(*Silliman's American Journal of Science and Arts*, vol. viii., No. 23, p. 273.)

4. *On Arkansite.*—This mineral, which Mr J. D. Whitney makes out to be Brookite, has been examined by M. Teschemacher (Proc. Bost. Soc. N. H., April 1849, p. 132), and he gives the following for its angles—(See figure in Silliman's Journal, vol. iv. p. 279)—M : M = 100° and 80°. M : c = 133° 35, c : c = 135° 45′, a : a = 125°. Shepard made M : M 101° to 101° 15′, and a : a 128°. According to the measurements of M. Teschemacher, the angles are those of Brookite.

5. *Baïerine*—(*L'Institut.* No. 793).—The metal pelopium, has been found in the Columbite of Bavaria, by G. Rose, and in that of Limoges by Damour. It is proposed to distinguish the variety of Columbite by the name Baïerine, given it by Bendant. The specimens from these two localities agree well in external characters, and in analyses.

6. *Notices of American Minerals. By Professor C. N. Shepard.*—(1.) *Pyrophyllite*, in beautiful white *stellæ*, occurs, along with very brilliant and perfect crystals of *rutile*, on a soft, semi-steatitic kyanite, at Crowder's Mountain, in North Carolina; from which region I also possess large masses of deep blue *Lazulite*, associated in some instances with *topaz*, the latter in distinct crystals.

(2.) *Wavellite.*—This mineral has been sent to me by Dr Pendleton of Athens, Ga. It occurs on a jaspery opal in Washington County, near Saundersville, Georgia.

(3.) *Babingtonite* at Athol, Mass. This mineral has been brought to light in the railroad excavations in this town, during the past year, in very splendid crystals, associated with epidote, *apophyllite*, &c. *New Haven, 20th June* 1849.

7. *Platinum and Diamonds in California.*—The existence of platinum in the gold sands of California has of late been often announced. Specimens from the region have recently been seen by the editors of this Journal. We also learn from a reliable source, that the diamond occurs at the placers. The writer (Rev. Mr Lyman) describes a crystal seen by him, of a straw-yellow colour, having the usual convex faces, and about the size of a small pea. He saw the crystal but for a few moments, and had no opportunity for close examination; but the appearances and form left little doubt that it was a true diamond.—(*American Journal of Science and Arts*, 2d Series, vol. viii., p. 294.)

8. *California Gold.*—The gold of California has been analysed by Dr Hofman, and found to consist of gold 89·61, silver 10·05 = 99·66, the loss being some copper and iron, which was not determined.

9. *Arkose* (*Bib. Univ. March* 1848).—The arkose of the Vosges, according to Delesse, is a metamorphic quartzite, consisting essentially of hyaline quartz, and crystals of orthose.

10. *Total Quantity of Lead Ore raised, and Lead Smelted, in the United Kingdom, in* 1848 (Official Report, by R. Hunt, keeper of Mining Records), *Mining Journal, August* 25, 1849.

England.	Lead Ore. Tons.	Lead. Tons.
Cornwall,	10,494	6,614
Devonshire,	1,334	844
Cumberland,	8,272	5,684
Durham and Northumberland,	18,815	14,658
Westmoreland,	519	388
Derbyshire,	5,185	3,370
Shropshire,	4,130	2,762
Somersetshire,	41	29
Yorkshire,	6,848	4,793
Total,	55,638	39,142

Wales.	Lead Ore. Tons.	Lead. Tons.
Cardiganshire,	4,902	3,180
Carnarvonshire,	21	14
Carmarthenshire,	307	204
Denbighshire,	...	...
Flintshire,	10,056	7,069
Montgomeryshire,	927	601
Merionethshire,	92	54
Total,	16,305	11,122
Ireland,	1,912	1,188
Scotland,	2,588	1,736
Isle of Man,	2,521	1,665

Making a total of 78,964 tons of lead ore, and 54,853 tons of lead.

Imported, 1298 tons lead ore; pig and sheet lead 3788 tons; retained for home consumption, 2157 tons.

Exported, 135 tons lead ore; pig and rolled lead, 4977 tons; shot, 1151 tons; litharge, red and white lead, 2292 tons; foreign lead in sheet and pig, 3747 tons.

11. *On the Decomposition of Trap-Rocks. By M. Ebelmen.*—(1.) A trap from near St Austle (Cornwall). This trap consists essentially of Labradorite and Pyroxene.

	Trap fresh or unchanged. A.	Altered trap. B.	Trap more altered. C.
Alumina,	100	100	100
Silica,	325	212	201
Lime,	36	5	6
Magnesia,	17	14	12
Oxide of iron,	106	107 }	79
Oxide of manganese,	3	2 }	
Potassa, } Soda, }	33	14	13
Water,	11	43	38
	631	497	449

Hence the trap, by decomposition, has lost more than a third of its silica, $\frac{5}{6}$ of the lime, and half of the alkalies; this last shews that the feldspar was the last to change, and had not been wholly decomposed.

(2.) *A Basalt from the Rhine*, consisting of Labradorite, about 54 per cent., Pyroxene 24, Chrysolite 10, with titanic iron 10, and water 2 per cent., afforded him—

	Unchanged Basalt. A.	Basalt altered. B.
Alumina,	100	100
Silica,	283	228
Lime,	63	43
Magnesia,	39	29
Oxide of iron and manganese,	80	78
Titanic acid,	6	6
Potassa,	7·4	2·6
Soda,	22·2	7·4
Water,	15·0	35·0
	615·6	529·0

Here two-thirds of the alkalies have disappeared, shewing that the decomposition of the feldspar was far advanced. The result of the changes, in both cases, is to produce, as the residue, an hydrated silicate of alumina, or a clay. The removal of the silica is shewn by M. Ebelmen to be independent of the alkalies present. The decomposition is attributed by him to carbonic acid and oxygen present in waters, to organic matters living, or in course of decomposition, and the phenomena of nitrification.—(*Annales des Mines*, 12, 627.—*Silliman's Journal*, vol. viii., No. 24, p. 421.)

BOTANY.

12. *Flora of the Date Country and Sahara.*—M. d'Escayrac de Lauture has announced to the Academy of Sciences of Paris, his return from a journey to the date country and Sahara. He brings with him about 200 species of plants, forming the peculiar flora of the Great Desert, and of the region of the oases. Some of these plants, although already known, are interesting in respect to geographical botany; and, in this point of view, the traveller mentions a fact worthy of observation, namely, the retardation of vegetation in the oasis, which is occasioned by the shade of the date trees, whose dense rampart affords future security to the botanical species, by arresting the hurricanes of sand which, in the Sahara, sometimes efface all marks of vegetation over a space of many days' journey, and for a period of many ages.

M. d'Escayrac's collection contains a pretty large number of species usually met with on the shores of the sea, and which find their way into the desert, either around the vast plains of moist salt, such as the Lake of Tazer (erroneously named by geographers the Lake *El Oudeleh*), or around the brackish springs containing sea salt, salts of lime, and magnesia.

The oasis, according to the remark of M. d'Escayrac, following in general the course of a river without outlet, or of some ravine which the sand is not long in filling up, present, like the richest plains of Lombardy, the spectacle of many different cultures one above another.

Below the palm *Degle*, planted in quincunx form, and surrounded at its base with a pedestal of earth into which its roots penetrate, are found orange trees covered with fruit almost the whole year, olives, figs, apricots, peaches; the vine twines from one date tree to another; and lower down we perceive pepper, beans, dourrak, barley, *henné*, and tobacco kept in constant humidity by the most skilful irrigation.

Besides the male date, *Dokkar*, M. d'Escayrac says, that he determined about thirty principal varieties of date trees; among which, he particularly mentions the *Menakher*, which yields fruit the length of the finger, and whose rarity and price are such, that the Bey of Tunis is almost the only individual who can afford to eat it; the *Degle*, the tallest and most majestic, whose fruit is commonly brought to Europe, and eaten in the country by the wealthier classes; the *Halig* which yields food to the poor and to slaves; lastly, the *Ammeri* and *Saroti*, whose flowers rarely fruitful, often yield dates curved upon themselves owing to a decay of the kernel, and to which the Arabs give the name of *Sich;* it is from this that the fable has arisen of dates without kernels. The dates of these two varieties which are fertile, are given to horses and beasts of burden in absence of barley, which they always prefer.—(*From L'Institute, No.* 821, p. 305.)

ZOOLOGY.

13. *The Infusoria of the Dead Sea and the River Jordan.*—The celebrated Ehrenberg has lately examined the water of the Dead Sea and of the river Jordan, in a zoological point of view. He finds the water of the Dead Sea to abound in infusoria, but nearly all of them of fresh or brackish water species,—a fact illustrative of the opinion which maintains that this lake never formed any part of the general ocean. The waters of the Jordan abound in infusoria, all of the fresh-water kinds, and the greater number peculiar to that river,—a fact rendering it probable that great rivers, like basins of the ocean, have their peculiar and characteristic species. These very interesting facts shew how the researches of Ehrenberg are opening up a novel field to the hydrologist: the microscope, indeed, in the hands of this illustrious naturalist, has already unfolded the marvels of a new world. We trust our naturalists are fully aware of the importance of accurate microscopical examinations of the sea around Britain, also of our springs, lakes, and rivers, and are convinced they will find in such delightful researches a rich harvest of most interesting and far-leading discoveries.*

MISCELLANEOUS.

14. *Alleged Burying Alive.*—In the midst of exaggeration and invention, there was one undoubted circumstance which formerly excited the worst apprehensions,—the fact that bodies were often found turned in their coffins, and the grave-clothes disarranged But what was ascribed, with seeming reason, to the throes of vitality, is now known to be due to the agency of corruption. A gas is developed in the decayed body, which mimics, by its mechanical force, many of the movements of life. So powerful is this gas in corpses that have lain long in the water, that M. Devergie, the physician to the Morgue at Paris, and the author of a text-book on legal medicine, says, that unless secured to the table, they are often heaved up, and thrown to the ground. Frequently, strangers seeing the motion of the limbs, run to the keeper of the Morgue, and announce, with horror, that a person is alive. All bodies, sooner or later, generate gas in the grave, and it constantly twists about the corpse, blows out the skin till it rends with the distension, and sometimes bursts the coffin itself. When the gas explodes with a noise, imagination has converted it into an outcry or groan; the grave has been reopened, the position of the body has confirmed the suspicion, and the laceration been taken for evidence that the wretch had gnawed his flesh in the frenzy of despair. So many are the circumstances which will occasionally concur to support a conclusion that is more unsubstantial than the fabric of a dream.—(*Athenæum*, No. 1140, p. 1115.)

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* Under Infusoria we here include *Poligastria*, &c.

List of Patents granted for Scotland from 22d September to 22d December 1849.

1. To John Mason, of Rochdale, in the county of Lancaster, machinist, and George Collier, of Barnsley, in the county of York, manager, "certain improvements in machinery or apparatus for preparing and spinning cotton and other fibrous materials, and also improvements in the preparation of yarns or threads, and in the machinery or apparatus for weaving the same."—24th September 1849.

2. To William Parkinson, of Cottage Lane, City Road, in the county of Middlesex, gas-meter manufacturer, successor to the late Samuel Crossley, "improvements in gas and water meters, and in instruments for regulating the flow of fluids."—24th September 1849.

3. To James Aitken, of Cook Street, in the city of Glasgow, North Britain, manufacturer, "certain improvements in the preparation of cotton and other yarns for weaving, and in the machinery employed therein."—27th September 1849.

4. To John Robinson, of Patterson Street, Stepney, in the county of Middlesex, engineer, "improvements in machinery for moving and raising weights."—3d October 1849.

5. To Ernest Grapel, of Birmingham, in the county of Warwick, Esquire, "improvements in marine vessels, in apparatus for the preservation of human life, and in moulding, forming, and finishing hollow and solid figures, composed wholly or in part of certain gum, or combination of certain gums, also improvements in dissolving the aforesaid gums, and in apparatus or machinery to be used for the purposes above mentioned."—8th October 1849.

6. To Robert Clegg, Joseph Henderson, and James Calvert, of Blackburn, in the county of Lancaster, manufacturers, "certain improvements in looms for weaving."—8th October 1849.

7. To Thomas Lightfoot, of Broad Oak within Accrington, in the county of Lancaster, chemist, "an improvement in printing cotton fabrics."—11th October 1849.

8. To William Gaspard Brandt, of No. 16 Compton Street, Brunswick Square, in the county of Middlesex, machinist, "improvements in the construction of the bearings of railway-engines and railway and other carriages now in use."—11th October 1849.

9. To Thomas Beale Browne, of Hampen, in the county of Gloucester, gentleman, "certain improvements in looms, and in the manufacture of woven and twisted fabrics," being a communication from a foreigner residing abroad.—15th October 1849.

10. To George Henry Dodge, citizen of the United States of America, but now residing at Manchester, in the county of Lancaster, "certain improvements in machinery for spinning and doubling cotton yarns and other fibrous materials, and in machinery or apparatus for winding reeling, balling, and spooling, such substances when spun."—15th October 1849.

11. To Charles Shepherd and Charles Shepherd, junior, of Leadenhall Street, in the city of London, chronometer makers, "certain improvements in working clocks and other time-keepers, telegraphs, and machinery, by electricity."—15th October 1849.

12. To Joseph Stovel, of Suffolk Place, Pall-Mall East, in the county of Middlesex, tailor, "improvements in coats, part of which improvements are applicable to sleeves of other garments."—19th October 1849.

13. To David Christie, of Saint John's Place, Broughton Lane, in the borough of Salford, in the county of Lancaster, merchant, "a process for welding and uniting cast-iron with steel and malleable iron," being a communication from abroad.—19th October 1849.

14. To George Park Macindoe, residing at Mountblow, in the parish of Kilpatrick, and county of Dumbarton, in that part of the United Kingdom called Scotland, "certain improvements in machinery or apparatus applicable to the preparation, spinning and doubling or twisting of cotton, wool, silk, flax, and other fibrous substances."—19th October 1849.

15. To William Frederick Norton, of Lascell's Hall, Lepton, in the parish of Kirbeaton, in the county of York, fancy cloth manufacturer, "improvements in manufacturing plain and figured fabrics."—19th October 1849.

16. To John Combe, of Leeds, in the county of York, civil engineer, "improvements in machinery for heckling, carding, winding, dressing, and weaving flax, cotton, silk, or other fibrous substances."—22d October 1849.

17. To Alexander Parkes, of Harborne, in the county of Stafford, chemist, "improvements in the deposition and manufacture of certain metals, and alloys of metals, and improved modes of treating and working certain metals, and alloys of metals, and in the application of the same to various useful purposes."—24th October 1849.

18. To William Conrad Finzel, of the city and county of Bristol, sugar refiner, "improvements in the processes and machinery employed in and applicable to the manufacture of sugar."—24th October 1849.

19. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in machinery for planing, tongueing, and grooving boards or planks, being a communication from abroad."—24th October 1849.

20. To David Owen Edwards, of Sydney Place, Brompton, in the county of Middlesex, surgeon, "improvements in the application of gas for producing and radiating heat."—26th October 1849.

21. To William Henry Ritchie, of Brixton, in the county of Surrey, gentleman, "improvements in fire-arms," being a communication from abroad.—31st October 1849.

22. To John Mercer, of Oakenshaw, in the county of Lancaster, and William Blythe, of Holland Bank, Oswaldtwistle, in the same county, manufacturing chemists, "improvements in certain materials to be used in the processes of dyeing and printing."—31st October 1849.

23. To Charles Cowper, of Southampton Buildings, Chancery Lane, in the county of Middlesex, patent agent, "certain improvements in the

manufacture of sugar," being a communication from abroad.—2d November 1849.

24. To Joseph Lowe, of Salford, in the county of Lancaster, surveyor, "certain improvements in grates or grids, applicable to sewers, drains, and other similar purposes."—2d November 1849.

25. To John Holt, of Todmorton, in the county of Lancaster, manager of the Waterside Works, "improvements in machinery or apparatus for preparing cotton and other fibrous substances, parts of which improvements are applicable to machinery used in weighing."—5th November 1849.

26. To William Buckwell, of the Artificial Granite Works, Battersea, in the county of Surrey, civil engineer, "improvements in compressing or solidifying fuel."—5th November 1849.

27. To Thomas John Knowleys, of Heysham Tower, in the county of Lancaster, Esq., "improvements in the application and combination of mineral and vegetable products; also in obtaining products from mineral and vegetable substances, and in the generation and application of heat."—5th November 1849.

28. To Henry Crosley, of the firm of Henry Crosley, Sons, and Galsworthy, of Emerson Street, in the county of Surrey, engineers and coppersmiths, "certain improved modes or methods of, and apparatus for, heating and lighting, for drying substances, and for employing air in a warm and cold state, for manufacturing purposes."—7th November 1849.

29. To Henry Knight, of Birmingham, in the county of Warwick, "certain improvements in apparatus for printing, embossing, pressing, and perforating."—12th November 1849.

30. To Adam Yule, of Dundee, master mariner, and John Chanter, of Lloyds, London, and Arnold Terrace, Bromley, in the county of Middlesex, gentlemen, "improvements in the preparation of materials for coating ships and other vessels."—14th November 1849.

31. To Alexander M'Dougal, of Longsight, in the county of Lancaster, chemist, "improvements in recovering useful products from the water used for washing, and in treating wool, woollen, and cotton fabrics, and other substances."—14th November 1849.

32. To John Parkinson, of Bury, in the county of Lancaster, brassfounder, "improvements in machinery or apparatus for measuring and registering the flow of liquids."—14th November 1849.

33. To Peter William Barlow, of Black Heath, in the county of Kent, civil engineer, "improvements in parts of the permanent ways of railways."—14th November 1849.

34. To George Edmond Donisthorpe and John Whitehead, of Leeds, manufacturers, "improvements in preparing, combing, and heckling fibrous matters."—16th November 1849.

35. To Walter Crum, of Thornliebank, in the county of Renfrew, in Scotland, calico-printer, "certain improvements in the finishing of woven fabrics"—16th November 1849.

36. To Alfred Barlow, of Friday Street, in the city of London, warehouseman, "certain improvements in weaving"—19th November 1849.

37. To Charles Edwards Amos, of the Grove, Southwark, in the county of Surrey, engineer, and Moses Clark, of St Mary's Crag, in the county of Kent, engineer, "improvements in the manufacture of paper, and in the apparatus and machinery used therein, part of which apparatus or machinery is applicable for regulating the pressure of fluids for various purposes."—21st November 1849.

38. To Joshua Proctor Westhead, of Manchester, manufacturer, "improvements in the manufacture of fur into fabrics," being a communication from abroad.—21st November 1849.

39. To John Jordan, of Liverpool, in the county of Lancaster, engineer, "certain improvements in the construction of ships and other vessels navigating on water."—26th November 1849.

40. William Garnett Taylor, of Burton Hall, in the county of Westmoreland, gentleman, "improvements in lint and linting machines, which improvements in linting machines are in whole or in part applicable to other purposes."—29th November 1849.

41. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in stoves, grates, or fire-places, and in warming or heating buildings," a communication.—30th November 1849.

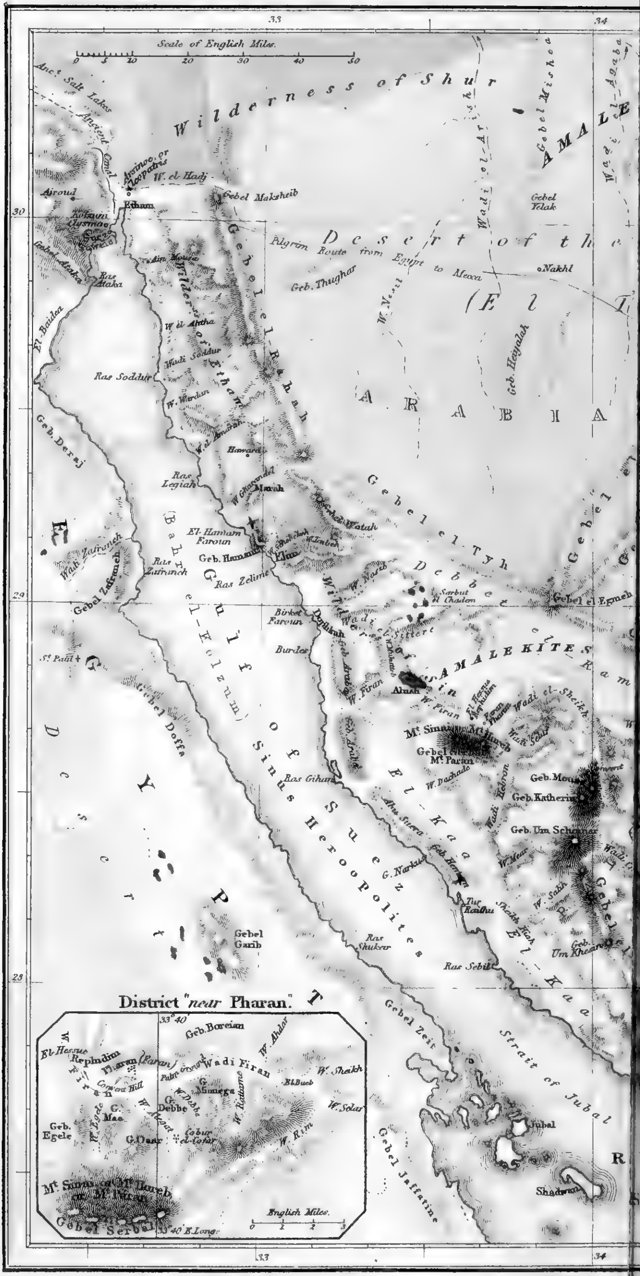
42. To John Buchanan, of the city of Edinburgh, civil engineer, "improvements in corks, valves, or stoppers, and in the use of flexible substances for regulating or stopping the passage of fluids, and also in making joints of tubes and pipes, or other vessels."—30th November 1849.

43. To Charles Morey, citzen of the United States of America, and now residing at Manchester, in the county of Lancaster, gentleman, "certain improvements in machinery or apparatus for sewing, embroidering, and uniting or ornamenting by stitches, various descriptions of textile fabrics."—3d December 1849.

44. To James Worsdel, of Birmingham, in the county of Warwick, manufacturer, "certain improvements in the manufacture of envelopes and cases, and in the tools and machinery used therein, parts of which may be applied to other purposes."—7th December 1849.

45. To John Macintosh, of Berners Street, in the county of Middlesex, "improvements in furnaces and machinery for obtaining power, and in regulating, measuring, and registering the flow of fluids and liquids."—10th December 1849.

46. To Peter Fairbairn, of Leeds, in the county of York, machinist, and John Hetherington, of Manchester, in the county of Lancaster, machinist, "certain improvements in machinery for preparing and spinning cotton, flax, and other fibrous substances."—11th December 1849.

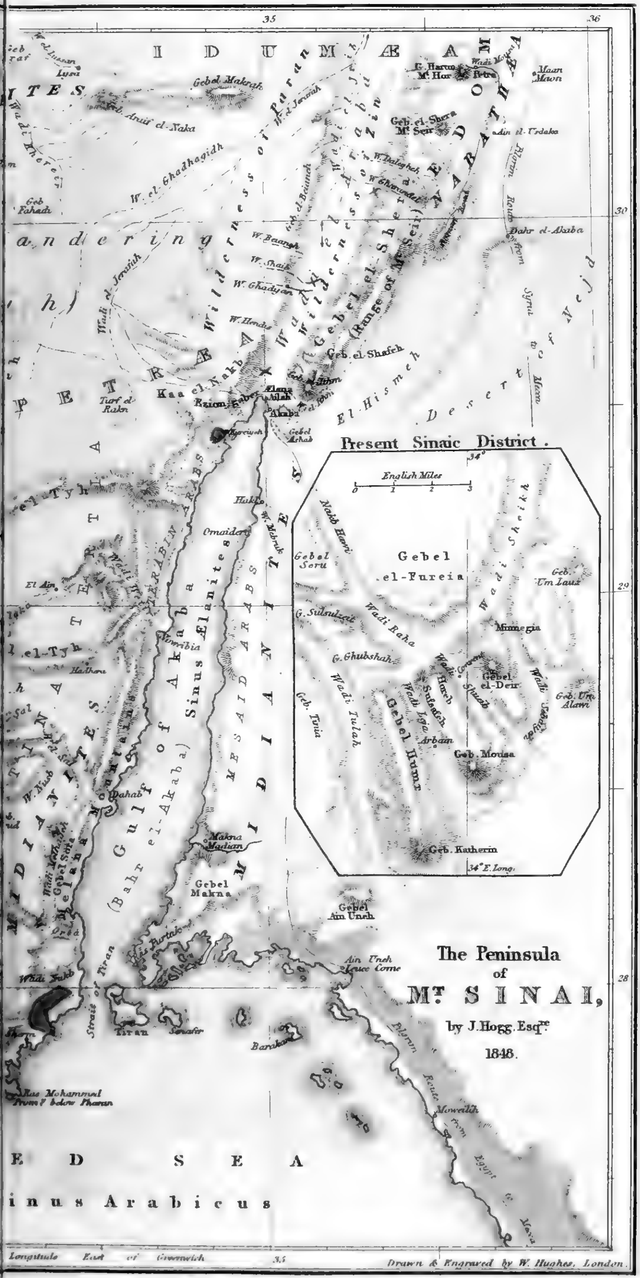


Diluvium, Alluvium, Sand, Marine formation, Coral rocks, &c. Tertiu[…]

& Marl. Limestone of the Cretaceous Series. Volcani[…]

-stone & its Marl (Lower Cretaceous Series.) Unstratified or Crysta[…]

Hornblende, Mica & Clay-slates, &c. Veins of differe[…]



...ndstone, Upper Nubian Sandstone, & Oldest Diluvium. [ ] Tertiary Limestone.
...ks: — Basalt, & Basaltic Lava. [ ] Older Sandstone, Nubian Sand-
...Rocks, — Granite, Syenite, Porphyry, Diorite, Greenstone, Felspar, Gneiss, Chlorite,
... ancient Copper & Iron Mines. ✕✕ Mineral springs.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

On the Geography and Geology of the Peninsula of Mount Sinai, and the adjacent Countries. By JOHN HOGG, M.A., F.R.S., F.L.S.; Honorary Secretary of the Royal Geographical Society, &c. (With a coloured Geological Map.) Communicated by the Author.*

IN giving a short account of the geography and geology of the Peninsula of Mount Sinai, and of the countries immediately adjoining to it, I propose, in the

First place, to take a brief survey of the principal features of the Peninsula, beginning at Suez, and following the Sinaic coast of the Gulf of Suez, as far as its southern point at Ras Mohammed, and thence up the Sinaic coast of the Gulf of Akaba to its northern extremity.

Secondly, From the Kalah-el-Akaba down the Arabian shores of that gulf, I will describe that region, the small islands of Tiran, Senafer, and others, which lie to the south of Ras Furtak, and then the districts near Ain Uneh, and Moweilih, on that Arabian coast.

Thirdly, Passing from Moweilih up the Gulf of Akaba, I will give some views of it, of the Wadi-el-Araba, and of the neighbouring mountains as far north as the ruins of Petra.

Fourthly, On the rocks of Petra I will offer a few re-

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* This memoir was read at Birmingham, on 17th September last, before the Section of Geology and Physical Geography of the British Association for the Advancement of Science.

remarks; also on Gebel-el-Harun and the mountains of the Nabathæan chain, those to the north-west of Wadi-el-Jerafah, the great desert of El Tyh,* and the range of El Egmeh, the Sinaic Mounts, Gebel-el-Tyh, and Gebel Thughar.

Fifthly, Starting again from Suez, I will shortly notice that eastern portion of Egypt which adjoins upon the Gulf of Suez, nearly as far south as the supposed site of Myos Hormus.

And Sixthly, I will conclude with some observations on the general features, the geological formations, the minerals, and ores of the Peninsula of Mount Sinai.

The map which accompanies this memoir was carefully reduced by Mr William Hughes, from one on a much larger scale, that was drawn and compiled by myself, from the maps of Professor Lepsius, Herr Russegger, Dr Robinson (executed by Kieppert at Berlin), and from the charts of the late survey of the Red Sea by Messrs Moresby and Wellsted of the Indian Navy; the cartoons or small plans of the "District near Pharan," and of the "Present Sinaic District," were reduced by the same able artist from the larger ground-plans comprised in the works of Professor Lepsius and Dr Robinson respectively.

In order that it should be as distinct as possible, and not rendered obscure and confused by a great crowd of names, I have only inserted those of the principal places: the Arabic, the ancient classical and scriptural appellations, I have given with as much accuracy as I could. It has been recently engraved by Mr W. Hughes, for the Transactions of the Royal Society of Literature, in purpose to illustrate my paper on the true Mount Sinai, which is published in Vol. III.,

* The article *al* or *el*, when preceding any word beginning with a t, r, sh, z, n, th, &c. (*solar* letter) ought strictly to be written Et Tyh, Er Rahah, Esh Sheikh, Ez Zeit, En Nakb, Eth Themed, &c.; or, as it may be abbreviated, thus—E'Tyh, E'Rahah, E'Sheikh, E'Zeit, E'Nakb, E'Themed, &c. In the pronunciation of the latter, care must be taken to give a *double* force to the *solar* letter. But with the vulgar people, this more correct mode of writing or pronouncing is seldom practised, and in the following pages I have accordingly retained the commoner form, solely because it is better understood.

Second Series, Part 2, of those Transactions; and I have coloured it geologically, chiefly after Russegger's beautiful maps of Egypt and the Sinaic Peninsula, lately published at Vienna; but the latter I have corrected in some places, so as to agree with the descriptions of Burckhardt or other travellers who have *personally* visited them.

First, The small and poor town of Suez, called *Suweis* by the Arabs,—meaning a " little mouth,"—now a place of such constant communication by the English, since the late establishment of steam navigation to and from India, is situated on a low neck of land, but little raised above the level of the high water of the gulf. The land there consists of gravel and sand placed upon rocks of a very recent marine formation; it is quite barren, having neither vegetation nor good water. The view of the opposite coast-line of the Peninsula, extending far southwards, with the more lofty mountain-summits rising behind it, is extremely fine. Near the town there is a ford upon a long narrow sandbank, which stretches across to the eastern shore, the water at low tide not exceeding five English feet in depth. Whilst the tide at Suez, and on the shoals to the north, is said to rise about seven feet. The plain behind the town is a hard diluvial gravel; proceeding northwards, a little short of the line of 30° north latitude, some heaps of rubbish or mounds point out the supposed site of the ancient town of *Clysma*, derived, I suppose, from Κλύσμα, which means " an estuary." *Colsum* or *Kolzum*, corrupted from that word, is still in use with the Arabs, who now call the Gulf of Suez *Bahr-el-Kolzum*, that is, " the Sea of Kolzum."

Around the head of the gulf are extensive shoals, apparently of sand, mingled, according to some, with coral; these are left bare at low tide. There exists evidence of a gradual filling up of the north part of the gulf, probably by the drifting in of sand from the north of the desert. This sand, brought by the north-east winds which often blow, is carried into the sea, and the process of filling up is still going on. The sea once extended much further north, and perhaps to the north-east. The ground at the north end is often covered by the sea in winter when south winds prevail, and pools of sea-water are left stagnant. The soil there is a fine *sand* rendered *solid* by the action of the waves. In some parts it is covered with a *saline crust,* exhibiting strips here and there quite white with shells. The marshy land on the north and north-west is soft, wherein the camels sink: this is called, as Burckhardt says, like all salt-marshes, "*Szabegha,*" and it is *below* the level of the sea. The broad tract of sand dividing these marshes from the gulf is about a yard higher than that level. The banks of the great canal of Ptolemy are visible, and may be traced for some distance northwards, and the bed of it resembles "a low and narrow *Wadi,*" that is to say, a valley or bed of a river.

According to M. de Laborde, the levels taken through the country, between Suez and Tineh, near Pelusium, gave a depth of 24 feet *below* the levels of the Mediterranean and the Red Seas.

On rising ground, a little beyond the head of the Gulf, *Arsinoe* or *Cleopatris** is, I believe, correctly placed. At this day several mounds would seem to fix the former position; but I am not aware that any travellers have yet made any excavations there, or in the conjectured site of *Clysma.* The gulf to the north of Suez, Dr Robinson says, "was anciently not much wider at its entrance than at present, while further north it spread itself out into a broader and deeper bay."†

From the extremity of the gulf, in a north-eastern direction, the ideal boundary line of Africa and Asia is laid down. Passing to the south, along the west coast of the Peninsula, opposite to Suez, beds of marine deposits, with low hills of sand and gravel, prevail; the latter, indeed, continue to the north-east and east for a great distance, thence southwards a gravelly desert plain succeeds.

Gebel-el-Rahah on the east extends parallel to the sea; this is a long range of mountains, forming "an ascent to the high plateau of the vast interior desert." About two-thirds of this range and of the wilderness of Etham, consist of a

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* See as to the canal and this town, *Strabo,* Geograph. lib. 17, 20, p. 1140, Edit. *Falconer,* tom. ii., Oxon. 1807; also *Pliny,* Nat. Hist., lib. vi., cap. 29.

† Biblical Researches, vol. i., p. 88.

tertiary formation of limestone and marl, called *kalk* and *mergelreihe* by Russegger; the range itself reaches to about 300 English feet in altitude. According to the best authorities, the Israelites probably *came out* from Egypt a little south of the opposite shore to Suez. The distance being from three to four miles.

Ain Mousa, or the *Springs of Moses*, number seven fountains; but the water is dark-coloured and brackish, being partly impregnated with lime and salt, and it deposits a hard substance, most likely a *calcareous tufa*.

Directly south the road goes over *sandhills*, appropriately named by the Arabs *El Kubeibat*,—the little domes,—on account of their form; then follows a gravelly level tract for several miles. The small valleys, called Wadi-el-Ahtha, Wadi Soddur, &c., are mere depressions in the ground, whose level is only a few feet lower than the adjacent desert. Soon after passing near Ras (*Cape*) Soddur, rocks of the *cretaceous* series of the *secondary* formation are found, as also in the adjoining hills. Near the sea-shore, in some places are deep *sands*, which, with the wind, create small mounds, whereon tamarisk trees grow. With the more violent winds these *sands* are blown over and driven about. After Wadi Wardan, there occurs a ridge of chalky limestone, exhibiting in many spots plenty of crystallized sulphate of lime (*selenite.*) At the head of this valley, the highest mountain of the range is named Gebel Wardan; it rises to 400 feet above the sea, and is of the same *secondary* formation. Before reaching Wadi-el-Amarah, a hilly country is entered, which consists of chalk and flint disposed in very irregular strata.

The fountain of Howara (*Ain Howarah, i. e.*, Spring of Corruption), is bitter and saltish; it is placed on a large mound composed of *calcareous tufa*, or a whitish rocky substance, formed by deposit from the water. This district is, according to Russegger, and also for some miles around, except on the east, which is *secondary*, of the same *tertiary* beds of lime and marl as the north-western portion of Gebel-el-Rahah. The mountain at the eastern extreme of Wadi Gharandel is called Ras (*Cape* or *Headland*) Wadi Gharandel. The Arabs told Burckhardt that the bed of a valley, or

Wadi, could be traced from thence across the desert El Tyh all the way to the sea. This, I conclude, can only be one of the branches of Wadi Nesil, which crosses that desert on the pilgrim route, between Gebel Thughar and Nakhl; and thence it probably joins the Wadi-el-Arish,—if Russegger be correct in laying down the course of *that Wadi* to the *west* of Gebel Yelak and Gebel Mishea. At that Ras, the range of Rahah terminates; and another chain, bending south-east and east, receives the name of Gebel-el-Tyh, and stretches out into two branches as far as the borders of the Gulf of Akaba.

Many trees of the tamarisk, acacia, and date-palm, are seen in Wadi Gharandel, where rock-salt occurs; the last likewise is detected in Wadi Useit, the *Oszaita* of Burckhardt,—a ravine between high chalky crags; consequently, the springs are all brackish and *bitter*, designated by the Arabs by the words *Morr* or *Morra*. After the small stony plain at the south-west extremity of Wadi Gharandel is passed, a more mountainous country begins, and is a continuation, for some distance southwards, of the *cretaceous* series, or *Kreidereihe* of Russegger.

The high limestone mount, through which the line of 33° east longitude passes, and which forms a conspicuous promontory in the gulf, is known to the Arabs under the appellation of Gebel Hamam, or the "Mount of the Baths," because at its northern end are situate the hot *sulphur springs*, called El Hamam Faroun, "Pharaoh's Baths," and afterwards more fully noticed.

Gebel Hamam itself is of the *secondary* formation, or *cretaceous* series, *chalk* mostly covered with *flints*, which give the entire mountain a dark appearance, except where the *chalk* is most visible. It is said to be about 1500 feet in height above the sea.

Wadi Schebekeh, Burckhardt's *Shebeyke* (a net) is a broad valley, having steep cliffs on both sides; there the strata are calcareous, and run in even horizontal beds. Much rock-salt occurs in Wadi Taibeh ("good") in pieces beautifully white; for this reason the water in both Wadis Schebekeh and Taibeh is very brackish.

Thence into Wadi Hommr, or *Humr* of Robinson, where are precipitous sides of chalky limestone, from 200 to 250 feet in height, with *flints* in some parts. On the north, the high pyramidal peak, termed *Sarbut-el-Gemel*, the "hump of a camel," is of the same limestone, and is connected with Gebel Watah by low ridges. At Gebel Zuweibin, on the south, the *sandstone* region is entered upon the right, whilst on the left, or east, the *limestone* is chiefly continued. Here all this lofty portion of Gebel-el-Tyh, possessing a long, regular, and level ridge, appeared to be calcareous. The plain named Debbet-el-Ramleh, or "plain of sand," is broad, sandy, uneven, with broken ridges and water courses; it commences to the south-east at an elevation of about 1600 feet, and extends to within a few miles of the Gulf of Akaba, varying in altitude during this distance; for at Alahadar, near the line of 34° east longitude, it is found to rise to above 4000 feet above the sea. The south-eastern portion, or branch, of the Tyh mountains is also *sandstone*.

This *Sinai sandstone*, with its *marls*, belongs to the *lower secondary* formation, which Russegger names "untere Kreidereihe," the *lower cretaceous* series.

The mountain on which remain those remarkable, and for long esteemed "mysterious monuments," called Sarbut-el-Chadem, "knob of the ring," is of this *sandstone*, reddish-brown, or mostly red, with strata of different shades here and there; it is 600 or 700 feet in height, the ascent to which is difficult, and even dangerous, for most sides of it are broken into deep precipices. The numerous *stelæ*, still existing on the west side of the small elevated plain, or rather ledge, are covered, according to Professor Lepsius, with hieroglyphics, which record the working of the neighbouring *copper-mines*. He says, the whole country was named in hieroglyphics, *Mafkat*, "the *copper* land."* Had any geologist visited these singular ruins, he must have discovered the heaps of *scoriæ*, and, consequently, the *true origin* of the district, and so would *not* have considered it as a place of Egyptian *pilgrimage*, as some travellers have incorrectly done.

* "Tour from Thebes to the Peninsula of Sinai," by Professor Lepsius, translated by C. H. Cottrell, p. 14.

In fact, both here and in the adjoining Wadi Maghara, there existed colonies of Egyptian miners from the earliest period. South of this most interesting spot, and especially on the south-east, in Wadi-el-Seih, the sandstone ceases, and porphyry with greenstone (*grünstein*) prevails. The mountains become more lofty and grand; and in Wadi Barak, at about 3000 feet in elevation from the sea, the formation is *granitic*, principally, indeed, porphyry with a belt of greenstone. Farther to the south-east, in Wadi Berah of Robinson, the rocks are porphyry and red granite, occasionally veined with grey granite. But in the adjoining east Wadi-el-Ush (*Alush*), gneiss, mixed with granite, is seen; then the central region of the remaining southern division of the country, being nearly *one-third* of the entire peninsula, strictly so considered, presents a magnificent rugged and alpine mass of hypogene schists, granite, and porphyry.

Returning again to the coast at the bottom of Wadi Schebekeh, and passing round the low sandy projecting point at Ras Zelime (*Elim*), where there is a harbour for small ships, a large dreary plain of marine formation, sand, and stones, is entered upon, which somewhat ascends to the bitter spring El-Morkha, the *Marcha* of Dr Lepsius, and *Murkhah* of others. This Bir, or "well," Burckhardt describes as being in the *sandstone* rock, near the foot of the mountains on the east; but since its water is very *bad*, it most probably issues either from the chalk, wherein *sulphur* may be present, as at Gebel Hamam, or through the plain itself, which is of marine formation, and very likely impregnated with *salt*. Moreover, the Wadi-el-Dhafary, or *Dhaph'ri* (Daphka), which is from Burckhardt's description distant about one and a quarter mile to the south-east by south from Morkha, near Wadi Naszb, which is clearly in the *sandstone* formation, "furnishes the only *sweet water* between Tor and Suez;"* and this fact, I think, would prove that the latter spring rises in the *sandstone* strata, unmixed with sulphur or salt, and not in those chalky or marine beds of the plain of Morkha.

* Travels in Syria, p. 623.

In Wadi-el-Naszb the remains of old *copper* mines have been noticed, as also in Wadi Maghara; and near these occurs the mineral named in Arabic, *El-Kohal, Kohol*, or *Kohl*, which is antimony. At the head of Wadi Sawuk are beds or veins of greenstone. Wadi Kamileh consists of sandstone, though, at its junction with *Wadi-el-Seih*, it is succeeded by greenstone, porphyry, and disintegrated granite.

Again, upon the arid and sandy plain before described, near the Birket Faroun or "Pharaoh's Pool," Wadi Shellal or the "Valley of Cataracts" opens; thence, the difficult pass, *Nakb-el-Butera* of Lepsius, the *Badera* of Burckhardt, and *Buderah* of others, which is of sandstone, leads to *Wadi-el-Sittere*, the latter traveller's "*Seyh Szeder*," where the sand rocks present abrupt cliffs, 20 or 30 feet high; large masses having separated themselves from the sides, lie at their base in the valley. These are thickly covered with *inscriptions* in the strange Shemitic characters.

From hence, the well-known and remarkable *Wadi Mukatteb*, meaning, in Arabic, the "Written Valley," is entered; the rocks there are also of a red sandstone, consisting of quartz grains mixed with mica; they, as in Wadi-el-Sittere, are everywhere inscribed in the same unknown letters. Proceeding along Wadi Firan, Burckhardt* says he "met with a kind of *basaltic tufa*, forming low hills covered with sand;" then he observed the road to be "overspread with silex" (*flints*), and the chain of *granite* mountains, commencing on the north-east, continued parallel with the road. On the south-west side, before coming to Wadi Romman, the sandstone ends, and granite begins; but between that valley and El Hessue, the formation is principally gneiss. From the last place, Wadi Firan assumes a more cheerful aspect, and a stream of pure water, flowing through a great part of the year, loses itself there in a cleft in the rocky ground, after having irrigated the valley above for several miles. This Wadi tends in a south-east direction, and after passing the isolated mountain named El Bueb, it joins Wadi-el-Sheikh, which then proceeds more eastwards, and leads to the pre-

* Syria, p. 620.

sent Sinaic district. At that craggy mount, Dr Lepsius was surprised in beholding many mounds of earth; the presence of which he could only account for, by conceiving that this portion of the valley had, at an early period, formed *a lake*, and which the general appearance of the locality seemed to confirm. As far as that spot, or nearly to El Bueb, Wadi Firan is considered the most fertile valley in the whole Peninsula; for, from that upper extremity, an uninterrupted row of gardens and date plantations, called by the Arabs, "El Gennaïn fel Wadi Firan;" "the gardens in Wadi Firan" extend downwards for three or four miles. The clear rivulet before mentioned, springing out of the ground in a remarkable manner about a mile below El Bueb, affords plenty of water to that richly-cultivated tract.

Gebel Serbal rises on the *south** side of this beautiful valley, and directly opposite to the ruins of the very ancient Amalekitish city of Pharan. This magnificent mountain, sometimes called also *Faran* or *Paran*, is of granite, having five principal peaks, which rise like cones, and are distinguishable from a great distance. The height of the *second* peak from the west is, according to Rüppell (and which he considered the loftiest), 6342 Paris feet, or 6759 English feet, above the sea. Mr Bartlett having ascended one of these peaks, probably that which Burckhardt, in 1816, was the *first* to climb, gives the following graphic description of the mountain, and of the panorama from it:—"We stood on the top of a rounded edge of polished granite, dangerously shelving down, from which the precipice, on either hand of us, sunk sheer 2000 feet below. We could not see the chasm by which we ascended; but looked across it to the other peaks,

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* It is, I believe, to Burckhardt that we *first* owe the correct position of *Mount* Serbal, as laid down in the map published in 1822, which accompanies his posthumous work on "Syria," edited by Col. Leake. In the edition, *Paris* 1818, of the splendid map of Egypt and the Peninsula of Sinai, made from the surveys of Col. Jacotin of the French army under Napoleon ("Description de l'Egypt"). *Mount Serbal* is placed, in Flle. II., on the *north* of Wadi Firan, and the town and convent of Faran are on the *south* of it. Such are also the errors in Niebuhr's "Tabula Itineraria" (Tab. XXIII.), engraved in his "Descrip. de l'Arabie," Copenhag. 1773, and in the ancient "Peutingerian Table," fol., Lips. 1824.

all consisting of similar terrific masses of granite, wildly upthrown from beneath by some awful convulsion, each capped with a similarly rounded weather-beaten summit, and each with the same precipitous sides. The appearance of the mountain itself was fearfully sublime, and the view from it, except where its intervening crags formed an impediment, all but boundless—the whole Peninsula lay at our feet. Though hazy, we could see very far up the Red Sea, towards Suez, making out different points of our route, and we looked across it far into the Egyptian Desert. Tur, and the coast downwards, also appeared through a cleft. The stern and steril mountains of the Peninsula lay below us, an intricate labyrinth, a confused sea of many coloured peaks, black, brown, red, and grey, with here and there a narrow valley of bright yellow sand, peeping through, Wadi-el-Sheikh being the most conspicuous opening; beyond these arose, irregularly, the plateaux of the Great Desert and the ranges of El Tyh, which support it, all fading away into a misty heat, but for which the hills of Palestine might perhaps have been seen in the remotest distance. The solitudes of (the present) Sinai, a darker, bolder congregation of wild peaks lay to the right, stern and black, and awful in colouring, and cut off all view of the Gulf of Akaba in this direction. Nothing could be more desolate than the vast region, over which floated the scorching haze beneath us, from east to west, from north to south; mountains, plains, valleys, and sea, formed by the slow abrasions and depositions of ages, and then fractured and up-heaved by the agency of fire, or protruded in molten masses through fissures thus created, seemed stamped by nature with eternal barrenness, and unfit for human habitation."* Having come back to the coast, a little north of Burdes, where red and yellow sandstone rocks are seen, then marine sand, gravel, and stones, extend from the low point Ras Burdes round the headland Ras Gihan close along the shore; from the former point, the chain of the Araba mountains runs to the south, a little beyond the latter Ras. This prominent chain is of the same secondary or cretaceous lime-

* Forty days in the Desert, p. 64.

stone as that which prevails over the Desert El Tyh, by Gebel Watah, and breaks off by the plain near the well of Morkha. Crossing the more southern part of the Araba range, the head of the vast plain El Kaa is entered upon; this, commencing at the western foot of Gebel Serbal, continues with a gentle slope, without interruption, to the south extreme of the Peninsula. It is some miles in width, varying a good deal in places, and is perfectly bare and arid. Its geological formation I will afterwards more particularly describe, and will here only observe, that it consists of beds of diluvial gravel, or of stones and rocks brought down by the winter torrents from the many Wadis, which open into it, and of sand, in part carried by the winds from those valleys, and in part drifted from the shores of the Gulf of Suez. Travelling along this plain from the southern end of Gebel Araba on the west, and Wadi Dachade, under the Serbal, on the east, a strip of the same Sinai sandstone is found, according to Russegger, on both sides, on that which adjoins the granitic formation on the east, as well as on that next the coast, which I have called, following Dr Lepsius's map, *Abu Suera*.

Here, near its south termination, Gebel Narkus, or the "Bell Mountain," is situate not far from the sea, from which a sandy plain slightly ascends to its base. Lieutenant Wellsted has given a neat lithographed view of it, and he describes its height as about 400 feet, and the material of which it is composed, a light-coloured friable sandstone. A mass, or rather an inclined plane, of very fine sand, rising at an angle of 40° with the horizon, rushes down in portions at times, and causes *hollow sounds* that the Arabs compare to the *ringing* of *bells*. That author has given an account of the *sounds* he heard on the spot, and also some remarks in furtherance of the explanation of them.*

The adjoining range, or Gebel Hemam ("death"), a limestone chain from 200 to 300 feet high, extends for about ten miles up to the north extreme of the Bay of Tur. Here, about

* Wellsted's Travels in Arabia, vol. ii, p. 26.

a mile from that town, at the termination of some marshy land, is El-Wadi, where many date trees grow luxuriantly. On the east of this place is situate a thermal spring, which Wellsted names Hamam Mousa, the "Bath of Moses." The water has a temperature of 86° Fahr., is bitter and salt in taste, and of a sulphureous smell. At Tur, in the bay, are many coral banks, but its harbour is very safe; and on each side to the north-east and south, a patch of the tertiary limestone and marl occurs; also a larger district of the same formation is observable a little south of Ras Sebil. Tur was formerly named Ραϊθοῦ, *Raithu* or *Raithe*: it is now the sole remaining town in the whole Peninsula.

Ras Mohammed, or more fully *Ras Abu Mohammed*—"Cape Father Mahomet"—or the "promontory below Pharan," is only a low point "formed of limestone mixed with fossils," as M. de Laborde describes it; or, according to Captain Newbold, of a "*tertiary* fossiliferous limestone."

The coast is rugged, and cannot be seen at sea further than three leagues and a half; and the land composing the *Ras* is a long narrow ridge, nearly divided, about six miles from the extremity, by a deep bay.

On the north, about five miles distant inland, the chain of granite mountains called Gebel-el-Turfa, the "Tamarisk Mountains," commences, and proceeds in a direction a little west of north to join the more lofty central mountains near Gebel-Um-Schomar.

Gebel-Um-Khesin, written *Om-Kheysyn* by Burckhardt, is considered one of the highest peaks of this chain, and is about 5000 feet above the sea-level; a patch of sandstone existing around Sherm (a creek) was first noticed hy Ehrenberg, and so coloured in his map of that region. Wellsted relates that red and yellow earths (*marls*) abound in the hills near the harbour, and are used by the Arab sailors for painting their boats. A few miles north-east of Sherm, Burckhardt writes:*

* Syria, p. 529. Burckhardt must here be understood to mean, that these were the *only* volcanic *crater-like* rocks, which he had seen in the Peninsula, because he observed *volcanic* remains in *two other* places, viz., basaltic tufa hills a little south-east of Wadi Mukatteb, and basaltic cliffs on the sea-shore west of the Isle of Kureiyeh. See pp. 620, 507.

he "saw for the first and *only time* in this Peninsula *volcanic* rocks." For a distance of about two miles, the hills presented perpendicular cliffs, formed in semicircles, and some of them nearly in circles, none exceeding 60 or 80 feet in height; in other places, there was an appearance of *volcanic craters*. The rock is black, with sometimes a slight red aspect, full of cavities, and of a rough surface.

But near Wadi Nukb, where are some plantations of date trees, according to the same traveller, the land is *chalky*, or calcareous, with fossil shells imbedded, and which, doubtless, formerly constituted a portion of that nearly opposite in the north of the Isle of Tiran, and in the north-east of the Ælanitic Gulf, near Ras Furtak. The separations, now occupied by the sea, in the Strait of Tiran, as well as in the straits between that isle, the point near Furtak, and the smaller isle of Senafer, have very possibly been caused by the same (or a like) volcanic action, which appears to have taken place in raising up the *basaltic* rocks between Sherm and a little below the line of 28° north latitude. This strip of *chalky* limestone seems to lie between sandstone strata north and south, and the alluvial, or marine deposit, along the sea-shore on the east of it.

After Wadi Nukb, a large sloping sandstone plain, called Mofassel-el-Korfa, stretches out towards Wadi Orta; over it, many beds of torrents, descending from the high chain of El Turfa, cross in their course to the Gulf of Akaba. Orta itself consists of greenstone, red porphyry, and granite, the last species of rock succeeding in Wadi's Chosib and Kyd. So also, in Wadi Melhageh (Burckhardt's *Molahdje*), a narrow and rocky passage enclosed by high cliffs, granite alone prevails. To the range intervening between the gulf and the last-named valley, as it is of a *dark* coloured granite, I have assigned the name of the "Melana (*black*) Mountains," Μέλανα Ὄρεα of Ptolemy, rather than to those volcanic hills nearer Sherm, as Burckhardt has done.

A little south of 28° 30′ north latitude is Dahab, or as Wellsted gives it, *Mersa Dahab*, signifying the "Port of Gold," which he adds, "is the only well-sheltered harbour in the sea." It is remarkable in shape, being a semicircle of marine formation, *i. e.*, of coral, on which is a layer of sand, slightly raised above high water level. Also the same author says "the epithet *Golden*" appears to have originated from "the sand in its vicinity resembling" *gold.* "The teeth of two ibices we received on board were covered with a substance resembling gold."* This sand may possibly be derived from pyrites, washed down by the winter rains from Wadi-el-Sal; the rocky sides of the lower part of which valley are sandstone.

Dahab extends into the gulf, about 2 miles beyond the line of the coast, and must have been in former ages, in accordance with every probability, an important sea-port. Wellsted observed, to the west of a long projecting point, some mounds like those covering ruins. Certain geographers have thought it likely that this was the site of *Eziongaber.* Bochart (*Canaan,* p. 764) having interpreted that word as meaning a "backbone" or "spine." Wellsted writes that the peculiar formation of the harbour at Dahab adds strength to the supposition, for within its *spine-like* ridge of rocks there is a spacious anchorage. Since that ancient port, however, was "*beside Eloth,* on the shore of the Red Sea, in the *land of Edom*" (1 Kings ix., 26), Dahab, which was not in that territory, but in the *land of Midian,* being distant full 75 miles from Ailah, could *not* possibly be said to be "beside *Eloth*" or *Ailah,* the ancient position of which is well determined. Busching† indeed, with less reason, places Eziongaber, at Sherm, *beyond* the Strait of Tiran. But Burckhardt (p. 523) suggested that "*Dahab* is probably the *Dizahab* mentioned in Deut. i., 1." This has been correctly acquiesced in by several authors.

Journeying northwards, the cliffs close to the sea at Ras Methna are of granite and red porphyry, crossing each other in irregular layers. On the shore near there, the granite sand carried down from the upper mountains, mixed with fragments of different rocks, has been cemented by the action of the waters into a beautiful *breccia.* Gebel Abu Ma consists of granite; but some miles further north, the promontory Ras

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* Arabia, vol. ii., p. 154.

† Geogr. of Asia, p. 620, Third Edition.

Bourka, or "Cape Veil," is so termed by the Arabs from being conspicuously *white* with a *chalky* limestone. The same sort of *chalk* appears before, just above the ground, near Noweibia, at the base of the moderately-high mountains, themselves composed of greenstone and granite rocks.

Noweibia (a diminutive Arabic word) signifies "springing up" like a fountain; here also is water, but brackish, as it is in the *same* cretaceous formation as along the coast of the Gulf of Suez. Some date plantations are met with, and much charcoal made from the acacia trees, is exported to Cairo.

Wadi *Boszeyra* of Burckhardt, the *El Sadeh* of Robinson, is of excellent grey granite, which the Arabs cut into stones for their handmills. This eastern granitic range presents very precipitous cliffs, about 800 feet high, from which a bank of gravel slopes down to the sea-shore; Dr Robinson more minutely particularises the cliffs in Wadi-el-Sadeh as affording alternations of granite and greenstone, occasionally capped with sandstone. According to that traveller, the mountains near the upper or western part of Wadi-el-Sal are chiefly greenstone, with some slate and veins of porphyry. Crossing the sandy plain, part of El Ramleh, which reaches to the foot of Gebel-el-Tyh, in the approach to Hadhera, that portion of those mountains is composed of sandstone strata, with layers of limestone towards its top. But the eastern ramifications of the south branch of Gebel-el-Tyh, beyond Hadhera, sink down into precipitous isolated hills and masses of sandstone rocks which have been rent to the bottom by narrow sandy valleys, or rather clefts. Then succeeds a plain of sand; and in Wadi Ghazaleh, leading eastwards into Wadi Wetir, the sides are perpendicular walls of sandstone, and very narrow. Next follow sandy plains, and rugged sandstone hills; and, indeed, at the north-west base of Gebel Samghy, the rocks exhibit sandstone, greenstone, and granite alternately.

I will here add from Burckhardt a sketch of the same district. The west portion of Wadi-el-Sal consists of the lower ridges of the primitive mountains; on the top is granite, lower down greenstone (*grünstein*) and porphyry; further on granite and porphyry cease, and the rock is solely greenstone, partaking of a slaty nature in many spots. Proceeding to the north-east, there is another valley beyond El Sal, where calcareous and sandstone rocks begin. Thence over the sandy plain of Ramleh, and then, descending towards Hadhera, a deep sandy valley covered with blocks of chalk rock is entered; and about a mile further is a pass between low hills of sandstone. In Wadi Rahab, the *Wadi-el-Ruwei-hibiyeh* of Robinson, flowing into Wadi Ghazaleh ('Gazelle'), the sands terminate; then, in another Wadi to the east, an alternation of sandstone and granite exists.

To the west is Wadi-el-Ain, *par excellence*, "the spring" or "fount," which lies on one of the chief roads, or rather camel paths, either to the convent by Wadi Zalaka and Ala-hadar, or from thence northwards, by the Mareikhi Pass over the central Tyh Mountains, west of Gebel-el-Egmeh, on to the table lands of the Great Desert. At El Ain, in a small plain, a fountain, and rivulet, and palm trees, delight the traveller; and they are placed nearly in the centre of the sandstone region between the northern and southern branches of Gebel-el-Tyh. Mr Bartlett relates, that beyond the fountain, "a singular *sandstone* mountain, in shape a truncated cone, rises, broken into strata of the most fantastic colouring—red, white, yellow, and purple,—glaring and flaming under a cloudless sky." The figure of the mount, as represented by him at p. 97, "Forty Days in the Desert," resembles on a smaller scale, the *highest* peak of *Mount Hor;* and its remarkable colouring too, *if* in fact it be *all sandstone*, is similar to that of the highly-tinted *sandstone* rocks at Petra. But I think the differently *coloured strata* of that mount will prove to be, on an examination of them, layers of granite, chalky limestone, sandstone, and porphyry, like the alternately-placed beds of these formations, which have been already detailed as seen in several Wadis contiguous to Wadi Wetir. Indeed, this seems probable, because Mr Bartlett adds, that after El Ain, and when he had approached "the jaws of a gloomy defile (*Wadi Wetir*), the *sandstone* gave place to the *dark purple* hue of the *porphyry*, precipices of which rose higher and higher."

The two great valleys of this portion of the Peninsula, *El Sal* and *Wetir*, are alike in their general form, though the latter is more curved northwards; they are in some degree parallel to each other, and each constitutes the principal channel for the waters that descend to it from the central mountains on the east side, and the many lateral Wadis, during the rainy season; and each conducts them into the Sea of Akaba. Again on the coast, and having passed round the chalk cliff Ras Bourka, which is washed by the waves, the traveller arrives at another spring of saltish water, named Soweira, (*Suweirah*, by Robinson, and *Zoara*, by Burckhardt,) most likely rising through the chalk or cretaceous beds, as at Noweibia: then the Cape or Ras Um Haye, "mother serpent," (the *Um-Haizeh* of the former author), makes the east termination of the northern branch of Gebel-el-Tyh. Before, however, reaching that point, immense masses, apparently of yellow sandstone, present themselves; these are intercepted by a row of granite cliffs between them and the beach, which are then crested with red sandstone; and an inclined bank of gravel and debris slopes from them to the sea.

The phenomena presented by the frequent alternations of the sandstone and cretaceous limestone, with the igneous and volcanic rocks, granite, porphyry, greenstone, &c., as well those which exhibit upheavings, displacements, or interruptions of the strata, as those which shew more simple disturbances, particularly in Wadi Wetir, and its neighbouring valleys along the coast of this gulf, near the east extremity of the Tyh range, and opposite to the Island of Kureiyeh, are highly interesting to the geologist; and they require his more minute exploration of the whole district.

From the last-named Cape (*Um-Haye*), the mountains become much lower in height towards the north, whilst the most lofty summit of the range on this east coast is that behind Noweibia. Beyond, to the north-east of Wadi-el-Huweimirat, the formation continues to be sandstone. And further on, near the spot called Wadi Mezeiryk, by Burckhardt, which Robinson's Arabs knew only by the former name, there appears what Burckhardt describes (p. 507), as "a range of *black basaltic* cliffs, into which the sea has worked several creeks," like small lakes, with narrow inlets to the gulf. Thence into Wadi Merakh, mistaken (according to Robinson), by Burckhardt for Wadi Taba, which opens upon a broad plain of gravel, sloping down to the sea. Next appears to follow the promontory, called by the latter, Ras Koreye,* (*Kureiyeh*), from the isle of that name ; and then a small bay opens to the sea over sands, opposite to which, at a short distance, stands Jezirat Kureiyeh, the " Island of Kureiyeh." Wellsted calls it " Pharaoh's Isle," *Jezirat Pharoun*, or more correctly *Faroun*. The Arabic word, *Kureiyeh* means a " town" or " village," or the " ruins" of either, and has been corrupted from the sound by De Laborde into *Graie* or *Graia*. It is a narrow granite rock, placed from north-west to south-east, and rising to 150 feet in height; it comprises two hills covered with ruins, and united by a low neck of land ; among these, " fragments of marble entablatures and pillars," were found by Wellsted ; and they are perhaps the remains of a temple or mosque.

I am inclined to agree with Herr Schubert† in considering this isle to have been the position of *Eziongaber*, or at least of a *part* of that very ancient port if an isthmus of rock, possibly of coral, ever united it to the peninsula, then the form of the harbour would have corresponded with the supposed origin of the word, viz., " a ridge of rocks like a *backbone*." This in time may have been destroyed by some igneous or *volcanic* power, of which remains are visible on the west, not far from the island, in the *basaltic* cliffs and creeks previously described. The island may originally have been used as the fortress, or rock of defence, of *Eziongaber*, and the rest of the town may have been erected on the Sinaic shore. This site fully answers the scriptural account of it, as being " *beside Eloth* on the shore of the Red Sea,‡ in the

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* Named in the map published in Burckhardt's " Syria," *C. Koreyk* (*k* being an error for *h* or *e*.) But the isle is placed on the Arabian instead of the Sinaic coast.

† Reise, vol. ii., p. 379.

‡ 1 Kings ix. 26. The Septuagint translation is, *Αἰλὰθ ἐπὶ τοῦ χείλους τῆς ἰσχάτης θαλάσσης ἐν γῆ̃ 'Εδώμ*; and in 2 Chron. viii. 17, it is, *'Αιλὰθ τὴν παραθαλασσίαν 'εν γῆ̃ 'Ιδουμαίᾳ.*

land of Edom ;" and that of the Jewish Historian, in the beginning of the Christian era, as lying not far from Ælana.* So, Makrizi, in the fourteenth century of our era, writes, "*near Aila* was formerly situated a large and handsome town, called *Aszyoun*,"†—that is to say, *El-Zyon* or *Ez-Zyon* (gaber.)

But Dr Robinson states (i., p. 237), that this island was "the former citadel of *Ailah*, mentioned by Abulfeda‡ (about A.D. 1300), as lying in the sea ;" and very likely it might have been so occupied *after the decay* of Eziongaber ; yet, as it was eight miles or more *distant* from *Ailah*, it could not have afforded any protection to that town, but only to its passing ships.

Opposite to the isle, on the west, low hills of chalk and sandstone interrupt the granite, then the broad gravelly plain of Taba ; and afterwards the granite rocks again coming down to the beach, constitute the headland, *El Musry*, "the Egyptian," which projects into the sea southwards. Then the mountains on the north-west retire from the coast more inland ; but near the shore, small hills of conglomerated gravel and sand, nearly as solid as rock, continue beyond the extremity of the gulf.

Secondly, Around the head of the Ælanitic Gulf, nearly as far as Kalah-el-Akaba, the "Castle of the Descent," the sea has cast up a bank of sand and gravel, which is higher than the level of the Great Northern Wadi-el-Araba, and prevents the passage of any stream from it. On the shore near the Castle and the Grove of Date Trees (*Phœnix dacty-*

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* *Josephus*, Antiq. Jud. lib. viii., c. 6, s. 4, who calls it Ἀσιωγγάβαρος, and adds, it was then named *Berenice*. Now, the Arabic name of "*Pharaoh's* Isle," and those of the neighbouring valley and Cape of the "*Egyptian*," seem to me to give some confirmation, by tradition, that this place, or one near it, formerly bore the *common Egyptian* name of "Berenice," for which the present ones have been substituted by the ignorant Arabs.

† Burckhardt's Syria, p. 511.

‡ In the passage afterwards cited by Robinson (p. 252), from *Abulfeda*, it appears that he only says, Ailah "had a *small castle in the sea* ;" but he does not state its position, or its distance from Ailah. So it is doubtful whether *this island* formed the site of *that small castle*, or not.

lifera), the presence of which plant always indicates a neighbouring spring,* about two miles and a-half from the top of the Bahr, " sea," upon digging *sweet water* is soon obtained. Even on making a hole with the hands, the water slowly rises. This is at first *salt*, but when it is thrown out, the hole gradually refills with *fresh* water. This probably comes from springs from the East Mountains, which percolate through the gravel, here forming a slope to the sea.

Vegetables and several sorts of fruit are here plentiful; proving that the district is much more capable of cultivation, and naturally more fertile than that of the opposite coast. Fishes also abound in the gulf, which, after a shallow rocky platform of some yards in width, here becomes excessively deep. Behind the Castle of Akaba, to the south-east rises the lofty Gebel Ashab, " grey mount;" then, on the north-east, in a lower part, opens Wadi-el-Ithm, the " Valley of the Crime," which leads up to the sandy tract, El Hismeh, surrounded by mountains. Further to the north-east, appears the Gebel-el-Shafeh, or " Mountain of the Summit."

Traversing the beach southwards over gravel and sand, and having passed the ruins of Kasr-el-Badawy, or " Bedouin Fort," the rows of hills that then succeed on the left are granite. At Hakl, meaning a " field," which Wellsted writes *Hagool*, as broadly pronounced, there is a little harbour; next a considerable plantation of date trees, to which the sandy valley, Wadi Mebruk, so called by the common Bedouins, and signifying a " kneeling place" for camels, adjoins. The small Isle Omaider, not far from the beach, presents nothing worthy of remark. A singular phenomenon is afforded, along this west Arabian coast, by the Wadis, between the mountains, rising to 2009 feet in solid inclined planes of *sand*. The chain of mountains at some distance from the sea, exhibits steep walls and cliffs of granite, which, according to Wellsted, in many places attain the altitude of

* The following remarkable instance of this is mentioned by Dr Robinson (i. p. 238), which he witnessed on the opposite coast in Wadi Taba. There was "a large square hole dug in the ground, walled up with rough stones, like a cellar, in it had once been a *well*, but the bottom was now *covered with young palm trees*."

6000 feet. He describes the surface of them "as dark, veined with numerous traces of torrents of a lighter colour, everywhere intersecting it."

Makna or Magnah is placed a little off the sea-shore, and has about 200 huts, wherein the cultivators of the numerous date trees, that are grown in the valley, running east and west, reside. A large stream—a very rare thing in this region—flows down the Wadi, and waters the trees. The groves are fenced round, and within the enclosures, corn, figs, grapes, limes, and other fruits, and some vegetables, are cultivated. Some ruins are visible on a mound near the end of the date plantation, indicating, perhaps, the site of *Madian* of the Arabian writers, or *Modiana* of Ptolemy; and other remains are said to exist about ten miles distant, at a place known to the Arabs by the name of *Magharat Shoaïb*, meaning, the "Cave of *Shoaïb*," or *Jethro* (Moses' father-in-law), where are reported to be some inscriptions containing the names of kings.

Gebel Makna, a mountain of granite, elevates itself from the other side of the fertile valley to the south.

Thence extends the low steril beach of marine formation, shells and gravel, the latter being often blown by the winds into ridges, like waves; afterwards, as far as the south-west point at Ras Furtak, a portion of the secondary limestone again succeeds. This formation also is found in the northern part of the opposite Isle of Tiran,* whilst in its southern part, which ascends to a high peak, sandstone prevails; but, in places along its shores, coral and other marine substances occur. Much naphtha is procured from the island. An admirable harbour, though somewhat difficult to enter, lies on the north-east side. The extreme low and bare tongues of land, south and east of

* As Procopius (De Bell. Pers., lib. i., cap. 19) tells us, that land was visible on all sides of the sea, in proceeding from Aila, until one came to the Isle of *Jotabe* (Ἰωτάβη) which was distant, not less than 1000 (itinerary) stadia from that town; and from thence, the sea became very extended. This description perfectly agrees with the present Isle of *Tiran*. But it seems not unlikely that one of the neighbouring isles, called *Jubah*, or *Jubeh*, has retained the name of Jotabe, or *Jutabeh*, now abbreviated into *Jubeh*.

Ras Furtak, together with the numerous isles, consist principally of gravel, sand, and coral.

The neighbouring, but smaller Island of Senafer, circular in its shape, rises from the sea to about 150 feet. Many Arab vessels resort to its commodious and safe harbour, on the west side. The lower portion of the isle is coral and sand, but the hills are composed of sandstone, like that of Tiran. Shushuah is formed of yellow and red sandstone, with coral beds in certain places. These islands are barren, and without trees.

Barakan, Jubeh, and the many other isles opposite and near to this coast of Arabia, are all principally of coral and marine debris.

Following the shore, which is sandy in some spots, but marshy in others, and partly clothed with brushwood, the sheltered sea-port Ain Uneh or Ainunah, is approached. The lofty granite mountain, Gebel Ain Uneh, stands back to the north about 12 miles from the sea.

The town of Ain Uneh, built of coral rock, is conspicuous for its *whiteness* from afar; Wellsted has therefore, for this and other causes identified it with the ancient *Leuce Come* Λευκὴ Κώμη, *i.e.*, "*White Town*," with great probability. About one mile and a half inland, to the north-east, lies Wadi-Ain-Uneh between two bare cliffs, which greatly resembles Wadi Makna, and, like it, is famous for its excellent water: although luxuriant by nature, it is uncultivated. Some ruins, as well as those of an aqueduct are to be seen. Here are a few *Doom* Palms called "Dom" in Arabic (*Hyphæne Thebaica*), and date trees. The caravan of pilgrims going from Egypt to the Hedjaz passes along this district to Mecca. As the nature of the country here, and southwards, is more verdant and fertile than any before described, I will add the late Lieutenant Wellsted's account of it (ii., p. 166): "The country bordering on the sea-coast in the vicinity of *Ainunah*, and extending thence to Mowilah, affords better pasturage than any part of the coast which I have seen. In this tract the Bedouin huts are numerous, as well as large flocks of sheep and goats. Their residence here is, however, merely temporary; for, should the rains fail them,—an event occurring about once in four years,—they retreat from the low country to their mountains. In this elevated range,—and many of the hills are 6000 feet in height,—they possess abundance of water, and a never-failing supply of herbage. Several of the valleys also have extensive date groves and fields of *dhurrah* (*Sorghum vulgare*) cultivated by slaves."

This mountain range, more in the interior, is of granite, and beyond that again, according to Russegger, the *secondary* or older sandstone returns. The same traveller likewise considers the district along the coast, from near Ain Uneh, many miles to the south, and below the granite chain, as being of the same *tertiary* formation of *sandstone* and oldest *diluvium*, as that in the north-west, a part of the Isthmus of Suez, which borders on the great desert El Tyh. Portions, nevertheless, close to the sea-shore are of a more recent nature, such as gravel, sand, coral, broken shells, and other marine substances. Several Wadis open out upon this lower land, and lead to the interior.

The last station of any note on the coast here, is Moweilih, or Mowilah, or Moileh, very probably the "Phœnicum" of antiquity; but it has no harbour protected against the north winds. The castle, built with coral rocks and cemented with mortar, is chiefly used for the deposit of corn. Many huts and some rude houses are situate amongst the date plantation surrounding the castle. Grapes, melons, and other fruits, as well as plenty of vegetables, are grown in some adjacent gardens. *Moweilih*, signifying in Arabic "salt places," was most likely so named from its position on the sea-beach; yet the water is good, and is contained in wells lined with stone. The ground rises gradually for about seven miles, afterwards steep hills of considerable height succeed; some of these exhibit peaks of remarkable forms. The highest behind, or to the east of the town (*Moweilih High Peak*) rises to an altitude of 6500 feet above the sea-level.

Thirdly, Crossing the Red Sea in a north-west direction, and entering the Gulf or Sea of *Akaba*,—called by the Arabs "Bahr-el-Akaba," and by the ancient geographers Κόλπος Ἐλανίτης, Sinus Ælanites or Ælaniticus, from Ἀειλὰ, or Aila, Æla, or Ælana, a town formerly situated at its upper or north-east extremity, and by the Septuagint translators Ἑσχάτη θάλασσα,—through the Strait of Tiran the gulf has the appearance not of an open sea, but more of a long narrow lake, or rather, as Wellsted has described it, "of a narrow deep ravine" stretching above one hundred miles in a straight north-easterly line; and the scenery of the dark blue waters, nearly surrounded by the mountains, which in places jut into, and in others impend over them, and extend their summits to a considerable elevation, in some spots to 2000 feet perpendicularly from their shores, is represented to be extremely bold and splendid. This, too, is highly increased by the exquisite colours produced by a burning eastern sun. Some views in the gulf are stated by the same author to surpass "in magnificence and extent any he had previously witnessed;" and he adds, that their "wild and romantic aspects more than compensated for the monotony so characteristic of desert mountain scenery."

Until a recent period this sea, and indeed the whole country, from the Strait of Tiran to the walls of Jerusalem, or at least to the southern borders of the Dead Sea, constituted to Europeans a "mare ignotum," and a "terra incognita," which became first known through the indefatigable labours and enterprise of MM. Burckhardt and Rüppell, the latter having given to geographers the most accurate map of the Gulf of Akaba until that lately published from the surveys made under the authority of the East Indian Government. It does not appear that any great tide sets in and out of, or that any very rapid currents take place at the Strait of Tiran, or between that island and the contiguous shore of Arabia; probably the *depth* of the sea causes a gentle and comparatively *equal* flow of water.

Wellsted mentions, that in one spot in that gulf, *no bottom* was found at 200 fathoms, so close to the beach as *fifty* yards; and in another, several yards only from the shore near Akaba, the water was of "an *almost unfathomable* depth." In sailing up the gulf, the mountains on the right hand are much higher than those on the left, but they become lower towards the north in the approach to Hakl, before which place the more lofty chain turns inland. One, however, is struck by the absence of boats upon its surface; this is caused by the violence of the winds suddenly raising up huge waves, which are the dread of the Arab sailors in their rude vessels.

The view near the head of the gulf is more pleasing and less gloomy than at the southern part of it. Wellsted describes (ii., p. 145), the prospect as seen from the Isle of *Kureiyeh*, thus :—" Instead of bold naked precipices rising abruptly from the sea, we have here a succession of sandy capes, sweeping into the waves at nearly the same angle; their inclination being the same as the valleys of which they are but a continuation." . . . . " Neither boats nor vessels animate the picture, and it has the appearance of a vast and solitary lake. On the other hand, beyond the extremity of the gulf, we obtain an extensive view of the valley of El Araba. For some distance it resembles a broad plain dotted with trees: but the mountains which bound it continue, as in the Sea of Akaba, in a *straight* direction, and the gulf is therefore merely a *prolongation of the valley*, and they form, thus united, a bolder, more extensive, and more regular feature than can probably be paralleled in any other portion of the globe."

At the head of the gulf, on reaching the foot of the west ascent, *El Nakb*, the hills of *conglomerate* already mentioned (p. 212), sink down into a steeply-inclined plain of *gravel*, extending far to the north. Next come low hills of crumbled granite, and then succeed the more lofty granitic cliffs. For the purpose of shewing how the sandstone, chalky limestone, and the crystalline or igneous rocks meet, or are displaced near the north-west corner of the gulf, I will here quote Dr Robinson (i., p. 257:)—" Our route now lay up along the large Wadi-el-Musry, just north of the Ras of that name," winding considerably, but on a general course about north-west. " The ridge upon the left was of yellow *sandstone* resting on *granite*, while on the right was *granite* and *porphyry*. The scenery around was wild, desolate, and gloomy." In a little distance " *limestone* appeared on the left: and we turned short from the Musry towards the left, into a narrow chasm between walls of *chalk* with layers of *flint*." In ascending to the north, El Nakb, which means a " steep pass or declivity," the road is cut in the thin bed of sandstone down to the limestone rock. At the top of the ascent is Ras-el-Nakb, and soon after follows the large plain called Kaa-el-Nakb;

here it was covered with black flint stones. This *Kaa*, or plain, extends to the west, and its height above the Wadi-el-Araba is considered about 1500 feet. A ridge of dark-coloured granite hills is seen running off south-west. "On emerging from this long and tedious ascent," Mr Bartlett says,—"the high western Desert expands in endless prospect—a vast plain of fine gravel covered with small pebbles, and varied by a long perspective of camel's bones, bleached perfectly white, pointing out the track of the pilgrims across its boundless level, and the mirage spreads out a shifting succession of blue lakes, with the tops of distant hills appearing like islands among its phantom waters."

Having descended this pass, and returned into the Wadi at the head of the gulf, some *mounds* are discoverable near the north-east angle, which are supposed to cover the ruins of the ancient *Aila*: these however ought to be excavated.

(*To be continued in our next Number.*)

On the Leading Characteristics of the Papuan, Australian, and Malayu-Polynesian Nations. By G. Windsor Earl, Esq., M.R.A.S.

The existence of a Negro race in the Indian Archipelago, so remote from the continent which is considered as the original seat of the race, has given rise to endless speculations as to how they got there, and probably will continue so to do until the end of time; for, being a nation without a written language, and surrounded by others whose records are carried back to no very distant date, and whose traditions have become, from lapse of time, mere fables, this point can only rest upon circumstantial evidence, and therefore will ever prove liable to dispute. Their position, in many of the larger islands, as occupants solely of the mountain fastnesses, surrounded by people who evidently belong to a distinct race, has certainly put an end to those theories of the last century which attributed their origin to the shipwrecked crews of Arabian slave-vessels, and has led to a very general opinion that they were, in fact, the aboriginal inhabitants of the countries in which they are found. That their existence was not altogether unknown to the ancients, is proved by the maps and writings of Ptolemy the Alexandrian, who flourished soon after the commencement of the Christian era, and was the first to reduce geography to a system. In the last map of his volume, that which contains the "Aurea Chersonesus," and the "Jabados Insulæ," (supposed to have meant respectively the Malayu Peninsula, or Sumatra and the Java Islands), he places a country far to the eastward of the Aurea Chersonesus, under the equinoctial line, which he states to be occupied by "Æthiopes Ichthyophagi," or "Negro fish-eaters;" the first term being that employed by the Romans to distinguish the black and woolly-haired Africans from the Mauritani and other brown races of the coast; and the second, that usually applied to all nations who derived a portion of their subsistence from the sea.* The position of this country, with regard to the Aurea Chersonesus, agrees well with that of New Guinea, the great seat of the Papuan race. The existence of a Negro people, at so remote a spot, which he must have learned from the information of Indian navigators, seems, indeed, to have led Ptolemy into the great error of his system; for, believing that the country of the "Æthiopes Ichthyophagi" formed part of the Continent of Asia, he has made that continent, in his general map of the world, come round by the south and join the African Continent about Point Prassum, in lat. 15° south (the then southern known limit of the east coast of Africa), thus making the Indian Ocean, and the seas of the Eastern Archipelago, form one vast Inland Sea.

The most striking peculiarity of the Oriental Negroes consists in their frizzled or woolly hair. This, however, does not spread over the surface of the head, as is usual with the Negroes of Western Africa, but grows in small tufts, the hairs which form each tuft keeping separate from the rest, and twisting round each other, until, if allowed to grow, they form a spiral ringlet. Many of the tribes, especially those who occupy the interior parts of islands whose coasts are occupied by more civilized races, from whom cutting instruments can be obtained, keep the hair closely cropped. The tufts then assume the form of little knobs, about the size of a large pea, giving the head a very singular appearance, which has, not inaptly, been compared with that of an old worn-out shoe-brush. Others again, more especially the natives of the south coast of New Guinea, and the Islands of Torres Strait, troubled with such an obstinate description of hair, yet admiring the ringlets as a head-dress, cut them off and twist them into skull-caps made of matting, thus forming very compact wigs. But it is among the natives of the north coast of New Guinea, and some of the adjacent islands of the Pacific,

* The system of naming nations from the food which formed their chief means of support, seems to have been very prevalent among the ancients; witness "Hippophagi," the horse-eating (Tartars), "Lotophagi," lotus-eaters, &c. This system, although not to be recommended at the present day, has proved highly useful, for these names are sometimes found to contain the only existing description of the habits of the people on whom they were conferred, as in the present instance. Dr Leichardt, in his late overland journey from Sydney to Port-Essington, found some tribes of genuine Lotophagi on the lagoons and table-land, as will come to be noticed below.

that the hair receives the greatest attention. These open out the ringlets by means of a bamboo comb, shaped like an eel spear, with numerous prongs spreading out laterally, which operation produces an enormous bushy head of hair, which has procured them the name of "Mop-headed Papuans." Among the natives of the Feejee Islands (the eastermost limit of the Oriental Negro race), the operation of dressing the hair occupies the greater part of a day.

The hair of the beard and whiskers, which generally grows very thick and bushy, is arranged in little tufts similar to those of the head; and the same peculiarity is found to exist in the hair with which the breasts and shoulders of the men are often covered; but the tufts are here farther apart than on the head and chin.

This woolly or twisted hair is peculiar to the full-blooded Papuans. A comparatively slight mixture with the brown-complexioned or Malayu-Polynesian race appears to destroy the peculiarity. The hair of people of the mixed race covers the surface of the head, or, at least, has done so in all cases that have come under my observation, and is sometimes only slightly curled. It is, therefore, very easy to distinguish the *pure* Papuans; and throughout this essay, those only will be called by that name who possess this their leading characteristic.

The term Papuan is derived from a Malayan word, "Papua, or Pua-Pua," *crisp-haired*. The term "Tanna Papua," or "Land of the Crisp-haired," is applied by them not only to New Guinea, but to all the adjacent islands, which are occupied exclusively by this race. It is so peculiarly applicable and comprehensive, and so entitled to respect, as having been conferred by a people who must have known them for ages before we ever heard of their existence, that, I trust, the ethnologists of Europe will excuse me for retaining it in preference to the newly-invented term "Melanesian," or "inhabitants of the black islands," which, although applicable enough to the Papuans, is equally applicable to the greater portion of the Australian tribes.

The features of the Papuans have a decidedly Negro character; broad, flat noses, thick lips, receding foreheads and chins, and that turbid colour of what should be the white of the eye which gives to the countenance a peculiar sinister expression. Their complexion is universally a deep chocolate colour, sometimes closely approaching to black, but certainly a few shades lighter than the deep black that is often met with among the Negro tribes of Africa.

With regard to stature, a great difference is found to exist between various tribes, even in New Guinea, and which has led to much confusion in the description given by travellers, who have, perhaps, each only seen a single tribe. On the south-west coast of New Guinea, within the space of one hundred miles, are to be found tribes whose stature is almost gigantic, and others whose proportions are so diminutive as almost to entitle them to the appellation of pigmies; while the manners and customs of each so exactly correspond as to preclude the supposition that these peculiarities can be other than accidental.* It is difficult to account for these peculiarities; but as the stout and stalwart Papuans are met with only among those coast tribes who have maintained their independence, and at the same time have acquired many of the agricultural and mechanical arts from their neighbours, the Malayu-Polynesians, while the pigmies are found only in spots where they have been driven to the mountain fastnesses, or have fallen under the influence of other races, we may conclude that their mode of life has much to do with this difference in point of stature and proportions.

With regard to form, the various tribes of Papuans differ as much as in stature. The more diminutive tribes, whose members chiefly come under the notice of Europeans, from their existing in great numbers as slaves throughout the Moluccas, are unprepossessing enough in appearance, when in their natural state, but when under good masters, the regularity and wholesome nature of their diet, coupled with their apparent utter forgetfulness of their native land, produce a roundness in their neat clean limbs, and a sprightliness of action, which is rarely met with among their more civilized neighbours, the Malayu-Polynesians. On the other hand, the larger Papuans are more remarkable for their strength than their symmetry. They have broad shoulders and deep chests, but a deficiency is generally found about the lower extremities, the splay feet and curved shins of the Western Africans being equally or even more common among whom I may be allowed to term the gigantic Papuans.

With regard to the general disposition of the Papuans, a great difference is found between those living in a state of independence, and those who exist in bondage among the neighbouring nations. The former are invariably found to be treacherous and revengeful; and even those who have long been accustomed to intercourse with strangers, the tribes of the north-west coast of New Guinea, for example, are never to be depended upon, and the greatest precautions are always taken by those who visit them for purposes of trade. The wilder tribes generally avoid intercourse with strangers, if the force which lands is sufficiently great to cause alarm; but if otherwise, they pretend friendship until an opportunity occurs, when they make a sudden and ferocious attack. But what distinguishes them most from their neighbours, the Malayu-Polynesians, and even from the Australians, is, the unextinguishable hatred they bear towards those who attempt to settle in their territory, and which is continued as long as a man of the tribe exists. It is, probably, this perfectly untameable nature that has led to their utter extermination in all those islands of the Indian Archipelago that did not possess mountain fastnesses to which they could retire, to lead a life similar to

* The celebrated philologist, Marsden, has adopted the term "Negrito," or "Little Negro," from the Spaniards of the Philippines, and has applied it to the entire race.

that of the Boschmen of South Africa. We have had recent instances of this in Van Diemen's Land, Melville Island (north-west coast of Australia), and at Port Du Bus, on the west coast of New Guinea, in all which settlements the country was occupied by a pure, or nearly pure, Papuan race. In the former, hostility was continued as long as a native remained on the island, and in the last two, until the settlements were abandoned in despair. On the other hand, their neighbours, the Australians, have invariably submitted after a single trial of strength; while the Malayu-Polynesians, when not under the influence of other foreigners, have always evinced a desire to have strangers, especially Europeans, settled among them, as shewn by the people of the Moluccas when first visited by the Portuguese, and as displayed at the present time in those remote parts of the Indian Archipelago where the race maintains its ancient purity.

The untameable ferocity of the Papuans only exists as long as they remain in their native country. On leaving it, their character seems totally changed, as far as regards this particular. The Papuan slaves, who exist in great numbers in the eastern parts of the archipelago, are remarkable for their cheerful disposition and industrious habits, and nothing could exceed the orderly conduct of the remnant of the Van Diemen's Land natives, after they had been hunted down and removed to an island in Bass' Strait.

Before proceeding to describe the localities in which the Papuan race is now found, I think it proper to allude to certain of their customs, which distinguish them from the Malayu-Polynesians, and which certainly are of Papuan, or, at least, of Negro origin. One of these is the custom of raising the skin in cicatrices over various parts of the body, especially on the shoulders, breast, buttocks, and thighs. This must not be confounded with the tatooing or puncturing the skin, which is practised by many of the Malayu-Polynesian tribes, and which is never met with among the Papuans, as the scarifications which I am about to describe are unknown to the others. The skin is cut *through* with some sharp instrument, in longitudinal stripes, and, if on the shoulder or breast, white clay, or some other substance, is rubbed into the wound, which causes the flesh below to rise, and these scarifications, when allowed to heal, assume the form of raised cicatrices, often as large as the finger. The process by which these cicatrices are produced, and which I have had opportunities of watching in their progress from day to day until duly formed, is perfectly inexplicable to a European, who would be thrown into a fever by any one of the wounds which these strange people bear, two or three at a time, without complaining, but certainly not without suffering. It is, however, quite evident that the Papuans, and also the Australians, as will be mentioned below, possess a callousness of skin, or insensibility of pain, which is quite unknown among more civilized races.

Boring the septum of the nose is universally practised among the

Papuans. In the first instance, they wear a roll of plantain leaf in the orifice, which, by its elasticity, enlarges it to a sufficient size to admit the thigh bone of a large bird, or some other ornament, which is then worn extending across the face on all great occasions. Our sailors have a very quaint name for this practice, which often comes under their observation among the Papuan islands of the Pacific; they call it "sprit-sail yarding," after a cruel method they have of treating sharks and dog-fish, which are frequently let go after having been hooked, a piece of wood being previously thrust through their nostrils, which, projecting on either side, prevents them from getting their heads under water, and they die a lingering and painful death. I have never met with or heard of this practice of boring the nose among people of the Malayu-Polynesian race; and I may say the same with regard to the scarifications mentioned above. The latter, or rather those among them who are sufficiently barbarous to resort to personal disfigurement, seem to have adopted tatooing and boring the ears in lieu of the more coarse and painful ornamental work of the Papuans.

Filing or grinding down the front teeth, until they become pointed, is practised by some of the tribes of New Guinea and of the adjacent islands of the Pacific. This custom, however, is not confined exclusively to the Papuans, as it is practised also at the Pagi Islands, on the west coast of Summatra, the natives of which appear to be Malayu-Polynesians. This custom must not be confounded with one which is common among many of the Malayan and Bugis tribes, that of grinding down the front teeth, until they become almost level with the gum.

Another singular custom, which is only met with among the Papuans, or the tribes closely bordering on them, is that of dyeing the hair (which is naturally black) a reddish or flaxen colour, by using applications of burnt coral and sea-water, in some instances, and preparations of wood-ashes, in others. This process seems to expel all the dark colour from the hair, leaving it a flaxen tinge, which appears to bear a close resemblance to the celebrated "capillus flavus," so much admired among the Roman ladies, and which seems to have been produced by a similar process. The only Malayu-Polynesians that I have known to practise this custom are some of the natives of Timor, Laut, Sermattan, and Baba (islands lying to the westward of New Guinea, and not very remote). I am, therefore, induced to consider it as a Papuan, or rather, perhaps, as a "Negro" custom, for it is equally prevalent in many parts of Africa, especially among the Soumaulis and other tribes in their neighbourhood. Travellers who have had opportunities of visiting our port at Aden, in the course of their voyages between Europe and India by the overland route, may have observed this custom among the African coolies employed in coaling the steamer, who sometimes appear with the plaster of coral still attached to their heads.

The Papuans, for the most part, exist only in a savage state, deriving a scanty subsistence from the productions of nature, living in conical-shaped huts ; or, where they appear as occupants of the sea-coasts, roaming about in small canoes in search of food. Some of the more *independent* tribes, by which I mean those who have exclusive possession of the country they inhabit, have, however, adopted many improvements. In several parts of the north and of the south coasts of New Guinea, the villages consist of one large house, erected on piles, and occupied by all the married people, with a smaller one adjacent for the bachelors. These houses bear a very close resemblance to those of the Dyaks of Borneo, but are smaller, and of more rough construction. Here the Papuans also cultivate fruits, yams, and sweet potatoes, and keep hogs and poultry to kill for food; in fact, are almost on a level, as far as regards agriculture, with the more uncivilised tribes of the Malayu-Polynesians, from whom, indeed, if we may judge from the names employed to designate their agricultural productions, they have derived this slight but important advance they have made in civilization.

The weapons of the Papuans are heavy wooden clubs, spears, or lances of *nibong* or other hard wood, and darts formed of a small kind of bamboo, provided with points of hard wood, or of sharpened bone. The lances are projected generally by means of a becket of sennit, about a foot and a half long, one end of which is provided with a toggle. This is held between the fingers, while the other end is fastened to the lance with what sailors call a "half-hitch" knot, which flies off when the lance is projected, thus allowing it to go free. The becket gives a greatly increased purchase to the thrower, but is much inferior, in this respect, to the *womera*, or "throwing-stick" of the Australians, which will be described when we come to speak of that people. The darts are projected by means of a powerful bow, often six feet in length, with a bowstring of rattan. I suspect that this instrument was not originally Papuan, but has been adopted from the Polynesians. Stone axes, and knives of quartz are now superseded among all those tribes, who have either direct or indirect communication with the traders of the Archipelago, by Parang, or chopping-knives of iron. Their agricultural instruments are mere stakes of wood, sharpened at one end, which prove sufficient to effect the rude interference with nature required by their mode of cultivation.

The art of navigation appears never to have been in a very advanced state among the Papuans, since their navigation has only extended to those countries which could be reached from the continent of Asia, without entailing the necessity of going out of sight of land; nor are they yet sufficiently advanced in the science of navigation to venture on any other than coasting voyages. Towards the eastern limits of the Papuan race, where they come in close contact, and are often mixed with the Polynesians, navigation is in a more advanced state than elsewhere; but this is, evidently, the result of contact with strangers, by whom, indeed, the navigation is personally conducted.

The highest state of the art among the Papuans, without foreign assistance, is met with in Torres Strait, and upon the south coast of New Guinea. Here they possess large canoes of such construction, and propelled in so peculiar a manner, that we must consider them purely Papuan. Some very excellent sketches of these canoes are given in Flinders' Voyage, with so full a description, that it will be unnecessary for me to enter into minute particulars. These canoes or boats are from thirty to forty feet long, and the planks with which they are constructed are sewed together with the fibres of the cocoa-nut. Each is provided with an outrigger, and a platform of bamboo occupies the centre of the boat on a level with the gunwale. They are propelled in calm weather by paddles with long handles, the rowers all standing, as is generally the case among the Papuans. But the most striking peculiarity of their vessels consists in the sail, which is an oblong piece of matting, set up in the fore part of the vessel, by means of two poles or masts, to which the upper corners of the sail are fastened. These masts are moveable, and the sail is trimmed by shifting the head of one of the masts aft. According to my experience, these boats sail very indifferently, except before the wind; but Captain Flinders, who had good opportunities of judging, maintains a more favourable opinion. They are often to be met with about the month of March, three or four hundred miles down the north-east coast of Australia, the islanders being in the habit of making an annual voyage in this direction. The stopping places are usually the islands lying off the coasts, where they obtain tortoise-shell and trepang, the chief objects of their voyages.

The natives of the south coast of New Guinea have very large canoes of a similar, but more unwieldy construction, and propelled by a similar description of sail. These have never been seen far from the coast, and, in fact, are almost unmanageable, from the difficulty experienced in steering such unwieldy masses with paddles alone. It is, therefore, difficult to conceive for what purpose they have been constructed, unless it should be for war, in which case their large size would give them an imposing appearance.

The New Guinea canoes generally are of light construction, and are provided with an outrigger. The larger ones have an *attap* roof, and are capable of containing an entire family, with household furniture and domestic animals.—(Vide *The Journal of the Indian Archipelago and Eastern Asia*, Vol. III., No. xi., p. 1, for this excellent Ethnological memoir.)

On the Works undertaken by the Governments of different States, for the Geological Examination of the Country: A Report on the Journey undertaken by Himself and Dr Hornes, at the instance of the Imperial Academy of Sciences, to Germany, England, France, and Switzerland. By FRANZ VON HAUER. Communicated by WARINGTON SMYTH, Esq., F.G.S., &c.

In pursuance of the instructions received from the Academicians, W. Haidinger and P. Partsch, our principal endeavour was to become acquainted with such operations as are now in progress, at the expense of Government, in the various States through which we travelled, for the investigation of the geological structure of the country.

To the following description I have added, by desire of the Bergrath Haidinger, from various writings, a general view of similar works in countries to which our journey was not extended, as Russia, Saxony, and North America; and, at the end, have brought forward, as a conclusion of the whole, those points which, in similar undertakings, have hitherto been chiefly kept in view.

*The Geological Survey of Great Britain and Ireland.**

1. *History.*—In none of the European States has the geological examination of the country been undertaken by the Government at such an outlay of money and force as in Great Britain. Starting from a small beginning, when Sir Henry De la Bêche was employed in the Survey entirely alone, the Institution here to be described extended itself farther from year to year, till it attained its present flourishing condition, in which, under the careful direction of the same person, a numerous body is employed on the field-work, on the examination of the materials obtained, and the graphic representation of the observations made,—a condition in which a hand-

* Professor Jameson, at the beginning of the present century, formally proposed and developed to the Highland Society of Scotland, a plan for Scotland, identical with that afterwards so efficiently and brilliantly carried into effect for Great Britain by the talent and energy of Sir H. De la Bêche, powerfully supported by the Government of the country. When Professor Jameson's proposals were laid before the Society, the geological spirit was not fully abroad, and pecuniary means were wanting.—*Prof. Jameson, Edit. Ed. Phil. Journal.*

some edifice has been built, in the best part of London, for the reception of the various collections, and in which, lastly, after the successful solution of numerous questions of industrial importance, practice is already beginning to reap the harvest which was sown by the application of science.

About the year 1833, Sir H. De la Bêche made a proposal to the Government to add the geological colouring to the Ordnance Map, which was then in progress, and which, from its beauty of execution, appeared remarkably well adapted for this purpose; the expenses incurred by him only were to be repaid. Already there had been published in England, by various *savans*, numerous detailed maps of particular districts, and even some general geological maps of the whole country, among the latter of which I may mention the map published by Smith in 1815, including, in fifteen sheets, all England and a part of Scotland, and the beautiful map by Greenough, of which the first edition appeared in 1819, the second, much improved, in 1839; yet the very great importance of specially executed detailed maps of the whole country, worked out on a uniform plan, was at once recognised, and the proposition of the celebrated geologist, whose scientific position guaranteed the corresponding execution of the work, was accepted.

Sir H. De la Bêche at once commenced operations: two assistants of the Trigonometrical Survey were supplied him at the expense of that department. He himself had only his travelling expenses paid, and was placed under the head of the Ordnance Survey, at that time Colonel Colby. Under these circumstances he completed alone the maps of Cornwall and Devonshire, and on so well-matured a plan, that the same is retained in all its important points even now, for the much enlarged working force of the Geological Survey. The "Report on the Geology of Devon, Cornwall, and West Somerset," which appeared in 1839, contains the geological descriptiou of the country investigated, with a geological map, numerous mine plans, geological sections, &c.

After the completion of this first part of the work, the geological examination of Glamorganshire was to commence; Sir H. De la Bêche felt how much aid might be afforded, in surveying this district, by a person acquainted with the localities, and provided with ample mining information; and as the Government, by degrees, better appreciated the importance of the whole undertaking, not only was Mr Williams, who possessed the above requisites in a high degree, appointed as assistant geologist to Sir H. De la Bêche, but the latter was definitively appointed director of the Geological Survey, and received a salary. At short intervals there followed the nomination of three more assistants, Messrs Rees, Aveline, and Logan. Mr Logan, at present director of the geological survey in Canada, had lived at Swansea, and of his own accord surveyed a great part of Glamorganshire, with such accuracy, that after an examination of his work, it was accepted unaltered by the Geological Survey. In this manner the map of Glamorganshire also was soon completed.

In 1841 Mr Ramsay was appointed assistant on the Geological Survey, and with his aid Pembrokeshire and a part of Carmarthenshire were finished between that time and 1845.

Independently of the investigations in England, a Geological Survey had, in the meanwhile, sprung up in Ireland. In consequence of a proposition of Colonel Colby, who had declared his opinion that the trigonometrical survey of the country should give the basis for extended statistical, antiquarian, and geological investigations, Captain Pringle was instructed to take the direction of the latter. The operations were commenced with great zeal simultaneously with the trigonometrical survey, but since the desired acceleration of the geographical maps required the whole force, the geological part was placed more in the background and neglected.

In 1832 Colonel Colby's original plan was again taken up by Captain Larcom. Particular and ably-written instructions were given to all the officers employed on the Trigonometrical Survey, to direct their attention specially to antiquarian and statistical inquiries, whilst Captain Portlock formed a separate geological department. Moreover, numerous investigations of the fauna and flora of the country were begun, and in 1835 some of the results were published in the Memoir of Londonderry.

But it was not till 1837 that the geological division received a complete organisation. At Colonel Colby's desire Captain Portlock then erected a geological and statistical office, a national museum for geological and zoological objects, and a laboratory for the analysis of minerals.

In 1840, the plan of continuing the Londonderry Memoir was again abandoned, the museum and laboratory transferred from Belfast to Dublin, and Captain Portlock instructed to collect, in a special work, all the geological data which he had collected in the county Derry and the barony of Dungannon. For this purpose various new investigations were set on foot in the neighbouring districts, and in 1843 the work was published under the title of Report on the Geology of the County of Londonderry, and of parts of Tyrone and Fermanagh. This work, in the completion of which Mr Oldham materially assisted, contained a general map on the scale of half-an-inch to the English mile, whilst the original surveys (which, however, were not published) were entered on the maps of the Trigonometrical Survey on the scale of six inches to the mile. Many geological sections, as well as engravings and descriptions of the fossils, accompany the work.

In the meanwhile, another undertaking had occasioned the foundation of the Museum of Economic, or, as it is now called, of Practical Geology, in London.* When the erection of the new houses of Parliament was determined, it became, in the first place, necessary to make a careful selection of the most suitable building-stone, in order to give a corresponding durability to an edifice which was to be carried out with a magnificence worthy of the greatness of the English nation. A special commission was placed under the direction of Sir H. De la Bêche, which, by the most careful examination, supplied the data needed for such a choice. On the one hand, they considered the quality of the stone as deducible from direct experiments,—such as the chemical composition,

* The Museum of Practical Geology orginated in Mr Henry De la Bêche having, during the progress of the Geological Survey, then in Cornwall, in 1835, represented to the Government the advantages that would arise if that survey were made available for collecting objects illustrative of the application of geology to the useful purposes of life, so that the requisite information might be obtained by those who might be required to direct, or might be anxious to promote works, either for the ornament or good of the country.

their relative strength, the expenses of their quarrying and facility of transport; on the other hand, they also directed great attention to their properties of withstanding decomposition, as evinced by the different kinds of stone in existing edifices, which date from the earliest periods; and thus, after long-continued exertions, they established a comparative collection, which, from having been published, not only solved the question for the special case, but gives every architect the means of choosing, with the greatest security, the most suitable material for every edifice to be erected in any part of Great Britain.

As at every opportunity Sir H. De la Bêche laboured to bring into general practical application the results furnished by science, so out of a work simply intended for the advantage of industry, he now saw how to derive a corresponding boon for science. According to his proposal, it was determined to preserve all the specimens of rock collected during this inquiry, and to arrange them in a special museum, the collections of which at once increased from year to year. Soon afterwards it was destined to receive specimens of all the minerals, rocks, and petrifactions found in England, to add by the side of the raw materials of the mineral kingdom examples of the industrial products obtained from the same by manufacture, and to exhibit, by complete series, the gradual changes which the original material undergoes, till it is metamorphosed into something of utility in ordinary life. Moreover, a bureau was attached to the museum, under the name of the Mining Record Office, and under the direction of Mr Robert Hunt, which collects historical and statistical information of importance to mining, maps of mines, plans, &c., publishing the most important, and keeping the rest prepared for the inspection of individuals.

But the most important epochs in the history of the undertakings here described, is the reorganisation which was effected in 1845. Sir H. De la Bêche had been till then always under the Ordnance Survey; but, in the above-mentioned year, he was placed under the department of Woods and Forests, and, at the same time, a new and comprehensive extension of his charge was granted.

Sir H. De la Beche received the title of Director-General of the Geological Survey of the United Kingdom, and the operations in England and Ireland were together placed under his direction.

Mr Ramsay was made Director of the Survey in England, and Captain James, now succeeded by Mr Oldham, of that of Ireland. An Act of Parliament was passed, the 31st July 1845, to facilitate the investigations in the field; and, in order to conduct the operations on a more extended scale, the staff of the institution was considerably increased. Dr Playfair was appointed as a chemist, Mr Forbes as palæontologist, and Mr Warington Smyth for the surveying and examination of the mining districts.

In the same manner the Museum received a great extension. It was resolved to erect a new building, which is already completed in Piccadilly, London; a library, and a collection of models of mining, machinery, and implements, as well as plastic representations of the most important mining districts were brought together. From this time the operations proceeded with a rapidity corresponding with the increased numerical force, without the intervening changes of ministry having caused material delay.

Besides the geological maps, which were always published as soon as a tolerably large portion of country was completed, in 1846 the publication of the Memoirs of the Geological Survey was commenced,—a work intended to make known the scientific labours of all persons employed in the institution. Up to the present moment three parts have appeared, to the contents of which I shall afterwards have occasion to refer. The Geological Survey was enabled to undertake the solution of numerous questions of immediate bearing on practical life, which were often connected with laborious investigations. As one of the most complete special investigations, I may instance the inquiry into the various qualities of coal, with respect to their fitness for steam navigation, instituted at the desire of the Lords of the Admiralty. In this inquiry, besides the heating power, the per-centage of ash, and other properties important in every application of this fuel, other points of particular importance had to be considered, viz., the degree of porosity, as influencing the weight of coal which can be stowed in a small compass, and the strength of the fragments, which ought to be great enough to prevent their crumbling under the influence of the shocks produced by the motion of the vessels, &c.

It was resolved, with a view to satisfy all conditions, to undertake not only chemical experiments, but really practical trials. The Lords of the Admiralty made over an annual sum of £600 to cover the expenses; and, under the direction of Playfair and Phillips, a boiler was erected for the purpose at the Polytechnic College, founded about three years ago at Putney, and the investigations were commenced, the results of which have been published in a First Report in the summer of 1848.

Execution of the Operations—Maps and Sections.

The persons entrusted with the Geological Surveys, the assistants, geologists, and directors, work separately, although, in general, at no great distance from each other. Each receives two copies of the map of the district he is to examine, one for entering his observations in the field, the other for copying down the results obtained. The survey is effected, as far as possible, by directly following the boundary lines in nature; but where this is impossible from the intervention of cultivated ground, it is considered allowable to draw the most probable line by connecting the two nearest points of observation. We had an opportunity of seeing the exactness with which they go to work on these observations, when accompanying Mr Ramsay, who, in this autumn, undertook the survey of the valley of Llanberris in North Wales. The flanks of Snowdon, facing this valley, consist partly of igneous rocks, partly of altered sandstone and slates, which being mingled with the products of submarine eruptions, lava, ashes, &c., and altered by subsequent metamorphoses, have often lost nearly every trace of stratification, and every sign of a neptunian origin. It is a labour not less difficult than (if considered by itself) thankless, to follow up the complicated lines of boundary between the two kinds of rock, over the bare and steep crags, which are frequently not to be attained without considerable danger. Yet Mr Ramsay submitted to it with untiring zeal, and left no spot till he had safely established the boundary line by frequent comparison and repeated observation.

The districts surveyed by the assistants and geologists, are, finally, revised every spring by the director, when faults on the part of beginners are corrected, and, if necessary, resurveyed; moreover, Sir H. De la Bêche himself from time to time crosses the country, in arbitrarily-chosen directions, to satisfy himself personally of the correctness of the work. In this manner, a degree of exactness is introduced into the geological maps, which appears to be the utmost that can be attained on the scale selected for the purpose.

Simultaneously with the determination of the boundary-lines of the rocks, observations on the strike and dip of the beds, and other remarkable phenomena, are made and entered in note-books. The fossil collectors are despatched to the localities where fossils are found to be most numerous, and the organic remains, one may say of each individual bed, are specially collected and sent to London.

After the completion of the geological map of a district, they proceed to the construction of sections, in all these directions which give promise of interesting results, and the work is conducted throughout with geometrical accuracy. The sea-level serves as the datum line in all; the form of the surface is measured with the theodolite and chain, the dip of the beds is taken with the clinometer, and in this manner they obtain a perfectly natural representation.

Besides the geological sections, whenever it appears feasible and interesting, drawings are taken of the succession of beds in shafts, although, in most cases, these have not been measured by the members of the Survey, but were communicated by the owners of mines. All these measurements, which, of course, are often taken in a line inclined to the plane of stratification, are reduced to a line at right angles to it, in order to represent at once the true thickness of the several rock-strata.

Publication of the Maps and Sections.

The maps of the Ordnance Survey, which are employed for the field-work, serve also as the foundation for the publication of the geological maps. They are prepared on the scale of one inch to the English mile of 800 Vienna fathoms, or 2½ times larger than our maps of the Staff of the Quartermaster General. The originals are engraved on copper, whilst, for the geological maps, an electrotype copy is taken, and upon those reproduced plates the boundary-lines are afterwards engraved. The colouring is done by hand.

The sections also are engraved on copper from the drawings prepared by the Survey. In all of them the same scale is employed both for heights and distances, so that the picture, instead of appearing distorted, is true to nature. The sections also are coloured by hand.

Works at the Museum in London.

With the labours for the preparation and publication of the maps and sections are connected those which are carried on at the Museum in London. Their full development, however, will not come into play before the completion of the above-mentioned new building in Piccadilly. Through a handsome portico one enters the principal apartment, occupying by far the greater part of the whole edifice, which is 170 feet long, 80 feet wide, and 80 feet high. It receives its light through a glass roof from above; two galleries, one above the other, are attached to the wall, in order to increase the space disposable for the exhibitions of the collections. Beneath the great room, and lighted up by the upper light which passes through a large circular opening in the middle of the room, is a lecture theatre, in which it is proposed to give, during the winter, lectures on geology, palæontology, and chemistry, and, it is hoped, afterwards on mining and metallurgy. In the same manner as in France, where the Ecole des Mines is situated in Paris, they will here proceed on the persuasion, that the theoretical part of education which the miners should have, can best be obtained in the capital, where the greatest amount of scientific force for lectures is formed, where the professors and students are aided in their studies by good libraries, collections, and various other institutions, and where also the more active vitality of science exercises a powerful influence on every individual, and spurs him on to the most energetic exertion; whilst, on the other hand, the practical part of the necessary knowledge cannot be imparted in general lectures, but must be acquired by residence and travelling in various mining and metallurgic districts of acknowledged high reputation.

Moreover, there are arranged in the edifice, a chemical laboratory of four chambers, from which, as it is placed at the top of the house, a lifting apparatus connects it with the lower story; also working-rooms for palæontology, for the Mining Record Office, &c. The communication between the various parts of the building may be effected by an electric telegraph.

In the principal room, in the middle of the edifice, the great collection of English minerals, rocks, petrifactions, and industrial products derived from them will be laid out. It is at present placed in a building at Craig's Court, Charing Cross, and a considerable part as yet unarranged. This collection, as far as regards the original natural products, is chiefly augmented by the diligence of the persons employed on the Survey, and of the collectors appointed for this purpose. It is only rarely that a few objects of particular interest are bought. By collecting the fossils for themselves, the Survey enjoy the particular advantage of obtaining a perfectly accurate and satisfactory statement, not only of the locality but of the very stratum to which the specimens belong.

As regards the products of industry, and the suites by which the transition from the raw material to the former is represented, this Institution will be very favourably received by the patriotism of the English, who most liberally support every work commenced for the advantage of their fatherland. Valuable minerals, manufactured articles, and the above mentioned suites, have been in great numbers presented by individual owners of mines and manufactories. Yet, on the other hand, it sometimes becomes necessary to enrich the collection by direct purchase.

By exhibiting specimens of the industrial products of ancient times, it is sought to illustrate the improvements and the progressive development which have taken place since the earlier periods of art.

Without entering farther into particulars, I will only add, that the collection of building-stones which gave rise to the establishment of the whole Museum, acquires on that account a special interest. The specimens are chiselled cubes of six inches in the side, some of which are polished for the better exhibition of the character of the stone. On the label is noted, not only the quarry from which they are taken, but there is also mention of the most important erections in which they have been employed. In the palæontological department, which is also at present in another *locale*, the examination and determination of the fossils is effected by the palæontologists ; and, whatever they discover to be new, is drawn and engraved, and published in the Memoirs of the Geological Survey.

The laboratory serves not only to carry out investigations and analyses which are necessary for the Survey, but, for certain fees, objects brought by private persons with this view are examined.

The collections of mining plans and statistical documents in the Mining Record Office, are principally dependent on the liberality of private individuals, since there is no obligation on the owners of mines to communicate them ; and, in fact, it does not appear that in either of the districts examined any difficulty has been experienced in obtaining the needful data ; the alterations produced by the progress of the operations will be entered from time to time. Particular attention is directed to the acquisition of the maps and plans of mines which have been abandoned, in order to supply the desired data at some later period, when, as so frequently occurs, an old work of this kind is to be opened afresh.

Results obtained.

As the most important of the results obtained, we may undoubtedly regard the geological maps and sections, published by the Geological Survey. A glance at the maps suffices to establish their excellence. Accuracy of mapping and beauty of execution combine to raise them to a grade of perfection which has not been attained in an equal degree by any work of a similar character.

Up to this time, 26 of the larger sheets, of $22\frac{2}{3}$ inches in height, and some 28, some $33\frac{1}{8}$ inches width, have been completed, as well as eight quarter sheets. At first the larger size was selected for publication, but afterwards the convenience of the smaller form was recognised. They include all the south-west of England, with Cornwall, Devonshire, Somersetshire, a part of Gloucester and Wiltshire, also the counties of Monmouth, Glamorgan, Brecknock, Carmarthen, Pembroke, and Radnor, with parts of Montgomery, Shropshire, and Hereford. They are all engraved on the scale of one inch to the English mile, or 1 to 64,000. All the sheets can be joined together, a great convenience in use, which unfortunately has of late been overlooked in the publication of the Maps of our Quarter-Master-General's Staff, which have been divided accorded to the separate provinces.

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Of the geological sections, 17 sheets have hitherto appeared. Their scale is 6 inches to a mile, or 1 to 10,666; the colours are the same as in the maps. The sea-level is taken as the base, and if the observations go below it, they are shewn by engraved lines, but not coloured. Numerous notes appended to the individual beds give information upon their petrographic properties, organic remains, &c. At the same time, the probable ancient configuration of the surface, as it would result from the direction of the beds, is given in outline; and special lines represent the dip of the cleavage which so often cuts across the variously-contorted beds in perfectly-parallel succession.

Fifteen sheets of the vertical sections of the strata have appeared. They are drawn on a scale of 1 inch to 40 feet, or 1 to 480, and are not coloured; the notes appended to them contain such an amount of detail as has scarcely been yet obtained in any other geological work.

All the data and results of experience which offered during the geological examination of the country, as well as the special labours of the members of the Survey, are contained in the *Memoirs of the Geological Survey of Great Britain and of the Museum of Practical Geology* in London. Two volumes of this collection, the second in two parts, have been published in large octavo. The numerous woodcuts interspersed in the text, the splendid engravings of fossils, and the many illustrations of every kind, increase not a little the utility of these excellent works.

* * * *

We must add among the results, the extensive collections in the Museum, which, in connection with the lectures it is proposed to give during the winter, will greatly contribute to the spread of useful knowledge through the country. The investigation of English building-stones, and the results thus obtained were above alluded to. Lastly, it must not be overlooked, that one of the greatest advantages of the Institution consists in the explanations which every person interested in mines or manufactures most readily receives there, upon all scientific questions having any bearing on his branch of business.

I cannot close this division of my report without thankfully acknowledging the liberality with which Sir Henry De la Bêche, as well as all the eminent *savans* employed in the work under his direction, allowed us to enter upon all the details of the same. We have to thank their friendly instruction for the more accurate acquaintance with an Institution, which, in magnificence of arrangement, in talented execution of the proposed plan, and in abundance of results, which are as important for science as for practical life, far surpasses all similar undertakings that have hitherto been carried out, and will long serve as a model for geological investigations in other portions of the globe.

On the Tides. By William Galbraith, M.A., F.R.A.S., Teacher of Mathematics. Communicated by the Author.

In my article in the last Number of this Journal, I endeavoured to point out the proper method of analysing a register of the tides, and deducing the necessary results when the sun and moon were at their mean distances from the earth, and in the plane of the equator.

These conditions cannot always be obtained, and then it becomes necessary to effect that by computation which cannot be had directly from observation.

To compute the actual rise of the spring-tide above the mean level of the sea—

Let f be the effect of the sun, and f' that of the moon, on the waters of the ocean, then will the whole effect be

$$F = f + f' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Now, if r be the semidiameter of the sun, and d the declination; ϱ the semidiameter of the moon, and δ the declination, at the time of syzygy, then $f = a \cos^2 d$, and $f' = b \cos^2 \delta$, and, therefore,

$$F = a \cos^2 d + b \cos^2 \delta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

in which a is the aggregate effect of the action of the sun's mass, combined with his real distance, indicated by his semidiameter, and b that of the moon in similar circumstances, while $\cos^2 d$ and $\cos^2 \delta$ give the effects of the distance from the plane of the equator measured by their declinations.

To facilitate this operation, I have computed the logarithmic values of a and b, contained in the following table.

Example 1. On the 3d of August 1849, I found the unit of height u (see page 16) of the tide at Broddick, in Arran, to be 4·34 feet, when the moon was full, at $15^h\ 52^m$ P.M., Greenwich mean time, what would be its value if the sun and moon were at their mean distances and on the equator?

By the Nautical Almanac

	°	′	″
To this time the sun's semidiameter, $r =$	0	15	47·5
declination, $d =$	17	19	8·6 N.
the moon's semidiameter, $\rho =$	0	15	1·4
declination, $\delta =$	15	28	20·7 S.

Hence, from the table for r and ϱ, we have,

$\text{Log } a = \ . \ . \ . \ 9{\cdot}37129$ $\text{Log } b = \ . \ . \ . \ 9{\cdot}82140$

$d = 17^\circ\ 19'\ 8''{\cdot}6 \ \cos^2 = 9{\cdot}95971$ $\delta = 15^\circ\ 28'\ 20''{\cdot}7 \ \cos_2 = 9{\cdot}96794$

$f = 0{\cdot}2143 \quad \log\ 9{\cdot}33100, f' = 0{\cdot}6157 \quad \log\ 9{\cdot}78934$

$f = 0{\cdot}6157$

$F = 0{\cdot}8300$

$\quad\ 1{\cdot}0000$ = mean height of spring-tides.

$1 - F = 0{\cdot}1700$ = defect.

Hence, the preceding value of u is too small by $0{\cdot}17 \times 4{\cdot}34 = 0{\cdot}7378$ foot; therefore, the true value of u is $4{\cdot}34 + 0{\cdot}74 = 5{\cdot}08$ feet, when the sun and moon are on the equator, and at their mean distances from the earth.

Example 2. On the 29th of December 1849, the moon was full at 2^h P.M., required the unit of height on that day, and the excess of the rise above 5·08 feet in the Frith of Clyde?

By the Nautical Almanac, to this time we shall have,—

		°	′	″
The sun's semidiameter,	$r=$	0	16	17·3
declination,	$d=$	23	13	30·0 S.
The moon's semidiameter,	$\rho=$	0	16	44·6
declination,	$\delta=$	19	28	53·0 S.

To these values of r and ρ, we have from the table,—

Log a, =	. . .	9·41164	Log b, =	. . .	9·96263
$d = 23°\ 13'\ 30''$	$\cos^2 =$	9·92660	$\delta = 19°\ 28'\ 53''$	$\cos^2 =$	9·94880
$f = 0·2179$	log	9·33824	$f' = 0·8155$	log	9·91143
$f = 0·8155$					
$F = 1·0334$					
1·0000					

$F - 1 = 0·0334$, the excess of this tide above unity.

Hence, $0·0334 \times 5·08 = 0·169672$ foot. Wherefore, $5·08 + 0·17 = 5·25$, consequently this tide would rise to 5·25 feet only above the mean level of the sea, or 0·17 foot (about 2 inches) more than the usual spring-tide, when the sun and moon are on the equator, and at their mean distance from the earth. Whence the whole rise from low water to high water would be $5·25 \times 2 = 10·5$ feet on that day, independent of the state of the barometer and force of the wind. In similar circumstances, the *greatest effect* would be (*Ainslie's Surveying*, 398), $5·08 + 5·08 \times 0·178 = 5·08 + 0·90 = 5·98$ feet, and twice this, that is, $5·98 \times 2 = 11·96$ feet, or nearly 12 feet, the total rise from low to high water, when the sun and moon exerted the greatest possible influence (a very rare occurrence), independent of the effects of the wind and state of the barometer—phenomena that cannot be predicted. The foolish exhibition made at many of our sea-ports on that day, will be long remembered.*

Indeed, in tropical climates, the effects of hurricanes and tornadoes are so great, that they occasionally raise the sea to such a height as to carry large ships over sand-banks, and up a low flat country, as was the case a few years ago with regard to the York Indiaman, of about 1200 tons burden, which, I was informed by one of her officers, was carried over the Sauger Sands and driven over land, so far as to be left high and dry at some distance from the usual sea-shore,

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* If great precision be required, especially when the *unit* of height is considerable, the correction must either be repeated or the true quantity found by proportion, thus:—

1. As 0·83 : 1 : : 4·34 : 5·23 feet $= u$.
2. As 1 1·0334 : : 5·23 : 5·40 feet = rise.

which are the true quantities, whereas the preceding are only near approximations if not repeated.

near the mouth of the Ganges, after the hurricane had ceased to blow from the southward into the Bay of Bengal. It also caused, on the low grounds in the vicinity, the destruction of much life and property. These catastrophes are greatly to be dreaded in tropical climates, because they cannot be foreseen, so as to be avoided. It is fortunate for us, however, that they can scarcely ever occur in our temperate climates, though it would be well to be apprized of the utmost limits to which they can attain.

Logarithms to compute the height of Spring Tides, the mean being represented by unity.

Semidiameter.	Log. for Sun, or Log. *a*.	Difference to 1″	Log. for Moon, or Log. *b*.	Difference to 1″
′ ″				
14 35	...	...	9·78267	148·6
14 40	...	...	9·79010	147·6
14 45	...	...	9·79748	146·8
14 50	...	...	9·80482	146·0
14 55	...	...	9·81212	145·2
15 0	...	...	9·81938	144·4
15 5	...	...	9·82660	143·4
15 10	...	...	9·83377	142·8
15 15	...	...	9·84091	142·0
15 20	...	...	9·84801	141·2
15 25	...	...	9·85507	140·6
15 30	...	...	9·86210	139·6
15 35	...	...	9·86908	139·0
15 40	...	...	9·87603	138·2
15 45	9·36785	137·6	9·88294	137·6
15 50	9·37473	136·8	9·88982	136·8
15 55	9·38157	136·0	9·89666	136·0
16 0	9·38837	135·4	9·90346	135·4
16 5	9·39514	134·6	9·91023	134·6
16 10	9·40187	134·0	9·91696	134·0
16 15	9·40857	133·4	9·92366	133·4
16 20	...	...	9·93033	132·6
16 25	...	...	9·93696	132·0
16 30	...	...	9·94356	131·2
16 35	...	...	9·95012	130·6
16 40	...	...	9·95665	130·0
16 45	...	...	9·96315	...

On the Limits of Perpetual Snow in the Himalayas. By Captain J. D. CUNNINGHAM, Engineers.

I have just read Lieutenant R. Strachey's interesting paper On the Limits of Perpetual Snow in the Himalayas, in which he satisfactorily establishes that the elevations hitherto assigned to the phenomenon have been under-estimated, and that, in truth, snow is only to be permanently found at about 15,000 feet on the southern, and at about 18,000 feet on the northern boundaries respectively, instead of at about 13,000 and 16,500 feet, as hitherto supposed. Lieutenant Strachey very well shews that Humboldt has attached undue weight to the casual or partial observations of travellers and others in fixing upon the smaller numbers; but he appears to me to be himself in error when he assigns the greater elevation on the northern side, almost solely to the smaller quantity of snow which there falls, although he is pleased to attach value to my testimony that such quantity is indeed relatively small, and thus to make me in a way a supporter of his theory.

Humboldt's view of causes correct.—Humboldt, in his "Cosmos" (*Sabine's Trans.* i., 328), enumerates the contingencies on which the limits of the snow-line are dependent; and to me he seems truly to refer the superior height on the northern side of the Himalayan chain to the general elevation of Tibet, *i. e.*, to the heat due to radiation and reverberation even at that great height above the sea. This view is strikingly borne out by what that able officer, the late Dr Lord, observed with reference to the Hindu Koosh.† He found the snow lying very much lower on the northern than on the southern face, and he gives, as a reason for the large difference, the existence of the high lands of Cabul on the south side, or the fact that these high lands contain latent

* Journ. As. Soc. of Bengal, No. 102, April 1849, and Edinburgh New Phil. Journal, vol. xlvii., p. 324.

† Reports on Sindh, Afghanistan, &c., by Sir A. Burnes, Lieutenant Leech, Dr Lord, and Lieutenant Wood (Geographical Memoirs, p. 48, &c.)

heat, which melts the snow; while, on the northern face, the slopes merge into the swampy flats of Toorkistan, scarce 500 feet above the sea, and are thus met by a cold atmosphere, down to a low level, in the aid of the coldness due to a northern aspect.

Relative heights on extreme edges of mountain-belts.—It will indeed be found, that in any *broad mountain-chain* resting on a plane inclined to the sea-level, and running nearly east and west, the effect of latitude on temperature may be discarded, and that elevation above the particular country, and not above the general ocean, is mainly, although not solely, to be considered in determining the limits of perpetual snow on the two edges of the belt. The line of snow will rise as the plane of the country rises, and keep above it at a continually decreasing distance, until the diminishing temperature due to increasing height, causes the two to coincide, a phenomenon which of course cannot occur in the temperate zones, as we know of no table-land so high as to be always frozen on the surface.

Relative heights on opposite sides of the same single hill of a chain.—This reasoning does not, however, apply to the limits of snow on the northern and southern slopes of any *one hill or mountain* of a broad and complex chain; and as a rule, the snow will be found to lie lower on the northern than on the southern face of a single peak. In such an instance, neither difference of latitude nor inclination of plane can ordinarily have any effect, and the only element to be taken into consideration is the direct play of the sun's rays, which, in the northern hemisphere have most power on a hill-side looking to the south. Captain Hutton, in his papers in Dr M'Lelland's Journal of Natural History, had such isolated hills in view when he asserted that the southern limit of snow was higher than the northern one, and when he sought the support of my experience on the subject, as I was then, 1842 moving about in Ludâkh and Kunâwur.

Description of illustrative sketch.—The accompanying sketch represents what I believe to be the true state of the case with regard to the Himalayas, whether a line be drawn north and south across them, between the Gogra and Ganges, or east and west in the neighbourhood of Cashmir. Towards the plains of India, the limit of snow on the southern sides of the extreme hills will be found at about 15,000 feet above the sea, as Lieutenant Strachey shews, and on the northern face of the same hill, at about 12,000 feet, a figure, however, which I have assumed for the sake of illustration, as I know of no observations directly bearing on the subject. On the Tibetan side of the chain, the heights will be found to be about 20,000 feet on the south, and 18,000 or 18,500 on the north face of the same hill. These latter estimates are Lieutenant Strachey's, and they are, I think, correct, while the southern height of 20,000 feet is an approximation only.

I have taken the height of the Manasarawar Lake, viz., 15,000 feet, in making this sketch, but even Humboldt's mean elevation of Tibet, viz., 11,500 feet (*Cosmos*, i., 330), will not affect the argument, that the distance between the planes of the mountain bases and of the snow-limits goes on decreasing as the former ascend.

Quantity of snow falling in Tibet, and the permanency or renewal of snow generally.—With regard to the quantity of snow which falls to the northward of the main peaks of the Himalayas, I may refer to my statement at p. 238 of the 148th No. of the Journal, where I say that it did not appear to exceed two feet and a half in depth, where not drifted. This refers to the tract around the junction of the Sutlej and Spiti rivers. In addition to the details there given, I may also mention, that the larger streams began (in 1842) to swell after the middle of February. This was due, I would say, to the radiation from the mountain masses causing the lower surface of the snow to melt, the recently accumulated snow itself forming a protection against the chilling winds, and so allowing the earth to part with its heat. At this period, the temperature of ordinary springs was about 42°, while the air at sunrise was sometimes below zero, and the mercury would not rise above 60°, when exposed to the sun's rays in the early part of the afternoon. I state these particulars partly in support of what I consider to be Captain Hutton's meaning with regard to snow not being perpetual; an opinion to which Lieutenant Strachey somewhat slightingly alludes.* Both observers are right, because the one simply means that snow is even being simultaneously destroyed and renewed, and the other, that hills of a certain elevation always exhibit a covering of snow.

The Tibet of the Himalayas not a plain or table-land.—Lieutenant Strachey, and indeed most people, talk of the "plains" or "table-land," of Tibet; but I doubt whether, between Imaus and Emodus, or anywhere in the valleys or basins of the Indus, and Brahmaputra to the north of the Himalayas, there are any plains. The range separating the upper courses of the Indus and Sutlej is indeed inferior in height to that which gives rise to the Ganges and Jumna, but it is still a lofty range. To the northward of the Indus, or on a line running from Garo towards Yarkund, I dare say that undulating ground or moderate slopes, rather than deep ravines with steep sides, may perhaps be found.

These downs or steppes, or at least tracts, afford pasturage to the best description of shawl-wool goats; and Lieutenant Strachey is right in his opinion that, elevated although they be, they are as free from snow during summer as the plains of India. What he supposes of the Kailás or Gagri of the Manasarawar Lake, viz., that the height of its (northern) snow-line may be 19,500 feet, would also be fully verified on any mountains which may break the sameness of these steppes, and not be so far to the north as to be much affected by the latitude.—(*Journal of the Asiatic Society of Bengal*, New Series, No. xxxi., July 1849, p. 694.)

Observations upon M. Boutigny's recent Experiment. By Professor PLÜCKER of Bonn.†

It may perhaps be a matter of interest to you to obtain a confirmation of Boutigny's recent experiment. With his usual kindness, he exhibited to me, last Easter, his former experiments; and, whilst admiring his rare perseverance in

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* Journ. As. Soc. of Bengal, April 1849, p. 302, note.

† From Poggendorff's Annalen, December 7, 1849.

following up a fertile idea, I then acquired an impression that it referred to a law of nature which was by no means completely revealed, and in which opinion I was further strengthened by the report of his last experiment. In consequence of an oral communication of this experiment, M. Fessel wrote to me from Cologne, stating that, on the following day, he had dipped his finger into lead heated to its highest point, by which means the projecting portion of the nail of the finger had been burnt, but, in other respects, the finger remained perfectly uninjured; he also stated further, that a workman in the employ of Messrs Behren and Co. manufacturing-engineers at Cologne, had made the experiment with melted iron, and would repeat it before me. I therefore accepted the offer, and, accompanied by several persons interested in the matter, proceeded to Cologne. The workman, in my presence, struck the unmoistened extremities of his fingers rapidly, and not without fear, against the surface of the iron which had just flowed from the melting furnace into a trough, and which was afterwards used in casting a large plate for a furnace. I was thus convinced of the perfect truth of Boutigny's experiment: and whilst carefully examining the extremities of the workman's fingers, one of the two assistants of the Physical Cabinet accompanying me, struck the entire surface of the open hand, which he had previously dipped in water, so strongly against the bright red surface of the iron that some of the fused metal was ejected; the other assistant, immediately afterwards also, struck it with his moistened hand. After these experiments, which were made in opposition to Boutigny's precautions not to strike the mass, experiments which, for the sake of precaution, I wished to make before the immersion became unnecessary; I moistened my right hand, inserted the index-finger almost completely into the melted mass, and moving it very slowly through it, withdrew it in two seconds; at the same time I felt how the iron moved before my finger, but *did not experience the slightest* sensation of heat.*

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* More than twenty years ago Professor H. Rose, in visiting the foundries at Avestad, in Sweden, saw a workman, for a small reward, take melted copper

I should have considered the temperature of the iron, which was about 2732° Fahr., as below 96° Fahr.; for, on withdrawing the finger, it was not so warm as the other hand. M. Fessel also, and the other three persons who accompanied me, repeated this experiment, with certain modifications; one of them with his hand dry; another remarked that the hand, after having been previously dipped in water, when withdrawn, was only dry in that part which had not been immersed; a third took up the iron with the hand made hollow. The minute hairs upon the inserted fingers had entirely disappeared; but the nails were not injured, nor was any penetration of heat through the nails remarked. The hand, when withdrawn, had a slight empyreumatic odour, which was stronger when there were warts upon it; but in no case was there the slightest burning sensation, or even a disagreeable sensation of heat. Hence, certain minor operations in surgery might be performed with least pain by placing the foot in a bath of red-hot iron. Lastly, I made one other experiment, the result of which might have been anticipated.

I held the finger of a leathern glove, which I had well wetted inside, and had placed on a wooden rod for nearly a a minute in the melted iron; on withdrawing it the glove was not only unburnt, but had only a temperature of about 132° Fahr. (I had not a thermometer with me). Conjectures and theoretical views upon these remarkable phenomena, would be premature without further experiments. I hope, however, soon to be able to communicate some remarks upon them.—(*Philosophical Magazine*, vol. xxxvi., No. 241, p. 137.)

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with the bare hand from a crucible, and throw it against the wall. This confirms his statement, as also some other facts which Boutigny himself mentions in his memoir, that the phenomenon mentioned has long been known, especially among the people engaged in the arts.—(*Poggendorf.*)

A Biographical Sketch of the late Astronomer Caldecott.

Our afternoon edition of Wednesday contained an intimation of the demise of Mr John Caldecott, astronomer to H. H. the Rajah of Travancore,—the melancholy event having occurred at Trevandrum on the night of the 16th instant. He had had an attack of illness, something like a tendency to apoplexy or paralysis, about two months ago, but after going through a course of medicine he seemed to have recovered his health and spirits. About the 8th ultimo a general derangement of the system made its appearance, under which he continued to labour till death released him from his sufferings. The name of Mr Caldecott has been too often and too long before the world to suffer the removal from amongst us of him who bore it to be passed over with a mere obituary notice. With the earlier portion of the career of Mr Caldecott we are not acquainted: our impression is that he was bred an architect, and that as an astronomer and meteorologist, he was entirely self-taught. About the year 1832 he appears to have become known to the Rajah of Travancore,—one of the most accomplished, enlightened, and liberal-minded princes in India,—and to have acquired the confidence and friendship of the Resident, Colonel Fraser, an envoy altogether worthy of the most intellectual native court in the East. The mind of the Rajah, eminently endowed by nature, had received all the cultivation the highest English education could bestow: he was well read alike in the theology, the literature, and the sciences, of Europe, and resolved to indulge his tastes in a way worthy of one who was not alone a philosopher amongst princes, but a prince amongst philosophers. To Mr Caldecott, by this time well known as a mechanician and astronomer, was entrusted the planning, erection, furnishing, and charge, of an astronomical and meteorological observatory. Though entrusted with unlimited powers on the occasion, he wisely determined that no outlay beyond what was absolutely essential to effectiveness should be incurred on the building; but that no expense should be spared in providing instruments of such size and quality as would secure to the observatory where they were employed with zeal and judgment, a rank second to none in the world.*

The Observatory was erected on a hill of laterite abounding in granite, 2 miles from the sea, and 130 feet above high-water mark: it was 70 feet in length, and 30 in breadth, with three revolving domes. The instruments with which it was at first provided consisted of an 18-inch altitude and azimuth, a 30-inch transit, a fine equatorial, a reflecting circle, a 46-inch telescope, and three chronometers, all first-rate of their kind, and all the private property of Mr Caldecott. The instruments ultimately provided by His Highness consisted of a 5-feet transit, a transit clock, two 5-feet mural circles, an astronomical clock, an altitude and azimuth, two powerful telescopes, one of them a reflector, with micrometers, with a complete set of magnetic and meteorological instruments. These were all of the first-rate description which skill or money could secure: they were received in safety, and put in their places without delay. In 1840, the Observatory received an additional supply of magnetic and meteorological instruments, similar to those prescribed by the Royal Society and British Association for the sixty different observatories up and down the world. The task of arranging and setting to work single-handed so large an establishment, was no easy one; and the admirable manner in which Mr Caldecott accomplished it in incredibly short space of time gave sufficient proof of his enthusiasm as well as his ability. The tasks accomplished which possessed paramount claims on his attention, we find Mr Caldecott in 1837 engaged with the late distinguished Madras astronomer, Mr Taylor, in a magnetic survey of Southern India. Mr Taylor had, as far back as 1831, projected, at the suggestion of Professor Kupffer, a series of observations

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* Madras Literary Transactions, vol. vi., page 56. Description of the Trevandrum Observatory, by John Caldecott, Esq. The reader must remember that though instruments of the largest size may be essential for making great discoveries in astronomy, it is excellence of workmanship more than magnitude, and zeal in the astronomer more than any quality in his instruments, that ensures value to the work.

on the magnetic dip, when inability to procure the proper instruments, either at Madras or from England, postponed the execution of the scheme. At length, a dipping-circle, and fine set of dipping-needles, the property of the Bombay Geographical Society,* and which had been placed at the disposal of Captain Moresby, were lent by that officer to Mr Taylor. While the Madras astronomer took the eastern portion of the Peninsula to himself, the examination of the western division was assigned to Mr Caldecott. The philosophers met in July at Tranquebar, and for a time laboured together. Subsequently, each pursued the enquiries allotted him, with that unbounded zeal and consummate ability for which both were so conspicuously distinguished. The Madras service at this time was replete to overflowing with the spirit of research, and abounded beyond example with men of activity, talent, and accomplishments. The Literary Transactions of the day wellnigh eclipse the Bengal Journal itself, then under charge of the illustrious James Prinsep, one of the very foremost amongst the intellects of India. Meteorological research had, in 1837, just begun to be pursued with that method and system, and in the spirit of co-operation and concert, from the want of which it had hitherto so sadly suffered. Dr Turnbull Christie had some years before drawn up an excellent scheme of research, which was published after his death in the Journal for 1835. The following year the admirable system of Sir John Herschel was promulgated, under the name of a report of the South African Association; and the astronomers at Madras and Trevandrum resolved to carry out the scheme of connected enquiry by means of hourly observations at least one day every month to its fullest extent. Mr Caldecott had now taken a conspicuous place amongst the scientific men of India, and his name speedily became as well

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* The Geographical Society had at this time provided itself with a large supply of instruments for the purpose of promoting physical research. When their turn came to ask from a large establishment the loan of magnetic instruments known to be employed, and to have been got on purpose for general service, they were told none such existed—at the very time that an account of their use in itinerant investigations was published in their reports! A sad contrast certainly.

known in Europe as it had for some time been in the East. He contributed several papers on meteorology generally, and on temperatures underground in particular, to the British Association, and was specially referred to in the address of the President as their "distinguished associate Mr Caldecott." He had from 1841, when the general scheme of magnetic and meteorological research was commenced all over the world, set himself with his usual zeal to the working out of the plan. It was not until 1845 that the Royal Society determined on the best mode of publishing the vast mass of matter that had up to this time been collected,—and the Rajah of Travancore, scarcely appreciating the importance of economy of time, and little apprehending the calamity that was at hand, was naturally anxious that a mass of facts that had been gathered together at his own expense, and under his own directions, should reach the world through his own press. Mr Caldecott had now become deeply engaged in preparations for publication, when his health began to fail him, and in January 1849 came to Bombay, and for some time travelled about in the Concan, Deccan, and Ghauts, for change of air. He returned to Trevandrum and resumed his labours in March, and was, up to the time of his demise, deeply occupied in passing through the press the results of the researches of the preceding ten years. He had for some time been complaining, when on the 16th he was cut off, deeply lamented by all who knew him—by none more deeply than by the illustrious Prince whom he served, and the distinguished resident, General Cullen, who so cordially aided and sympathised in his exertions. Though only a few fragments of the results of his labours have hitherto been laid before the world, no man was less given to mystery-making or concealment than Mr Caldecott, as to the progress of his pursuits, or more anxious to place all his MSS., finished or unfinished, at the disposal of any one desiring to use them. When applied to contribute to the collection of observations now being made by the Geographical Society, he at once offered copies of the unpublished reports for the past ten years; and this being more than enough, he supplied a full series of observations from January 1848—the last fasciculus despatched just before his demise,—coming down to the present time. In the true spirit of philosophy, Mr Caldecott considered it a very small matter by whom truth was extended and promoted, in comparison to the fact of its extension and promotion. No man has deserved better of science than the Rajah of Travancore, and but few have been more fortunate in the selection of agencies through the means of which science has been aided. No man could have more ably or effectually assisted him in the noble plans he had conceived than Colonel Fraser; nor could India have supplied a more talented, upright, and accomplished, successor to the officer just named than was found in General Cullen. It will be a long time indeed before a fitting successor can be looked for to Mr Caldecott. The removal of the astronomer of Trevandrum completes the desolation accomplished in little more than a single year in all our observatories. Mr Taylor, of Madras, died in March 1848; Mr Curnin, formerly of the Bombay Observatory, in July; Colonel Wilcox, astronomer to the King of Oudh, in November; and, within twenty months of the removal of the first of the four, the last follows his illustrious brethren to the grave.—*Edit., Bombay Times.*

Thus well have Mr Caldecott's physical observations been described; of his astronomical labours there is not much known, and it is believed that they were rather devoted to the educational advancement of those he was amongst, than to the promotion and extension of the science. He had supplied the Observatory with sufficient instruments for the highest purposes; indeed it was better furnished than most European observatories, and than all the extra-European ones; and being blessed with a clearer sky than any other in the world, and a large staff of assistants being attached,—important results were confidently expected. Yet, perhaps from the natural bent of his mind being in another direction, he allowed himself to be carried away by the more showy but ephemeral matters of magnetical and meteorological observations, and by the extraordinary opinions propagated by the British Association at the time, in their Report on Meteorology: wherein it was inferred that there was nothing more to be discovered in astronomy; that Astronomical Observatories were now only occupied in doing over again what had been better done elsewhere; and that the astronomers would be better and more honestly employed in making magnetical and meteorological observations with their astronomical funds; a small portion of which, if spent in the above manner, would be sufficient, it was contended, to *create* new sciences of observation. Nervously anxious that the means so singularly at his disposal for the advancement of science, should be applied in the most unexceptionable manner, Mr Caldecott unfortunately was carried away by the above-mentioned peculiar views; and abandoning astronomy, applied himself to what was pointed out by so influential a body at home as a more proper subject of investigation. But though such vast sums have since been spent on magnetism and meteorology; though so many public and private observatories have been established by various nations; and exploring expeditions sent by sea to the Antarctic as well as to the Arctic regions; still those sciences are far from being on a sure scientific basis; and even the instruments and methods of employing them are so uncertain and insecure, that the value of the numerous observations accumulated not only by Mr Caldecott, but by the other magneticians and meteorologists of the day, is doubtful in the extreme, even in the interest which they may excite after the present hour. Meanwhile the tide of astronomical discovery passed him by; the honour of adding to that accumulating number of exact determinations of standard quantities, which serve to establish old discoveries in the realms of space, and lead to new ones,—was lost; and the certainty of procuring for his enlightened patron, the Rajah of Travancore, and for the country of his adoption, an estate in the zodiac, more enduring than all the empires of the East, was removed from him for ever.

In addition to all this, Mr Caldecott, though a good observer, is said by an intimate friend to have been deficient in mechanical skill; a serious failing in an astronomical observatory so far removed as Trevandrum. Hence, he never felt that perfect confidence in the instruments, and the results procured by them, which is only the case, and can only be, when the observer is both theoretically and practically an optician and a mechanician; when he has examined every possible source of error in the instruments; when he has detected the exact amount of failing in each particular direction; when he has found the reason of it, and has ascertained the laws both periodical and secular; and when he has contrived mechanical means to correct those errors which are excessive in size and constant in amount; and invented theoretical methods of correcting the effects of all the others.

Doubtless, with a new instrument in this country, and employed but a few miles from the workshop of its maker, good observations and reputable may have been produced, without any such searching and probing examinations; but, nevertheless, the power to make them is an absolutely necessary requisite with a practical astronomer in charge of a foreign observatory; and unless he does make them, and act upon them, he may be perfectly sure that however good he may fancy his observations to be, or they may actually and absolutely be, still they will never inspire confidence in the minds of others, nor his calculated results any conviction.

Mr Caldecott did, however, send three communications to the Royal Astronomical Society of London, and, from the talent he displayed in them, it is so much the more to be regretted that he did not devote a greater portion of his time to such subjects.

The first was a series of observations made with the fine equatorial of the Trevandrum Observatory, on the Great Comet of 1843, in right ascension and declination; to which descriptions of the physical appearance night after night, and computed elements of the orbit, were appended.

The second was an account of the total eclipse of 1843, to see which, in its totality, he had journeyed to a distant part of the country, where he had computed that the total obscuration, if any (for the case was very doubtful owing to the almost exactly equal apparent diameters of the sun and moon), would occur. And he made very excellent series of observations on the interesting physical circumstances attending the astronomical phenomena.

The third paper was a communication similar in nature to the first, but relative to the comet of 1844–5; also a very fine one, an unusually fine one in this so-called degenerate age of comets, but, like that, invisible to Europe during all the brightest part of its apparition. P. S.

On the Distribution of the Superficial Detritus of the Alps, as compared with that of Northern Europe. By Sir RODERICK IMPEY MURCHISON, F.R.S., &c. &c.

Referring to his previous memoir upon the structure of the Alps, and the changes which those mountains underwent, the author calls attention to the fact, that as during the formation of the molasse and nagelflue a warmer climate prevailed, so after the upheaval of those rocks, an entire change took place, as proved by the uplifted edges of such tertiary accumulations being surmounted by vast masses of horizontally-stratified alluvia, the forms of whose materials testify that they were deposited under water. The warm period, in short, had passed away, and the pine had replaced the palm upon the adjacent lands, before a glacier was formed in the Alps, or a single erratic block was translated.

Though awarding great praise to the labours of Venetz, Charpentier, and Agassiz, which have shed much light on glaciers, and particularly to the work of Professor J. D. Forbes, in clearly expounding the laws which regulate their movement, Sir Roderick conceives, that the physical phenomena of the Alps and Jura compel the geologist to restrict the former extension of the Alpine glaciers within infinitely less bounds than have been assigned to them by those authors.

True old glacier moraines may, he thinks, be always distinguished, on the one hand, from the ancient alluvia, and, on the other, from tumultuous accumulations of gravel, boulders, and far-transported erratic blocks, as well as from all other subsequent detritus resulting from various causes which have affected the surface. He first shews, from the remnants of the old water-worn alluvia which rise to considerable heights on the sides of the valley, that in the earliest period of the formation of the Alpine glaciers, water, whether salt, brackish, or fresh, entered far into the recesses of these mountains, which were then at a considerably lower level, *i. e.*, not less, perhaps, than 2500 or 3000 feet below their present altitude.

He next appeals to the existing evidences in the range of

Mont Blanc, to shew that, as each glacier is formed in a *transverse* upper depression, and is separated from its icy neighbour by an intervening ridge, so by their movement such separate glaciers have always protruded their moraines across the adjacent longitudinal valleys into which they descended, and never united to form one grand stream of ice in the valley below. To prove this, it is affirmed that there are no traces of lateral moraines on the sides of the adjacent main valleys, whether on the side of the great ridge from whence the separate glaciers issued, or on the opposite side of such main valley, which must have been the case, if a large mass of glacier ice had ever descended it. On the contrary, examples of the transport of moraines and blocks across such *main* or *longitudinal* depressions are cited from the valley of Chamonix on the one flank, and from the Allée Blanche and Val Ferret on the other or south side of the chain of Mont Blanc. Another proof is seen in the ancient moraine of the Glacier Neuva, the uppermost of the valley of the Drance; and a still stronger case is the great chaotic pile of protogine blocks accumulated on the Plan y Bœuf, 5800 French feet above the sea, which have evidently been translated right across the present deep valley of the Drance from the opposite lofty glacier of Salenon.

Having thus shewn that not even the upper longitudinal and flanking valleys around Mont Blanc were ever filled with general ice streams, the author has no difficulty in demonstrating that all the great trunk or lower valleys of the Arve, the Doire, and the Rhone, offer no vestiges of what he calls a true moraine; since, although they contain occasional large erratic blocks, for the most part irregularly dispersed, all the other detritus is more or less water-worn, to great heights above their present bottoms. As Venetz and Charpentier have attached great importance to the original suggestion of an old peasant of the Upper Vallais, that a great former glacier alone could have carried the erratic blocks to the sides of the lower valley of the Rhone; so, on the other hand, the author, if he had had any doubt himself, would have relied on the practised eye of his intelligent Chamonix guide, Auguste Balmat, who never recognised the remains of "moraines" in that detritus of the larger valleys which has been theoretically referred to old glacier action.

In descending from the higher Alps into the main or trunk valleys, Sir Roderick found many examples of rocks rounded on that side, which had been exposed to the passage of boulders and pebbles, with abrupt faces on the side removed from the agent of denudation, all of them reminding him forcibly of the *storm* and *lee* sides of the Swedish rocks over which similar water-worn materials have passed.

Seeing, then, that this coarse drift or water-worn detritus is distributed sometimes on the hard rocks, and often on the remnants of the old valley alluvia, he believes that the whole of the phenomena can be explained by supposing that the Alps, Jura, and all the surrounding tracts have undergone great and unequal elevations since the period of the formation of the earliest glaciers—elevations which, dislodging vast portions of those bodies, floated away many huge blocks in ice-rafts, down straits then occupied by water, and also hurled on vast turbid accumulations of boulders, sand, and gravel. To these operations he attributes the purging of the Alpine valleys of the great mass of their ancient alluvia, and also the conversion of glacier moraines into shingle and boulders. He denies that the famous blocks of Monthey, opposite Bex, can ever have been a portion of the left lateral moraine of a glacier which occupied the whole of the deep valley of the Rhine, as Charpentier has endeavoured to shew; and he contends that, if such had been the case, they would have been associated with numberless smaller and larger fragments of all the rocks which form the sides of the valley through which such glaciers must have passed. They are, however, exclusively composed of the granite of Mont Blanc; and must therefore, he thinks, have been transported by ice-rafts, which, having been forced with great violence through the gorge of St Maurice, served to produce many of the striæ which are there so visible on the surface of the limestone.*

* Mr Charles Darwin, in a recent letter to the author, adheres to his old opinions on this point, derived from observations in America, and says: "I

Fully admitting that the stones and sand of the moraines of modern glaciers scratch, groove, and polish rocks, Sir Roderick Murchison still adheres to the idea he has long entertained from surveys in Northern Europe,* that other agents more or less subaqueous, including icebergs and heavy masses of drift, have produced precisely similar results. He cites examples in the Alps, where, perfectly water-worn or rounded gravel being removed, the subjacent rocks are found to be striated in the directions in which such gravel has been moved; and he quotes a case in the gorge of the Tamina, above the Baths of Pfeffers, where this ancient striation, undistinguishable from that caused by existing glaciers, has, by a very recent slide of a heavy mass of gravel from the upper slope of the same rock, been crossed by fresh scorings and striæ, transverse to those of former date, from which the markings made in the preceding year only differ in being less deeply engraved. He also adverts to the choking up of some valleys, particularly of the Vorder or Upper Rhine, below Dissentis, by fracture, *in situ*, of mountains of limestone, which constitute masses of enormous thickness, made up of innumerable small fragments, all of which have been heaped together since the dispersion of the erractic blocks; and he further indicates the effects of certain great slides or subsidences within the historic era.

In considering the distribution of the erratic detritus of the Rhone, the author having denied that it can ever have been carried down the chief valley to the Lake of Geneva in a solid glacier, he still more insists on the incredibility of such a vast body of ice having issued from that one narrow valley, as to have spread out over all the low country of the

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feel most entirely convinced that *floating ice* and *glaciers* produce effects so similar, that at present there is, in many cases, no means of distinguishing which formerly was the agent in scoring and polishing rocks. This difficulty of distinguishing the two actions struck me much in the *lower parts* of the Welsh valleys."

* See Silurian System, pp. 509 to 547; Russia in Europe and the Ural Mountains, vol. i., pp. 507 to 559; Presidential Discourses, Proc. Geol. Soc., Lond., vol. iii., p. 671, and vol. iv., p. 93; Journ. of Geol. Soc., Lond., vol. ii., p. 349; and Trans. R. Geol. Soc., Cornwall, vol. vi.

Cantons Vaud, Friburg, Berne, and Soleure, and to have protruded its erratics to the slopes of the Jura, over a region of about 100 miles in breadth from north-east to south-west, as laid down in the map of Charpentier. He maintains, that in the low and undulating region between the Alps and the Jura, the small debris derived from the former has everywhere been water-worn, and that there is in no place which he saw anything resembling a true moraine; and he, therefore, believes, that the great granitic blocks of Mont Blanc were translated to the Jura by ice-floats, when the intermediate country was under water. He further appeals to the water-worn condition of all the detritus of the high plateaux around Munich, 1600 feet and 1700 feet above the sea, to shew that a subaqueous condition of things must be assumed, for the whole of the northern flanks of the Alps, when the great erratic blocks were carried to their present positions.

Professor Guyot of Neufchâtel has endeavoured to shew, that the detritus of the rocks of the right and left sides of the upper valley of the Rhone have also maintained their original relative positions in the great extra-alpine depression (Lake of Geneva), and that these relations are proofs that nothing but a solid glacier could have arranged the blocks in such linear directions. But the author meets this objection by suggesting, that there are notable examples to the contrary. He also refers to the great *trainées* of similar blocks, which preserve linear directions in Sweden and the low countries south of the Baltic, to shew, that, as this phænomenon was certainly there produced by powerful streams of water, so may the alpine detritus have been arranged by similar agency. In alluding to the drainage of the Isère, he further points to the admission of Professor Guyot, that nearly all its erratic detritus, both large and small, is rounded, and has undergone great attrition; and he quotes a number of cases in which such boulders and gravel, derived from the central ridges of Mont Blanc, have been transported *across* tracts now consisting of lofty ridges of limestone with very deep intervening valleys; and, therefore, he infers that the whole configuration of these lands has been since much changed, including the final excavations of the valleys and the translation of enormous masses of broken materials into the adjacent low countries of France.

In conclusion, it is suggested, that the dispersion of the far-travelled alpine blocks is a very ancient phænomenon, in reference to the historic era, and must have been coeval with the spread of the northern or Scandinavian erratics, which it has been demonstrated was accompanied chiefly by floating ice, at a time when large portions of the Continent and of the British Isles were under the sea. Viewing it, therefore, as a subaqueous phænomenon, Sir Roderick is of opinion that the transport of the alpine blocks to the Jura falls strictly within the dominion of the geologist who treats of far bygone events, and cannot be exclusively reasoned upon by the meteorologist, who invokes a long series of years of sunless and moist summers, to account for the production of gigantic glaciers upon land under present terrestrial conditions. This last hypothesis is, it is shewn, at variance even with the physical phenomena in and around the Alps, while it is in entire antagonism to the much grander and clearly-established distribution of the erratics of the north during the glacial period. The effect in each case is commensurate with the cause. The Scandinavian chain, from whence the blocks of northern Europe radiated, is of many times larger area than the Alps; and hence its blocks have spread over a much greater space. All the chief difficulties of the problem vanish, when it is admitted that enormous changes of the level of the land, in relation to the waters, have taken place since the distribution of large erratics—the great northern glacial continent having subsided, and the bottom of the sea further south having been elevated into dry land, whilst the Alps and Jura, formerly at lower levels, have been considerably and irregularly raised.—(*Quarterly Journal of the Geological Society,* No. xxi., p. 65.)

Observations on the Size of the Brain in various Races and Families of Man. By SAMUEL GEORGE MORTON, M.D., Author of *Crania Americana*, &c., &c.

I have great pleasure in submitting to the Academy* the results of the internal measurements of 623 human crania, made with a view to ascertain the relative size of the brain in various races and families of Man.

These measurements have been made by the process invented by my friend, Mr J. S. Phillips, and described in my *Crania Americana*, p. 253, merely substituting leaden shot, one-eighth of an inch in diameter, in place of the white mustard-seed originally used. I thus obtain the *absolute capacity of the cranium, or bulk of the brain, in cubic inches;* and the results are annexed in all those instances in which I have had leisure to put this revised mode of measurement in practice. I have restricted it, at least for the purpose of my inferential conclusions, to the crania of persons of sixteen years of age and upwards, at which period the brain is believed to possess the adult size. Under this age, the capacity measurement has only been resorted to for the purpose of collateral comparison; nor can I avoid expressing my satisfaction at the singular accuracy of this method, since a skull of an hundred cubic inches, if measured any number of times with reasonable care, will not vary a single cubic inch.

All these measurements have been made with my own hands. I at one time employed a person to assist me; but, having detected some errors in his measurements, I have been at the pains to revise all that part of the series that had not been previously measured by myself. I can now, therefore, vouch for the accuracy of these multitudinous data, which I cannot but regard as a novel and important contribution to Ethnological science.

I am now engaged in a memoir which will embrace in detail the conclusions that result from these data; and, meanwhile, I submit the following tabular view of the prominent facts.

* Academy of Natural Sciences of Philadelphia.

Table, shewing the Size of the Brain in Cubic Inches, as obtained from the Internal Measurement of 623 Crania of various Races and Families of Man.

Races and Families.	No. of Skulls.	Largest I. C.	Smallest I. C.	Mean.	Mean.
MODERN CAUCASIAN GROUP.					
Teutonic Family:					
Germans	18	114	70	90	92
English	5	105	91	96	
Anglo-Americans	7	97	82	90	
Pelasgic Family: Persians, Armenians, Circassians	10	94	75	84	
Celtic Family: Native Irish	6	97	78	87	
Indostanic Family: Bengalees, &c.	32	91	67	80	
Semitic Family: Arabs	3	98	84	89	
Nilotic Family: Fellahs	17	96	66	80	
ANCIENT CAUCASIAN GROUP.					
From the Catacombs. *Pelasgic Family:* Græco-Egyptians	18	97	74	88	
From the Catacombs. *Nilotic Family:* Egyptians	55	96	68	80	
MONGOLIAN GROUP.					
Chinese Family	6	91	70	82	
MALAY GROUP.					
Malayan Family	20	97	68	86	85
Polynesian Family	3	84	82	83	
AMERICAN GROUP.					
Toltecan Family:					
Peruvians	155	101	58	75	79
Mexicans	22	92	67	79	
Barbarous Tribes: Iroquois, Lenapé, Cherokee, Shoshoné, &c.	161	104	70	84	
NEGRO GROUP.					
Native African Family	62	99	65	83	83
American-born Negroes	12	89	73	82	
Hottentot Family	3	83	68	75	
Alforian Family: Australians	8	83	63	75	

The measurements of children, idiots, and mixed races, are omitted from this table, excepting only in the instance of the Fellahs of Egypt, who, however, are a blended stock of two *Caucasian* nations,—the true Egyptian and the intrusive Arab, in which the characteristics of the former greatly predominate.

No mean has been taken of the Caucasian race* collectively, because of the very great preponderance of Hindu, Egyptian, and Fellah skulls over those of the Germanic, Pelasgic, and Celtic families. Nor could any just *collective* comparison be instituted between the Caucasian and Negro group in such a table, unless the small-brained people of the latter division (Hottentots, Bushmen, and Australians) were proportionate in number to the Hindoos, Egyptians, and Fellahs of the other group. Such a computation, were it practicable, would probably reduce the Caucasian average to about 87 cubic inches, and the Negro to 78 at most, perhaps even to 75, and thus confirmatively establish the difference of at least nine cubic inches between the mean of the two races.

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* It is necessary to explain what is here meant by the word *race*. Further researches into Ethnographic affinities will probably demonstrate that what are now termed the *five races* of men, would be more appropriately called *groups*; that each of these groups is again divisible into a greater or smaller number of primary races, each of which has expanded from an aboriginal nucleus or centre. Thus I conceive that there were several centres for the American group of races, of which the highest in the scale are the Toltecan nations, the lowest the Fuegians. Nor does this view conflict with the general principle, that all these nations and tribes have had, as I have elsewhere expressed it, a common origin; inasmuch as by this term is only meant an indigenous relation to the country they inhabit, and that collective identity of physical traits, mental and moral endowments, language, &c., which characterize all the American races. The same remarks are applicable to all the other human races; but in the present infant state of Ethnographic science, the designation of these primitive centres is a task of equal delicacy and difficulty. I may here observe, that whenever I have ventured an opinion on this question, it has been in favour of the doctrine of *primeval diversities* among men,—an original adaptation of the several races to those varied circumstances of climate and locality, which, while congenial to the one are destructive to the other; and subsequent investigations have confirmed me in these views. See *Crania Americana*, p. 3; *Crania Ægyptiaca*, p. 37; *Distinctive Characteristics of the Aboriginal Race of America*, p. 36; *Silliman's American Journal of Sciences and the Arts*, 1847; and my *Letter to J. L. Bartlett, Esq.*, in vol. ii. of the Transactions of the Ethnological Society of New York.

Large as this collection already is, a glance at the table will shew that it is very deficient in some divisions of the human family. For example, it contains no crania of the Esquimaux, Fuegians, Californians, or Brazilians. The skulls of the great divisions of the Caucasian and Mongolian races are also too few for satisfactory comparison, and the Sclavonic and Tchudic (Finnish) nations, together with the Mongol tribes of Northern Asia and China, are among the especial *desiderata* of this collection.

Among the facts elicited by this investigation are the following:—

1. The Teutonic or German race, embracing, as it does, the Anglo-Saxons, Anglo-Americans, Anglo-Irish, &c., possess the largest brain of any other people.

2. The nations having the smallest heads, are the ancient Peruvians and Australians.

3. The barbarous tribes of America possess a much larger brain than the demi-civilized Peruvians or Mexicans.

4. The ancient Egyptians, whose civilization antedates that of all other people, and whose country has been justly called "the cradle of the arts and sciences," have the least-sized brain of any Caucasian nation, excepting the Hindoos; for the small number of Semitic heads will hardly permit them to be admitted into the comparison.

5. The Negro brain is nine cubic inches less than the Teutonic, and three cubic inches larger than the ancient Egyptian.

6. The largest brain in the series is that of a Dutch gentleman, and gives 114 cubic inches; the smallest head is an old Peruvian, of 58 cubic inches; and the difference between these two extremes is no less than 56 cubic inches.

7. The brain of the Australian and Hottentot fall far below the Negro, and measures precisely the same as the ancient Peruvian.

8. This extended series of measurements fully confirms the fact stated by me in the *Crania Americana*, that the various artificial modes of distorting the cranium, occasion no diminution of its internal capacity, and consequently do not affect the size of the brain.

Enumeration of the Races of Man. By CHARLES PICKERING, M.D., Member of the Scientific Corps attached to the United States Exploring Expedition.

Three races of men are familiarly known in the United States, and are admitted by general consent. The same three physical races have been considered by eminent naturalists (who, however, have not travelled), to comprise all the varieties of the human family. Blumenbach has indicated a fourth race, the Malay; and even a fifth has been shadowed forth in the accounts of the Australian Seas. It was impossible, however, from the materials furnished by books, to define the geographical boundaries of these races; a point which seemed of importance, as forming in a good degree the basis of our reasoning on the whole subject.

This, then, was one of the objects of investigation I proposed to myself, on joining the Exploring Expedition; and my previous experience as a naturalist, a pursuit calling for the constant exercise of the powers of discrimination, gave me some advantage in conducting the inquiry.

At one time during the voyage I thought my task nearly accomplished; and after visiting Australia and New Zealand, I actually penned an opinion, that the races of men were five in number. Soon, however, I was compelled to admit three more; neither was this the limit of the productiveness of nature, in new and undreamt-of combinations of features.

More careful observation than at the outset had seemed necessary, was now called into requisition; and often, for a time, I experienced perplexity. One difficulty arose in fixing in the mind, while passing from place to place, the relative shades of complexion. Fortunately for my purpose, tattooing was practised in many of the countries visited, and these markings afforded a convenient test of the depth of hue. Individuals also, of three or more races, being present among the crews of our vessels, afforded the means of making some direct comparisons. In the end all difficulties vanished, and I was enabled to arrive at satisfactory conclusions.

It should be observed, that in the countries visited by the Expedition, the inhabitants present among themselves great uniformity of feature and complexion; while in the Arab countries, and in Western Hindostan, there is an astonishing diversity of aspect in the population; independently, to all appearance, of the great mixture of races. The mountain region of Abyssinia is said likewise to present a seemingly heterogeneous population; but in all the countries which I have myself visited, the varieties of feature have appeared susceptible of reduction to the arrangement adopted in the present work.

I have seen in all eleven races of men; and though I am hardly prepared to fix a positive limit to their number, I confess, after having visited so many different parts of the globe, that I am at a loss where to look for others. They may be enumerated conveniently enough in the order of complexion; and, beginning with the lightest, I will add some of the more obvious distinctive characters.

a. *White.*

1. *Arabian.*—The nose prominent, the lips thin, the beard abundant, and the hair straight or flowing.

2. *Abyssinian.*—The complexion hardly becoming florid, the nose prominent, and the hair crisped.

b. *Brown.*

3. *Mongolian.*—Beardless, with the hair perfectly straight, and very long.

4. *Hottentot.*—Negro features, and close, woolly hair, and the stature diminutive.

5. *Malay.*—Features not prominent in the profile, the complexion darker than in the preceding races, and the hair straight or flowing.

c. *Blackish-Brown.*

6. *Papuan.*—Features not prominent in the profile, the beard abundant, the skin harsh to the touch, and the hair crisped or frizzled.

7. *Negrillo.*—Apparently beardless, the stature diminutive,

the features approaching those of the Negro, and the hair woolly.

8. *Indian or Telingan.*—The features approaching those of the Arabian, and the hair, in like manner, straight or flowing.

9. *Ethiopian.*—The complexion and features intermediate between those of the Telingan and Negro, and the hair crisped.

d. Black.

10. *Australian.* — Negro features, but combined with straight or flowing hair.

11. *Negro.*—Close, woolly hair, the nose much flattened, and the lips very thick.

In an absolute sense, the terms "white" and "black," are both inapplicable to any shade of the human complexion; but they are sanctioned by general usage, and there may be some convenience in retaining the above four general divisions. Two of the races may therefore be designated as white, three as brown, four as blackish-brown, and two as black.

Five of the races have the hair straight or flowing; while in the others it is more or less crisped, and in two of them it may with propriety be termed woolly.

Other modes of associating the races may also be mentioned. Maritime habits, and the part they appear to have taken in colonizing the globe, would lead us to separate the Malay, Negrillo, and Papuan; or the three islands from the eight continental races.

Again, looking to their distribution over the surface of the globe, six of the races may be regarded as Asiatic or East Indian, and four as African; the eleventh (the White race) being in common, or holding geographically an intermediate position.

The existence of races, it should be observed, is a phenomena independent of climate. All the physical races that occur in cold regions can be traced by continuity to the tropics, where, moreover, we find other races in addition.

By the same evidence of geographical continuity, the population of one hemisphere can be satisfactorily derived from the other; but a difficulty arises in narrowing the circle. On the one hand, it seems quite impossible to trace the four African races to any part of Asia; and, on the other, it will be equally difficult to connect the Mongolian race with the African continent.

Description of the Chronoscope, an instrument proposed for finding the Time by Observation, and thence deducing the Latitude and Longitude of the place of the Observer. By the Rev. W. Hodgson, Old Brathay, Ambleside. Communicated by the Author.

Sir,—As the importance of ascertaining correct time is generally acknowledged by both scientific and practical men, probably little apology is necessary for introducing to your readers some particulars relative to a simple double-altitude and meridian-instrument, which will accomplish that object without requiring either the latitude or longitude of the place of observation, or any graduated circle, or any assistance from any observatory, or anything beyond a plumb-line and a watch of uniform rate.

The instrument is susceptible of several different forms. Of these, perhaps, the most commodious may be described as a right rhombic parallelopiped of glass, formed by a pair of equilateral triangular prisms, with two of their faces in contact, and with their axes parallel to each other. The brass frame, on which the prisms are mounted, covers about one-third of each of their faces at the parts where they are in contact, *i. e.*, at the greater angles of the rhomb, and is fitted (either in the same way as the reflector of an ordinary microscope, or by some other similar plan) so as to allow the instrument to be moved at pleasure on either of two planes, which are perpendicular to each other. These planes being respectively parallel and perpendicular to those faces of the prisms which are in contact.

If the instrument thus described be clamped to a post-foot or support (by means of a screw bearing upon one of its axes,) in such a position that all the edges of the prisms are parallel to the horizon (an adjustment which is readily made by observations on a plumb-line) and so that the plane, passing through the faces of the prisms which are in contact, is inclined to the horizon at any angle which is a few degrees less than the sun's meridian altitude, then, by moving the instrument upon that axis which is not clamped, two images of the sun may be seen, by looking towards that body through either of the prisms, to approach, coincide, and separate, at certain instants before noon; and again, at corresponding instants after noon, to approach, coincide, and separate, as before. By observing the times at which these phenomena occur, and taking the semi-sum of the intervals between the corresponding pairs of observations, or a mean deduced from the whole six observations (correcting, if necessary, for the change of declination) the *true apparent noon* is at once determined. By repeating this process on successive days, the rate of the watch may be ascertained and corrected.

When the time is thus found, the instrument, at any succeeding noon, may be permanently fixed with the plane passing through those faces of the prisms which are in contact, perpendicular to the horizon, and with the two solar images in accurate coincidence. In this position, the instrument affords the means of observing with accuracy the passage of the sun across the plane of the meridian on any future occasion when that luminary is visible at the time. The passage of the moon also across the meridian may be similarly observed, and from thence the *longitude* of the place, if unknown, may be computed.

If the time of the true apparent noon is supposed to be known, and the instrument is required only to keep the true time deduced from some other source, the two axes above mentioned may both be dispensed with, and it will be sufficient to place the edges of the prisms so as to be perpendicular to the horizon, and to have the two solar images seen in exact coincidence at the instant of true apparent noon.

If the prisms, instead of being placed in close contact, are separated by a narrow strip of thin sheet-metal placed between their edges at one angle of the rhomb, the number of transits is increased from one to four or even five; thus affording five *pairs* of observations for altitudes, or, counting first and last contacts, as many as twenty observations in all.

If the axes, about which the instrument is moveable, are fitted with graduated circles and verniers, it becomes capable of measuring angles in a vertical, horizontal, or any other plane, between any objects which are sufficiently bright to be seen after three reflections: or if the rhomb is fitted perpendicularly upon the circumference of a graduated circle, which is moveable in a plane parallel to the equator, it becomes an accurate *solar clock*, which will give the true apparent time at *any* hour when the sun is visible.

The principles employed are similar to, but not identical with, those of several well-known instruments. In the ordinary way of using the sextant or reflecting circle, the inclination of the planes passing through two objects is found by bringing one of the objects, seen without any reflection, to coincide with an image of the other produced by *two* reflections. When these instruments are used with the artificial horizon for measuring altitudes, in the one case, the unreflected object is brought to coincide with an image of the other produced by *three* reflections; and in the other case, "the plane in which the body is, is determined by three reflecting planes combined" in a manner "whereby they are used as one *single* and *double* reflector." The principle thus last stated, has been very ingeniously employed by the inventor of the dipleidescope, but it is obvious that, before the production of that instrument, the principle itself had been in almost constant use. In order, however, to effect the end aimed at by the dipleidescope or by the sextant and artificial horizon, it is not necessary to restrict the comparison to the two cases of (1.) the object and the trebly-reflected image; and (2.), the singly and doubly reflected images: for the same purpose is answered by bringing into coincidence with the object, or with its image produced by 2, 4, 6, &c., $2n$, reflections, *any* of its images produced by 1, 3, 5, 7, &c., $\overline{2n+1}$ reflections. In the instrument described in this paper, the planes are so arranged that an image produced by *three internal* reflections, is brought to coincide with an image arising from *two* such reflections ; and when good flint-glass prisms are used, the loss of light is much less than would be imagined by those who are not practically familiar with the phenomena of internal reflection. The image of three reflections may also, in this instrument, be compared with the unreflected object, but then, in order to secure *exact* coincidence of the image and object, the instrument requires to be placed (as is necessary for the sextant, reflecting circle, and dipleidescope) in such a manner, that the rays of light are incident upon the reflectors in a plane perpendicular to their intersection. With the images arising from two and three reflections this is not essential ; for as parallel rays, incident upon the prisms, are symmetrically reflected and refracted, they emerge parallel to each other even at great obliquities, and are free from chromatic confusion.

In the case above referred to, in which the faces of the prisms, instead of being placed in contact, are inclined to each other at a small angle, if the instrument is fixed so that any coincidence of the solar images occurs when the sun's centre is on the meridian, the azimuths of the other planes in which this phenomenon takes place may be easily found ; and from these, the sun's declination and the observed time of his passing any one or more of these planes, the *latitude* of the place may be readily computed.

In default of a more appropriate name for this simple instrument for finding the time of observation, and thence deducing the latitude and longitude of the place of the observer, the term CHRONOSCOPE is suggested by the inventor, who has the honour to be, Sir, your obedient servant,

WILLIAM HODGSON.

A Description of two additional Crania of the Engé-ena (*Troglodytes gorilla*, Savage), *a second and gigantic African species of a Man-like Ape, from Gaboon, Africa.* By JEFFRIES WYMAN, M.D.*

The evidence now existing of a second and gigantic African species of man-like ape, as appears from published reports, consists of the following remains :—1. Four crania in the United States, two males and two females ; of a large portion of a male skeleton ; and of the pelvis, and of some of the bones, of a female. These were the first remains of this animal which had been brought to the notice of naturalists, and were described in the Boston Journal of Natural History.† 2. Three other crania subsequently discovered, exist in England, and have been made the subject of an elaborate memoir by Professor Owen, in the Transactions of the Zoological Society of London.‡ 3. Quite recently, Dr George A. Perkins, for many years an able and devoted labourer in the missionary enterprise at Cape Palmas, West Africa, has brought to the United States, two additional crania, one of which is deposited in the Museum of this Society, and the other in that of the Essex Institute in Salem. Both of these have been referred to me for the purpose of description, and it is the object of this communication, to notice the more important anatomical features of this, the largest of African Quadrumana, with regard to which additional information is desired.

Cranium 1.—*Male.*—This belonged to an adult Engé-ena,§ as is evident from the fact, that the teeth are all perfectly

* Read before the Boston Society of Natural History, Oct. 30, 1849.

† See Proceedings of the Boston Soc. Nat. Hist., August 18, 1847 ; also a Description of Characters and Habits of Troglodytes gorilla, by Thomas S. Savage, M.A., Corresp. Mem. Bost. Soc. Nat. Hist. ; and of the Osteology of the same, by Jeffries Wyman, M.D., Boston Jour. Nat. Hist., vol. v., p. 417, 1847.

‡ Osteological contributions to the Natural History of the Chimpanzees (*Troglodytes*, Geoff.), including the description of the skull of a large species (*T. gorilla*, Savage), discovered by Thomas S. Savage, M.D., in the Gaboon country, West Africa ; by Professor Owen, F.R.S., F.Z.S., &c. Read, Feb. 22, 1848. Trans. Zoolog. Society of London, vol. iii. p. 381, 1849.

§ Professor Owen designates *T. gorilla* as the "Great Chimpanzée." The Mponges (natives inhabiting the banks of the Gaboon), call this species the Engé-ena, a more desirable name, as the term Chimpanzée has been always associated with the black or smaller species.

developed; yet not to an old one, as appears from the circumstances, that the points of the molars are but very slightly worn, and the crests on the top of the head and occiput, are but imperfectly formed. Its size, as well as that of all the other crania of this species which have been measured, when compared with that of *T. niger* (Chimpanzée), and a well-marked Negro head, may be learned from an inspection of the following table.

TABLE I.—*Measurements of the Crania of T. gorilla, of T. Niger, and of the Cranium of a native African, in inches and tenths. Nos.* 2, 6, 7, *and* 8, *are in inches and lines.*

	Troglodytes gorilla.						T. niger.		Man.
	Males.				Females.		Male.	Female.	
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Length of head from occiput to edge of incisive alveolus . . .	11·2	11·4	11·0	10·2	9·10	9·0	8·0	7·9	9·6
Greatest breadth across post-auditory ridges .	6·1	6·10	6·4	5·9	5·2	5·6	5·0	4·6	5·4
Smallest diameter behind orbits	2·5	3·3	2·9	2·7	2·5	2·4	2·6	2·8	3·4
Diameter of face across zygomatic arches .	6·5	6·6	7·0	6·4	5·5	5·3	5·0	4·8	5·7
Diameter of face outside of the middle of the orbits	4·9	6·0	5·8	5·7	4·3	4·8	4·3	4·0	4·9
From occiput to most prominent part of supra-orbitar ridge . .	7·3	...	7·6	6·5	6·5	6·1	5·4	5·3	7·2
From supra-orbitar ridge to edge of incisive alveolus . . .	4·8	...	5·7	6·0	...	...	4·0	4·4	3·5
Breadth of zygomatic fossa	1·7	1·8	1·9	1·8	1·4	1·5	1·3	1·1½	1·1
Inter-orbitar space .	1·1	1·3	1·2	1·1	1·0	1·1	0·8	0·7	1·2
Transverse diameter of orbits	1·5	1·9	1·8	1·6	1·4	1·6	1·5	1·6	1·6
Vertical	1·6	1·7	1·6	1·6	1·4	1·7	1·3	1·3	1·3
Length of bony palate from outer edge of incisive alveolus . .	3·7	4·1	...	4·3	3·4	3·3	...	...	1·7
From anterior edge of foramen magnum to outer edge of incisive alveolus	7·2	...	...	7·4	3·5	...	...	...	...

Crania I. and V. were the ones brought by Dr Savage to this country; II., VI., VII. and VIII., are the crania described by Professor Owen; III. and IV. are the crania which were obtained by Dr Perkins; IX. the cranium of a Negro born in Africa, in whom the characteristics of the race were well marked, and which belongs to the Cabinet of the Boston Society for Medical Improvement. (See Catalogue of Society's Cabinet, Specimen No. 61.)

This cranium does not agree with that figured by Professor Owen in his Memoir (Plate LXI.), in the exclusion of the orbits from view, by the prominent malar bones, when the skull is seen in profile, but as was the case in those discovered by Dr Savage, the nasal bones are wholly, and the orbit in part brought into view. In none of them is it more excluded than in the first figures of our Memoir. The great ridges above the orbits, which are so widely developed in *T. niger*, are still more so in the present species, and in the specimen now under consideration, sustain the former statements, with regard to them. Professor Owen remarks, in connection with them—" The prominence of the whole supra-orbitar ridge, reaches its maximum in the present species, and forms the most marked distinction in the comparison of its skull with that of Man."—(*Memoir*, p. 405.)

Sutures.—I have shewn in a former communication, from an examination of several crania of the Chimpanzée, that nearly all the sutures are completely obliterated early, during the adult period.* From a careful examination of the six crania of the Engé-ena to which I have had access, there is every reason to believe that an early coossification takes place in them also. In the skull now under consideration, which, it is to be remembered, has not long passed the adult period, the frontal, the sagittal, the coronal, the squamous portion of the temporal sutures, all those in the temporal fossa, as well as the transverse portion of the lambdoidal, are no longer persistent. The crania which have been examined by Professor Owen, or some of them at least, indicate an opposite state of things. To ascertain, therefore, the value of cranial sutures as specific signs, it is quite obvious, that a large number crania of different ages must be critically examined.

Intermaxillaries.—These bones, so important as zoological indications, are completely coossified with the maxillaries, and with each other. No indication of a suture exists between and the last-mentioned bones, either on the external surface below the nasal openings, or in the roof of the mouth. I was

* Boston Journal of Natural History, April 1843.

not able to find any indications of the ascending portion of the intermaxillary bone, which articulates with the nasals, until led by Professor Owen's description to make a more careful search. Although, externally, there was no mark which would lead an anatomist to infer its existence, yet within the nasal cavity, at a short distance from its margin, the edge of the process was easily detected, it not having become coossified in that region with the adjoining bone.

The extension of the intermaxillary upwards, as far as the ossa nasi, so as to form the lateral walls of the external nasal orifice, as was indicated in a specimen of Chimpanzée, examined by Professor Owen, is still obvious in a young skull of the same species in my possession, where it reaches the nasals by a slender and pointed process. The enlargement of this process in the Engé-ena,* so as to form an extensive articulation with the nasal bones, inasmuch as it is a repetition of what exists in the lower quadrumana, and nearly all the mammalia, must be regarded as an index of degradation.

Ossa Nasi.—Professor Owen, in his Memoir† on the Engé-ena, in speaking of the sutures between the nasal maxillary and intermaxillary bones, says, " It is remarkable, indeed, since these sutures remain so distinct in the adult female skull, and the two adult male skulls, in the Bristol Museum, that no trace of them should have been detected in either of the four skulls taken to America by Dr Savage, in which the ossa nasi are described as being firmly coossified with each other, and the surrounding bones" (the concluding words of the above sentence he does not quote, viz., " but their outline is sufficiently distinct"). In the cranium brought by Dr Perkins, the consolidation of these bones is equally complete, and their outline is but indistinctly traceable.

In the crania formerly described, the ossa nasi form, on the median line, a sharp elevation or crest ; in the specimen figured by Professor Owen, (Plate LXII.), this is represented by a more rounded and convex ridge, and, thus offering a feature of approximation to the human structure, which is

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* This is very distinctly shewn in Pl. LXII. of Professor Owen's Memoir.

† Op. Cit., . 420.

very faintly indicated, if at all, in the skull of the *T. niger.** In the cranium now under consideration, when compared with the Plate above referred to, the convexity is still more remarkable, and will bear a more favourable comparison, with the " bridge" of the nose in some of the human races.

The expansion of the nasals above, where they are interposed between the frontals, as described by Professor Owen, was overlooked in my former description, only very faint indications of sutures remaining. On a more careful examination, the outline of the portion of bone interposed between the orbitar process of the frontals is indistinctly traceable in the male skull discovered by Dr Savage, and in both of the crania brought to this country by Dr Perkins; and in all of them, on a line with the upper extremity of the ascending process of the superior maxillary bone, at the point where the nasal bones become the most contracted, there exists an equally strong indication of a transverse suture, which separates the portion marked 15 in Professor Owen's figure from the true nasals; and equally distinct indications of this suture exist in his figure just referred to. Thus we have strong ground for the supposition that the part marked 15 by Professor Owen may not be the expanded portion of the nasals but an additional osseous element intercalated between the frontals. In this event, my original description of the ossa nasi, " as having a more triangular form than in the Chimpanzée, the apex being more acute," still holds good. If, however, the bone referred to prove to be a portion of the nasals, we shall have in this another index of the inferiority to the Chimpanzée, as it is a repetition of what is met with in the lower quadrumana.

Teeth.—The molars alone remain, the incisors and canines having been lost. The length of the grinding surface of the molar teeth is 2·9 inches, the two rows being nearly parallel to each other. This is true of the alveoli, though the crowns slightly diverge from each other posteriorly, in consequence of an inclination outwards. Nearly all of the cusps of the teeth are perfect, those of the first molar being the most worn, as would naturally be expected, it being the first which

* Op. Cit., p. 393.

is protruded. The inner cusps of this tooth are worn nearly to the base; the outer are but slightly abraded, and the same is the case with the inner cusps of the second molar; with these exceptions, the points of the different crowns of the molars and premolars are entire.

In comparing their grinding surface with that of the human jaw, one cannot but be struck with its greater extent, with the much greater development of the outer row of cusps, and the high ridge which, on all three of the molars, connects the outer row of cusps with the anterior inner cusp. In these respects, as well as in having the third molar, or the "dentes sapientiæ" of equal size with the others, the Engé-ena recedes from the Chimpanzée, and still farther from Man.

In the left upper jaw, and on the level with the lower extremity, or the pterygoid process, a supernumerary molar existed, still buried in its bony cavity, the roots not having as yet been developed. In the configuration of its grinding surface it did not conform with either of the other teeth.

Bony Palate.—By reference to the table of measurements, it will be seen that the space between the incisive alveoli, and the edge of the hard palate is much greater proportionally than in the Chimpanzée. The median suture has disappeared, and only slight indications remain of a former suture between the maxillaries and the ossa palati. The emargination on the middle of the edge of the palate is much less distinct than in either of the other specimens which I have examined, or than in that figured by Professor Owen.

The *Vomer* has the same thin and delicate structure as in the other crania, and does not meet the ossa palati at the posterior edge.

Cranial capacity.—In studying the anatomical characters of this and the allied quadrumana, with reference to their zoological position, nothing can be more desirable than to have accurate knowledge with regard to the structure and dimensions of the brain, for this may be regarded as one of the most important of all the tests of elevation or degradation. The bodies of the adult anthropoid animals so seldom fall into the hands of the anatomist, that it becomes extremely difficult to accumulate observations on the actual condition of this organ. In the comparative study of human crania, with reference to national peculiarities, much light has been derived from accurate measurements of their internal capacity. These may be readily obtained, and form a very important substitute for the actual dimensions of the brain itself. In the subjoined tables, I have given the results of the measurements of all the crania, both of the Engé-enas and Chimpanzees, to which I have had access while writing these remarks ; and as they have been repeated in each case several times over, they may be regarded as nearly accurate. The capacity of the third cranium is alone doubtful ; a portion of the occiput having been destroyed, rendered exact measurement impracticable, though it is believed that the result can differ but little from the truth.

TABLE II.—*Cranial capacity of Adult Engé-enas.*

	Cubic Inches.
1. Male, from Dr Perkins	34·5
2. Male, from Dr Savage	28·3
3. Male, from Dr Perkins	28·0 ?
4. Female, from Dr Savage	25·0
Mean of the four crania	28·9½

TABLE III.—*Cranial capacity of Adult Chimpanzees.*

	Cubic Inches.
1. Female	26·0
2. Female	24·0
3. Female	22·0
Mean capacity of three skulls	24·0

Cranial capacity of Young Chimpanzees.

4. First dentition complete	20·0
5. First dentition complete, but the sutures obliterated to a less extent than in the preceding	18·0

The above results clearly indicate that there exists a wide range in the cranial capacity of the Engé-enas, amounting to nine cubic inches, when both sexes are included in the observation. While it would be desirable to have the measurements of a much larger number, we still have evidence for concluding, that in the Engé-ena, as in Man,* the capacity of the cranium of the male is larger than that of the female; the smallest male skull of the Engé-ena measuring twenty-eight cubic inches, and the female only twenty-five cubic inches.

In Table III., the three adults are females, and it is quite worthy of notice, that the internal capacity of these differs so little from that of the female Engé-ena, while at the same time, the body of the Chimpanzée is so much smaller than that of the other species. By comparing the measurements given of the corresponding portions of the skeleton of the Engé-ena and Chimpanzée, it will be seen that a much wider difference exists between them, than exists between the dimensions of their respective brains.

It is interesting to contrast the measurement of the cranial capacity of these members of the Quadrumanous group with that of some of the human races. It results from Dr Morton's table, at page 263 of this volume, that the smallest mean capacity in Man is that derived from Hottentots and Australians, which equals only 75 cubic inches, while that of the Teutonic nations amounts to 90 cubic inches. The maximum capacity of the Engé-ena, is therefore considerably less than one-half of the mean of the Hottentots and Australians, who give us the minimum average of the human races.

Cranium 2, *Male.*—This cranium belonged to an individual much older than the one described in the preceding pages, the inner row of cusps of all the molars having been worn to their bases. The same obliteration of the sutures had taken place, the malar bones are more tumid, rendering the edge of the lower and outer part of the orbit more rounded. The floor of the nasal orifice slopes gradually from the anterior extremity of the vomer to the edge of the incisive alveoli, and presenting a groove on the median line. In man, the inter-

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* "Although many female brains exceed in weight particular male brains, the general fact is sufficiently shewn, that the adult male encephalon is heavier than that of the female, the average difference being from 5 to 6 oz." From the examination of 278 male brains, and 191 females, "an average weight is deduced of $49\frac{1}{2}$ oz. for the male, and 44 oz. for the female." Quain and Sharpey's Anatomy, edited by Joseph Leidy, M.D., vol. ii., p. 185, Philadelphia, 1849.

maxillary bones form a projecting ridge on the median line, both in and below the nasal orifice, and at the middle of the border of this opening form the projecting "nasal spine," which is not met with in any of the lower animals, and is, therefore, *an anatomical character peculiar to man.* With regard to this conformation of the intermaxillary bones, the Engé-ena recedes farther from man than the Chimpanzée. Two infra-orbitar foramina exist on each side. The crests are not so well developed as in the cranium just described. The occiput having been in part destroyed, the cavity of the cranium is completely exposed. A groove for the lodgement of the longitudinal sinus is well defined; "digital impressions," formed by the cerebral convolutions, exist, but not well marked, the crista-galli is merely rudimentary, and is represented by a very slight median ridge, the olfactory fossa is quite deep, the cribriform plate being on a level with the middle of the orbit; about five parallel grooves for the lodgement of the branches of the dura matral artery exist on each side.

Zoological position of the Engé-ena.

With the knowledge of the anthropoid animals of Asia and Africa which now exist, derived from the critical examinations of their osteology, their dentition, and the comparative size of their brains, by various observers, especially Geoffroy, Tiedemann, Vrolik, Cuvier, and Owen, it becomes quite easy to measure, with an approximation to accuracy, the hiatus which separates them from the lowest of the human race. The existence of four hands instead of two, the inability to stand erect, consequent on the structure of a skeleton adapted almost exclusively to an arboreal life, the excessive length of the arms, the comparatively short and permanently flexed legs, the protruding face, the position of the occipital condyles in the posterior third of the base of the skull, and the consequent preponderance of the head forwards, the small comparative size of the brain, the largely-developed canines, the interval between these last and the incisors, the three roots to the bicusped teeth, the laryngeal pouches, the elongated pelvis, and its larger antero-posterior diameter, the flattened and pointed coccyx, the small glutæi, the smaller size of the lower compared with the upper portion of the vertebral column, the long and straight spinous processes of the neck,—these, and many other subordinate characters, are peculiarities of the anthropoid animals, and constitute a wide gap between these and the most degraded of the human races, so wide that the greatest difference between these last and the noblest specimen of a Caucasian, is inconsiderable in comparison.

Whilst it is thus easy to demonstrate the wide separation between the anthropoid and the human races, to assign a true position to the former among themselves is a more difficult task. Mr Owen, in his earlier Memoir, regarded the *T. niger* as making the nearest approach to Man, but the more recently discovered *T. gorilla,* he is now induced to believe approaches still nearer, and regards it as " the most anthropoid of the known brutes."* This inference is derived from the study of crania alone, without any reference to the rest of the skeleton.

After a careful examination of the Memoir just referred to, I am forced to the conclusion, that the preponderance of evidence is unequivocally opposed to the opinion there recorded ; and, after placing side by side the different anatomical peculiarities of the two species, there seems to be no alternative but to regard the Chimpanzée as holding the highest place in the brute creation. The more anthropoid characters of the *T. gorilla* which are referred to by Professor Owen, are the following—

1. " The coalesced central margins of the nasals are projected forwards, thus offering a feature of approximation to the human structure, which is very faintly indicated, if at all, in *T. niger*."† This statement is applicable to all the crania which I have seen, and especially to the two crania described in this paper. Nevertheless, the extension of the nasals between the frontals, or the existence of an additional osseous element, is a mark of greater deviation from Man.

2. " The inferior or alveolar part of the premaxillaries, on

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* Op. Cit., Vol. iii., p. 414. † P. 393.

the other hand, is shorter and less prominent in *T. gorilla* than in *T. niger*, and, in that respect, the larger species deviates less from Man."* The statement in the first portion of this sentence is certainly correct, but a question mayb e fairly raised on that in the second. The lower portion of the nasal opening in the Engé-ena is so much depressed, especially in the median line, that the intermaxillary bone becomes almost horizontal, and the sloping of the alveolar portion takes place so gradually, that it is difficult to determine where the latter commences, and the nasal opening terminates, and in this respect, it deviates much farther from man than *T. niger*.

3. "The next character, which is also a more anthropoid one, though explicable in relation to the greater weight of the skull to be poised on the atlas, is the greater prominence of the mastoid processes in the *T. gorilla*, which are represented only by a rough ridge in the *T. niger*."†

4. The ridge which extends from the ecto-pterygoid along the inner border of the foramen ovale, terminates in *T. gorilla* by an angle or process answering to that called "styliform" or "spinous" in Man, but of which there is no trace in *T. niger*.‡

5. "The palate is narrower in proportion to the length in the *T. gorilla*, but the premaxillary portion is relatively longer in *T. niger*."§

These constitute the most important, if not the only, characters given in Professor Owen's Memoir, which would seem to indicate that the Engé-ena is more anthropoid than the Chimpanzée, and some of these, it is seen, must be received with some qualification.

If, on the other hand, we enumerate those conditions in which the Engé-ena recedes farther from the human type than the Chimpanzée, they will be found far more numerous, and by no means less important. The larger ridge over the eyes, and the crest on the top of the head and occiput, with the corresponding development of the temporal muscles, form the most striking features. The intermaxillary bones articulating with the nasals, as in the other Quadrumana and most

* P. 39. † Op. Cit., p. 394. ‡ P. 395. § Ibid.

brutes, the expanded portion of the nasals between the frontal,—or an additional osseous element, if this prove an independent bone,—the vertically broader and more arched zygomata, contrasting with the more slender and horizontal ones of the Chimpanzée, the more quadrate foramen lacerum of the orbit, the less perfect infra-orbitar canal, the orbits less distinctly defined, the larger and more tumid cheek-bones, the more quadrangular orifice with its depressed floor, the greater length of the ossa palati, the more widely-expanded tympanic cells, extending not only to the mastoid process, but to the squamous portion of the temporal bones,—these would of themselves be sufficient to counterbalance all the anatomical characters stated by Professor Owen, in support of the more anthropoid character of the Engé-ena.

When, however, we add to them the more quadrate outline of the upper jaws, the existence of much larger and more deeply-grooved canines, molars with cusps on the outer side, longer and more sharply pointed, the dentes sapientiæ of equal size with the other molars, the prominent ridge between the outer posterior and the anterior inner cusps, the absence of a crista-galli, a cranial cavity almost wholly behind the orbits of the eyes, the less perfectly marked depressions for the cerebral convolutions, and above all, the small cranial capacity in proportion to the size of the body, no reasonable ground for doubt remains, that the Engé-ena occupies a lower position, and consequently recedes farther from Man than the Chimpanzée.

It does not appear that any other bones of the skeleton have as yet fallen into the hands of any European naturalist. A description of some of the more important of them will be found in the Memoir above referred to,* in which it will be seen that there are two anthropoid features of some importance, which go to support the view advanced by Professor Owen, and these are the comparative length of the humerus and ulna, the former being seventeen, and the latter only fourteen inches, and in the proportions of the pelvis. This last is of gigantic size, and is a little shorter in proportion to its breadth than in *T. niger*.

* Boston Journal of Natural History, vol. v., p. 417.

While the proportions of the humerus and the ulna are more nearly human than in the Chimpanzée, those of the humerus and femur recede much farther from the human proportions than they do in the Chimpanzée, as will be seen by the following measurements—

	Humerus.	Femur.
Man, ...	15·0	18·5
Chimpanzée, . . .	10·9	11·0
Engé-ena, . . .	17·0	14·0

Thus, in Man the femur is three inches longer than the humerus; in the Chimpanzée, these bones are nearly of the same length; and in the Engé-ena, the humerus is three inches longer than the femur, indicating on the part of the Engé-ena a less perfect adaptation to locomotion in the erect position, than in the Chimpanzée.

Description of a Canine Tooth of a Male Engé-ena.—In only one of the crania of the male Engé-ena which I have seen were the canines remaining; and these were so much abraded that they had lost, to a great extent, their natural outline, and, consequently, their most striking and distinctive marks. In the females, as in the Chimpanzée and the Quadrumana, generally, the canines are much less elongated than in the males. Among the bones first sent to this country by Dr Savage, was a canine tooth, which I was not able to identify, until an opportunity occurred of comparing it with Professor Owen's descriptions of more perfect teeth. The crown is laterally compressed, the posterior edge being trenchant, and its base provided with a prominent tubercle, which is doubtless rendered more conspicuous by the wearing of the edge beneath it. On its inner surface the crown is impressed with two strongly marked grooves, which extend from the base nearly to its apex; and include between them a prominent rounded ridge. The following table gives the comparative measurements of two canines from the upper jaw of the Engé-ena, and one from that of the Chimpanzée. The figures in the first column relate to the tooth described above; those in the second and third to the measurements given by Professor Owen,* the measurements being in inches and lines.

* Trans. Zoolog. Soc. London, vol. iii., p. 395.

	T. gorilla.		T. niger.
Length,	2·8	2·8	2·0
Length of Crown,	1·3½	1·3	0·10½
Breadth of Base,	1·0	0·10	0·7
Thickness of do.,	0·7½	0·7½	0·5½

The following note from Dr G. A. Perkins to the author, dated Salem, October 15, 1849, confirms the statements made by Dr Savage, in his description of the habits of the Engé-ena, as to its ferocity, and the fact of its attacking human beings.

"The two crania were received from a person on board a vessel trading in the Gaboon and Danger Rivers, W. Africa. They were obtained from the natives on the banks of the latter, by whom they had been preserved as trophies. From the gentleman who gave them to me, I learned that the killing of one of these animals was by no means a common occurrence. He describes the animal as being remarkably ferocious, even attacking the natives when found alone in the forests, and in one instance which fell under his observation, horribly mutilating a man who was out in the woods felling trees to burn. His shouts brought to his aid several other natives, who, after a severe contest, succeeded in killing the Engé-ena. The man was afterwards in the habit of exhibiting himself to foreigners who visited the river, and of receiving charity from them."—(*American Journal of Science and Arts*, Vol. ix., No. 25, p. 34, Jan. 1850.)

Agriculture and Chemistry.

[The following observations on the bearing of Chemistry on Agriculture, although in opposition to prevalent opinions on the subject, yet, being from the highest British authority, cannot fail to interest agriculturists and chemists.]

No one should be taught to undervalue the services which the sciences may render to agriculture and all the arts. In the history of the arts there are two periods, in one of which results only are regarded, and common experience depended upon; another, in which principles are investigated and results explained, and the resources of one branch of knowledge made to contribute to the advancement of another. This is the period in which science, properly so called, is brought to improve the arts; and this is the period in which the arts are enabled to achieve their most memorable triumphs. Past generations, as well as we, could strip the flax of its fibres, and the cotton seeds of their covering, and weave them into raiment; but it was reserved for mechanical science to construct for these ends machines, so beautiful and wonderful, that they seem instinct with life. And have we not ourselves lived to witness inventions that may be termed the triumph of knowledge; and these continually multiplying, each discovery giving rise to a train of others? This is the result of science applied to the arts; and can we suppose that agriculture can be exempt from the like analogy, and fail to profit by the means by which so many other arts have been perfected? We should distrust the whole history of inventions, if we should come to such a conclusion. Agriculture has, indeed, peculiarities which modify, in manner and degree, the means by which the sciences can be made to react upon it. Being the first and most necessary of the arts, it was, we may suppose, the earliest cultivated, and has been the longest pursued, and so perhaps sooner brought to a certain degree of perfection than many others. From this cause, it seems to have been brought very early to a condition which even now we may wonder at. The Roman agriculture, as we know from authorities that have come down to us,—the Catos, the Virgils, the Columellas, the Plinys,—of former ages, was not inferior to that of the finest parts of modern Italy, and superior, certainly, to that of many parts of the British islands at the present day. The state of the same art in the countries of the East, where the habits of men do not change from age to age, as in the great empire of China, where scarcely anything that can bear the name of science is cultivated, evinces to us that agriculture had, like the sister art of gardening, been brought to a degree of excellence before the sciences, properly so called, had been applied to the investigation of principles. The use of the plough, the harrow, the spade, and the hoe, were known from the earliest times, and to every people emerged from barbarism. Our Roman instructors had really left us less to learn than many persons are aware of. They were familiar with the use and preparation of the most useful manures, whether mineral or derived from plants and animals; with the practice of sowing the cultivated crops in rows, and hoeing them during their growth, which many suppose to be a modern discovery; with the suitable modes of cultivating the plants yet most generally grown, with the exception of rice, which was derived from the countries of the East, and of maize, which has been derived from the New World. They were acquainted with the order in which the cultivated plants should follow one another on the same ground, which we call the rotation of crops; with the modes of preparing the summer fallow, and the green or fallow crops; with draining, irrigating, and other branches of rural labour. Or if we shall go farther back still, long ere the City of the Seven Hills had even a name, we shall find that the most necessary labours of the field were known and practised. In the ruined monuments of Egypt, we see, as fresh as if they were sculptured yesterday, the labours of rural life depicted to us. Agriculture being thus early pursued, it has probably left less for science to add to truths before known than almost any of the other useful arts. But let us not draw, from this fact, conclusions which the history of the arts themselves does not warrant. If the origin of agriculture was in the rudest school of practice, and if its subsequent advances have been made by mere additions to experience acquired, it may not the less be that its ultimate triumph shall be due to science.

Of the sciences, Chemistry seems to be that which has the most immediate relation to Agriculture. The nature of the soil, its composition and properties, and the relations between it and the plants which it produces; and the composition of manures, and their modes of action, and the best means of using them; seem to bring agriculture in an especial manner within the domain of chemical research. Further, chemistry has been applied with the happiest results to numerous arts, as to that of the metallurgist, the dyer, the bleacher, and others; and it were hard to be believed that chemistry, which has improved so many arts, can not be without benefit to that which is employed in cultivating the earth for food.

Chemists, however, it is to be regretted, set forth with more pretensions than their own knowledge of agriculture itself justified; and did not seem to be sufficiently aware of the difference between the processes of the laboratory and those of the field, and of the conditions and limitations under which conclusions from the results of the one must be applied to the practice of the other. They made an ample number of mistakes, and held out to the farmer an ample number of expectations, which could not be realised. The first book on the subject which attracted much attention in our country was the excellent one of Sir Humphrey Davy, which was received with the favour due to its illustrious author, the novelty of the researches, and the importance of the subject to which they related. Agricultural chemistry, however, as it has been since called—and which means simply the application of chemistry to agriculture, and not a peculiar kind of chemistry, as if we should speak of cast-iron chemistry, or calico-printing chemistry—was chiefly derived from Germany, where it had been received with extraordinary favour, and prosecuted with great zeal, so that now there is scarcely a German university in which there is not a professor for the purpose of teaching the application of chemistry to agriculture. Books and innumerable memoirs on the subject have issued from the ever-teeming press of that country; and if we are compelled to say that much has not yet been done to make the agriculturists of Germany better farmers, we must admit that this has not arisen from the want of zeal and learning on the part of their instructors. A good many years ago, a distinguished chemist of that country, one of the most distinguished indeed of his age—Dr, now, justly and to the honour of his sovereign, Baron Liebig—published a work, which was immediately received with singular favour in this country, where most people were as ignorant of what had been passing on the subject elsewhere, as if it had been taking place in the moon. The book was read everywhere, and everywhere extolled. The learned German, indeed, had a little overrated the state of chemical knowledge in England; and his work, though perfectly intelligible to any student of chemistry, was certainly not understood by nine out of ten of those who read and admired it amongst ourselves. But it was something new, and from a great authority; and the doctrines and discoveries which it professed to make known were announced in a tone so bold and confident, even beyond the ordinary precedents, that it is not to be wondered at if the country gentlemen and farmers of England believed that a new and golden era was about to dawn upon them. The learned chemist not only shewed, that neither the chemists of his own country, nor of any other, had understood anything about the matter before, but rated, in good set terms, the dunderheaded farmers themselves for their past ignorance, and gave them to understand that they had been all along groping like moles in the mud which they thought they had been cultivating, and had known nothing at all of what they had been thinking about all their lives, until chemistry had come to their assistance. He denounced farmyard muck as being quite unscientific; eulogised ammonia to the skies, as the only source of fertility, and dismissed some other sources of fertility, as existing, not in the soil, but in the imagination of chemists; and predicted that the time would come when farmers would get rid of their cumbrous apparatus of muck-wains, and, in place of the dirty material itself, would get silicates and phosphates, which they could manufacture for themselves. The thing took amazingly, and our honest countrymen were all eager to become "scientific farmers." Many, we may suppose, advanced so far as to get hold of some of the words of the language, before so strange to them. They could now call the glauber salts, which their doctors had so often made them swallow, sulphate of soda; the saltpetre, with which their cooks had so often powdered their beef, nitrate of potash; and the salt which entered into all their messes, chloride of sodium. The learned chemist followed up his victory with vigour. He announced a grand manure of his own compounding, which, of course, was to supply to the ground the precise quantity of silicates, phosphates, &c., which the growing crops took away from it, and so to supersede the lime, the marl, the bones, the guano, the rape-dust, which farmers had thought to be pretty good kinds of things, not to speak of the ill-used muck of former times. The patent was obtained; but, alas for the vanity of human hopes! the patent manure was laughed at in its own country, and found to be of little more use in this than the same quantity of sawdust. It was the fruitful mother, however, of an infinite number of infallible manures, each to have its day of infallibility, and then to be forgotten. The trade prospered, and has continued, though now somewhat on the wane, to the present time. The farmers had, fortunately for themselves, got guano in abundance, and which, though no more a chemical discovery than the muck of their forefathers, was found to be tolerably efficient for their purpose; and the farmers really prospered, notwithstanding the frequent failures of the infallible fertilisers presented to them on every hand.

But this was not all: the soil was to be analysed, and no farmer was to presume to fancy that he could cultivate it until an agricultural chemist told him what it consisted of. Analyses of soils accordingly prospered, the farmers received and paid for the documents, with wonderfully little profit, it may be believed, to the soils themselves. The Government of the time was solicited by two great agricultural societies —one in England and one in Scotland—to make a grant of money from the then not very thriving Exchequer, for getting all the soils of the kingdom at once analysed. Government, it is believed, was rather anxious to oblige so many influential applicants, and commence the great work of analysing all the soils of Great Britain. Lord Althorpe, however, then Chancellor of the Exchequer, resolved, contrary to the usual precedent, to consult first some persons who might chance to know something about the matter. He learned that to analyse *all* the soils of the country was a hopeless affair, even with the help of an army of chemists; that to analyse any great number of them would probably require a century or so; that the work would need to be begun again and again, whenever a soil was changed by improvements made upon it; that the analysis of a soil was not quite such a simple matter as a country gentleman supposed it to be; that about a dozen only which deserved the name of chemical analyses had as yet been made in all the laboratories of Europe; that the great mass of those which had been given to farmers, as something essential to their practice, were nearly as useless to them as the same quantity of blotting paper; and, finally, that Government might safely leave this affair to individuals, and find fifty better ways of laying out money for the improvement of agriculture, than analysing all the soils of the country. Nothing daunted, however, the scientific agriculturists of the day resolved that farmers should be taught to analyse soils for themselves: in England, schools were established for the purpose; and in Scotland the plan was so far matured, that it was resolved, that the teachers of parish schools, in addition to their multitudinous duties, be made to teach agricultural chemistry to the country boys. It seems to have been overlooked, that the persons least fitted to teach agricultural chemistry in the capacious laboratory of a village schoolroom, would probably be found to be the schoolmasters, and that there were some such things to be taught in parish schools, as reading a little, writing a little, and casting accounts; not to speak of a little geometry, if time could be found for it, &c; that, to acquire any tolerable knowledge of such a subject as chemistry, requires a pretty tolerable proportion of a youth's time; and that twenty times the knowledge of chemical analysis that a country youth could acquire, at a village school, would not enable him to analyse a single soil; and, that, after all this miserable scum of knowledge has been thrust upon the poor boy, he should be no better fitted for being a farmer than before. The notion, in short, that agricultural chemistry was to enable us to reap golden crops was spread everywhere, nay, to our distressed colonies. The planters of one of these did me the honour to consult me about sending out an agricultural chemist, to help them to cultivate their plantations, and retrieve their affairs. It would have been uncourteous to reply, by recommending them to take out an agricultural fiddler, who would be equally useful and more amusing. To farm scientifically, and by the rules of chemistry, became, in short, a fancy everywhere cherished, and not yet altogether sobered down by experience. There is something taking, indeed, in the idea of being a man of science; but when some of us have heard honest country friends of our own talking of their science, and of the great benefits it was to confer on their home-farms and tenants, we may have been charitable enough to wish that their occupations would allow them to go back to school, and learn a little of science itself. I have rarely been able to get from our scientific country gentlemen any very satisfactory definition of this somewhat comprehensive term, science; much less to get a sight of that uncomfortable sort of monitor, called the balance-sheet, to learn how their science had worked with them in the humble matter of pounds, shillings, and pence. They seem to have had a sort of notion that all science was agricultural chemistry, and that it was a mighty good thing for raising rents and great crops of potatoes; and that, if they could but get their tenants to analyse their soils, these tenants would be able to cultivate them a great deal better. The farmers, too, were generally little behind their landlords in the matter of science, however little relishing the practical application of it to the tender matter of rents. Some of them seemed to think that guano was science; and a very good science I can tell them it is; and better by far than one half of what they heard on the subject. Why, we are all men of science in a certain way. Our dairymaids are persons eminent in science. They can make cheese; and the making of cheese is a tolerably complicated chemical process. It has puzzled all the chemists till our own day to explain it. They think they have nearly settled the question now; but we may back the dairymaids still against them all, in the really useful part of the matter,—the making of the cheese itself. Our cooks are eminent chemists; and there is not a sirloin cooked, or a pudding manufactured, that does not involve a vast variety of chemical changes. One of the most learned chemists of the day, before mentioned, Baron Liebig, has written profoundly on this savoury subject. He has found out that if we boil the beef, and throw away the broth, there will be a waste of nutritive matter; and that it is a very good thing in roasting to preserve as much of the gravy as possible. It had been discovered that the fat is in the grass before it is in the goose that eats it, and straightway the Baron discovered what precise part of our turnips and potatoes goes to form the fat, what the flesh, and what the other multifarious products of the stomach. But let not our cooks lose heart; they will beat Baron Liebig to nothing in preparing a comfortable dinner for us, which, after all, is rather an important part of the affair. And let our farmers in like manner be comforted. They are very good chemists, if they would but think so, and perform every day, in their fields, better chemical operations by far than all the agricultural chemists can perform for them.

This somewhat excessive zeal, however, has really had useful results. It has directed the attention of many country gentlemen to agriculture, as a liberal as well as a useful pursuit, and induced them to pay increased attention to the improvement of their estates. It is the chemists only who are to blame, for having addressed themselves to farmers in a more authoritative manner than their own knowledge of the subject warranted; and for having held forth expectations which could not be realised in the manner which their boasting had led the people to expect; the effect of which has been to give practical farmers a distaste for what is called agricultural chemistry, and to retard, rather than promote the beneficial application of the discoveries of science to the practice of the farm. But a more serious offence of theirs remains to be noticed, which is, the holding out to the farmers of the country this agricultural chemistry as a means of supporting them in their present adversity, and enabling them to surmount the difficulties in which they are involved. We have seen how ready politicians, struggling against their own growing convictions, and anxious to justify their past proceedings, are to catch up this cry of agricultural chemistry, and to tell the farmers of the country what science has done for them and what it is likely to do. What true science is likely to do, we do not know; but what agricultural chemistry has yet done we do know, and thus can appreciate it, as a means of enabling the farmers of this country to bear up under the evils under which they suffer, and the dangers which menace them.

Agricultural chemistry, so called, has explained, or attempted to explain, effects long before observed, and familiar in the practice of good farmers; but it has not taught the farmer to till his land better and more cheaply, to drain it better, or to clean it better: it has not added one plant to those before cultivated, nor taught the farmer to cultivate one crop better than he could do before. It has not taught him to employ one mechanical machine more usefully; nor taught the mechanics who construct these machines to adapt them better to the uses of the farmer. It has not added one manure to the list half so important as those before known, such as lime, marl, gypsum, bones, rape-dust, soot, ashes, the refuse of towns and manufactories, guano, and it may be said, any one of the animal and vegetable substances which had been before familiar. These were all in use, and the times and modes of applying them had been determined by farmers themselves, before agricultural chemistry was heard of. Or if we allow that some additional compounds have been added to the former ample list of such substances, no one surely will say that this has affected in a sensible degree the condition and prospects of the farmers of this country. Even the use of the alkaline salts, of which so much has been said, was known to the degree in which it was thought beneficial to employ them. Saltpetre, which may be regarded as the type of the class—the type, I mean, as regards its effects upon the soil—has been used by the farmers of England for more than a hundred years, either by itself, or in the refuse matter of gunpowder works. They had learned that, though a powerful stimulant, it did not add that permanent fertility to the soil which it is the great end of good farming to communicate. A similar substance, nitrate of soda, which can be obtained in unlimited quantity from a vast deposit of it in South America, and which can be imported at a cheap rate, was employed by farmers several years ago, in large quantities; but the use of it has now almost ceased, which could not have happened had the employment of it been found very advantageous. But if, in the matter of manures, which, of all others useful to the farmer, is the subject which chemistry is best fitted to investigate, more has not been done than we know to have been done, how can we be so thoughtless as to hold out agricultural chemistry to farmers, as the means of aiding them in times like these, or affording them any hope on which they ought to be called upon to rely in the times that are to follow? Yet we shall doubtless again hear statesmen, who ought to be careful how they hold out delusive expectations to the farmers in such times as the present, speaking of the benefits which agricultural chemistry has conferred upon agriculture, although they themselves know nothing more of the matter than what they hear others, not very competent to give them information, state. Besides, even if agricultural chemistry had done what they suppose it to have done, but which any practical farmer knows it has not done, cannot other countries practice agricultural chemistry as well as we can do? It appears that there are amateur farmers, or scientific farmers as they call themselves, reverend doctors, and others, who are now hastening forward to announce to Government the great things now done by means of agricultural chemistry. They have doubled the number of their stacks, they tell us. Why, they might have done that without the help of chemistry at all. Every farmer knows that, by a large expenditure, he can increase the produce of his farm. But the farmer is compelled to compare the gain with the expenditure, and to limit the expenditure to what will afford him a return. No tenant-farmer can farm, as these reverend gentlemen propose, on a system of experiments. He must farm according to experience already acquired; and experiments must be the exception, and not the rule, of any well-ordered farm. A tenant-farmer could not afford to farm for a single season on a system of experiments; and, were he to farm as some of these reverend gentlemen are doing, he would probably soon cease to be able to farm at all. These reverend gentlemen, to make their example worth quoting, should furnish us with the only document which can tell whether they have farmed well or ill,—namely, their account of profit and loss. But we may return to say, without even having seen these instructive documents, that not one in ten of these gentlemen has been paying half the expenses of his farm, not to speak of rent and profits of his capital in trade. (*Professor Low's Appeal to the Common Sense of the Country.*)

α Centauri, and the Absolute Size of the Fixed Stars. By Piazzi Smyth, Professor of Astronomy in the University of Edinburgh, F.R.S.E., &c. Communicated by the Author.

The absolute size of the fixed stars, and the place of our sun amongst them, has been, from the first ages of astronomy, a question which has excited the keenest inquiry and the most extended speculation, but has always baffled the search. Many, however, even at an early period of the science, flattered themselves that they had determined both the distance and the size of those bodies; but they were invariably found to be wrong by the succeeding age, which again, in its turn, flattered itself with having arrived at the great desideratum, and was in its turn disappointed. Absurdly close and miserably small were the stars made at first, before the development of science enlarged men's minds and their powers of contemplation and perception; but every successive determination gradually expanded the bounds, until at last, when true methods of philosophy were adopted, it was confessed that the distance of the stars was so great as to be utterly immeasurable by our best instruments; while of their absolute size, from that reason, in addition to their not presenting any visible disc, no guess even could be made.

In this unsatisfactory state the question long remained, the solution being constantly attempted the while, but never with success, until at length, a few years since, Professor Henderson, from his observations at the Cape of Good Hope, determined the distance of α Centauri, and Bessel that of 61 Cygni. The results they arrived at, are now conclusively received by all astronomers; and those two stars are still the only two of which the distance from our system is certainly known. The fact of the great barrier, which had obstructed our excursions into the realms of space, having actually been overleapt in two points, was rapturously received as an earnest and a prelude of its being soon passed at many more, and before long being broken down altogether: and such a spirit there was amongst astronomers; but it has not yet been productive of the expected consequences. M. Peters, certainly, of the Pulkowa Observatory, fancies that he has determined the parallax of a number of stars, and can state the average parallax of stars of the first, second, and other magnitudes ; while M. Faye, of the Paris Observatory, supposed that he had found the star, No. 1830 Groombridge, to have a larger parallax (*i. e.*, a closer distance) than α Centauri. But this result has been completely overthrown by subsequent, more extended, and accurate observations by M. O. Struve at Pulkowa Observatory, and Mr Main at the Greenwich Observatory; the former making it only 0″·03 at most, and the latter finding it something less than nothing, according to the rigid interpretation of his observations, while M. Faye's quantity was 1″·08. Then M. Peter's parallaxes having been arrived at by means of a meridian instrument, and being also very small, so as to be barely within the power of such means, never commanded much confidence; and more, they have had a further doubt thrown on them by Mr O. Struve's admirable observations with the great equatorial of the same Observatory, and by the Greenwich Observatory; for the star which they have very satisfactorily determined to have an insensible parallax, or certainly not more than ″·03, M. Peters had attributed so large a quantity as ″·25. α Centauri and 61 Cygni thus remain the only hold that we have on the whole of the sidereal system; and, as they are both double stars, they will give the means of estimating the mass or weight of the components of the system as compared with our sun, when the orbits of the stars round their mutual centre of gravity shall have been well determined.

To this end, the former object presents by far the more favourable opportunity ; first, from its very much closer distance to us, and the consequent greater angle that the orbit is seen under; second, from the period of the orbit being very much shorter, so that a determination may be obtained much sooner ; and, third, from the stars being not only really larger, thence exhibiting a greater amount of measurable attraction; but apparently longer also, and therefore visible and observable in the day as well as by night, and so continuedly throughout the year ; as well as to our having much earlier micrometrical observations of it than of the other.

And further, it claims the more pressing attention just now, as the perihelion passage of the small star is close at hand; and, on the correct observation of the phenomenon at that particular epoch, all exact determination of the great desiderata previously mentioned, will depend.

As, however, another opinion has been published, with regard to the nature of the orbit of α^1 and α^2 Centauri; and, if observers are guided by that, and it should not prove to be correct, all this important part of the orbit which seems to be impending, may be lost to us; and near a hundred years must elapse before another equally favourable opportunity occurs, it becomes of importance to examine into the exact particulars, and see what degree of probability is to be attached to either hypothesis.

For the first part of the question, I may refer to a notice which I had the honour of reading before the Royal Society of Edinburgh, on April 5, 1848.

The star α Centauri, situated in $14^h\ 29^m$ A. R., and 150° 12′ N.P.D., is in many respects a notable object, and though its greatest claims to attention have all arisen within the last few years, under the applications of the advanced astronomy of the present day, yet even to the naked eye it has much to raise it above the general crowd. It is a star of the first magnitude, and one of the brightest indeed of that class, and is situated in a peculiarly splendid region of the sky, the same as that occupied by the Southern Cross; a constellation, by the way, which, on account of its small dimensions, and the few stars it contains visible to the naked eye, is by no means entitled to the too warm encomiums so lavishly bestowed upon it by the early Southern navigators and travellers. The *region* of the Cross, however, abundantly compensates for the poverty of the constellation itself; for such is the general blaze of star-light from that part of the sky, that a person is immediately made aware of its having risen above the horizon, though he should not be at the time looking at the heavens, by the increase of general illumination of the atmosphere, resembling the effect of the young moon.

This excessive splendour is caused not only by the profusion of first, second, and third magnitude stars in the neighbourhood, but by the extraordinary general breadth and brightness of the Milky Way thereabouts; for, separating into so many distinct luminous clouds, as it were, and exhibiting between them void black spaces unchequered by a single luminous object of any kind whatever, it forcibly impresses the idea of our being situated there near the confines of the sidereal system, or in the southern side of the vast ring in which the generality of the stars are arranged. The superior brightness of so large a proportion of the stars is then naturally accounted for by their greater proximity to us; and this fact was actually *proved* by my predecessor, who found from his own observations of α Centauri, an annual parallax of the large amount of 1″, *i. e.*, that at the distance of this star, the radius of the earth's orbit, or 95 million of miles, subtended an angle of 1″; the greatest quantity previously found for any star in the Northern hemisphere being only 0·23″.

Professor Henderson's results were fully confirmed by a very much longer series of observations subsequently made at the Cape Observatory by different observers, and with different instruments, and he then computed his old observations of the other principal stars in that region, and finding a considerable number* which shewed also indications of a sensible parallax, he immediately sent out a notice of the results to the present energetic Director of the Cape Observatory, for the purpose of procuring from him a greater number of observations of those suspicious stars. Such a series was accordingly commenced, and is still going on, and we may expect before long to hear of trustworthy results having been obtained, and there is little doubt that these labours will still more strongly tend to establish the proximity of that part of the sky.

On the application of the telescope to α Centauri, it proves to be composed of two stars, one very much brighter than the other, but still both may be placed in the list of first magnitude, the smaller occupying the lowest possible step in that grade. Early observers have indeed assigned it a much smaller rank, and in the British Association Catalogue published only two years ago, and intending to apply to the year 1850, it is actually made as low as the fourth magnitude; this, however, is manifestly an error, for the present epoch, as I can state from the experience derived from making the observations which served to confirm Professor Henderson's parallax; for, during the whole year, there was not a single day when, if the larger star was seen at all, the smaller one was not abundantly visible also; and during that part of the year when they transited the meridian by daylight, they were even then invariably seen with the mural circle telescope, whatever the state of the atmosphere, unless actual clouds intervened. But that the smaller star was never in ages past as low as the fourth magnitude, the marvellous change which has occurred in the case of η Argus in our own times, would render a most hazardous assertion.

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* β Hydri.	η Argus.	ι Centauri.
α Phœnicis.	α Crucis.	α Trianguli Austr.
α Eridani.	γ Crucis.	β Trianguli Austr.
α Columbæ.	β Crucis.	α Pavonis.
ι Argus.	β Centauri.	α Gruis.

A proper motion of the large amount of 3·58″ is participated in by both the stars, a fact which pretty clearly proves a physical connection between them; for while they are now very nearly in the position they were in 100 years ago, when observed by the Abbé Lacaille, they would have separated by this time upwards of five minutes, if one only was pursuing this anomalous path amongst the rest of the stars.

The first person to remark on this physical connection was Professor Henderson, who, in the concluding paragraph of his memoir on the parallax, says,

"The two stars appear to be approaching each other. The earliest observations of α Centauri made with a telescope which I have found, are those of Richer at Cayenne in 1673, but neither he nor Halley, who observed it at St Helena in 1677, mentions it as being double. Their telescopes were of course anachromatic, and probably not of much power. Feullée appears to have been the first person who observed the star to be double, as he mentions in the Journal of his Voyage in South America in July 1709. La Condamine next observed the star during the scientific expedition to Peru for measuring an arc of the meridian." But neither of them made any observations of real service in determining the nature of the physical connection of the two stars. "From Lacaille's observations in 1751–2, the distance of the two stars appears to have been then 22·5″. Maskelyne, who observed them at St Helena in 1761, says (*Philosophical Transactions*, 1764, p. 383): The bright star in the foot of the Centaur, marked α in the catalogues, when viewed through a telescope, becomes divided into two stars, one of which is about the second and the other the fourth magnitude. They were both observed by the Abbé De Lacaille. I found their distance by the divided object-glass micrometer, fitted to the reflecting telescope, to be 15″ or 16″. I have not found any observations," continues Professor Henderson, "of the distance of the two stars made between 1761, and the institution of the Paramatta Observatory: there, in the end of 1825 or the beginning of 1826, the distance was observed to be 23″ (*Memoirs of Astronomical Society*, Vol. iii., p. 265), since which time it has been decreasing at the rate of more than half a second *per annum*. The angle of position scarcely appears to have changed since Lacaille's time, whence it may be inferred, that the relative orbit is seen projected into a straight line or very excentric ellipse; that an apparent maximum of distance was attained in the end of the last or the beginning of the present century; and that about twenty years hence the stars will probably be seen very near each other, or in apparent contact, but the data are at present insufficient to give even an approximation to the major axis of the orbit and time of revolution."

The next authority on the subject is Sir John Herschel, who specially applied himself to the subject of the Southern double stars when at the Cape, and had far superior instruments for such a purpose to any of his predecessors; he thus describes and sums up all that was known to him of this star, in his recently-published work.

"This superb double star, beyond all comparison the most striking object of the kind in the heavens, and to which the discovery of its parallax by the late Professor Henderson has given a degree of astronomical importance no less conspicuous,—consists of two individuals, both of a high ruddy or orange colour, though that of the smaller is of a somewhat more sombre and brownish cast. They constitute together a star which to the naked eye is equal or somewhat superior to Arcturus in lustre." After describing the magnitude which he considered should be assigned to each, and which agrees more nearly with what I have already stated as being my own opinion, and after giving some optical and physiological reasons which may tend to explain the under-estimation of former observers,—Sir John then cites the fact of the remarkable amount of proper motion of the stars, and says, "This consideration alone suffices to decide us in admitting a binary connection between them, and it will therefore be interesting to see what evidence observation furnishes of orbitual motion round their centre of gravity. For this, however, the data are somewhat precarious, as we have, until recently, only catalogued differences of A.R. and Polar distances, from which to calculate the angle of position and distance at the epochs of observation. This done, and the results tabulated, together with my own positions and distances, obtained by direct measurement with the equatorial, we have as follows:"—

Authority.	Epoch of Observation.	Position.		Distance.
		°	′	
Lacaille,	1750	218	44	20·51
(Maskelyne,	1761			15·5)
Fallowes,	1822	209	36	28·75
Brisbane,	1824	215	25	22·45
Dunlop,	1825	213	11	22·45
Johnson,	1830	215	2	19·95
Taylor,	1831	215	58	22·56
Herschel,	1834–68			17·43
	1834–79	218	30	
	1835–86	219	30	
	1837–34	220	42	
	1837–44			16·12

I have inserted here the observation of Maskelyne in 1761, with which, probably, Sir J. Herschel was unacquainted; it makes an apparently bad figure among the rest, but is by no means to be left out on that account merely, seeing the care and the superior means for that day with which the measures were made.

" Mr Fallowes' determinations," continues Sir John, " in this series, are open to objection, from the decidedly inadequate instrumental means by which they were furnished (a small altitude and azimuth circle). Mr Taylor's results also rest on so few observations, as to entitle them to little weight.

" Though it is obviously impracticable to deduce any elliptic elements from such a series, there are some features which it is impossible not to recognise. There can be no doubt that the distance has gone on decreasing since 1822 at least ; and the comparison of the measures least open to objection, leads us to conclude, that, for the ten years previous to 1838, the rate of decrease was $\frac{7}{13}$, or a little more than half a second per annum, which, if continued, will bring on an occultation, or exceedingly close appulse, about the year 1867. The small amount of variation in the angle of position shews that the plane of orbitual motion passes nearly, but not quite through our system, while its actual tendency to increase exemplifies the general law of increase of angular velocity, with diminution of distance. Mr Fallowes' distance is probably too great by 3″ or 4″ ; but in the long interval between 1750 and 1822 (at the former of which epochs the distance must have been on the increase), there is room for a very much greater excursion of the small star towards its apparent aphelion, so that, although we are sure that the major axis of the real orbit *must* materially exceed 24″, it is impossible to say *how much* it may exceed that limit. Taking, therefore, the co-efficient of parallax for α Centauri, as determined by Professor Henderson, at 1″, it will follow from what has been said, that the real orbit of one star about the other cannot be so small as that of the orbit of Saturn about the sun, and exceeds, in all probability, that of the orbit of Uranus.

" The plane of the orbit in the case of α Centauri, passing nearly through our system, my method of approximating to the elliptic elements becomes inapplicable, and for their determination, measures of the distance of the stars from each other can alone be relied on. No subject more worthy of continued and diligent inquiry can possibly be urged on the attention of southern astronomers."

Thus the result arrived at, both by Professor Henderson and by Sir J. Herschel, and which, though proved since to be erroneous, would have been probably concluded by any one else from the same data, seems to be, that the smaller star had been employed during the last century in gaining its aphelion, without any sensible change of angle of position. What the aphelion distance, the diameter of the orbit, and the period of revolution, might be, no guess could be attempted : but in his address, on the occasion of giving the gold medal to Bessel for his discovery of the parallax of 6′ Cygni, Sir John Herschel stated that the orbit of the smaller star of α Centauri might subtend the large angle of about 1 minute. As it had been actually observed at an elongation of 28″ on one side of the large star, the very reasonable supposition of a nearly circular orbit, seen in profile, would, in course of time, give the same distance on the opposite side. Both authorities also predicted the probability of an appulse of the same stars somewhere about the year 1867.

At the time of Sir John Herschel going to press, he knew of no micrometrical measures subsequent to 1838, but soon after that period, most fortunately for the interests of sidereal astronomy, Captain Jacob came into the field. On visiting the Cape from India, where he had been engaged in the great Trigonometrical Survey, he spent most of his time at the Observatory, and not only witnessed, but took part in the parallax observations of α Centauri. He then ordered a good achromatic telescope from Dollond, and on its arrival in India, after his return there, erected a small observatory, and devoted all his spare time with great perseverance and eminent success to that most difficult species of observation,—viz. the double stars.

About a year ago, he wrote to me to send him out all the old observations known of α Centauri, for the two stars were approaching more and more rapidly, and his own observations seemed to give a most unexpected orbit. The first document which reached him was Professor Henderson's memoir on the parallax, and then Captain Jacob found that he had been forestalled as to the actual facts of an appulse being shortly to be expected, though he indeed fixed the time as being very much closer at hand, bringing it from 1867 to 1851; but as to the idea that the small star had only been gaining its aphelion. without sensible alteration of angle of position since 1751,—he found, on computing the orbit, that within that interval it had made a whole revolution, or had altered its angle of position by 360°. The subsequent arrival of Sir J. Herschel's observations fully confirmed Captain Jacob's views, who has now recomputed the orbit, including all the known observations up to the present time; and though this performance is to be considered but a first approximation, still it will probably not be very much altered by future observations in any of the important elements.

The difficulty that might be started at the first mention of this new opinion, would be, that supposing the small star, instead of having remained almost stationary in its orbit for the last 100 years, to have really made a whole revolution,—how came it to pass that every observer in the interval saw it always in about the same position on the west, and never on the east of the large star? This objection is fully met by the extraordinary nature of the orbit, which turns out much more nearly like that of a comet than of a planet, the greatest distance being 21·85″, and the least 0·5″, in consequence of which, the small star moves with such surpassing rapidity at its periaster, actually 2° 40′ per day; that it is but a very short space of time on the eastern side of its primary, and when at its aphaster on the west, moves again with proportionate slowness, and so is seen there for a long period with hardly any sensible alteration of place. The time of revolution seems to be as short as 77 years; and Lacaille and Maskelyne's observations, which had before appeared somewhat anomalous, are fully reconciled, as belonging to a former revolution; indeed the small star seems to have been almost in precisely the same situation with respect to the large one when observed by Maskelyne in 1761, as it appeared to Sir J. Herschel in 1838; and had observations been continued for twelve years after Maskelyne's time, our knowledge of sidereal astronomy might have been almost a century in advance of its present position.

Captain W. S. Jacob's Observations of α^1 *and* α^2 *Centauri (A.R.* $14^h\ 29{\cdot}5^m$, *N.P.D.* 150° 12′*), made at Poonah, Lat.* 18° 31′ *N., Long.* $4^h\ 55^m\ 42^s$ *E., with a Five-Feet Achromatic Telescope.*

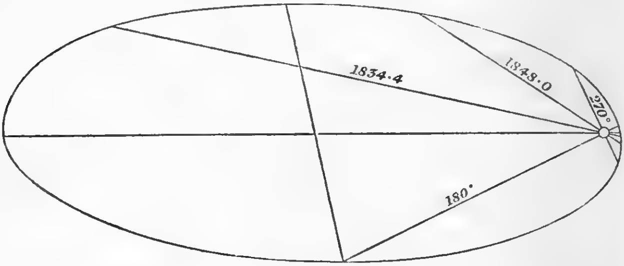
Angle of Position of the Two Stars.	Weight of Observations.	Number of Observations.	Magnifying Power.	Distance of the Two Stars.	Weight of Observations.	Number of Observations.	Magnifying Power.	Estimated Magnitudes.	Date.	Remarks.
230·1	8	4	87	11·03	9	4	87	1 – 2·5	1846, ·17	flaring.
232·7	15	6	87	11·03	4	4	87	1 – 3	·20	do.
233·3	13	5	152	11·09	5	4	152	1 – 3	·20	do.
232·5	13	5	87	10·70	12	4	87	1 – 3	·26	definition tolerable.
232·2	12	5	87	10·12	8	4	87	1 – 3	·61	daylight; dancing.
234·1	21	7	87	9·47	19	6	87	1 – 3	·65	daylight; definition tolerable.
234·8	12	5	87	9·40	11	4	87	1 – 3	·97	do.
236·0	12	5	87	9·27	8	4	87	1 – 3	·99	do.
236·4	18	5	87	9·67	14	4	87	1 – 3	1847, ·03	do. good.
236·0	17	5	87	9·30	18	4	87	1 – 3	·03	do. tolerable.
				9·35	14	4	87	1 – 3	·04	do. do.
234·7	12	5	152	9·00	10	4	152	1 – 3	·26	do.
233·6	12	5	87	9·47	11	4	87	1 – 3	·36	flaring.
234·0	14	5	152	9·64	11	4	152	1 – 3	·37	do.
235·9	16	5	152	9·81	13	4	152	1 – 3	·44	definition tolerable.
236·6	11	5	87	8·25	14	4	87	1 – 3	·93	daylight; definition tolerable.
237·1	12	5	87	8·25	8	4	87	1 – 3	·93	do. do.
238·0	18	5	152	7·96	13	4	152	1 – 3	·96	do. very good.
238·0	13	5	152	8·31	10	4	152	1 – 3	·97	do. do.
238·1	12	5	152	8·12	11	4	152	1 – 3	1848, ·00	do. fair.
238·1	18	6	152	8·03	14	4	152	1 – 3	·00	do. excellent.
238·2	19	5	152						·01	do. do.
238·0	21	5	152	7·95	16	4	152	1 – 3	·01	do. do.
238·3	18	5	152	8·03	14	4	152	1 – 2·5	·02	do. good.
238·1	21	5	152	8·07	14	4	152	1 – 2·5	·04	do. do.
238·0	17	5	152	7·78	14	4	152	1 – 2·5	·05	do. do.
238·3	19	5	152	7·89	16	4	152	1 – 2·5	·05	do. slightly tremulous.

Orbit of α^1 and α^2 Centauri.

Position of perihelion, . .	π =	26° 24′
Inclination to the plane of projection,	γ =	47 56
Position of ascending node, .	Ω =	86 07
Angular distance of perihelion from node on the plane of the orbit, . .	λ =	291 22
Excentricity, . . .	ε =	0·950
Epoch of perihelion passage, .	τ =	1851·50 year
Periodic time, . . .	P =	77·0 years
Mean motion, . . .	ν =	4°·675
Semiaxis major, . . .	a =	15·50″
	Mass =	$\frac{3}{4}$ of the Solar

Apparent Orbit.

Maximum distance, . .	= 21·85″ at 207·5°
Minimum, ...	= 0·50 ... 5·0
Greatest daily motion, . .	= 2° 40′



We have thus here altogether *one* of the most, if not *the* most, interesting and important sidereal system in the heavens; the only one which can compare with it is γ Virginis, and that has been looked upon as being amongst the double stars what Halley's comet is amongst comets; but though so well and frequently observed of late years, it was not instrumentally measured so early as α Centauri, and it is a much smaller star, with an orbit of only one-fourth the apparent dimensions, and a period of time double the length of its southern rival; so that, while the actual observation for the purpose of comparing theory with fact would be eight times more difficult in the case of γ Virginis, and loaded with eight times the probable error of observation, there is the further objection, that on account of the greater length of the period, but a small portion of the orbit could be determined by one observer, or even by one instrument.

But the crowning importance of the binary system of α Centuari, is the accurate determination of the parallax or distance from us, by the late Professor Henderson, as we are thereby enabled to speak, not only of the *proportions* of the different parts of the orbit, but of their *actual size*, and *the weight* of the two bodies. Thus the least distance of these two suns is only half that of the earth from the sun, or a little less than that of Venus, while the greatest distance is a little more than that of Uranus; and the mass of the two stars comes out three-quarters of that of our sun, their distance from us being 226,100 times our distance from the sun.

Well, therefore, may Sir J. Herschel have said, "that no subject more worthy of diligent and continued inquiry can possibly be urged on the attention of southern astronomers."

But the most interesting part of the orbit is still to come, viz., the periaster in 1851, and that this be well observed is indeed to be earnestly hoped, for the period will be an eminently crucial one; it proved so in the case of γ Virginis, imperatively requiring an excessive alteration in all the elements except one, as previously calculated; and in the case of α Centauri, the characteristic features of the orbit are of a much more violently marked nature, besides being represented altogether on a larger scale.

The extreme importance of obtaining an abundance of observations at that epoch may be further indicated by the mere statement, that it cannot yet be considered as fully *proven*, that the law of gravity extends absolutely unaltered to the most distant parts of the sky, and the only mode of proof open to us, is by observing the double stars. It is true that most of the orbits yet computed on the theory of gravity have turned out very near the truth, but still not quite so near, it must also be confessed, as could have been desired; and in the luciferous case of γ Virginis, every orbit that has been computed for it yet, has persisted in giving a minimum distance of not less than 0·5″, while observation at the time of the periastral passage made it certainly much smaller.

I do not, of course, by any manner of means, wish to express any doubt on these grounds as to the sufficiency of gravity to explain all the observed phenomena; a great part of the *onus*, or the whole of it, may rest on the excessive difficulty of the species of observation, and their inappropriateness for calculation in all ordinary manners, caused by the extreme roughness of even the very best procurable data; resembling, indeed, those of the comet of 1556, whose return, calculated on such wretched notices of its former perihelion passage, we have been looking out for in vain so long.

But whatever weight we may attach to the insufficiency of our observations and methods of calculation, it is always proper to draw a distinct line of demarcation between those things which are proved and those which are merely inferred, and not seek to enjoy a triumph before the victory has been decidedly achieved.

The above observations and calculations seemed to many abundantly convincing, but the illustrious author of the "Outlines of Astronomy," published last year, says, after speaking of 61 Cygni (849): "The data in the case of α Centauri are more uncertain. Since the year 1822, the distance has been steadily and pretty rapidly decreasing, at the rate of about half a second per annum, and that with very little changes in the angle of position. Hence it follows evidently, that the plane of its orbit passes nearly through the earth, and (the distance about the middle of 1834, having been $17\frac{1}{2}''$) it is very probable that either an occultation, like that observed in ϵ Herculis, or a close approximation of the two stars, will take place about the year 1867."

This result being so very different from his, Captain Jacob has re-examined his observations and calculations, and has endeavoured to deduce limits within which the elements of the orbit must lie; his results, contained in several letters, are nearly as follows, and are given pretty fully, as so much importance is to be attached, in astronomical inquiry, to the fixation of distinct limits, within which it may be predicted that an unknown quantity will certainly be found: this it was which formed so great a difference between the researches of Le Verrier and those of Adams, with regard to the new planet Neptune, and made the former carry so much more conviction to the minds of those to whom they were communicated:—

Since coming here (Madras)—writes Captain Jacob—I have got some rough measures of α Centauri (there not being proper instruments for the purpose), which give 244°·5 for the angle of position, and 6″·23 for the distance, at the epoch 1849–63; these render probable a later perihelion passage, with less excentricity and inclination than what I assigned, but the difference cannot be very great.

On looking at the whole observations to see what is really *known* on the subject; it seems this much. The enclosed slip represents the area described between 1825 and 1848 (scale 4″ to an inch), as actually observed; though allowing reason-

able error of observation, say 1° in position and 0″·5 in distance, yet this will not produce much variation. This then gives about 2 square seconds for the amount described annually. This quantity being carried back to 1750, it cannot be fitted even approximately into *any ellipse whatever*, so as to give a distance not exceeding 25″, and a position in the S.P. quadrant; it will be impossible, unless a revolution be allowed to intervene. The period is therefore limited at the widest to between 75 and 80 years (two revolutions cannot be admitted); and this again limits the area of the apparent ellipse to 150″ or 160″. The section being fitted into an ellipse of that area, the greatest possible value of minimum distance is found to be about 2″·5; and the *latest possible* time of nearest approach about 1854. If I could have got a decent measure within the last six months, I might have got closer limits.

With regard to the mass, the least possible value of the major-semi axis (true) is about 11″·5, which corresponds to a mass of 0·325; the upper limit is not so easy to fix, because the excentricity may be almost anything above 0·9, and consequently, the inclination and major axis may be very great; but it appears highly improbable that e is above ·975, in which case a might be about 1·98 and $m = 1{\cdot}73$; so that the mass cannot differ very widely from the solar.

I have also been able to arrive at same result with regard to the quantity of light emitted by α Centauri, as compared with our Sun, through means of some results arrived at on the intensity of solar light and the absorption of the atmosphere, during my employment on the Trigonometrical Survey of India.

On commencing work with heliotropes in 1837, I soon found that for long distances it was necessary to enlarge the aperture more than in the simple ratio of the distance, though such was Colonel Everest's practice; and before the end of the first season, I had found a scale of apertures for corresponding distances which afterwards needed very little alterations; but when finally corrected by subsequent years' observation stood as follows:—

Aperture.	Inches.	0·5	1·0	2·0	4·0	8·0
Max. distance.	Miles.	15	23	33	45	60

Our heliotropes were circular glass mirrors, 8 inches diameter, and for the smaller apertures, diaphragms were used between the heliotrope and observer. At the distances stated, the light was just visible to the naked eye in clear weather, and when seen over a *valley*; if the ray grazed near the surface, its light was much reduced. On one occasion, I employed a heliotrope at $6\frac{1}{4}$ miles, and used an aperture of $\frac{1}{6}$ inch, and found it rather brighter than my usual allowance, so that probably $6\frac{1}{2}$ or 7 miles would be the nominal distance for that size. This agrees well enough with the rest of the scale, but there is no need to employ a conjectural quantity; and if the rate of absorption be computed corresponding to the above, a very close agreement will be found; and the mean of the whole shews a loss of ·0610 in passing through one mile of atmosphere, with the barometer at 27·0 inches, that being about the average height at my stations. With barometer = 30·0 inches, the value will be ·0672, and the loss in passing from the zenith through a homogeneous atmosphere of 5·2 miles will be ·303, or only about one per cent. less than Professor J. D. Forbes' result (*Phil. Trans.*, 1842, Part 2); and as my air was considerably drier than his (mean humidity probably not much above ·30 instead of ·56), this will probably account for the difference.

Applying this, then, to the intensity of solar light, and eliminating the atmospheric absorption, the diameter of the object that shall be about as bright as Venus, comes out 0·0205 inches per mile, or in arc 0″·0668. Now, the object viewed being the reflection of the Sun from a *metallic* surface after passing twice through glass, by which at least $\frac{5}{9}$ would be lost; the diameter of the portion of the Sun which would be equally bright, is therefore 0·045″. Also, we never observed between 21^h and 3^h, consequently the Sun's altitude was always below 45°, at a mean it might be taken about 23°, where the loss of light is ·41 per cent. more than in the zenith; consequently there remains ·59, $\sqrt{59} = \cdot 77$ and $77 \times \cdot 45 = \cdot 35$; therefore the portion of the zenith Sun equal to Venus is 0·035″. Now, I do not know the ratio of Venus' light to Sirius, but shall not be far wrong in estimating it at 4·0; then the portion of Sun equal to Sirius will be 0·017″, and as Sirius is 4 × α Centauri, this last will be = 0·009″ of the Sun, or the Sun must be removed to 213,333 times its present distance to = α Centauri, or must have a parallax = 0·97″. This makes α Centauri, in fact, about $\frac{1}{9}$ brighter than the Sun. Sir J. Herschel makes the disproportion greater, but that is on the estimate that the Moon is only $\frac{1}{800,000}$ of the Sun, which I cannot but think too low, also. If I had the correct comparison of Venus with Sirius, our results might agree better; but, considering the rudeness of the method, it is a satisfaction to have come so near. Probably the zenith correction ought not to have been applied, as we seldom see Venus in the zenith; in which case the light of α Centauri would come out $=1{\cdot}67$ of the Sun.

Thus much, therefore, we may assume as already known regarding this interesting star, and, on full consideration of the whole of the facts, it certainly seems that all observers who have α Centauri above their horizon should lose no time in beginning their observations to determine the nature of the perihelion passage of the star; and we may then hope in a very short space of time to have much more exact results than any we at present possess on the various results which may be deduced from the observations; the importance of which will be still further increased by micrometrical measurements with extra meridian telescopes being carried on at the same time, to perfect the determination of the parallax.

Notice respecting a Deposit of Shells near Borrowstounness. By Charles Maclaren, Esq., F.R.S.E., &c.*

This deposit of shells is situated about a mile and a half west from Borrowstounness, where the Carse of Falkirk terminates in a strip of flat land a furlong in breadth. The shells are exposed in two openings, made in the soil to procure limestone for Mr Wilson's iron-works, and which have been subsequently converted into pools by unfiltered water. They are each about 300 feet in length, and from 20 to 30 in breadth. The bed can be traced in these openings along lines having an aggregate length of 1000 feet. Over all that space the shells form an unbroken stratum of very uniform depth (nearly three inches), and almost perfectly horizontal, as shewn by their parallelism with the surface of the water

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* Read before the Royal Society of Edinburgh, on 7th January 1850.

in the pools. The upper and under surface of the stratum (seen everywhere in section) form lines as straight as if adjusted by levelling, and their position in this respect corresponds perfectly with that of the adjoining beach, which is remarkably smooth and uniform, and declines only at the rate of 1 foot in 400. The shells are covered by a bed of dark-brown sandy clay, from two to three feet thick, and rest on a deposit of the same substance, which closely resembles the mud spread over the present beach. They are all of one species, the cockle, or *Cardium edule,* and of various sizes down to the most minute. Though mixed with a small portion of the clay which covers them, they lie so compactly, that they present to the eye the appearance of a layer of chalk nodules; and they are seen in myriads on the surface of the clay dug out of the pools, and piled up on the space between them. Very few of them are fractured, and the two valves are generally united. The pools reach within a few yards of the high-water line; but the number of broken shells seen on the beach shews that the bed had once extended farther northward, and that part of it has been cut away by the sea. We have here apparently a picture of what passes at the bottom of the sea in depths beyond our reach; a colony or settlement of the *Cardium edule* in its native seat, covering at least one, but probably several, acres. The bed is at present about the level of high water, or a little above it, while the natural abode of the cockle, according to Mr Broderip, is from the low-water line to a depth of 13 fathoms. The continuity of the bed, its regular level, its remarkable uniformity, its composition confined to a single species, and the state of the shells, which are generally entire, and have the two valves united, shew that they are in their native locality, and prove that they could only have been raised to their present elevation by an upheaval of the land. This upheaval must have been to the extent at least of 18 feet, which is the difference betwixt high and low water, but very probably it was twice as much; for evidence of a change of level to the extent of 30 or 40 feet is found along both shores of the Forth. Inundations of the sea, caused by storms, have been called in to account for such deposits, but in my opinion very inconsiderately. That a sudden and violent movement of the sea should sweep a bed of shells from its original locality, is intelligible enough; but that, while transporting them over some hundred feet or yards, it should preserve them unbroken, with the valves still united,—that the rushing water, instead of ploughing up the dry land it invaded, should smooth and level an area of more than an acre, then spread out the shells upon it with mathematical regularity, in an uninterrupted stratum of nearly uniform depth,—that, finally, it should cover them with a bed of clay two or three feet thick, and then withdraw;—these seem to me to be effects utterly irreconcilable with the known agency of floods. I would as soon believe that the West India hurricane, instead of levelling the planter's house, transports it *en masse*, with its walls, roof, and furniture all entire, from one end of a field to the other.

On the Waters of the Dead Sea. By Mr THORNTON J. HERAPATH, and WILLIAM HERAPATH, Esq., F.G.S., *President of the Bristol Philosophical and Literary Society, and Lecturer on Chemistry and Toxicology at the Bristol School of Medicine, &c., &c.**

The Dead Sea, or as it is called by the Arabs, *Bahr Lout* (Lot's Sea), though somewhat insignificant in size, has, nevertheless, in consequence of the extraordinary physical character of its waters, and the awe and mystery which ancient tradition has thrown around its history, attracted the attention of mankind from time immemorial. Under the several appellations of the "Salt Sea," (Num. xxxiv. 3; Deut. iii. 17; Josh. xv. 5); the "Sea of the Plains," (Deut. iv. 49); and the "East Sea," (Ezek. xlvii. 18; Joel ii. 20), frequent mention of it is to be met with in the Holy Scriptures; and, in fact, it is now supposed to occupy the site of the cities of Sodom and Gomorrah, the destruction of which, by the wrath of the Almighty, is so graphically described in the eighteenth chapter of Genesis. In the works of the Greek and Roman

* Extracted from the Memoir on the Waters of the Dead Sea, in No. VIII. of the Quarterly Journal of the Chemical Society of London.

authors, again, it is often referred to by the name of "Lacus Asphaltites," or the "Bituminous Lake," and many remarks upon the exceeding saltness of its waters, and the sterility and desolate aspect of its shores, are to be found in the pages of Tacitus and Pliny.*

The lake itself, as is well known to every person acquainted with geography, is situated in the south of Palestine, at no great distance from Jerusalem, and is principally supplied by that venerated stream, the Jordan. Its breadth, it would appear from a recent survey, undertaken by Messrs Moore and Beke, in 1837, is about nine miles, and its length, according to the same authorities, is thirty-nine or forty miles. The latter, however, is found to vary considerably at different times of the year, according to the extent of the influx derived from the Jordan and other tributary rivers.† The bottom these gentlemen found to be rocky and of very unequal depth, ranging 120, 180, 240, and even 480 feet, all within the distance of a few yards. With regard to its geological situation, the lake lies in a deep basin, of an irregular oblong figure, and is surrounded by steep cliffs of naked limestone, which, on the western side, run up to the height of 1500, and on the eastern to 2500 feet above the level of the water.

On the surface of the sea, there is often found floating an immense quantity of asphaltum, which is generally carried by the influence of the wind to the western and southern shores, where it is carefully collected by the Arabs, who use it as pitch and sell it for medicinal purposes. It was this substance which seems to have been employed in ancient times, by the Egyptians, to a very great extent, for embalming bodies. There are also several mines of sulphur and rock-salt in the sides of the mountains on the western coast, which not only afford supplies of those useful articles to the Arabs, but even to the inhabitants of the Holy City. Indeed, many travellers have stated that the remarkable saltness of

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* Tacit. lib. v. Hist. cap. vi.; Strabonis Geogr. Plinii, lib. v. cap. xv. and xvi.; see also vol. ii., p. 1107.

† It is more than probable that its dimensions have become contracted in modern times, as, if we may believe Josephus, at the period when he wrote, it was 72 miles long, by 18 broad.

the waters is principally occasioned by the existence of similar saline formations at the bottom of the sea. So deeply, in fact, is the surrounding soil impregnated with this ingredient, that few or no vegetables will grow there, and it is from this circumstance, combined with the absence of all animal life,* either in the waters or on the shore, that recent travellers have conferred upon the lake the name "Mare Mortuum," or the Dead Sea.

The water, like that of the sea, is stated to be of a deep blue colour, shaded with green; but it is considerably more salt, and intolerably nauseous and bitter to the taste. Rae Wilson, who wrote some years ago, describes it to be not unlike the Harrowgate waters in taste and smell, but more disagreeable; although it approached more closely in character to bilge-water. Its specific gravity is so great, that it is almost impossible for a man to sink in it; persons who are entirely unacquainted with swimming, can lie or swim in it with the greatest ease. Josephus relates that the Emperor Vespasian, for the sake of an experiment, caused certain men to be thrown into this sea, with their hands and feet bound with cords, and they floated on the surface.

Bathers in this lake, however, experience a curious sensation of the eyes, which has been described by Mr Legh as temporary blindness; and upon getting out of the water, evaporation proceeds only very slowly, leaving a thick, oily incrustation of salt adherent to the skin, which remains for many days, as it is impossible to remove it completely, even by repeated ablution.

Notwithstanding, however, that the most obvious peculiarities of the waters of the Dead Sea have been known and recognised for many ages, it has only been in comparatively modern times that scientific men have attempted its chemical examination. Within the present century, Lavoisier, Marcet, Klaproth, Gay-Lussac, Gmelin, and Apjohn have each analysed it.

The celebrated Lavoisier experimented upon it in conjunc-

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* Ehrenberg, as stated at page 188 of this volume of the Philosophical Journal, proves that living infusorial animals occur in the Dead Sea.—(*Edit. Edin. Phil. Journal.*)

tion with his no less renowned countrymen MM. Macquier and Sage,* in the year 1778 (*vide* Table). At that early period, however, analytical chemistry had not attained to such a degree of accuracy as that of which it is now susceptible, and consequently there is little or no doubt but that they must have overlooked many of the most important constituents. The same remarks apply to all of the three analyses which follow next in the series; namely, that of Dr Marcet in 1807, of Professor Klaproth, and of M. Gay-Lussac in 1818. The former of these analysts was, moreover, inconvenienced by the smallness of the quantity which he operated upon, which did not amount to more than an ounce and a half.

The great differences which are to be observed in Professor Klaproth's numbers (*vide* Table) as compared with those obtained by the other two experimenters, according to Dr Marcet, are occasioned by that chemist having employed too low a temperature for the purpose of desiccation.

The last two analyses of these waters that have been published, are those by Professor Gmelin of Tübingen, and by Dr Apjohn of Dublin. The former appeared in the year 1826, and the latter in 1837; they are given in the synoptical table at the end.†

On comparing the results of these six analyses, it will be seen, that in no two instances do they agree either in the proportion or composition of the contained salts. The two latter, by Gmelin and Apjohn, are evidently the ones most to be depended upon, for the reasons already stated; but even between these, many very great differences occur. The lower specific gravity of Apjohn's specimen, which was occasioned by its having been collected at the close of the rainy season, and at about half a mile's distance from the mouth of the Jordan, may, it is true, partly account for these; but it certainly will not explain the absence of the chlorides of aluminum and ammonium, both of which were found by

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* Mémoires de l'Academie des Sciences, p. 69.

† An analysis by Dr R. Marchand appeared this year in the Journ. für Prakt. Chem. B. xlvii. 353.—Ed.

Gmelin, the former, particularly, in rather considerable quantity. For these reasons, it was therefore obvious that another analysis of the waters, performed with all the care and precautions that are now usually employed in this species of investigation, was absolutely necessary, in order that we might be enabled to determine which of the above analyses was the most trustworthy, or to point out the cause or causes which led to the discrepancies observed. Consequently, when Mr C. J. Monk, (son of the venerable Bishop of Gloucester and Bristol), who has recently returned from a long journey in Syria and the Holy Land, kindly offered to place at our disposal for this purpose a bottle of the water, we most willingly acceded to his proposal, with what result the following pages must testify.

The specimen so presented to us was collected by Mr Monk himself, on the 10th of March last, near the north-western extremity of the lake, about half-a-mile from the spot where the Jordan enters, but quite apart from all direct influence arising from the stream of fresh water which flows into it.

The water was perfectly clear and colourless, and did not deposit any crystals on standing in closed vessels, even when cooled considerably below its ordinary temperature. Its taste, as we have before observed, was intensely bitter and nauseous, and when swallowed, even in small quantity, it produced a sensation bordering upon sickness. It possessed no unpleasant odour. Its specific gravity, at 66° F., was 1·17205. The boiling-point, as determined in a glass vessel, with the barometer at 29·74 inches, and the thermometer at 47·75°, was 221·75° F.* It did not exert any definite reaction upon either blue or reddened litmus paper, proving the absence of all uncombined acid and carbonated alkali; neither did it in the slightest degree affect acetate-of-lead paper, as from Rae Wilson's statement we should have expected it. Only the slightest perceptible opalescence was produced in it upon boiling, or on the addition of an ammo-

* Dr Apjohn found that of his specimen to be 221°.

niacal solution of chloride of calcium, when, in the latter case, care was taken to add previously a sufficient quantity of muriate of ammonia, to prevent the precipitation of the magnesia. Consequently, only the faintest traces of carbonic acid or carbonate of lime were present.

The chloride of gold test of Dupasquier gave unmistakeable proofs of the existence of an abnormal proportion of organic matter. Other reagents shewed that it likewise contained magnesia or magnesium, lime, alumina, the oxides of iron, and manganese, soda, potash and ammonia; also chlorine, bromine, and sulphuric acid, with traces of silica, bitumen, and iodine. The latter occurred only in exceedingly minute proportion.

From the careful analyses of Messrs Thornton and Herapath, the specimen of the water of the Dead Sea, collected by Mr Monk, gave the following result:—

Chloride of calcium	2·455055 per cent.
Chloride of magnesium . . .	7·822007 ...
Bromide of magnesium . . .	0·251173 ...
Iodide of magnesium . . .	doubtful traces
Chloride of sodium	12·109724 per cent.
Chloride of potassium . . .	1·217350 ...
Chloride of ammonium . . .	0·005999 ...
Chloride of aluminum . . .	0·055944 ...
Chloride of manganese . . .	0·005998 ...
Chloride of iron	0·002718 ...
Organic matter (nitrogenous) . .	0·061730 ...
Nitric acid	very doubtful traces
Carbonate of lime	faint trace
Sulphate of lime	0·067866 per cent.
Silica	traces
Bituminous matter	ditto
	24·055564
Total amount of salt as determined by actual experiment . . .	24·048330

Table giving the composition, per Gallon, of the Waters of the Dead Sea, as shewn by the Analyses of different Chemists.

	MM. Lavoisier, Macquier, and Sage.	Dr Marcet.	Prof. Klaproth.	M. Gay-Lussac.	Prof. Gmelin.	Dr Apjohn.	Messrs Herapath.
Specific gravity	1·2403	1·2110	1·2450	1·2283	1·2120	1·1530	1·17205
Boiling-point	undetermined.	undetermined	undetermined.	undetermined.	undetermined.	221°	221°·75
Chloride of Calcium }	33·122·2115 }	3·221·2600	9·237·900	3·439·2400	2·726·8424	1·967·7098	2·014·2129
,, ,, magnesium }		8·561·7700	21·090·300	13·163·6911	9·988·5526	5·948·3270	6·417·6780
Bromide of magnesium	... ...	... ...	... ...	... ...	372·7022	162·2272	206·0708
Iodide of magnesium ?	... ...	... ...	... ...	... ...	... ...	... ...	very minute traces.
Chloride of potassium	... ...	... ...	... ...	... ...	1·420·0519	687·6492	998·7570
,, ,, sodium	5·426·3125	9·050·0452	6·170·220	5·975·6795	6·004·7206	6·326·8569	9·935·2036
,, ,, ammonium	... ...	... ...	... ...	... ...	6·1084	... ...	4·9226
,, ,, aluminum	... ...	... ...	... ...	... ...	76·0167	... ...	45·8975
,, ,, iron	... ...	... ...	... ...	... ...	... ...	... ...	2·2304
,, ,, manganese	... ...	... ...	... ...	... ...	179·6063	4·0355	4·9216
Organic matters	... ...	... ...	... ...	... ...	... ...	... ...	50·6510
Nitric acid	... ...	... ...	... ...	... ...	... ...	... ...	doubtful traces
Carbonate of lime	... ...	... ...	... ...	... ...	... ...	... ...	traces.
Sulphate of lime	... ...	45·77588	... ...	... ...	44·7107	60·5325	55·6800
Silica and bitumen	... ...	... ...	... ...	... ...	... ...	... ...	traces.
Fixed salts	38·548·5240	20·878·8510	36·498·420	22·578·6106	20·819·3118	15·157·3380	19·736·5254
Water	48·272·4760	63·891·1490	50·651·580	63·402·3894	64.020·6882	65·552·6620	62·306·9690
	86·821·0000	84·770·0000	87·150·000	85·981·0000	84·840·0000	80·710·0000	82·043·4944

On the Chronological Exposition of the Periods of Vegetation, and the different Floras which have succeeded each other on the Earth's surface. According to the views of M. BRONGNIART.

If, after having studied fossil vegetables, with respect to their organization, and the manner of determining their relations, with vegetables actually existing, without attending to the geological position which they occupy, we compare the different forms which have inhabited the surface of the earth at the different epochs of its formation, we shall find that great differences are observable in the nature of the vegetables which have been successively developed, and which replaced those which had been destroyed by the revolutions of the globe, and changes in the physical state of its surface. These are not merely specific differences, or slight modifications of the same types, but often of a much more important character,—such as new genera or families replacing genera and families destroyed and completely distinct; or rather, a numerous and varied family is reduced to a few species, while another, which was faintly marked by a few rare individuals, becomes all of a sudden numerous and predominating.

This is what is most commonly remarked in passing from one geological formation to another; but, when considering these transformations as a whole, a more general and important result presents itself in an unquestionable manner, namely, the predominance, in the most ancient periods, of acrogenous-cryptogamous vegetables (*Ferns* and *Lycopodiaceæ*); later, the predominance of gymnospermous dicotyledons (*Cycadeæ* and *Coniferæ*), without any mixture hitherto of angiospermous dicotyledons; and, in the last place, during the chalk formation, the appearance and speedy predominance of angiospermous vegetables, both dicotyledons and monocotyledons. These differences, so remarkable in the composition of the vegetation of the earth, which I have long since pointed out, and which all sound observations, of recent date, appear to me to confirm, shew that we may divide the long series of ages, during which this successive production of the different forms of the vegetable kingdom has taken place, into three long periods, which I shall call the reign of the Acrogens, the reign of the Gymnosperms, and the reign of the Angiosperms.

These expressions indicate nothing more than the successive predominance of each of these three great divisions of the vegetable kingdom, and not the complete exclusion of the others. Accordingly, in the two former, the Acrogens and Gymnosperms exist simultaneously, only the former at first predominate over the second in number and size, while the reverse takes place at a later period.

But, during these two reigns, the angiospermous vegetables appear to me, on the contrary, either to be completely wanting, or announced only by a few rare indications of doubtful character, and very different from their actual forms, marking rather the presence of certain monocotyledons, than that of the angiospermous dicotyledons.

Each of these three kingdoms, thus characterised by the predominance of one of the great divisions of the vegetable kingdom, is most commonly subdivided into many periods, during which very analogous forms, belonging to the same families, and often to the same genera, are perpetuated. Then these periods themselves comprehend many epochs, during which vegetation does not appear to have undergone any notable changes; but often materials are still wanting to establish these latter subdivisions with precision, either because the exact geological position of the beds enclosing vegetable impressions is not well determined, or because the division of the vegetable species in the different beds of the same formation has not been carefully established. Accordingly, I have no doubt that these different epochs, during which vegetation has preserved its characters in an invariable manner, will be much more multiplied than in the present state of our knowledge, and when materials carefully collected have accumulated on our hands.

At present, the following is the general division which I think the case admits of—

I. Reign of the Acrogens.—I. *Carboniferous Period.*
(Not susceptible of subdivision into distinct epochs in the present state of our knowledge.)

II. *Permian Period.*
(Forming only one epoch?)

II. Reign of the Gymnosperms.—III. *Vosgian Period.*
(Constituting a single epoch.)

IV. *Jurassic Period.*
Keupric epoch—Lias epoch—Oolithic epoch—Wealdian epoch.

III. Reign of the Angiosperms.—V. *Cretaceous Period.*
Sub-cretaceous epoch—Cretaceous epoch—Fucoidian epoch.

VI. *Tertiary Period.*
Eocene epoch—Miocene epoch—Pliocene epoch.

Passing these various epochs in review, I shall enumerate the different species of fossil plants which have been observed in the formations which correspond to them. In the carboniferous period, I shall merely indicate the genera and the approximate number of the species contained in each of these genera, the characters of the vegetation of this period being very strongly marked, and depending essentially on the nature of the genera. The number of species, particularly in the genera numerous in species, cannot be very strictly determined, because many of those described by authors would require further examination, and many of them are designated only nominally, not having been either described or figured. In the other periods, I shall give, as far as possible, a complete list of the species described as belonging to each particular epoch, because the same genera being pretty frequently continued during many successive epochs, the differences depend in a great measure on specific distinctions.

1. Reign of the Acrogens.—The great predominance of the section of Acrogens, and particularly the families of Ferns and Lycopodiaceæ, the considerable number of the species of the former of these families, the great development of the vegetables of the second, and the arborescent form of the Lepidodendrons, are the most prominent characters of this epoch; but to these must be added the presence of families

altogether anomalous, which we rank in the department of the Gymnosperms, but which evidently differ from the actually existing families of that department. These families have ceased to exist at the end of this reign of the Acrogens, which is, at the same time, that of the anomalous Gymnosperms, Sigillariæ, Nœggerathiæ, and Asterophylliteæ.

1. *Carboniferous Period.*—This long period commences with the appearance of the first terrestrial vegetables deposited in certain beds of the Transition-class, and extends to the new red sandstone which covers the coal-formation. In fact, throughout the whole of this period, there is no important difference among the vegetable forms; they consist of the same families, the same genera, and often the same species; and, in the present state of our knowledge on this subject, a flora of the vegetables of the Transition-class does not differ more from that of a true coal-formation, than the floras of different beds of the same coal-basin, or those of different coal-basins in near neighbourhood, differ from one another.

I may observe, besides, that the real epoch of many of the formations considered as Transition, which include coal-beds, with impressions of vegetables, is often imperfectly determined, and is either doubted, or still under discussion, by geologists; that many of these are perhaps nothing more than true coal-formations, accompanied by rocks modified by metamorphic phenomena, and that, as long as we cannot with certainty refer these formations to such as are well defined, under the names of Devonian, Silurian, or Cambrian formations, the specific comparison of their fossil vegetables with those of the coal-formations cannot be attended with any useful result.

The only coal-formations, considered by many distinguished geologists as more ancient than the ordinary coal-formation, which are very rich in fossil vegetables, are those on the banks of the Lower Loire, between Angers and Nantes. But the impressions they contain are all referable, without exception, to the ordinary coal-formations, and, taken as a whole, they furnish no character capable of distinguishing them from the latter.

I may add, that, very recently, some observations made on a carboniferous formation of very ancient date, since it is covered by beds containing fossil animals characteristic of the silurian formation, confirm this opinion as to the extension of the coal-vegetation to the commencement of the Transition-class. In fact, I find, in a memoir by M. Sharpe, on the Geology of the Neighbourhood of Oporto, that pretty thick and numerous beds of coal, which cover slates with trilobites, orthites, orthocerates, graptolites, &c., contain some impression of plants, and these impressions all of ferns, although somewhat imperfect, appear, according to M. Bunbury, identical with, or very closely allied to, well-known species of the ordinary coal-formation. These are, *Pecopteris cyathea* and *muricata*, and *Neuropteris tenuifolia.*

What I have said as to formations which appear more ancient than the coal-formation applies equally to the red sandstone which covers it; the fossils, which I have seen from thence, differ in no respect from those of the upper beds of the coal-formation, properly so called.

But if the vegetation of our globe has maintained itself without undergoing great changes during the whole of this period, it is not less certain that there have often been very decided changes in the species during the deposition of these different beds. Thus, in the same coal-basin, each bed often incloses some characteristic species, which are not found either in the more ancient or more recent beds, and which the miners have learned to regard as a distinctive mark of these beds.

M. Graeser, at Eschweiler, has carefully observed this fact, and pointed it out to me. At St Etienne, in like manner, I have determined it in many of the beds mined in that basin. To give an example, I may state that the beds in this basin which appear to be the lowest, contain abundance of *Odontopteris Brardii*, with very large pinnules, without a trace of any other Odontopteris; while the upper beds of the excavations of Treuil very frequently present us with *Odontopteris minor*, without intermixture of any other species. In general, each bed of coal is accompanied only by the remains of a somewhat limited number of vegetables. Sometimes this number, particularly in the most ancient beds, is extremely limited, and appears scarcely to reach eight or ten. In other cases, and more generally in the middle and superior beds, this number becomes more considerable, but I believe that it very rarely surpasses from thirty to forty species. We perceive that each of these small local and temporary floras, which here give rise to a bed of coal, is extremely limited. We observe something of the same kind in our own day in the case of extensive forests, particularly such as are composed of Coniferæ, where one or two species of trees cover with their shade only four or five different phenogamous plants and a few mosses.

But, in order to know whether these small floras, thus limited with regard to time and space, characterise so many special epochs in the vegetation of the globe, it is necessary to determine their succession in many of the principal coal-basins of Europe, and to notice if the nature of the vegetation is modified in the same manner in these different basins ; if, in a word, vegetation in different countries was everywhere the same at the same epoch, or if it was subjected to local variations, analogous to those which now distinguish the vegetation of a forest of *Pinus sylvestris* of Germany, of a forest of *Abies taxifolia* of the Vosges, *Picea excelsa* of the Jura, or *Pinus pinaster* of the Landes.

I am persuaded that this examination, if performed in a somewhat perfect manner, would shew that there are some general changes owing to the succession of the seasons, such as the predominance of certain genera, or of certain specific forms, combined with other differences altogether local, or to be ascribed to the influence of geographical position.

It accordingly appears to me to be the result of many local observations, that the Lepidodendrons are more abundant in the ancient beds than in the superior beds of the greater part of the coal-formations ; that the true Calamites are often in the same condition ; that the Sigillariæ would appear to predominate in the middle and superior beds ; that the Asterophyllites, and particularly the *Annularia*, are found much more abundantly in the superior beds ; that it is the same with the Coniferæ ; and it is only in the superior beds of St

Etienne, Autun, &c., that branches have been found, at least in France.

But these facts, which I state with much diffidence, as the result of observations made by me in the different coal-basins of France, have so much the more need of being generalised by observations made in other localities where the position of the beds is frequently surrounded with much obscurity, and differently designated by the most distinguished geologists.

After enumerating the genera of fossils belonging to the carboniferous period, and indicating the number of species supposed to be determined, amounting to 500, M. Brongniart continues :—

"What strikes us most forcibly, is the small number of vegetables which constituted this flora of the ancient world. It is true that this enumeration of the fossil vegetables of the carboniferous period contains scarcely any others than those of the coal-formation of Europe. A considerable number, however, have been brought from North America, and the observations hitherto made upon them shew that the greater part of the species are identical with those of Europe.

Thus, while this enumeration does not exceed 500 species, the existing flora of Europe comprehends upwards of 6000 Phænerogams; that of Germany, or rather Central Europe alone, more than 5000; and, if we include the Cryptogams, these numbers will rise to at least 11,000, and 9000 for Central Europe alone.

The flora of the carboniferous period comprehended, therefore, at most, a twentieth part of the number of vegetables now growing on the surface of Europe; and this number of species, moreover, corresponds to a long period during which diverse species succeeded each other, so that we may admit, with much probability, that never more than 100 species existed simultaneously. We thus perceive what was the poverty, and especially the uniformity of this vegetation, with regard more particularly to the number of species, compared with the abundance and variety of the forms of the present period.

The complete absence of the ordinary Dicotyledons or Angiosperms, and the almost equally complete absence of the Monocotyledons, sufficiently explain this low state of the ancient flora; for at the present time these two departments of the vegetable kingdom form at least four-fifths of the whole living species known. But the families, so few in number, existing at this period, absolutely contain many more species than they now do, as existing on the surface of Europe. Thus the Ferns of the coal-formation in Europe comprehend about 250 different species, and all Europe now produces only 50 species.

In like manner, the Gymnosperms, which at the present time in Europe comprehend only about 25 species of Coniferæ and Ephedreæ, then contained upwards of 120 species of very dissimilar forms.

These families, existing alone, and much more numerous than they now are in the same climates, if we embrace the whole carboniferous period, were still more remarkable on account of the different forms under which they presented themselves. Thus, among the Cryptogams, we remark genera of Ferns now completely destroyed, and numerous arborescent species: *Prêles* (reeds?) or allied vegetables almost arborescent; Lycopodiaceæ, forming gigantic trees, all forms at present unknown, either throughout the whole world, or at least in the temperate zones.

Among the vegetables which we classify along with the Gymnospermous Dicotyledons the differences are still more decided; for they constitute families completely extinct since that period; such are the Sigillariæ, Nœggerathiæ, and the Asterophylliteæ.

The characters of the vegetation during the carboniferous period may be thus briefly stated:—

The complete absence of Angiospermous Dicotyledons:

The complete, or almost complete, absence of Monocotyledons:

The predominance of Acrogenous Cryptogams, unusual forms, and such as are now destroyed, in the families of Ferns, Lycopodiaceæ, and Equisetaceæ:

The great development of Gymnospermous Dicotyledons, but resulting from the existence of families completely destroyed, not only in the present day, but from the end of this period.

Does this vegetation, thus reduced to the forms which we are led to consider as the most simple and least perfect, owe this special nature to a first phase of development in the organization of the vegetable kingdom, which had not yet attained the perfection which it afterwards reached; or is it owing to the influence of the physical conditions under which the surface of the earth was then placed? These are questions we cannot answer.

I shall merely remind the reader that I have already pointed out the analogy which this predominance of Acrogenous Cryptogams establishes between the vegetation of this first period, and that of the small islands of the equatorial and southern temperate zone, in which the maritime climate reaches its highest degree.

However, this predominance is not such as to entail, as during the carboniferous period, the exclusion of phanerogamous vegetables, and this complete exclusion would seem more favourable to the idea of a gradual development of the vegetable kingdom.

Lastly, we are not sufficiently acquainted with the influence of the nature of the atmosphere on the life of vegetables, when it is prolonged during their whole existence, to know whether considerable differences in the composition of this atmosphere, and in particular the very probable presence of a much greater proportion of carbonic acid, might not favour the existence of certain classes of the vegetable kingdom, and be opposed to that of other groups.

I shall terminate this glance at the vegetation of the carboniferous period, by remarking, that the coal-formation, which is almost the only one containing these remains, is evidently a terrestrial formation and from fresh water; that the beds of coal which it contains are the result of the accumulation on the place of the remains of vegetables which covered the surface after the manner of layers of turf on the soil of great forests; that it is only in exceptional circumstances that these beds alternate with beds containing the remains of marine animals, and may be considered as resulting from the transportation into the sea of the terrestrial vegetables which are found there.

This vegetation of the great carboniferous period disappeared almost completely along with it; the Permian period which succeeded it, presents only a kind of residuum of it, already deprived of the greater part of its most characteristic genera, and during the vosgian, or variegated sandstone period, we no longer find any trace of it.

I cannot terminate this account of the vegetation of the carboniferous period, without saying a few words respecting the incomprehensible exception to this regular and uniform distribution of the fossil vegetables, presented by the anthraxiferous formations of the Alps, if they really belong to the epoch of the lias, as is admitted by M. Elie de Beaumont, as well as by many other distinguished geologists, who concur in his opinion. I cannot here discuss the reasons derived from geological observations properly so called, which have led M. de Beaumont to this conclusion. I am well aware of the weight which the precise and well-directed observations of my learned friend have in the science. But when we consider that the researches undertaken by so many men of science and collectors have shewn that the vegetables contained in these beds are, without exception, those of the coal epoch, without the mixture of a single fragment of the fossil vegetables of the lias, of the jurassic period, of the keuper or variegated anomalous sandstone, we ask in vain what explanation can be given of this singular fact, and whether the shells, so few in number, which have particularly contributed to cause these formations to be referred to the jurassic period, are a very positive proof of this geological position. Their small number, their state of preservation so imperfect that their specific determination is either impossible or doubtful,—do these circumstances admit of more value being assigned to them than to this assemblage of numerous vegetables, the greater part easily determined as to species, which are found in the anthracitic beds? In 1828 I gave a list of these fossils, containing 25 species, 20 of which were specifically determined, and all identical with the species of the coal-formation. M. Bunbury has undertaken a similar task with regard to the collections deposited in the museum of Turin; he has come to the same result. I may add that, many years ago, I received from M. Scipion Gras, Chief Engineer of the Mines of Grenoble, collections of fossils from the mines of Lamure and Tarentaise, which comprehend upwards of 40 species, among which a great number belong to the most characteristic genera of the coal-formation. Such are the Sigillariæ, eight or nine in number, five of which are well determined, the *Stigmaria ficoides*, three *Lepidodendron*, a *Lepidophloios*, the *Annularia longifolia* and *brevifolia*; in a word, the entire coal vegetation, such as it presents itself at St Etienne or Alais.

With regard to the explanation derived from transportation from remote regions, where this vegetation is maintained, it becomes less admissible every day in proportion as the number of specimens increases, and we perceive that there does not occur a single example of vegetables peculiar to the lias period mingled with them.

(*To be concluded in our next Number.*)

Remarks on Dr Morton's Tables on the Size of the Brain. By Sir WILLIAM HAMILTON, Bart., Professor of Logic and Metaphysics in the University of Edinburgh. Communicated by the Author.

[Having laid a copy of Dr Morton's Tables, at page 262 of the present Number of this Journal, before my friend and colleague, Sir W. Hamilton, who has been long engaged in researches into the natural history of the brain of man, he kindly sent me the following important remarks, which I have great pleasure in communicating to the readers of the Philosophical Journal. They are, I hope, percursors of more extended observations from the same distinguished philosopher]:—*Edit. Ed. N. P. Journal.*

"What first strikes me in Dr Morton's tables completely invalidates his conclusions,—he has not distinguished male from female crania. Now, as the female encephalos is, on an average, some four ounces troy less than the male, it is impossible to compare national skulls with national skulls, in respect of their capacity, unless we compare male with male, female with female heads, or, at least, know how many of either sex go to make up the national complement.

" A ridiculous blunder of this kind is made by Mr Sims, in his paper and valuable correlative table of the weight of 253 brains (*Medico-Chirurgical Transactions*, vol. xix.). He there attacks the result of my observation (published by Dr Monro, *Anatomy of the Brain*, &c., 1831)—*that the human encephalos* (brain proper and after-brain) *reaches its full size by seven years of age*, perhaps somewhat earlier. In refutation of this paradox, he slumps the male and female brains together; and because he finds, that the average weight of his adults, among whom the males are greatly the more numerous, is larger than the average weight of his impuberals, among whom the females preponderate, he jumps at once to the conclusion, that I am wrong, and that the encephalos continues to grow, to diminish, and to grow again (!), for—I forget how long after the period of maturity. Fortunately, along with his crotchets, he has given the detail of his weighings; and his table, when properly arranged, confutes himself, and superfluously confirms me. That is, comparing the girls with the women, and the boys with the men, it appears, from his own induction, that the cranial contents do reach the average amount even before the age of seven.

" Tiedemann (*Das Hirn des Negers*, &c., 1837, p. 4) notes the contradiction of Sims's result and mine; but he does not solve it. The same is done, and not done, by Dr Bostock, in his *Physiology*. Tiedemann, however, remarks, that his own observations coincide with mine (p. 10); as is, indeed, evident from his table (p. 11) "of the cranial capacity from birth to adolescence," though, unfortunately, in that table, but in that alone, he has not discriminated the sex.

" Dr Morton's conclusion, as to the comparative size of the Negro brain, is contrary to Tiedemann's larger, and to my smaller, induction; which concur in proving, that the Negro encephalos is not less than the European, and greatly larger than the Hindoo, the Ceylonese, and sundry other Asiatic brains. But the vice, already noticed, of Dr Morton's induction, renders it, however extensive, of no cogency in the question.

"Dr Morton's method of measuring the capacity of the cranium, is, certainly, no 'invention' of his friend Mr Philips, being, in either form, only a clumsy and unsatisfactory modification of mine. Tiedemann's millet-seed affords, likewise, only an inaccurate approximation to the truth; for seeds, as found by me, vary in weight according to the drought and moisture of the atmosphere, and are otherwise ill adapted to recover the size of the brain in the smaller animals. The physiologists who have latterly followed the method of filling the cranium, to ascertain the amount of the cranial contents, have adopted, not without perversion, one-half of my process, and altogether omitted the other. After rejecting mustard-seed, which I first thought of employing, and for the reasons specified, I found that pure siliceous sand was the best mean of accomplishing the purpose, from its suitable ponderosity, incompressibility, and equality of weight in all weathers. Tiedemann (p. 21) says, that he did not employ sand, 'because, by its greater specific gravity, it might easily burst the cranial bones at the sutures.' He would, by trial, have found that this objection is futile. The thinnest skull of the youngest infant can resist the pressure of sand, were it many times greater than it is; even Morton's lead shot proved harmless in this respect. But, while nothing could answer the purpose better than sand, still this afforded only one, and that an inadequate, mean towards an end. Another was requisite. By weighing the brain of a young and healthy convict, who was hanged, and afterwards weighing the sand which his prepared cranium contained, I determined the proportion of the specific gravity of cerebral substance (which in all ages and animals is nearly equal), to the specific gravity of the sand which was employed. I thus obtained a formula by which to recover the original weight of the encephalos in all the crania which were filled; and hereby brought brains weighed and skulls gauged into a universal relation. On the contrary, the comparisons of Tiedemann and Morton, as they stand, are limited to their own tables. I have once and again tested the accuracy of this process, by experiment, in the lower animals, and have thus perfect confidence in the accuracy of its result, be the problem to recover the weight of the encephalos, from the cranium of a sparrow or from the cranium of an elephant.

"I may conclude by saying, that I have now established, apart from the proof by averages, *that the human encephalos does not increase after the age of seven, at highest.* This has been done, by measuring the heads of the same young persons, from infancy to adolescence and maturity; for the slight increase in the size of the head, after seven (or six), is exhausted by the development to be allowed in the bones, muscles, integuments, and hair."

Analysis of the Anthracite of the Calton Hill, Edinburgh. By Dr A. Voelcker, Professor of Chemistry in the Agricultural College, Cirencester. Communicated by the Author.*

We are in possession of analyses of anthracite from several localities, and we have learned by them, that the composition of this mineral, like that of coal, varies very much according to the locality where it is found; so that there are scarcely two localities which furnish anthracite of exactly the same composition.

All samples of anthracite which have been analysed, have been found to contain carbon, hydrogen, oxygen, nitrogen, and more or less inorganic matter, as well as sulphur (at least where it has been looked for), in a proportion which differs but slightly from that in which it occurs in common coal. Generally speaking, the per-centage of carbon is larger in anthracite than in common coal, whilst hydrogen predominates in the latter; and we find, likewise, that the more the anthracitic character of a sample is pronounced, the

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* Read before the Royal Society of Edinburgh, 4th March 1850.

greater is the deviation from the composition of common coal. On the other hand, the more an anthracite resembles common coal in its physical character, the closer is the approximation to the latter in chemical composition. The sulphur which has been found in every specimen of anthracite in which it has been sought for, is generally considered as existing in it as well as in coal, in combination with iron, as iron pyrites, but the subjoined results shew that the sulphur found in anthracite does not always occur in the form of iron pyrites, but is, in part at least, in combination with the organic elements of the mineral. In the following analyses the greatest care was taken to deprive the anthracite of any hygroscopic water, by keeping it finely powdered in a glass tube, at a temperature of about 230° F., and passing over it a current of dry air for several hours.

The per-centage of carbon was ascertained by burning from three to four grains with a mixture of oxide of copper and oxide of lead, and the simultaneous application of oxygen gas, in order to secure complete combustion of the carbon. The oxygen, for that purpose, was disengaged from chlorate of potash, mixed with pure oxide of copper, and placed at the closed end of the combustion-tube. A mixture of the oxides of copper and lead possesses the advantage over pure oxide of copper, of being much less hygroscopic ; for that reason it is peculiarly adapted for combustions in which the exact amount of hydrogen is to be ascertained. The nitrogen was determined according to Will and Varrentrapp's method, by heating the finely-powdered anthracite with soda-lime in the usual way.

For the determination of ash about 10 grains were burned in a platina capsule. The ash was coloured red by oxide of iron.

The proportion of sulphur was ascertained by introducing, into a red-hot crucible, a mixture of anthracite with carbonate of soda and nitre, in small quantities at a time, and heating the whole afterwards a little more strongly. The resulting fused and perfectly white mass was dissolved in water, supersaturated with hydrochloric acid, and the sulphuric acid then precipitated with chloride of barium.

According to the different results obtained, the composition of the anthracite of the Calton Hill is :—

Carbon	=	91·23
Hydrogen	=	2·91
Nitrogen	=	0·59
Oxygen	=	1·26
Sulphur	=	2·96
Ash	=	1·05
		100·00

For comparison with this analysis, I subjoin a few analyses of anthracite from different localities.

	Carbon.	Hydrogen.	Oxygen.	Nitrogen.	Ash.
From Lamure, Isère Department, according to Jacquelin (Annal. de Chimie et de Phys. lxxiv., 200),	89·77	1·67	3·63	0·36	4·57
From Sablé, Sarthe Department, ...	87·22	2·49	1·08	2·31	6·90
From Vizille, Isère Department, ...	94·09	1·85	...	2·85	1·90
From another locality in Isère Department, . . .	94·00	1·49	...	0·58	4·00
Anthracite from Pembrokeshire, according to Schafhäutl, Lond. & Edin. Phil. Mag. xvii., 215, .	94·100	2·390	1·336	0·874	1·300
From Coalbrook in Carmarthenshire, ...	90·58	3·60	3·81	0·29	1·72
Anthracite from Wales, .	91·44	3·46	2·58	0·21	1·52
				Sulphur. 0·79	

The last analysis is taken from Sir Henry de la Beche and Dr L. Playfair's Coal Report, the others from Hausmann's Mineralogy.

The most remarkable peculiarity of the anthracite of the Calton Hill is the comparatively large quantity of sulphur which it contains. By far the greater portion of this sulphur must have been in combination with the organic elements of anthracite; for, even supposing the whole of the ash to consist of oxide of iron, the quantity of iron would still be too small to combine with all the sulphur. I am not aware that attention has been drawn to the fact of sulphur occurring in anthracite in organic combination; but a little consideration, I think, will shew that such a compound may exist in nature, as we can prepare artificially, similar combinations. It is well known that, in preparing sulphide of carbon, by passing sulphur in vapour over red-hot charcoal, the charcoal which remains in the vessel in which the experiment has been made, contains sulphur in such a state of combination that it cannot be expelled by heat, provided the air be excluded. According to Prout, a similar combination of sulphur with carbon is easily obtained by washing on a filter common gunpowder with water till all the nitre is removed, and heating the insoluble part of the gunpowder in a retort; some of the sulphur will distil off, and part of it remain in combination with the charcoal in the retort. This sulphur, and the nitrogen, which is always found in anthracite, testify in favour of the vegetable origin of this mineral, and appear to support the opinion of those who regard it as the carbon-remains of organized bodies of the oldest formation, in which the process of carbonification has proceeded still farther than in coals.

At all events, the above analysis furnishes an additional proof of the erroneous notion of former naturalists, who regarded anthracite as primitive carbon. This notion, probably, has arisen from the fact, that anthracite, exposed to a red heat, produces no hydrocarbons like coals, and that it resembles carbon likewise, inasmuch as it is consumed by fire almost entirely, leaving but a small proportion of mineral matter in the form of ash behind. The loss incurred by incineration of anthracite was generally calculated as carbon by chemists, before the present methods of analysing organic substances were known. Some observers, however, inferred that water existed in a state of chemical combination in anthracite, as appears from a statement of Lampadius, in an able paper on the Anthracite of Schönfeld in Saxony, which appeared in Erdmann's Journal der Chemie, 1835, 4th Bd., p. 393. By a careful observer like Lampadius, the presence of sulphur and nitrogen in anthracite was not overlooked. He likewise examined all the products of its dry distillation, and obtained, besides water, a mixture of gases, which consisted of carbonic acid, carburetted hydrogen, carbonic oxide, and nitrogen.

Similar results were obtained on analysis of two varieties of anthracite from North America, which Professor Breithaupt of Freiberg procured for him. These samples, from Manchchunk in North America, and from Rhode Island, are described by Professor Breithaupt as remarkably fine anthracite. The imperfections of the analytical methods at that time, however, led Lampadius to draw false conclusions from his analytical results, and induced him to consider all anthracites as hydrates; but we know at present that the hydrogen and oxygen in anthracite are not united as water. Though mistaken in his quantitative analyses, Lampadius, nevertheless, has the credit of having pointed out the qualitative composition of several varieties of anthracite more accurately than any chemist who examined this mineral before him. In all samples he detected carbon, oxygen, hydrogen, nitrogen, and sulphur, besides the ash, or the same substances which were found to enter into the composition of the anthracite of the Calton Hill.

On the Possible Derivation of the Diamond from Anthracite and Graphite. By Dr George Wilson, F.R.S.E.* Communicated by the Author.

The recent analysis by Dr Voelcker of the anthracite of the Calton Hill, which some mineralogists had thought entitled to the name of *carbon*, as not sensibly differing from graphite, led me, though dissenting from that view, to spe-

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* Read before the Royal Society of Edinburgh, 4th March 1850.

culate on the possibility of anthracite being converted into transparent crystalline carbon. Anthracite itself seems always to contain hydrogen, oxygen, and nitrogen, as well as sulphur and fixed inorganic matter. Some specimens of the mineral, indeed, do not contain more than 70 per cent. of carbon, although the majority contain about 90, and some nearly 95 per cent. of that substance. It cannot, therefore, be ranked along with graphite and the diamond, as a variety of pure carbon ; but there seem some very significant reasons for thinking that it may be a substance from which the diamond is developed. I speak on this point as a chemist, assuming that there are no such difficulties of a geological or mineralogical kind, opposed to the supposition that anthracite may crystallise into the diamond, as forbid the entertainment of such a view. So little, indeed, is known concerning the original matrix, or earliest geological or mineralogical *situs* of the diamond, that we are free, within very wide limits, to speculate on the origin of this gem. Little, however, has been recently contributed to our hypotheses concerning the production of the diamond. Chemists have pretty generally abandoned the subject, for a time at least, and have been content to draw attention to the supposed fact, that carbon is neither fusible nor vaporizable,* and that it crystallises from

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* The late Mr Kenneth Kemp, whose ingenuity was inexhaustible, endeavoured to crystallise carbon from its vapour, by producing the voltaic arc (or so-called electric light) between charcoal-points, within the Torricellian vacuum. It may, perhaps, be questioned whether carbon was truly vaporised in this experiment, or only detached in the state of minute particles from the intensely heated charcoal ; at all events, it did not crystallise, but deposited itself as impalpable soot on the sides of the barometer tube. Professor Silliman senior, made similar but independent experiments many years ago, and witnessed, as he believed, the true fusion and volatilisation of carbon, but did not obtain it in distinct or transparent crystals. He has lately redirected attention to these observations. (*American Journal of Science and Arts,* November 1849, p. 413.)

M. Despretz has recently exposed charcoal to the combined influence of a powerful voltaic current, the concentrated rays of the sun and the blow-pipe. Small needles of anthracite, exposed to this triple source of intense heat, seemed to fuse, and permitted drops to fall from them, which, when received on a platina capsule, appeared as minute *black* globules. (*Comptes Rendus,* 18th Juin melted cast-iron (the solitary liquid which dissolves it in any quantity) as graphite, not as diamond; so that we are apparently precluded from supposing that it can have been obtained, by any of the three processes by which we ordinarily crystallise bodies artificially, and have reason to believe they are crystallised in nature.

To Liebig alone, so far as I know, among recent chemists, we are indebted for the publication of a new theory of the origin of diamonds. After explaining the slow oxidation, or eremacausis, as he names it, of woody fibre, when it is undergoing decay, he points out that the other elements of the wood are removed in much greater proportion than the carbon, which comes to preponderate more and more the further decay has proceeded. He then adds: " If we suppose decay to proceed in a *liquid* containing carbon and hydrogen, then a compound with still more carbon must be formed, in a manner similar to the production of the crystalline colourless napthalin from a gaseous compound of carbon and hydrogen. And if the compound thus formed were itself to undergo farther decay, the final result must be the separation of carbon in a crystalline form.

" Science can point to no process capable of accounting for the origin and formation of diamonds, except the process of decay. Diamonds cannot be produced by the action of fire; for a high temperature, and the presence of oxygen gas, would call into play their combustibility. But there is the greatest reason to believe that they are formed in the humid way, that is, in a liquid; and the process of decay is the only cause to which their formation can with probability be ascribed."*

Liebig's theory thus implies that the diamond is formed

1849, p. 755.) These globules may only have been the ash of the anthracite, coloured by contained charcoal; if they were pure carbon, they probably consisted of graphite, for their colour forbids the conclusion that they were minute diamonds. M. Jacqueline has shewn that the diamond, when suddenly exposed to the intense heating power of voltaic electricity, changes into coke or graphite. Other chemists have sought for special solvents of carbon, and may yet be successful in their search.

* Agric. Chem., p. 341.

by the slow spontaneous decomposition of a *liquid* compound of carbon.

My present object is to urge, that, in addition to this and similar views, we are free to inquire whether carbon may not be crystallised from graphite and amorphous carbon, or from one of its solid compounds, without passing through an intermediate condition of fluidity.

Professor Jameson long ago suggested that the diamond was of vegetable origin; but I have not been able to procure his original paper. Sir David Brewster also read a communication to this Society in 1820, on the occurrence, in some diamonds, of a polarising structure, occasioned by the existence within them of small portions of air, "the expansive force of which has communicated a polarising structure to the parts in immediate contact with the air." This structure, Sir David thinks, "can arise only from the expansive force exerted by the included air on the diamond, when it was in such a *soft state* as to be susceptible of compression from so small a force. That this compressible state of the diamond could not arise from the action of heat, is manifest from the nature and recent formation of the soil in which it is found: that it could not exist in a mass formed by aqueous deposition, is still more obvious; and hence we are led to the conclusion, rendered probable by other analogies, that the *diamond* originates, like amber, from the consolidation of perhaps vegetable matter, which gradually acquires a crystalline form by the influence of time and the slow action of corpuscular forces."*

Sir David Brewster and Liebig are thus, to some extent, at issue. The latter thinking "that there is the greatest reason to believe that diamonds are formed in the humid way;" the former contending that "they cannot have been formed by aqueous deposition."

In extension of these speculations, I would suggest the probability of anthracite being one of the substances most likely to crystallise into the diamond. It does not seem necessary, however, to adopt Sir David's view, that the diamond

* Edin. Phil. Jour., 1820, pp. 99-100.

must once have been soft, because specimens containing air-bells exhibit a polarising structure around those bubbles; for, as the author himself points out, a similar structure may be developed in glass, "by a compressing force propagated circularly from a point;" and a solid mass of carbon contracting in dimensions whilst it became transparent, would, if I mistake not, have a similar structure developed in it, around any bubbles of gas which it might inclose. Taking for granted, then, that it is not necessary, in a theory of the origin of the diamond, to provide for its once having been soft,* the following reasons may be assigned for supposing that anthracite is more likely than other compounds of carbon to yield the diamond.

1*st*, Anthracite, as it occurs in nature, is described by the mineralogist, as passing, by insensible gradations, into common coal on the one hand, and into graphite or pure carbon on the other; so that it may be regarded as marking the transition from fossilized vegetable matter to uncombined carbon. In speaking thus, I do not seek to affirm that, in every locality, anthracite can be shewn to have been derived from coal and to be passing into graphite; but this can be demonstrated for many anthracites, and may be extended to all, if it be conceded that, from the less bituminous coals onwards to graphite, we have a series of chemical compounds in which the proportion of carbon constantly increases till it excludes every other organic constituent, whilst the majority are ranked by the mineralogist under the common title of anthracite.

2*d*, Anthracite consists in greater part of carbon, sometimes containing nearly 95 per cent. of it.

3*d*, The other constituents of anthracite, with the exception of the ash, which is often under 1 per cent., form volatile compounds with each other and with the oxygen of the air. There is thus a provision, in the spontaneous slow oxidation of anthracite by air, for depriving it of all its constituents (the ash excepted) but carbon. I do not attach impor-

* I do not wish, however, to be understood as affirming that no diamond was ever soft, but simply that it is not necessary to assume that all were.

tance to the mere preponderance of the latter in the mineral, for if abundance of carbon were the criterion of the convertibility of a chemical compound into diamond, then graphite should have a decided preference to anthracite. as the source of the gem; and there are many reasons for thinking that graphite may change its crystalline form, and become the transparent octahedral diamond. But anthracite has the great advantage over graphite, that, whilst it consists in greatest part of carbon, it contains other ingredients which can be volatilised out of it by the action of the air on it, and the escape of those bodies (carbon, oxygen, nitrogen, hydrogen, and sulphur) must disturb the molecular equilibrium of the anthracite, and leave gaps between its particles of carbon. We may suppose these, accordingly, to fall in, or move towards each other, and that in the act of so moving, they arrange themselves in the crystalline form of the diamond.

We have evidence so ample at the present day, that the most solid bodies can crystallise, though persistent during their crystallisation as solids, that it certainly is not necessary to assume any such interstitial motion in the anthracite as has been referred to. But it is not less certain, on the other hand, that crystallisation is much more easily induced among moving particles returning to rest, than among molecules locked together so as to have little freedom of motion among themselves. The elimination of the hydrogen, oxygen, nitrogen, and sulphur, along with a certain amount of carbon, would give the opportunity for this molecular motion which is so desirable, and yet give it to but a small extent at any one time, so that the particles of carbon would approximate with great slowness, and assume a position of very stable equilibrium. All, I think, will acknowledge that the diamond must have been formed by a very slow process. Its physical characters, but especially its general chemical purity, its solidity, and its regular symmetrical form, forbid the notion that it can have been the product of a hasty crystallisation.

Sir David Brewster has suggested that a body like amber may have been the origin of the diamond, as Sir Isaac Newton had previously supposed that it was "probably an unctuous substance coagulated." Such bodies, however, as amber, the resins, bitumen, wax, fat, &c., &c., contain so large a proportion of other ingredients than carbon, particularly of hydrogen, the number of equivalents of which frequently exceeds that of the equivalents of carbon, that it is difficult to suppose any process by which the hydrogen could be extracted so as to leave the carbon. In anthracite, on the other hand, we have little foreign matter to extract, and yet sufficient to disturb the previous molecular arrangement of the mineral.

I do not attempt to indicate at what temperature (except that it must be comparatively low), and under what exact circumstances, anthracite may change into diamond. One point, however, seems to deserve notice. It may seem difficult to concede that the action of oxygen on the surface of a mass of anthracite can determine the evolution of bodies from a depth within its substance. Whatever difficulties, however, may attend the conception of the process, it is quite certain that masses of anthracite may be changed by heat into coke or carbon, throughout their entire substance, whilst they increase in solidity and density. We may either suppose, accordingly, that anthracite is so porous that the oxygen of the air can penetrate to the centre, or that the superficial oxidation of the mineral determines a transference of the particles more deeply seated to the surface, where they are oxidised. The last reference will be better understood if I adduce a case in point. A bar of steel, *i.e.*, a compound of iron and carbon, if raised to a red heat, and exposed to a current of air or oxygen, may be changed into a bar of iron; yet during the process carbon must have travelled from the centre of the bar to the surface, or oxygen must have travelled from the surface to the centre and back again, before the carbon could have been volatilised as carbonic oxide. By a similar process, hydrogen and the like might be extracted from a mass of anthracite. As for the non-volatile ingredient or ash, I may, on the one hand, notice that crystallising substances are notoriously possessed of the power of extruding heterogeneous or foreign matter; and, on the other, that the majority of diamonds, when burned, leave a slight ash.

I have no wish, however, to affirm that anthracite is the only body which can crystallise into the diamond. On the other hand, I think we have been too ready to assert that all diamonds must have been produced in the same way. This does not seem probable. Many substances can be crystallised in six different ways, viz., by melting, by dissolving, or vaporising them, by decomposing their gaseous or liquid compounds, and by inducing crystallisation in them whilst they are solid. Carbon may crystallise in most, perhaps in all of those ways, and the genesis of one diamond be quite different from that of another.

I would only further remark, that the question, whether carbon will crystallise as graphite, or as diamond, will mainly be determined by the rapidity with which crystallisation is effected, and the temperature at which it occurs.

Graphite certainly represents the most stable equilibrium of the crystalline molecules of carbon *at a high temperature;* for melted cast-iron, containing excess of carbon, separates the latter as graphite when it solidifies; and the diamond, if suddenly raised to a white heat, changes into the same substance; but at lower temperatures the diamond must be regarded as exhibiting the more stable molecular equilibrium. In truth, a crystal like the diamond, belonging to the tessular system, with its three equal crystallographic axes, and its inability to refract doubly or polarise light, appears the most complete expression of crystalline molecular equilibrium. Whenever, therefore, carbon crystallises very slowly, and at moderate temperatures, it may be expected to become the diamond; and graphite, when not maintained at a high temperature, must be looked upon as a substance whose particles are in a state of unstable equilibrium, and as constantly tending, therefore, to have this equilibrium overturned so as to attain that which characterises the diamond.

On the proportion of Fluoride of Calcium present in the Baltic. By Professor FORCHAMMER of Copenhagen. *With some Preliminary Remarks on the presence of Fluorine in different Ocean Waters.* By Dr GEORGE WILSON, F.R.S.E.* Communicated by the Author.

In 1846, I announced the discovery to the Royal Society of Edinburgh of fluorine as a new element of sea-water. I was led to search for it, after observing that fluoride of calcium possesses a certain small but marked solubility in water, which explains its occurrence in springs and rivers, and necessitates its occasional, if not constant presence in the sea.

The only specimens of sea-water I had examined before last summer, were taken from the Frith of Forth, about three miles from Edinburgh. I obtained the mother-liquor or bittern, from the pans of a salt-work there, and precipitated it by nitrate of baryta. The precipitate, after being washed and dried, was warmed with oil of vitriol, in a lead basin, covered with waxed glass with designs on it. The latter were etched in two hours, as deeply as they could have been by fluor-spar, treated in the same way, the lines being filled with the white silica, separated from the glass.

Last summer I examined in the same way bittern from the salt-works at Saltcoats, in the Frith of Clyde, but the indications of fluorine were much less distinct than in the waters on the east coast. On procuring, however, from the same place, the hard crust which collects at the bottom and sides of the boilers used in the evaporation of sea-water at the salt-works, I found no difficulty in detecting fluorine in the deposit. The crust, or deposit in question, consists in greater part of sulphate of lime, and of carbonate of lime, and magnesia, but it contains also much chloride of sodium, and the other soluble salts of sea-water, entangled in its substance. When sulphuric acid, accordingly, is poured upon it, it gives off much hydrochloric and carbonic, as well as some hydro-

* Read before the Royal Society of Edinburgh, 4th March 1850.

fluoric acid; and the latter is thus swept away, before it has time to corrode the glass deeply. I preferred, nevertheless, to use the crust exactly as I got it, that the proof of the presence of fluorine might not be impaired in validity, by the possibility of that substance being introduced in the water and re-agents, which must have been employed had the chlorides and carbonates been separated from the crust by a preliminary process. The crust, accordingly, after having been dried and powdered, was placed, along with oil of vitriol, in a lead basin covered by a waxed square of plate-glass, with letters traced through the wax. A single charge of the crust corroded the glass only slightly, but by replenishing the basin with successive quantities of the powdered crust and acid, whilst the same plate of engraved glass was used as the cover, I found no difficulty in etching the glass deeply. I am indebted to my friend Mr Stevenson Macadam, for this simple but effective way of increasing the corrosion of the glass, which seems worth the adoption of chemists in all cases where fluorine is sought for. Four charges of material have been sufficient, in all the specimens of sea-water deposit I have examined, to mark the glass strongly. It was kept wet on the upper side, and exposed undisturbed to the action of each charge during twelve hours.

Operating in this way, I have found fluorine readily in the boiler-deposit from the waters of the Friths of Forth and Clyde. It is a less easy matter to subject the waters of the open sea to the requisite concentration before examination. It occurred to me, however, that the incrustations which are periodically removed from the boilers of the ocean steamers would serve to determine the question, whether fluorine is a general constituent of the sea.

I have obtained accordingly at Leith, the crust from the boiler of the *St Kiaran*, which traded between that port and Wick, so that the greater part of the water consumed as steam by its engines is derived from the German Ocean, although a portion is necessarily obtained from the Frith of Forth. The crust from the boilers of this vessel, was treated in the way described, and at once yielded hydrofluoric (and probably also hydrofluosilicic) acid. A single charge, indeed, of the materials marked the glass distinctly, and four charges deeply. We may therefore infer that fluorine is present in the waters of the German Ocean, for different portions of the deposit yielded it readily, and marked glass as deeply as the deposit from the water of the Frith of Forth did, which could not have been the case if the whole crust had not contained fluorine pretty equally diffused through it.

The results I have detailed, were communicated to the British Association at its meeting in 1849, and were confirmed by Professor Forchammer, so far as the Baltic is concerned.* At my request, he kindly furnished me with the letter which follows this communication, in which he announces his discovery of the proportion of fluoride of calcium present in sea-water, as determined by analysis of that obtained from the Sound, near Copenhagen. The letter is otherwise interesting, and is printed in full.

Since the results given above were communicated to the British Association, I have procured from Liverpool, a boiler-incrustation, from one of the Dublin and Liverpool Steam-Packet Company's vessels, which, when treated with sulphuric acid, yields an acid vapour, readily corroding glass. From the same port, I have obtained a crust from the boiler of the "Canada," transatlantic steamer; and from Portsmouth, a deposit from Her Majesty's war-steamer, Sidon, which was three years on the Mediterranean station. Both of these crusts, after reduction to powder, yielded hydrofluoric acid (or rather hydrofluosilicic acid) so abundantly, when treated with oil of vitriol, that a single charge of each, etched glass distinctly in two hours.† I can now,

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* Report of the Proceedings of Brit. Assoc., *Athenæum* for September 1849.

† The crusts which give the best results, are those consisting of the most insoluble salts of sea-water, which are found adhering to the walls of the boiler on which they have evidently deposited very slowly. They frequently contain a mere trace of chlorides, and but a small quantity of carbonates; their chief constituents being sulphates and fluorides of the alkaline earths, along with silica. Such crusts not only contain more fluorine than the looser deposits, but do not effervesce when heated with sulphuric acid, so that the hydrofluoric or hydrofluosilicic acid they evolve, is not swept away by carbonic or hydrochloric

therefore, state as the result of direct examination of deposits which may be fairly taken to represent the following waters, that fluorine is present in the Friths of Forth and Clyde, the German Ocean, the Irish Sea, the Atlantic, and the Mediterranean ; and Professor Forchammer bears testimony to its presence in the Baltic. We may, therefore, infer, that as the sea within narrow limits is very uniform in chemical composition, fluorine will be found universally present in the ocean. Indirect proof of this has already been derived from the presence of fluorine in corals and shells, as well as in marine fishes and mammalia.* My present object, however, is to refer solely to the results obtained by a direct analysis of sea-water.

Professor Forchammer's letter is as follows—

COPENHAGEN, *20th December* 1849.

Dr GEORGE WILSON,

MY DEAR SIR,—Enclosed in this letter you will find an abstract of a paper which I have been preparing for the Royal Society at Copenhagen, and which I hope soon to be able to have published. It contains experiments on many other substances contained, in minute quantities, in sea-water, for instance, manganese, ammonia, baryta, or strontia, besides iron and silica, which occur in proportionally large quantities.—Believe me, my dear Sir, yours,

G. FORCHAMMER.

Abstract of a Paper by Professor Forchammer, on the Rarer Substances which occur in Sea-Water. (To be read at the Royal Society at Copenhagen.)

Fluorine and Phosphoric Acid.

100 lb. of sea-water as it occurs in the Sound, near Copenhagen, of which the average quantity of salts is between 2 per cent. and 2½ per cent., was evaporated. When the solution was so concentrated that it began to deposit salt, it was, without filtering it, mixed with an excess of ammonia, and the pre-

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acid before it can act on the glass. The Atlantic and Mediterranean crusts were of this description, and etched more readily than the Irish crusts, which abounded in chlorides.

* Edin. Royal Soc. Trans., Vol. xvi., Part ii., p. 155.

cipitate collected and washed. The whole precipitate, which contains carbonate, sulphate and phosphate of lime, fluoride of calcium, silica, and magnesia, was redissolved in muriatic acid, which left the greater part of silica undissolved. The solution was mixed with muriate of ammonia, and a second time precipitated by an excess of ammonia. This precipitate from 100 lb. of sea-water weighed 3·104 grains, and consisted of phosphate of lime and fluoride of calcium. It was divided into two equal parts, of which the one was in a platina crucible mixed with concentrated sulphuric acid, and allowed to act on a slip of glass, covered with wax, in which some words were scratched with a copper needle. The glass was most decidedly etched, but the words appeared more clear and legible if breathed upon. The second half part was likewise mixed with sulphuric acid, but in a bent tube, and distilled into a small vessel which contained a weak solution of ammonia. The tube was etched, and the vessel contained precipitated silica. It was thus completely proved that sea-water contains fluoride of calcium, but the quantity in 100 lb. sea-water from the Sound, at Copenhagen, can hardly exceed half of a grain, or since the proportion of the different salts varies very little in sea-water, it will be about 1 grain in 100 lb. of water of the ocean, which contains between 3·5 and 4 per cent. of salts.

All the residuums from the trials to find fluorine were dissolved in muriatic acid, and thrown down by an excess of ammonia. The precipitate, washed, dried, and heated, was mixed with potassium in a glass tube [and heated] until the excess of potassium was driven off. The lower part of the tube was cut off and thrown into water, where it, for hours, continued to give out small bubbles, distinguished by the peculiar smell of phosphuretted hydrogen, although they did not inflame by themselves. Thus, the existence of phosphoric acid was likewise proved, although I could not try the delicate test for phosphoric acid which we owe to Mr Svanberg, it not being known at the time when I made my experiments.

In all the different species of corals which I have analysed, I likewise found fluorine.

On the Geology of the Baltic.

The following observations in Mr R. Chambers's graphic account of his journey through Scandinavia, now in course of publication, being on generally interesting geological topics, will, we doubt not, be prized by our readers.

1. *Sir Charles Lyell's imaginary depression and elevation of the Land near Stockholm,—and Professor Playfair's hypothesis of the rising of the Land in Scandinavia.*

I found rather a small vessel; no saloon besides the *spiese-kammer* (eating-room), and only a double series of small cabins, each with two beds, running transversely to the length of the vessel, with a narrow space between. Starting at an early hour on Sunday morning, we in a few hours passed through the Sodertelje Canal into the open sea. The passage was the scene of a very remarkable antiquarian discovery, to which Sir Charles Lyell alludes in his paper in the "Philosophical Transactions" on the movements of the Baltic shores. It seems that this canal required a cutting of more than sixty feet through soft matter between two lines of rocky ground. In the course of that cutting the workmen found, at the depth of sixty feet, and at the level of the sea, the remains of an ancient hut. There were a floor and a hearth—distinct traces of its having been a human habitation. Sir Charles tells us, that the superincumbent matter was composed of a marine formation. He says, "the stratification of the mass over the house was very decided, but for the most part of that wavy and irregular kind which would result from a meeting of currents." His theory is thus expressed:—"It appears that this building must have been submerged beneath the waters of the Baltic to the depth of sixty-four feet; and before it was raised again to its present position, it had become covered with strata more than sixty feet thick."

To imagine that the land at this place can have been sunk sixty-four feet since it was first inhabited by man, is a supposition so violent, that only the most incontestable evidence could justify its being advanced. There could not only be strong positive evidence for the assumed fact, but there should be no other way of accounting for it. Now, are we quite sure that there is no other way of accounting for the existence of a human habitation below sixty feet of soft matter in that situation? I find in Mr Laing's work on Sweden a remarkable passage. Speaking of the branch of the Maeler Lake, out of which this short canal proceeds, he says: "It was in this branch of the Maeler, if I am not mistaken, that St Olaf, when a viking, was penned up on one of his piratical expeditions, in the eleventh century, by the united fleets of the Swedish and Danish monarchs; they expected to starve him out, or force him to engage with his few ships to a disadvantage. *He made a ditch or canal from the lake to the Baltic*, through which he carried his vessels to sea, leaving his enemies blockading the entrance of the branch of the lake." The line of this ditch would necessarily be the same as that of the modern canal. In such a trench a house may have been built. The trench may have been subsequently filled up with wind-driven materials; against which supposition there is nothing positive on record in the case; for though Sir Charles states that the superincumbent matter was stratified, and of marine origin, he only alludes to the banks of the sides of present canal at the spot, and not to the matter actually above the house, which indeed he never saw. On the contrary, there is something in the record positively in favour of our surmise, for Sir Charles ascertained that the sand immediately in contact with the remains of the house was of the fine kind which is accumulated by the wind. Behold, then, a *possible modern origin* for this hut, without any necessity of supposing a comparatively modern submersion of the land sixty-four feet under the sea!

It is of course to be feared that there has been some rashness in assuming the dip and subsequent re-emergence of the land since the hut was formed and inhabited. Such rashness is not to be wondered at, for a geologist in the condition of a determined partiality for a particular theory, is much as Mrs Slipslop, in her conversation with Joseph Andrews, described her sex to be in analogous circumstances: 'If we like a man, the *lightest hint sophisticates*.' When a man of science likes a theory, the lightest hint (accordant with it) sophisticates. The modern geologist is so determined that the land shall move up and down, to account for *every* trace of marine formations above the present level of the sea, that whatever falls in with that view is accepted without challenge or investigation, while the most elaborate display of facts that even *seems* or *tends* to hint at a different way of explaining such phenomena is made but light of. The error is part of a larger one, resting on an oracular dictum of modern times, that we can only explain ancient phenomena by causes which we see in operation at the present time; whereas the causes which actually operated may not be now under observation, and, if we confine ourselves solely to those now visibly working, we may pitch upon wrong ones. The paucity of theoretical wisdom in modern science is illustrated by such things. With regard to the change in the relative level of seas and lands, as it appears to be a universal phenomenon (for from every continent it is now reported), why may not the idea of a fall of the sea apply to it as well as the local one of a rise of the land, which Playfair only preferred because he thought the phenomenon local?* These gentlemen do not see that their own asser-

* Professor Playfair's remarks were as follows:—" The imagination naturally feels less difficulty in conceiving that an unstable fluid like the sea, which

tion of a so-great mobility in the crust of the earth actually involves a possible fall of the sea also, since a sinking of some great ocean-bed would undoubtedly produce a decadence of the surface of the ocean, and leave every shore on earth to some extent exposed. For my own part, trusting to reason and observation, I shall continue to disclaim an exclusive hypothesis ; prepared to find it ultimately ruled that both the sea falls and the land rises, and that a fall of the sea to the extent of some thousands of feet, by whatever means brought about, was actually one of the last of the great geological events.

2. *A Source of Possible Fallacy regarding the Level of the Baltic.*

I had some conversation with Professor Lovén regarding the proofs which exist of recent and continued change in the relative level of sea and land in Scandinavia. Like all northern men of science, he was well aware of the facts bearing upon this subject, and had given his accession to the conclusion now generally arrived at, that the phenomena depend upon a rise of the land, not a depression of the sea. Since Professor Playfair made his famous remark, that a depression of the sea cannot be of a local nature, while an uprise of the land may be so, the superior probability of the latter phenomenon has been generally seen and admitted. The conclusion was

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changes its level twice every day, has undergone a permanent depression in its surface, than that the land, the *terra firma* itself, has admitted of an equal elevation. In all this, however, we are guided much more by fancy than by reason ; for, in order to depress or elevate the absolute level of the sea, by a given quantity in any one place, we must depress or elevate it by the same quantity over the whole surface of the earth ; whereas no such necessity exists with respect to the elevation or depression of the land. To make the sea subside thirty feet all round the coast of Great Britain, it is necessary to displace a body of water thirty feet deep over the whole surface of the ocean. It is evident that the simplest hypothesis for explaining those changes of level is, that they proceed from the motion, upwards or downwards, of the land itself, and not from that of the sea. As no elevation or depression of the sea can take place but over the whole, its level cannot be affected by local causes, and is probably as little subject to variation as any thing to be met with on the surface of the globe."

It is evident here that the learned professor only makes a choice between hypotheses with a regard to their comparative simplicity, as accounting for phenomena assumedly local. He shews no reason why the sea may not fall and rise, though he thinks it less probable than local rises and depressions of the earth's crust. It is on such a basis that the English geologists have established their conclusion, on which they can endure no breath of scepticism, that there *can* be no change in the level of the sea. A late president of the Society thus spoke, in his annual address in 1847 : 'Notwithstanding that this unanswerable doctrine was thus clearly laid down so far back as 1802, we still find geologists of authority speaking of the sea having risen or fallen, in their endeavours to explain certain phenomena," &c. A very grave delinquency indeed ! I must, nevertheless, profess my total inability to trace the logic which makes Mr Playfair's remarks an "unanswerable doctrine."

clenched by the actually observed uprise of a large tract in Chili in 1820, and by the ascertained rising and falling in recent times of a part of the coast of Naples. I readily admitted to Professor Lovén the value of the facts observed with respect to the level of the Baltic, the force of Playfair's remark, and the importance of the observations in South America and Italy. Still, I said, there was a source of possible fallacy open regarding the level of the Baltic, which I was surprised had not as yet been thought of. The Baltic was an inland sea, and it was ascertained that inland seas do not always maintain the same mean level as the outer ocean. It was remarkable that not one of the observations of the Scandinavian investigators, nor of those instituted by Professor Johnston and Sir Charles Lyell, was made beyond the space within which the inland and tideless character of this sea prevails. As cases shewing the inequality in question, reference may be made to the Red Sea, found by M. Lepere to be 26½ feet above the level of the Mediterranean. The Mediterranean itself has been set down by a French surveying party as within 2 feet of the level of the ocean at Amsterdam—a difference too small to have any stress laid upon it; but it is a startling fact that three different surveys of the most rigid character assigned differences between the levels of the Adriatic and Mediterranean, revolving a very little way from a mean of 8½ metres, or about 26 feet, the Adriatic, being, like the Red Sea, at the superior height.* Considering, indeed, the nature of this evidence, we cannot be rigidly certain that these differences are as they appear. They are, however, sufficient to give us reason for supposing that the Baltic—the throat of a vast number of rivers (the fifth part of Europe is drained into it), and furnished with but narrow communications towards the outer ocean—may heretofore have been kept up at a somewhat higher level than the ocean; a condition to which its temperate clime is of course favourable. From changes in the natural drainage of the basin, whether from variations of climate or otherwise, from a clearing of the channels of communication, or some other local causes, the abnormal level may be diminishing, and hence it may be that so many parts are shallowing, and so many rocks formerly submerged are coming above the surface. All this is purely hypothetical;† but I submitted to Mr Lovén that it makes out a case for inquiry, because it is not comfortable to sit down with a conclusion on a scientific subject while any source of fallacy stands yawning behind us.

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* Humboldt's Asie Centrale, ii. 301.

† It must also be admitted that the shallowing of the Baltic is only announced in some parts of the coast, not in all. The whole of the German shore, for instance, is said to betray no mark of change. I do not, however, feel assured that this partial exhibition of the phenomena as respects locality is well established.

I proposed, with all deference, that the Academy of Sciences should endeavour to induce the Government to execute a levelling survey from the medium level of the sea at Trondhiem to the ordinary or medium level of the Gulf of Bothnia, with a view to ascertaining if these were identical; in which event, of course, the conclusion would stand good as at present; whereas a contrary result, if at all considerable in degree, would shew that the observed facts were liable to be accounted for without necessarily presuming a movement of the land. The learned Professor was at first exceedingly unwilling to entertain my doubts; but he at length admitted their weight, and undertook to make a report on the subject to the Academy, and for this I supplied him with the materials. Of the result I have as yet heard nothing; but at least, I trust, I may take this means of warning all who feel an interest in the subject against a too implicit trust in the theories which have been somewhat over-confidently, if I may not say somewhat arrogantly, maintained with regard to the changes of the apparent level of the Baltic.—(*Chambers's Edinburgh Journal for March* 1850.)

Chemical Notices. By ALEXANDER KEMP, Esq., Teacher of Practical Chemistry, and Assistant to Dr Gregory, in the University of Edinburgh. Communicated by the Author.

1. *On the Purification of Oil of Vitriol from Nitric Acid.*

In consequence of the oil of vitriol of commerce containing a considerable amount of nitric acid, which renders it unfit for many of its applications, I have been induced to make a few experiments on the different methods commonly recommended for its removal.

In the last edition of the Edinburgh Pharmacopœia, we are directed to boil the acid along with a small quantity of sugar, until the colour first produced disappears. This method was tried by Dr Schlossberger and myself, in Professor Gregory's laboratory in 1845, but without success, as we found that we could not remove the whole of the nitric acid, even by continuing the process for several days, unless so much sugar was added as permanently blackened the liquid. I again tried this method a short time ago, and increased the proportion of sugar until the acid became quite black, and gave off a large quantity of sulphurous acid; after removing this, by long-continued boiling, the liquid was diluted with distilled water, but from its dark colour, I found it to be impossible to test for nitric acid by the solution of proto-sulphate of iron, or by narcotine. It then occurred to me, that as chlorine is always produced when nitric or nitrous acids act on common salt, that this might afford me an indirect mode of ascertaining whether the whole of the nitric acid had been removed.

A quantity of oil of vitriol, previously boiled with sugar as described, was diluted with water, and caused to act on purified common salt, as directed by Professor Gregory in his process for preparing pure hydrochloric acid, but it was found that the acid obtained had a yellow colour, and when mixed with protosulphate of iron, it became nearly black, which does not occur when hydrochloric acid free from nitric is used. I explain the fact that nitric acid remained in a liquid containing much sulphurous acid, in the following way: Sulphurous acid, when boiled with oil of vitriol, does not destroy the nitric acid entirely, or nearly so; but if the oil of vitriol be diluted with water, the sulphurous acid then decomposes the nitric acid.

The second method is one recommended a few years ago by M. Pelouze, and consists in boiling the impure oil of vitriol with the addition of sulphate of ammonia, which it appears first changes into nitrate of ammonia, and subsequently is decomposed into nitrous oxide and water. I have tried this method, and find that when a considerable quantity of the sulphate is added, the whole of the nitrous compounds are removed; but it is exceedingly difficult, if not impossible, to add the exact quantity requisite to decompose the whole of the nitric acid, without the risk of leaving some ammonia in the liquid.

The first experiment I made, was with the view of learning whether carbonic oxide in the nascent state, might not decompose the nitric acid contained in strong oil of vitriol. About four fluid ounces of the acid were heated in a flask, and successive quantities of oxalic acid added, but on testing, it was found the nitric acid had not been removed.

Having frequently observed, on diluting large quantities of oil of vitriol with water, that red vapours were disengaged, I made several experiments, by mixing various proportions of these two liquids, and afterwards boiling them so as to favour the expulsion of the vapours, but although a part of the nitrous compound was removed, enough remained to render the liquid dark-coloured on adding sulphate of iron.

After the trial of a great many other bodies, including sulphuretted hydrogen, in various ways, I found that the only substance likely to answer the purpose, was sulphurous acid. The experiments made with this substance led to the result that, if the oil of vitriol be diluted to the specific gravity of 1·715, or thereabouts, and a stream of sulphurous acid passed through it, the whole of the nitric, nitrous, or hyponitrous acid will be reduced to binoxide of nitrogen, which, along with the excess of sulphurous acid, may be totally removed by boiling.

For the removal of the nitric acid from oil of vitriol, either of the two following methods may be adopted :—

1. Three volumes of the acid are to be mixed with one of water and sulphurous acid transmitted through the liquid until it smells strongly, it is then to be boiled till all odour of sulphurous acid has disappeared.

2. Instead of diluting the acid with water, the same bulk of a saturated solution of sulphurous acid may be used, which has the advantage that a supply of the solution may be kept for use when required.

In preparing an acid free from lead, as well as nitric acid, it will be found necessary to dilute the oil of vitriol, with half its bulk of water. As sulphate of lead is slightly soluble in oil of vitriol of specific gravity 1·715 ; mixed with half its bulk of water, its density is about 1·650. The oil of vitriol used in these experiments was from one of the first manufactories in the country, and had a specific gravity of 1·835.

2. *On the Absence of Iron in Hydrochloric Acid, prepared by Professor Gregory's process.*

Professor Gregory recommended the use of patent salt, as free from iron, to yield a pure acid. But, although an acid free from iron may thus be obtained, I was struck with the fact that the sulphate of soda remaining in the flask had always a yellowish colour. On testing it, I found iron present in the residue in every case. It was therefore plain that even the patent salt was not free from iron, and that the absence of iron in the hydrochloric acid, made from such materials, depended on some cause which prevented the perchloride of iron from passing over. This cause, Professor Gregory suggests, may be the low temperature at which the operation is carried on, or the probable effect of an excess of sulphuric acid in preventing the formation of the perchloride of iron. At all events, I found that, even when iron filings, or peroxide of iron were added to the materials in considerable quantity, no iron could be detected in the hydrochloric acid. This was the case, even when the oil of vitriol contained so much nitric acid as to yield a very dark-coloured product, coloured by free chlorine and nitrous acid.

This observation is practically valuable, since it enables us to obtain, by Professor Gregory's process, perfectly pure and colourless hydrochloric acid from the commonest sea-salt, although it contains a good deal of iron, and thus still further to reduce the cost of a reagent so indispensable as pure hydrochloric acid.

Professor Gregory formerly detected traces of iron in the hydrochloric acid made with the common kitchen salt, which induced him to use patent salt. This iron may have been carried over as perchloride, in consequence of the distillation having been pushed too far, that is, till the temperature rose sufficiently. Or, as sulphocyanide of potassium was the test employed, the test may have contained a trace of iron. I have tested with galls after neutralising with ammonia, and the other tests usually employed, and could not detect any compound of iron in the acid.

SCIENTIFIC INTELLIGENCE.

ASTRONOMY.

1. *On the Extinction of Light in the Atmosphere. By W. S. Jacob, Esq., H.E.I.C. Astronomer, Madras. Communicated by Professor Piazzi Smyth.*—In a letter dated Madras, November 1849, Captain Jacob says, "I have been much interested in reading, lately, Professor Forbes's paper in the Philosophical Transactions, 1842, Part 2, on the Extinction of Light and Heat in the Atmosphere." As his results agree very closely with those of my experience on the Trigonometrical Survey of India, and which, though not founded on any precise measures, being still the conclusions of some years' experience, are perhaps worth noticing, particularly when they agree with the results of more exact measures.

On commencing work with heliotropes in 1837, I soon found that for long distances it was necessary to enlarge the apertures *more* than in the simple ratio of the distance (though such was Colonel Everest's practice); and before the end of the first season, I had formed a scale of apertures for corresponding distances, which afterwards needed very little alteration, but when finally corrected by subsequent years' observation, stood as follows:—

Aperture. Inches.	Maximum Distance. Miles.	Maximum Distance without Absorption.
0·5	15	15
1·0	23	30
2·0	33	60
4·0	45	120
8·0	60	240

Our heliotropes were circular glass mirrors, 8 inches in diameter; and for the smaller apertures, diaphragms were used between the heliotropes and the observer. At the distances stated the light was just visible to the naked eye in clear weather, and when seen over a *valley:* if the ray *grazed* near the surface, the light was much reduced. On one occasion I employed a heliotrope at $6\frac{1}{4}$ miles, and used an aperture of $\frac{1}{6}$ of an inch, and found it rather brighter than usual, so that probably $6\frac{1}{2}$ or 7 miles would be the normal distance for that size.

This agrees well enough with the rest of the scale, but there is no need to employ a conjectural quantity; and if the rate of absorption corresponding to the above be computed, so close an agreement will be found, as may entitle the numbers to be looked on as something better than mere estimates,—as the results, indeed, of a species of observation.

The mean of the whole shews a loss of ·0610 in passing through one mile of atmosphere; with the barometer at 27·0 inches (that being about the average height of my stations), but reduced to 30·0 inches, the quantity will be ·0671.

Hence the loss of light in passing from the zenith through a homogeneous atmosphere of 5·2 miles will be ·303, or only about one per cent. less than Professor Forbes's result. And as my air was considerably drier than his (the mean humidity being not much above ·30 instead of ·56), this will probably account for the difference; and, at any rate, the agreement is much closer than could have been expected.

I once mentioned this matter to Captain Waugh, the present Surveyor-General of India, then my fellow-assistant; but he not only had not noticed the thing, but did not even apprehend my meaning. He assented to my remark on the *loss* of light in passing through the atmosphere, but asserted that the aperture should vary as the distance, thus allowing for *no* loss! 0·1 inch per mile answered, he said, for all distances that he had tried! So it might answer for the distances most usually occurring on the Survey; for 4 inches would be proper for 40 miles, and·2 inches not much too bright at 20, and it is not often that these limits would be passed. Yet it is hardly possible to conceive that he should not have noticed the different intensity of the lights; had not his opportunities been perhaps rather unfavourable, as his work lay chiefly in plains, where, as mentioned above, the light of a grazing ray is very much reduced, and the atmospheric effect would therefore be mixed up with disturbing local causes.

I myself was much astonished at first discovering that the air had so great absorbent powers, and many ideas are suggested by the fact. We see at once how easily many of the planets may be rendered habitable to beings like ourselves. Mars, *e. g.*, may enjoy a temperature little inferior to our own, by having a *less* absorbent envelope; and Venus may be kept as cool as we are, by having one *more* so.—(*Proceedings of Roy. Soc. Edin.*, vol. ii., No. 36.)

METEOROLOGY.

2. *Climate of Australia. By John Gould, Esq., F.R.S., F.G.S., &c.*—In a country of such vast extent as Australia, spreading over so many degrees of latitude, we might naturally expect to find much

diversity in the climate, and such is really the case. Van Diemen's Land, from its isolated and more southern position, is cooler, and characterized by greater humidity than Australia; its vegetation is therefore abundant, and its forests dense and difficult of access. The climate of the Continent, on the other hand, between the 25th and 35th degrees of latitude, is much drier, and has a temperature which is probably higher than that of any other part of the world, the thermometer frequently rising to 110°, 120°, and even 130° in the shade, and this high temperature is not unfrequently increased by the hot winds which sweep over the country from the northward, and which indicate most strongly the parched and steril nature of the interior. Unlike other hot countries, this great heat and dryness is unaccompanied by night-dews, and the falls of rain being uncertain and irregular, droughts of many months' duration sometimes occur, during which the rivers and lagoons are dried up, the land becomes a parched waste, vegetation is burnt up, and famine spreads destruction on every side. It is easier for the imagination to conceive than the pen to depict, the horrors of so dreadful a visitation. The indigenous animals and birds retire to the mountains, or to more distant regions exempt from its influence. Thousands of sheep and oxen perish, bullocks are seen dead by the roadside, or in the dried-up water holes, to which, in the hope of relief, they had dragged themselves, there to fall and die; trees are cut down for the sake of the twigs as fodder; the flocks are driven to the mountains, in the hope that water may there be found, and every effort is made to avert the impending ruin; but, in spite of all that can be done, the loss is extreme. At length a change takes place, rain falls abundantly, and the plains, on which, but lately, not a blade of herbage was to be seen, and over which the stillness of desolation reigned, become free with luxuriant vegetation. *Orchideæ*, and thousands of flowers of the loveliest hues are profusely spread around, as if nature rejoiced in her renovation, and the grain springing up vigorously, gives promise of an abundant harvest. This change from sterility to abundance, in the vegetable world, is accompanied by a correspondent increase of animal life; the waters become stocked with fish, the marshy districts with frogs and other reptiles, hosts of caterpillars and other insects make their appearance, and, spreading over the surface of the country, commence the work of devastation, which, however, is speedily checked by the birds of various kinds that follow in their train. Attracted by the abundance of food, hawks, of three or four species, in flocks of hundreds, depart from their usual solitary habits, become gregarious and busy at the feast, and thousands of Straw-necked Ibises (*Ibis spinicollis*) and other species of the feathered race, revel in the profusion of a welcome banquet. It must not, however, be imagined that this change is effected without its attendant horrors; the heavy rains often filling the river beds so suddenly that the onward-pouring flood carries with it everything that may impede its course, and woe to the unhappy settler whose house or grounds may lie within the influence of the overwhelming floods!

So little has as yet been ascertained respecting the climatology of Western, North-Western, and Northern Australia, that it is not known whether they also are subject to these tremendous visitations; but as we have reason to believe that the intertropical parts of the country are favoured with a more constant supply of rain, as well as a lower degree of temperature, it is probable that they do not there occur.—(*Vide Gould's Birds of Australia,—a magnificent and important work, Price* £100 *sterling.*)

GEOLOGY.

3. *On the Volcanic Formations of the Alban Hills, near Rome.*—Professor J. D. Forbes, in a memoir on the Alban Hills, thus sums up the general results of his observations:—

"In the first place, it appears that the Alban volcano (for it is essentially one) has acted throughout a great period of time; for not only has it evidently repeatedly changed its form and materials of eruption, but it is surrounded by knolls of basaltic formations which seem to indicate very ancient and very repeated ejections, without taking the regular form of craters. Such are probably Monte Algido, Civita Lavinia, Monte Giove (Corioli), the Capuccini of Albano, Rocca Priore, Colonna, and perhaps even Capo di Bove, and several open craters, such as one a little below Albano, the Lago Cornufelle near Frascati, the Lake of Gabii, and one near Colonna, which, on the authority of Ponzi, appear to have ejected peperino. The horse-shoe form of the old crater of the Alban Mount, which, whether formed by the elevation process or not, appears to be composed of beds of basalt, lapilli, tuff, or peperino, and here and there of the lava called *Sperone*, gave way, like that of Somma, on the western or seaward side, and I cannot but think it in no small degree probable, that the vast lava beds which lie under Nemi and Genzano, and which dip at a small angle under Monte Cavo, are part of the dislocated walls of the ancient crater displaced by the convulsion which rent it on the western side, and which was accompanied by a prodigious fluid discharge of peperino, which then formed the strata of La Riccia and Albano, and which, overwhelming the broken-down wall of the ancient crater, formed at the same time the Monte Gentile, and the peperino beds above Nemi. This is confirmed by the prodigious lava blocks imbedded in these rocks, which bespeak the violence of the convulsion during which they were formed. Ages later, the present summit of Monte Cavo and the crater of the Campo d'Annibale were formed, and the latter gave out its currents of *tefrine* or grey basalt, and raised the crater of La Tartaruga and others in the valley of La Molara, and in the central crater; at the same time ejecting great volumes of purverulent lapilli. It may have been coeval with these perfectly regular and comparatively modern eruptions, or it may have preceded them, that, after a period so long that the surface of the ancient eruptions of peperino were covered with vegetation and timber, the tremendous outbursts which forced open the craters of Albano and Nemi took place, the former producing some slight ejections of peperino or boiling mud, near Castel Gaudolfo; and at the same time a separate orifice, opening at the foot of Monte Cavo, may have discharged into the valley of Marino the remarkable variety of peperino described in this paper, and containing vegetable stems. A long, perhaps even a final, repose succeeded this paroxysm. Even from the very dawn of Italian history these scenes of previous turmoil and desolation appear to have enjoyed profound tranquillity, and to have been immemorially covered with impenetrable groves sacred to the sports of Diana.

"It will be seen, then, that we admit tufas or peperinos of three very different periods, one of which is coeval with, or even anterior to, the formation of the exterior cone; another largely developed, which accompanied the great breach in it towards the sea; and a third, which probably produced some local streams, such as that of Marino, which has evidently flowed since the ground took its present configuration, and was covered with plants. Of lavas, likewise, we must admit at least three periods; 1*st*, the compact basalts of the outer circuit, which, if Von Buch's theory be correct, have flowed under a less inclination than they at present have; 2*dly*, The well-marked leucitic, or partridge-eyed lavas, which form the interior circuit; and, 3*dly*, the compact basaltic lava which flows past Rocca di Papa towards Grotta Ferrata, which is possibly coeval with the dikes occurring at Capo di Bove and elsewhere. This leaves the origin of the *lava sperone* still uncertain. It is undoubtedly one of the more recent products, for it not only overlies the whole of the old basaltic series at Tusculum and Nemi, but the leucitic lavas of the newer cone at Rocca di Papa. The easiest solution would be to consider it as a scoriform basalt; but even to this there are difficulties, not only mineralogical, but from position. For how can we connect the mantle-shaped covering of Monte Cavo up to its highest point, with the basalt, which nowhere attains a height (so far as I know) within several hundred feet of it? It is still more difficult to conceive any continuity between the sperone of the central cone and that of Tusculum, which is separated from it by the great valley of La Molara."—(*Proceedings of Roy. Soc. Edin.*, vol. ii., No. 35.)

4. *On Infusorial Deposits on the River Chutes in Oregon. By M. Ehrenberg* (Monatsb. Acad., Berlin, Feb. 1849, p. 76).—

Ehrenberg first draws attention to the results of his former researches, that the Rocky Mountains are a more powerful barrier between the two sides of America, than the Pacific Ocean between America and China; the infusorial forms of Oregon and California being wholly different from those on the east side of the mountains, while they are partly identical with Siberian species. This fact is confirmed by his examinations of the earth from the gold region of California, and from the Chutes river of Oregon, obtained by Fremont. The latter deposit is situated at an elevation of 700 to 800 feet, and constitutes a bed 500 feet thick of porcelain clay. It is overlaid by a layer of basalt 100 feet thick.

Prof. Bailey, who examined this material for Fremont, reported that it consisted of fresh-water infusoria, and many species were distinguished.* Ehrenberg, on farther investigation, has made out seventy-two species of Polygastrica with siliceous shells, sixteen species of Phytolithuriens, and three of crystalline forms. The more prevalent species are *Discoplea oregonica*, *Gallionella granulata*, *G. crenata*, *Eunotia Westermanni*, *Cocconema asperum*, &c. The *Discoplea* and *Raphoneis oregonica* are the only two species characteristic of the locality. The beds are more recent than those of the Klakamus river, a few miles from the Falls of the Willammet.—(*American Journal of Science and Arts,* vol ix., No. 25, Second Series, p. 140.)

ZOOLOGY.

5. *Low State of Development of Mammals and Birds in Australia and New Zealand.*—Geological researches into the structure of the globe, shew that a succession of physical changes have modified its surface from the earliest period up to the present time, and that these changes have been accompanied with variations not only in the phases of animal and vegetable life, but often in the development also of organization: and as these changes cannot be supposed to have been operating uniformly over the entire surface of the globe in the same periods of time, we should naturally be prepared for finding the now existing fauna of some regions exhibiting a higher state of development than that of others; accordingly, if we contrast the fauna of the old continents of geographers with the zoology of Australia and New Zealand, we find a wide difference in the degree of organization which creation has reached in these respective regions. In New Zealand, with the exception of a *Vespertilio* and a *Mus* which latter is said to exist there, but which has not yet been sent to this country, the most highly organized animal

* Fremont's Second Expedition, p. 302.

hitherto discovered, either fossil or recent, is a bird; in Australia, if compared with New Zealand, creation appears to have considerably advanced, but even here the order *Rodentia* is the highest in the scale of its indigenous animal productions; the great majority of its quadrupeds being the Marsupiata (kangaroos, &c.) and the *Monotremata*, (*Echidna*, and *Ornithorynchus*), which are the very lowest of the Mammalia; and its ornithology being characterized by the presence of certain peculiar genera, *Talegalla*, *Leipoa*, and *Megapodius;* birds which do not incubate their own eggs, and which are perhaps the lowest representations of their class,* while the low organization of its botany is indicated by the remarkable absence of fruit-bearing trees, the *Cerealia, &c.—(Gould's Birds of Australia.)*

6. *Migratory Birds of Australia*, &c.—Mr Gould gives the following summary of the distribution of the birds of Australia. 385 species inhabit New South Wales, 289 South Australia, 243 Western Australia, 230 Northern Australia, and 181 Van Diemen's Land; and that of these, 88 are peculiar to New South Wales; 16 to South Australia; 36 to Western Australia; 105 to Northern Australia; and 32 to Van Diemen's Land.

The great excess in the number of species inhabiting New South Wales is doubtless attributable to the singular belt of luxuriant vegetation, termed brushes, which stretches along the southern and south-eastern coasts, between the ranges and the sea, and which is tenanted by a fauna peculiarly its own.

Although this part of the Continent is inhabited by a larger number of species than any other, it is a remarkable fact that the species peculiar to Northern Australia are much more numerous than those peculiar to New South Wales.

It is curious to observe also, that, while Southern Australia is inhabited by a much larger number of species than Western Australia, those peculiar to the former are not half so numerous as those peculiar to the latter.

The more southern position, and, consequently, colder climate of

* The genera, *Talegalla*, *Leipoa*, and *Megapodius*, form part of a great family of birds inhabiting Australia, New Guinea, Celebes, and the Philippine Islands, whose habits and economy are most singular, and differ from those of every other group of birds which now exist upon the surface of the earth. In their structure they are most nearly allied to the *Gallinaceæ*, while in some of their actions, and in their mode of flight, they much resemble the *Rallidæ;* the small size of their brain, coupled with the extraordinary means employed for the incubation of their eggs, indicates an extremely low degree of organization.

The three species of the family inhabiting Australia, although referable to three distinct genera, have many habits in common, particularly in their mode of nidification, each and all depositing their eggs in mounds of earth and leaves, which, becoming heated either by the fermentation of the vegetable matter or of the sun's rays, form a kind of natural hatching apparatus, from which the young at length emerge fully feathered, and capable of sustaining life by their own unaided efforts.

Van Diemen's Land, will readily account for the paucity of species found in that island.

By the term peculiar, I do not mean to convey the idea that the birds are strictly confined to the respective countries, but that as yet they have not been found elsewhere.

Independently of the vast accession of birds attracted by the great supply of food, as mentioned above, there are many species which make regular migrations, visiting the southern parts of the Continent and Van Diemen's Land during the months of summer, for the purpose of breeding and rearing their progeny, and which retire again northwards on the approach of winter, following, in fact, the same law which governs the migrations of the species inhabiting similar latitudes of the Old World. There are also periods when some species of birds appear to entirely forsake the part of the country in which they have been accustomed to dwell, and to betake themselves to some distant locality, where they remain for five or ten years, or even for a longer period, and whence they as suddenly disappear as they had arrived. Some remarkable instances of this kind came under my own observation. The beautiful little warbling Grass Parrakeet (*Melopsittacus undulatus*), which, prior to 1838, was so rare in the southern parts of Australia, that only a single example had been sent to Europe, arrived in that year in such countless multitudes on the Liverpool plains, that I could have procured any number of specimens, and more than once their delicate bodies formed an excellent article of food for myself and party. The *Nymphicus Novæ Hollandiæ* forms another case in point, and the Harlequin bronze-winged pigeon (*Peristera histrionica*) a third; this latter bird occurred in such numbers on the plains near the Namoi in 1839, that eight fell to a single discharge of my gun; both the settlers and natives assured me that they had suddenly arrived, and that they had never before been seen in that part of the country. The Aborigines who were with me, and of whom I must speak in the highest praise, for the readiness with which they rendered me their assistance, affirmed, upon learning the nature of my pursuits, that they had come to meet me. The *Tribonyx ventralis* may be cited as another species whose movements are influenced by the same law. This bird visited the colony of Swan River in 1833, and that of South Australia in 1840, in such countless myriads, that whole fields of corn were trodden down, and destroyed in a single night; and even the streets and gardens of Adelaide were, according to Captain Sturt, alive with them.

BOTANY.

7. *On the Gamboge Tree of Siam. By Dr Christison.*—Although Gamboge has been known in European commerce for nearly two centuries and a half, and its applications in the arts have been ex-

tended in recent times, the tree which produces it is still unknown to botanists.

The late Dr Graham, in 1836, was the first to describe accurately a species of Garcinia, which inhabits Ceylon, and which is well known there to produce a sort of Gamboge, not, however, known in the commerce of Europe. Resting on a peculiarity in the structure of the anthers, which are circumscissile, or open transversely by the separation of a lid on the summit, he constituted a new genus for this plant, and called it *Hebradendron cambogioides*. At the same period the author examined the properties of this Gamboge, and found that it possesses the purgative action of the commercial drug in full intensity, and that the two kinds agree closely also, though not absolutely, in chemical constitution.

At an earlier period Dr Roxburgh described, in his "Flora Indica," another species of Garcinia, under the name of *Garcinia pictoria*, which inhabits the hills of Western Mysore, and which also was thought to produce a sort of Gamboge of inferior quality. In 1847 specimens of the tree and its exudation were obtained near Nuggur on the ghauts of Mysore by Dr Hugh Cleghorn of the East India Company's service; and the author, on examining the Gamboge, found it all but identical with that of Ceylon in physiological action, in properties as a pigment, and in chemical constitution. The same plant, with its Gamboge, was about the same time observed by the Rev. F. Mason, near Mergui in Tavoy, one of the ceded Burmese provinces.

A third species, inhabiting the province of Tavoy, and also producing a kind of Gamboge, was identified by Dr Wight in 1840 with Dr Wallich's *Garcinia elliptica,* from Sylhet, on the north-east frontier of Bengal. Its exudation was long thought to be of low quality. But, although this substance has not yet been examined chemically, it has been stated by Mr Mason to be, in his opinion, quite undistinguishable as a pigment from Siam Gamboge.

It is a matter of doubt whether Graham's character is sufficiently diagnostic to be a good generic distinction. But it was shewn by Dr Wight in 1840, that a well characterised section at least of the genus Garcinia consists of species which have "sessile anthers, flattened above, circumscissile, and one-celled;" and that all these species, and no others, appear to exude a gum-resin differing probably very little from commercial Gamboge.

Still the tree which produces Siam Gamboge, the finest and only commercial kind, continues unknown. A strong presumption however arose, that the last species was the Siam tree, as it grows in the same latitude with the Gamboge district of Siam, and not above 200 miles farther west. But if the information recently communicated to the author be correct, the Siam tree is a fourth distinct species of the same section. In December last he received from Mr

Robert Little, surgeon at Singapore, specimens taken from two trees which were cultivated there by Dr Almeida, a resident of the colony, and which were obtained by him "direct from Siam" as the Gamboge tree of that country. These specimens are not such as to allow of a complete description; yet they are sufficient to shew that the plant presents the characters of Wight's Gamboge-bearing section of the genus Garcinia; but that is not any of the species hitherto so fully described as to admit of comparison with it. The fruit is round, not grooved, crowned by a four-lobed knotty stigma, and surrounded by numerous sessile or subsessile aborted anthers, and by a persistent calyx of four ventricose fleshy sepals. The male flowers consist of a calyx of the same structure, a corolla of four ventricose fleshy petals, and a club-shaped mass of about forty subsessile anthers, closely appressed, connected only at the mere base, one-celled, flattened at the top, and opening by a circular lid along a line of lateral depression; and there is no appearance of an aborted ovary amidst them. These are the characters of the three species presently known. These three species very closely resemble one another in general appearance and special characters. The new species presents the same close resemblance to them all; and, in particular, its foliage is undistinguishable from that of *Garcinia elliptica*, the leaves being elliptic, acuminate, and leathery, exactly as described and delineated by Wight. But it differs from them all in the male flowers and fruit being peduncled. The male flowers are fascicled, and have a slender peduncle three-tenths of an inch in length. The single young fruit attached to one of the specimens has a thick fleshy peduncle, like an elongated receptacle, half as long as the male peduncle. All the other species hitherto described have both male and female flowers sessile or subsessile. As this difference cannot arise from a mere variation in the same species, the plant must be a new one. The evidence however that it produces Gamboge, and more especially the commercial Gamboge of Siam, is not yet complete; and, until further information on this point be obtained, which the author expects to receive in the course of the year, it appears advisable not to attach to it a specific name. A question may even arise whether the male flowers and the fruit here described may not belong to two species instead of one; but this is far from probable.—(*Proceedings of the Royal Soc. Edin.*, vol. ii., No. 36.)

AGRICULTURE.

8. *Analysis of Soils.*—One objection, which is often made to the analysis of soils, merits some attention. It is argued, that as the surface of a field is changing at almost every step, when you take a sample from one square yard, you are by no means sure that it all agrees with the next. A field is not a crystallised mineral of de-

finite composition,—it is a confused mixture of various substances, perhaps deposited at random from water. This objection, however, fortunately only has weight where absolute exactness is required. A similar objection applies, in nearly an equal degree, to the analysis of rocks, and yet it is found useful to know the composition of the feldspars, micas, and granites. Not that it is for one moment expected that the morsel of these rocks which may be examined agrees with the rest of the rock; the *average* composition of these rocks is the information sought, without pretending to arrive at a degree of exactness which does not exist.

It is the same with the analysis of soils, but with this difference, that the mean composition of soils, brought from different situations, will often vary to such a marked degree, as to convey nearly the same amount of information that would have been derived from the analysis, if even extreme exactness had been possible. Having thus reduced to its just importance the degree of exactness requisite in an analysis of a soil, it must be admitted that its usefulness, thus fairly estimated, must be limited to an extent varying with the special locality. For instance, in some neighbourhoods the soil will vary, most unfortunately, perhaps, half-a-dozen times in one field, whilst in others no change can be observed over many hundred acres. In this much must always be left to the judgment of the farmer; and, whilst it undoubtedly qualifies the advantage to be derived from the chemical analysis of a soil, yet it far from justifies any one in concluding hastily that no information is to be derived from it.—*Gasparin, Cour d'Agriculture, Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland*, No. 28, New Series, March 1850, p. 330.

9. *Notes on American Agriculture.*—The great difference between British and American farming is this,—that, while in the former, small farms, in the majority of instances, can only be obtained, such is their abundance and cheapness in the latter country, that even the small capitalist can easily obtain large tracts of land. This state of matters, viewed in a superficial way, would lead the inquirer naturally to suppose, that to the American farmer it would prove a benefit; but that it is the reverse, a cause of much loss of money and waste of land and time, the experience of past years has fully proved. Scientific educated farmers of America complain, that parties get possession of large tracts of land, and, while trusting to its extent and the natural richness of soil, they impoverish the land by overworking, without corresponding efforts to improve it,—not being able to perceive the benefit of thoroughly cultivating one small portion, they waste their energies by farming in a slovenly way hundreds, it may be, of acres. The result is, that the great majority of the small farmers in the older States are struggling to make both

ends meet at the year's end, upon poor land, made poorer every year by the same exertion which, if rightly applied, would make two stalks of corn grow where only one grew before. That this system, or rather want of system, has a remarkable influence in the aggregate production of wheat and other grains in the United States, is proved by the fact, that the average produce of such in England is double that of the United States,—in the former about 28 or 30 bushels, in the latter about 14 or 15,—shewing, what *we* have found out by experience, that superior cultivation on an old soil is an overmatch for the natural resources of "virgin land" or good soil, with slight or careless tillage. This maxim is gaining ground by slow but sure steps in the States; agriculturists are beginning to see that it is better diligently to cultivate one acre, and of course profitably, then spend fruitless and ill-directed exertions on a hundred. Men, who were formerly the veriest slaves to industry, getting up early and lying down late, eating anything but the bread of idleness, are beginning to sell portions of their large farms, and are studying agriculture as a science, not, as formerly, a mere haphazard undertaking. Manure, formerly wasted, is now carefully kept and skilfully applied; draining is carried into effect thoroughly, and all the improvements made in this country are eagerly sought after by the educated farmers. The result of this new spirit has been exemplified in a striking degree during the last twenty years. Since that period a spirit of enterprise and degree of improvement has been manifested by the farming interest. The actual products of the soil in the old States, north and east of Washington, have probably been doubled; and so has also, in some of the States, the aggregate value of land. The old States in the south have been applying their energies in earnest to the renovation of their impoverished lands.—(*The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland*, No. 28, New Series, p. 357.)

MISCELLANEOUS.

10. *Funeral Obsequies of the celebrated Danish Poet, Oehlenschläger.*—The Danish Oehlenschläger is dead. The most fertile and famous dramatic poet that the Scandinavian kingdoms have produced. He died of apoplexy in the seventy-first year of his age. A poet counts for something in Scandinavia. Such marks of public mourning as we reserve here for the more material royalties, have signalised the Danish loss, and the people's sense of it. The three theatres of Copenhagen were ordered to be closed for a week, and all other public amusements were suspended for the same space of time. The poet was accompanied to his tomb, in the church of Fredericksburg, by the largest attendance that has been seen in Copenhagen since the funeral of Thorwaldsen. Upwards of twenty thousand

persons, a sixth of the entire population of the capital, representing every class of the community, from the crown Prince downwards,—the ministers of State, with their president at their head, the diplomatic body, the council of State, the clergy, the professors and pupils of the University, and of other schools, and those of the Royal Academy of Fine Arts,—all waited on the dead poet to his grave. The streets through which the procession passed were strewed with sand and green boughs, and the houses hung out black flags hemmed with silver. The deceased poet was born in 1778, at the royal residence of Fredericksburg, near Copenhagen, of which his father was intendant-general. He filled the chair of Œsthetics at the University of Copenhagen. It was the last of his personal distinctions, but an honour to the country which conferred it, that he was a Knight of various orders of Scandinavian chivalry.—(*Athenæum*, No. 1163, p. 160.)—[Compare the manly and affectionate enthusiasm of the noble Danes for their great poet, with the paltry and vulgar public expressions of regard for our illustrious poets, painters, and philosophers, on conveying their remains to the grave.—*Edit.*]

11. *Glass as a Non-conductor.*—Mahanama, who wrote his history before A.D. 477, mentions that Sanghatissa, King of Ceylon (who was poisoned A.D. 246), placed a pinnacle of glass on the spire of Ruanwelli Dagoba, "to serve as a protection against lightning." This shews that the Cingalese were then aware that glass was a non-conductor of the electric fluid.—(*From Forbes' Recent Disturbances and Military Executions in Ceylon.* Blackwood, 1850. P. 51.) *Sir W. C. Trevelyan.*

12. *Mean Annual Export of Wool from the Faröe Islands.*

In Ten Years	Pounds.
From 1790 to 1800,	85,686
From 1819 to 1829,	92,776
From 1829 to 1839,	94,000
From 1839 to 1849,	115,000
In 1847,	113,000
In 1848,	130,200
In 1849,	153,200

—*In a Letter to Sir W. C. Trevelyan.*

13. *Outline of the Tamil System of Natural History.*—(Read before the Asiatic Society of Ceylon, December 1, 1849.)—The writer of this paper shews that, at a period long prior to the knowledge of natural history by Europeans, and before Aristotle had given his system to the world, the Tamils had classified as many animals, vegetables, and minerals, as were known, according to their external cha-

racters. It is true that the Tamils have now no faithful transcripts of their zoological works as they existed in olden times, but enough remains scattered over their general literature, amongst dictionaries and essays, to shew what has been lost; and the careful and laborious writer of this paper has done his best to collect and embody whatever is to be met with amongst those miscellaneous works, a task for which the thanks of the scientific world are due.

It appears that the Tamils had a twofold system of animated nature, embracing the Mythological and the Natural. The former makes the "*Gods*" a part of the zoological family: it divides all organized bodies into moveable and fixed, which are again subdivided into seven genera and many species. The natural portion of the system treated of, approaches nearly to that of Linnæus in many respects. All living beings are divided into four classes, and these again into numerous genera and species. The first class comprehends all which are *viviparous*, such as man, quadrupeds, the whale, the bat, the ray, &c. The second class embraces all which are *oviparous*, viz., birds, fishes, the snake, the frog, the crocodile, &c. The third class takes in all those which are engendered by heat and damp, as worms, maggots, gnats, &c. The fourth class comprises all which are *germiniparous*, such as trees and herbs. Quadrupeds are divided into classes by their habitat;—such as dwell in hilly countries; such as live in woodland country; those which are found in cultivated fields; in sandy deserts; and those which are found in the branches of trees. The birds are divided in like manner; whilst fishes are merely classed as salt-water or fresh-water fish. Trees are divided by a system which depends on the texture of the bark and the wood, and again subdivided according to whether they bear fruit or not, to whether they bear fruit with or without blossom, or if they bear fruit once only and die.—*Sir W. C. Trevelyan.*

List of Patents granted for Scotland from 20*th December* 1849 *to* 22*d March* 1850.

1. To Edward Lyon Berthon, of Fareham, in the county of Southampton, clerk, master of arts, "certain instruments for ascertaining and indicating the course or way, velocity, trim, and draught of ships, and the rate of currents; also for discharging water from ships, and for taking altitudes and levels at sea and on land."—20th December 1849.

2. To James Smith, of Deanston, in the county of Perth, presently

residing in Glasgow, "certain improvements in treating the fleeces of sheep, when on the animals."—20th December 1849.

3. To James Usher, of Edinburgh, gentleman, "improvements in machinery for tilling land."—24th December 1849.

4. To John Stoughton Christie, of No. 13 Craven Street, Strand, in the county of Middlesex, Esquire, "an improved construction of wrought-iron wheels and machinery for effecting the same;" being a communication from abroad.—24th December 1849.

5. To Richard Hobson, of Leeds, in the county of York, doctor of medicine, "certain improvements in the manufacture of horse-shoes, and in apparatus for taking the measurement of horse-shoes, or horses' hoofs." 26th December 1849.

6. To William Ackroyd, of Birkenshaw Mills, near Leeds, in the county of York, manufacturer, "improvements in dressing and cleaning worsted, and worsted mixed with cotton, and other fabrics, after they have been woven."—31st December 1849.

7. To John Barsham, of Kingston, in the county of Surrey, manufacturer, "improvements in separating the fibre from cocoa-nut husks." —31st December 1849.

8. To John Christopher, of Heavitree, in the county of Devon, formerly merchant and shipowner, "improvements in naval architecture." —31st December 1849.

9. To Alexander Brodie Cochrane junior, and Archibald Slate, both of Dudley, in the county of Worcester, engineers, "improvements in the manufacture of iron pipes or tubes."—31st December 1849.

10. To Joseph Burch, of Craig Works, near Macclesfield, engineer, "improvements in printing on cotton, woollen, silk, paper, and other fabrics and materials."—31st December 1849.

11. To Winceslas Le Baron de Traux de Wardin, of Liege, in the province of Liege, in the kingdom of Belgium, "certain improvements in looms for weaving linen, woollen, and cotton cloths, and in machines for preparing the yarns for such cloths before entering the loom, and in a machine for finishing grey and bleached linen cloths."—3d January 1850.

12. To William Henry Wilding, of the New Road, in the county of Middlesex, gentleman, "certain improvements in engines and machinery for obtaining and applying motive power."—4th January 1850.

13. To Charles Cowper, of Southampton Buildings, in the county of

Middlesex, patent agent, "improvements in machinery for raising and lowering weights and persons in mines, and in the arrangement and construction of steam-engines employed to put in motion such machinery, parts of which improvements are applicable to other useful purposes;" being a communication.—7th January 1850.

14. To Reuben Peant, of Holly Hall Colliery, near Dudley, in the county of Worcester, coalmaster, "improvements in making wrought and bar iron."—7th January 1850.

15. To Samuel Colt, of Trafalgar Square, in the county of Middlesex, gentleman, "improvements in fire-arms."—7th January 1850.

16. To Thomas Lightfoot, of Broad Oak, within Accrington, in the county of Lancaster, chemist, "improvements in printing and dyeing fabrics of cotton, and of other fibrous materials;" being a communication from abroad.—7th January 1850.

17. To Thomas Richardson, of the town and county of Newcastle-upon-Tyne, chemist, "improvements in the manufacture of Epsom and other magnesian salts, also alum and sulphate of ammonia."—11th January 1850.

18. To Jerome Andre Drieu, of Manchester, machinist, "certain improvements in the manufacture of wearing apparel, and in the machinery or apparatus connected therewith."—14th January 1850.

19. To Thomas Auchterlonie, of Glasgow, North Britain, manufacturer and calico-printer, "improvements in the production of ornamental fabrics."—14th January 1850.

20. To Andrew Barclay, of Kilmarnock, in the county of Ayr, North Britain, engineer, "improvements in the smelting of iron and other ores, and in the manufacture or working of iron and other metals, and in certain rotary engines, and fans, machinery, or apparatus as connected therewith."—16th January 1850.

21. To Peter Armand Le Compte de Fontainemoreau, of No. 4 South Street, Finsbury, English and Foreign Patent Office, "improvements in spinning fibrous substances;" being a communication from a foreigner residing abroad.—16th January 1850.

22. To Joe Sidebottom, of Pendlebury, in the county of Lancaster, manager, "certain improvements in steam-engines."—16th January 1850.

23. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in pumps, and in machinery or apparatus for working the same, which latter

improvements are also applicable for working other machinery;" being a communication from abroad.—18th January 1850.

24. To Robert Wilson, of Low Moor Iron-Works, Bradford, in the county of York, engineer, "improvements in steam-engines, boilers, and methods of preventing accidents in working the same."—21st January 1850.

25. To John George Barton, of the Regent's Park, in the county of Middlesex, gentleman, "improvements in dyeing, and dyeing materials;" being a communication from abroad.—21st January 1850.

26. To Joseph Clinton Robertson, of 166 Fleet Street, in the city of London, civil engineer, "improvements in machinery, apparatus and processes for extracting, depurating, forming, drying, and evaporating substances;" being a communication from a foreigner residing abroad.—23d January 1850.

27. To William Thomas Henley, of Clerkenwell, in the county of Middlesex, philosophical instrument-maker, "certain improvements in telegraphic communication, and in apparatus connected therewith, parts of which improvements may be also applied to the moving of other machines and machinery."—23d January 1850.

28. To Christopher Nickels, of York Road, Lambeth, in the county of Surrey, gentleman, "improvements in the manufacture of woollen and other fabrics."—24th January 1850.

29. To Ewald Reipe, of Finsbury Square, in the county of Middlesex, merchant, "improvements in the manufacture of steel;" being a communication from abroad.—24th January 1850.

30. To Benjamin Thomson, of Newcastle-upon-Tyne, civil engineer, "improvements in the manufacture of iron."—31st January 1850.

31. To Thomas Marsden, of Salford, in the county of Lancaster, machine-maker, "improvements in machinery for hackling, combing, or dressing flax, wool, and other fibrous substances."—1st February 1850.

32. To Elijah Galloway, of Southampton Buildings, Chancery Lane, in the county of Middlesex, civil engineer, "improvements in furnaces."—1st February 1850.

33. To Robert Fayrer, of Surrey Street, Strand, Commander in the Royal Navy, "improvements in steering apparatus."—1st February 1850.

34. To Macgregor Laird, of Birkenhead, gentleman, "improvements in the construction of metallic ships or vessels, and in materials for coating the bottoms of iron ships or vessels, and in steering ships or vessels."—6th February 1850.

35. To James Templeton, of Glasgow, in the kingdom of Scotland, manufacturer, "certain improvements in manufacturing figured fabrics, principally designed for the production of carpeting."—12th February 1850.

36. To William Henry Green, of Basinghall Street, in the city of London, gentleman, "improvements in the preparation of peat fuel, and in the mode of applying the products derived therefrom to the preservation of certain substances which are subject to decay;" being a communication from a foreigner residing abroad.—12th February 1850.

37. To Joseph Long, and James Long, of Little Tower Street, in the city of London, mathematical instrument-makers, and Richard Pattenden, of Nelson Square, in the county of Surrey, engineer, "an improvement in instruments and machinery for steering ships, which is also applicable to vices and other instruments, and machinery for obtaining power."—12th February 1850.

38. To James M'Donald, of the city of Chester, coachmaker, "certain improvements in the mode of applying oil or grease to wheels and axles, and to machinery, and in connecting the springs of wheel-carriages with the axles or axle-braces."—13th February 1850.

39. To William Mayo, of the firm of Mayo and Warrington, Silver Street, Wood Street, Cheapside, in the city of London, manufacturers of mineral aërated waters, "improvements in connecting tubes and pipes, and other surfaces of glass and earthenware, and in connecting other matters with glass and earthenware."—13th February 1850.

40. To Henry Attwood, of Goodman's Fields, in the county of Middlesex, engineer, and John Renton, of Bromley, in the same county, engineer, "certain improvements in the manufacture of starch, and other like articles of commerce, from farinaceous and leguminous substances."—14th February 1850.

41. To William Furness, of Lawton Street, Liverpool, builder, "improvements in machinery for cutting, tenoning, planing, moulding, dovetailing, boring, mortising, tonging, grooving, and sawing wood; also for sharpening and grinding tools or surfaces, and also in welding steel to cast-iron."—15th February 1850.

42. To Sir John M'Neill, Knight, of Dublin, and Thomas Barry, of Lyons, near Dublin, mechanic, "improvements in locomotive engines, and in the construction of railways."—15th February 1850.

43. To Benjamin Goodfellow, of Hyde, in the county of Chester, engineer, "certain improvements in steam-engines."—21st February 1850.

44. To Matthew Cochran, of High Street, Paisley, in the county of

Renfrew, North Britain, manufacturer, "improvements in machinery for the production of, and ornamenting of fabrics and tissues generally, parts of which improvements are applicable to the regulation of other machinery, and to purposes of a similar nature."—21st February 1850.

45. To Louis Napoleon Le Gras, of Paris, in the republic of France, civil-engineer, "improvements in the separation and disinfection of fæcal matters in the manufacture of manure, and in the apparatus employed therein."—21st February 1850.

46. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in manufacturing and refining sugar;" being a communication from abroad.—22d February 1850.

47. To Ernest Gaston, of the Erechtheum Club, Saint James's, in the county of Middlesex, gentleman, "certain improvements in artificial fuel, and in machinery used for manufacturing the same."—22d February 1850.

48. To Auguste Reinharde, of Leicester Street, Leicester Square, in the county of Middlesex, chemist, "improvements in preparing oils for lubricating purposes, and in apparatus for filtering oil and other liquids." —25th February 1850.

49. To Onesiphore Recqueur, of Paris, in the republic of France, civil engineer, "certain improvements in the manufacture of fishing-nets and other net fabrics."—25th February 1850.

50. To James Young, of Manchester, in the county of Lancaster, manufacturing chemist, "improvements in the treatment of certain ores, and other matters containing metals, and in obtaining products therefrom." —26th February 1850.

51. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in manufacturing leather;" being a communication from abroad.—27th February 1850.

52. To Eugine Albon, of Ponton Street, Haymarket, in the county of Middlesex, "improvements in increasing the draft in chimneys of locomotive and other engines."—4th March 1850.

53. To William Brown, of Airdrie, Lanarkshire, electrician, and William Williams the younger, of St Dennis, in the county of Cornwall, gentleman, "improvements in electric and magnetic apparatus for indicating and communicating intelligence."—4th March 1850.

54. To Alexandre Hediard, of Paris, in the republic of France, gentleman, "certain improvements in propelling."—5th March 1850.

55. To Thomas Richards, William Taylor, and James Wylde the younger, all of Falcon-Works, Walworth, in the county of Surrey, cotton-manufacturers, "improved rollers, to be used in the manufacture of silk, cotton, woollen, and other fabrics."—6th March 1845.

56. To James Hill, of Stalybridge, in the county of Chester, cotton-spinner, "improvements in, or applicable to, certain machines for preparing, spinning, and doubling cotton, wool, and other fibrous substances."—8th March 1850.

57. To John Fowler junior, of Melksham, in the county of Wilts, engineer, "improvements in draining land."—8th March 1850.

58. To Gerard John De Witte, of Brook Street, Westminster, in the county of Middlesex, gentleman, "improvements in machinery, apparatus, metallic and other substances, for the purpose of letterpress and other printing."—8th March 1850.

59. To Daniel Christie, of No. 3 St John's Place, Broughton, in the borough of Salford, and county of Lancaster, merchant, "improvements in machinery for preparing, assorting, straightening, tearing, teazing, doubling, twisting, braiding, and weaving cotton, wool, and other fibrous substances;" being a communication from abroad.—13th March 1850.

60. To Edward Omerod, of Manchester, in the county of Lancaster mechanical engineer, and Joseph Shepherd, of Chorlton-upon-Medlock, in the same county, mechanical engineer, "improvements in, or applicable to, apparatus for changing the position of carriages on railways."—13th March 1850.

61. To Frank Clarke Hills, of Deptford, in the county of Kent, manufacturing chemist, "an improved mode of compressing peat, for making fuel or gas, and of manufacturing gas, and of obtaining certain salts."—15th March 1850.

62. To William Handley, of Chiswell Street, Finsbury, confectioner, George Duncan, of Battersea, in the county of Surrey, engineer, and Alexander M'Glashan, of Long Acre, engineer, "improvements in the construction of railway-breaks."—20th March 1850.

63. To Warren Delarue, of Bunhill Row, in the county of Middlesex, "improvements in the manufacture of envelopes."—20th March 1850.

64. To James Higgins, of Salford, in the county of Lancaster, machine-maker, and Thomas Schofield Whitworth, of Salford aforesaid, mechanic, "certain improvements in machinery for preparing, spinning, and doubling cotton, wool, flax, silk, and similar fibrous materials."—22d March 1850.

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END OF VOLUME FORTY-EIGHT.

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