

"twelve lunar hours. This would, as we shall see, be the case if the geological hypothesis of a thin crust were true. The actual phenomena of tides, therefore, give a secure contradiction to that hypothesis. We shall see, indeed, presently, that even a continuous solid globe of the same mass and diameter as the earth, would, if homogeneous and of the same rigidity as glass or as steel, yield in its shape to the tidal influences three-fifths as much or one-third as much as a perfectly fluid globe; and further, it will be proved that the effect of such yielding in the solid, according as its supposed rigidity is that of glass or that of steel, would be to reduce the tides to about $\frac{2}{3}$ or $\frac{1}{3}$ of what they would be if the rigidity were infinite."

"§ 834. To prove this, and to illustrate this question of elastic tides in the solid earth, we shall work out explicitly the solution of the general problem of § 696 for the case of a homogeneous elastic solid sphere exposed to no surface traction; but deformed infinitesimally by an equilibrating system of forces acting *bodily* through the interior, which we shall ultimately make to agree with the tide generating influence of the moon and sun."

"§ 847. We intend in our second volume to give a dynamical investigation of precession and nutation, in which it will be proved that the earth's elastic yielding influences these phenomena in the same proportionate degree as it influences the tides. We have seen already that the only datum wanted for a comparison between their observed amounts and their theoretical amounts on the hypothesis of perfect rigidity, to an accuracy of within one per cent., is a knowledge of the earth's moment of inertia about any diameter within one per cent. We have seen that the best theoretical estimates of precession hitherto made, are in remarkable accordance with the observed amount. But it is not at all improbable that better founded estimates of the earth's moment of inertia, and more accurate knowledge than we yet have from observation, of the harmonic of the second degree in the expression of external gravity, may show that the true amount of precession (which is known at present with extreme accuracy) is somewhat smaller than it would be if the rigidity were infinite. Such a discrepancy, if genuine, could only be explained by some small amount of deformation experienced by the solid parts of the earth under lunar and solar influence. The agreement between theory on the hypothesis of perfect rigidity, and observation as to precession and nutation, are, however, on the whole so close as to allow us to infer that the earth's elastic yielding to the disturbing influence of the sun and moon is very small—much smaller, for instance, than it would be if its effective rigidity were no more than the rigidity of steel."

"§ 848. It is interesting to remark that the popular geological hypothesis, that the earth is a thin shell of solid material, having a hollow space within it filled with liquid, involves two effects of deviation from perfect rigidity, which could influence in opposite ways the amount of precession. The comparatively easy yielding of the shell must, as we shall see in our second volume, render the effective moving couple, due to sun and moon, much smaller than it would be if the whole interior were solid, and on this account must tend to diminish the amount of precession and nutation. But the effective moment of inertia of a thin solid shell containing fluid, whether homogeneous or heterogeneous, in its interior, would be much less than that of the whole mass if solid throughout; and the tendency would be to much greater amounts of precession and nutation on this account. It seems excessively improbable that the defect of moment of inertia due to fluid in the earth's interior, should bear at all approximately the same ratio to the whole moment of inertia, that the actual elastic yielding bears to the perfectly easy yielding which would take place if the earth were quite fluid. But we must either admit this supposition, improbable as it seems, or conclude (from the close agreement of precession and nutation with what they would be if the earth were perfectly rigid) that the defect of moment of inertia, owing to fluid in the interior, is small in comparison with the whole amount of inertia of the earth about any diameter; and that the deformation experienced by the earth from lunar and solar influence is small in comparison with what it would be if the earth were perfectly fluid. It is, however, certain that there is some fluid matter in the interior of the earth; witness eruptions of lava from volcanoes. But this is probably quite local, as has been urged by Mr. Hopkins, who first adduced the phenomena of precession and nutation to disprove the hypothesis that the solid part of the earth's mass is merely a thin shell."

The Kiltorcan Fossils

I HAVE just seen Mr. Carruthers' letter in your number of January 4th, to which I beg leave to reply.

In this communication it now appears that Mr. Carruthers' former remarks in the discussion upon Prof. Heer's paper were intended as a personal attack upon me; as he now states that on me alone rests the credit of misleading Prof. Heer by my erroneous determination of the Kiltorcan plant.

I have no hesitation in acknowledging to having referred the Kiltorcan plant in question to *Sagenaria veltheimiana*, and I think it very possible I may even now be correct. I will however now state the reason for my afterwards adopting Professor Schimper's name in preference. When that gentleman was in Ireland he spent some time in the examination of the Kiltorcan fossils, and did not then object to my determination of the species; it was afterwards, on my sending him a collection, that his further study of these fossils and comparison with the original species (of which I had only seen figures) enabled him to announce to me what he believed to be the distinctive characters in relation to the fruit which accompanied it, of those I had named *Sagenaria veltheimiana*; these fossils in his letter to me he referred to *Sagenaria*, and afterwards in his work "Traité Paléontologie Végétale," to *Knorria* under the name of *K. bailyana*. In the meantime I had read my report on these fossils at the British Association, and naturally adopted the generic name first applied to it by Prof. Schimper, which I afterwards corrected to *Knorria*, on his authority, in my "Figures of British Fossils," as Mr. Carruthers states.

In my letter to Professor Heer (June 1870) accompanying the specimens which I was requested to send him for his comparison with the Bear Island flora, I named those from Kiltorcan *Sagenaria bailyana* in accordance with Prof. Schimper's determination, whilst others from Tallow Bridge, co. Waterford, which he specially wished to see, I still referred to *S. veltheimiana*. I made him aware of Prof. Schimper's views on these plants, stating distinctly that they were originally referred by me to *S. veltheimiana*, but that Prof. Schimper, in consequence of his being enabled to compare the fruit accompanying it with that of the true *S. veltheimiana*, had arrived at the conclusion that it could not be that species, and therefore he had named it as a distinct one. Under these circumstances I cannot see how Mr. Carruthers can charge me with misleading Prof. Heer, who had the whole facts, with examples of the specimens from both localities, to draw his own conclusions from; with his acknowledged powers of discrimination, surely he was fully competent to judge for himself as to their correct identity.

The amount of Mr. Carruthers' knowledge on the subject about which he writes, is evidenced from his intimation that the fossil figured by me in the explanation to Sheet 187, &c., of the Irish Survey maps, is from Kiltorcan (co. Kilkenny), whereas it was sketched by me, on the spot, at Tallow Bridge (co. Waterford), where the section exposed exhibited a profusion of these plants in various conditions and stages of growth. The character of the rock in which they occur is totally different from that at Kiltorcan, the former being a grey shale, corresponding with the Lower Carboniferous shales, the latter a fine-grained greenish sandstone; neither has any of the associated Kiltorcan fossils, including the fish which are of typical Devonian or Old Red sandstone genera, ever been found at Tallow Bridge. I did however state in this memoir my belief that the *S. veltheimiana*, as identified by me at Tallow Bridge, was similar to the Kiltorcan plant in question, and also that it corresponded with the so-called *Knorria* of the Marwood beds, N. Devon.

With reference to Mr. Carruthers' announcement that *Sagenaria veltheimiana* is a "coal measure plant," I may remark that it is a particularly abundant fossil, occurring in various conditions, but seldom, if ever, met with in the typical coal series of Great Britain; I have identified it from the sandstones of the lower coal measures in the North of Ireland, as well as at various localities in the Lower Carboniferous shales of the Counties of Cork and Kerry. On the Continent, especially in Germany, it appears to be still more universal, and has been recorded under various names by fossil botanists, as Dr. H. R. Goepfert, in his "Fossile Flora der Silurischen der Devonischen," &c., mentions more than twenty synonyms for this species; moreover the same author states its occurrence to be "In der Kulmgrauecke, dem Kohlenkalke und in der jüngsten Grauwacke." Dr. F. Unger and Dr. H. B. Geinitz, the latter of whom personally inspected the collections from Kiltorcan and Tallow Bridge, also mentioned similar lower geological horizons at which it occurs; and Dr.

W. P. Schimper in the work before cited places it in Lepidodendron as a characteristic plant "des formations houillères inférieures (grauwacke culm) correspondant au calcaire carbonifère." It is therefore evidently more characteristic of the Lowest Carboniferous than of the coal measures; the older of these formations being considered by Sir Charles Lyell "as equivalents of the Lower Carboniferous, and were even formerly referred to the Devonian group."

I believe enough has now been said to show the part I took in misleading this eminent Professor, and I will leave those interested to judge between the merits of Mr. Carruthers' or Prof. Heers' classification, but in conclusion I must request to be allowed to state that prior to this gentleman's accusation against me, he made me a proposal to help him out of his controversy with Prof. Heer, and to "join him in a memoir to describe and figure the valuable materials I had collected;" this I had to decline, because it would not only have interfered with my official duties, but might also have drawn me into a discussion in which I had no interest, besides the probability of its committing me to what may prove to be erroneous opinions.

Dublin, Jan. 10 WM. HELLIER BAILY

Circumpolar Lands

IN NATURE of December 28 there is an interesting letter endeavouring to show that the land everywhere about the North Pole down to lat. 57° is rising. We know less about the South Polar regions, but there are active volcanoes in the Antarctic Continent, and Darwin has shown in his work on volcanic islands that the land and sea-bottom are rising. This appears to be at least a remarkable coincidence.

The earth must be cooling by the escape of the central heat in volcanic eruptions and hot springs, and by slow upward conduction through the strata. As it cools it must contract. Can any mathematical reason be assigned why the contraction should be least in the direction of the polar diameter? This would account for the rising of the land at the poles. J. J. MURPHY

English Rainfall

IN NATURE of the 11th inst. your reviewer, "J. K. L." (p. 201), makes a mistake in stating that the greatest English rainfall takes place at Cockley Bridge, Seathwaite. The greatest fall takes place at the Styne and on the north side of Styne Head, Seathwaite, Borrowdale; whereas the Cockley Bridge named by your reviewer is Seathwaite, Valley of the Duddon, and many miles from the place of greatest fall. He has evidently confounded the two Seathwaites. A reference to Mr. J. G. Symons' annual rainfall returns will show that the Seathwaite named is the one in Borrowdale. G. V. VERNON

Wanted, a Government Analyst

I AM a grocer in a small way in a country place, so that I retail almost all that comes under the name of food; and I am very desirous that all should be unadulterated and worth its price, as far as a fair profit will allow. But how am I to ensure this, even supposing I possessed the requisite knowledge and appliances? Time would be wanting to carry out a systematic analysis, and the ordinary "rule of thumb" tests are not a match for the increasing cleverness of "manufacturing chemists." It only remains to send samples to some known food analyst; but here the expense becomes a barrier, when the dealings dependent on it are on a small scale. Is there (or, if not, ought there not to be?) some Government functionary to whom samples could be sent for testing, at a charge to just cover necessary expenses? After reading a very sad article on "Artificial Milk," in your paper of Dec. 15, I feel emboldened to ask whether, either of yourself or through any of your readers, you could assist me to render practical a feeling I am sure you must sympathise with. For obvious reasons, I ask you to receive in strict confidence the name and address I have given to show the genuine nature of my application. GROCER

Earthquakes in Celebes

I WISH to contribute to the list of earthquakes and eruptions in your journal the following, all of which I have witnessed:—

- 1871
- May 1 . Eruption of a volcano on the Island Camiguin, south of the Philippine Islands.
- June 13 . Earthquake in Kakas, at the Lake of Tondano in Minahassa, North Celebes, 7½ P.M. This shock was at the same time felt throughout Minahassa.
- July 15 . Earthquake at Gorontalo, North Celebes, Bay of Tomini, 12½ P.M. and 10½ P.M.
- „ 19 . Earthquake at Gorontalo, 12¼ A.M., heavy.
- August 7. Eruption of the volcano of Ternate. This eruption had not ended August 23. Most of the inhabitants of Ternate ran away. Stones and ashes were thrown as far as Halmahera.
- „ 19. Earthquake at Gorontalo, 5 A.M.
- „ 25. Earthquake at Gorontalo, 3 P.M.
- „ 31. Earthquake at Gorontalo, 1 P.M., very strong, vertically.

In the month of August there were at Gorontalo a series of earthquakes, all of which I did not notice in my diary, some of them very severe, shocks so severe and numerous have not been experienced for years at that place. I do not doubt that they were in connection with the long-continued eruption of the volcano of Ternate in the same month.

Some years ago there was communicated to the Paris Academy, from South America, the fact that permanent magnets lose their magnetism during earthquakes. I will not discuss here the theoretical point of view of the question. During my whole stay in the northern part of Celebes I have always hung up a magnet, with a maximum weight attached to it, but never, not even during the severe earthquakes of Gorontalo, has the weight fallen down. I therefore doubt the fact.

Earthquakes are felt throughout the northern part of Celebes, on the coasts of the Bay of Tomini, at the Togan Islands in the Bay of Tomini; whereas in the southern part of Celebes, for instance at Macassar, earthquakes are scarcely ever felt or only very slight ones. The geological structure of the southern part of Celebes differs entirely from that of the northern.

I enclose a list of earthquakes observed at Gorontalo from 1866-70 by Mr. Riedel.

List of earthquakes at Gorontalo (N. lat. 0° 29' 42", W. long. 23° 2' 50") between the year 1866 and 1870:—

Year.	Month.	Day.	Hour.	Direction.	Direction of the Wind.
1866	February	18	1 p.m.	E.—W.	N.W.
	April	5	7½ p.m.	—	E.S.E.
	April	6	10½ a.m.	E.—W.	—
	June	20	6½ a.m.	E.—W.	E.S.E.
	September	5	8½ a.m.	—	S.E.
1867	December	2	3½ p.m.	E.—W.	W.
	February	26	11½ p.m.	E.—W.	W.
	March	22	4½ p.m.	—	N.W.
	March	30	9 p.m.	—	—
	April	22	10 a.m.	E.—W.	N.W.
	May	17	3 p.m.	E.—W.	S.E.
	June	26	8½ p.m.	E.—W.	S.E.
	July	26	8 a.m.	—	S.E.
	August	27	2 a.m.	E.—W.	S.E.
	September	14	10½ p.m.	E.—W.	S.E.
	December	23	10 p.m.	E.—W.	W.
	1868	April	7	9½ p.m.	E.—W.
May		27	6½ p.m.	E.—W.	S.S.E.
June		13	9½ p.m.	E.—W.	S.S.E.
July		27	11½ a.m.	E.—W.	S.E.
September		4	9½ p.m.	—	S.E.
November		18	6½ a.m.	E.—W.	S.E.
December		3	10½ a.m.	E.—W.	S.E.
1869	March	3	7 p.m.	E.—W.	N.W.
	August	22	9½ p.m.	E.—W.	S.E.
	November	17	4½ p.m.	E.—W.	W.N.W.
	December	17	4½ p.m.	E.—W.	W.S.W.
1870	April	7	12½ a.m.	E.—W.	W.S.W.
	July	12	5½ a.m.	E.—W.	W.S.W.
	August	28	3½ a.m.	E.—W.	W.S.W.

I am now going to the southern parts of the Philippine Islands, and in the following year to New Guinea. A short communication about my travels in Celebes will be found in *Petermann's Geographische Mittheilungen*.

Macassar, Celebes, Nov. 10, 1871 A. B. MEYER