severally twisted in opposite directions. A straight thick wire passed through the ring, the weight of which afforded a ready means of varying the force necessary to balance the torsion of the wire. My first object was to prove that the force of the latter was, at any rate to some considerable extent, independent of the tension. Suppose that with this arrangement, the wire being horizontal, a balance has been effected when the ring has been turned about the wire as an axis three or four times. What will happen when the wire is further strained? I think it would be a natural expectation (apart from special knowledge) that the weight will rise; on the other hand, a knowledge of the law of torsion teaches (?) that there is no increase of the force sustaining the weight, which therefore will *not* rise. But who would suppose that, on the contrary, it would sink? Such, nevertheless, is what takes place. I continued increasing the strain, and the weight continued to sink. I had to go on lessening the weight again and again (by shifting the balancing cross-rod), in order to restore the horizontality of the ring; until at last there was scarcely any force of torsion left! To repeat the experiment of course the ring had to receive three or four fresh turns. I did so several times, always continuing, as I thought, to increase the strain. All the time the wire was absorbing the torsion, and did not break. I then thought to try the effect of a high initial torsion. But I did not seem to get any such by turning the ring more than five or six times. I then thought to see how much twisting the wire would bear. Expecting it every instant to break, I counted up to 100 half turns. *By this time the wire was quite slack!* I added *another hundred* half turns. The wire was now half an inch longer, without any strain having been kept on it except just enough to keep it straight. I went on twisting. At 218 one wire broke. The other then had only sixteen half-turns of twist in it, out of the 230 or more received. I afterwards went on twisting, mending each time that the wire broke, till the twist (quite visible under the microscope) amounted to sixteen turns per inch. The length kept on increasing. After breaking, the wire always untwisted one turn in four inches.

I feel myself here in presence of laws of which I know *nothing*; and my object in writing this short experience is to ascertain whether it is sufficiently in accord with what *is* known to cause no surprise to any one but myself. In that case I shall be greatly obliged to any one who will tell me where I can learn all about it. J. HERSCHEL

Collingwood, October 4

I forgot to say that in no case did slackening of the strain reverse the sinking of the weight due to increase of strain.

The Magnetic Storm

BY the mail just arrived from Australia I have received copies of the photographic traces produced by the declination magnetograph at the Melbourne Observatory during the magnetic storm of August 12 to 14, kindly forwarded by Mr. Ellery, the Government astronomer there.

A comparison of these curves with those from the Kew instrument for the same period shows that the disturbance commenced and ended at both places at the same time.

It is not easy however to trace much similarity in the two sets of curves, as the individual excursions of the magnet east and west of the normal position which form the record of the magnetic storm, cannot be at all times followed in both curves, but the periods of greater disturbance seem to have been simultaneous. For example, the commencement of the disturbance was well marked at August 11d. 8h. 10m. p.m. at Melbourne, which corresponds to 11d. 10h. 33m. a.m. G.M.T., whilst here (*vide* Mr. Ellis's letter in NATURE, vol. xxii. p. 361) it commenced at 10h. 30m. a.m.; then again the large deviation to the eastward noted in the Rev. S. J. Perry's letter in NATURE, which occurred here between 12d. 11h. 30m. a.m. and 12h. 30m. p.m., seems to have had its effect, as a movement of the needle at Melbourne to the westward between 12d. 9h. 15m. p.m. and 10h. 30m. p.m. The maximum deflection which exceeded the limits of registration of the instrument, I estimate to have taken place at 10 p.m. The corresponding G.M. times for the above are 12d. 11h. 38m. a.m., 12h. 53m. p.m., and 12h. 23m. p.m.; the maximum deflection recorded here seems to have been at 12h. 25m. p.m.

The disturbed period may be considered to have died out at Kew at 14d. 8h. a.m. G.M.T., and at Melbourne at about 14d. 7h. a.m., but there is no very distinctive movement which would enable us to fix this limit with accuracy.

These interesting comparisons are extremely satisfactory, for it is but recently that the Government of Victoria was considering the advisability of discontinuing the system of photographic registration of the magnetometers at Melbourne, and consulted the Kew Committee upon the subject.

A circular was accordingly issued to the leading physicists of Europe, and their replies being almost unanimously in favour of the continuance of the recording system, the Government erected a new magnetic observatory, and decided upon carrying on the work.

Mr. Ellery has also forwarded a month's curves for the purpose of assisting in the international comparison of magnetograms now being prosecuted by the Kew Committee.

The preliminary results of their investigations have been already indicated by Prof. Adams in his recent speech at Swansea (NATURE, vol. xxii. p. 416). G. M. WHIPPLE

Kew Observatory, October 2

Coral Reefs and Islands

I HAVE been greatly interested in Mr. John Murray's paper on coral reefs and islands published in NATURE, vol. xxii. p. 351. I hope you will allow me space to draw scientific attention to the fact that as early as 1857 I published a paper on the Formation of the Peninsula and Keys of Florida (*Am. Jour.* vol. xxiii. p. 46), in which I maintain that the theory of Darwin, although so beautifully (as I thought) explaining the phenomena of the Pacific reefs, *wholly fails to explain those of the Florida coast.*

In 1851 I spent the months of January and February on the Keys of Florida, assisting Prof. Louis Agassiz in his investigations on the growth of reefs and formation of keys in this region. An abstract of these investigations and their results was published in the Report of the United States Coast Survey for 1851, p. 145 *et seq.*[1]

In this report Agassiz shows that the Keys and nearly the whole Peninsula of Florida have been formed by the growth of successive reefs, one beyond the other from north toward the south. In my paper above alluded to, and also in my "Elements of Geology," p. 152, I state further, that the reefs of Florida, if we accept Darwin's theory, are entirely peculiar. For according to Darwin barrier-reefs are formed *only by subsidence*, while on the Florida coast we have well-marked barriers with channels 10–40 metres wide where there cannot be any subsidence, for continuous increase of land is inconsistent with subsidence. Again, according to Darwin barriers and atolls always show a *loss of land*, only a small portion of which is recovered by coral and wave agency; while on the Florida coast, on the contrary, there has been a continuous growth of the Peninsula by coral accretion, until a very large area, viz., about 20,000 square miles, has been added.

I have attributed the formation of *successive* reefs from north toward the south to the successive formation of the depth-condition necessary for coral growth; and this latter, in the absence of any evidence of elevation, to the steady building up by sedimentary deposit, and extension southward, of a submarine bank within the deep curve of the Gulf Stream. The formation of barriers instead of fringes on a coast which has certainly not subsided—for continuous land-growth negatives the idea of subsidence—I attribute to the shallowness and muddiness of the bottom along this coast. Only at a distance of twenty to forty miles, where the depth of twenty fathoms is reached, and where, therefore, the bottom is no longer chafed by the waves, the conditions necessary for coral growth would be found, and here a line of reefs would be formed, limited on one side by the depth and on the other by the muddiness of the water.

In brief then, according to my view, the Peninsula and Keys of Florida were formed by the co-operation of several agents:—1. The Gulf Stream building up and extending a submarine bank within its loop. 2. Corals building successive barriers on the bank as the latter was pushed farther and farther southward. 3. Waves beating the reefs into lines of islands. 4. *Débris* from the reefs and keys on the one side and the already formed mainland on the other filling up the successive channels and converting them first into swamps and finally into dry land.

Whether this view is true in all its parts or not, there can be

[1] This report has been recently published in full as one of the memoirs of the Harvard Museum of Comparative Anatomy, but I have not yet seen it.