arms were found to commence as a series of ciliated tentacles placed around the mouth, and as nearly as possible identical with the lophophor of such a Polyzoon as *Pedicellina*. At first but six of these tentacles are seen; these increase in number, whilst the lophophor takes on its horse-shoe shape; and finally by the development of the free ends of the two sides of the horse-shoe the great Brachiopodian arms are produced. This is very interesting, and confirms *a priori* notions. At the same time we must dissent from the stress which Mr. Morse lays on the affinities of structure of Brachiopoda and Polyzoa, in so far as he wishes to separate these two widely from the Mollusca, and join them to a group which he calls Vermes. The Vermes have never been accurately defined, and are in fact at present, as Carl Gegenbaur (whom Mr. Morse cites) fully admits, one of those classificatory lumber-rooms, which are so convenient from time to time in the progress of zoological science. Whilst we fully admit the close affinities of the Polyzoa and the Brachiopoda—now long recognised by all zoologists—we cannot overlook the very strong affinities of these to the true Mollusca. Even a hasty study of the embryology of the Mollusca is sufficient to bring under one's eyes larval forms of various classes bearing many of the characteristics of the Polyzoa on the one hand, and of certain Vermes on the other. The early condition of the gill-plates in some Lamellibranchs is only to be compared to the tentacula of the Molluscoidan lophophor, though presenting so large a shifting in some relations. Rather than detach the Molluscoida (with regard to the Tunicata there are a variety of new facts and considerations which require long discussion) from the Mollusca to place them in the lumber-room Vermes—we should prefer to put the whole of the Mollusca along with them there—a proceeding at present useless, but which would express a truth which Mr. Morse does not seem to admit, though it is indicated by Gegenbaur, and accepted also by Huxley, namely, that there are close genetic ties between the group Mollusca (including Molluscoida), and certain so-called Vermes, such as the Turbellaria, Archi-annelida, &c.

In a paper published prior to this, Mr. Morse has spoken of the Brachiopoda as a division of Annelida, on the ground of certain resemblances between Lingula and Annelids. We are not sure whether Mr. Morse adheres to this startling proposition, or whether it was due to the intensity of the impressions produced by his study of living Lingulæ, which must have been exceedingly interesting. By the way, we may mention that Semper has also studied living Lingula. That there is a fundamental community of organisation between Lingula and Annelids we are, as stated above, not indisposed to believe, but that this can be expressed advantageously by making the Brachiopoda a division of Annelida, or that such a classification would be anything more than reactionary exaggeration, we cannot for a moment suppose. Mr. Morse attaches importance in this regard to the setæ of Lingula, and equal or perhaps more importance to the red colour of the blood. The discovery of red-coloured blood in Lingula is interesting, because in all probability it is due, as in vertebrates and all other causes where it is really red, to the presence of hæmoglobin, and is another instance of the exceptional appearance of this chemical principle in the blood of an animal whose nearest congeners do not

possess it. We should be very glad of confirmation with the spectroscope of the supposed existence of hæmoglobin in the blood of Lingula. But how can Mr. Morse suppose that this red blood, or hæmoglobin-bearing blood, is a character of the slightest classificatory importance? A great number of Annelids do not possess the vascular system at all, which in others carries this red blood; in some the fluid in that vascular system is coloured green by chlorocruorin, in others the hæmoglobin is present in the perivisceral fluid, which is in most Annelida colourless. Certain Mollusca have blood coloured red by hæmoglobin (Planorbis) as deeply and brightly as that of any lob-worm, so again have some Crustacea and Insect larvæ. The presence or absence therefore of hæmoglobin in the blood of Lingula is a matter of complete indifference as far as the relations of that animal to the Annelida are concerned.

We are much interested by a reference in Mr. Morse's paper on Terebratulina to some observations which he has made on the development of Lingula, observations which we hope before long to see published. From these he states that he is led to believe that the supposed Discina larva figured by Fritz Müller might equally as well be that of a Lingula. Some further information about this remarkable larval form will be very welcome.

Mr. Morse apologises for the undetailed character of his drawings, and for the absence of information in his paper upon the development of Terebratulina *ab ovo*—a great desideratum—by the fact that when he went to Eastport to study the development of Terebratulina he had a microscope with him which he found to be utterly inadequate to the purpose. Since this is an error which is easily remedied, we trust that Mr. Morse will soon return to the attack, if he has not yet already done so, duly armed.

E. RAY LANKESTER

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

The Solar Eclipse

It does not happen more than once in a lifetime to see such a glorious and magnificent sight as that from which I have just returned; that is, the total eclipse of the sun. I have seen many eclipses before, but never anything to equal this. I was engaged to go with the Morgans to the top of the hill to see it. Got up at six, and found it a lovely morning; rode up to Morgan's, about half a mile, carrying with me glasses, smoked glass, and sun hat. Got there before seven, and found eclipse already begun. Got our two mirrors and watched the hole in the sun grow bigger and bigger. It began from the top, and we all went off to the highest point on the hill, from whence we could see all Ooly and the mountains round. When the eclipse got so far, the cold on the mountain grew much greater, the grass was so wet that no one's boots kept it out, the feet and hands grew cold, and with your back to the sun the light over the country was like twilight, or the earliest dawn. Gradually the lower streak got thinner and thinner, until at last there shone a light like the famous lime-light, and in a moment or two that went out and the sun was totally concealed; many stars were visible, the whole country looked dark—that is, half dark, like moonlight—the crows stopped cawing, and for two minutes and a half the total eclipse lasted, a sight I shall never forget, and then the lime-light again appeared at the bottom rim of the sun, and gradually more and more of him appeared, the crows began again at once, and the

cocks began to crow, the shadow now was inverted, and by degrees got smaller, until at nine o'clock the eclipse was over. I cannot but suppose that the scientific men must have had grand opportunities of observation, and that to-day's pencil will carry home many a description. Anything more beautiful, more sublime, or more perfect, it would be impossible to conceive.

Upway House, Mercara, Coorg, R. N. TAYLOR
Ooly, Dec. 12, 1871

The Rigidity of the Earth

I HAVE been urged from several quarters to defend my argument for the rigidity of the earth against attacks which are supposed to have been made upon it. It has, in fact, never been attacked to my knowledge, and I feel under no obligation to defend it. There is, I believe, a general impression that grave objections to it have been raised by M. Delaunay, and it seems that even in this country some geological writers and teachers, in their reluctance to abandon the hypothesis of a thin solid crust, enclosing a wholly liquid mass, hastily concluded that all dynamical arguments against it had been utterly overthrown by Delaunay.

In point of fact Delaunay made no reference at all to the tidal argument, and clearly was unaware that I had brought it forward when he made his communication on the "Hypothesis of the interior fluidity of the terrestrial globe,"* to the French Academy, three years and a half ago, objecting to Hopkins's argument founded on precession and nutation, and merely quoting me as having expressed acquiescence. On this subject I say nothing at present, except that ten years ago, before I expressed (in my first communication of the tidal argument to the Royal Society) my assent to Hopkins's argument from precession and nutation, I had thought of the objection to this argument since brought forward by Delaunay, and had convinced myself of its invalidity. But I hope to be able on some future occasion to return to the subject, and to prove that any degree of viscosity, acting in the manner and to the effect described by Delaunay, must in an extremely short time abolish the distinction between summer and winter. My reason for writing to you at present is that I see in Mr. Scrope's beautiful book on Volcanoes (just published as a second edition) a sentence ("Prefatory Remarks," page 24), written on the supposition that the tidal argument had been brought forward for the first time at the recent meeting of the British Association in Edinburgh. I therefore take the liberty of suggesting to you that a reprint of the short abstract of my tidal argument, which appeared in the Proceedings of the Royal Society, for May 16, 1862, might not be inappropriate to your columns. I ought, however, to inform you that the tidal argument was carefully re-stated in the first volume of the treatise on Natural Philosophy, by Prof. Tait and myself, published in 1867, but as the volume is at present out of print, you may not consider this objection fatal to my proposal.

Glasgow University, Jan. 9 WILLIAM THOMSON

Abstract of Paper on the Rigidity of the Earth, by Prof. Sir William Thomson, F.R.S., received April 14, 1862

The author proves that unless the solid substance of the earth be on the whole of extremely rigid material, more rigid for instance than steel, it must yield under the tide-generating influence of sun and moon to such an extent as to very sensibly diminish the actual phenomena of the tides, and of precession and nutation. Results of a mathematical theory of the deformation of elastic spheroids, to be communicated to the Royal Society† on an early occasion, are used to illustrate this subject. For instance, it is shown that a homogeneous incompressible elastic spheroid of the same mass and volume as the earth, would, if of the same rigidity as glass, yield about $\frac{1}{5}$, or if of the same rigidity as steel about $\frac{2}{3}$ of the extent that a perfectly fluid globe of the same density would yield to the lunar and solar tide-generating influence. The actual phenomena of tides (that is, the relative motions of a comparatively light liquid flowing over the outer surface of the solid substance of the earth), and the amounts of precession and nutation, would in one case be only $\frac{1}{5}$ and in the other $\frac{2}{3}$ of the amounts which a perfectly rigid spheroid of the same dimensions, of the same figure, the same homogeneous density, would exhibit in the same circumstances. The close agreement with the results of observation presented by the theory of precession and nutation, always hitherto worked out on the supposition that

the solid parts of the earth are perfectly rigid, renders it scarcely possible to admit that there can be any such discrepancy between them as 3 to 5, and therefore almost necessary to conclude that the earth is on the whole much more rigid than steel. But to make an accurate comparison between theory and observation, as to precession, it is necessary to know the absolute amount of the moment of inertia about some diameter; and from this we are prevented by the ignorance in which we must always be as to the actual law of density in the interior. Hence the author anticipates that the actual deformation of the solid earth by the lunar and solar influence may be more decisively tested by observing the lunar fortnightly and the solar half-yearly tides.* These tides, it may be supposed, will follow very closely the "equilibrium theory" of Daniel Bernoulli for all oceanic stations, and the author suggests Iceland and Tenerife as two stations well adapted for the differential observations that would be required.

The earth's upper crust is possibly on the whole as rigid as glass, more probably less than more. But even the imperfect data for judging referred to above render it certain that the earth as a whole must be far more rigid than glass, and probably even more rigid than steel. Hence the interior must be on the whole more rigid, probably many times more rigid, than the upper crust. This is just what, if the whole interior of the earth is solid, might be expected when the enormous pressure in the interior is considered, but it is utterly inconsistent with the hypothesis held by so many geologists that the earth is a mass of melted matter enclosed in a solid shell of only from 30 to 100 miles' thickness. Hence the investigations now brought forward confirm the conclusions arrived at by Mr. Hopkins, that the solid crust of the earth cannot be less than 800 miles thick. The author indeed believes it to be extremely improbable that any crust thinner than 2,000 or 2,500 could maintain its figure with sufficient rigidity against the tide-generating forces of the sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they are.

Extract from Thomson and Tait's "Natural Philosophy."

"§832. . . . All dynamical investigations (whether "static or kinetic) of tidal phenomena, and of precession and "nutation, hitherto published, with the exception referred to "below, have assumed that the outer surface of the solid earth "is absolutely unyielding. A few years ago, for the first time, "the question was raised: Does the earth retain its figure with "practically perfect rigidity, or does it yield sensibly to the de- "forming tendency of the moon's and sun's attractions on its "upper strata and interior mass? It must yield to some extent, "as no substance is infinitely rigid. But whether these solid "tides are sufficient to be discoverable by any kind of observa- "tion, direct or indirect, has not yet been ascertained. The "negative result of attempts to trace their influence on ocean "and lake tides, as hitherto observed, and on precession and "nutation, suffices, as we shall see, to disprove the hypothesis "hitherto so prevalent, that we live on a mere thin shell of solid "substance enclosing a fluid mass of melted rocks or metals, "and proves, on the contrary, that the earth is much more rigid "than any of the rocks that constitute its upper crust."

"§833. The character of the deforming influence will be "understood readily by considering that if the whole earth were "perfectly fluid, its bounding surface would coincide with an "equipotential surface relatively to the attraction of its own "mass, the centrifugal force of its rotation and the tide generat- "ing resultant of the moon's and sun's forces, and their kinetic "reactions. Thus there would be the full equilibrium lunar and "solar tides; of $2\frac{1}{2}$ times the amount of the disturbing deviation "of level if the fluid were homogeneous, or of nearly twice "this amount if it were heterogeneous with Laplace's hypotheti- "cal law of increasing density. If now a very thin layer of "lighter liquid were added, this layer would rest covering the "previous bounding surface to very nearly equal depth all round, "and would simply rise and fall with that surface, showing only "infinitesimal variations in its own depth, under tidal influences. "Hence had the solid part of the earth so little rigidity as to "allow it to yield in its own figure very nearly as much as if it were "fluid, there would be very nearly nothing of what we call tides "—that is to say, rise and fall of the sea relatively to the land; "but sea and land together would rise and fall a few feet every

* *Comptes Rendus* for July 13, 1868.

† Communicated August 22, 1862, and read November 27, of same year "Dynamical Problems regarding Elastic Spheroidal Shells and Spheroids, of incompressible Liquids."

* High tide, as far as the influence of either body is concerned, is produced at the poles, and low average water at the equator, when its declination, whether north or south, is greatest, and low water at the poles and high water at the equator, when the disturbing body crosses the plane of the equator.