such matters, or the exhibiting departments of the government will be forced to the necessity (to do proper credit to themselves) of maintaining exhibition series, which, with slight modifications for special occasions, may be kept at hand, to send wherever and whenever required. If we are rightly informed, the national museum has already decided on some such step; and, if international exhibitions are to be a yearly occurrence, the museum should add to its staff a special exhibitionary force, and not weaken its efficiency for its proper work by these constant extra draughts upon its energy.

LETTERS TO THE EDITOR.

Classification of the Mollusca.

IN Mr. Dall's kindly notice of the article 'Mollusca' in the 'Encyclopaedia Britannica,' published in your journal of June 13, he attributes to me "the erroneous statement that the radula of Glossophora is horny," and adds that 'it is really chitinous.' In the ordinary sense of the word 'horny,' chitin is (I venture to think) correctly described as horny. That the radula is generally considered to consist of the chemical body known as chitin is distinctly stated in the article criticised by Mr. Dall. At the bottom of p. 460 occur the words, 'a chitinous band (the radula).' I should be glad to know if Mr. Dall has undertaken any special chemical analysis of the substance of the radula (1).

With regard to the very general presence of jaws in glossophorous Mollusca, I must maintain my statement. The presence of a calcareous impregnation is, it is true, not usual, but exceptional (2).

Mr. Dall is mistaken in supposing that I have followed Macdonald in regard to formulae for the teeth of the radula. The other writers whom he cites as not followed are precisely those from whom my statements on the details of this subject were drawn (3).

I have no fault to find with Mr. Dall for differing from me as to certain points of classification, but I should be glad to know his *grounds* for regarding the Zygobranchia as an artificial group. He merely reasserts the old view, which I think I have sufficiently shown to be untenable (4). Mr. Dall also asserts that the orders of Lipocephala, based on the characters of the adductor muscles, are defunct. In spite of this opinion, the muscles themselves still exist, and, in my opinion, furnish indications of natural and important divergent groups among the bivalves (5).

I should be glad to know on what grounds Mr. Dall considers the three divisions of Lipocephala adopted by me to be unnatural.

Lastly, let me say that I do not know on what authority Mr. Dall asserts that the calcareous developments of the integument in Chaetoderma and Neomenia have no relation to the shells of Chiton. That they also represent or replace the spines of Chitons is sufficiently obvious. But what is to prevent our conceiving of the epidermic shelly plate of a Chiton as originally developed by the gradual coalescence of a number of small calcareous denticles, in the same way as the mesodermic dermal bones of bony fishes have developed from the shagreen denticles of the sharks (6)?

E. RAY LANKESTER.

University college, London,
July 23.

(1) Not being an organic chemist, I have not attempted analyses, but have tested many radulae with one result, — the cutting points of the teeth are always, and the whole radula generally, of a substance allied to chitin. The very generally erroneous statements in the text-books led to the criticism of the language of Professor Lankester as tending to continue the confusion. Chitin is surely as different chemically from horn as bone is, and it cannot be desirable to continue to treat the two substances in a way to perpetuate an error. Further data on this topic may be found in the August *Naturalist*, pp. 776–778.

(2) I should be grateful to Professor Lankester for the name of any recent mollusk having a 'shelly' or even a partially 'calcified' jaw.

(3) The formulae given for the teeth, and the method used in making a formula, as inferred from the text, which were the particular details criticised, are partly incorrect. I was wrong, however, in assigning a source to them. One (for instance, Patella vulgata) has the formula 3+3+1+3+3, instead of 3.1.4.1.3. No mollusk has more than one median tooth; and the central figure of the formula must in all cases be 1 or 0. I find the erroneous formula in Sars's text, though he figures the teeth correctly. Again: Chiton stelleri has, like all Chitons hitherto examined, the formula 6+2+1+2+6, instead of 0000.1.1.1.0000, which is given; but this is doubtless copied from some other authority. However, accurate formulae for the Chitons and Limpets have been accessible for some years. Again: the teeth of the radula are divided by nearly all modern students of that organ into rhachidian or median, lateral, and uncinal teeth, — three series which have anatomical relations to the radula, which are usually pretty clear. For 'lateral' Professor Lankester substitutes the term 'admedian,' which is not, as far as I know, in use; and for the 'uncini' he adopts the term 'laterals,' which I venture to think is undesirable as leading to confusion, and not in accord with general usage.

(4) The grounds on which I sustain the generally accepted views of malacologists, as to the relations of the groups Professor Lankester has compounded into the order Zygobranchia, are, that the mere abortion of one of a pair of organs is not a character of ordinal value; nor are the characters assigned to Zygobranchia applicable to all its members. Moreover, I am of the opinion that the characters which unite the Rhipidoglossa among themselves and the Docoglossa among themselves are of higher systematic value than the characters here relied upon for dismembering them. I believe, that, had the learned professor made researches among a large number of these forms, he would probably be of this opinion also.

(5) The characters of the adductor muscles, as long as we were ignorant of intermediate forms, seemed to afford a good basis for orders in the Lipocephala. Now that we know of forms which are more or less intermediate, in the Pectinidae, Ostraeidae, Mytilidae, and other families, and that in the young (not embryonic) there are frequently two adductors discernible in supposed monomyarians, with such forms turning up as Dimya, and, more recently, Chlamydoconcha, all tending to efface the supposed definite limits between the alleged orders, it seems impos-

sible to retain these orders any longer. Stoliczka came to this view long ago, and much corroborative evidence has come to hand since. In fact, there does not at present seem to be any good basis for ordinal divisions in the Lipocephala. The divisions adopted by Professor Lankester are not unnatural; but they appear to have merely an approximate value, and shade into one another to such an extent as to be of little systematic use.

(6) There is nothing to prevent any such conception; but, unfortunately, there is no evidence, as yet, that it would conform to any subjective reality. A parallel statement would be, that the wool on a ewe 'replaces' the horns on a ram. We can conceive that woolly or hairy secretions may be so modified as to produce horns, and, in fact, do produce them occasionally. The importance of the shell-gland in the embryonic condition of the Mollusca, as shown by Professor Lankester, than whom none have contributed more valuable investigations on this topic, forbids that we should consider these secondary cuticular products as its equivalent. That they are nothing less than identical with Chiton spines will, I think, be admitted by any one who compares the figures of Reincke and Hubrecht on Chitons and Neomenia respectively. There are also a great variety of other Chiton spines; and on some Fissurellidae, and even in some brachiopods, analogous structures may be found.

In conclusion, Mr. Editor, permit me to express the hope that these more or less unimportant defects in detail, which are inevitable to all work of a general character, may not obscure what I have endeavored to state clearly (namely, the great value and usefulness of Professor Lankester's work), nor delay what I believe will be its eventual consequence, — an important reformation in our general molluscan systems.

W. H. DALL.

The earthquake of Aug. 10.

It is a little remarkable that the earthquake-shock of yesterday should have been felt with considerable force in the city of New Haven, which is built upon a sandy plain, while it was perceptible only as a short series of lateral vibrations, lasting about a second and a half, and so slight that it was unnoticed by most persons in the vicinity of the observatory. The observatory is built on a sandstone ledge, and is about a hundred and fifty feet above tide-water, in (geodetic) longitude west 72° 55′ 19.15″, and latitude north 41° 19′ 28.48″.

At the time of the vibration the writer was sitting at a table, and its probable origin at once occurred to him. Allowing for the few seconds occupied in taking out his watch, the tremor occurred at 2 h. 7 m. 25 s.; and, as the watch at that time was 1.5 s. slow of the fifth hour west from Greenwich local mean time, the tremor may be set down as beginning at 2 h. 7 m. 27 s. by this mean time; and I should estimate the uncertainty at not more than 2 s.

LEONARD WALDO.

Yale college observatory, Aug 11.

On Sunday, Aug. 10, at 2 h. 8 m., I felt an earthquake, lasting three or four seconds. The oscillatory movement was from a little south of west, toward a little north of east. The oscillations were rapid but slight, with maximum intensity between the first and second second, when the movement began gradually to decrease. The accompanying sound was like the rumble of artillery-wagons. JULES MARCOU.

Cambridge, Aug. 10.

EPIDEMIC CHOLERA AND INFECTIOUS DISEASES.

THE presence of cholera this summer in epidemic form in southern France, the appearance of sporadic cases at widely scattered places and on shipboard at various seaports of the European continent and of England, have brought western civilization once more face to face with two of the most important problems which modern science and social organization can be called upon to solve. These problems just now come home to every one, but in ordinary years are put out of mind, or left to the care of laboratory devotees, or of officials charged with departments concerned with public hygiene.

The first involves a purely scientific question as to the causes, modes of origin, and ways of propagation, of the infectious or so-called zymotic diseases: the second, evolving itself naturally from the first, is of a more immediately practical nature, and deals with the processes best calculated to prevent and antagonize these diseases, especially when presenting themselves as epidemics. And these problems owe this much to such epidemics, — that by them men as individuals, and governments (their representatives), are stimulated to a vigor of inquiry and action which are never evoked by a customary rate of mortality, however high, from endemic diseases, such as are always with us; just as the stimulus of prospective want often meets with a ready response where chronic destitution makes an ineffectual appeal to action. Typhoid-fever, resembling cholera very much in its propagation, demands a steady toll from the populations of Europe and North America, compared to which the occasional ravages of cholera become insignificant; and yet it is impossible to inspire them with an intelligent dread of that enemy expressing itself in possible and comparatively simple precautions. The self-reliant Anglo-Saxon continues to regard typhoid-fever with a measure of the same indifference felt by the fatalist of India toward cholera; and the explanation is to be found,