

pend on the numbers in which it was reproduced. Many species of animals, it is needless to point out, such as rats and mice, are ceaselessly persecuted by enemies, and yet survive, and from time to time spread over vast areas. The lemming, notwithstanding that thousands yearly perish by their own act, and from the attacks of enemies during their migration, has not become extinct. Nor can I see that 2000 miles is such an "enormous extent of land" for a migration to extend over, even in face of carnivorous mammals and reptiles. It is at least not so great as the distance covered during the migration of the South American tapirs from Central Europe *via* Behring's Straits to Brazil, the route supposed by Mr. Wallace to have been taken by the ancestors of these interesting animals.

Mr. Wallace asks, "What difficulty is there in the same or closely allied species of this widespread group finding their way at some remote epoch to Mauritius and the Chatham Islands, and from similar causes in both islands, losing their power of flight while retaining their general similarity of structure?" I must reply, none; and then ask in turn, from where did they find their way? which is the point under discussion. I am constrained to believe that they came from an extensive land, capable of supporting large numbers of them, which must have been continuous with (as indicated by other evidence) or approaching close to both regions, otherwise we have to believe that this strictly Notogæan group has "found its way" across half the globe, or has arisen independently in both regions from different sections of the family—an occurrence which we have no evidence to warrant our believing has ever taken place.

I am unable to speak for the present opinions of Prof. Newton or his brother; but I know of no additional evidence that has come to light that is likely to have modified their well-considered opinion of a few years ago. On the contrary, it seems to me confirmatory of their views.

I beg, however, to protest against the implication that I have invoked this "tremendous hypothesis" to account for the distribution of the *Aphanapteryx* and *Fulica* I discovered. I have given prominence no doubt to the valuable evidence their presence contributes, additional only, however, to the numerous other facts I have adduced in my paper before the Royal Geographical Society, in support of the theory that a land of extensive dimensions—not isolated islands only as Mr. Wallace agrees to—existed in the southern seas, in order to explain the distribution of plants and animals, *unknown in the northern side of the equator*, in regions so distant as South America, Australia, New Zealand, and "Lemuria." I have, in my own opinion, adduced no more cogent facts pointing in this direction than those published by the late Prof. W. K. Parker, showing plainly the common ancestry existing between the Notogæan (*Gymnorhine*) crows of Australia, and the Deudroclaptine birds of South America. Their common progenitor must have occupied some *southern* land connected with both Australia and South America.

I might adduce still other weighty examples from the domain of ornithology, tending to support my opinion, which have been kindly communicated to me by Dr. Bowdler Sharpe, but I forbear now, as I understand that this will form the subject of the second lecture of the course he is now delivering on Thursday afternoons at the Royal Institution.

104, Philbeach Gardens, May 20. HENRY O. FORBES.

Phagocytes of Green Oysters.

IN your issue of May 4 you refer in a note to a suggestion made by my friend and former pupil, Dr. Paul Pelseneer that the green amoeboid cells described by me as occurring on the surface of the gills of green oysters are to be interpreted as out-wandered phagocytes. It is, I think, only right to point out that Dr. Pelseneer (as he is careful to explain in the note published by him) has made no new observations on the matter, and merely professes to give an interpretation of the facts which I described in 1886 in the *Quart. Journ. Micr. Sci.* in my article on green oysters. I there described and figured large granular cells occurring in and upon the epithelium of the gill-filaments and regarding them as epithelial secretion-cells attributed to them the active part in the elimination of the blue pigment "marennin" taken in by the oyster in its food—the diatom *Navicula ostreararia*. At that time the general doctrine of "phagocytosis" had not been so fully developed as it is

now seven years later. But I may say that already in 1887 one of my pupils (Mr. Blundstone) had established to my satisfaction the existence of extensive out-wandering of phagocytes through the surface epithelium of *Anodon* in various regions of the body, and that I was very soon led by the accumulating evidence of a similar kind (*e.g.* Durham's observations on star-fishes) to adopt the view that the large "secretion-cells" discovered by me both in the epithelium of the oyster's gill and freely moving on its surface, were out-wandered phagocytes. I have taught this view in my lectures, and have made some further observations (two years ago) on similar out-wandering phagocytes in other Lamellibranchs. The subject is one well worthy of minute study, phagocytosis in Mollusca being as yet an unexplored ground likely to yield results of great physiological importance.

E. RAY LANKESTER.

Oxford, May 7.

The Conjoint Board's Medical Biology.

THE pertinent remarks of L. C. M. (*NATURE*, vol. xlviii., p. 29), and G. B. H. (vol. xlvii., p. 530), respecting the course of elementary biology prescribed by the Conjoint Board, expresses, I think, the feelings of most biologists.

Either it is desirable, or not, that previous to entering upon a course of purely medical studies the student should have a training in elementary biology. The Board have decided in the affirmative, and have prescribed a course as amusing as it is absurd. It demands a practical acquaintance with the structure of certain protozoa, *Hydra*, the leech, two or three parasitic worms, a scappy knowledge of botany, and a few generalities. The insecta, crustacea, mollusca, and the whole of the vertebrata, are entirely omitted in the practical work. Under such circumstances it is almost ridiculous to attempt to impart any true knowledge of biology, in fact it is quite impossible to do so, for in the absence of such types as the crayfish, dogfish or cod, very many important morphological facts cannot be illustrated.

It would be interesting to learn the constitution of the committee who have drawn up this inexecutable syllabus. Onerally cannot for a moment suppose that they are acquainted with the scope and aim of present-day biological teaching, but from hazy memories of their student days, and an acquaintance with *Tenia*, *Ascaris*, and the leech, have drawn up the present course. The examination, I should remark, is in *perfect keeping* with the syllabus.

The important morphological facts to be gained by a dissection of the leech are probably best known to the Board.

It is sincerely to be hoped that the matter may not be allowed to rest here, but that some steps will be taken to impress upon the Board the utter absurdity of their present syllabus and mode and standard of examination, and the need for a recognized course in both zoology and botany.

WALTER E. COLLINGE,

Mason College, Birmingham, May 15.

Vectors versus Quaternions.

AS in recent numbers of *NATURE* my views on analysis have been quoted, and not very correctly, I ask for space to state them more explicitly. I see truth in the quaternion analysis and in the vector analysis; but I believe that neither the one nor the other, nor the two combined, contain the whole truth. The vector is an important idea, and the quaternion is an important idea, but there are in physical science many other important ideas which call for a more direct notation. To avoid any narrow hypothesis I denominated my first paper "Principles of the Algebra of Physics"; but in the notice which *NATURE* honoured it with it was printed as "Principles of the Algebra of Vectors." The title I gave it indicates briefly my position. I have been looking at analysis from the point of view of the physicist, and one of my guiding ideas has been that the fundamental rules of analysis, instead of being assumed as so many arbitrary rules of operation, should be grounded on the fundamental laws of physics.

What is the greatest want of the physicist of the present day? It is a generalised analysis which shall not contradict the Cartesian analysis, but be a logical generalisation of it, which shall include and harmonise such methods as the Double Algebra of Argand, Cauchy, and De Morgan (an excellent presentation of which has recently been published by Mr. Hayward), the method of Determinants, the Matrices of Cayley, the