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 *Deudrocolaptes* 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 amœboid 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 ostrearia*. 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 scrappy 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 inexplicable syllabus. One really 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 *Tænia*, *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

Quaternions of Hamilton and Tait, the *Ausdehnungslehre* of Grassmann, the vector analysis of Gibbs and Heaviside. It is this problem of how to harmonise, unify, generalise, and extend that I have been studying. Analysts and physicists dislike Mr. McAulay's idea of an independent plant; they prefer to cultivate the old tree venerable with the growth of ages.

After studying impartially all the writers at my command I came to the conclusion that the analysis of vectors is complementary to the analysis of versors, and that the fundamental rules for the former are:—

$$i^2 = + \quad j^2 = + \quad k^2 = + \quad (1)$$

$$ij = -ji \quad jk = -kj \quad ik = -ki \quad (2)$$

$$ij = k \quad jk = i \quad ki = j \quad (3);$$

whereas for the latter they are:—

$$i^2 i^2 = - \quad j^2 j^2 = - \quad k^2 k^2 = - \quad (4)$$

$$i^2 j^2 = -j^2 i^2 \quad k^2 j^2 = -j^2 k^2 \quad i^2 k^2 = -k^2 i^2 \quad (5)$$

$$i^2 k^2 = -k^2 i^2 \quad j^2 k^2 = -k^2 j^2 \quad i^2 j^2 = -j^2 i^2 \quad (6)$$

It follows that in the manipulation of the products of vectors, the distributive rule applies but not the associative; while in the products of versors both apply. These fundamental rules for vectors are based on physical considerations, the principal one of which is that the square of a vector is essentially positive, whereas, according to quaternionists, it is essentially negative. My view agrees with that principle of analysis which considers the cosine in the first and fourth quadrants to be positive; to make it negative produces confusion and error. These principles harmonise with those of Gibbs and Heaviside; and in the memoir quoted I have carried them out to their logical development. It is this development which Prof. Knott characterises as "a pseudo-quaternionic system of vector algebra, which is non-associative in its products." I see no worthy aim in being canny about the matter; my sole aim was to develop the system so that its truth or falsity might the more readily appear. At the end of his article Prof. Knott admits that the *assumption* that the square of a unit vector is positive unity leads to an algebra which is essentially different from the algebra of quaternions. As regards the fundamental principle being an assumption, I refer him to that same chapter of "Kelland and Tait" which he quotes, where he will find, italics and all:—"We retain what Sir Wm. Hamilton terms the *associative laws of multiplication*; the law which assumes that it is indifferent in what way operations are grouped, provided the order be not changed; the law which makes it indifferent whether we consider abc to be $a \times bc$ or $ab \times c$. This law is *assumed* to be applicable to multiplication in its new aspect (for example that $ijk = i(jk)$ and being assumed it limits the science to certain boundaries, and, along with other assumed laws, furnishes the key to the interpretation of results. The law is by no means a necessary law. Some new forms of the science may possibly modify it hereafter. In the meantime the assumption of the law fixes the limits of the science." Here an authoritative expounder places the quaternion algebra on precisely the same footing that Dr. Knott places the "pseudo-quaternionic;" and he even predicts that in the course of time such a complementary algebra will be developed. It is incumbent on a critic, having admitted the logical development, to show that the assumptions are absurd, or correspond to nothing in physical science; instead of which he informs us that he is appalled by the complexity, but nevertheless he feels sure that it contains nothing new. As regards newness I invite his attention to pr. 9; of the "Principles," where I have investigated the rules for the several partial products of any number of vectors in space of not more than four dimensions (and they may be easily extended to space of higher dimensions). These consist of certain rules of reduction which are to be taken along with the rule of signs of determinants, thus embracing determinants and Grassmann's combinatory products in the general theory of products of vectors. He will also find there some reasons for believing that the triad of rules No. 3 are very different in nature from the other two triads, Nos. 1 and 2. It is possible to get along without No. 3.

That vectors should be treated vectorially, and versors vectorially, and rotors vectorially, is neither nonsense nor a truism. It is an important maxim, and of growing importance in these days. Violation of it has produced the fundamental weakness of Hamilton's analysis. In a more recent paper I have pub-

lished the generalisations for space of the exponential, binomial, multinomial, and other fundamental theorems of analysis, and I show that it was from treating versors vectorially that Hamilton failed to discover them.

Prof. Knott defines a quaternion as the quotient of two vectors. Why choose the quotient; is not the product always the simpler idea? But further on vectors are identified with quadrantal quaternions, from which it follows that a quaternion is the quotient of two quadrantal quaternions. I have devoted some attention to logic; but I fail to extract any meaning out of this implicit definition.

Prof. Knott informs the reader that whereas Heaviside and myself find that $\nabla^2 u = d^2 u/dx^2 + d^2 u/dy^2 + d^2 u/dz^2$ the real $\nabla^2 u$ is minus that quantity; but he does not explain why Prof. Tait prefers the unreal $\nabla^2 u$ in his "Treatise on Natural Philosophy." A scientific critic would, instead of using exclamation points, proceed to show that in every case $\nabla(\nabla\omega) = (\nabla\nabla)\omega$. If that can be proved, not from any fancied properties of italic letters, but from physical considerations, then I shall readily admit that ∇ behaves as a versor rather than a vector. The *onus probandi* lies on the minus men.

Austin, Texas, May 6.

ALEXANDER MACFARLANE.

An Atmospheric Phenomenon in the North China Sea.

DURING a recent wintry cruise in H.M.S. *Caroline* in the North China Sea, a curious phenomenon was seen which may be of interest to your readers. The ship was on passage between Shanghai and the western entrance of the famous inland sea of Japan. On 24th February, at 10 p.m., when in latitude $32^\circ 58' N.$, longitude $126^\circ 33' E.$, which, on reference to the map, will be seen to be sixteen to seventeen miles south of Quelpart island (south of the Korean peninsula) some unusual lights were reported by the officer of the watch between the ship and Mount Auckland, a mountain 6,000 feet high. It was a windy, cold, moonlight night. My first impression was that they were either some fires on shore, apparently higher from the horizon than a ship's masthead, or some junk's "flare up" lights raised by mirage. To the naked eye they appeared sometimes as a mass; at others, spread out in an irregular line, and, being globular in form, they resembled Chinese lanterns festooned between the masts of a lofty vessel. They bore north (magnetic), and remained on that bearing until lost sight of about midnight. As the ship was passing the land to the eastward at the rate of seven knots an hour, it soon became obvious that the lights were not on the land, though observed with the mountain behind them.

On the following night, February 25th, about the same time, 10 p.m., the ship having cleared Port Hamilton, was steering east, on the parallel of 34° , when these curious lights were again observed on the same bearing, at an altitude of 3° or 4° above the horizon. It was a clear, still, moonlight night, and cold. On this occasion there was no land in sight on a north bearing when the lights were first observed, but soon afterwards a small islet was passed, which for the time eclipsed the lights. As the ship steamed on at a rate of seven knots an hour, the lights maintained a constant bearing (magnetic) of $N.2^\circ W.$, as if carried by some vessel travelling in the same direction and at the same speed. The globes of fire altered in their formation as on the previous night, now in a massed group, with an outlying light away to the right, then the isolated one would disappear, and the others would take the form of a crescent or diamond, or hang festoon-fashion in a curved line. A clear reflection or glare could be seen on the horizon beneath the lights. Through a telescope the globes appeared to be of a reddish colour, and to emit a thin smoke.

I watched them for several hours, and could distinguish no perceptible alteration in their bearing or altitude, the changes occurring only in their relative formation, but each light maintained its oval, globular form.

They remained in sight from 10 p.m. until daylight (about 5.30 a.m.). When lost sight of the bearing was one or two points to the westward of north. At daylight land 1300 feet high was seen to the north and north-north-west, distant fifty miles, the mirage being extraordinary.

Thus, these lights were seen first in longitude $126^\circ 33' E.$, and last in longitude $128^\circ 29' E.$ At first the land was behind them, but during the greater part of the distance run it was forty-five or fifty miles away to the north; and the bearing of the lights for at least three-fourths of the distance did not change.

On arrival at Kobé I read in a daily paper that the "Unknown