whist is dealt with by Robert F. Green, piquet, écarté, euchre, bézique, and cribbage by "Berkeley," and the round games, including napoleon, loo, &c., by Baxter-Wray. Although we have only just mentioned these games by name, it must be understood that each is thoroughly well described, and numerous illustrations for the explanation of difficult hands are inserted. The work will be most popular as a book of reference; and we may add that those who wish only to be instructed in one or two games in particular can obtain different sections in separate volumes.

Object-Lessons from Nature: a First Book of Science. By L. C. Miall, Professor of Biology in the Yorkshire College, Leeds. (London: Cassell and Co., Limited, 1890.)

THIS little volume is a very laudable attempt to form a first guide to the study of Nature for children; the lessons are meant to serve as guides to the intelligent teacher, who should in a half-demonstrative, half-catechetical way, bring the various subjects treated of before the young pupil's mind. Throughout there is no attempt to teach the student that there are a number of different sciences, but the various natural phenomena witnessed in the history of a "summer shower," in the "burning of a candle," or the "growth of a plant or insect," are gradually led up to, the various stages in the story of each being explained as they arise. Teachers must use their personal experience in illustrating their teachings, as based on these lessons, and those who are able to use the chalk and blackboard will find an immense advantage in doing so, as the little student's attention is thereby doubly attracted, and a much greater impression is made upon his mind.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The International Congress of Hygiene and Demography.

AS I notice that you give an excellent account of the meeting of the General Committee of this Congress recently held under the presidency of H.R.H. the Prince of Wales, I have no doubt that your readers will be interested to know how the Congress came to be invited to hold its session in London this year.

In 1884 I was asked by the Organising Committee of the Congress to be held at The Hague that year to give an address at a general *séance* of the Congress; I did so, selecting as my subject "La Science Ennemie de la Maladie." During the meeting of that Congress, one of the most pleasant that I ever attended, I was frequently asked why we had never invited the Congress to London, and I was obliged to answer that our Government would give no pecuniary assistance for such a purpose, and that we had no Hygienic Society strong enough to undertake it; but I determined that anything I might be able to do to bring about that result should be done, as I felt that it was a disgrace to this Country that this Congress, of all others, had not met here.

The next Congress was to be held in Vienna in 1886, but was postponed until 1887, and in that year my opportunity came in the proposed amalgamation of the Sanitary Institute of Great Britain and the Parkes Museum. I urged the importance of the matter on the Councils of both those bodies, and induced them to pass resolutions inviting the Congress to hold its next session in London. Sir Douglas Galton (as Chairman of both Councils) and I were appointed delegates to attend the Congress at Vienna, and present the invitation; the Society of Medical Officers of Health was also asked to co-operate, and appointed Mr. Shirley Murphy as its delegate. We three went to Vienna, and presented the invitation, which was accepted. After our return to this country we set to work to form, with the aid of the other English members of the Vienna Congress, especially Sir Spencer Wells, Prof. (now Sir George) Humphry, Dr. Cameron, M.P., Prof. Frankland, and Dr. Mapother, the large and influential General Committee.

In the early days it was thankless and rather trying work, especially as the money did not come in as readily as we could have wished; but now, thanks to the patronage of Her Majesty the Queen, to the presidency of His Royal Highness the Prince of Wales, and to the support of the Lord Mayor and Corporation of the City of London, it is comparatively plain sailing, and there can be no doubt that the Congress will be a great success, to which no one will have contributed more than Sir Douglas Galton, the indefatigable Chairman of the Organising Committee. To ensure this, however, and to enable us to bear the heavy expense of printing the Transactions, &c., funds are still urgently needed. I may add that all general correspondence relating to the Congress should be addressed to Dr. G. V. Poore, the Hon. General Secretary, at the offices, 20 Hanover Square, W.

W. H. CORFIELD,
Hon. Foreign Secretary.

19 Savile Row, W., March 28.

On the Rôle of Quaternions in the Algebra of Vectors.

THE following passage, which has recently come to my notice, in the preface to the third edition of Prof. Tait's "Quaternions," seems to call for some reply:

"Even Prof. Willard Gibbs must be ranked as one of the retarders of quaternion progress, in virtue of his pamphlet on 'Vector Analysis,' a sort of hermaphrodite monster, compounded of the notations of Hamilton and of Grassmann."

The merits or demerits of a pamphlet printed for private distribution a good many years ago do not constitute a subject of any great importance, but the assumptions implied in the sentence quoted are suggestive of certain reflections and inquiries which are of broader interest, and seem not untimely at a period when the methods and results of the various forms of multiple algebra are attracting so much attention. It seems to be assumed that a departure from quaternionic usage in the treatment of vectors is an enormity. If this assumption is true, it is an important truth; if not, it would be unfortunate if it should remain unchallenged, especially when supported by so high an authority. The criticism relates particularly to notations, but I believe that there is a deeper question of notions underlying that of notations. Indeed, if my offence had been solely in the matter of notation, it would have been less accurate to describe my production as a monstrosity, than to characterize its dress as uncouth.

Now what are the fundamental notions which are germane to a vector analysis? (A vector analysis is of course an algebra for vectors, or something which shall be to vectors what ordinary algebra is to ordinary quantities.) If we pass over those notions which are so simple that they go without saying, geometrical addition (denoted by $+$) is, perhaps, first to be mentioned. Then comes the product of the lengths of two vectors and the cosine of the angle which they include. This, taken negatively, is denoted in quaternions by $S\alpha\beta$, where α and β are the vectors. Equally important is a vector at right angles to α and β (on a specified side of their plane), and representing in length the product of their lengths and the sine of the angle which they include. This is denoted by $V\alpha\beta$ in quaternions. How these notions are represented in my pamphlet is a question of very subordinate consequence, which need not be considered at present. The importance of these notions, and the importance of a suitable notation for them, is not, I suppose, a matter on which there is any difference of opinion. Another function of α and β, called their product and written $\alpha\beta$, is used in quaternions. In the general case, this is neither a vector, like $V\alpha\beta$, nor a scalar (or ordinary algebraic quantity), like $S\alpha\beta$, but a quaternion—that is, it is part vector and part scalar. It may be defined by the equation—

$$\alpha\beta = V\alpha\beta + S\alpha\beta.$$

The question arises, whether the quaternionic product can claim a prominent and fundamental place in a system of vector analysis. It certainly does not hold any such place among the fundamental geometrical conceptions as the geometrical sum, the scalar product, or the vector product. The geometrical sum $\alpha + \beta$ represents the third side of a triangle as determined by the sides α and β. $V\alpha\beta$ represents in magnitude the area of the parallelogram determined by the sides α and β, and in direction the normal to the plane of the parallelogram. $S\gamma V\alpha\beta$ represents the volume of the parallelopiped determined by the edges α, β, and γ. These conceptions are the very foundations of geometry.

We may arrive at the same conclusion from a somewhat narrower but very practical point of view. It will hardly be denied that sines and cosines play the leading parts in trigonometry. Now the notations $V\alpha\beta$ and $S\alpha\beta$ represent the sine and the cosine of the angle included between α and β, combined in each case with certain other simple notions. But the sine and cosine combined with these auxiliary notions are incomparably more amenable to analytical transformation than the simple sine and cosine of trigonometry, exactly as numerical quantities combined (as in algebra) with the notion of positive or negative quality are incomparably more amenable to analytical transformation than the simple numerical quantities of arithmetic.

I do not know of anything which can be urged in favour of the quaternionic product of two vectors as a *fundamental* notion in vector analysis, which does not appear trivial or artificial in comparison with the above considerations. The same is true of the quaternionic quotient, and of the quaternion in general.

How much more deeply rooted in the nature of things are the functions $S\alpha\beta$ and $V\alpha\beta$ than any which depend on the definition of a quaternion, will appear in a strong light if we try to extend our formulæ to space of four or more dimensions. It will not be claimed that the notions of quaternions will apply to such a space, except indeed in such a limited and artificial manner as to rob them of their value as a system of geometrical algebra. But vectors exist in such a space, and there must be a vector analysis for such a space. The notions of geometrical addition and the scalar product are evidently applicable to such a space. As we cannot define the direction of a vector in space of four or more dimensions by the condition of perpendicularity to two given vectors, the definition of $V\alpha\beta$, as given above, will not apply *totidem verbis* to space of four or more dimensions. But a little change in the definition, which would make no essential difference in three dimensions, would enable us to apply the idea at once to space of any number of dimensions.

These considerations are of a somewhat *a priori* nature. It may be more convincing to consider the use actually made of the quaternion as an instrument for the expression of spatial relations. The principal use seems to be the derivation of the functions expressed by $S\alpha\beta$ and $V\alpha\beta$. Each of these expressions is regarded by quaternionic writers as representing two distinct operations; first, the formation of the product $\alpha\beta$, which is the quaternion, and then the taking out of this quaternion the scalar or the vector part, as the case may be, this second process being represented by the selective symbol, S or V. This is, I suppose, the natural development of the subject in a treatise on quaternions, where the chosen subject seems to require that we should commence with the idea of a quaternion, or get there as soon as possible, and then develop everything from that particular point of view. In a system of vector analysis, in which the principle of development is not thus predetermined, it seems to me contrary to good method that the more simple and elementary notions should be defined by means of those which are less so.

The quaternion affords a convenient notation for rotations. The notation $q(\ )q^{-1}$, where q is a quaternion and the operand is to be written in the parenthesis, produces on all possible vectors just such changes as a (finite) rotation of a solid body. Rotations may also be represented, in a manner which seems to leave nothing to be desired, by linear vector functions. Doubtless each method has advantages in certain cases, or for certain purposes. But since nothing is more simple than the definition of a linear vector function, while the definition of a quaternion is far from simple, and since in any case linear vector functions must be treated in a system of vector analysis, capacity for representing rotations does not seem to me sufficient to entitle the quaternion to a place among the *fundamental* and *necessary* notions of a vector analysis.

Another use of the quaternionic idea is associated with the symbol ∇. The quantities written $S\nabla\omega$ and $V\nabla\omega$, where ω denotes a vector having values which vary in space, are of fundamental importance in physics. In quaternions these are derived from the quaternion $\nabla\omega$ by selecting respectively the scalar or the vector part. But the most simple and elementary definitions of $S\nabla\omega$ and $V\nabla\omega$ are quite independent of the conception of a quaternion, and the quaternion $\nabla\omega$ is scarcely used except in combination with the symbols S and V, expressed or implied. There are a few formulæ in which there is a trifling gain in compactness in the use of the quaternion, but the gain is very trifling so far as I have observed, and generally, it seems to me, at the expense of perspicuity.

These considerations are sufficient, I think, to show that the position of the quaternionist is not the only one from which the subject of vector analysis may be viewed, and that a method which would be monstrous from one point of view, may be normal and inevitable from another.

Let us now pass to the subject of notations. I do not know wherein the notations of my pamphlet have any special resemblance to Grassmann's, although the point of view from which the pamphlet was written is certainly much nearer to his than to Hamilton's. But this is a matter of minor consequence. It is more important to ask, What are the requisites of a good notation for the purposes of vector analysis? There is no difference of opinion about the representation of geometrical addition. When we come to functions having an analogy to multiplication, the product of the lengths of two vectors and the cosine of the angle which they include, from any point of view except that of the quaternionist, seems more simple than the same quantity taken negatively. Therefore we want a notation for what is expressed by $-S\alpha\beta$, rather than $S\alpha\beta$, in quaternions. Shall the symbol denoting this function be a letter or some other sign? and shall it precede the vectors or be placed between them? A little reflection will show, I think, that while we must often have recourse to letters to supplement the number of signs available for the expression of all kinds of operations, it is better that the symbols expressing the most fundamental and frequently recurring operations should not be letters, and that a sign between the vectors, and, as it were, uniting them, is better than a sign before them in a case having a formal analogy with multiplication. The case may be compared with that of addition, for which $\alpha + \beta$ is evidently more convenient than $\Sigma(\alpha, \beta)$ or $\Sigma\alpha\beta$ would be. Similar considerations will apply to the function written in quaternions $V\alpha\beta$. It would seem that we obtain the *ne plus ultra* of simplicity and convenience, if we express the two functions by uniting the vectors in each case with a sign suggestive of multiplication. The particular forms of the signs which we adopt is a matter of minor consequence. In order to keep within the resources of an ordinary printing-office, I have used a dot and a cross, which are already associated with multiplication, but are not needed for ordinary multiplication, which is best denoted by the simple juxtaposition of the factors. I have no especial predilection for these particular signs. The use of the dot is indeed liable to the objection that it interferes with its use as a separatrix, or instead of a parenthesis.

If, then, I have written $\alpha . \beta$ and $\alpha \times \beta$ for what is expressed in quaternions by $-S\alpha\beta$ and $V\alpha\beta$, and in like manner $\nabla . \omega$ and $\nabla \times \omega$ for $-S\nabla\omega$ and $V\nabla\omega$ in quaternions, it is because the natural development of a vector analysis seemed to lead logically to some such notations. But I think that I can show that these notations have some substantial advantages over the quaternionic in point of convenience.

Any linear vector function of a variable vector ρ may be expressed in the form—

$$\alpha\lambda.\rho + \beta\mu.\rho + \gamma\nu.\rho = (\alpha\lambda + \beta\mu + \gamma\nu).\rho = \Phi.\rho,$$

where

$$\Phi = \alpha\lambda + \beta\mu + \gamma\nu;$$

or in quaternions

$$-\alpha S\lambda\rho - \beta S\mu\rho - \gamma S\nu\rho = -(\alpha S\lambda + \beta S\mu + \gamma S\nu)\rho = -\phi\rho,$$

where

$$\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu.$$

If we take the scalar product of the vector $\Phi . \rho$, and another vector σ, we obtain the scalar quantity

$$\sigma.\Phi.\rho = \sigma.(\alpha\lambda + \beta\mu + \gamma\nu).\rho,$$

or in quaternions

$$S\sigma\phi\rho = S\sigma(\alpha S\lambda + \beta S\mu + \gamma S\nu)\rho.$$

This is a function of σ and of ρ, and it is exactly the same kind of function of σ that it is of ρ, a symmetry which is not so clearly exhibited in the quaternionic notation as in the other. Moreover, we can write $\sigma.\Phi$ for $\sigma.(\alpha\lambda + \beta\mu + \gamma\nu)$. This represents a vector which is a function of σ, viz. the function conjugate to $\Phi.\sigma$; and $\sigma.\Phi.\rho$ may be regarded as the product of this vector and ρ. This is not so clearly indicated in the quaternionic notation, where it would be straining things a little to call $S\sigma\phi$ a vector.

The combinations $\alpha\lambda$, $\beta\mu$, &c., used above, are distributive with regard to each of the two vectors, and may be regarded as a kind of product. If we wish to express everything in terms of i, j, and k, Φ will appear as a sum of ii, ij, ik, ji, jj, jk, ki, kj, kk, each with a numerical coefficient. These nine coefficients may be arranged in a square, and constitute a matrix; and the study of the properties of expressions like Φ is identical with the study of ternary matrices. This expression of the matrix as a sum of products (which may be extended to matrices of any order) affords a point of departure from which the properties of matrices may be deduced with the utmost facility. The ordinary matricular product is expressed by a dot, as $\Phi . \Psi$. Other important kinds of multiplication may be defined by the equations—

$$(\alpha\lambda) \underset{\times}{\overset{\times}{}} (\beta\mu) = (\alpha \times \beta)(\lambda \times \mu), \qquad (\alpha\lambda) : (\beta\mu) = (\alpha.\beta)(\lambda.\mu).$$

With these definitions $\frac{1}{6}\Phi \underset{\times}{\overset{\times}{}} \Phi : \Phi$ will be the determinant of Φ, and $\Phi \underset{\times}{\overset{\times}{}} \Phi$ will be the conjugate of the reciprocal of Φ multiplied by twice the determinant. If Φ represents the manner in which vectors are affected by a strain, $\frac{1}{2}\Phi \times \Phi$ will represent the manner in which surfaces are affected, and $\frac{1}{6}\Phi \underset{\times}{\overset{\times}{}} \Phi : \Phi$ the manner in which volumes are affected. Considerations of this kind do not attach themselves so naturally to the notation $\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu$, nor does the subject admit so free a development with this notation, principally because the symbol S refers to a special use of the matrix, and is very much in the way when we want to apply the matrix to other uses, or to subject it to various operations.

J. WILLARD GIBBS.

New Haven, Connecticut.

The Meaning of Algebraic Symbols in Applied Mathematics.

PROF. GREENHILL, on p. 462 (March 19), gives a naïve and most instructive description of the straits to which a "practical man" is put when he wishes to interpret the simplest general formula.

I have always held, not in sarcasm but in sorrow, that students brought up on the system of specialized and limited numerical formulæ used by Prof. Greenhill and some other Professors of Engineering in this country, must necessarily go through the tentative trial-and-error sort of process which he so graphically describes, whenever they have to obtain a numerical result from anything not already arithmetical. In other words they are unable to deal with complete algebraic symbols or concrete quantities.

The symbol "v" to them does not completely represent a velocity, it only represents a number; and to make it represent a velocity some words, such as "in feet per second," must be added. Whereas, since it is plain that the velocity of light does not vary with its numerical specification, nor the size of a room change according as it is measured in feet or in inches, it is surely better to make a symbol express the essential and unchanging aspect of the thing to be dealt with, *i.e.* the thing itself, and not merely the number of some arbitrary and conventional units which the thing contains.

May I, then, assure Prof. Greenhill very seriously, *and with entire appreciation of and accord with his insistence that all expressions should be complete and capable of immediate practical numerical interpretation*, that the equation $T = \rho v^2$ is perfectly complete, and that it is true and immediately interpretable in every consistent system of units that has been or that can be invented? T is the tenacity, ρ the density, of the material of a ring, and v is its critical or bursting velocity. There is no need to say a word more. And no properly taught student ought to have the slightest difficulty in obtaining a numerical result directly in any system of conventional units that may be offered him.

It is the frequent recurrence of such ghastly parodies of formulæ as—

$$T = \frac{62 \cdot 4}{2240 \times 144} \cdot \frac{\rho v^2}{g}$$

in many engineering treatises which makes them such dismal reading. It is a standing wonder to physicists how a man of Prof. Greenhill's power can fail to see the inadequacy and tediousness of expressions which are only true in one particular system of units, and which to be true even in that require the special statement of every unit employed. For not only is Prof. Greenhill's expression long in itself, but it is incomplete without the tiresome addition, "ρ being measured in so and so, v in &c., T in something else, and g meaning nothing more than the pure number 32·2." All this has been needlessly put into the formula, and so has to be wearisomely taken out again.

If there be any physicist who does *not* contend for the concrete interpretation of algebraic terms (wherein each symbol is taken to represent the quantity *itself*, and not merely a numerical specification of it in some conventional unit—see, for fuller explanation, NATURE, vol. xxxviii. p. 281), I trust he will write and uphold his position on the side of Prof. Greenhill.

I suspect that the cause of Prof. Greenhill's failure at present to recognize the extreme simplicity and reasonableness of the physicist's procedure is to be found, partly in a vague idea that in order to get numerical results in British units from a general expression it is necessary to work it out in C.G.S. units first, and then translate, which I assure him is not the case; and partly in the general difficulty which most people feel in thinking it possible that they can be mistaken.

I would gladly convince Prof. Greenhill if I could, because he would carry with him so many other teachers, and thus a mass of waste labour would be saved annually to several thousands of students. Would it be too much to ask him to consider the matter with care, and, if possible, from our point of view; setting me a sum to do if that would be any assistance towards bringing him to the desired point of view?

OLIVER J. LODGE.

Tension of a "Girdle of the Earth."

IT is perfectly true, as Prof. Lodge has asserted, that a cord or chain running on its own track as an endless band in a frictionless groove *of any form* will not require the sides of the groove to keep it in that form. But whatever velocity it moves with, such a tension will exist all round it as to resist the centrifugal forces of its windings; and to preserve them by virtue of the curvature and constant tension, invariable in shape however the speed of coursing of the belt may be increased, without any external guidance and assistance. If w is the cord's mass per unit of its length, and v the velocity with which it pursues its course, wv^2 is the tension in dynes which will be set up all round it. The speed may of course be so increased as to tear the cord or chain to pieces; and this will occur in steel tires of railway wheels, for example, if the train's velocity on which the wheels are carried is much more than 120 miles an hour. Mr. Bourne long ago pointed out, in his works on the steam-engine, that a very low limit of speed in railway trains is enforced for safety in view of this dynamical condition so as not to approach and exceed working and proof-stresses at least in the material of which steel wheel-tires are formed.

But the truth of the proposition $T = wv^2$ rests entirely on the supposition that the running cord or cable pursues *exactly its own curve* in its motion. For a tension of 30 tons per square inch to be reached in a maritime cable in virtue of its being carried round either at the equator or at any distance of latitude from the equator, it must be presupposed that while buoyed with its own submerged lightness so as to be practically weightless in the water, it must from one end point of support to the other *follow accurately* the equator's circle of curvature, or the circle of curvature of the small circle of latitude along which it is laid, because this is *the line of motion* along which its parts are carried along by the earth's rotation. These circles are practically straight lines for any mile or two of cable, and truly enough, if in the presence of even the weak force acting "centrifugally" on the cable's mass by the earth's rotation, it is attempted (supposing it to be quite weightless otherwise) to pull it as nearly straight as the hardly sensible curvature of the earth requires, mathematics would not yield its point an inch, and a pull of