

LETTERS TO THE EDITOR.

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The Thieving of Assyrian Antiquities.

1. HAD I known that after having dissected my reply to the article entitled "Thieving of Assyrian Antiquities," which appeared in NATURE of the 10th ultimo, you had intended to add further objectionable remarks to it, I should have certainly declined to have had it published.

2. You seem, even now, to ignore the judgment of the High Court of Justice in the slander case of "Rassam v. Budge," and volunteer your own version of the story with which you have been supplied.

3. May I ask where you have found it reported about the evidence of the British Museum accountant and Sir Henry Rawlinson's deposition regarding the fragments of the national collection? If you have obtained your information from the latter's deposition that was certainly not revealed in the Press, and if it was supplied you by men who had no business to do so, then in fairness you ought to have quoted the other parts of the evidence. As for the "accountant," no paper reported what the Principal Librarian wanted him to say, and that was for a very good reason, because the Judge did not consider his evidence of any use, seeing that no one had disputed the purchase by the authorities of the British Museum, of Babylonian antiquities before I began my researches in Southern Mesopotamia, at the time when I was there and afterwards.

4. With regard to the cock-and-bull story about the bas-reliefs which are alleged to be at "Comford Hall," if you had said in your article, above referred to, that they existed in a private house in England, instead of asserting that they were obtained by purchase, I would have surprised you with further revelations that such "slabs" do exist in other houses in England and in different parts of Europe and America. Even half of the sculptures I had discovered in Assur-beni-pal's palace in 1853, belonging *legitimately* to the national collection, have been squandered, and part of them are now in the bottom of the Tigris.

5. As you seem to have allowed yourself to be imposed upon by malicious men who are not brave enough to put their names to the information with which they have supplied you, I must now close my correspondence, as it seems to me that your journal is not a proper channel through which justice can be obtained.

H. RASSAM.

6, Gloucester-walk, Kensington, W. September 23.

[THE above letter calls for some additional "remarks." We trust Mr. Rassam will find them less "objectionable" than the former ones.

1. The dissection to which reference is made consisted only of omissions of personal attacks, not even courteously worded, which moreover had nothing to do with the question of importance to the public.

2. Mr. Rassam is not happy here in his expressions. Nothing was stated in our article which was not openly stated in Court.

3. He is still less happy here. In his last letter he wished to make our readers believe that Sir H. Rawlinson's opinion on the "rubbish" Mr. Rassam had sent home was not stated in Court, and had been obtained by us in some improper way from the British Museum. In our "objectionable remarks" we charitably suggested that he had *forgotten* Sir H. Rawlinson's deposition containing this opinion was read in Court. It now seems that Mr. Rassam had not forgotten it in the least.

With regard to the accountant; the counsel for the defendant did say what the accountant was to prove, and the Editor does not see what the Principal Librarian had to do with it.

4. Why does Mr. Rassam take the trouble to misquote us by writing "Comford" instead of "Canford," and then to put his misquotation in inverted commas? The "story of a cock and bull," which we took from one edition of Murray's Guide is repeated in more detail in a later one, and even the name of the donor is mentioned, Sir A. H. Layard.

The more "revelations" Mr. Rassam can supply; the more he can show that property "*belonging legitimately*" (the italics are Mr. Rassam's) to the national collection" has been squandered;

the more reason there is for the inquiry to which we have pointed.

5. Requires no comment except that not a single inaccuracy on our part has been established.—ED. NATURE.]

Vectors and Quaternions.

I WISH to make some observations in reply to the letter of Prof. Knott which appeared in NATURE (June 15, p. 148). For my part I have nowhere condemned the system of Hamilton and Tait as "unnatural" and "weak"; on the contrary, I have always spoken of it with respect and admiration. To appreciate its value and high place in analysis it is not necessary to be blind to its imperfections and limitations. As to whether my work is mere innovation and a recasting of quaternion investigations, I leave to the judgment of those who read my papers. I wish merely to remark that Prof. Knott says nothing about exponentials, and that he has not pointed out what quaternion investigations are recast in my paper on "The Fundamental Theorems of Analysis Generalised for Space." It is the duty of a critic to state correctly and fully the principles which he criticises; this has not been done; my position has been misrepresented. It may aid the scientific discussion of this matter if I state briefly the principal positions I have taken, and the replies that have been given.

I have said that the quaternion notation can be improved. As regards notation, Hamilton himself was an innovator, and in his writings he apologises for the introduction of the strange symbols $S, V, T, K, U, I, &c.$ My aim has been to generalise as much as possible the notation of ordinary analysis, as it is desirable to have one harmonious algebra, with easy transition from line algebra to plane algebra, and from plane algebra to space algebra. Prof. Tait himself has said in one of the prefaces to his treatise that a revolution in the matter of notation must ultimately come; but I infer from the ecstasy of his admiration, that Prof. Knott considers it part of the original brightness of the Archangel.

I have said that the quaternion definitions are not all that can be wished for; I have pointed out what appear to be defects, and I have attempted to remove them. According to Prof. Knott, "the quaternion originally defined as the quotient of two vectors, can also be represented as the product of two quadrantal versors." I reply that what is wanted is not an original or temporary definition of "quaternion," but one that will stand throughout; that in strains we have a quotient of two vectors which is not a quaternion, but a dyad; that we do not ask for a representation, but a definition; and that the representation indicated involves the idea of a versor, which, leaving out a mere multiplier, is the very thing to be defined. Further, the following questions may be asked: If by a quaternion is meant the quotient of two vectors, how can the product of two vectors be a quaternion? We have also the nice distinction that a quaternion may be represented by the product, but not by the quotient, of two quadrantal versors. It is certain that the product and the quotient of two quadrantal versors are quantities of the same kind; if the one is a quaternion, so is the other.

I have said that some of the fundamental principles of quaternions require to be corrected, especially the one which identifies versors with vectors. I have said that if a denote a unit-vector, then $a^2 = 1$, not -1 . It is not a bare assertion that "to my mind" it appears so; a reason is given. Let a body of mass, m , have at any time a linear velocity whose rectangular components are a along the axis of i , b along j , and c along k ; the kinetic energy of the body is $\frac{1}{2}m(ai + bj + ck)^2$, that is, $\frac{1}{2}m(a^2 + b^2 + c^2)$, not as quaternionists would have it, $-\frac{1}{2}m(a^2 + b^2 + c^2)$. The convention involved is one that pervades the whole of analysis, namely, that the product of two lines having the same direction is positive, while the product of two lines having opposite directions is negative. As kinetic energy is a square, the two lines must always have the same direction.

I have said that if $a^{\frac{\pi}{2}}$ denote a quadrantal versor, then $(a^{\frac{\pi}{2}})^2 = a^{\pi} = -1$, and that Hamilton's rules apply to versors, not to vectors. Prof. Knott says that I advocate a system which loses the associative principle and gains nothing but a positive sign and an undesirable complexity in transforming by permutations. Readers of NATURE will be surprised to learn

that I advocate nothing of the sort. What I do advocate is to treat vectors as vectors, and versors as versors, and I show that the products of versors differ essentially from the products of vectors in that the associative rule applies to the former, but not to the latter. Prof. Knott justifies the treatment of quadrantal versors as vectors, because they are compounded according to the parallelogram law. It is true that the components of a quadrantal versor are so compounded, because every versor involves an axis; but the minus comes in, not on account of the axis, but on account of the angle of the versor, the very element which differentiates it from a vector.

I have said that $\nabla^2 = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}$ is more consistent with analysis than $\nabla^2 = -\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}\right)$, and I have remarked that in works on mathematical physics, even in Kelvin and Tait's "Natural Philosophy," the minus was dropped. A sign that can be so readily dropped has probably got no good reason for its appearance. In reply, Prof. Knott says that "when $\nabla^2 v$ occurs in ordinary non-quaternion analysis, it is used in the sense of the *tensor*, for only as such can it come in." This explanation does not explain; for "the name *tensor* is applied to the *positive* number which represents the length of a line" ("Hamilton's Elements," p. 164). Now the ordinary analysis is not limited to signless quantities, but embraces quantities which may be positive or negative. Why then is the minus dropped in an analysis where sign is essential? I asked for a proof of the principle that $\nabla(\nabla\omega) = \nabla^2\omega$; it is replied that "in quaternions there is no doubt whatever." Are we permitted, then, to doubt it as a truth in ordinary analysis, being true only in quaternions? If it is a matter of convention, no one desires two contradictory systems of analysis; if it is a matter of truth, it cannot be true "in quaternions" and not in ordinary analysis.

I have said that the rule $ij = k$ expresses what is true in space of three dimensions. Prof. Knott asks: "If a vector cannot be a versor in product combinations, what is the signification of the equation $ij = k$?" Let us first of all remove every ambiguity from the equation. We have then in all three cases: first, i and j both quadrantal versors; second, i a versor and j a vector; third, i and j both vectors. To distinguish between a quadrantal versor and a vector, let the former be

denoted by $i^{\frac{\pi}{2}}$. Then $i^{\frac{\pi}{2}} j^{\frac{\pi}{2}} = -k^{\frac{\pi}{2}}$ means the forward order being taken, that a quadrant round i followed by a quadrant round j is equivalent to a quadrant round the opposite of k .

Again, $i^{\frac{\pi}{2}} j = k$ means that the vector j , when turned through a quadrant round i coincides with k . Finally, ij means the unit of directed area which has i for base and j for altitude; for some purposes it may be represented by k on the principle that the axis of a plane may be specified by the axis which it wants; but at p. 92 of "The Principles of the Algebra of Physics," I have shown that the several types of products of vectors may be formed independently of that principle. Prof. Knott states that he fails to see what physical considerations have to do with mathematics of the fourth dimension. It is evident, however, that his perception cannot be taken as a criterion of truth, for every type of product of four vectors is geometrically real excepting the one which supposes them all independent of one another.

I have said that the rules for differentiation are much simplified when vectors and versors are not confounded. In proof of this I invite comparison.

I have said that the principles of quaternions can be greatly extended. In my papers will be found for the first time the extension of space analysis to logarithmic spirals and to hyperbolic trigonometry. The connection of the latter with non-euclidean geometry is also pointed out. As further evidence of the fruitfulness of my notation and principles I may mention that I have just read before the Mathematical Congress assembled at Chicago two papers—one on "The Definitions of the Trigonometric Functions," the other on "The Principles of Elliptic and Hyperbolic Analysis." These papers give the trigonometry of the elliptic and hyperbolic surfaces.

As regards Prof. Knott's closing quotation from "Paradise Lost," I feel like the Senior Wrangler who, having read through the poem, remarked that it was all very pretty, but he didn't quite see what it proved. I close with a quotation which is

from as good a book, and possesses more logical force: "Ye shall know them by their fruits. Do men gather grapes of thorns, or figs of thistles?" ALEXANDER MACFARLANE.
Chicago, Ill., August 26.

Astronomical Photography.

THE letter from Lord Rayleigh in your issue of August 24, on the subject of "Astronomical Photography," will, it is to be hoped, elicit some information from photographic experts.

Meanwhile, accepting what Lord Rayleigh says as to the present possibilities in the preparation of plates, I fail to see where any considerable saving is to be effected in the cost of the apparatus, as he appears to suggest.

For astronomical photography a pair of telescopes are required. The larger of these is employed to take the photographs, and the smaller acts as a guider. Supposing that plates could be obtained which were acted upon by visual rays, while comparatively insensible to the violet and ultra-violet light, this would simply mean that both the objectives would have to be made visually perfect, instead of having one of them as heretofore corrected for violet and ultra-violet light. A photographic objective is no more costly than a visual one of the same aperture; and as to mounting clockwork and dome, there could be no difference in expense.

Of course, if the necessity for a separate guiding telescope could be avoided by the adoption of Lord Rayleigh's suggestion, there would in general be some saving of expense; it should, however, be noted, that even when reflectors are employed for taking the photographs, it has not been always found desirable to dispense with the guiding telescope, though in this case, of course, the question as to the nature of the plates cannot arise at all.

In the particular instance of the instrument now proposed for Cambridge, the guiding telescope is already to hand in the shape of the present Northumberland instrument.

It is certainly easier to test the qualities of an objective corrected for visual rays than for photographic rays (if I may still use language which Lord Rayleigh has pointed out as incorrect). On this account it would, therefore, be desirable to have plates such as he refers to, rendered available for astronomers engaged in photographic work.

ROBERT S. BALL.
Observatory, Cambridge, September 12.

P.S.—Sir Gabriel Stokes, after reading the above, writes: "I would ask whether in an orthochromatic plate the blue and violet are impressed more feebly than the rays which are visually the brightest. It may be so, but I do not happen to know whether it is."

The Constellations of the Far East.

WITH regard to the questions asked by "M. A. B." about the grouping of stars into constellations (NATURE, August 17), I venture to answer the last two, which the limited knowledge of an Oriental may partly meet, hoping thereby to interest some of your readers.

I do not consider that each race necessarily relies on its own plan in the fabrication of constellations. The Coreans and Anamese are said to be still adhering to the Chinese system, and till lately the Japanese were doing so. It is strange to find the latter, replete with so peculiar mythology, on which the national claim for high ancestry rests, possessing very few vernacular constellations.

Undoubtedly the Chinese system is of peculiar aspect. A name is given to a "Seat," which is sometimes a single star, but in general a group of stars, varying in number from two to twenty or thirty; and in one group, the Imperial Bodyguards, they amount to forty-five. Occasionally the same stars are at once named collectively and individually; thus, the first seven stars of Ursa Major are grouped into Peh-tau or the North Ladle, of which the scoop consists of Shu α , Siuen β , Ki γ , and Kiuen δ , and the handle of Yuh-hang ϵ , Kai-yang ζ , and Yau-Kwang η . With Polaris as the centre, the heavens are radiantly divided into the twenty-eight "Inns" of unequal breadths, each division being denominated after its typical constellation, besides enclosing numerous Seats subordinate to the latter.

The fundamental idea of the plan is enigmatically expressed thus: "Sing (the star) is Tsing (the spirit)." Its solution con-