QUATERNIONS

Peter Guthrie Tait, M.A. Sec. R.S.E.

Encyclopædia Britannica, Ninth Edition, 1886, Vol. XX, pp. 160-164.

THE word quaternion properly means "a set of four." In employing such a word to denote a new mathematical method, Sir W. R. Hamilton was probably influenced by the recollection of its Greek equivalent, the Pythagorean Tetractys, the mystic source of all things.

Quaternions (as a mathematical *method*) is an extension, or improvement, of Cartesian geometry, in which the artifices of coordinate axes, &c., are got rid of, *all* directions in space being treated on precisely the same terms. It is therefore, except in some of its degraded forms, possessed of the perfect isotropy of Euclidian space.

From the purely geometrical point of view, a quaternion may be regarded as *the quotient of two directed lines in space*—or, what comes to the same thing, as *the factor, or operator, which changes one directed line into another*. Its analytical definition cannot be given for the moment; it will appear in the course of the article.

History of the Method.—The evolution of quaternions belongs in part to each of two weighty branches of mathematical history—the interpretation of the *imaginary* (or *impossible*) quantity of common algebra, and the Cartesian application of algebra to geometry. Sir W. R. Hamilton was led to his great invention by keeping geometrical applications constantly before him while he endeavoured to give a real significance to $\sqrt{-1}$. We will therefore confine ourselves, so far as his predecessors are concerned, to attempts at interpretation which had geometrical applications in view.

One geometrical interpretation of the negative sign of algebra was early seen to be mere *reversal* of direction along a line. Thus, when an image is formed by a plane mirror, the distance of any point in it from the mirror is simply the negative of that of the corresponding point of the object. Or if motion in one direction along a line be treated as positive, motion in the opposite direction along the same line is negative. In the case of time, measured from the Christian era, this distinction is at once given by the letters A.D. or B.C., prefixed to the date. And to find the position, in time, of one event relatively to another, we have only to subtract the date of the second (taking account of its sign) from that of the first. Thus to find the interval between the battles of Marathon (490 B.C.) and Waterloo (1815 A.D.) we have

$$+1815-(-490)=2305 \quad \text{years.}$$

And it is obvious that the same process applies in all cases in which we deal with quantities which may be regarded as of one directed dimension only, such as distances along a line, rotations about an axis, &c. But it is essential to notice that this is by no means necessarily true of *operators*. To turn a line through a certain angle in a given plane, a certain operator is required; but when we wish to turn it through an equal negative angle we must not, in general, employ the negative of the former operator. For the negative of the operator which turns a line through a given angle in a given plane will in all cases produce the negative of the original result, which is *not* the result of the reverse operator, unless the angle involved be an odd multiple of a right angle. This is, of course, on the usual assumption that the sign of a product is changed when that of *any one* of its factors is changed,—which merely means that -1 is commutative with all other quantities.

The celebrated Wallis seems to have been the first to push this idea further. In his *Treatise of Algebra* (1685) he distinctly proposes to construct the imaginary roots of a quadratic equation by going *out* of the line on which the roots, if real, would have been constructed.

In 1804 the Abbé Buée[1], apparently without any knowledge of Wallis's work, developed this idea so far as to make it useful in geometrical applications. He gave, in fact, the theory of what in Hamilton's system is called *Composition of Vectors in one plane*—*i.e.*, the combination, by $+$ and $-$, of complanar directed lines. His constructions are based on the idea that the imaginaries $\pm\sqrt{-1}$ represent a unit line, and its reverse, *perpendicular to* the line on which the real units ± 1 are measured. In this sense the imaginary expression $a+b\sqrt{-1}$ is constructed by measuring a length a along the fundamental line (for real quantities), and from its extremity a line of length b in some direction perpendicular to the fundamental line. But he did not attack the question of the representation of products or quotients of directed lines. The step he took is really nothing more than the kinematical principle of the composition of linear velocities, but expressed in terms of the algebraic imaginary.

In 1806 (the year of *publication* of Buée's paper) Argand published a pamphlet[2]

---

[1] *Phil. Trans.*, 1806.

[2] *Essai sur une manière de représenter les Quantités Imaginaires dans les Constructions Géométriques*. A second edition was published by Hoüel (Paris, 1874). There is added an important *Appendix*, consisting of the papers from *Gergonne's Annales* which are referred to in the text

in which precisely the same ideas are developed, but to a considerably greater extent. For an interpretation is assigned to the *product* of two directed lines in one plane, when each is expressed as the sum of a real and an imaginary part. This product is interpreted as another directed line, forming the fourth term of a proportion, of which the first term is the real (positive) unit-line, and the other two are the factor-lines. Argand's work remained unnoticed until the question was again raised in *Gergonne's Annales*, 1813, by Français. This writer stated that he had found the germ of his remarks among the papers of his deceased brother, and that they had come from Legendre, who had himself received them from some one unnamed. This led to a letter from Argand, in which he stated his communications with Legendre, and gave a résumé of the contents of his pamphlet. In a further communication to the *Annales*, Argand pushed on the applications of his theory. He has given by means of it a simple proof of the existence of n roots, and no more, in every rational algebraic equation of the nth degree with real coefficients. About 1828 Warren in England, and Mourey in France, independently of one another and of Argand, reinvented these modes of interpretation; and still later, in the writings of Cauchy, Gauss, and others, the properties of the expression $a+b\sqrt{-1}$ were developed into the immense and most important subject now called the *theory of complex numbers*. From the more purely symbolical point of view it was developed by Peacock, De Morgan, &c., as *double algebra*.

Argand's method may be put, for reference, in the following form. The directed line whose length is a, and which makes an angle θ with the real (positive) unit line, is expressed by

$$a(\cos\theta + i\sin\theta),$$

where i is regarded as $+\sqrt{-1}$. The sum of two such lines (formed by adding together the real and the imaginary parts of two such expressions) can, of course, be expressed as a third directed line—the diagonal of the parallelogram of which they are conterminous sides. The product, P, of two such lines is, as we have seen, given by

$$1 : a(\cos\theta + i\sin\theta) :: a'(\cos\theta' + i\sin\theta') : P,$$

or

$$P = aa'\{\cos(\theta+\theta') + i\sin(\theta+\theta')\}.$$

Its length is, therefore, the product of the lengths of the factors, and its inclination to the real unit is the sum of those of the factors. If we write the expressions for the two lines in the form $A+Bi$, $A'+B'i$, the product is $AA'-BB'+i(AB'+BA')$;

above. Almost nothing can, it seems, be learned of Argand's private life, except that in all probability he was born at Geneva in 1768.

and the fact that the length of the product line is the product of those of the factors is seen in the form

$$(A^2+B^2)(A'^2+B'^2) = (AA'-BB')^2+(AB'+BA')^2.$$

In the modern theory of complex numbers this is expressed by saying that the *Norm* of a product is equal to the product of the norms of the factors.

Argand's attempts to extend his method to space generally were fruitless. The reasons will be obvious later; but we mention them just now because they called forth from Servois (Gergonne's *Annales*, 1813) a very remarkable comment, in which was contained the only yet discovered trace of an anticipation of the method of Hamilton. Argand had been led to deny that such an expression as i^i could be expressed in the form $A+Bi$,—although, as is well known, Euler showed that one of its values is a real quantity, the exponential function of $-\pi/2$. Servois says, with reference to the general representation of a directed line in space:—

> "L'analogie semblerait exiger que le trinôme fût de la forme
>
> $$p\cos\alpha + q\cos\beta + r\cos\gamma;$$
>
> α, β, γ étant les angles d'une droite avec trois axes rectangulaires; et qu'on eût
>
> $$(p\cos\alpha+q\cos\beta+r\cos\gamma)(p'\cos\alpha+q'\cos\beta+r'\cos\gamma)$$
>
> $=\cos^2\alpha+\cos^2\beta+\cos^2\gamma=1$. Les valeurs de p, q, r, p', q', r' qui satisferaient à cette condition seraient *absurdes*; mais seraient-elles imaginaires, reductibles à la forme générale $A+B\sqrt{-1}$? Voilà une question d'analyse fort singulière que je soumets à vos lumières. La simple proposition que je vous en fais suffit pour vous faire voir que je ne crois point que tout fonction analytique non réelle soit vraiment reductible à la forme $A+B\sqrt{-1}$."

As will be seen later, the fundamental i, j, k of quaternions, with their reciprocals, furnish a set of six quantities which satisfy the conditions imposed by Servois. And it is quite certain that they cannot be represented by ordinary imaginaries.

Something far more closely analogous to quaternions than anything in Argand's work ought to have been suggested by De Moivre's theorem (1730). Instead of regarding, as Buée and Argand had done, the expression $a(\cos\theta+i\sin\theta)$ as a directed line, let us suppose it to represent the *operator* which, when applied to *any* line in the plane in which θ is measured, turns it in that plane through the angle θ, and at the same time increases its length in the ratio a : 1. From the new point of view we see at once, as it were, *why* it is true that

$$(\cos\theta + i\sin\theta)^m = \cos m\theta + i\sin m\theta.$$

For this equation merely states that m turnings of a line through successive equal angles, in one plane, give the same result as a single turning through m times the common angle. To make this process applicable to *any* plane in space, it is clear that we must have *a special value of i* for each such plane. In other words, a unit line, drawn in any direction whatever, must have -1 for its square. In such a system there will be no line in space specially distinguished as the *real unit line*: all will be alike imaginary, or rather alike real. We may state, in passing, that every quaternion can be represented as $a(\cos\theta + \omega\sin\theta)$,—where a is a real number, θ a real angle, and ω a directed unit line whose square is -1. Hamilton took this grand step, but, as we have already said, without any help from the previous work of De Moivre. The course of his investigations is minutely described in the preface to his first great work[3] on the subject. Hamilton, like most of the many inquirers who endeavoured to give a real interpretation to the imaginary of common algebra, found that at least two kinds, orders, or ranks of quantities were necessary for the purpose. But, instead of dealing with points on a line, and then wandering out at right angles to it, as Buée and Argand had done, he chose to look on algebra as the science of *pure time*[4], and to investigate the properties of "sets" of time-steps. In its essential nature a set is a linear function of any number of *distinct* units of the same species. Hence the simplest form of a set is a *couple* and it was to the possible laws of combination of couples that Hamilton first directed his attention. It is obvious that the way in which the two separate time-steps are involved in the couple will determine these laws of combination. But Hamilton's special object required that these laws should be such as to lead to certain assumed results; and he therefore commenced by assuming these, and from the assumption determined how the separate time-steps must be involved in the couple. If we use Roman letters for mere numbers, capitals for instants of time, Greek letters for time-steps, and a parenthesis to denote a couple, the laws assumed by Hamilton as the basis of a system were as follows:—

$$(B_1, B_2) - (A_1, A_2) = (B_1 - A_1, B_2 - A_2) = (\alpha, \beta);$$
$$(a, b)(\alpha, \beta) = (a\alpha - b\beta, b\alpha + a\beta).^5$$

[3] *Lectures on Quaternions*, Dublin, 1853.

[4] *Theory of Conjugate Functions, or Algebraic Couples, with a Preliminary and Elementary Essay on Algebra as the Science of Pure Time*, read in 1833 and 1835, and published in *Trans. R.I.A.*, XVII. ii. (1835).

To show how we give, by such assumptions, a real interpretation to the ordinary algebraic imaginary, take the simple case $a = 0$, $b = 1$, and the second of the above formulægives

$$(0,1)(\alpha,\beta) = (-\beta,\alpha).$$

Multiply once more by the number-couple $(0,1)$, and we have

$$\begin{aligned}(0,1)(0,1)(\alpha,\beta) &= (0,1)(-\beta,\alpha) = (-\alpha,-\beta)\\ &= (-1,0)(\alpha,\beta) = -(\alpha,\beta).\end{aligned}$$

Thus the number-couple $(0,1)$, when twice applied to a step-couple, simply changes its sign. That we have here a perfectly *real* and intelligible interpretation of the ordinary algebraic imaginary is easily seen by an illustration, even if it be a somewhat extravagant one. Some Eastern potentate, possessed of absolute power, covets the vast possessions of his vizier and of his barber. He determines to rob them both (an operation which may be very satisfactorily expressed by -1); but, being a wag, he chooses his own way of doing it. He degrades his vizier to the office of barber, taking all his goods in the process; and makes the barber his vizier. Next day he repeats the operation. Each of the victims has been restored to his former rank, but the operator -1 has been applied to both.

Hamilton, still keeping prominently before him as his great object the invention of a method applicable to space of three dimensions, proceeded to study the properties of *triplets* of the form $x + jy + jz$, by which he proposed to represent the directed line in space whose projections on the coordinate axes are x, y, z. The composition of two such lines by the algebraic addition of their several projections agreed with the assumption of Buée and Argand for the case of coplanar lines. But, assuming the *distributive* principle, the product of two lines appeared to give the expression

$$xx' - yy' - zz' + i(yx' + xy') + j(xz' + zx') + ij(yz' + zy').$$

For the square of j, like that of i, was assumed to be negative unity. But the interpretation of ij presented a difficulty,—in fact *the main difficulty* of the whole investigation, —and it is specially interesting to see how Hamilton attacked it. He saw that he could get a hint from the simpler case, already thoroughly discussed, provided the two factor lines were in one plane through the real unit line. This requires merely that

$$y : z :: y' : z'; \qquad \text{or} \qquad yz' - zy' = 0;$$

[5]Compare these with the long-subsequent ideas of Grassmann, presently to be described.

but then the product should be of the same form as the separate factors. Thus, in this special case, the term in ij ought to vanish. But the numerical factor appears to be $yz' + zy'$, while it is the quantity $yz' - zy'$ which really vanishes. Hence Hamilton was at first inclined to think that ij must be treated as *nil*. But he soon saw that "a less harsh supposition" would suit the simple case. For his speculations on sets had already familiarized him with the idea that multiplication might in certain cases not be commutative; so that, as the last term in the above product is made up of the two separate terms $ijyz'$ and $jizy'$, the term would vanish of itself when the factor lines are coplanar provided $ij = -ji$, for it would then assume the form $ij(yz' - zy')$. He had now the following expression for the product of any two directed lines

$$xx' - yy' - zz' + i(yx' + xy') + j(xz' + zx') + ij(yz' - zy').$$

But his result had to be submitted to another test, the Law of the Norms. As soon as he found, by trial, that this law was satisfied, he took the final step. "This led me," he says, "to conceive that perhaps, instead of seeking to *confine* ourselves to *triplets*, ... we ought to regard these as only *imperfect forms* of Quaternions, ... and that thus my old conception of *sets* might receive a new and useful application." In a very short time he settled his fundamental assumptions. He had now three distinct space-units i, j, k; and the following conditions regulated their combination by multiplication:—

$$i^2 = j^2 = k^2 = -1, \quad ij = -ji = k, \quad jk = -kj = i, \quad ki = -ik = j.$$[6]

And *now* the product of two quaternions could be at once expressed as a third quaternion, thus—

$$(a + ib + jc + kd)(a' + ib' + jc' + kd') = A + iB + jC + kD,$$

where

$$\begin{aligned} A &= aa' - bb' - cc' - dd', \\ B &= ab' + ba' + cd' - dc', \\ C &= ac' + ca' + db' - bd', \\ D &= ad' + da' + bc' - cb'. \end{aligned}$$

Hamilton at once found that the Law of the Norms holds,—not being aware that Euler had long before decomposed the product of two sums of four squares into this

[6] It will be easy to see that, instead of the last three of these, we may write the single one $ijk = -1$.

very set of four squares. And now a directed line in space came to be represented as $ix + jy + kz$, while the product of two lines is the quaternion

$$-(xx' + yy' + zz') + i(yz' - zy') + j(zx' - xz') + k(xy' - yx').$$

To any one acquainted, even to a slight extent, with the elements of Cartesian geometry of three dimensions, a glance at the extremely suggestive constituents of this expression shows how justly Hamilton was entitled to say—"When the conception ... had been so far unfolded and fixed in my mind, I felt that the *new instrument for applying calculation to geometry*, for which I had so long sought, was now, at least in part, attained." The date of this memorable discovery is October 16, 1843.

We can devote but a few lines to the consideration of the expression above. Suppose, for simplicity, the factor lines to be each of unit length. Then x, y, z, x', y', z' express their direction-cosines. Also, if θ be the angle between them, and x'', y'', z'' the direction-cosines of a line perpendicular to each of them, we have

$$xx' + yy' + zz' = \cos\theta, \quad yz' - zy' = x''\sin\theta, \quad \&c.,$$

so that the product of two unit lines is now expressed as

$$-\cos\theta + (ix'' + jy'' + kz'')\sin\theta.$$

Thus, when the factors are parallel, or $\theta = 0$, the product, which is now the square of any (unit) line, is -1. And when the two factor lines are at right angles to one another, or $\theta = \pi/2$, the product is simply $ix'' + jy'' + kz''$, the unit line perpendicular to both. Hence, and in this lies the main element of the symmetry and simplicity of the quaternion calculus, *all systems of three mutually rectangular unit lines in space have the same properties as the fundamental system* i, j, k. In other words, if the system (considered as rigid) be made to turn about till the *first* factor coincides with i and the second with j, the product will coincide with k. This fundamental system, therefore, becomes unnecessary; and the quaternion method, in every case, takes its reference lines solely from the problem to which it is applied. It has therefore, as it were, a unique *internal* character of its own.

Hamilton, having gone thus far, proceeded to evolve these results from a train of *a priori* or metaphysical reasoning, which is so interesting in itself, and so characteristic of the man, that we briefly sketch its nature.

Let it be supposed that the product of two directed lines is something which has quantity; i.e., it may be halved, or doubled, for instance. Also let us assume (a) space to have the same properties in all directions, and make the convention (b) that to change the sign of any one factor changes the sign of a product. Then the product of two lines which have the same direction *cannot be*, even in part, *a directed quantity*. For, if the directed part have the same direction as the factors, (*b*) shows that it will be reversed by reversing either, and therefore will recover its original direction when both are reversed. But this would obviously be inconsistent with (*a*). If it be perpendicular to the factor lines, (*a*) shows that it must have simultaneously every such direction. Hence it *must* be a mere number.

Again, the product of two lines at right angles to one another cannot, even in part, be a number. For the reversal of either factor must, by (*b*), change its sign. But, if we look at the two factors in their new position by the light of (*a*), we see that the sign must not change. But there is nothing to prevent its being represented by a directed line if, as farther applications of (*a*) and (*b*) show we must do, we take it perpendicular to each of the factor lines.

Hamilton seems never to have been quite satisfied with the apparent *heterogeneity* of a quaternion, depending as it does on a numerical and a directed part. He indulged in a great deal of speculation as to the existence of an *extra-spatial unit*, which was to furnish the *raison d'être* of the numerical part, and render the quaternion *homogeneous* as well as linear. But, for this, we must refer to his own works.

Hamilton was not the only worker at the theory of sets. The year after the first publication of the quaternion method, there appeared a work of great originality, by Grassmann[7], in which results closely analogous to some of those of Hamilton were given. In particular two species of multiplication ("inner" and "outer") of directed lines in one plane were given. The results of these two kinds of multiplication correspond respectively to the numerical and the directed parts of Hamilton's quaternion product. But Grassmann distinctly states in his preface that he had not had leisure to extend his method to angles in space. Hamilton and Grassmann, while their earlier work had much in common, had very different objects in view. Hamilton, as we have seen, bad geometrical application as his main object; when he realized the quaternion system, he felt that his object was gained, and thenceforth confined himself to the development of his method. Grassmann's object seems to have been, all along, of a much more ambitious character, viz., to discover, if possible, a system or systems in which every conceivable mode of dealing with sets should be included. That he made very great advances towards the attainment of this object all will allow; that his method, even as completed in 1862, fully attains it is not so certain. But his claims, however great they may be, can in no way

[7] *Die Ausdehnungslehre*, Leipsig, 1844, 2d ed., "*vollständig und in strenger Form bearbeitet*," Berlin, 1862. See also the collected works of Möbius, and those of Clifford, for a general explanation of Grassmann's method.

conflict with those of Hamilton, whose mode of multiplying *couples* (in which the "inner" and "outer" multiplication are essentially involved) was produced in 1833, and whose quaternion system was completed and published before Grassmann had elaborated for press even the rudimentary portions of his own system, in which the veritable difficulty of the whole subject, the application to angles in space, bad not even been attacked. Grassmann made in 1854 a somewhat savage onslaught on Cauchy and De St Venant, the former of whom had invented, while the latter had exemplified in application, the system of *"clefs algébriques,"* which is almost precisely that of Grassmann. [See letter now appended to this article. 1899.] But it is to be observed that Grassmann, though he virtually accused Cauchy of plagiarism, does not appear to have preferred any such charge against Hamilton. He does not allude to Hamilton in the second edition of his work. But in 1877, in the *Mathematische Annalen,* XII., he gave a paper "On the Place of Quaternions in the *Ausdehnungslehre*," in which he condemns, as far as he can, the nomenclature and methods of Hamilton.

There are many other systems, based on various principles, which have been given for application to geometry of directed lines, but those which deal with products of lines are all of such complexity as to be practically useless in application. Others, such as the *Barycentrische Calcül* of Möbius, and the *Méthode des Équipollences* of Bellavitis, give elegant modes of treating space problems, so long as we confine ourselves to projective geometry and matters of that order but they are limited in their field, and therefore need not be discussed here. More general systems, having close analogies to quaternions, have been given since Hamilton's discovery was published. As instances we may take Goodwin's and O'Brien's papers in the *Cambridge Philosophical Transactions* for 1849.

Relations to other Branches of Science.—Even the above brief narrative shows how close is the connexion between quaternions and the ordinary Cartesian space-geometry. Were this all, the gain by their introduction would consist mainly in a clearer insight into the mechanism of coordinate systems, rectangular or not—a very important addition to theory, but little advance so far as practical application is concerned. But we have now to consider that, as yet, we have not taken advantage of the *perfect symmetry* of the method. When that is done, the full value of Hamilton's grand step becomes evident, and the gain is quite as extensive from the practical as from the theoretical point of view. Hamilton, in fact, remarks[8], "I regard it as an inelegance and imperfection in this calculus, or rather in the state to which it has hitherto been unfolded, whenever it becomes, or *seems* to become, necessary to have recourse ... to the resources of ordinary algebra, for the *solution*

[8] *Lectures on Quaternions,* # 513.

of equations in quaternions." This refers to the use of the x, y, z coordinates,—associated, of course, with i, j, k. But when, instead of the highly artificial expression $ix + jy + kz$, to denote a finite directed line, we employ a single letter, α (Hamilton uses the Greek alphabet for this purpose), aud find that we are permitted to deal with it exactly as we should have dealt with the more complex expression, the immense gain is at least in part obvious. Any quaternion may now be expressed in numerous simple forms. Thus we may regard it as the sum of a number and a line, $a + \alpha$, or as the product, $\beta\gamma$, or the quotient, $\delta\varepsilon^{-1}$, of two directed lines, &c., while, in many cases, we may represent it, so far as it is required, by a single letter such as q, r, &c.

Perhaps to the student there is no part of elementary mathematics so repulsive as is spherical trigonometry. Also, everything relating to change of systems of axes, as for instance in the kinematics of a rigid system, where we have constantly to consider one set of rotations with regard to axes fixed in space, and another set with regard to axes fixed in the system, is a matter of troublesome complexity by the usual methods. But every quaternion formula is a proposition in spherical (sometimes degrading to plane) trigonometry, and has the full advantage of the symmetry of the method. And one of Hamilton's earliest advances in the study of his system (an advance independently made, only a few months later, by Cayley) was the interpretation of the singular operator $q(\quad)q^{-1}$, where q is a quaternion. Applied to *any* directed line, this operator at once turns it, *conically,* through a definite angle, about a definite axis. Thus rotation is now expressed in symbols at least as simply as it can be exhibited by means of a model. Had quaternions effected nothing more than this, they would still have inaugurated one of the most necessary, and apparently impracticable, of reforms.

The physical properties of a heterogeneous body (provided they vary *continuously* from point to point) are known to depend, in the neighbourhood of any one point of the body, on a quadric function of the coordinates with reference to that point. The same is true of physical quantities such as potential, temperature, &c., throughout small regions in which their variations are continuous and also, without restriction of dimensions, of moments of inertia, &c. Hence, in addition to its geometrical applications to surfaces of the second order, the theory of quadric functions of position is of fundamental importance in physics. Here the symmetry points at once to the selection of the three principal axes as the directions for i, j, k; and it would appear at first sight as if quaternions could not simplify, though they might improve in elegance, the solution of questions of this kind. But it is not so. Even in Hamilton's earlier work it was shown that all such questions were reducible to the *solution of linear equations in quaternions*; and he proved that this, in turn, depended on the determination of a certain operator, which could be represented for purposes of calculation by a single symbol. The method is essentially the same as that developed, under the name of "matrices" by Cayley in 1858 but it has the peculiar advantage of the simplicity which is the natural consequence of entire freedom from conventional reference lines.

Sufficient has already been said to show the close connexion between quaternions and the theory of numbers. But one most important connexion with modern physics must be pointed out, as it is probably destined to be of great service in the immediate future. In the theory of surfaces, in hydrokinetics, heat-conduction, potentials, &c., we constantly meet with what is called *Laplace's operator*, viz.,

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}.$$

We know that this is an *invariant*; *i.e.*, it is independent of the particular directions chosen for the rectangular coordinate axes. Here, then, is a case specially adapted to the isotropy of the quaternion system and Hamilton easily saw that the expression

$$i\frac{d}{dx}+j\frac{d}{dy}+k\frac{d}{dz}$$

could be, like $ix+jy+kz$, effectively expressed by a single letter. He chose for this purpose ∇. And we now see that the square of ∇ is the negative of Laplace's operator; while ∇ itself, when applied to any numerical quantity conceived as having a definite value at each point of space, gives *the direction and the rate of most rapid change* of that quantity. Thus, applied to a potential, it gives the direction and magnitude of the force; to a distribution of temperature in a conducting solid, it gives (when multiplied by the conductivity) the flux of heat, &c.

No better testimony to the value of the quaternion method could be desired than the constant use made of its notation by mathematicians like Clifford (in his *Kinematic*) and by physicists like Clerk-Maxwell (in his *Electricity and Magnetism*). Neither of these men professed to employ the calculus itself, but they recognized fully the extraordinary clearness of insight which is gained even by merely translating the unwieldy Cartesian expressions met with in hydrokinetics and in electrodynamics into the pregnant language of quaternions.

Works on the Subject.—Of course the great works on this subject are the two immense treatises by Hamilton himself. Of these the second (*Elements of Quaternions*, London, 1866; 2nd ed. 1899) was posthumous—incomplete in one short part of the original plan only, but that a most important part, the theory and applications of ∇. These two works, along with Hamilton's other papers on quaternions (in the *Dublin Proceedings* and *Transactions*, the *Philosophical Magazine*, &c.),

are storehouses of information, of which but a small portion has yet been extracted. A German translation of Hamilton's *Elements* has recently been published by Glan.

Other works on the subject, in order of date, are Allegret, *Essai sur le Calcul des Quaternions* (Paris, 1862); Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867; 2nd ed., Cambridge, 1873; 3rd, 1890; German translation by V. Scherff, 1880, and French by Plarr, 1882-84); Kelland and Tait, *Introduction to Quaternions* (London, 1873; 2nd ed. 1882); Hoüel, *Éléments de la Théorie des Quaternions* (Paris, 1874); Unverzagt, *Theorie der Quaternionen* (Wiesbaden, 1876); Laisant, *Introduction à la Méthode des Quaternions* (Paris, 1881); Graefe, *Vorlesungen über die Theorie der Quaternionen* (Leipsig, 1884). [To these must now be added McAulay, Utility of Quaternions in Physics, London, 1893; as well as a number of elementary treatises. 1899.]

An excellent article on the "Principles" of the science, by Dillner, will be found in the *Mathematische Annalen*, vol. XI., 1877. And a very valuable article on the general question, *Linear Associative Algebra*, by the late Prof. Peirce, was ultimately printed in vol. iv. of the American Journal of Mathematics. Sylvester and others have recently published extensive contributions to the subject, including quaternions under the general class *matrix*, and have developed much farther than Hamilton lived to do the solution of equations in quaternions. Several of the works named above are little more than compilations, and some of the French ones are painfully disfigured by an attempt to introduce an improvement of Hamilton's notation; but the mere fact that so many have already appeared shows the sure progress which the method is now making.

[In an article by Prof. F. Klein (Math. Ann. LI. 1898) a claim is somewhat obscurely made for Gauss to a share, at least, in the invention of Quaternions. Full information on the subject is postponed till the publication of Gauss' Nachlass, in Vol. VIII. of his Gesammelte Werke. From the article mentioned above, and from a "Digression on Quaternions" in Klein und Sommerfeld Ueber die Theorie des Kreisels (p. 58), this claim appears to rest on some singular misapprehension of the nature of a Quaternion:—whereby it is identified with a totally different kind of concept, a certain very restricted form of linear and vector Operator. 1899.]

APPENDIX.

QUATERNIONS AND THE AUSDEHNUNGSLEHRE.

[Nature, June 4th, 1891.]

Prof. Gibbs' second long letter was evidently written before he could have read my reply to the first. This is unfortunate, as it tends to confuse those third parties who may be interested in the question now raised. Of course that question is naturally confined to the invention of methods, for it would be preposterous to compare Grassmann with Hamilton as an analyst.

I have again read my article "Quaternions" in the *Encyc. Brit.*, and have consulted once more the authorities there referred to. I have not found anything which I should wish to *alter*. There is much, of course, which I should have liked to extend, had the Editor permitted. An article on Quaternions, rigorously limited to four pages, could obviously be no place for a discussion of Grassmann's scientific work, except in its bearings upon Hamilton's calculus. Moreover, had a similar article on the *Ausdehnungslehre* been asked of me, I should certainly have declined to undertake it. Since 1860, when I ceased to be a Professor of Mathematics, I have paid no special attention to general systems of *Sets, Matrices,* or *Algebras*; and without much further knowledge I should not attempt to write in any detail about such subjects. I may, however, call attention to the facts which follow for they appear to be decisive of the question now raised. Cauchy (*Comptes Rendus*, 10/1/53) claimed quaternia as a special case of his "clefs algébriques." Grassmann, in turn (Comptes Rendus, 17/4/54; and *Crelle*, 49), declared Cauchy's methods to be precisely those of the Ausdehnungslehre. But Hamilton (*Lectures*, Pref. p. 64, foot-note), says of the clefs algébriques (and therefore, *on Grassmann's own showing*, of the methods of the Ausdehnungslehre) that they are "*included* in that theory of SETS in algebra ... announced by me in 1835 ... of which SETS I have always considered the QUATERNIONS ... to be merely a *particular* CASE."

But all this has nothing to do with Quaternions, regarded as a calculus "*uniquely* adapted to Euclidian space." Grassmann lived to have his fling at them, but (so far as I know) he ventured on no claim to priority. Hamilton, on the other hand, even after reading the first *Ausdehnungslehre*, did claim priority and was never answered. He quoted, and commented upon, the very passage (of the *Preface* to that work) my allusion to which is censured by Prof. Gibbs. [*Lectures*, Pref. p. 62, footnote.] I still think, and it would seem that Hamilton also thought, that it was *solely because Grassmann had not realized the conception of the quaternion*, whether as $\beta\alpha$ or as $\beta\alpha^{-1}$, that he felt those difficulties (as to angles in space) which he says he had not had leisure to overcome. I have not seen the original work, but I have consulted what professes to be a *verbatim* reprint, produced under the author's supervision. [*Die Ausdehnungslehre von* 1844, *oder die lineale Ausdehnungslehre, &c. Zweite, im Text unverunderte Auflage*. Leipzig, 1878.] Prof. Gibbs' citations from my article give a very incomplete and one-sided representation of the few remarks I felt it necessary and sufficient to make about Grassmann. I need not quote them here, as anyone interested in the matter can readily consult the article.

In regard to Matrices, I do not think I have ever claimed anything for Hamilton beyond the *separable* ϕ, and the symbolic cubic (or biquadratic, as the case may be) with its linear factors and these I still assert to be exclusively his. My own work in this direction has been confined to Hamilton's ϕ, with its square root, its applications to stress and strain, &c.

As to the general history, of which (as I have said above) I claim no exact or extensive knowledge, Cayley and Sylvester will, no doubt, defend themselves if they see fit. It would be at once ridiculous and impertinent on my part were I to take up the cudgels in their behalf.