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*THE FUNDAMENTAL PRINCIPLES OF ALGEBRA.**

THIS section of the Association, over which I have the honor of being called upon to preside, may be said to be a double section, for it comprises both mathematics and astronomy; as a consequence, the addresses which have been delivered by my predecessors fall into two distinct groups, the mathematical and the astronomical. Of the former class I have had the pleasure of listening to three: Professor Gibbs on Multiple Algebra, Professor Hyde on the Development of Algebra, and Professor Beman on a Chapter in the History of Mathematics. Each of these addresses was devoted to one feature or other of the development of Algebra, and the subject which I have chosen for to-day is another aspect of the same wonderful phenomenon. It is a subject which interests alike the mathematician and the philosopher, and indeed all thinking men, for it concerns the foundations of that science which is generally acknowledged to be the most perfect creation of the human intellect.

I propose then to review historically and critically the several advances which have been made respecting the fundamental principles of algebra. Here I am mindful of the advice which Horace gives a young

* Address by the Vice-President and Chairman of Section A., Astronomy and Mathematics, of the American Association for the Advancement of Science, Columbus meeting, August, 1899.

poet, not to begin his epic at the origin of things, but to hasten on to the event proper; consequently, I shall not go back to the Egyptians, Greeks, Hindoos, or Arabs, but at once proceed to the advances made in the present century.

One of the first results of the differential notation of Leibnitz was the recognition of the analogy between $\frac{d}{dx}$ the symbol of differentiation and the ordinary symbol of algebra; later the same analogy was perceived to hold for Δ_x the symbol of the calculus of finite differences. Guided by this analogy, Lagrange and other mathematicians of the French school, which flourished at the beginning of the century, inferred that theorems proved to be true for combinations of ordinary symbols of quantity might be applied to the differential calculus and the calculus of finite differences. In this way many theorems were enunciated, which appeared to be true, but of which it was thought to be almost impossible to obtain direct demonstration. Gradually, however, the view was reached that the logical connection amounted to more than analogy, and that the common theorems were true because the symbols in the three cases were subject to the same fundamental laws of combination. This advance was principally made by Servais, who enunciated the laws of commutation and distribution.

About the year 1812 a school of mathematicians arose at Cambridge which aimed at introducing the d-ism of the Continent in place of the dot-age of the University; in other words they believed in the practical superiority of the differential notation of Leibnitz over the fluxional notation of Newton. Their attention was naturally drawn to the questions which had sprung from the differential notation; and of the three founders of the school—Babbage, Herschel, Peacock—the last named took up the problem of placing the teaching of algebra more in consonance with the views which had been reached of the nature of symbols. Peacock considered algebra, as then taught, to be more of an art than a science; a collection of rules rather than a system of logically connected principles; and with the object of placing it on a more scientific basis, he made a distinction between arithmetical algebra and symbolical algebra. He treated these names as denoting distinct sciences, and he wrote an algebra in two volumes, of which one treats of arithmetical algebra and the other of symbolical algebra. He thus describes what he means by the former term: "In arithmetical algebra we consider symbols as representing numbers and the operations to which they are submitted as included in the same definitions as in common arithmetic; the signs $+$ and $-$ denote the operations of addition and subtraction in their ordinary meaning only, and those operations are considered as impossible in all cases where the symbols subjected to them possess values which would render them so in case they were replaced by digital numbers; thus in expressions such as $a + b$ we must suppose a and b to be quantities of the same kind; in others like $a - b$, we must suppose a greater than b and therefore homogeneous with it; in products and quotients, like $a b$ and $\frac{a}{b}$ we must suppose the multiplier and divisor to be abstract numbers; all results whatsoever, including negative quantities, which are not strictly deducible as legitimate conclusions from the definitions of the several operations must be rejected as impossible, or as foreign to the science."

Here it may be observed that Peacock is not true to his own principle; for $\frac{a}{b}$ is as impossible when b is not a divisor of a, as is $a - b$, when b is not less than a; in neither case do we get a digital number. He draws the line so as to exclude the fraction as a multiplier but not as a multiplicand; according to his own principle it should be wholly excluded from arithmetical algebra. But arithmetic so restricted would be a very narrow science, and the logical result would be to divide arithmetic itself into an arithmetical arithmetic and a symbolical arithmetic.

He then describes what he means by 'symbolical algebra.' "Symbolical algebra adopts the rules of arithmetical algebra but removes altogether their restrictions; thus symbolical subtraction differs from the same operation in arithmetical algebra in being possible for all relations of value of the symbols or expressions employed. All the results of arithmetical algebra which are deduced by the application of its rules, and which are general in form, though particular in value, are results likewise of symbolical algebra, where they are general in value as well as in form; thus the product of a^m and a^n which is a^{m+n} when m and n are whole numbers, and therefore, general in form, though particular in value, will be their product likewise when m and n are general in value as well as in form; the series for $(a+b)^n$ determined by the principles of arithmetical algebra when n is any whole number, if it be exhibited in a general form, without reference to a final term, may be shown upon the same principle to be the equivalent series for $(a+b)^n$ when n is general both in form and value."

The principle here brought forward was named by Peacock the 'principle of the permanence of equivalent forms'; by means of it the transition is made from arithmetical algebra to symbolical, and at page 59 of 'Symbolical Algebra' it is thus enunciated: "Whatever algebraical forms are equivalent, when the symbols are general in form but specific in value, will be equivalent likewise when the symbols are general in value as well as in form."

One asks naturally, 'What are the limits set to the generality of the symbol?' Peacock's answer is, 'Whatsoever.' In the theory of reasoning the great question is not, 'How do we pass from generals to particulars?' but 'How do we pass from particulars to generals?' The application of general principles is plain enough—the difficulty is in explaining how we arrive at the truth of the general principles. The logician, seeking for light on this question, is apt to turn to exact science, and especially to algebra, the most perfect branch of exact science. Should he turn to Peacock, he finds that all that is offered him is this 'principle of the permanence of equivalent forms'; which, paraphrased, amounts to the following: We find certain theorems to be true when the symbol denotes integer number; let these theorems be true without restriction, and let us try to find the different interpretations which may be put on the symbol. Is not the following attitude more logical? We find certain theorems to be true, when the symbol denotes number; how far and no further may the conception of number be generalized, yet these theorems remain true without any alteration of form?; and, should the conception of number be still further generalized, what is the modified form which the theorems then assume? This is the logical process of generalization, whereas Peacock's process is "essentially arbitrary, though restricted with a specific view to its operations and their results admitting of such interpretations, as may make its applications most generally useful." (Report on Recent Progress in Analysis, p. 194.)

The two processes may be illustrated by their application to the binomial theorem, proved to be true for a positive integer index. According to Peacock's process,

$$(a+b)^n = a^n + na^{n-1}b + \frac{n(n-1)}{1.2}a^{n-2}b^2 +$$

is to be made a theorem in symbolical algebra, whether the series be finite or infinite, and all that remains is to find the different ways in which it may be interpreted. The process of generalization proceeds by steps. For instance, it asks: Will the series retain the same form when n is generalized so as to include any rational fraction? This is one of the questions which Newton proposed to himself, and settled in the affirmative; and it is recorded that he verified the truth of his conclusion by squaring the series for $(1-x^2)^{\frac{1}{2}}$. Peacock's principle does not distinguish divergent from convergent series; it is nothing but hypothesis, and any result suggested by it must stand the test of independent investigation.

An important advance in the philosophy of the fundamental principles of algebra was made by D. F. Gregory, a younger member of the Cambridge school of mathematicians. Descended from a Scottish family, already famous in the annals of science, he early gave promise of adding additional luster to the name; this he accomplished in a brief life of thirty-one years. In 1838 he read a paper before the Royal Society of Edinburgh 'On the Real Nature of Symbolical Algebra,' in which he says: "The light in which I would consider symbolical algebra is, that it is the science which treats of the combination of operations defined not by their nature, that is, by what they are or what they do, but by the laws of combination to which they are subject. And as many different kinds of operations may be included in a class defined in the manner I have mentioned, whatever can be proved of the class generally, is necessarily true of all the operations included under it. This, it may be remarked, does not arise from any analogy existing in the nature of the operations which may be totally dissimilar, but merely from the fact that they are all subject to the same laws of combination. It is true that these laws have been in many cases suggested (as Mr. Peacock has aptly termed it) by the laws of the known operations of number; but the step which is taken from arithmetical to symbolical algebra is, that leaving out of view the nature of the operations which the symbols we use represent, we suppose the existence of classes of unknown operations subject to the same laws. We are thus able to prove certain relations between the different classes of operations, which, when expressed between the symbols are called algebraical theorems. And if we can show that any operations in any science are subject to the same laws of combination as these classes, the theorems are true of these as included in the general case; provided always that the resulting combinations are all possible in the particular operation under consideration."

It will be observed that he places algebra on a formal basis; for its symbols are defined, not to represent real operations, but by laws of combination arbitrarily chosen. In a subsequent paper, however, entitled 'On a Difficulty in the Theory of Algebra,' he practically gave up the formal view, and appears inclined to adopt the realist view instead. He says: "In previous papers on the theory of algebra I have maintained the doctrine that a symbol is defined algebraically when its laws of combination are given; and that a symbol represents a given operation when the laws of combination of the latter are the same as those of the former. This, or a similar theory of the nature of algebra seems to be generally entertained by those who have turned their attention to the subject; but without in any degree leaning on it, we may say that symbols are actually subject to certain laws of combination, though we do not suppose them to be so defined; and that a symbol representing any operation must be subject to the same laws of combination as the operation it represents." This is a departure from conventional definitions to rules founded upon the universal properties of that which is represented.

In the paper first quoted, Gregory considers five classes of operations. He supposes + and — to be defined by the rules of signs; and he finds in arithmetic a pair of operations which come under it, namely, addition and subtraction; and in geometry another pair, namely, turning through a circumference, and a semi-circumference respectively. But it is instructive to note that the difficulty referred to in the title of the later paper is none other than the view that + and — represent the operations of addition and subtraction; and he there shows that addition (including subtraction) is subject to a couple of very different laws, the commutative and the associative, though he does not use the latter term. It may be observed that the rule of signs applies to × and ÷ also; hence if + and — embraced addition and subtraction, so would × and ÷. The truth of the matter is that in ascending from arithmetic to algebra, we replace the coördinate ideas of *addition* and *subtraction* by the more general idea of *sum* and the subordinate functional idea of *opposite*. Similarly the coördinate ideas of multiplication and division are replaced by the more general idea of a *product* and the subordinate functional idea of *reciprocal*. The symbols — and ÷ then denote opposite and reciprocal respectively, while the ideas of sum and product are not expressed by symbols, but are sufficiently indicated by the manner of writing of the several elements. This difficulty appears to have upset his belief in the existence of classes of operations subject to the same laws of combination, yet totally dissimilar in nature, and without any real analogy binding them together.

According to Gregory, the second class of operations are index operations, subject to the two laws:

$$f_m(a)f_n(a) = f_{m+n}(a) \quad \text{and} \quad f_m f_n(a) = f_{mn}(a).$$

The third class comprises the ordinary symbol of algebra, and the symbols d and Δ of the calculus; they are subject to the distributive law

$$f(a) + f(b) = f(a + b),$$

and to the commutative law

$$f_1 f(a) = f f_1(a).$$

The fourth class comprises the logarithmic operations subject to the law

$$f(a) + f(b) = f(ab).$$

The fifth class are the sine and cosine functions, subject to the laws expressed by the fundamental theorem of plane trigonometry, namely, the connection between the sine and cosine of the sum of two angles and the sines and cosines of the component angles.

Following as far as may be the chronological order, we come next to Augustus De Morgan, distinguished for his contributions alike to logic and to mathematics. In his 'Formal Logic' he takes a formal view of the nature of reasoning in general, and in his 'Trigonometry and Double Algebra' he lays down an excessively formal foundation for algebra. Indeed, it may be said that he carries formalism to its logical issue; and, thereby, he renders a service, for its inadequacy then becomes the better evident. In the chapter of the book mentioned, which is headed, 'On Symbolic Algebra,' he thus expresses the view he had arrived at: "In abandoning the meanings of symbols, we also abandon those of the words which describe them. Thus addition is to be, for the present, a sound void of sense. It is a mode of combination represented by +; when + receives its meaning, so also will the word addition. It is most important that the student should bear in mind that, *with one exception*, no word nor sign of arithmetic or algebra has one atom of meaning throughout this chapter, the object of which is symbols, and their laws of combination, giving a symbolic algebra which may hereafter become the grammar of a hundred distinct significant algebras. If any one were to assert that + and − might mean reward and punishment and A, B, C, etc., might stand for virtues and vices, the reader might believe him, or contradict him, as he pleases, but not out of this chapter. The one exception above noted, which has some share of meaning, is the sign = placed between two symbols, as in $A = B$. It indicates that the two symbols have the same resulting meaning, by whatever steps attained. That A and B, if quantities, are the same amount of quantity, that if operations, they are of the same effect, etc." Let us apply to the theory quoted the logical maxim that the exception proves the rule, *prove* being used in the old sense of test. Well then, I say, because one symbol at least is found to be refractory to the theory, it follows that the theory is fallacious.

De Morgan proceeds to give an inventory of the fundamental symbols and laws of algebra, that for the symbols being 0, 1, +, −, ×, ÷, $(\ )^{(\ )}$ and letters. With respect to it the following questions may be asked: Why should $(\ )^{(\ )}$ be included, while the inverse idea, denoted by *log* is left out? What of the functional symbols *sin* and *cos*? Can they be derived from the above? As − denotes opposite and ÷ reciprocal, what are the signs for sum and product? Can they be derived from the above?

His inventory of the fundamental laws is expressed under fourteen heads, but some of them are merely definitions. The laws proper may be reduced to the following, which he admits are not all independent of one another:

I. Law of signs: $++=+$, $+-$ or $-+=-$, $--=+$, $\times\times=\times$, $\times\div$ or $\div\times=\div$, $\div\div=\times$.

II. Commutative law: $a+b=b+a$, $ab=ba$.

III. Distributive law: $a(b+c)=ab+ac$.

IV. Index laws: $a^b\times a^c=a^{b+c}$, $(a^b)^c=a^{bc}$, $(ab)^c=a^cb^c$.

V. $a-a=0$, $a\div a=1$.

These last may be called the rules of reduction. What Gregory gave was a classification of the more important operations occurring in algebra; De Morgan professes to give a complete inventory of the laws which the symbols of algebra must obey, for he says "Any system of symbols which obeys these rules and no others, except they be formed by combination of these rules, and which uses the preceding symbols and no others, except they be new symbols invented in abbreviation of combinations of these symbols, is symbolic algebra."

Compare this inventory with Gregory's classification. De Morgan brings × and ÷ under the same rule as + and −; he applies the commutative law to a sum as well as to a product; he introduces the third index law, which makes the index distributive over the factors of the base; he leaves out the logarithmic and trigonometrical principles and introduces what may be called the rules of reduction. From his point of view, none of them are rules; they are laws, that is, arbitrarily chosen relations to which the algebraic symbols must be subject. He does not mention the law pointed out by Gregory, afterwards called the law of association. It is an unfortunate thing for the formalist that a^b is not equal to b^a, for then his commutative law would have full scope; as it is, the index operations prove exceedingly refractory, so that in some of the beautifully formal systems they are left out of account altogether. Here already we have sufficient indication that to give an inventory of the laws which the symbols of algebra *must* obey, is as ambiguous a task as to give an inventory of the *a priori* furniture of the mind.

Like De Morgan, George Boole was a mathematician who investigated and wrote in the field of logic. The character of the work done by the two men is very different. De Morgan's work bristles with new symbols; Boole uses only the familiar symbols of analysis. The former polished many small stones; the latter raised an edifice of grand proportions. The work done by Boole in applying mathematical analysis to logic necessarily led him to consider the general question of how reasoning is accomplished by means of symbols. The view which he adopted on this point is stated on page 68 of the 'Laws of Thought.'

"The conditions of valid reasoning by the aid of symbols are: *First*, that a fixed interpretation be assigned to the symbols employed in the expression of the data, and that the laws of the combination of these symbols be correctly determined from that interpretation; *Second*, that the formal processes of solution or demonstration be conducted throughout in obedience to all the laws determined as above, without regard to the question of the interpretability of the particular results obtained; *Third*, that the final result be interpretable in form, and that it be actually interpreted in accordance with that system of interpretation which has been employed in the expression of the data."

As regards these conditions it may be observed that they incline toward the realist view of analysis. True, he speaks of interpretation instead of meaning, but it is a fixed interpretation; and the rules for the processes of solution are not to be chosen arbitrarily, but are to be found out from the particular system of interpretation of the symbols. The thoroughgoing realist view is that a symbol stands for some definite notion in the subject analyzed, and that the rules of the analysis are founded upon universal properties of the subject analyzed. The realist view of mathematical science has commended itself to me ever since I made an exact analysis of relationship and devised a calculus which provides a notation for any relationship; can express in the form of an equation the relationship existing between any two persons, and provides rules by means of which a single equation may be transformed, or a number of equations combined so as to yield any relationship involved in their being true simultaneously. The notation is made to fit the subject, and the rules for manipulation are derived from universal physiological laws and the more arbitrary laws of marriage. The basis is real; yet the analysis has all the characteristics of a calculus, and throws light by comparison on several points in ordinary algebra. Its fundamental symbol expresses a relation; and what is the ultimate meaning of the algebraical symbol or of the symbol of the calculus but an operation or relation?

It is Boole's second condition which principally calls for study and examination; respecting it he observes as follows: "The principle in question may be considered as resting upon a general law of the mind, the knowledge of which is not given to us *a priori, i. e.*, antecedently to experience, but is derived, like the knowledge of the other laws of the mind, from the clear manifestation of the general principle in the particular instance. A single example of reasoning, in which symbols are employed in obedience to laws founded upon their interpretation, but without any sustained reference to that interpretation, the chain of demonstration conducting us through intermediate steps which are not interpretable to a final result which is interpretable, seems not only to establish the validity of the particular application, but to make known to us the general law manifested therein. No accumulation of instances can properly add weight to such evidence. It may furnish us with clearer conceptions of that common element of truth upon which the application of the principle depends, and so prepare the way for its reception. It may, where the immediate force of the evidence is not felt, serve as verification, *a posteriori*, of the practical validity of the principle in question. But this does not affect the position affirmed, viz., that the general principle must be seen in the particular instance—seen to be general in application as well as true in the special example. The employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry furnishes an illustration of what has been said. I apprehend that there is no mode of explaining that application which does not covertly assume the very principle in question. But that principle, though not, as I conceive, warranted by formal reasoning based upon other grounds, seems to deserve a place among those axiomatic truths which constitute in some sense the foundation of general knowledge, and which may properly be regarded as expressions of the mind's own laws and constitution" (p. 68).

We are all familiar with the fact that algebraic reasoning may be conducted through intermediate equations without requiring a sustained reference to the meaning of these equations; but it is paradoxical to say that these equations can, in any case, have no meaning, no sense, no interpretation. It may not be necessary to consider their meaning; it may even be difficult to find their meaning, but that they have a meaning is a dictate of common sense. It is entirely paradoxical to say that, as a general process we can start from equations having a meaning and arrive at equations having a meaning by passing through equations which have no meaning. The particular instance in which Boole sees the truth of the paradoxical principle is the successful employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry. As soon, then, as the $\sqrt{-1}$ occurring in these processes is demonstrated, the evidence for the principle fails. As a matter of fact, the doctrine of algebraists about $\sqrt{-1}$ has long been a dark corner in exact science; and as a consequence it has been made the foundation for all sorts of crank theories. Recently I noticed that an ingenious individual had applied the $\sqrt{-1}$ and its successive powers to construct a mathematical theory of sensation. Before the introduction by Descartes of the geometrical idea of the opposite the use of $-$ in algebra might have been made the foundation for a similar transcendental theory of reasoning. Algebra, as the analysis of quantity in space, has a clear meaning for $\sqrt{-1}$ as the operation of turning through a right angle round a definite or an indefinite axis; in the former case it is vector in nature, because the axis must be specified; in the latter it is scalar in nature, because the axis may be any suitable one. It follows that $-$ denotes turning through two right angles, and this includes 'opposite' as a particular case. Thus an instance is still wanting on which to build the transcendental theory of reasoning enunciated by Boole.

The object of Boole's work, 'The Laws of Thought,'' is to investigate the fundamental laws of thought, to give expression to them in the symbolical language of a calculus, and upon that foundation to establish the science of logic. In the concluding chapter he considers the light which the inquiry throws on the nature and constitution of the human mind. Now, as a matter of fact, the subject analyzed is quality, and its connection with the nature and constitution of the human mind is nowise more inanimate than is the connection of algebra the science of quantity.

It is interesting to compare Boole's inventory of the symbols and laws for a calculus of reasoning (analysis of quality) with the inventory made by De Morgan for the symbols and laws of algebra (the analysis of quantity). The symbols are the same, excepting that ( )^{0} is omitted. The law of signs for + and − is the same, but none is given for × and ÷ on account of the ambiguity of the reciprocal; the commutative law applies to both sum and product; the distributive law applies to the product of sums; there are no index laws, excepting the peculiar one $a^2 = a$. The law of reduction $a - a = 0$ remains, but the complementary law $\frac{a}{a} = 1$ is not true in general.

How is the truth or suitability of these laws established? He says that it would be mere hypothesis to borrow the notation of the analysis of quantity, and to assume that in its new application the laws by which its use is governed would remain unchanged; to establish them he investigates the operations of the mind in reasoning as expressed by language, and applies Kant's theory of seeing the general truth in a particular instance. As regards the commutative law it may be remarked that Boole overlooks the fact that two notions may in their definition be coördinate with one another, or subordinate the one to the other, just as in the theory of probability there is a difference between two events which are independent of one another, and two events which are dependent the one on the other; and in the latter case it is not true that the order of the notions is indifferent. This is not the place to enter into a discussion of these so-called laws of thought; I wish merely to point out that Boole's view is essentially that of the realist; the fundamental rules of an analysis are not to be assumed arbitrarily, but must be found out by investigation of the subject analyzed.

Contemporaneously with Boole, and living on the same Emerald Isle, another mathematician spent many days reflecting on the fundamental principles of algebra—Sir W. R. Hamilton. His investigations started from the reading of some passages in Kant's 'Critique of the Pure Reason' which appeared to justify the expectation that it should be possible to construct *a priori* a science of time as well as a science of space. The principal passage is as follows: "Time and space are two sources of knowledge from which various *a priori* synthetical cognitions can be derived. Of this pure mathematics gives a splendid example in the case of our cognitions of space and its various relations. As they are both pure forms of sensuous intuition, they render synthetical propositions *a priori* possible." Thus, according to Kant, space and time are forms of the intellect; and Hamilton reasoned that, as geometry is the science of the former, so algebra must be the science of the latter. He amplifies that view as follows: "It early appeared to me that these ends might de attained by consenting to regard algebra as being no mere art, nor language, nor primarily a science of quantity, but rather as the science of order in progression. It was, however, a part of this conception that the progression here spoken of was understood to be continuous and unidimensional, extending indefinitely forward and backward, but not in any lateral direction. And although the successive states of such a progression might, no doubt, be represented by points upon a line, yet I thought that their simple successiveness was better conceived by comparing them with moments of time, divested, however, of all reference to cause and effect; so that the 'time' here considered might be said to be abstract, ideal or pure, like that 'space' which is the object of geometry. In this manner I was led to regard algebra as the science of pure time, and an essay containing my views respecting it as such was published in 1835." (Preface to 'Lectures on Quaternions,' p. 2.) If algebra is based on any unidimensional subject a difficulty arises in explaining the roots of a quadratic equation when they are imaginary. To get over the difficulty

Hamilton invented a theory of algebraic couplets, but the success of the invention is doubtful. In his presidential address before the British Association the late Professor Cayley said that he could not appreciate the manner in which Hamilton connected algebra with the notion of time, and still less could he appreciate the manner in which he connected his algebraical couplet with the notion of time. Whether Hamilton has effected the explanation or not, it appears to be logically possible, for a complex quantity can be represented by two segments of one and the same straight line.

But, be that as it may, Hamilton was led from algebraic couplets to algebraic triplets and to the problem of adapting triplets to the representation of lines in space. His guiding idea was to extend to space the mode of multiplication of lines in a plane already discovered by Argand, Warren and others; and it was here that he stepped from the time basis to the space basis—that is, passed from a unidimensional to a tridimensional subject, the latter including the former as a special case. To his surprise, he found that the multiplication of two lines in space, either one being expressed in terms of three elements, led to a product composed not of three, but of four elements; and this result he deemed so novel and characteristic that he selected it to give a name to the new method—'Quaternions.' As finally developed, the method rests on a geometrical basis; nevertheless it is the logical generalization of ordinary algebra, for the distinctive theorems of algebra, such as the exponential, binomial and multinomial theorems, have their generalized counterparts in quaternions. Since the time of Gauss, mathematicians have considered double or plane algebra to be the logical generalization of ordinary algebra; now quaternions bears to plane algebra the same logical relation which plane algebra bears to ordinary algebra. It is all algebra in the sense of being the analysis of quantity and the relations of quantities. Any one who admits De Moivre's theorem into algebra is logically bound to admit quaternions as the highest form of algebra. It is a common belief that quaternions has only a remote connection with algebra; that it is only one of several systems of noncommutative algebra, and that the mathematician can get on very well without it. But if the above is the true logical relation, then it must be the duty of every analyst to master its principles. It may be remarked here that the logical relation of quaternions to plane algebra is obscured by the prevalent but erroneous idea that the complex quantities of the form $x + iy$ represent vectors. They really represent, in their planar meaning, coaxial quaternions; that is, x is a scalar and the axis of y is the common perpendicular to the plane. Let, as usual, $w + ix + jy + kz$ denote a quaternion; the complex quantity is identical not with $w + ix$ or $ix + jy$, but with $w + kz$. The fallacy in question almost baffled Hamilton in his attempts at generalization, as may be seen from the account which he gives of the discovery in the *Philosophical Magazine* for 1844.

We shall obtain additional insight into the nature of the fundamental laws of algebra by considering the part which they played in the discovery of the quaternion generalization. In the endeavor to adapt the general conception of a triplet to the multiplication of lines in space Hamilton started out with the principles of commutation, distribution and reduction; but in order that the theorem about the moduli might remain true he soon felt obliged, not indeed to abandon the principle of commutation entirely, but to modify it so as to preserve the order of the factors while leaving the order of combination of the factors commutable. This principle, which had previously been pointed out by Gregory as an independent principle, he called the law of association. As the principle of commutation was still assumed to apply to the terms of a sum, it followed that the principle of association also applied to them. Here, then, we have an important difference in the inventory of the laws of algebra. According to De Morgan algebra follows all the laws which he enumerated, and them only; but Hamilton showed that the legitimate extension of algebra to space requires the commutative law to be modified in the case of a product. And still further light is obtained on the nature of these laws by considering the way by which Hamilton satisfied himself of the truth of the principle of association. He sought for and obtained a geometrical proof, independent of the principle of distribution, and depending on theorems taken from spherical trigonometry or spherical conics. Thus a notable generalization of algebra was made, not by arbitrary choice of fundamental rules, nor by arbitrary extension of the rules for integer number, but by finding out the universal properties of the subject analyzed.

We have already found that the index operations form a valuable test of the soundness of any theory of algebra. If the method of quaternions is the true extension of algebra to space we expect it to throw new light on these operations. As a matter of fact, most of the works on quaternions ignore the subject or present instead the treatment for the plane. In Hamilton's 'Elements of Quaternions' there is a chapter headed 'On Powers and Logarithms of Diplanar Quaternions,' but what it contains is practically limited to the plane. Why? Because the author believed, and there states, that the fundamental exponential law is not true for diplanar quaternions; that is, for space

$$e^p \times e^q \text{ not } = e^{p+q}.$$

The source of error lies in regarding the sum of indices as commutative, for that amounts to holding that $e^p \times e^q = e^q \times e^p$, which is contrary to the principles of quaternions. Were $p + q$ a sum without any real order of the terms, then we might have an order of factors, that is, we might have

$$(p + q)(p + q) = p^2 + pq + qp + q^2 = p^2 + q^2 + 2Spq.$$

But when the sum has a real order of p, prior to q, then we cannot at the same time, hold that one factor $p + q$ can be prior to another factor $p + q$; for in the expansion we should have the contradiction of p being prior to q and q at the same time prior to p. Hence when p is prior to q the second power is not formed in accordance with the distributive principle; it is $p^2 + 2pq + q^2$. When this is admitted the exponential principle stands, but the commutative principle for a sum of such indices goes, as does also the distributive manner of forming the power of such a sum.

As regards the third index law it is evident from the non-commutability of the factors in general that in space it ceases to be true. The rule of reduction for a sum of terms requires to be modified when the terms have a real order; for $p + q - q = p$, but $q + p - q$ is not equal to p. The term and its opposite must follow one another immediately in order that the reduction may be legitimate. Similarly, in the case of a product the factor and its reciprocal must follow one another immediately in order that the reduction may be legitimate. From these principles the generalization for space of all the fundamental theorems of algebra follows without difficulty, and the theory of logarithms and exponents becomes the most fruitful part of quaternion analysis.

We may now consider briefly how the advance made by Hamilton struck a contemporary mathematician—Professor Kelland, of the University of Edinburgh. It was his custom to teach the elements of quaternions to the students of his senior class, and I remember how all went well till he came to multiplication, where the part played by a vector as a multiplier was likened, in some mysterious manner, to the action of a corkscrew. In the introductory chapter of the 'Introduction to Quaternions' he remaks as follows on the process by which algebra is generalized: "It is only by standing loose for a time to logical accuracy that extensions in the abstract sciences—extensions at any rate which stretch from one science to another—are effected." And further on: "We trust, then, it begins to be seen that sciences are extended by the removal of barriers, of limitations, of conditions on which sometimes their very existence appears to depend. Fractional arithmetic was an impossibility so long as multiplication was regarded as abbreviated addition; the moment an extended idea was entertained, ever so illogically, that moment fractional arithmetic started into existence. Algebra, except as mere symbolized arithmetic, was an impossibility so long as the thought of subtraction was chained to the requirement of something adequate to subtract from. The moment Diophantus gave it a separate existence—boldly and logically as it happened—by exhibiting the law of *minus* in the forefront as the primary definition of his science, that moment algebra in its highest form became a possibility, and indeed the foundation stone was no sooner laid than a goodly building arose on it."

It seems to me that no greater paradox could be enunciated than to say that higher principles in exact science are reached by standing loose for a time to logical accuracy. How long a time does that which is illogical take to become logical? The true process is generalization, not illogical extension. No doubt, the generalized principle may at first be merely an hypothesis, and in that form it may be applied so that it may be verified by its results; but this is not standing loose to logical accuracy.

The same author gives the following account of how Hamilton *extended* algebra to space: "He had done a considerable amount of good work, obstructed as he was, when, about the year 1843, he perceived clearly the obstruction to his progress in the shape of an old law which, prior to that time, had appeared like a law of common sense. The law in question is known as the commutative law of multiplication. Presented in its simplest form it is nothing more than this: 'five times three is the same as three times five'; more generally it appears under the form of $ab = ba$ whatever a and b may represent. When it came distinctly to the mind of Hamilton that this law is not a necessity with the extended signification of multiplication he saw his way clear and gave up the law. The barrier being removed, he entered on the new science as a warrior enters a besieged city through a practicable breech." This account is, of course, inadequate, for Grassmann jumped over the same barrier in the shape of an 'old law,' yet he was unable to deal with angles in space. There is no occasion to speak disrespectfully of the law of commutation; it has its own place; Hamilton did not cast it aside as an obstruction; he modified it for a product of factors having a real order, and the modified form amounts to the law of association.

We shall now go back to another independent source of the development of the principles of algebra—Hermann Grassmann. Like his contemporary, Hamilton, he was remarkable alike for attainments in mathematics and philosophy, and, besides, he made important contributions to philology. No doubt specialists are necessary, but the investigation of the fundamental principles of a science requires one who is more than a specialist, one who has not only studied a portion minutely, but has also taken a comprehensive glance over the whole. From the preface to the *Ausdehnungslehre* of 1844 we get an insight into the origin and development of his course of investigation, and we find that it was in a manner the reverse of Hamilton's. The former started from a variety of geometrical facts and developed a method which is independent of space, and has perhaps suffered from its *philosophische Allgemeinheit*; the latter started from general philosophical ideas and developed an algebra which is uniquely adapted to space of three dimensions. But, as their subjects were largely the same, their results, so far as they involve truth, must also be capable of unification to a large extent.

In the preface quoted, Grassmann informs us that he started from the treatment of negatives in geometry; he observed that the straight lines AB and BA were opposite, and that AB + BC = AC, whether the point C is beyond B or between A and B. This led him to the principle of geometrical addition—namely, that AB + BC = AC, whether A, B, C are in one straight line or not. It may be remarked here that this principle is all right so long as the components have no real order, such as forces applied at a point or the coördinates of a point; but that it does not apply where the components have a real order, as, for example, the sides of a polygon. In successive addition the straight line from the origin to the end of the polygon is the scalar result, but the area enclosed is another result, which depends on the form of the path.

Then turning to the product in geometry, he adopted the view that the parallelogram is the product of its two sides, whether these are at right angles or not. He next found that the geometrical ideas of a sum and a product which he had adopted satisfied the principle of distribution, but not the principle of commutation so far as the factors of a product were concerned. In the case of the products commutation could be made, provided the sign of the product were changed also—that is, they were subject to negative commutation. Another set of basal facts was taken from the doctrine of the center of gravity. He observed that the center of gravity may be considered as the sum of several points, the line joining two points as the product of the points, the triangle as the product of its three points, and the pyramid as the product of its four points; and from these facts he developed a method similar to the 'Barycentric Calculus,' of Möbius.

He also considered the geometrical meaning of the exponential function. He observed that if a denote a finite straight line and α an angle in a plane through the line, then ae^{α} denotes the line a turned through the angle α. The treatment of angles in one plane is easy, but on attempting to treat of angles in space he encountered difficulties which he was unable to surmount. This fact has been cited as indicating the superiority of Hamilton's method; while that is true, it must not be forgotten that Hamilton failed to generalize the exponential theorem.

What is the view which Grassmann takes of the fundamental principles of algebra? An answer to this question is found in the introduction to the *Ausdehnungslehre* of 1844. He divides the sciences into the real and the formal; the former treat of reality, and their truth consists in the agreement of thought with reality; the latter treat of thought only, and their truth consists in the agreement of the processes of thought with one another. Pure mathematics is the doctrine of forms. As a consequence he is obliged to place geometry under applied mathematics, for it has a real subject, and should anyone think otherwise he must deduce from pure thought the tridimensional character of space. Were space a form of thought, so would be time and motion, and kinematics would also be a part of pure mathematics. So he relegates geometry to the real sciences, and he has a difficulty in retaining arithmetic even, for is it not based on axioms, whereas a formal science is based on conventions?

From the notion of the combination of terms he deduces that the placing of the brackets and the order of the terms may or may not be indifferent. There is a synthetic combination and an analytic combination; when the latter is unambiguous (that is, $a - a = 0$) then the placing of the brackets and the order of the terms is indifferent; synthetic combination is then called addition, and the analytic subtraction. Thus in Grassmann's view the commutative and associative laws are involved in the ideas of addition and subtraction. It may be observed that the old difficulty with subtraction is due to the fact that it is not thoroughly commutative, and that it is only to the generalized idea of composition that the commutative law applies. Besides, to define addition so as to exclude terms having a real order is an arbitrary restriction of algebra.

According to Grassmann's view multiplication is a combination of a higher order; that is, he assumes as the definition of multiplication the distributive principle in the two-fold form

$$(a + b)\,c = ac + bc \text{ and } c\,(a + b) = ca + cb.$$

It may be observed, however, that the true expression for the distributive principle is

$$(a + b)\ (c + d) = ac + ad + bc + bd,$$

which assumes that if there is any real order of the terms there can be only one real order $a\ b\ c\ d$.

As regards the laws of indices he says that involution is a combination of the the third order, and that for the sake of shortness he will omit all consideration of it. Besides, its formal definition would be of no use, for in the nature of things it can be applied only in the special sciences through real definitions. This failure to treat of the index laws tells against his whole theory of the nature of algebra. In fact, these laws are the touchstone whereby the soundness of any theory of the foundations of algebra may be tested.

In 1867 Hermann Hankel published his 'Theory of Complex Numbers.' The full title of the work is '*Theorie der complexen Zahlensysteme insbesondere der gemeinen imaginären Zahlen und der Hamilton'schen Quaternionen nebst ihrer geometrischen Darstellung.*' He had studied the writings of both Hamilton and Grassmann, and the aim of the book is to give a complete theory of the several systems, uniting them all under the notion of complex number. From the title we gather that he considered the algebraic imaginaries and the Hamiltonian quaternions as two distinct systems, formal in their nature, but having a representation in space. He begins with positive integer numbers, and finds from a consideration of the notion that the addition of such numbers satisfies the two laws of association and commutation, which he treats as independent of one another. But as regards the notion of the multiplication of such numbers he says that the truth of the commutative law or of the associative law is not self-evident; that the former law can be proved by a geometric construction in a plane, and the latter by a geometric construction in space. As regards the distributive law he says merely that it is a universal property of multiplication. As regards the base and index relation he says that neither the commutative law nor the associative law applies; he enunciates the same three index laws as De Morgan, but does not say whether they are self-evident or require a proof by geometric construction. Here, then, in a professedly scientific work, some of these fundamental laws are treated as self-evident, others as requiring geometric proof, and others yet are merely enunciated. If in the case of multiplication the commutative law requires proof, so does it also in the case of addition, for it is just as self-evident that $2 \times 3 = 3 \times 2$ as that $2 + 3 = 3 + 2$.

The manner in which Hankel passes from arithmetic and arithmetical algebra to general algebra is as follows: Algebra, being formal mathematics, can be founded on any system of independent rules; but, in order that its results may be interpretable and that it may be capable of application, it is found convenient to choose the system of fundamental rules satisfied by common arithmetic; in other words, the laws of integer arithmetic are made the laws of algebra. This he calls the 'principle of the permanence of the formal laws,' and enunciates as follows (p. 11): "If two expressions stated in terms of the general symbols of arithmetical algebra (*arithmetica universalis*) are equal to one another they shall remain equal to one another when the symbols cease to denote simple magnitudes and the operations receive any other meaning." Peacock speaks of the permanence of equivalent forms; Hankel of the permanence of the formal laws. Peacock says, "Let any general equivalence in arithmetical algebra be true also in universal algebra"; Hankel says, "Let the fundamental laws of the former be made the fundamental laws of the latter." Hankel gives a more scientific form to what was meant by Peacock.

However, Hankel labors under a logical difficulty from which Peacock was exempt, for he does not take the laws of arithmetical algebra without exception; he rejects the commutative law for a product, in order that quaternions may be included among his complex numbers. But, it may be asked, why not reject the commutative law for addition also? So far as arithmetical algebra is concerned they stand on the same basis. If, as has been shown, the sum of quaternion indices is not commutative we are logically bound, on his principles, to reject the commutative rule for addition also. We are reduced to the alternative: the choice of the fundamental rules is arbitrary, or else they must be founded on the properties of the subject analyzed. The permanence of the formal laws is nothing but hypothesis, and in the case of any generalization must be tested by real investigation.

One of the clearest thinkers on mathematical subjects in recent times was Professor Clifford, who like several of the mathematical philosophers we have spoken of, was cut down in the midst of his scientific activity. In his posthumous work entitled 'The Common Sense of the Exact Sciences' there are chapters on number and quantity in which he explains his views of the fundamental principles of algebra. He starts out from the principle, which he attributes to Cayley and Sylvester, that the number of any set of things is the same in whatever order we count them, and deduces from it, by means of diagrams, the commutative and associative rules for positive integer number. He says that they amount to the following: "If we can interchange any two consecutive things without altering the result then we may make any change whatever in the order without altering the result." It may be remarked that this shows that the commutative and associative properties are not independent, but that the former involves the latter. He next shows, by a diagram, that the distributive rule is true for the two forms $a(b + c) = ab + ac$ and $(b + c)a = ba + ca$, but he does not consider the complete form of the rule $(a + b)(c + d) = ac + ad + bc + bd$.

As regards the impossible subtraction and division he says (p. 33): "Every operation in mathematics that we can invent amounts to asking a question, and this question may or may not have an answer according to circumstances. If we write down the symbols for the answer to the question in any of those cases where there is no answer, and then speak of them as if they meant something, we shall talk nonsense. But this nonsense is not to be thrown away as useless rubbish. We have learned by very long and varied experience that nothing is more valuable than the nonsense which we get in this way; only it is to be recognized as nonsense, and by means of that recognition made into sense. We turn the nonsense into sense by giving a new meaning to the words or symbols which shall enable the question to have an answer that previously had no answer."

This is the true phenomenon in algebra; it is more logical than its framer. How can it be possible, unless the algebraist founds his analysis upon real relations? It is the logic of real relations which may outrun the imperfect definitions and principles of the analyst and make it necessary for him to return to revise them.

To get over the impossible subtraction he introduces instead of the discrete unit supposed by number, the idea of a step, making plus mean 'forwards' and minus 'backward.' The summing of steps is independent of the order in which they are taken, and a minus step is just as independent as a plus step. When these symbols occur in multipliers he gives them, not the meaning of 'forwards' and 'backwards,' but that of 'keep' and 'reverse.' He gives them these meanings in addition to their former meanings, and leaves it to the context to show which is the right meaning in any particular case. It may be remarked that it is doubtful whether in any case two distinct meanings can be given to a symbol at one and the same time without producing confusion. It seems to me, as already stated, that the most general meanings of + and − are the angular ideas of an even and an odd number of semi-circumferences, but this reduces in certain cases to the linear ideas of direct and opposite.

From the idea of step he passes to the idea of operation, on the theory that a product may be composed either of a step and an operation or of two operations. As a matter of fact, an operation is merely a relationship which may subsist between two quantities; and we may have two distinct products, one expressing a related quantity, the other a compound relationship. The analysis of operations is a special part of the more general analysis of relationships. According to Clifford's view, because a sum of operations of the kind considered is independent of the order of the operations, it follows that

$$a + b = b + a \qquad ab = ba$$
$$a(b + c) = ab + ac \qquad (a + b)c = ac + bc.$$

As regards the advance from numbers to quantity he says ('Philosophy of the Pure Sciences,' p. 240): "For reasons too long to give here, I do not believe that the provisional use of unmeaning arithmetical symbols can ever lead to the science of quantity; and I feel sure that the attempt to found it on such abstractions obscures its true physical nature. The science of number is founded on the hypothesis of the distinctness of things; the science of quantity is founded on the totally different hypothesis of continuity. Nevertheless, the relations between the two sciences are very close and extensive. The scale of numbers is used, as we shall see, in forming the mental apparatus of the scale of quantities, and the fundamental conception of equality of ratios is so defined that it can be reasoned about in terms of arithmetic. The operations of addition and subtraction of quantities are closely analogous to the operations of the same name performed on numbers, and follow the same laws. The composition of ratios includes numerical multiplication as a particular case, and combines in the same way with addition and subtraction. So close and far-reaching is this analogy that the processes and results of the two sciences are expressed in the same language, verbal and symbolical, while no confusion is produced by this ambiguity of meaning, except in the minds of those who try to make familiarity with language do duty for knowledge of things."

What is the analogy here spoken of? It cannot be a mere rhetorical analogy; it is a true logical analogy. But what is a logical analogy, except that the subjects have something in common, which is the basis of the common properties. The logical relation of number to quantity is that of subordination; we cannot pass deductively from the former to the latter, but we can pass deductively from the latter to the former. It is easy to pass downwards from quantity to number; the difficulty is in passing upwards from number to quantity.

The most elaborate treatise on algebra written in the English language within recent times is Chrystal's 'Text-book of Algebra,' published in two volumes. The task which the author sets before himself is the same as that which Peacock undertook—namely, to place the teaching of the elements of algebra on a scientific basis, and abreast of what may be called the technical knowledge of the day. In the first volume he starts out with the idea of building up the science on the three laws of association, commutation and distribution, the two former being applicable to addition and subtraction, multiplication and division, and the third to multiplication. The view which he takes of these laws is expressed by the phrase 'canons of the science,' as is evidenced by the following passage: "As we have now completed the establishment of the fundamental laws of ordinary algebra, it may be well to insist once more upon the exact position which they hold in the science. To speak, as is sometimes done, of the proof of these laws in all their generality is an abuse of terms. They are simply laid down as the canons of the science. The best evidence that this is their real position is the fact that algebras are in use whose fundamental laws differ from those of algebra. In the algebra of quaternions, for example, the law of commutation for multiplication and division does not hold generally."

If it is an abuse of terms to speak of the proof of these laws why does Hamilton devote page upon page to the proof of the associative law for a product of quaternions? He is not content with laying it down as a canon; he investigates whether it corresponds to nature. No doubt, the function of the expositor is different from that of the investigator; the latter must establish principles in the best way he can; the former may proceed deductively from these principles as the axioms of the science. But the idea of 'canon' involves something arbitrary and formal which is not involved in the idea of an 'axiom.'

But if we turn to the second volume we find evidence against the canonical nature of these laws, for the author admits that they must be modified within the bounds of algebra itself. The law of association cannot be applied to the terms of an infinite series, unless it is convergent; the law of commutation cannot be applied to the terms of an infinite series, unless it is absolutely convergent; and the law of distribution requires modification when applied to the product of two infinite series. If, in any case, the so-called canons are modified there must be some higher authority to which appeal is made. The only conclusion left is that the rules in question are not canons at all, excepting in so far as they represent properties of the subject analyzed.

I may here refer to the prevalent doctrine that the number-system of arithmetic closes with the complex number, and that the operations of algebra give no indication of any higher imaginary form. For instance, in an article on 'Monism in Arithmetic,' Professor Schubert says: "In the numerical combination $a + ib$, which we also call number, we have found the most general numerical form to which the laws of arithmetic can lead, even though we wished to extend the limits of arithmetic still further. * * * With respect to quaternions which many might be disposed to regard as new numbers it will be evident that though quaternions are valuable means of investigation in geomtry and mechanics they are not numbers of arithmetic, because the rules of arithmetic are not unconditionally applicable to them." When the plane of the complex quantity is that of the axes of x and y it is true that no higher form appears, because in multiplication we get only k and k^2, which is -1. But when Hamilton took for the common plane a general plane passing through the axis of x he immediately encountered a higher form jk, and the problem resolved itself into finding the meaning of that new imaginary combination. He had a great difficulty in emerging out of 'Flatland,' but he succeeded in doing it. The reason given for excluding the quaternion cannot apply, for it would exclude infinite series, as the rules of arithmetic are not unconditionally applicable to them.

Last year there appeared the first volume of a 'Treatise on Universal Algebra,' by Mr. Whitehead, of Trinity College, Cambridge. By universal algebra the author means the various systems of symbolic reasoning allied to ordinary algebra, the chief examples being Hamilton's Quaternions, Grassmann's Calculus of Extension and Boole's Symbolic Logic. The author does not include ordinary algebra in his treatment, and the main idea of the work is not unification of the methods, nor generalization of algebra so as to include them, but a detailed study of each structure, to be followed by a comparative anatomy. In this idea of comparative anatomy there is involved the assumption that these methods are essentially distinct and independent. But that they overlap to a large extent is very evident.

The author preaches the view of the extreme formalist; nevertheless, at various places he makes admissions which are very damaging to it. As regards the fundamental rules he says: "The justification of the rules of inference in any branch of mathematics is not properly part of mathematics; it is the business of experience or philosophy. The business of mathematics is simply to follow the rules. In this sense all mathematical reasoning is necessary; namely, it has followed the rule." Must the mathematician wait for the experimenter or the philosopher to justify the rules of algebra? Was it no part of Hamilton's business to test whether the associative law is true of a product of spherical quaternions? To advance the principles of analysis is surely the special work of the mathematician; to follow the rules discovered is work of a lower order.

Mr. Whitehead thus describes a calculus: "In order that reasoning may be conducted by means of substitutive signs it is necessary that rules be given for the manipulation of the signs. The rules should be such that the final state of the signs after a series of operations according to rule denotes, when the signs are interpreted in terms of the things for which they are substituted, a proposition true for the things represented by the signs. The art of manipulation of substitutive signs according to fixed rules, and of the deduction therefrom of true propositions, is a calculus." By substitutive sign is meant one such that in thought it takes the place of that for which it is substituted. He quotes with approval a saying of Stout's that a word is an instrument for thinking about the meaning which it expresses, whereas a substitutive sign is a means of not thinking about the meaning which it symbolizes; and he adds that the use of substitutive signs in reasoning is to economize thought.

It seems to me that a sign economizes thought in precisely the same way that a word economizes thought, but to greater degree. A word is introduced to dispense with a long phrase or description, and in using the word one no more thinks of its meaning than in using an algebraic symbol does one think of the particular meaning it is made to stand for, for the time being. There seems to be a lurking fallacy that thought is economized by dispensing with it altogether. I prefer the saying of Clifford, with reference to $(a + b)^2 = a^2 + 2ab + b^2$ and its expression in English: "Two things may be observed on this comparison —first, how very much the shorthand expression gains in clearness from its brevity; secondly, that it is only shorthand for something which is just straightforward common sense and nothing else. We may always depend upon it that algebra which cannot be translated into good English and sound common sense is bad algebra."

In his statement of the fundamental principles of algebra Whitehead follows Grassmann to a large extent. He divides them into two classes, the general and the special; the former apply to the whole of ordinary and universal algebra; the latter apply to special branches only. The general principles are as follows: Addition follows the commutative and associative laws; multiplication follows the distributive law, but does not necessarily follow the commutative and associative laws. The theory looks beautiful and plausible, but it does not stand the test of comparison with actual analysis, for quaternions is one of the principal branches of universal algebra, and in it the addition of indices is in general non-commutative, and the power of a binomial of indices is not formed after the distributive law.

But in addition to this formal bond we find in the book another bond uniting the several parts into one whole. In the preface Mr. Whitehead says: "The idea of a generalized conception of space has been made prominent in the belief that the properties and operations involved in it can be made to form a uniform method of interpretation of the various algebras. Thus it is hoped in this work to exhibit the algebras, both as systems of symbolism and also as engines for the investigation of the possibilities of thought and reasoning connected with the abstract general idea of space." The chance for any arbitrary system of symbolism applying to anything real is very small, as the author admits; for he says that the entities created by conventional definitions must have properties which bear some affinity to the properties of existing things. Unless the affinity or correspondence is perfect, how can the one apply to the other? How can this perfect correspondence be secured, except by the conventions being real definitions, the equations true propositions and the rules expressions of universal properties? The placing of the algebra of logic on a space basis has been criticised, but in reply it may be pointed out that logicians have been accustomed ever since the time of Euler to prove their principal theorems by means of diagrams.

Our conclusion about the fundamental rules of algebra is: If the elements of a sum or of a product are independent of order, then the written order of the terms is indifferent, and the product of two such sums is the sum of the partial products; but when the elements of a sum or of a product have a real order, then the written order of the elements must be preserved though the manner of their association may be indifferent, and a power of a binomial is then different from a product. This applies whether the sum or product occurs simply or as the index of a base.

Descartes wedded algebra to geometry; formalism tends to divorce them. The progress of mathematics within the century has been from formalism towards realism; and in the coming century, it may be predicted, symbolism will more and more give place to notation, conventions to principles and loose extensions to rigorous generalizations.

ALEXANDER MACFARLANE.
LEHIGH UNIVERSITY.

PROCEEDINGS OF THE BOTANICAL CLUB OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AT COLUMBUS, AUGUST 21–24, 1899.

THE Botanical Club met in the room assigned for the meetings of Section G pursuant to the resolution adopted by the Boston meeting, Tuesday morning at 9 o'clock, Dr. Byron D. Halsted presiding. The sessions were continued at that hour each morning, and completed by a meeting at 1:30 p. m., Thursday. In the absence of the Secretary, A. D. Selby was chosen Secretary *pro tempore*.

The attendance and interest in the proceedings of the Club were very satisfactory. The number of papers read was quite equal to the time secured for them.

Under the title 'A Greasewood Compass Plant,' Dr. C. E. Bessey reported that on the high, western Nebraska foot-hills a shrubby species of *Sarcobatus* was observed to bear its leaves in an upright position, with their blades parallel to the meridian. Specimens were obtained for microscopic examination.

The same author gave an account of 'A Visit to the Original Station of the Rydberg Cottonwood.' "This species (*Populus acuminata* Ryd.) was discovered a few years ago in Roubedean Township, in Scott's Bluff County, in western Nebraska, in Carter Cañon. This is a broad cañon bounded by high pine-covered buttes, and in the bottom of the broad cañon is a narrower one fringed with deciduous trees—box elder, elm, cottonwood, willow, plum, red cedar, etc., and among them are clumps of the Rydberg cottonwood. The trees are symmetrical and of much greater beauty than those of the common cottonwood. When old the bark of the trunk is light-colored and very deeply fissured.

Dr. N. L. Britton reported to the Club that Mr. and Mrs. A. A. Heller, who were sent to Porto Rico last winter as collectors for the New York Botanical Garden, had returned, having secured about 1,400 numbers, representing probably over 1,200 species, and over 6,000 specimens of plants. They are now being studied.

Dr. William Saunders gave a brief account of 'The Arboretum and Botanic Garden of the Central Experimental Farm at Ottawa, Canada, established in 1889.' During that year 200 species and varieties of woody plants were planted in botanical groups. Additions were made from year to year, and by the end of 1894 the collection included about 600 different species and varieties. Since 1894 progress has been much more rapid, and up to the present time the total number of species under test or which have been tested is 3,071—of these 1,465 have been found hardy, 320 half hardy, 229 tender, 307 winter-killed and 740 have not been tested long enough to admit of an opinion on their hardiness. Where specimens pass the winter uninjured, or with very small injury to the tips of the branches only, they are classed as hardy. When killed back one-fourth to one-half, half-hardy; when killed to the snow-line, tender. A considerable collec-