

integrals of the three kinds. It can then be essentially considered as a product of divers  $\theta$  functions.

All of these functions can appear as integrals of differential equations, a fact which we hope to discuss on some future occasion.

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## ON THE ARITHMETIZATION OF MATHEMATICS.

BY PROFESSOR JAMES PIERPONT.

(Read before the American Mathematical Society at the Meeting of February 25, 1899.)

*Introduction.\**—The following lines are an attempt to show why arithmetical methods form the only sure foundation in analysis at present known. Certain general reasons are indicated in a very suggestive paper by Klein.† I have striven to carry these ideas further and indicate exactly why arguments based on intuition cannot be considered final in analysis. To do this I have grouped certain well known facts so as to support the conclusion which is formulated at the end of this paper. Doubtless a similar train of thought has occurred to others who have dwelt on this fascinating subject, lying on the border line between mathematics and metaphysics; but I have seen nothing of the kind in print. The argument falls under two heads. The first deals with magnitudes or quantities (Grössen). It is very easy to point out the gross lack of rigor in this respect and to show how its correction leads inevitably to the modern theory of irrational numbers as developed by Weierstrass, Dedekind, or G. Cantor. The matter is so obvious that I have devoted, only a few lines to it. The second heading treats of our intuition. This requires more detail, and I have not hesitated to make the argument appeal to all by citing numerous examples.

\* These prefatory remarks have been added to the paper since its presentation.

† "Ueber Arithmetisirung der Mathematik." *Göttinger Nachrichten* (Geschäftliche Mittheilungen) 1895, p. 82. See also Miss Maddison's translation in the *BULLETIN*, 2d series, vol. 2, p. 241.

## §1.

We are all of us aware of a movement among us which Klein has so felicitously styled the arithmetization of mathematics. Few of us have much real sympathy with it, if indeed we understand it. It seems a useless waste of time to prove by laborious  $\epsilon$  and  $\delta$  methods what the old methods prove so satisfactorily in a few words. Indeed many of the things which exercise the mind of one whose eyes have been opened in the school of Weierstrass seem mere fads to the outsider. As well try to prove that two and two make four!

I wish to present a few reflections which, I hope, will destroy the easy confidence felt by the rank and file of our members in regard to matters of rigor and lead them to give the new movement more thoughtful attention.

Let me begin by remarking that there is no absolute criterion of what constitutes rigor in a demonstration. This is largely a personal matter; it also varies greatly with the period. A large part of the reasoning of the last century in analysis would probably be accepted by no one to-day as rigorous. Finally we remark that it is not necessary or even advisable to be equally rigorous at all times. In a vast and almost unexplored region where the notions dealt with are half defined and still plastic, where the methods improvised at each step are too novel to be completely tested we certainly will allow the greatest freedom to the intuition and divining power of creative effort.

There are, however, a few standards which we shall all gladly recognize when it becomes desirable to place a great theory on the securest foundations possible. We should begin with a few simple and evident concepts and postulates and demand that all other notions be defined with all possible clearness by the aid of these. We should admit no demonstration which was not rigorously deduced from what had preceded; we should avoid surreptitiously introducing tacit assumptions, in particular assuming existence theorems. What can be proved should be proved. In attempting to carry out conscientiously this program, analysts have been forced to arithmetize their science. Let us see why.

## §2.

*Quantity and Number.*—For concreteness consider how the standards of rigor we have just laid down apply to the calculus. If we look over the non-arithmetical treatises on the calculus we see confusion reigns from the first moment,

caused by introducing the term quantity (Grösse). We look in vain for a definition of this term and yet any one familiar with modern researches in this extensive field knows that there are many kinds of quantities having widely different properties. What class is meant? At any rate we find all the algebraic operations performed upon them, the rational as well as the irrational (extraction of roots, logarithms, etc.). We ask what do these operations mean and are they possible? For example, one of the first things to know is, when are two quantities equal? Suppose they are two areas bounded by different closed curves. An attempt to decide this question without showing first *how areas can be measured* is obviously futile. Or suppose we ask what we mean by the product of two areas. Now the essence of the rational operations when applied to *numbers* is this: given two numbers, a third is uniquely determined. We know what the product of 5 and 7 is, but until we have defined what the product of two areas is, the term product is an empty word. It is not sufficiently appreciated to-day that the Greeks were thoroughly alive to this fact; they did not consider geometrical magnitudes as numbers. Their attempts to develop an arithmetic for them as far as ratios are concerned is familiar to us in the fifth book of Euclid. Less familiar are the further developments given in the tenth book. If we wish to treat quantity directly without the agency of number we must follow their example. Suppose we avoid the difficulty by saying that, after all, the quantities we are dealing with are numbers. Such an admission is more sweeping than one might think at first sight, but I see no other way out of the difficulty. It is my opinion that this is what one has in mind when one speaks of quantity in works on analysis. The question now arises what numbers are we going to deal with. Certainly the rational numbers. Their arithmetic is easy to establish in a rigorous manner. But the use of radicals, logarithms, and other non-rational numbers, as well as the use of limits, requires an infinity of new numbers, the irrational numbers. Their employment by the non-arithmetician rests on the tacit assumption that the arithmetic of these numbers is identical with that of the rational numbers. He contents himself with an approximate decimal representation and has a hazy idea that operations performed on these are the same as those performed on the numbers themselves. Until these assumptions are demonstrated according to the standards laid down above, we can lay claim to no great rigor.

## §3.

*Intuition.*—With the preceding step analysis is already half arithmetized; the quantities we deal with are numbers; their existence and laws rest on an arithmetic and not on an intuitional basis. The following considerations will lead us to complete the process of arithmetization. The arguments which follow treat of the value we should attach to the evidence of intuition. In my estimation no final value can be given such evidence, and therefore if we are endeavoring to secure the most perfect form of demonstrations it must be wholly arithmetical. The intuitionist does not agree with this. He asserts that in many questions of analysis the evidence of intuition is as binding as any other. Let us see if this is so.

Our geometric intuition is made possible by graphically representing functions of one and two variables by means of curves and surfaces. This brings with it such notions as continuity, tangent lines and planes, curvature, torsion, contact, rectification, quadrature, cubature, etc. Also in considering the nature of a curve or surface we are brought in contact with the concepts of motion, boundaries, velocity, acceleration, etc. Evidently if we propose to apply these notions to our analysis we must be sure 1° that they are clearly and sharply defined in our mind, and 2° that they are coextensive with their analytical equivalents. I shall attempt to show that these absolutely essential conditions are not fulfilled.

I begin by observing that it is a common error to believe that all these notions are given us, either inherently or by experience, in a complete and definite manner. The intuitionist talks of a curve or a surface as if they were as simple as the notion of a straight line or of a plane. I wish to show this is far from true and that, therefore, neither of the conditions just mentioned is fulfilled.

*Notion of a Curve.*—Let us begin by investigating our ideas of a curve as given us by our intuition. Without attempting to define what a curve is, let us enumerate some of its properties more or less undisputed:

1° It can be generated by the motion of a point. 2° It is continuous. 3° It has a tangent. 4° It has a length. 5° When closed it forms the complete boundary of a region. 6° This region has an area. 7° A curve is not superficial. 8° It is formed by the intersection of two surfaces. 9° Its equations are  $x = f(t)$ ,  $y = g(t)$ ,  $z = h(t)$  where  $f$ ,  $g$ ,  $h$  are continuous; and conversely such equations represent a

curve. Of all the properties probably the first is the one that is most conspicuous and characteristic. Indeed many employ it as the definition of a curve. Let us then look at our ideas of motion.

*Motion.*—Here two ideas are essential. ( $\alpha$ ) motion is continuous, ( $\beta$ ) at each instant the motion takes place in a definite direction and with a definite speed. The direction is given, so we agree, by  $dy/dx$ , the speed by  $ds/dt$ . Suppose the curve along which the motion takes place has a *point saillant*, what is the direction of motion at that point? Evidently we must say the motion is impossible, or admit that the ordinary idea of motion is imperfect and must be extended in accordance with the notion of posterior and anterior derivatives. But  $ds/dt$  may also give two speeds at such a point of the function  $s = \varphi(t)$ . This last is a rather novel idea. Again, we will admit that at any point of the path of motion, motion may begin and take place in either direction. Consider what happens for a path given by the continuous function,

$$y = 0, \quad \text{for } x = 0; \quad y = x \sin \frac{1}{x} \quad \text{for } x \neq 0. \quad (1)$$

This curve lies between two right lines making each an angle of  $45^\circ$  with the  $x$ -axis, and oscillates with indefinitely increasing frequency as  $x$  approaches the origin. At the origin the curve has *no tangent*. We ask how does the point move as it passes the origin? Or to make the question still more embarrassing suppose the point at the origin, in what direction does it start to move. We will all agree that no such motion is possible or at least that it is not the motion given us by our intuition. We have then the fact that not all continuous curves can be described by motion.

*Continuous curves without tangents.*—But analysts are familiar with much more singular functions than these. It is easy to construct continuous functions which have absolutely no derivative at all rational points in a given interval, so that in any little interval there are an infinite number of points with tangents, and an infinite number without. Our intuition is utterly helpless to give us any information in regard to such curves. Indeed our intuition would rather say such curves do not exist. The first account of such pathological functions was published by Hankel in 1870. It is interesting to read the indignant paper it called forth by P. Gilbert in the *Mémoires of the Belgian Academy* in 1873. He writes, “Nous croyons faire chose utile en mettant à nu l’erreur de raisonnement sur laquelle repos-

ent de semblables paradoxes qui, répandus dans le champ de la géométrie, auraient pour résultat d'en altérer l'esprit et d'entraîner dans de nouvelles erreurs les géomètres trop confiants. C'est ainsi que nous voyons M. Houël dans un compte-rendu du mémoire de M. Hankel en accepter sans restriction les déductions et les plus étranges résultats. \* \* \* Si des hommes de ce talent peuvent être le jouet de telles illusions, que faut-il attendre des jeunes géomètres !' It is only justice to Gilbert to remark that Hankel's reasoning was not always correct, although his results in the main were.

*To Draw a Conclusion.*—Let us see what kind of a dilemma we are in. Either 1° there are curves which our intuition had not informed us about and we must enlarge our notion of a curve, or 2° we must deny that point ensembles corresponding to these pathological functions are curves, or 3° we must deny that the arithmetical definition of a tangent line to a curve, say a plane curve,

$$y - y_0 = \frac{dy}{dx} (x - x_0)$$

corresponds to our intuitional notion of a tangent or 4° doubt that, although the functions in question satisfy the arithmetical test of continuity, the point assemblage really is continuous from an intuitional standpoint.

None of these alternatives will help us. If we accept the first we admit that our concept of a curve was not complete and clear ; if we accept the second we admit there are continuous functions which do not admit of being represented by a curve and that therefore, when we are establishing the properties of continuous functions without further restriction, our demonstrations must be arithmetical. In regard to the third, one might admit that the definition is not adequate. I must confess that I customarily think of a tangent not as the limit of the secant line, but as a line laid on the curve and that rolls along it. But in default of a better definition we must accept the one we have employed. We come finally to the last point, continuity.

*Continuity.*—Who knows how to describe adequately this concept, over which philosophers have quarrelled since the days of Demokrit and Aristoteles ? As far as our senses go we say a magnitude is continuous when it can pass from one state to another by imperceptible gradations. The minute hand of a clock appears to move continuously, although it really moves by little jerks. Its velocity is thus

to our senses continuous. Our sense notion of continuity we idealize. We not only say that the magnitude shall pass from one state to another by gradations imperceptible to our senses, but we also demand that between any two states another state exists, and this without end. Does such a system form a continuum. No less a mathematician than Bolzano admitted this in his philosophical tract, "Paradoxen des Unendlichen." No one admits it however to-day. But we are not so interested in what constitutes a continuum in the abstract but in what constitutes a continuous curve or a continuous surface. The more one thinks over this knotty question the more one becomes convinced that any definition we can give and which will serve as the base for rigorous deduction, can at best be but an approximate interpretation of the hazy and illusive nature of this concept. The only value it can have therefore is derived from *à posteriori* verification that it is in harmony with the other facts of our intuition. Such a definition is the familiar  $\epsilon, \delta$  criterion of Cauchy-Weierstrass. With this definition we can reason with absolute precision and fineness. The consequences deduced from it are sufficiently in accord with the evidence of our intuition. We can show, for example, by purely arithmetic methods (and, as we see, only such methods are of any use here), that a continuous function of several variables does *attain* its maximum; that if such a function takes on at the point  $P$  the value  $a$  and at the point  $Q$  the value  $b$ , then the function takes on all intermediate values between  $a, b$  when the variables move from  $P$  along any continuous path to  $Q$ ; we can also show that a closed curve without double point, corresponding to the continuous functions

$$x = \varphi(t), \quad y = \psi(t),$$

does form the complete boundary of a region, and so on.

#### §4.

We have now fairly established the justness of the position of the arithmetician. This may be stated as follows: From our intuition we have the notions of curves, surfaces, continuity, etc. To make use of these, the intuitionist translates them into arithmetic language and applies them to reasoning regarding functions of one or more variables which themselves stand for abstract numbers. The arithmetician maintains this procedure is inadmissible since no one can show that the arithmetic formulations are *coextensive*

with their corresponding intuitional concepts. The most we can do is to show by arithmetical verification that the definitions chosen harmonize with the facts of our intuition.

Before leaving this subject I wish to make a couple of remarks. 1° With our  $\varepsilon$ -definition of a continuous function  $y = f(x)$  we have seen that  $y$  must pass through all intermediate values as  $x$  passes from  $x_1$  to  $x_2$ . This last property has been sometimes taken as the definition of a continuous function. It is, however, not coextensive with the first. For example, the function

$$y = 0 \text{ for } x = 0, \quad y = \sin \frac{1}{x} \text{ for } x \neq 0$$

is continuous according to the last definition but not according to the first. 2° A function of two variables  $z = f(x, y)$  has been defined as continuous in a region  $R$  if for every  $y$  in  $R$ ,  $z$  is continuous in respect to  $x$  and for every  $x$  in  $R$ ,  $z$  is continuous in respect to  $y$ . This definition, however, is not equivalent to one currently accepted to-day as is shown by the function

$$z = 0 \text{ for } x = y = 0; \quad z = \frac{xy}{x^2 + y^2} \text{ for all other points.}$$

We have said the arithmetic definitions chosen harmonize sufficiently. The accord is not as complete as we could wish. In the first place we have already seen that continuous functions can present peculiarities which the continuous intuitional curve does not present, viz., unlimited number of oscillations in any interval however small, and absence of tangents. There are other peculiarities we must call attention to. We consider them briefly under separate headings.

*Rectification of curves.*—We think of a curve as having length. Indeed we read as definition of the curve in Euclid's Elements: a line is length without breadth. When we see two curves we can compare one with the other in regard to size, i. e., length, without having consciously established a way to measure them. Perhaps we unconsciously suppose them to be described at a uniform rate and consider the time it takes. It may be that we think of them as thin, inextensible strings whose length is got by straightening them out. A less obvious way to measure their lengths would be to roll them over a straight edge. The problem we have is: how shall we formulate arithmetically our intuitional ideas regarding the length of the curve. The intuitionist says this: the curve or rather



arc of a curve has a length, this length is a number  $L$ , this number is got by taking a number of points on the curve between the two ends  $P, P_1, P_2, \dots, P'$  and building the sum  $\sum P_i, P_{i+1}$ . The limit of this sum when the points become everywhere dense between  $P, P'$  is  $L$ .

To this the arithmetician objects. He says whatever arithmetic formulation I give to this problem, I have no *à priori* assurance that my formula adequately represents my geometric intuition. With the intuitionist I will build the above sum, but whatever may be my opinion whether intuitional curves have a length expressible as a number, I will refrain from assuming the above sum has a limit. On the contrary, I will test it and see if it has a limit, I will investigate if this limit has the same value, however, the points  $P_i$  are chosen. If it has, I will see if this number used as a definition of length will lead me to consequences which are in harmony with my intuition. The fact now is that merely assuming that our curve is continuous the above sum does not always have a limit. Jordan was the first to call attention to the fact that, if the curve is to have a length according to the above definition, besides continuity it must have a limited variation, that is, oscillate in a certain prescribed way. An arc of the curve of equation (1) which includes the origin has no length. Weierstrass's function

$$y = \sum_0^{\infty} b^n \cos a^n \pi x$$

where  $b$  is a positive constant  $< 1$  and  $a$  is an odd integer  $> 1$  is another example. This curve for  $ab > 1$  has no length for an arc contained between two ordinates however near but fixed. It is interesting to remark that the above arithmetic formulation of our intuitional notions of rectification does not depend upon the existence of a derivative. The usual definition does, viz.,

$$(2) \quad s = \int dt \sqrt{f'(t)^2 + g'(t)^2}.$$

These two arithmetic definitions are not coextensive, as it is easy to construct a function for which the above integral is without sense while the first definition given gives a perfectly definite number as length. Du Bois-Reymond questions our right to speak of the length of a curve when not given by (2). This illustrates again that our arithmetic interpretations are more or less arbitrary, at least that we have no *à priori* evidence that they are adequate.

*Quadrature.*—The remarks made in the last paragraph apply equally well here. We shall deny the claims of the intuitionist that every closed plane curve encloses an area, although F. Lindemann, the excellent critic of Euclid takes this stand, if I understand him rightly.\* Without the assurance of the intuitionist we will formulate arithmetically our notions of an area as Peano and Jordan have done. This arithmetic definition we will test and see if the conclusions it leads to are in accordance with our notions of area. So for example this, we will divide our area, not into squares, but by any system of curvilinear squares, which themselves have an area according to the definition being tested and see if we get the same results as before. Evidently the reasoning we here use, by the very nature of the case, must be purely arithmetical. I add that, accepting the arithmetic definition just mentioned, no one has yet proved the statement that a closed figure has an area. In passing we make the remark, now evident, that the proof commonly employed of the existence of a definite integral because it may be regarded as representing an area is absolutely illusory.

In regard to the quadrature of surfaces in space the matter is still more delicate, as is evident when we consider how infinitely more manifold are the complexities a continuous surface may present. An arithmetic formulation of our notions for the area of a surface which was a long time favorably received is this: Inscribe in the surface a polyhedron with little triangular faces; the limit of the surface of such polyhedra when the sides get infinitely small is the area we are seeking. This we observe is analogous to inscribing a polygon in a curve to find its length. This formulation, however, was shown by Schwarz to lead to contradictions and it has been replaced by another.

A remark before leaving the question of measuring magnitudes by numbers. It is commonly thought that all the magnitudes of geometry and physics are measurable and that the way in which this is to be done does not depend upon ourselves, but is inherent in the magnitude itself. This we certainly cannot admit. Magnitudes become measurable only when they have, or are made to have, certain properties. The particular measure chosen lies with us.

*Curves filling areas.*—However vague our notion of a curve and a surface may be, it has been believed that a curve is essentially a one dimensional, the surface a two dimensional

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\* Vorles. über Geometrie, vol. 2, p. 557. The passage in question is, "einer allseitig umgrenzten Figur kommt ein bestimmter Flächeninhalt zu."

assemblage of points. The vague notion of dimensionality was considered to find its interpretation in the number of independent variables defining the position of one of its points in space. From this standpoint the equations

$$(1) \quad x = f(t), \quad y = g(t), \quad z = h(t),$$

depending on the single parameter  $t$  would represent a curve, while

$$(2) \quad x = f(t, u), \quad y = g(t, u), \quad z = h(t, u),$$

having two parameters  $t, u$  represent a surface. This, as we observed, was the means whereby the intuitionist was enabled to employ his geometric intuition when discussing analytically such functions. The researches of Cantor in the theory of point multiplicities showed long ago (1877) that this view was false. He showed that a multiplicity of any number of dimensions could be put in one to one correspondence with a multiplicity of a single dimension. From this follows that the number of parameters used to define the position of a point in the assemblage is altogether arbitrary. The correspondence when one to one is not continuous. Peano was the first to give an example in which the functions in (1) are continuous and yet the corresponding point assemblage is a square or cube,  $t$  varying from say 0 to 1. Cesàro has shown how these functions may be represented analytically. Hilbert has shown how such curves may be construed by geometric considerations. In passing I wish to call attention to the fact that the auxiliary curves given by Hilbert illustrate very prettily the need of Jordan's elaborate proof\* that his polygons never cease to have interior points. These curves of Peano and Hilbert illustrate more forcibly than perhaps any other example how incomplete our notion of a curve is and how rash is the assumption that because certain simple continuous functions represent intuitional curves that therefore they all do. I will remark that the nearest previously known approach to these curves is the cycloids. For example the hypocycloid has for equation

$$x = f(t) = a\{m \cos t + \cos mt\},$$

$$y = g(t) = a\{m \sin t - \sin mt\}.$$

As the parameter  $t$  increases indefinitely,  $m$  being irrational, the point  $x, y$  approaches indefinitely near any point in the

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\* *Cours.*, I, p. 94 *seq.*

ring in which the motion takes place. No part of this curve, however, fills an area however small. This is then quite in accordance with our notion of a curve.

The examples of Peano and Hilbert showed that the line of demarcation between our intuitional notions regarding curves and surfaces was not sharply drawn. Consider another example. Take for simplicity a square. Cut out all points both of whose coördinates are rational. The result is an assemblage of points such that we may pass continuously from any point of it to any other without leaving the multiplicity. Is it a surface or a curve? If we say it is not a surface because it contains only frontier points then Peano's curve is not a curve but a surface. What kind of a function would represent it if we called it a curve?

We will break off now our investigation regarding our notions of a curve. The result certainly is that the various properties we naturally assign a curve are so conflicting that we must agree with Klein "dass vom rein mathematischen Standpunkte aus heutzutage Nichts dunkler und unbestimmter erscheint als die genannte Idee."\*

### §5.

To conclude, we believe that we have shown that the opinion the intuitionist holds in regard to the value of the evidence of our intuition is untenable. Everywhere we have seen that the notions arising from our intuition are vague and incomplete and that it is impossible to show the coextension of these notions and their arithmetical equivalents. The practice of intuitionists of supplementing their analytical reasoning at any moment by arguments drawn from intuition cannot therefore be justified.

The intuitionist believes that in writing down the equation of a curve, the criterion of its continuity, the expression for its length, etc., he has an adequate arithmetic representation of the geometric facts. The arithmetician believes it is impossible to have any *à priori* assurance in this respect. For him these are essentially arithmetic formulæ which stand in more or less close relation with certain geometric notions. The properties of such formulæ are to be deduced by arithmetic methods and their results compared with what we would expect from our intuition. Only when agreement takes place can we conclude that these arithmetic formulations have been fortunately chosen. Or to put the matter differently: we have two worlds, the world

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\* *Math. Annalen*, vol. 50, p. 586.

of our senses and of intuition, and the world of number. Objects in the first world give occasion to form certain objects in the world of number which we strive to make as close to the original as possible. How close the copy is we can never know. Doubtless they are sufficiently approximate.

The mathematician of to-day, trained in the school of Weierstrass, is fond of speaking of his science as “*die absolut klare Wissenschaft.*” Any attempts to drag in metaphysical speculations are resented with indignant energy. With almost painful emotions he looks back at the sorry mixture of metaphysics and mathematics which was so common in the last century and at the beginning of this.\* The analysis of to-day is indeed a transparent science. Built up on the simple notion of number, its truths are the most solidly established in the whole range of human knowledge. It is, however, not to be overlooked that the price paid for this clearness is appalling, it is total separation from the world of our senses.

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## TWO BOOKS ON TIDES.

*The Tides and Kindred Phenomena of the Solar System*; the substance of lectures delivered in 1897 at the Lowell Institute, Boston, Mass. By GEORGE HOWARD DARWIN. Houghton, Mifflin & Company, 1898. 8vo, xviii+378 pp.

*Leçons sur la Théorie des Marées*; professées au Collège de France. Par MAURICE LÉVY. Paris, Gauthier-Villars et Fils, 1898. 4to, xii+298 pp.

It is not often that the reviewer has the opportunity of noticing a volume containing a popular account of an abstruse and difficult subject set forth by an author who stands in the forefront as an investigator of the matters on which he writes. It is never easy for any one who spends most of his time at the confines of his science to tear him-

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\* Hamilton, *Life*, vol. 1, p. 304, writes in a letter dated 1828: “An algebraist who should thus clear away the metaphysical stumbling blocks that beset the entrance to analysis without sacrificing those concise and powerful methods which constitute its essence and its value would perform a useful work and deserve well of science.”