THE ARITHMETIZING OF MATHEMATICS.*

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THOUGH the details of mathematical science, by their very nature, elude the comprehension of the layman, and therefore fail to arouse his interest, yet the mathematician may profitably indicate certain general points of view from which he surveys his science, especially if these points of view determine his attitude to kindred subjects. I propose therefore on the present occasion to explain my position in regard to an important mathematical tendency which has as its chief exponent Weierstrass, whose eightieth birthday we have lately celebrated. I refer to the *arithmetizing* of mathematics. Some account of this tendency and its origin may be given by way of preface.

The popular conception of mathematics is that of a strictly logically coördinated system, complete in itself, such as we meet with, for instance, in Euclid's geometry; but as a matter of fact, modern mathematics in its origin was of a totally different character. With the contemplation of nature as starting point, and its interpretation as object, a philosophical principle, the principle of continuity, was made fundamental; and the use of this principle characterizes the work of the great pioneers, Newton and Leibnitz, and the mathematicians of the whole of the eighteenth century—a century of discoveries in the evolution of mathematics. Gradually, however, a more critical spirit asserted itself and demanded a logical justification for the innovations made with such assurance, the establishment, as it were, of law and order after the long and victorious campaign. This was the time of Gauss and Abel, of Cauchy and Dirichlet. But this was not the end of the matter. Gauss, taking for granted the continuity of space, unhesitatingly used space intuition as a basis for his proofs; but closer investigation showed not only that many special points still needed proof, but also that space intuition had led to the too hasty assumption of the generality of certain theorems which are by no means general. Hence arose the demand for exclusively arithmetical methods of proof; nothing shall be

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accepted as a part of the science unless its rigorous truth can be clearly demonstrated by the ordinary operations of analysis. A glance at the more modern text-books of the differential and integral calculus suffices to show the great change in method; where formerly a diagram served as proof, we now find continual discussions of quantities which become smaller than, or which can be taken smaller than, any given small quantity. The continuity of a variable, and what it implies, or does not imply, are discussed, and a question is brought forward whether we can, properly speaking, differentiate or integrate a function at all. This is the Weierstrassian method in mathematics, the "Weierstrass'sche Strenge," as it is called.

Of course even this assigns no absolute standard of exactness; we can introduce further refinements if still stricter limitations are placed on the association of the quantities. This is exemplified in Kronecker's refusal to employ irrational numbers, and consequent reduction of mathematics to relations between whole numbers only; and in another way in the efforts made to introduce symbols for the different logical processes, in order to get rid of the association of ideas, and the lack of accuracy which creeps in unnoticed, and therefore not allowed for, when ordinary language is used. In this connection special mention must be made of an Italian mathematician, Peano, of Turin, to whom we are indebted for various interesting notes on other points.

Summing up all these developments in the phrase, *the arithmetizing of mathematics*, I pass on to consider the influence of the tendency here described on parts of the science outside the range of analysis proper. Thus, as you see, while voluntarily acknowledging the exceptional importance of the tendency, I do not grant that the arithmetized science is the essence of mathematics; and my remarks have therefore the two-fold character of positive approbation, and negative disapproval. For since I consider that the essential point is not the mere putting of the argument into the arithmetical form, but the more rigid logic obtained by means of this form, it seems to me desirable—and this is the positive side of my thesis—to subject the remaining divisions of mathematics to a fresh investigation based on the arithmetical foundation of analysis. On the other hand I have to point out most emphatically—and this is the negative part of my task—that it is not possible to treat mathematics exhaustively by the method of logical deduction alone, but that, even at the present time, intuition has its special province. For the sake of completeness I ought also to deal with the algorithmic side of mathematics, discussing the importance of symbolic methods, but as this subject does not appeal to me personally, I shall not enter upon it. It must be understood that I have not much that is new to say on special points ; my object is rather to collect and arrange material already familiar, justifying its existence where necessary.

In the short time at my disposal I must content myself with presenting the most important points ; I begin therefore by tracing the relation of the positive part of my thesis to the domain of geometry. The arithmetizing of mathematics began originally, as I pointed out, by ousting space intuition ; the first problem that confronts us as we turn to geometry is therefore that of reconciling the results obtained by arithmetical methods with our conception of space. By this I mean that we accept the ordinary principles of analytical geometry, and try to find from these the geometrical interpretation of the more modern analytical developments. This problem, while presenting no special difficulty, has yet many ramifications, as I have had the opportunity of showing during the past year in a seminar devoted to this subject. The net result is, on the one hand, a refinement of the process of space intuition ; and on the other, an advantage due to the clearer view that is hereby obtained of the analytical results considered, with the consequent elimination of the paradoxical character that is otherwise apt to attach itself to them. What is the most general idea of a curve, of a surface? What is meant by saying of a curve, etc., that it is "analytic" or "non-analytic?" These and similar questions must be thoroughly sifted and clearly explained. The next point is that we must subject the fundamental principles of geometry to a fresh investigation. As far as the theory of the matter is concerned, this might very well be done, as it was originally, on purely geometrical lines ; but in practice on account of the overwhelming complications that present themselves, recourse must be had to the processes of analysis, that is to the methods of analytical geometry. The investigation of the formulæ by means of which we represent the different forms in space (that is, the so-called non-Euclidian geometry, and all that is connected with it) disposes of only one side, and that the more obvious one, of the inquiry ; there still remains the more important question : What justification have we for regarding the totality of points in space as a number-manifoldness in which we interpolate the irrational numbers in the usual manner between the rational numbers arranged in three directions ? We ultimately perceive that space intuition is an inexact conception, and that in order that we may subject it to mathematical treatment, we idealize it by means of the so-called axioms, which actually serve as postulates. Kerry, who died at an early age, dealt with the philosophical side of these questions, and I agree with his results in the main and especially as regards his criticism of DuBois Reymond. Conversely this fresh determination of our conception of space has in its turn given rise to new refinements of our analytical ideas. We picture before us in space an infinite number of points and forms composed of them ; from this idea have sprung the fundamental investigations on masses of points and transfinite numbers with which G. Cantor has opened up new spheres of thought to arithmetical science. Finally it is much to be desired that full use should be made of the new point of view in the further exposition of geometry, especially infinitesimal geometry ; this result will be most easily attained by treating the subject analytically. Of course, I do not mean by this a blind calculation with x, y and z, but merely a subsidiary use of these quantities when the question concerns the precise determination of boundary conditions.

From this outline of the new geometrical programme you see that it differs greatly from any that was accepted during the first half of this century, when the prevailing tendencies led to the development of projective geometry, which has long been established as a permanent constituent of our subject. Projective geometry has opened up for us with the greatest ease many new tracts of country in our science, and has been rightly called a royal road to its own particular branch of learning; our new road is on the contrary arduous and thorny, and unremitting care is needed to clear a way through the obstacles. It leads us back to what is more nearly the geometry of the ancients, and in the light of our modern ideas we learn to understand precisely the true nature of the latter, as Zeuthen has lately shown in the most brilliant manner.

Moreover we must introduce the same process of reasoning into mechanics and mathematical physics. To avoid going too much into detail I will merely explain this by two examples. Throughout applied mathematics, as in the case of space intuition, we must idealize natural objects before we can use them for purposes of mathematical argument; but we find continually that in one and the same subject we may idealize objects in different ways, according to the purpose that we have in view. To mention only a single instance, we treat matter either as continuous throughout space, or as made up of separate molecules, which we may consider to be either at rest or in rapid motion. How and to what degree are these different hypotheses equivalent in regard to the mathematical consequences that can be deduced from them? The earlier expositions of Poisson and others, as also the developments of the Kinetic Theory of Gases, are not sufficiently thorough in this respect for the modern mathematician; the problem requires to be investigated afresh ab initio. I expect that a publication by Boltzmann, which will shortly appear, will contain some interesting conclusions on this subject.

Another question is this: Practical physics provides us plentifully with experimental results, which we unconsciously generalize and adopt as theorems about the idealized objects. The existence of the so-called Green's function on any closed surface with an arbitrarily chosen pole, corresponding to the fact in electricity that a conductor under the influence of a charged point is in a state of electrical equilibrium, belongs to this category; as also the theorem that every finite elastic body is capable of an infinite series of harmonic oscillations, and my deduction of the fundamental propositions of Riemann's Theory of Abelian Functions from our knowledge of the electric currents started on any conductor when the poles of a galvanic battery are applied to it. Are these indeed, taken in the abstract, exact mathematical theorems, or how must they be limited and defined in order that they may become so? Mathematicians have successfully sought to investigate this; first, C. Neumann and Schwarz with their theory of Potential, and later the French school, following on the lines of the German, with the result that the theorems taken from physics have been shown to hold good to a very considerable extent. You see here what is the precise object of these renewed investigations; not any new physical insight, but abstract mathematical argument in itself, on account of the clearness and precision which will thereby be added to our view of the experimental facts. If I may use an expression of Jacobi's in a somewhat modified sense, it is merely a question of intellectual integrity, "die Ehre des menschlichen Geistes."

After expressing myself thus it is not easy, without running counter to the foregoing conclusions, to secure to intuition her due share in our science; and yet it is exactly on this antithesis that the point of my present statements depends. I am now thinking not so much of the cultivated intuition just discussed, which has been developed under the influence of logical deduction and might almost be called a form of memory; but rather of the naive intuition, largely a natural gift, which is unconsciously increased by minute study of one branch or other of the science. The word intuition (Anschauung) is perhaps not well chosen; I mean it to include that instinctive feeling for the proportion of the moving parts with which the engineer criticises the distribution of power in any piece of mechanism he has constructed; and even that indefinite conviction the practiced calculator possesses as to the convergence of any infinite process that lies before him. I maintain that mathematical intuition—so understood—is always far in advance of logical reasoning and covers a wider field.

I might now introduce a historical excursus, showing that in the development of most of the branches of our science, intuition was the starting point, while logical treatment followed. This holds in fact, not only of the origin of the infinitesimal calculus as a whole, but also of many subjects that have come into existence only in the present century. For example, I may remind you of Riemann's Theory of the Functions of a Complex Variable; and I am glad to add also that the Theory of Numbers, a subject which for a long time seemed to be most unsuited for intuitive methods of treatment, appears to have received a fresh impetus from the application of intuition in the hands of Minkowski and others. After this it would be a matter of great interest to trace from the present standpoint the development, not of particular mathematical subjects, but of the individual mathematician; but in regard to this it must suffice to mention that the two most active mathematical investigators of the present day, Lie in Leipzig, and Poincaré in Paris, both originally made use of intuitive methods. But all this, if I pursued it further, would lead us too much into detail, and finally bring us only to particular cases. I prefer to sketch the every day results of this somewhat refined intuition, as regards the quantitative, rather than the merely arithmetical or constructive, treatment of physical or technical problems. Let me refer again to the two examples from the theory of electricity already adduced; any physicist would be able to trace, without further difficulty, and with tolerable accuracy, the form of the surface of Green's function, or, in the second experiment, the shape of the lines of force in a given case. Again, consider any given differential equation, I will say, to take the most simple instance, a differential equation of the first order in two variables. Most probably the analytical method of solution fails; nevertheless we can at once find graphically the general form of the integral curves, as has recently been done for the renowned differential equation of the Problem of Three Bodies by Lord Kelvin, a master in the art of mathematical intuition. The question in all such cases, to use the language of analysis, is one of interpolation, in which less stress is laid on exactness in particular details than on a consideration of the general conditions. I will once more emphasize the fact that in stating all our laws of nature, or in trying to formulate mathematically any actual occurrence, the art lies in making a similar use of interpolation; for we have to consider the simple laws connecting the essential quantities, apart from the multitude of fortuitous disturbances. This is ultimately what I have termed above the process of idealization. Logical investigation is not in place until intuition has completed the task of idealization.

I beg that you will consider these remarks as a description, not as an explanation, of what actually occurs. The mathematician can do no more than state the character of each particular psychical operation from observations of his own mental process. Perhaps some day physiology and experimental psychology will enable us to draw more accurate conclusions as to the relation between the processes of intuition and those of logical thought. The great differences shown by observations of different individuals confirm the supposition that it is indeed a question of distinct, that is, not necessarily connected, mental activities. Modern psychologists distinguish between visual, motor and auditory endowments; mathematical intuition, as above defined, appears to belong more closely to the first two classes, and the logical method to the third class. In common with many of my fellow mathematicians I gladly welcome these investigations which psychologists have only just undertaken, for it is to be hoped that with the increase of accurate information about the psychological conditions of mathematical thought and their particular varieties, many of the differences of opinion which necessarily remain unsettled at present will disappear.

I must add a few words on mathematics from the point of view of pedagogy. We observe in Germany at the present day a very remarkable condition of affairs in this respect; two opposing currents run side by side without affecting one another appreciably. Among the teachers in our Gymnasia the need of mathematical instruction based on intuitive methods has now been so strongly and universally emphasized that one is compelled to enter a protest, and vigorously insist on the necessity for strict logical treatment. This is the central thought of a small pamphlet on elementary geometrical problems which I published last summer. Among the university professors of our subject exactly the reverse is the case; intuition is frequently not only undervalued, but as much as possible ignored. This is doubtless a consequence of the intrinsic importance of the arithmetizing tendency in modern mathematics. But the result reaches far beyond the mark. It is high time to assert openly once for all that this implies, not only a false pedagogy, but also a distorted view of the science. I gladly yield the utmost freedom to the preferences of individual academic teachers, and have always discouraged the laying-down of general rules for higher mathematical teaching, but this shall not prevent me from saying that two classes at least of mathematical lectures must be based on intuition; the elementary lectures which actually introduce the beginner to higher mathematics—for the scholar must naturally follow the same course of development on a smaller scale, that the science itself has taken on a larger—and the lectures which are intended for those whose work is largely done by intuitive methods, namely, natural scientists and engineers. Through this one-sided adherence to logical form we have lost among these classes of men much of the prestige properly belonging to mathematics, and it is a pressing and urgent duty to regain this prestige by judicious treatment.

To return to theoretical considerations, the general views which I uphold in regard to the present problems of mathematical science need scarcely be specially formulated. While I desire in every case the fullest logical working out of the material, yet I demand at the same time an intuitive grasp and investigation of the subject from all sides. Mathematical developments originating in intuition must not be considered actual constituents of the science till they have been brought into a strictly logical form. Conversely, the mere abstract statement of logical relations cannot satisfy us until the extent of their application to every branch of intuition is vividly set forth, and we recognize the manifold connections of the logical scheme, depending on the branch which we have chosen, to the other divisions of our knowledge. The science of mathematics may be compared to a tree thrusting its roots deeper and deeper into the earth and freely spreading out its shady branches to the air. Are we to consider the roots or the branches as its essential part? Botanists tell us that the question is badly framed, and that the life of the organism depends on the mutual action of its different parts.

DARBOUX'S MEMOIR ON CYCLIQUES.

Sur une Classe Remarquable de Courbes et de Surfaces Algébriques et sur la théorie des Imaginaires, par GASTON DARBOUX, Doyen de la Faculté des Sciences de Paris. Second tirage. Paris, A. HERMANN, 1896.

IT is a pity that M. Darboux did not make some additions to this work since its first publication in 1873. He is so full of ideas in a number of mathematical directions that it is a cause for regret that he has not devoted some more time to a subject which offered him once such a fruitful field for original investigation. However, those who know the work will be glad to make acquaintance with it again, and others who are tired of conic sections and quadrics may be gratified by finding in it a somewhat similar but still novel field of investigation.

Darboux calls the curves and surfaces which he treats of cycliques and cyclides, respectively. The latter word has been generally adopted in English as a name for the surfaces, but the former has been replaced by bicircular quartics and sphero-quartics, which two designations conveniently distinguish between the plane and spherical curves. The name cyclide had already been used for a particular form of the surface by Dupin, and soon came to be adopted generally in its present sense, while the different names of the curves arose from their being studied independently in Great Britain and France. At one time, from about 1865 until early in the seventies, these curves and surfaces were studied enthusiastically in France by Darboux, Laguerre, de la Gournerie, Moutard, Mannheim and others, while they received attention in England at the hands of Crofton and Clifford, and in Ireland secured very full and adequate treatment from Casey. All these mathematicians worked at the same time and nearly entirely independently of each other. Thus Darboux's book contains much that has been more fully worked out elsewhere, and, besides, of course, it has no references to many striking results that have been arrived at in this branch of geometry, as, for instance, Casey's ingenious method of rectification of the bicircular quartics and sphero-quartics. But these wants are made up