RIEMANN AND HIS SIGNIFICANCE FOR THE DEVELOPMENT OF MODERN MATHEMATICS.*

ADDRESS DELIVERED AT THE GENERAL SESSION OF THE VERSAMMLUNG DEUTSCHER NATURFORSCHER UND AERZTE, IN VIENNA, SEPTEMBER 27, 1894,

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It is no doubt uncommonly difficult to entertain a large audience with the discussion of any mathematical question or even of the general tendencies in the development of mathematical science. This difficulty arises from the fact that the very ideas with which the mathematician works and whose multifarious connections and interrelations he investigates are the product of long-continued mental labor and are therefore far removed from the things of ordinary life.

In spite of this I did not hesitate in accepting the honor conferred upon me by the Executive Committee of your Association in requesting me to address you to-day. In doing this I was moved by the illustrious example of the great investigator, so recently deceased, who had originally been expected to speak here to you. It must always be regarded as a particular merit of Hermann von Helmholtz that, from the very beginning of his career, he took pains to present in lectures intelligible to a wider circle of scientific men the problems and results of special work in all the manifold branches of science that engaged his attention. He thus succeeded in being of assistance to each one of us in his own special field.

While for pure mathematics it would, in the nature of the case, be impossible to do this completely, it is becoming more and more recognized that in the present state of mathematical science it is eminently desirable to try, at least, to accomplish as much as can be attained in this respect. In saying this I do not express an individual opinion; I speak in the name of all the members of that Mathematical Association which was formed some years ago in connection with the Association of Naturalists and Physicians and is practically, if not formally, identical with your Section I. We cannot help feeling that in the rapid development of modern thought our science is in danger of becoming more and more isolated. The intimate mutual relation between mathematics and theoretical natural science which, to the lasting benefit of both sides, existed ever since the rise of modern analysis, threatens

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to be disrupted. As members of the Mathematical Association we desire to counteract this serious, ever-growing danger with all our power; it is for this reason that we decided to meet with the general Association of Naturalists. Through personal intercourse with you we wish to learn how scientific thought develops in your branches, and where accordingly an opportunity may arise for applying the work of the mathematician. On the other hand, we desire and hope to find in you a ready interest in and an intelligent appreciation of our aims and ideas.

With these considerations in mind I shall now attempt to give you an idea of the life-work of BERNHARD RIEMANN, a man who more than any other has exerted a determining influence on the development of modern mathematics. I hope that my remarks may prove of some interest to those, at least, among you who are familiar with the general trend of ideas prevailing in mechanics and theoretical physics. But all of you, I trust, will feel that the ideas of which I shall have to speak form a connecting link between mathematics and natural science.

The outward life of Riemann may perhaps appeal to your sympathy; but it was too uneventful to arouse particular interest. Riemann was one of those retiring men of learning who allow their profound thoughts to mature slowly in the seclusion of their study.

He was twenty-five years of age when, in 1851, he took his doctor's degree in Göttingen, presenting a dissertation which showed remarkable power. It took three more years before he became a docent at the same university. At this time began the appearance in rapid succession of all those important researches of which I wish to speak. At the death of Dirichlet, in 1859, Riemann became his successor in the University of Göttingen. But already in 1863 he contracted the fatal disease to which he succumbed in 1866, at the early age of 40 years.

His collected works, first published in 1876 by Heinrich Weber and Dedekind, and since issued in a second edition, are neither numerous nor extensive. They fill an octavo volume of 550 pages, and only about half of the matter contained in this volume appeared during his lifetime. The remarkable influence exerted by Riemann's work in the past and even to the present time is entirely due to the *originality* and, of course, to the *penetrating power* of his mathematical considerations.

While the latter characteristic cannot be the object of my remarks to-day, I believe that you will understand the peculiar originality of Riemann's mathematical work if I point out to you right here the unifying fundamental idea to which as a common source all his developments can be traced.

I must mention, first of all, that Riemann devoted much time and thought to physical considerations. Grown up under the great tradition which is represented by the combination of the names of Gauss and Wilhelm Weber, influenced on the other hand by Herbart's philosophy, he endeavored again and again to find a general mathematical formulation for the laws underlying all natural phenomena. These investigations do not appear to have been carried by him to a satisfactory completion; they are preserved in his posthumous papers in a very fragmentary form. We find there several incomplete attempts at a solution, all of which have, however, in common the supposition which at the present day, under the influence of Maxwell's electromagnetic theory of light, seems to be adopted by at least all younger physicists, viz., that space is filled with a continuous fluid which serves as the common medium for the propagation of optical, electrical, and gravitational phenomena.

I shall not stop to explain the details, which at the present time could only have a historical interest. The point to which I wish to call your attention is that *these physical views are the mainspring of Riemann's purely mathematical investigations.* The same tendency which in physics discards the idea of action at a distance and explains all phenomena through the internal stresses of an all-pervading ether appears in mathematics as the attempt to understand functions from the way they behave in the infinitesimal, that is, from the differential equations satisfied by them. And just as in physics any particular phenomenon depends on the general arrangement of the conditions of the experiment, thus Riemann particularizes his functions by means of the special boundary conditions which they are required to satisfy. From this point of view the formula required for the numerical calculation of the function appears as the final result, and not as the starting-point, of the investigation.

If I may be allowed to push the analogy so far, I should say that *the work of Riemann in mathematics offers a parallel to the work of Faraday in physics.* While this comparison has reference in the first place to the *qualitative* content of the leading ideas due to the two men, I believe it to hold even *quantitatively;* i.e., the results reached by Riemann are as *important* for mathematics as the results of Faraday's work are for physical science.

On the basis of this general conception let us now pass in review the various lines of Riemann's mathematical researches. It will only be natural to begin with *the theory of functions of complex variables,* which is most intimately connected with his name, although he himself may have regarded it merely as an application of tendencies having a much wider range.

The fundamental idea of this theory is well known. To investigate the functions of a variable z we substitute for this variable a binomial quantity $x + iy$ with which we operate so as to put always -1 for i^2. The result is that the properties of the ordinary functions of simple variables become intelligible to a much higher degree than would otherwise be the case. To repeat the words used by Riemann himself in his dissertation (1851) in which he laid down the fundamental ideas of his peculiar method of treating this theory: *The introduction of complex values of the variable brings out a certain harmony and regularity which otherwise would remain hidden.*

The founder of this theory is the great French mathematician Cauchy;* but only later, in Germany, did this theory assume its modern aspect which has made it the central point of our present views of mathematics. This was the result of the simultaneous efforts of two mathematicians whom we shall have to name together repeatedly,—of Riemann and Weierstrass.

While pointing to the same end, the particular methods of these two men are as different as possible; they appear almost to be opposed to each other, though, from a higher point of view, this only means that they are complementary.

Weierstrass defines the functions of a complex variable analytically by means of a common formula, viz., by infinite power-series. In all his work he avoids as far as possible the assistance to be derived from the use of geometry; his special achievement lies in the systematic rigor of his demonstrations.

Riemann, on the other hand, begins, in accordance with his general conception referred to above, with certain differential equations satisfied by the functions of $x + iy$. Thus the problem at once assumes a physical form.

Let us put $f(x + iy) = u + iv$. Each of the two component parts of the function, u as well as v, then appears, owing to the differential equations, as a *potential* in the space of the two variables x and y; and Riemann's method can be briefly characterized by saying that *he applies to these parts u and v the principles of the theory of the potential.* In other words, his starting point lies in the domain of mathematical physics.

* In the text I refrained from mentioning Gauss, who, being in advance of his time in this as in other fields, anticipated many discoveries without publishing what he had found. It is very remarkable that in the papers of Gauss we find occasional glimpses of methods in the theory of functions which are completely in line with the later methods of Riemann, as if unconsciously a transfer of leading ideas had taken place from the older to the younger mathematician.

You will notice that even in mathematics free play is given to individuality of treatment.

It should also be observed that the theory of the potential, which in our day, owing to its importance in the theory of electricity and in other branches of physics, is quite universally known and used as an indispensable instrument of research, was at that time in its infancy. It is true that Green had written his fundamental memoir as early as in 1828; but this paper remained for a long time almost unnoticed. In 1839 Gauss followed with his researches. As far as Germany is concerned, it is mainly due to the lectures of Dirichlet that the theory was farther developed and became known more generally; and this is where Riemann finds his base of operations.

Riemann's specific achievement in this connection consists, of course, in the first place in the tendency to make the theory of the potential of fundamental importance for the whole of mathematics; and secondly, in a series of *geometric constructions*, or, as I should prefer to say, of *geometric inventions*, of which you must allow me to say a few words.

As a first step Riemann considers the equation $u + iv = f(x + iy)$ throughout as a *representation* (Abbildung) of the xy-plane on the uv-plane. It appears that this representation is conformal, i.e., that it preserves the magnitude of the angles; indeed, it is directly characterized by this property. We have thus a new means of defining the functions of $x + iy$. Riemann develops in this way the elegant proposition that there always exists a function f that transforms any simply-connected region of the xy-plane into any given simply-connected region of the uv-plane; this function is fully determined, apart from three constants which remain arbitrary.

Next he introduces as a fundamental idea the conception of what is now called a *Riemann surface*, i.e., a surface which overlies the plane several times, the different sheets hanging together at the so-called branch-points. This step in the development, while it must have been the most difficult to take, proved at the same time of the greatest consequence. Even at the present time we can daily notice how hard it is for the beginner to understand the essential idea of the Riemann surface and how he comes at once into the possession of the whole theory as soon as he has fully grasped this fundamental mode of conceiving the function. The Riemann surface furnishes us a means of understanding many-valued functions of $x + iy$ in the course of their variation. For on this surface there exist potentials just like those on the simple plane, and their laws can be investigated by the same methods; moreover, the representation (Abbildung) is again conformal.

As the primary principle of classification appears here the order of connectivity of these surfaces, i.e., the number of the cross-sections or cuts that can be made without resolving the surface into separate portions. This is again an entirely new geometrical method of attack, which in spite of its elementary character had never been touched upon by anybody before Riemann.

Perhaps I have gone too far into the details with what I have said. I wish only to add that all these new tools and methods, created by Riemann for the purposes of pure mathematics out of the physical intuition, have again proved of the greatest value for mathematical physics. Thus, for instance, we now always make use of Riemann's methods in treating the *stationary* flow of a fluid within a two-dimensional region. A whole series of most interesting problems, formerly regarded as insolvable, has thus been solved completely. One of the best known problems of this kind is Helmholtz's determination of the shape of a free liquid jet.

Perhaps less attention has been paid to another physical application in which Riemann's ways of looking at things are laid under contribution in a most attractive manner. I have in mind the theory of *minimum surfaces*. Riemann's own investigations on this subject were not published till after his death, in 1867, almost simultaneously with the parallel investigations of Weierstrass concerning the same question. Since that time the theory has been developed much farther by Schwarz and others. The problem is to determine the shape of the least surface that can be spread out in a rigid frame,—let us say, the form of equilibrium of a fluid lamina that fits in a given contour. It is noteworthy that, with the aid of Riemann's methods, the known functions of analysis are just sufficient to dispose of the more simple cases.

These applications to which I here call particular attention represent, of course, merely one side of the matter. There can be no doubt that the main value of these new methods in the theory of functions is to be found in their use in *pure mathematics*. I must try to show this more distinctly, in accordance with the importance of the subject, without presupposing much special knowledge.

Let me begin with the very general question of the present state of progress in the domain of pure mathematics. To the layman the advance of mathematical science may perhaps appear as something purely arbitrary because the concentration on a definite given object is wanting. Still there exists a regulating influence, well recognized in other branches of science, though in a more limited degree; it is the *historical continuity*. *Pure mathematics grows as old problems are* *worked out by means of new methods. In proportion as a better understanding is thus gained for the older questions, new problems must naturally arise.*

Guided by this principle we must now briefly pass in review the working material that Riemann found ready for use in the theory of functions when he entered upon his scientific career. It had then been recognized that particular importance attaches to three classes among the various kinds of analytic functions (i.e., functions of $x + iy$). The first class comprises the *algebraic* functions which are defined by a finite number of elementary operations (addition, multiplication, and division); in contradistinction to these we have the transcendental functions whose definition requires an infinite series of such operations. Among these transcendental functions the simplest are, on the one hand, the logarithms, on the other the trigonometric functions, such as the sine, cosine, etc.

But in Riemann's time mathematical science had already advanced beyond these elementary functions, first, to the *elliptic* functions derived from the inversion of the elliptic integrals, and, second, to the *functions connected with Gauss's hypergeometric series,* viz., spherical harmonics, Bessel functions, gamma-functions, etc.

Now what Riemann accomplished may be stated briefly by saying that for each of these three classes of functions he found not only new results, but entirely new points of view which have formed a continual source of inspiration up to the present time. A few additional remarks may serve to explain this more in detail.

The study of *algebraic functions* practically coincides with the study of algebraic *curves* whose properties are investigated by the geometer, whether he calls himself an "analyst," regarding the analytical formula as of primary importance, or a "synthetic geometer," as the term was understood by Steiner and von Staudt, who operates with the row of points and the pencil of rays. The essentially new point of view, here introduced by Riemann, is that of the general single-valued transformation (or one-to-one correspondence). This point of view allows to group the innumerable variety of algebraic curves into large classes; and by making abstraction of the peculiarities of shape of the individual curves, those general properties can be studied that belong in common to all the curves of the same class. The geometers have not been slow to derive the results so obtained from their point of view and to develop them still farther; Clebsch, in particular, worked in this direction, and he even began to attack the corresponding problems for algebraic configurations of more dimensions. But it must be insisted upon that the theory of curves should try to assimilate the *methods* of Riemann in their true essential character. A first step will consist in constructing on the curve itself the analogue of the two-dimensional Riemann surface; and this can be done in various ways. A further step would have to show us how to operate with the methods of the theory of functions in the configuration thus defined.

The theory of *elliptic integrals* finds its further development in the consideration of the general integrals of algebraic functions, a subject on which the Norwegian mathematician Abel published the first fnndamental investigations in the twenties of the present century. It must always be regarded as one of the greatest achievements of Jacobi that, by a sort of inspiration, he established for these integrals a problem of inversion which furnishes single-valued functions just as the simple inversion does in the case of the elliptic integrals. The actual solution of this problem of inversion is the central task performed at the same time, but by different methods, by Weierstrass and Riemann. The great memoir on the Abelian functions in which Riemann published his theory in 1857 has always been recognized as the most brilliant of all the achievements of his genius. Indeed, the result is here reached, not by laborious calculations, but in the most direct way, by a proper combination of the geometrical considerations just referred to. I have shown in another place that his results concerning the integrals, as well as the conclusions that follow for the algebraic functions, can be obtained in a very graphic manner by considering the stationary flow of a fluid, say of electricity, on closed surfaces situated in any way in space. This, however, has reference only to one half of Riemann's memoir. The second half, which is concerned with the theta-series, is perhaps still more remarkable. The important result is here deduced that the theta-series required for the solution of Jacobi's problem of inversion are not the general theta-series; and this leads to the new problem of determining the position of the general theta-series in our theory. According to an observation made by Hermite, Riemann must have known the proposition published at a later time by Weierstrass and recently discussed by Picard and Poincaré, that the theta-series are sufficient for defining the most general periodic functions of several variables.

But I must not enter into these details. It is difficult to give a connected account of the further development of Riemann's theory of Abelian functions because the far-reaching investigations of Weierstrass on the same subject are as yet known only from written lectures of his students. I will therefore only mention that the important treatise published by Clebsch and Gordan in 1866 had in the main the object of deriving Riemann's results on the algebraic curve by means of analytic geometry. At that time Riemann's methods were still a sort of secret science confined to his immediate pupils and were regarded almost with distrust by other mathematicians. I can in this respect only repeat what I just remarked in speaking of the theory of curves, viz., that the growing development of mathematics leads with necessity to the incorporation of Riemann's methods into the general body of mathematical science. It is interesting to compare in this respect the latest French text-books.*

The third class of functions referred to above comprises those laws of dependence that are connected with Gauss's *hypergeometric series*. In a wider sense, we have here the functions defined by linear differential equations with algebraic coefficients. Riemann published on this subject during his lifetime only a preliminary study (in 1856) which is devoted entirely to the hypergeometric case and shows in a surprising way how all the remarkable properties of the hypergeometric function, that had been known before, can be derived without calculation from the behavior of this function when the variable passes around the singular points. We now know from his posthumous papers in what form he had intended to carry out the corresponding general theory of the linear differential equations of the nth order. Here, also, he wanted to take as a starting point and primary characteristic for the classification the group of those linear substitutions which the integrals undergo when the variable passes around the singular points.

This idea, which in a certain sense corresponds to Riemann's treatment of the Abelian integrals, has not yet been carried out according to Riemann's extensive plan. The numerous investigations on linear differential equations published during the last decades have really disposed only of certain parts of the theory. In this respect the researches of Fuchs deserve special mention.

As regards the differential equations of the *second* order, they are capable of a simple geometrical illustration. It is only necessary to consider the conformal representation which the quotient of two particular integrals of the differential equation furnishes for the region of the independent variable. In the simplest case, i.e. that of the hypergeometric function, we obtain the representation of the half-plane on a triangle formed by circular arcs; and this establishes a noteworthy connection with spherical trigonometry. In the general theory there are cases which admit of one-valued (*eindeutig*) inversion and thus produce those remarkable functions of a single

* See, for instance, PICARD, *Traité d'analyse*, and APPELL et GOURSAT, *Théorie des fonctions algébriques et de leurs intégrales.*

variable which, like the periodic functions, are transformed into themselves by an infinite number of linear transformations and which I have therefore called *automorphic* functions.

All these developments which occupy the mathematicians of our time appear more or less explicitly in Riemann's posthumous papers, particularly in the memoir on minimum surfaces referred to above. For further details I must refer you to Schwarz's memoir on the hypergeometric series and to the epoch-making researches of Poincaré in the theory of automorphic functions. With this group of investigations must also be classed those on the elliptic modular functions and the functions of the regular bodies.

I cannot leave the discussion of Riemann's work in the theory of functions without mentioning an isolated paper in which the author makes interesting contributions to the theory of definite integrals. This paper has become celebrated mainly owing to the application which the author makes of his results to a problem in the theory of numbers, viz., *the law of distribution of the prime numbers* in the natural series of numbers. Riemann arrives at an approximate expression for this law which agrees more closely with the results obtained by actual enumerations than the empirical rules that had been deduced up to that time from such enumerations.

Two remarks naturally present themselves in this connection. First, I should like to call your attention to the curious way in which the various branches of higher mathematics are interwoven: a problem apparently belonging to the elements of the theory of numbers is here in a most unexpected manner brought nearer to its solution by means of considerations derived from the most intricate questions in the theory of functions. Second, I must observe that the proofs in Riemann's paper, as he notices himself, are not quite complete; in spite of numerous attempts in recent times, it has not yet been possible to make all these proofs perfectly satisfactory. It appears that Riemann must have worked very largely by intuition.

This, by the way, is also true of his manner of establishing the foundations of the theory of functions. Riemann here makes use of a mode of reasoning often employed in mathematical physics; he designated this method as *Dirichlet's principle,* in honor of his teacher Lejeune Dirichlet. When it is required to determine a continuous function that makes a certain double integral a minimum, this principle asserts that the *existence* of such a function is evident from the existence of the problem itself.* Weierstrass has shown that

* It appears from the context that, contrary to a common way of

this inference is faulty: it may happen that the required minimum represents a limit which cannot be reached within the region of the continuous functions. This consideration affects the validity of a large portion of Riemann's developments.

Nevertheless the far-reaching results based by Riemann on this principle are all correct, as has been shown later by Carl Neumann and Schwarz with the aid of rigorous methods. It must be supposed that Riemann originally took his theorems from the physical intuition which here again proved to be of the greatest value as a guide to discovery, and that he connected them afterwards with the method of inference mentioned above, in order to obtain a connected chain of mathematical reasoning. It appears from the long developments of his dissertation that in doing this he became conscious of certain difficulties; but seeing that this mode of reasoning was used without hesitation in analogous cases by those around him, even by Gauss, he does not seem to have pursued these difficulties as far as might have been desired.

So much about the functions of complex variables. They form the only branch of mathematics that Riemann has treated as a connected whole; all his other works are separate investigations of particular questions. Still we should obtain a very inadequate picture of the mathematician Riemann if we were to disregard these latter researches. For, apart from the notable results reached by him, the consideration of these researches will place in better perspective the general conception that ruled his thoughts and the programme of work that he had laid out for himself. Besides, every one of these investigations has exerted a highly stimulating and determining influence on the further development of mathematical science.

To begin with, let me state more fully what I indicated above, viz., that Riemann's treatment of the theory of functions of complex variables, founded on the partial differential equation of the potential, was intended by him to serve merely as an *example* of the analogous treatment of all other physical problems that lead to partial differential equations, or to differential equations in general. In every such case it should be inquired what discontinuities are compatible with the differential equations, and how far the solutions may be

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speaking, I mean by the "principle" the mode of reasoning, and not the results deduced thereby.

I wish to use this opportunity to call attention to an article by Sir Wm. Thomson, published in Liouville's *Journal*, vol. 12 (1847), which does not seem to have been sufficiently noticed by German mathematicians. In this article the principle is expressed in a very general form.

determined by the discontinuities and accessory conditions occurring in the problem. The execution of this programme which has since been considerably advanced in various directions, and which has in recent years been taken up with particular success by French geometers, amounts to nothing short of a *systematic reconstruction of the methods of integration required in mechanics and mathematical physics.*

Riemann himself has treated on this basis only a single problem in greater detail, viz., the propagation of plane waves of air of finite amplitude (1860).

Two main types must be distinguished among the linear partial differential equations of mathematical physics: the elliptic and the hyperbolic type, the simplest examples being the differential equation of the potential and that of the vibrating string, respectively. As an intermediate limiting case we may distinguish the parabolic type to which belongs the differential equation of the flow of heat. The recent investigations of Picard have shown that the methods of integration used in the theory of the potential can be extended with slight modifications to the elliptic differential equations generally. How about the other types? Riemann makes in his paper an important contribution to the solution of this question. He shows what modifications must be made in the well-known "boundary problem" of the theory of the potential and in its solution by means of Green's function in order to make this method applicable to the hyperbolic differential equations.

This paper of Riemann's is noteworthy for various other reasons. Thus, the reduction of the problem named in the title to a linear differential equation is in itself no mean achievement. Another point to which I desire to call your particular attention is the *graphical treatment of the problem* which evidently underlies the whole memoir. While this mode of treatment will have nothing surprising to the physicist, its value is in our day not infrequently underrated by the mathematician accustomed to more abstract methods. I take, therefore, particular pleasure in pointing to the fact that an authority like Riemann makes use of this mode of treatment and derives by its means most interesting results.

It remains to discuss the two great memoirs presented by Riemann, at the age of 28 years, when he became a docent at Göttingen, in 1854, viz., the essay *On the hypotheses that lie at the foundation of geometry*, and the memoir *On the possibility of representing a function by means of a trigonometric series.* It is remarkable how differently these two papers have been treated by the wider scientific public. The importance of the disquisition on the hypotheses of geometry has long since been adequately recognized, no doubt mainly owing to the part that Helmholtz took in the discussion of the problem, as is probably known to most of you. The investigation of trigonometric series, on the other hand, has so far not become known outside the circle of mathematical readers. Nevertheless the results contained in this latter paper, or rather the considerations to which it has given rise and with which its subject is intimately connected, must be regarded as of the highest interest from the point of view of the theory of knowledge.

As regards the *hypotheses of geometry*, I shall not enter here upon the discussion of the philosophical significance of the subject, as I should not have anything new to add. For the mathematician the interest of the discussion centres not so much in the origin of the geometrical axioms as in their mutual logical interdependence. The most celebrated question is that as to the nature of the axiom of parallels. It is well known that the investigations of Gauss, Lobachevsky, and Bolyai (to mention only the most prominent names) have shown that the axiom of parallels is certainly not a consequence of the other axioms, and that by disregarding the axiom of parallels a more general, perfectly consistent geometry can be constructed which contains the ordinary geometry as a particular case. Riemann gave to these important considerations a new turn by introducing and applying the mode of treatment of *analytic* geometry. He regards space as a particular case of a triply-extended numerical manifoldness in which the square of the element of arc is expressed as a quadratic form of the differentials of the co-ordinates. Without discussing the special geometrical results thus obtained and the subsequent development of this theory, I only wish to point out that here again Riemann remains faithful to his fundamental idea: *to interpret the properties of things from their behavior in the infinitesimal.* He has thereby laid the foundation for a new chapter of the differential calculus, viz., *the theory of the quadratic differential expressions of any number of variables,* and in particular of the *invariants* of such expressions under any transformations of the variables. I must here depart from the prevailing character of my remarks and call special attention to the abstract side of the matter. There can be no doubt that in trying to *discover* mathematical relations it is by no means indifferent whether we endow the symbols with which we operate with a definite meaning or not; for it is just through these concrete representations that we form those associations of ideas which lead us onward. The best proof of this will be found in almost everything that I have said to-day as to the intimate relation of Riemann's mathematics to mathematical physics. But the final result of the mathematical investigation is quite independent of these considerations and rises above these special auxiliary methods; it represents a general logical framework whose content is indifferent and can be selected in various ways according to the nature of the case. Considered from this point of view it will no longer appear surprising that at a later period (1861), in the prize essay presented to the Paris Academy, Riemann made an application of his investigation of differential expressions to the problem of the flow of heat, i.e., to a subject which surely has nothing to do with the hypotheses of geometry. In a similar way these researches of Riemann are connected with the recent investigations concerning the equivalence and classification of the general problems of mechanics. For it is possible, according to Lagrange and Jacobi, to represent the differential equations of mechanics in such a way as to make them depend on a single quadratic form of the differentials of the co-ordinates.

I now pass to the consideration of the memoir *On the trigonometric series,* which I have intentionally left to the last, because it brings into prominence a final essential characteristic of Riemann's conception. In all the preceding remarks it has been possible for me to appeal to the current ideas of physics, or at least of geometry. But Riemann's penetrating mind was not satisfied with making use of the geometrical and physical intuition; he went so far as to investigate this intuition critically, and to inquire into the *necessity* of the mathematical relations flowing from it. The question at issue is nothing less than the *fundamental principles of the infinitesimal calculus.* In his other works Riemann has nowhere expressed any definite opinions concerning these questions. It is different in the paper on trigonometric series. Unfortunately he considers only detached problems, viz., the questions whether a function can be discontinuous at every point, and whether in the case of functions of such a general nature it is still possible to speak of integration. But these problems he treats in so convincing a manner that the investigations of others on the foundations of analysis have received from him a most powerful impulse.

Tradition has it that in later years Riemann pointed out to his students the fact which must be regarded as the most remarkable result of the modern critical spirit: the existence of functions which are not differentiable at any point. A more detailed study of such "nonsensical" functions (as they used to be called formerly) has, however, been made only by Weierstrass, who has probably contributed most to give its present rigorous form to the *theory of the real functions of real variables,* as this whole field is now usually designated.

As I understand Riemann's developments on trigonometric series, he would, as far as the foundation is concerned, agree with the presentation given by Weierstrass, which discards all space-intuition and operates exclusively with arithmetical definitions. But I could never believe that Riemann should have considered this space-intuition (as is now occasionally done by extreme representatives of the modern school) as something antagonistic to mathematics, which must necessarily involve faulty reasoning. I must insist on the position that a compromise is possible in this difficulty.

We touch here upon a question which I am inclined to consider as of decisive importance for the further development of mathematical science in our time. Our students are at present introduced at the very beginning to all those intricate relations whose possibility has been discovered by modern analysis. This is no doubt very desirable; but it carries with it the dangerous consequence that our young mathematicians frequently hesitate to formulate any general propositions, that they are lacking in that freshness of thought without which no success is possible in science as well as elsewhere.

On the other hand, the majority of those engaged in applied science believe that they may entirely leave aside these difficult investigations. Thus they detach themselves from rigorous science and develop for their private use a special kind of mathematics which grows up like a wild sprig from the root of the grafted tree. We must try with all our might to overcome this dangerous split between pure and applied mathematics. I may therefore be allowed to formulate my personal position concerning this matter in the following two propositions:

First, I believe that those defects of space-intuition by reason of which it is objected to by mathematicians are merely temporary, and that this intuition can be so trained that with its aid the abstract developments of the analysts can be understood, at least in their general *tendency*.

Second, I am of the opinion that, with this more highly-developed intuition, the applications of mathematics to the phenomena of the outside world will, on the whole, remain unchanged, provided we agree to regard them throughout as a sort of *interpolation* which represents things with an approximation, limited, to be sure, but still sufficient for all practical purposes.

With these remarks I will close my address, which I hope has not taxed your indulgence unduly. You may have noticed that even in mathematics there is no standstill, that the same activity prevails there as in the natural sciences. And this, too, is a general law: that while many workers contribute to the development of science, the really new impulses can be traced back to but a small number of eminent men. But the work of these men is by no means confined to the short span of their life; their influence continues to grow in proportion as their ideas become better understood in the course of time. This is certainly the case with Riemann. For this reason you must consider my remarks not as the description of a past epoch, whose memory we cherish with a feeling of veneration, but as the picture of live issues which are still at work in the mathematics of our time.

THE MULTIPLICATION OF SEMI-CONVERGENT SERIES.

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In *Math. Annalen*, vol. 21, pp. 327-378, A. Pringsheim developed sufficient conditions for the convergence of the product of two semi-convergent series, formed by Cauchy's multiplication rule, when one of the series becomes absolutely convergent, if its terms are associated into groups with a finite number of terms in each group. The necessary and sufficient conditions for convergence were obtained by A. Voss (*Math. Annalen*, vol. 24, pp. 42–47) in case that there are two terms in each group, and by the writer (*Am. Jour. Math.*, vol. 15, pp. 339–343) in case that there are p terms in each group, p being some finite integer. In this paper it is proposed to deduce the necessary and sufficient conditions in the more general case when the number of terms in the various groups is not necessarily the same.

Let $U_n = \sum_0^n a_n$ and $V_n = \sum_0^n b_n$ be two semi-convergent series, and let the first become absolutely convergent when its terms are associated into groups with some finite number of terms in each group. Let r_n represent the number of terms in the $(n+1)$th group, and let g_n represent the $(n+1)$th group embracing r_n terms. Let, moreover, a_{R_n} represent the first term in the group g_n, where $R_0 = 0$ and $R_n = r_0 + r_1 + r_2 + \ldots + r_{n-1}$, then

$$g_n = (a_{R_n} + a_{R_n+1} + a_{R_n+2} + \ldots + a_{R_n+r_n-1}) \quad \text{and} \quad U_n = \sum_0^n g_n.$$

Since, by a theorem of Mertens, the product of an absolutely convergent series and a semi-convergent series, formed