SOME CHARACTERISTIC FEATURES OF TWENTIETH CENTURY PURE MATHEMATICAL RESEARCH

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1. If I have undertaken to try and give some indications as to the nature of the progress of mathematical science in the first quarter of the present century, it is not because I underrate the difficulty of the task, or because I can hope, in such a résumé, to do justice to the remarkable work accomplished in this period by my brother mathematicians, even in the region of research with which I shall more especially deal. A lecture on so wide a theme constitutes to a certain extent a new departure, and, if I have ventured to attempt it, it is because my example may be followed in succeeding Congresses. Such an attempt at abstraction within a science which is concerned with abstractions, and with abstractions of abstractions, seems indeed peculiarly appropriate and the more necessary that, even after more than a quarter of a century of Congresses, we mathematicians still resemble the workers at the Tower of Babel, trying to construct one and the same edifice, but speaking different mathematical tongues.

I can only regret that the time at my disposal on this occasion has not permitted me to carry out the process to its proper conclusion and in a manner worthy of the theme.

2. Among the characteristic features of 20th century research are those bound up with the Encyclopaedic movement, inaugurated at the first Mathematical Congress in 1896, a movement which has done much to remedy the state of things just referred to. It has tempted workers to move over into neighbouring fields, and, by attempting to define the frontier of existing knowledge, it has given a tremendous impulse to mathematical research. But we have a right to expect even more. It should be the task of Encyclopaedic workers also to present the subject-matter in such a form as to reduce the material of knowledge to as few simple and easily understood principles, concepts and properties as possible, and the encouragement of efforts in this direction should be one of the aims of each succeeding Mathematical Congress.

3. Another feature of mathematical research in our century, equally furthered by the Encyclopaedic Movement, has been the interest shown in the application of Pure Mathematics to the Applied Sciences and to the Industries; an interest which has been felt in the Pure Mathematical camp itself as well as outside it. It is unnecessary to emphasize this point, for a mere glance at

the Programme of Sessions of Sections of the present Congress suffices to show how tangible has been this progress in the present century.

The whole attitude of the world of intellectuals towards mathematics has changed, and it is for the most part only within the ranks of the mathematicians themselves that any pessimism exists as to the future of Mathematical Science. Biologists and chemists, equally with physicists and engineers, claim the help of the mathematician, yea, clamour for it, while training in accurate mathematical thinking, as distinct from mathematical calculation, has been demanded from their intending pupils by some of the most eminent Historians and professors of Linguistic, not to speak of other more closely affiliated Arts subjects, such as Philosophy and Economics.

I do not mean by this that there are no survivals of the old order of things. For example, in England there has always been, and still is, a school of engineers, bitterly opposed to all but the rudiments of Pure Mathematics. But against such an isolated fact we may set the brilliant success of the French and Italian engineers, all of whom are trained on highly advanced mathematical lines, while at Zurich, even in the beginning of the century, Hurwitz, who was nothing if not a Pure Mathematician, was called on successfully by his engineering colleagues for the solution of an engineering problem involving a question of dynamical stability, a problem which he only succeeded in solving by employing well nigh all the resources of the Analysis of the day. We may in this connection refer to the problems which present themselves in Hydrodynamics, interesting, as they do, not only Hydraulic engineers but workers in the theory of Ballistic and Aeroplane theory. These problems, many of which are mainly three-dimensional, require a mathematical apparatus which we do not yet possess, owing to there not having yet been found a proper analogon in space for Conformal Representation in the plane.

And we may allude here to the difficulties, essentially of a mathematical character, connected with the Paradox of d'Alembert, where it has been usual to make certain assumptions with respect to the conditions satisfied at infinity, and to the yet wider problem of reconciling theory with practice in the investigation of the motion of liquids, where what is at issue appears to be the difference between regarding the viscosity σ as finite until after integration of the equations, and then making it approach zero, or putting it equal to zero before proceeding to integrate.

This is perhaps the place to lay stress on a circumstance which is not always realized. The question nowhere arises in Pure Mathematics whether there is anything in Nature corresponding even approximately to a mathematical concept. The discoveries of the Quantum Theory, hypotheses as to the nature of matter, the finiteness, if we regard that as demonstrated, of the Time-universe in its spatial aspect, do not and cannot render nugatory or valueless the concepts of differential coefficient, limit, integral and the like, though these employ the notion of infinity, any more than the fact that -1 does not possess a square root can be said to vitiate the applications to science of theorems obtained by means of the complex variable. Infinite integrals, Euclidian Geometry, lose none of their potency, none of their usefulness. The reason why they have come to be employed is that they facilitate mathematical reasoning, they are useful tools, enormously more useful than tools easier to construct. The Calculus of Finite Differences cannot possibly displace the Infinitesimal Calculus, whatever be the progress of the human mind. None of the arguments, therefore, used by the mere working engineer against Mathematics have any value whatever. At most they can be employed to discourage the attempt to give to a man who is not to be a chief, to a mere artisan, in fact, knowledge which he is, and always will be, incapable of appreciating.

4. Confining ourselves now more particularly to the advance of Pure Mathematics itself, one other tendency should be noted, not wholly unconnected with the Encyclopaedic Movement, though preceding it. In proportion as knowledge of mathematical theories has increased, the interest in purely formal work has diminished even in England, which may perhaps be said to have been its last refuge. It has begun to be understood all over the world that a mathematician is only a calculator when he must be. He is by nature a creator, a poet, not an artisan, an architect, not a mere builder. The interest in a result for its own sake has diminished accordingly; if it is obtained by the application of a known method, presents no particular beauty, or elucidates no new or difficult theory, if it does not appreciably add to the equipment of the human mind, it attracts little or no attention, and is fortunately not always accepted for publication. There is a tendency even to reduce the labour spent on mere problems for examination purposes in England and France, though it may be said *en passant* that these have in the past occasionally served the progress of learning.

In the main, then, *Theory not Theorems*, *Principles not Formulae*, are what matter. There is some hope now that even in the higher classes of schools, even of our less progressive English ones, the master will not content himself with setting examples and explaining how they are to be done, and that in this way school mathematics will repel fewer of those gifted minds to whom calculation for its own sake does not appeal.

If among Pure Mathematicians only a few have been able to follow closely the work of the Logical School, of which Peano is the most illustrious member, an increasingly large number have interested themselves in the profound examination, which appears to have taken its rise in Italy, of the Foundations and Axioms of Mathematics. Time, however, does not permit me to go further into this interesting subject at present. It is, however, inevitable that from time to time much energy should be spent in underpinning, where necessary, our mathematical edifice. This movement is, in fact, only a part of a larger movement which has consisted in careful scrutiny of the legitimacy of our fundamental analytical and geometrical processes.

5. One of the remarkable things about the work of the 20th century has been the way in which mathematicians have been led to go back to the work of the early 19th century. Cauchy's theorems have been utilized in all sorts of unexpected directions, notably in Theory of Numbers, as you will have heard in Professor Fueter's communication. In the Theory of Functions of a Complex Variable itself, for example in the Theory of Integral Functions, it has

proved a most potent weapon. Contour Integration also has now taken its place among the weapons of the Applied Mathematician.

Still more remarkable is the renewed activity centering round the idea of Group. The Theory of Relativity may be said to have taken its rise in the recognition of the fact that the Lorentz-Maxwell equations of Electro-magnetic Theory remained unaltered for a group of transformations in which the space and time variables were interchanged. Cartan's work involves the use of what might be called a Tangent Group in connection with Levi-Civitâ's theory of parallelism in n-dimensional space, the object being the application of the group theory to Riemannian space. Moreover Galois Theory, as applied to Algebraic Equations, has achieved notable triumphs in the domain of Differential in the hands of Drach and Vessiot.

6. In Geometry the passage from the 19th to the 20th century is marked by the closing of purely Algebraic Geometry and its resuscitation in the hands of the Analysts, while Pure Geometry as such may be said to have almost disappeared as a living independent entity. Simultaneously the step has been taken of passing from one to two dimensions, or rather from curves to surfaces. Severi has obtained notable results in a domain which had occupied the attention of Castelnuovo and Enriques in papers which marked an epoch; in Severi's work the most highly specialized analytical tools are employed.

Geometry has also developed by ultra-analytical means, by means of the concepts of the Theory of Sets of Points. The notion of Group here again plays a very prominent part, but I wish more particularly to refer not to Analysis Situs, properly so called, or the determination or the minimum number of hypotheses which permit of a geometrical entity possessing a particular property, I wish to say a few words on the definition of the terms *curve* and *surface* and the associated concepts of the *length of a curve* and the *area of a surface*. Minkowsky, in order to deal with the difficult notion of the area of surface, conceived the idea of defining it by means of the concept of volume. The corresponding definition for the length of a curve would be that obtained as follows: Describe round every point of a plane curve a circle of radius r, find the area of the part of the plane covered over by these circles, divide it by $2r$ and then make r approach zero, this should be the length of the curve.

I tested this definition many years ago and found that it led to the most paradoxical conclusions. It naturally suggested, however, a definition of a curve which my wife and I gave in our book on the Theory of Sets of Points. The conclusion is that a curve defined as we defined it will not in general have a length. This does not mean, however, that that definition is to be finally rejected as giving a generalization of the naïve notion of a curve. Obviously it would be more general than a Jordan curve, and would require to have a different cognomen attached.

7. Let us for a moment consider what are the chief features of a generalization. One of the most striking questions is precisely the retention or rejection of the previous name. The concept in its original form involved certain properties or characteristics, some of which in the generalization are lost necessarily, inevitably, while others are retained. According as one set or another of these

properties are dropped you will get different generalizations, and when there are two generalizations it may be a matter of taste which has the right to inherit the name, or whether both shall retain it and be distinguished by a second name added.

Sometimes, however, as in the case of fractional integration, in which, among the moderns, Pincherle has interested himself, no finality seems possible; a definition suitable for one kind of function does not do for another. Sometimes it will happen that a generalization is attempted, but fruitlessly, only because a property discovered later was not intuitively perceived. This was, for instance, the case with an attempt of Lebesgue's to deal with the repeated differential coefficients of integrals, where later on success crowned the efforts of Tonelli, Fubini and myself.

Let us now return to the consideration of curves and surfaces. As I pointed out a moment ago, the definition which defines curve by means of a two-dimensional and surface in terms of a three-dimensional entity is in itself legitimate as a mode of generalization of our intuitive notions, though it sacrifices the property of possessing a length and an area. In dealing with length we have to take a definition of curve involving order, and, as I remarked in a communication to the Congress at Strasbourg, the step from curves to surfaces naturally involves the definition of surface in terms of double order. It is the ignoring of this fact that led to the shipwreck of Minkowski's definition of area, and also that of Lebesgue, which, though quite different, equally lacked the notion of order.

While on the subject of curves and surfaces I am tempted to refer to the work of Janiskewski, cut off in the prime of life. Had he lived, the quality of the small quantity of work he has left, suggests he would have done much to extend our knowledge of curves in space, and the limits of curves other than Jordan curves. But neither he, nor others who have been working in this domain, appear to have exhausted the subject of curves, while very little indeed is known of the topology of highly generalized surfaces, other than results already classical. Difficulties arise even at the threshold, when, for example, we seek to generalize for higher space the conditions under which the ordinary formula for a volume holds good. The word *topology* at once suggests the great name of Henri Poincaré, still better known for his researches in other domains and at an earlier date, but who was active to the middle of our period.

I should like also here to refer to Bianchi's great work in Differential Geometry, continued right through the century, and to the very interesting work of Fubini in the previously imperfectly exploited domain of Projective Differential Geometry. The former is careful to limit to a minimum the hypotheses required in his theorems. Bianchi belongs in this way to the modern school of thought. Fubini, on the other hand, illustrates the tendency nowadays of Geometry to merge into Analysis.

The idea even of a Jordan curve was by no means a clear one at the end of the last century, and has in the course of the present twenty-four years been considerably cleared up. Nevertheless we have to be careful in glancing over the literature not to fall into misconceptions owing to the fact that, though

Jordan himself defined a curve merely as the locus of points (x, y) whose co-ordinates x and y are continuous functions of a continuous variable t, or in more geometrical language points which are in continuous correspondence with the points of a straight line, some writers use the expression "Jordan curve" where others use "simple Jordan curve" when the correspondence is (1, 1), so that the curve has no multiple points.

Already at the end of the 19th century it had been perceived that without some such restriction the locus might fill up the whole interior of a square, and could not, therefore, without offending our intuition, be called a curve at all. This was due to Peano, and was followed by discussion by different authors, including E. H. Moore, of curves which, without themselves filling up any region, took up so much room in the plane that they must be regarded as possessing positive area.

The question then arose whether the boundary of a simply connected region was always a Jordan curve, and after Osgood had shown that this was certainly not the case, and several proofs, none absolutely satisfactory, had been given of Jordan's theorem that a simple Jordan curve divides the plane into two parts, the converse of this theorem was found by Schoenflies, who gave conditions under which the boundary of a simply connected region is a simple Jordan curve. Finally in this connection my wife and I gave in a note in the Comptes Rendus a discussion and a classification of the different kinds of singular points which the boundary of a simply connected region may present, and whose appearance may prevent the boundary from having the property characteristic of a simple Jordan curve. Such a singular point in its most exaggerated form is what we called a "sticklepoint," which is such that the points of the region and of its complementary region are packed so close together that they stick out of the singular point much as the quills of a porcupine do out of its body, no angle, however small, being free of points of both regions.

8. The consideration of what would have been called at the beginning of the century "funny curves and surfaces" leads up to and is closely connected with the conception of the nature of a continuous function which was almost completely foreign to the 19th century, and this, in spite of the fact that Bolzano, Weierstrass and Cellerier had devised functions which either did not, or all but did not, possess differential coefficients anywhere.

And when we pass from continuous to discontinuous functions we have evidence of a perfectly amazing state of ignorance twenty-five years ago. Baire has told me that his statement made to eminent mathematicians of that day of there being a distinction between continuity with respect to each of two variables separately and continuity with respect to the pair of variables was received with absolute incredulity. The idea of a function as being defined by a (1, 1)-correspondence between a stretch on the x-axis and a stretch on the y-axis, or, more generally, of an ideal table such that it gave corresponding to each value of x one, and only one, value of y, was barely grasped, and, when grasped, not utilized.

Yet for the discussion of such functions the ideas of George Cantor were available.

We now know, as I pointed out at the Congress at Rome, that there is complete symmetry at every point as regards the limits on the left and on the right of a point in the case of a function of a single variable x, except at a countable set of points. In the case of a function of two variables there is what my wife and I have called *complete crystalline symmetry* except at a set of points lying on a countably infinite set of monotone curves, with a similar statement for higher space.

By the expression "complete crystalline symmetry" we mean that all possible limiting values of the function in the neighbourhood of the point can be obtained by passing along sequences tangent at the point to a given direction arbitrarily chosen, and it must be borne in mind that the exceptional set is independent of the choice of that direction.

9. The question of the limiting values of $f(x)$ at a point suggests the further question of the differential properties of the function. That a continuous function need not have a differential coefficient even at a single point of the interval considered was known, and the concept of differential coefficient had been already replaced by the manifold of derivates. Isolated properties of derivates were to be found in the works of Du Bois Reymond, Dini and Scheeffer. It remained for our century to show that a certain symmetry exists also with respect to the derivates of a function. It is only at points forming a set of content zero that we do not have either a finite differential coefficient, or else both upper derivates positively infinite and both lower derivates negatively infinite. An infinite differential coefficient is only possible at a set of content zero. Moreover it has been shown that these results, which are due to Lusin, Denjoy and my wife, and are to be found in the newest treatise on the real variable by Professor Hobson of Cambridge, are the same whether the function to be derived be continuous or not.

10. If it be asked what possible applications there can possibly be of these considerations outside the realm of abstract thought, I am tempted to reply to you in the words of Jean Perrin, describing the dance of the golden molecules in an emulsion as seen through the ultra-microscope:

"The tangles of the trajectory are so numerous and so rapid that it is impossible to follow them, and the trajectory recorded is infinitely more simple and shorter than the real path. Again the mean apparent velocity of a grain in a given time varies madly in magnitude and in direction without tending towards a definite limit when the time of observation is diminished. We see this in a simple way by noting the positions of a grain on a bright background minute by minute, then, for example, every 5 seconds, and, still better, by photographing every twentieth of a second, as has been done by Victor Henri, Conandon or de Broglie, so as to cinematograph the motion. In the same way it is quite impossible to fix a tangent, even approximately, at any point of the trajectory. This is a case where it is really natural to think of those continuous functions which have no differential coefficients, imagined by mathematicians, and regarded quite wrongly as simple mathematical curiosities. Nature suggests them just as much as she does the functions which have differential coefficients."

11. But the obtaining of these results far transcends the powers of Analysis, using the term even in the most modern sense consistent with the exclusion of the Theory of Sets of Points (*théorie des ensembles*). In this theory we have an instrument far more delicate than any analytical machinery, and available when all such machinery fails. The first earnest of its power was the utilization by Hurwitz of the non-countability of the continuum in proving a theorem in Analysis, and this half a century ago. But it has been reserved for the twentieth century to present it in all its power and to show the far-reaching purposes to which it can be put. The school of French mathematicians at the turn of the century may justly claim to have given the first impulse. Baire in his discussion of discontinuous functions, Borel in his definition of the content of an open set, and Lebesgue in his adaptation of this notion to the service of the theory of integration. But the work has proceeded far beyond the point at which they left it, and the Theory of Functions of a Real Variable which has emerged has now a beauty and a fascination and a certain completeness of its own which is continually drawing to it fresh votaries.

Moreover the influence of this theory is felt in every branch of Analysis; it has given an impulse even to the Theory of Functions of a Complex Variable; it has revolutionized the Calculus of Variations and created the new Theory of Functionals. It has also created a new Geometry, while at the same time it has served to curb our intuition, by letting us perceive infinite possibilities which no intuition could grasp.

At the same time, by enabling us, for example, to visualize a plurality of limits, it has rendered it possible for us to simplify and correct old and abandoned attempts at demonstration in Analysis, while it has at the same time introduced the profoundest changes into its language.

12. But the most momentous step of all is the extension of the notion of integration. The work of Riemann, though epoch-making, had left the theory of integration in a *cul-de-sac*. The definition of an integral that Riemann found before him was in terms of a summation, in other words of an analytical expression, which was shown to have an unique limit when the function to be integrated had a form prescribed beforehand. The step taken by Riemann was equivalent to the securing that a function had an integral whenever this analytical expression had an unique limit, and he expressed in an obscure, not easily understood, form the condition that this should be the case. In the language of the present day this condition is equivalent to the requirement that the points of discontinuity of the function, which in general form an open set, should form a set of zero content.

Darboux gave a precise and easily intelligible form to Riemann's definition, and virtually obtained, without being aware of it, the Lebesgue integral of functions now recognizable as the upper and lower semi-continuous functions of Baire. These *integrals by excess and by defect* formed the starting-point of my own work on the subject. In the meanwhile Lebesgue had published his well-known *thèse* in which he introduced the integral which, if I am not mistaken, I was the first to call the *Lebesgue integral*; and I am sure that no one here present will grudge to the distinguished French mathematician, whose absence we deplore, the honours that have accrued to him from his discovery, and the consequences that he drew from it by his bold employment of transfinite numbers. The qualifying cognomen, however, as characterizing the most general absolutely convergent integral when the integrator is a single variable or set of variables, is doomed to disappear by reason of the very inevitableness and naturalness of the generalization, as we now see it, and the finality with which its introduction has closed the chain of such operations in the bounded field. Indeed, every bounded converging succession of functions, whatever be the nature of the limiting function, with the single obvious limitation that the functions are mathematically definable, is integrable term-by-term with the new definition.

This important theorem of Lebesgue's has enormously simplified modern analysis. As an immediate corollary we see that a bounded differential coefficient may be integrated in the new sense, and that its integral is the primitive function.

13. The definition of the new integral given by Lebesgue himself is open to certain objections of a pedagogic nature, and the same reproach may be made to the first of the definitions given by myself. Both of these involve as a basis the notion of the content of a set of points. Moreover a one-dimensional integral is the content of a plane set. I ought here to remind my hearers that I have always found it convenient to drop the term *measure* employed by Lebesgue and Borel for open sets, and to speak of the *content* whether the set be open or closed.

Now this concept of content requires for its justification an existence theorem which is precisely the same as that subsequently required in the case of the integral. The content of a set of points is in point of fact the integral of a function which is unity at all the points of the set and zero elsewhere.

The definition which I now usually employ, which not only enables us to establish the whole theory most easily and concisely, but also leads to the extension to integration with respect to a function of bounded variation, and this, whatever the number of independent variables, is based on the method of monotone sequences. For this purpose I classify all functions mathematically definable beginning with functions which are constant in each of a finite number of stretches, that is, intervals, filling up the whole segment which is the range of the independent variable. The values of the function at the points of division, that is the end points of the intervals, are conveniently chosen to be in the one case always greater than, and in the other case always less than, the values at the neighbouring points on each side. Such functions are a particular case of the *upper and lower semi-continuous functions* invented by Baire, and they generate the general upper and lower semi-continuous functions as limits of monotone sequences of these simple functions. It is unnecessary to weary you with the complete system of classification of which this is the commencement. We are able to show that, for the purpose of the integration of bounded functions, we need not go beyond functions which I have called *ul* and *lu* functions, an *ul*-function being defined as the limit of a monotone decreasing sequence of lower-semi-continuous functions, and a *lu*-function as the limit of a monotone ascending sequence of upper semi-continuous functions. In accordance with the definition of absolutely convergent integral, one more monotone sequence is in the first instance required in the case of an unbounded function; this complication is, however, removable, and we have only to define the integrals of bounded and unbounded (positive) *ul* and *lu*-functions.

This is immediate, as soon as the theorem of consistency has been proved. Beyond these functions we do not need to go, for we easily show that *any bounded function mathematically definable, and any unbounded function possessing a Lebesgue integral, can be enclosed between a lu and an ul function both having he same integral, which these accordingly share with the function.*

14. The advantages of the method of monotone sequences are not merely methodic, two may be mentioned.

The first is that, given a function possessing a Lebesgue integral, we see intuitively that a function of almost any convenient simple type can be found which is such that its integral differs from that of the given function by a quantity as small as we please, or, which is the same, such that the integral of the difference of the two functions is as small as we please. The auxiliary function may, for instance, be constant in stretches, or it may be continuous.

Secondly, it is equally intuitive that we can find two functions, one upper semi-continuous and not greater than the given function, the other lower semi-continuous and not less than the given function, whose integrals differ by a quantity as small as we please.

By means of the first of these properties we are able to prove in a line almost all the principal theorems with regard to the integration of oscillating successions of functions. From the second of these properties we obtain, again almost intuitively, all the results obtained by Lebesgue by the use of transfinite numbers. The integrals of these two auxiliary semi-continuous functions are indeed effectively identical with the *fonctions majorante et minorante* of M. de la Vallée Poussin.

These proofs hold almost word for word when the integration is made with respect to a positive and completely monotone increasing function; hence, a function of bounded variation being defined as the difference of two such functions, it follows that the whole theory holds for integration with respect to a function of bounded variation, and this whatever may be the number of independent variables.

15. I have dwelt for a moment on this mode of presentation instead of attempting to give an account of the rival methods of M. Lebesgue, M. de la Vallée Poussin and others, because these latter are still much better known, as having formed the subject of lectures which have appeared in book form.

The theory of content then appears as a special case of the theory of integration, capable of being treated simultaneously with the latter, or, if desired, before it in point of time, but not as a logical preliminary to the general theory. On the other hand, it is inevitable that, the theory once established, my first form of definition, adopted by Signor Tonelli in his very interesting researches in the Calculus of Variations, should have its place, as a new theorem in the theory to be used, whenever found desirable.

The idea of attaching a number to a set of points, a number not necessarily the content of the set, arises also naturally, and we thus come to the great and growing Theory of Functionals, which took its earliest impulse, if I am not mistaken, in the work of Pincherle, and is now being prosecuted on somewhat different lines by M. Frechet and others. M. Frechet's researches are interesting from another point of view, as involving the concept of sets of elements which have not the property that every infinite set of elements has a limiting element, a concept which appears in embryo in the writings of Arzelà.

16. Before passing to the application of the Lebesgue integral to the field of trigonometrical series, which was the point of departure of Riemann in his treatment of integrals, it will be desirable to occupy ourselves with another branch of modern research, that of the theory of series.

Euler's bold treatment of series without any regard to their convergence led in his skilful hands to correct results. He was followed by not a few, among others by applied mathematicians. In modern times the attitude of the electrician, Oliver Heaviside, was remarkable; he was frankly antagonistic to the use of convergent series, and he used frequently to say, when he came to a divergent series: "This series is fortunately divergent, and so its treatment will be simple." Physicists for the rest have troubled little about the convergence of their series, claiming that Mathematics is merely a convenient tool, and that they could be certain that any results they obtained by means of it were correct, provided they only kept the physical interpretation before their minds.

One of the characteristic features of 20th century research has been the recognition of the underlying mathematical principles responsible for the correctness of the result when the method of proof employed was apparently fallacious. This, of course, must not be taken to mean that all the results of earlier writers obtained by incorrect reasoning on series are right. It has happened frequently enough in the case of inferior mathematicians that the results were false. But the attitude of Heaviside has been justified. It is sometimes convenient to transform a convergent series into a divergent one, because the sum of the latter can be more easily obtained. To the word *sum* we must now, however, attach a generalized meaning. We are here in fact on the brink of far-reaching generalizations. One of the greatest impulses to work of this kind was given by the Italian mathematician, Cesaro. Instead of defining the sum of a series as the limit of the sum s_n of n terms, we may define it, following Cesaro, as the limit of the arithmetic mean S_n of the sum $s_1, s_2, \ldots, s_n$. If S_n does not have an unique limit, we may repeat the process on the S's, and so on as far as we please. And the theorem of consistency holds good. This idea, essentially that of Cesàro, has been modified in a number of ways, and particularly in such a manner as to yield continuously varying methods of summation. We, of course, get in this way $\frac{1}{2}$ as the sum of the series $1-1+1- \ldots$, which is Euler's result. There are, of course, divergent series to which this method will not apply, but which yield to analogous methods of procedure.

17. One of the most remarkable cases where Cesaro summation is effective is in the theory of Trigonometrical series, when the coefficients increase like a power of n. We now pass, therefore, to the application of what we have been

saying to such series and to the utilization of the Lebesgue integral in that theory. This is the more desirable because we shall be able in this way to illustrate the striking progress effected in Analysis by the introduction of the Lebesgue integral.

Fourier was led specially to consider those harmonic trigonometrical series

$$\sum_{n=1}^{\infty} \{a_n \cos nx + b_n \sin nx\}$$

whose coefficients are expressible as integrals in what we call *the Fourier form*, involving a function f, called *the function associated with the series*. It is evident that the generalization of the concept of integration automatically enlarges the class of such series, and it may be added that the necessary and sufficient condition that the sum of the squares of these coefficients should converge is that the square of $f(x)$ should be integrable in the sense of Lebesgue. The sum of the series is then, to a constant factor *près*, the integral of the square of $f(x)$ over the interval of periodicity. This theorem, known as the Theorem of Parseval and its converse, was one of the first great triumphs of the Lebesgue integral.

The theorem is the more remarkable in view of results in the same order of ideas obtained by myself later. Although various attempts made to generalize this theorem had failed, it was possible, I found, to generalize both halves of the theorem, but only in such a manner as to make them not converses of one another. It is a *sufficient* condition for the series

$$\Sigma\{|a_n|^{1+p} + |b_n|^{1+p}\}$$

to converge, that the integral $\int |f(x)|\}^{1+1/p}\, dx$ exist, p being an integer. The the sum of series is then expressible as a repeated integral involving $f(x)$. But this condition is not *necessary*.

On the other hand, the convergence of the series

$$\Sigma\{|a_n|^{1+1/p} + |b_n|^{1+1/p}\}$$

is a *sufficient* condition that the integral $\int |f(x)|^{1+p} dx$ should exist in the Lebesgue sense, but this condition is not *necessary*.

I obtained these results in the first instance by the method of multiplication of Fourier constants, and then, for greater security, devised a method by inequalities. Quite recently Hausdorf, employing the latter method, and using my inequalities, has succeeded in removing the limitation that p should be an integer, and I see that a joint pupil of Hardy and my own alludes to the completed theorem in an abstract of a paper recently presented to the L.M.S. as the Theorem of Hausdorf. In a recent number of the Fortschritte, however, it is called the Theorem of Young-Hausdorf.

§18. Many years back I gave as my reason for working at the details of the theory of Fourier Series my conviction that this was the best way of approaching the more general theory of series of Normal Functions, a conviction which has been justified since. I have been interested to see that in a paper still more recent than that of Hausdorf, F. Riesz has now succeeded in proving the whole theorem for series of Normal Functions.

19. We come now to the utilization of the concept of Cesaro convergence, and I give at once one of the most striking theorems, namely, that *the well-known conditions for the convergence in an interval inside the interval of periodicity are the same for the nth derived series of a Fourier series as for the Fourier series, provided only we sum the series (Cn) and regard as the associated function of the derived series in the interval in question the nth differential coefficient of the function associated with the Fourier series.*

20. In the prosecution of these researches I was led to a still further generalization of Fourier series, which justifies once more the introduction of the Lebesgue integral. I had been led to call the derived series of a Fourier series "Restricted Fourier Series," but I have since found it more convenient to use the term somewhat differently, in such a manner as to emphasize the fact that the Trigonometrical series about to be described involve a generalization of Fourier series of a marked character, such as cannot be secured by any further extension of the concept of integral.

The matter will be best understood if I revert to the fundamental properties of Fourier series as discussed by Riemann. He showed that, if the integrals in terms of which the coefficients are expressed are integrals in accordance with his definition, these coefficients a_n and b_n converge to zero. The first great step in the generalized theory rendered possible by the use of the Lebesgue integral was taken by Lebesgue, when he proved that this property of the coefficients holds good. This is indeed obvious in the light of one of the intuitive consequences of our definition of the integral by the method of Monotone Sequences.

It is also a necessary and sufficient condition for a trigonometrical series to be a Fourier Series, that the integrated series

$$\sum\left\{\frac{a_n}{n}\sin nx - \frac{b_n}{n}\cos nx\right\}$$

should converge to a Lebesgue integral throughout the whole closed interval of periodicity, so that the first of these statements follows from the second.

Now suppose we assume a given trigonometrical series to converge to an integral only in an interval, or a set of intervals, inside the interval of periodicity, so that the coefficients are no longer expressible in the Fourier form, and the condition as to the convergence of the coefficients to zero no longer holds good of itself. Suppose, however, we make the further hypothesis that this last condition holds good, we then have what I call *an ordinary Restricted Fourier Series*, or *R-F-series*. Such a series I have found to possess all the properties of a Fourier series which do not directly or indirectly involve the expression of the coefficients in the Fourier form.

We are thus able in particular to prove that if, and only if, the coefficients converge to zero, do trigonometrical series having the Fourier form, with the limitation that the integrals are improper integrals, behave like ordinary Fourier series; and we deduce without any difficulty the order of Cesaro convergence needed in the contrary case to take the place of ordinary convergence.

21. Before leaving the subject of the definition of the Lebesgue integral of which we have just shown the importance from the point of view of Fourier series, I ought I think, to make an analogy which inevitably suggests itself to anyone who has worked seriously with the concept. The introduction of the Lebesgue integral involves a revolution in Analysis. It marks a great epoch. The gain can only be compared with that secured at each successive stage in the generalization of the concept of number. Each stage involved an economy of labour. We were enabled to perform our calculations without inquiring whether particular restrictions were effectively satisfied. So in the present instance we have no longer to inquire, for example, whether a series of continuous functions which converges boundedly has for sum a function possessing an integral according to Riemann, in order to be able formally to integrate it term-by-term. We know it must have an integral according to Lebesgue and that is sufficient for our purpose. With suitable modification the same is true for a series of positive functions which converges except at a set of content zero. And we may remark that, in this connection, *convergence except at a set of content zero*, takes the place of *convergence everywhere* in the older theory.

22. Finally, as a further example of the usefulness of the Theory of Sets of Points and of Cesaro Convergence in the theory of Fourier Series in the Lebesgue sense, we may quote the necessary and sufficient condition that a trigonometrical series should be a Fourier series, and the necessary and sufficient condition that it should be the Fourier series of a function of which a given power, the $(1+p)-$th, is known to be summable, conditions applicable also to R-F-series. The former condition is that $\int_E f_n(x)\,dx$ tends doubly to zero, where $f_n(x)$ denotes the nth Cesaro partial summation, and integration is over any set of points E, whose content, as well as the index n, tend to zero. The latter condition is that $\int |f_n(x)|^{1+p}dx$ should be bounded.

23. It will be noticed that, in our account so far of the modern theory of integration, we have given no place to non-absolutely convergent integrals. In other words, we have confined our definition, following Lebesgue, to functions which remain integrable according to the definition when they are made everywhere positive by changing the sign, where necessary. This is the case in particular for all bounded functions (mathematically definable), but in general ceases to be the case when the function is unbounded.

In this more general case great progress has been made, when the integration is with respect to a single variable. The generalization has, in fact, proceeded so far as to involve the loss of some of the more familiar properties of an integral. Here the name of Denjoy stands out, though some of us have gone still farther. Every finite differential coefficient possesses an integral according to Denjoy, and his integral retains some of the more elementary properties of ordinary integrals.

Practically nothing, however, has been done when the integration is to be taken with respect to several variables. This suggests a large unexplored field of research. It appears to be a necessary preliminary to such researches that further progress should be made in the theory of plane sets of points, to say nothing of sets in higher space, sets such as those referred to previously, which lie on a countably infinite number of monotone curves, appearing to form one of a chain of entities to be taken into consideration.

Needless to say change of order of integration is not, as in the case of a Lebesgue integral, usually allowable, and the investigation of sufficient conditions is one of the first desiderata. In the same connection the problem arises to determine when a double non-absolutely-convergent integral is obtainable by repeated integration.

24. We have not yet referred to the advance made in the theory of integrals with infinite limits; methods analogous to those of Cesaro for the treatment of series have been shown to be applicable to divergent integrals. This was *a priori* assured, but the details of the theory are very interesting, and it still needs systematic elaboration and presentation.

25. I cannot even give a passing mention of the progress made in the setting forth of rules under which the processes of differentiation and of proceeding to the limit under the integral sign are allowable. The movement here has had an immense influence on the handling of Contour Integrals in the Theory of Functions of a Complex Variable. Nor can I do more than refer to the immense progress made in the Theory of Functions of two or more complex variables, alike from the older and the more modern standpoints.

My time is at an end, I am conscious, as I close, of the extremely imperfect way in which I have been able to carry out my original intention. I can only ask you to accept this imperfect attempt as an earnest of better to come.

COMMUNICATIONS

SECTION I

ALGEBRA, THEORY OF NUMBERS, ANALYSIS