FIFTY YEARS OF AMERICAN MATHEMATICS

BY

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It is indeed a great honor to participate in this Semicentennial Celebration of the founding of the New York Mathematical Society in 1888, which became in 1894 the American Mathematical Society. As one of the speakers I have set myself the challenging task of tracing our mathematical development under the auspices of the Society during the years which have passed. Obviously in such a *coup d'oeil* only the principal factors involved can be alluded to, and the point of view adopted must necessarily be more or less personal.

At the very outset it is well to recall the general mathematical background of our country at the time when the Society came into existence. In colonial days scientific and mathematical knowledge had a certain definite standing, largely for its practical value but in part also for its own sake. George Washington was a scientifically-minded gentleman farmer for much of his life, and in his youth was a skilled surveyor, familiar with trigonometry; Benjamin Franklin discovered experimentally the electrical nature of the lightning discharge, theorized concerning electricity as a fluid, and had enough mathematical interest to devise ingenious magic squares; Thomas Jefferson regarded geometry and trigonometry as "most valuable to every man," algebra and logarithms as "often of value," while he classed "conic sections, curves of the higher orders, perhaps even spherical trigonometry, algebraic operations beyond the 2d dimension, and fluxions" as a "delicious luxury"; in his later years Jefferson spent much of his time in mathematical reading, and was ever a true friend of mathematics. The interest in science and mathematics continued to be genteel and amateurish among American scholars and devotees until towards the middle of the last century, with few notable exceptions. The best mathematicians of those days looked appreciatively toward Europe without much thought of high emulation.

Then came a gradual change in the temper of the times which led to the formation of our Society. Characteristic of this change were the outstanding figures of Benjamin Peirce, of Josiah Willard Gibbs, and of George William Hill. Peirce died in 1880, Gibbs in 1903, and Hill in 1913, having been fourth president of the Society in the years 1894 to 1896. But it was the contagious enthusiasm of a group of young Americans, returning from mathematical studies in Europe, which proved the immediate cause of the formation of our Society; and in this so important enterprise Thomas Scott Fiske, seventh president of the Society, and Frank Nelson Cole, long its devoted secretary, took leading parts. The year 1888 of our beginning as a professional body devoted to the interests of research, marks with precision our coming to a fitting mathematical position among the nations of the earth.

Of the three figures mentioned it was Benjamin Peirce who was by far the most influential in America as a scientific personage. I remember a talk about Peirce with his last pupil, the late Dr. Leonard Waldo, mathematical meteorologist. Waldo said that the first sight of Peirce seated behind his desk at home rendered him quite speechless. Ex-President A. Lawrence Lowell of Harvard University fell under Peirce's mathematical spell while an undergraduate and wrote a few years ago: "Looking back over the space of fifty years since I entered Harvard College, Benjamin Peirce still impresses me as the most massive intellect with which I have ever come into close contact, and as being the most profoundly inspiring teacher that I ever had. His personal appearance, his powerful frame and his majestic head seemed in harmony with his brain."

Benjamin Peirce's papers on *Linear associative algebra*, announced at the first meeting of the American Association for the Advancement of Science in 1864, give him a just claim to be considered an eminent mathematician. His researches in this field were made at a time when English and American mathematicians looked upon the great invention of quaternions by W. R. Hamilton as a supreme achievement, destined to be of incalculable importance for mathematics and physics. Peirce saw more deeply into the essence of quaternions than his contemporaries, and so was able to take a higher, more abstract point of view, which was algebraic rather than geometric. However, he was much more than an algebraist, for he was well informed about some of the most significant mathematical developments of his day. His volumes *Curves, Functions, and Forces* testify to a real interest in the function-theoretic work of Cauchy, albeit somewhat superficial in character. His large book *Analytical Mechanics* showed that he had read and mastered the works of Hamilton, Jacobi, and others in the extensive field of dynamics. In addition, he was skilled in the theory and methods of computation useful for dynamical astronomy, and spent a considerable amount of time during later years in a somewhat unhappy attempt to show that Leverrier and Adams had no adequate basis for the calculations leading to the celebrated discovery of the planet Neptune; one naturally calls to mind the calculations by the eminent astronomer, the late Percival Lowell (brother of A. Lawrence Lowell), which brought about the discovery of the small planet Pluto in 1930, since these calculations have also been occasionally criticized for similar reasons.

Despite Peirce's remarkable ability to inspire especially capable and advanced students, he was not regarded as a good teacher for the rank and file; a characteristic feature of his lectures was a reaching toward seemingly endless vistas of abstract generalizations.

Josiah Willard Gibbs was a man of modest and not especially impressive personality, who did far more to advance physics and chemistry through his work on statistical mechanics and the equilibria of chemical systems than Peirce ever did for pure mathematics. Gibbs' title to be considered a mathematician rests mainly upon his largely notational contributions to vector analysis, a subject also closely related to Hamilton's quaternions. The late Maxime Bôcher, who with William Fogg Osgood really succeeded Peirce at Harvard, later attached the name of "Gibbs' phenomenon" to a fundamental fact about Fourier's series which was observed by Gibbs; this is related to the peculiar behavior of the successive curves of approximation $y=s_n(x)$ to a discontinuous function near the point of discontinuity. As has happened from time to time here and elsewhere, the fundamental contribution of Gibbs' physical work was first recognized by admirers in other countries, in particular by James Clark Maxwell, so that it was only somewhat tardily, by reflected light as it were, that Gibbs came to be properly appreciated in the United States.

George William Hill was a scientific figure of altogether unconventional type who spent more than three decades of his life as an assistant in the Nautical Almanac office in Washington and then went back to the place of his birth, West Nyack, New York, to continue his researches. Hill, like Gibbs, never married. His life was devoted to what were essentially mathematical studies of the solutions of the three-body problem useful to the lunar theory and in making specific astronomical computations. His work on periodic motions was the worthy forerunner and inspiration of the splendid theoretical advances of Henri Poincaré in celestial mechanics, who thus owed much to Hill's achievements. The free introduction of infinite determinants by Hill in his celebrated papers on the restricted problem of three bodies was especially noteworthy, although it is only recently that this interesting analytic instrument has been perfected.

Of these men, Hill would be claimed for themselves by the theoretical astronomers, along with Nathaniel Bowditch, translator and commentator of Laplace's *Mécanique Céleste*, and Simon Newcomb, great perfecter of lunar and planetary theory; while Gibbs would be justly taken by physicists and chemists for their own. Thus there remains to the mathematicians of America only Benjamin Peirce for their undisputed possession. He appears as a kind of father of pure mathematics in our country. In his deep appreciation of the elegant and abstract we may recognize a continuing characteristic of American mathematics. In his concern with its many applications there resides a virtue which we are finding it more difficult to realize, because of the trend towards professional specialization. Without doubt, however, there is a spiritual necessity upon us today to regain a similar breadth of outlook.

Any account, however brief, of American mathematics before 1888 must chronicle an event of the preceding decade which was of extraordinary importance not only to mathematics, but to the whole field of scholarly endeavor, namely, the foundation of the Johns Hopkins University at Baltimore in 1876. Although the Graduate Schools of Yale University (1847) and of Harvard University (1872) were in existence, nevertheless as has been said by Dr. Abraham Flexner in his book *Universities: American, English, German*, the Johns Hopkins University was the first American institution "consciously devoted to the pursuit of knowledge, the solution of problems, the critical appreciation of achievement, and the training of men at a really high level." Thus there was called to the new mathematical department at Baltimore the great English algebraist, James Joseph Sylvester, who remained there until 1884. Under the direction of the department there began in 1878, the American Journal of Mathematics, our first journal given over to mathematical research, and now completing its sixtieth year of high achievement. Ever since, there has continued to be at Baltimore, despite material limitations, an important center of mathematical activity, of which the staunch and kindly remembered British geometer, Frank Morley, was the titular leader from 1900 to 1928.

In all previous mathematical history perhaps no mathematical development in any country has been so extensive and rapid as that which ensued here upon the founding of the Society. All the great nations of Europe had produced illustrious mathematicians of whom they had the right to be extremely proud. The French and German mathematical traditions were particularly well established and of incomparable brilliancy, represented at that moment by Henri Poincaré, the young David Hilbert, and a number of other figures of very high rank. Italy and the Scandinavian countries were also flourishing vigorously. Yet up to that time there had scarcely arisen any occasion for European mathematicians to note the work of their American colleagues. A solitary exception was the early recognition of Hill's lunar theory by Poincaré, while the algebraic advances of Peirce failed to receive the attention which they deserved.

But now able young mathematicians, fresh from studies abroad, began to carry on vigorous and independent research at home, and their contagious enthusiasm soon aroused a deep interest in the younger men around them. Almost over night as it were, the great University of Chicago sprang into existence in 1892, with a mathematical department made up of Eliakim Hastings Moore, Oskar Bolza and Heinrich Maschke from Germany, and others. Of these men, only Bolza is living today. They formed a notable and inspiring group which will ever be remembered in our mathematical annals. At about the same time Osgood and Bôcher, inspired by their German sojourn and in particular by the great Felix Klein of Göttingen, bent their every effort to strengthen the tradition at Harvard. Under the genial leadership of Moore at Chicago, who had studied with Gibbs at Yale University and for a year in Berlin, there was emphasized the abstract and algebraic side of mathematics, although Moore was remarkably catholic in his outlook. At Harvard attention was turned towards the vast field of analysis. The center in Cambridge was much strengthened by the transference of the Massachusetts Institute of Technology from Boston across the Charles River in 1916. Its mathematical group and that at Harvard University have been in close and mutually stimulating association since that date.

A few years later, under the wise and benevolent guidance of Dean Henry Burchard Fine, who had been strongly influenced by his studies under Leopold Kronecker, promising young men were called to the mathematical staff at Princeton, in particular L. P. Eisenhart, Oswald Veblen, and J. H. M. Wedderburn. From that day forth there has always been an important mathematical group at Princeton. There came a notable further impetus with the founding there in 1930 of the Institute for Advanced Study, with Abraham Flexner as Director. At the outset the new Institute devoted its attention to the fields of mathematics and theoretical physics, calling at first Albert Einstein, Veblen, and Hermann Weyl to ideal research posts. Up to the present time the mathematical staff of the Institute has worked side by side with the staff of the University in Fine Memorial Hall. The others at the Institute have in general already obtained their doctor's degree and come to enjoy a period of uninterrupted study and research under favorable conditions. The Institute is fortunately able not only to augment its staff through distinguished temporary appointments, but also to give partial financial support to many of those who come to study at the Institute.

By great good fortune I have been intimately associated with the centers at Chicago, Harvard, and Princeton. I feel deeply indebted to them all. Indeed there are not many American mathematicians who have not been profoundly influenced by one or another of these three groups. It was in the spring of 1902 that I made a first journey across the city of Chicago to the University, and found my way into the excellent mathematical library in Ryerson Physical Laboratory; before that time I had only been in contact with the mathematical books of the John Crerar Library and the small mathematical collection at Lewis Institute. I remember the thrill which the sight of the well-filled shelves gave me. Soon I met Professor Moore of whom I had already heard, and found him then and always extraordinarily inspiring, suggestive, and kind. During my first (Junior) year at the University I profited much from my contact with Bolza also. At his suggestion I read the work of Gauss on the cyclotomic equation and the equally celebrated paper of Abel on the impossibility of solving the general quintic by radicals. Bolza's lectures were marvels of clarity and finish. But it was Moore above all who seemed to me to have the true fire of genius within him.

The year following I went to Harvard, with Moore's approval, for two years of study. There I learned more analysis, particularly from Osgood and Bôcher. I found Bôcher's lectures the equal of Bolza's in lucidity and superior in placing important points in high relief. It was only later, however, that I came to realize how much I owed to Bôcher for his suggestions, for his remarkable critical insight, and for his unfailing interest in the often crude mathematical ideas which I presented.

On my return to Chicago in the fall of 1905, I profited greatly by two further years of work with Moore, both in his seminar on analysis and outside the class room. Moore was a deep admirer of Hilbert and was then following closely the rapid developments at Göttingen, attendant upon Fredholm's fundamental work in linear integral equations of 1900. It happened that I saw Moore's program of General Analysis taking shape day by day, as he came to appreciate the full abstract significance of the papers of Hilbert and the beautiful dissertation of Erhard Schmidt.

In 1907 I started teaching at the University of Wisconsin and in my two years there I especially valued my scientific and personal relationship with my senior colleague, Edward Burr Van Vleck, whose distinguished son is now at Harvard as a member of the Departments of Physics and Mathematics.

It was in the fall of 1909 that I became a member of the staff at Princeton. The presence of Veblen, nearly of my own age, with large ideas for American mathematics in general and for the Princeton Department in particular, meant much to me during my three years there. Veblen was then completing his important *Projective Geometry*, volume 1, written in collaboration with J. W. Young, whom many will remember kindly. It was my privilege to read the book in page proof, and to learn of Veblen's geometric program and ideas directly from him in our frequent walks and talks together.

I have recounted these personal circumstances only because I know that in their essence they are not very dissimilar from those of many American mathematicians.

In selecting Chicago, Cambridge, and Princeton for especial reference I have realized fully that American mathematics reaches overwhelmingly beyond what is to be found in any three or even in any ten centers. And yet I think it is a comforting thought for American mathematicians everywhere to know that there are centers like these where scholarly conditions have been uniformly good and where high ideals have been steadily maintained. Such places, by their influence and example, support and stimulate mathematical scholarship and achievement throughout the whole of our country.

Concerning the other mathematical centers suffice it to say that there are now about thirty institutions where the advanced student of mathematics may go with advantage to study for the doctorate, while only fifty years ago he was forced to go to Europe to secure adequate training! Among the privately endowed institutions may be mentioned Brown, Bryn Mawr, California Institute of Technology, Chicago, Cincinnati, Columbia, Cornell, Duke, Harvard, Institute for Advanced Study, Johns Hopkins, Leland Stanford, Massachusetts Institute of Technology, Notre Dame, Princeton, Rice Institute, and Yale; and among our state universities, California (at Berkeley and at Los Angeles), Illinois, Iowa, Michigan, Minnesota, Ohio State, Pennsylvania, Texas, Virginia, and Wisconsin; and in Canada, the University of Toronto. The number of such centers should increase still further. All that is required in many cases is that mathematicians in a position of influence take the proper steps. As instances in point, I would cite what was done by Fine at Princeton and by Harris Hancock at Cincinnati.

The extraordinary contrast between 1888 and 1938 is equally manifested by the fact that fifty years ago there were a mere handful of competent mathematicians in the country, whereas there is now a body of over two thousand American members of our Society. Among these, between one and two hundred have gone far beyond a doctoral dissertation to make important additions to mathematical knowledge, and some forty or fifty are highly creative with established international reputations. Later on I shall have occasion to refer to a number of these mathematicians and their specific contributions.

For the moment, however, I should like to direct attention to two interesting and important special groups. The first is made up of mathematicians who have shown the rare quality of leadership, of which Moore was an outstanding instance. Among the earlier of these I would mention the late eccentric geometer, George Bruce Halsted, who attracted to mathematics two notable figures, L. E. Dickson and R. L. Moore, both of whom in their turn have been able to exert a large personal influence. I would also mention with high esteem James Pierpont, who for many years was a source of inspiration at Yale. Among the other and younger men, besides Dickson, R. L. Moore, and Veblen, the names of G. A. Bliss, G. C. Evans, Solomon Lefschetz, Marston Morse, J. F. Ritt, M. H. Stone, and Norbert Wiener come to mind as having shown the same quality to an exceptional degree.

The second special group to which I wish to refer is made up of mathematicians who have come here from Europe in the last twenty years, largely on account of various adverse conditions. This influx has recently been large and we have gained very much by it. Nearly all of the newcomers have been men of high ability, and some of them would have been justly reckoned as among the greatest mathematicians of Europe. A partial list of such men is indeed impressive: Emil Artin, Solomon Bochner, Richard Courant, T. H. Gronwall, Einar Hille, E. R. van Kampen, Solomon Lefschetz, Hans Levy, Karl Menger, John von Neumann, Oystein Ore, H. A. Rademacher, Tibor Radó, J. A. Shohat, D. J. Struik, Otto Szász, Gabor Szegö, J. D. Tamarkin, J. V. Uspensky, Hermann Weyl, A. N. Whitehead, Aurel Wintner, Oscar Zariski.

With this eminent group among us, there inevitably arises a sense of increased duty toward our own promising younger American mathematicians. In fact most of the newcomers hold research positions, sometimes with modest stipend, but nevertheless with ample opportunity for their own investigations, and not burdened with the usual heavy round of teaching duties. In this way the number of similar positions available for young American mathematicians is certain to be lessened, with the attendant probability that some of them will be forced to become "hewers of wood and drawers of water." I believe we have reached a point of saturation, where we must definitely avoid this danger.

It should be added, however, that the very situation just alluded to has accentuated a factor which has been working to the advantage of our general mathematical situation. Far-seeing university and college presidents, desirous of improving the intellectual status of the institutions which they serve, conclude that a highly practical thing to do is to strengthen their mathematical staffs. For, in doing so, no extraordinary laboratory or library expenses are incurred; furthermore the subject of mathematics is in a state of continual creative growth, ever more important to engineer, scientist, and philosopher alike; and excellent mathematicians from here and abroad are within financial reach.

Having thus glanced at our mathematical firmament which shines so brightly today, let us turn to survey briefly the general situation in our country which has made it possible. In the year 1888 there were probably about two hundred thousand students in our high schools and preparatory schools; today there are between two and three millions. This enormous increase is a consequence of the unquestioning belief in higher education which pervades our country. At the same time our colleges, universities, and advanced technical schools have increased correspondingly in numbers and resources. There are today nearly a thousand such institutions scattered throughout our land, serving half a million or more students, with a total physical plant staggering the imagination and representing billions of dollars of endowment. Probably the majority of these institutions struggle along under financial as well as educational difficulties, although rendering distinct service. But when all is said and done, there remain some two hundred fifty of them which meet the exacting requirements of approval by the Association of American Universities.

As far as the mathematical side of this vast American enterprise of higher education is concerned, its magnitude is probably best appreciated by means of a different approach. The American Mathematical Society has a membership of over two thousand persons, the great majority of whom hold positions in our institutions of learning. Our highly esteemed sister organization, the Mathematical Association of America, devoted primarily to the interests of collegiate mathematics, has nearly twenty-five hundred members. The conclusion then is plain. There must be between two and three thousand mathematical teaching positions in our higher institutions, with an average salary which must certainly lie between two to three thousand dollars. We see in this way that there is probably a sum of about six millions of dollars which goes each year to the support of higher mathematics!

Since the Great War salaries have been increased and the teaching burden has been reduced, at least in the better institutions. I remember talking some twenty years ago with the late J. C. Fields of Toronto about the status of professors throughout the world; it will be recalled that Fields did more than anyone else to bring about the important International Mathematical Congress held at Toronto in 1924. He told me that, after making a special study of the facts, he had come to the conclusion that the American professor was the worst treated of all! At that time there was much in his contention, even though there were already in existence a number of American professorial chairs where the salary was good and the teaching duties not excessive. Today there are many such positions. In this connection it may be well to mention the fact that Harvard University has been able to reduce the amount of teaching and tutorial routine of the regular mathematical staff to six hours a week, of which only three hours are devoted to more or less elementary mathematical instruction. Such a schedule gives to all concerned a notable opportunity to carry on mathematical research, and would be socially unjustifiable unless the highest standards of achievement were being maintained. Although such ideal conditions are impracticable at present except in a few fortunate institutions, it should be strongly emphasized that twelve hours of instruction a week (including at least one course of advanced grade) is about all that can be required if the best standards of scholarship are expected. Indeed, wherever possible, the hours of instruction should be reduced to not more than nine, and if there are heavy outside duties there should be a compensating diminution in teaching.

But the situation has very definitely a complementary aspect. On our part there is an unescapable, deep responsibility to the nation which, somewhat unwittingly perhaps, has afforded us such splendid support. It is our duty to take an active and thoughtful part in the elementary mathematical instruction of our colleges, universities, and technical schools, as well as to participate in the higher phases. To these tasks we must bring a broad mathematical point of view and a fine enthusiasm. Insofar as possible we must actively continue as competent scholars and research workers. Only by so doing can we play our proper part.

It is interesting to note that the other material accessories useful for our extensive mathematical edifice have also been provided. With our Bulletin and Transactions, with the American Journal of Mathematics, all under Society auspices, and with the Annals of Mathematics, the Duke Mathematical Journal, the Journal of Mathematics and Physics, and the

American Mathematical Monthly, we possess excellent facilities for the publication of original articles in periodicals. Aside from the Journal of Mathematics and Physics there is as yet no journal directed towards applied mathematics. More extensive publication in book form is afforded by our Colloquium Publications, and a similar new series in contemplation by the Institute for Advanced Study. For the prompt publication of short articles there is available the Proceedings of the National Academy of Sciences. There are in addition certain facilities to be found in the annual publications of learned societies (such as the American Academy of Arts and Sciences), and of higher institutions of learning (such as the Rice Institute Pamphlets), etc. Thus far, however, the commercial publishing houses of the country have not contributed much towards the publication of important advanced mathematical texts. In this respect they suffer by comparison with progressive European publishers, who take pride in the publication of significant mathematical books. The University Presses of the country have partly made up for this lack.

In addition to our regular meetings, the Colloquium Lectures, the annual Gibbs Lecture, and the Visiting Lectures of the Society provide important means for direct scientific interchanges among mathematicians. The coming International Congress of Mathematicians to be held at Cambridge in September, 1940, will present still other opportunities of this kind. In fact the facilities for mathematicians to meet intimately with their colleagues at sister institutions are increasing constantly. The importance of such facilities in speeding up mathematical progress has long been recognized in European mathematical centers.

Then there is always the arduous administrative work of the Society, carried on unselfishly by its officers and especially by its present secretary, Dean R. G. D. Richardson, true successor of Frank Nelson Cole. The way in which this work has been carefully and devotedly done without any paid officer has helped to unite the Society more than anything else.

All in all, then, our American mathematical situation is about as favorable as can be hoped for on this very troubled planet. Our one real danger perhaps concerns the general standard of achievement. It is not enough for those who go into the rank and file of our colleges to devote themselves to a useful academic routine; they have a duty to live up to their highest mathematical potentialities, and to awaken a deep mathematical interest in their students. It is not enough for the exceptional man whose early work has led to professional recognition, to take thenceforth an easy-going attitude; such a man should continue with the devotion of a leader in a great cause. Furthermore, we ought all to provide our share of first-rate elementary teaching, by which we justify our privileged positions in immediate practical terms. If we do these things, mathematics in America will rise to still greater heights and there will appear among us mathematical figures comparable to the greatest in the past.

My main purpose today, however, is to lay before you briefly some of the significant mathematical advances which have been made in America during the last fifty years. It is these which really measure the success of our efforts. But in attempting to summarize what has been done, I can do no more than present a bold outline, imperfect because of the limitations of my own knowledge and point of view, and necessarily stressing the less technical aspects of the subjects referred to. I propose to take up successively the fields roughly characterized as logic, algebra, analysis, geometry, and applied mathematics.

Symbolic logic and axiomatics

It was the remarkable discovery of the English mathematician George Boole, contained in his book, *The Laws of Thought*, (1854), that the classical Aristotelian logic can be given a more adequate formulation as a kind of algebra now called Boolean algebra. He saw too that his algebra of propositions could equally well be regarded as an algebra of classes: for to say that the proposition a implies the proposition b is the same thing as to say that the class of objects for which a holds is a subclass of the class of objects for which b holds. Although Leibniz and others, among them some of Boole's own contemporaries, sought to construct a logical calculus, it was Boole's work more than anything else which stimulated a deeper critical study of logical processes, and thus paved the way for such great contributions as the complete logical symbolism of Giuseppe Peano (1888) and the theory of the hierarchy of logical propositions known as the theory of types, due to Bertrand Russell and A. N. Whitehead (1910). On the other hand the works of F. L. G. Frege (1879) and Georg Cantor (1883) have led to an increased understanding of the ideas of number which permeate and at the same time are partly deducible from purely logical ideas. Opposing schools have arisen from attempts to avoid certain deep seated paradoxes: L. E. J. Brouwer in 1912 proposed to reject the law of the "excluded middle," which asserts that a proposition is either true or false, and instead, to require the explicit construction of all logical entities, thereby abandoning such instruments of thought as the principle of arbitrary choice of Zermelo (1907). Hilbert in 1922 and others have asserted that mathematics is a kind of mechanical game played with marks, among which the special marks of the positive integers $1, 2, 3, \cdots$, are to be admitted without question, while the rules of the game are essentially those systematized in the *Principia Mathematica* of Whitehead and Russell. Among these rules would then be found a principle of arbitrary choice, carefully formulated.

Although the logician and philosopher, C. S. Peirce, son of Benjamin Peirce, contributed to Boolean algebra, definite mathematical work on this subject may be said to have begun in America with E. V. Huntington's set of postulates of 1904. The independence of these postulates was established on the basis of the Boolean sum $a+b$ (interpreted as a or b) and product $a\times b$ (interpreted as a and b) as fundamental operations; and it was also shown that the relation of inclusion, $a<b$, could replace either of these.

A highly elegant paper by H. M. Sheffer, destined to have an important influence upon general logical development, appeared in 1913. In it Sheffer showed that the single operation of nonconjunction, $a|b$ (interpreted as neither a nor b), would serve to replace the two operations of Boolean addition and multiplication. His three essential postulates may be written in very brief compass:

$$(a \mid a) \mid (a \mid a) = a, \qquad a \mid (b \mid (b \mid b)) = a \mid \cdot a,$$
$$a \mid ((b \mid c) \mid (b \mid c)) = ((b \mid b) \mid a \mid a) \mid ((c \mid c) \mid a).$$

These postulates may be immediately verified: thus the first postulate asserts merely that not (not a) is a. In this manner Boolean algebra was completely transformed in outward aspect and given a simpler form which inevitably suggested the possibility of further syntactical analysis of logical formulae. The single "stroke" operation, $|$, of Sheffer was made basic in the revised edition of the *Principia*. Sheffer's postulates were somewhat abbreviated by B. A. Bernstein shortly afterwards.

Other extremely important contributions to Boolean algebra have been made within a year or two by M. H. Stone. The gist of his idea is very simple: In the broad modern sense a set of objects $a, b, c, \cdots$ is to be regarded as a "ring" of numbers with reference to suitably defined operations of addition $(+)$ and multiplication $(\times)$ if the usual associative and commutative laws for addition and the associative law and distributive laws of multiplication hold, with unique solution x of the equation $a+x=b$, and with a unit u such that $a\times u=a$ for any a. There is then a unique zero z such that $a+z=a$ for any a.

We can associate a unique n-partite number a in this sense with each subclass of a class of n objects, by taking to correspond to each object of the class a symbol 1 or a symbol 0 according as the particular object in question does or does not belong to the subclass. Thus $1\ 1\ 0\ 1\ 0\ 0\ 1 \cdots$ would be a number specifying the subclass containing the first, second, fourth, seventh, $\cdots$ but no others of the given objects. If a and b are two such numbers, $a+b$ may be taken to be the similar number formed by adding corresponding figures according to the rule:

$$1+0=0+1=1, \qquad 0+0=1+1=0$$

(that is, addition modulo 2), while $a\times b$ is formed by multiplying corresponding figures as follows:

$$0\times 0=0\times 1=1\times 0=0, \qquad 1\times 1=1.$$

We may denote the particular numbers

$$1\ 1\ 1 \cdots \quad \text{and} \quad 0\ 0\ 0 \cdots$$

by u and z, respectively.

It is then immediately verified that the numbers so defined will form a ring, and in addition will obey the commutative law of multiplication $a \times b = b \times a$, and the special laws $a + a = z$, $a \times a = a$. Furthermore $a + b$ will represent precisely that subclass made up of the objects in a but not in b, and in b but not in a. In other words the Boolean sum is replaced by what Stone terms the symmetric difference. However, the product $a \times b$ coincides with the Boolean product, since it represents the subclass of objects contained in both a and b. Furthermore, the Boolean sum can readily be expressed in terms of the operations $+$ and $\times$ used by Stone, being precisely

$$(a + b) + (a \times b).$$

Thus Boolean algebra at last appears in its true light as identifiable with a special algebra in the ordinary sense, with all its elements idempotent ($a \times a = a$) and so necessarily commutative. Conversely, Stone shows that such an algebra with or without unit may be regarded as an algebra of subclasses. Through this identification Stone is able to employ modern algebraic methods in his study of Boolean algebra. To my mind his thoroughgoing papers constitute the most considerable advance in our understanding of Boolean algebras since Boole's own work.

Let us turn next to the broader aspects of symbolic logic as a whole. Here, too, much has been accomplished by Americans. Indeed a world movement in this field is under way in which the Polish school and our own have been foremost. Among our active figures are C. A. Baylis, A. A. Bennett, B. A. Bernstein, Alonzo Church, H. B. Curry, E. V. Huntington, S. C. Kleene, C. H. Langford, C. I. Lewis, J. B. Rosser, H. M. Sheffer, and W. V. Quine. Recently Rudolf Carnap, widely known for his studies of logical syntax, has taken a post in this country. The creation in 1936 of the Journal of Symbolic Logic is symptomatic of the activity of an increasing group of American specialists.

The significance and importance of this movement are unmistakable. For over two thousand years mathematicians have been exerting their every effort in marching upward towards the high summits of mathematical power. But the vast continual ascent bids fair to overwhelm and confuse. It is time to reconsider and determine the paths which we have followed, so that we may go forward more judiciously. Those who desire to help in this task will occupy themselves either with the study of mathematics as symbolic logic, or with axiomatics which aims at the unification of mathematics by the formation of suitable abstractions. It seems certain that work of this kind will be of the utmost importance in the coördination and simplification of all mathematics.

One of the earliest American papers having importance for logic at large was a short note of E. L. Post in 1921, in which he showed that the ordinary propositional calculus was complete in the sense that every proposition is either true or demonstrably false (by a *reductio ad absurdum*). As he stated, this was not a theorem of the propositional calculus itself, but rather a theorem about the calculus. It afforded an early instance of what is called metamathematics, being related to mathematics in much the same way as the general analysis of chess would be to specific games.

An interesting study of the principle of arbitrary choice was made by Alonzo Church in his dissertation of 1927. This principle is a very important one; as Zermelo established in 1907, it leads to a proof of the well-ordering of the continuum when accepted without restriction. On the other hand it is almost impossible to get along in ordinary mathematical reasoning without the use of this principle in some form or other; and indeed its validity had never been questioned until the explicit formulation by Zermelo. Church showed that it was possible to maintain a consistent position, intermediate between the extremes, in which the principle of choice was allowed for an ordered set belonging to the second number class, and that then certain theorems could be deduced.

In the last few years Church, Curry, Kleene, and Rosser, have been studying metamathematical questions involving questions of explicit logical constructibility and similar open logical problems. These belong to the fascinating metamathematical borderland between mathematics and philosophy, in which one is likely to be led astray at times by dubious subtleties.

As developments of the *Principia* reference may be made first to Lewis' theory of "strict implication" developed in C. I. Lewis' *Survey of Symbolic Logic*, 1917. Roughly speaking, this type of implication differs from the "material implication" of Whitehead and Russell in that for them any false proposition implies every other proposition, while for Lewis this is true only of necessarily false propositions. Lewis' modification flows from the omission of one of the postulates of the *Principia*, namely that two propositions which mutually imply one another are equivalent. His ideas are clarified and extended in a recent article by Huntington. Here Huntington introduces a modification $a \equiv b$ (read a quad b) of the $a = b$ of Whitehead and Russell. An essentially similar concept was introduced by A. A. Bennett and C. A. Baylis in 1935.

Secondly, in his *System of Logistic* of 1934 and subsequently, W. V. Quine has studied very deeply various syntactical matters. His theory of ordination makes especial use of an early remark of Wiener's (1914) to the effect that the ordered couple or dyad (a, b) can be logically defined as the class formed by the elements a and the (unordered) couples a, b, while an ordered n-ad can then be readily defined in terms of a set of dyads. By means of ordination a more satisfactory and explicit understanding of the syntax of the well-formed formula can be obtained. Like Church, Quine has devoted a great deal of attention to the highly difficult task of giving the basic theory of types of Whitehead and Russell a different and more satisfactory form.

Correlated with such investigations of the structure of logic as a whole are those of axiomatics, which aim to unify mathematics by the abstract method. A strong impetus towards axiomatics was given by the work of Hilbert on the foundations of geometry in 1899. Veblen's elegant dissertation of 1905 on the same subject was based on the relation of "betweenness" of B as to points A and C on a line, and served as the approach to geometry adopted in the *Principia*. Likewise in his and Young's *Projective Geometry* a postulational basis was adopted which allowed for finite geometries, with only a finite number of points, as well as for the ordinary n-dimensional projective geometries. Such geometries were immediately at hand when the marks of Galois' fields replaced ordinary numbers as coordinate elements.*

A novel approach to ordinary geometry was developed by A. R. Schweitzer in 1909. He showed, for instance, that one could use the intuitive relation of the sameness of sense of coplanar point-triads ABC and $A'B'C'$ as the basic relation in a set of geometric postulates; more generally, Schweitzer showed how to "generate m-dimensional geometry n-dimensionally $(m \geqq n)$" by the use of a similar relation of dimensionality n. Another new approach to geometry was achieved by Huntington in 1913 who showed that one could take the sphere (point = null sphere) as the undefined element, and inclusion as the single undefined relation.

In this connection let us refer to a mathematically much less significant postulational approach to geometry of the author's (1932), which embodies the intuitive ideas of linear and angular one-dimensional measurement (ruler and protractor), and so lies intermediate between the usual qualitative attack and the other extreme of purely analytic systematization.

E. H. Moore was, of course, the great American protagonist, in his day, of the abstract point of view. In a well known dictum contained in his New Haven Colloquium Lectures of 1906 he said that "The existence of analogies between the central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features." By temperament he was an abstractionist, and, where most mathematicians pass from the special to the general, he appeared to do the opposite. At any rate, upon being asked once if he too did not really begin with the special case, he replied that he was not conscious of doing so. Unfortunately his efforts to develop a movement toward the abstract were not very successful. This was for two reasons: his addiction to unusual symbolism which, while

* This possibility had been pointed out by Veblen and W. H. Bussey in 1906.

masterly in its way, failed to attract most of those around him; and a desire to perfect and generalize indefinitely, without regard to any question of publication. But it must be admitted today that his faith in the abstract point of view is being more and more justified.

Whether or not his most ambitious abstract project, that of General Analysis (1906), will leave any important aftereffects remains to be seen. In this he proposed a study of functions of a general variable on an absolutely general range. The nearest analogous work was that of Fréchet (1905) who, however, used the notion of limit in restricting the independent variable. Ideal instances of General Analysis in Moore's sense are furnished by the direct abstraction of the Fredholm theory of linear integral equations and of Schmidt's dissertation already alluded to. But no great interest in these abstractions has so far arisen among mathematicians inasmuch as the natural and easy way to proceed seems to be to grasp the classic results as originally obtained, and to observe that the specified range of the independent variable plays little or no rôle. In this way generalizations or slight modifications will suggest themselves as needed, as for instance the generalization from 1 to n dimensions.

It remains to refer to one other recent abstract development which perhaps will contribute more to the actual unification of mathematical thought than the General Analysis of Moore. We refer to the work of hierarchies, for which the name of "lattices" has been suggested by Garrett Birkhoff, and "structures" by Ore. Such lattices are of as pervasive occurrence in various branches of mathematics as finite groups. It still remains to be seen, however, whether or not the theory of lattices will lead to a technical development as imposing as that of finite groups.

Although the idea of lattices goes back partially to C. S. Peirce and Ernst Schröder, it was Richard Dedekind who first saw their true nature and importance (1897). The fundamental relation in a lattice is, of course, that of inclusion, a includes b, which we write $a > b$. It is interesting to see how analogous the postulates for lattices and Boolean algebras are. In a lattice with elements a, b the sum $a+b$ may be used to indicate the least element containing a and b, while the product $a \times b$ denotes the greatest element contained in both a and b. It is obvious that the associative and commutative laws of addition and multiplication are obeyed, as well as the special laws

$$a = a + (a \times b) = a \times (a + b).$$

Birkhoff's first paper (1933) served to initiate the recent active development of lattice theory. His work, that of von Neumann, and of Ore have been of especial importance. Some of their results will be sketched.

An important type of lattice is the so-called modular lattice of which an example is furnished by the normal subgroups of a given group. Such lattices satisfy the (self-dual) modular postulate of Dedekind:

$$a + (b \times c) = (a + b) \times c, \qquad \text{if } a < c,$$

as well as those already mentioned. Birkhoff showed that any complemented* modular lattice may be regarded as a sum of projective geometries, the elements of the lattice being given by the collection of linear subspaces through the origin. The corresponding fundamental theorem for the arithmetization of Boolean algebras (distributive lattices) was announced by Stone somewhat earlier.

Von Neumann (1936) was led in this manner to construct projective geometries in the full sense, but *without points*, by abandoning the requirement that each element has a next greater element. This has led him to his remarkable "continuous geometry" with a dimension function which ranges continuously from 0 to 1.

Ore has established that a considerable part of the theory of the representation of groups by direct products in an essentially unique way, and of the corresponding theories for linear algebras, Lie algebras, and finite rings, can be obtained immediately by means of the lattice theory; this is especially clear in the case of groups. Another very significant result of Ore's is the so-called Kurosch-Ore theorem that the invariance of the number of components in irredundant representations of an ideal follows at once from the fact that such ideals form a modular lattice.

The subject of lattices has also important applications to analysis as von Neumann first pointed out. Birkhoff has more recently shown its significance for the theory of dependent probabilities and the related ergodic theorem, and for the partially ordered function spaces first considered by F. Riesz, Kantorovitch, and Freudenthal in Europe.

A partially ordered set is of course one in which the elements of any pair are in a definite order or else are unordered. Obviously a lattice is a particular set of this kind. Any partially ordered set may be constructively imbedded in a lattice, as has recently been proved by H. M. MacNeille.

Summarizing, the Society can be very proud of the numerous distinguished achievements which have been made in symbolic logic and axiomatics. We may also look confidently to the future, because of the central importance and interest of the field with its many open problems, and because of the fact that a strong group is actively pressing forward. A word of warning may not be amiss, however, to those who contemplate specializing in logic, namely that there is a definite attendant danger of overspecialization and of sterility much like that long recognized to exist in the theory of numbers. This danger can be avoided only by definitely seeking to broaden one's outlook, and by engaging in useful systematic work as well as in attempts to solve what may be well-nigh insoluble problems.

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* That is with a least element 0, a greatest element 1, and such that for any element a there is a complementary element a' such that $a+a'=1$, $a\times a'=0$.

Linear algebra, finite groups, and the theory of numbers

By algebra, in a broad sense, we shall mean not only the subject of linear associative algebras, but also that of finite groups, which is inevitably introduced by the consideration of algebraic equations, and of the theory of numbers, which arises as soon as the idea of the integer comes into play.

America's contribution to algebra in this sense has been a notable one. There are limitations, it is true, but the record since 1888 is very substantial. In glancing over this record it seems natural to begin by reference to the work of Dickson who has probably been the foremost American algebraist of the period. He has added extensively to all three branches of the subject. His *History of the Theory of Numbers* in three volumes (1919, 1920, and 1923) is an invaluable aid to every student of number theory. Moreover, the influence which he has exerted through his students has been very considerable. In the height of his activity today, Dickson will always remain one of our great figures.

It would be difficult for anyone not an algebraist to give a balanced, brief account of his achievements. But there are certain things which are especially impressive and which we venture to single out from among his numerous contributions.

His definition in 1923 of a set of "integral elements" of any linear associative algebra with a modulus 1, over the field of rational numbers, has turned out to be an important advance. In this way he modified the definition of du Pasquier by replacing the condition that the set have a finite basis by the condition that the rank equation of the set have leading coefficient 1 while all of the other coefficients are integers of the field. This definition, contained in his book, *Algebras and their Arithmetics*, has been generally accepted ever since in the extensive German development of the arithmetics of associative algebras, largely centering upon the development of a unitary theory of ideals, by Emil Artin, Helmuth Hasse, the late Emmy Noether, B. L. van der Waerden, and others.

It should be stated at once that three absolutely fundamental theorems of J. H. M. Wedderburn (1908), somewhat overlooked until Dickson's book called attention to them, marked a veritable turning point in the theory of linear associative algebras. These so-called structure theorems make it clear how the study of a particular algebra can be reduced in large measure to the study of certain associated isomorphic "division algebras" (that is, permitting of division except by 0), and the maximum nilpotent subalgebra (that is, such that some power of every element reduces to 0). A simple illustration of such a decomposition is afforded by the ring

$$a + b\epsilon, \qquad a, b \text{ real}; \epsilon^2 = 0,$$

in which the division algebra is the ordinary one of real numbers, and the maximum nilpotent subalgebra is that of the subring $b\epsilon$. It should also be said in this connection that the important work of A. A. Albert deals largely with the structure of algebras, so that the algebraic tradition at Chicago, begun by Moore and Maschke, is being continued in full vigor by Dickson and Albert.

This contribution of Dickson's was mentioned first because of its relation to Wedderburn's work and of its importance for the modern unified point of view towards linear associative algebras and their arithmetics.

An early group-theoretic contribution of Dickson's consisted in his theorems concerning the orthogonal modular groups in 3, 4, 5, and 6 variables, analogous to classical theorems for real orthogonal groups which connect these with isomorphic projective groups in fewer variables (see his *Linear Groups* of 1901).

There comes to mind next his varied work on the invariants of modular forms under unimodular substitutions, much of which appeared for the first time in his Madison Colloquium Lectures of 1913. This algebraic work has of course a number-theoretic side. As Dickson says, the theory of modular invariants "should be of an importance commensurate with that of the theory of invariants in modern algebra and analytic projective geometry, and should have the advantage of introducing into the theory of numbers methods uniform with those of algebra and geometry." While these expectations may not have been fully realized, nevertheless the subject as developed by him is decidedly interesting.

One thing emerges clearly concerning modular invariant theory, namely that the modular case is fairly analogous to the familiar algebraic case. Suppose, to take an extremely simple illustration, that we consider a ternary quadratic form (mod 5)

$$q_3 = \sum_{i,j=1}^{3} \beta_{ij} x_i x_j$$

of determinant $D = |\beta_{ij}| \neq 0$. If $\beta_{11} \neq 0$ the substitution of

$$x_1 - \beta_{11}^{-1}(\beta_{12}x_2 + \beta_{13}x_3)$$

for x, reduces q_3 to a more specialized form $\beta_{11}x_1^2 + q_2(x_2, x_3)$ with the same determinant. If, however, $\beta_{ii} = 0$, $(i = 1, 2, 3)$, we may replace x_1 for instance by $x_1 + x_2$ and proceed as before. Thus we immediately reduce q_3 to a sum of squares

$$\alpha_1 x_1^2 + \alpha_2 x_2^2 + \alpha_3 x_3^2$$

by use of unimodular substitutions. But if α_1 is a quadratic residue, the further substitution

$$x_1 = \alpha_1^{-1/2} x_1', \qquad x_2 = x_2', \qquad x_3 = \alpha_1^{1/2} x_3',$$

reduces α to 1. Thus we readily reduce two of the α_i's to 1 whenever two of the α_i's are residues, and so obtain a normal form

$$x_1^2 + x_2^2 + Dx_3^2 .$$

In any other case, however, since 2 is a nonresidue we obtain one of the alternative forms

$$x_1^2 + 2x_2^2 + (D/2)x_3^2 \qquad \text{or} \qquad 2x_1^2 + 2x_2^2 + (D/4)x_3^2$$

without difficulty, where we can suppose that D is a residue in the first case and a nonresidue in the second. But in the first case the form may be written

$$D(D^{-1/2}x_1)^2 + (x_2 + \tfrac{1}{2}D^{1/2}x_3)^2 + (x_2 - \tfrac{1}{2}D^{1/2}x_3)^2,$$

which is essentially in the first written normal form. And the second form may be written

$$(x_1 - x_2)^2 + (x_1 + x_2)^2 + D(x_3/2)^2,$$

which is in the same normal form. Thus for such a ternary quadratic form of determinant $D \neq 0$, the normal form is entirely analogous to that for the corresponding algebraic case, with single invariant D.

Turning now to the theory of numbers, we first refer to Dickson's result (1908) that Fermat's celebrated equation

$$x^p + y^p + z^p = 0, \qquad p \text{ an odd prime},$$

has no solution in integers prime to p for $p<7000$. This is obtained by an extension of the well-known elementary method of Sophie Germain beyond the previous limit $p<257$.

In 1930 Dickson published his book, *Studies in the Theory of Numbers*, which contains varied results largely concerning those "universal" quadratic forms which represent all positive integers n as, for instance, xy for $x=n$, $y=1$, or $x^2+y^2+z^2+w^2$ since every number is a sum of four or fewer squares. One of his simple results, obtained once more by elementary methods, is that every universal indefinite ternary quadratic form is a "zero form," that is, takes on the value 0 when not all the variables vanish.

These investigations show a direction of interest on Dickson's part akin to that of his work on Waring's problem in more recent years. It has been well known for a long time that every integer is expressible as the sum of not more than four squares; similarly every integer can be expressed as a sum of not more than nine cubes. A more general theorem is embodied in the conjecture of Waring that every integer is the sum of a finite fixed number of nth powers, say $g(n)$, so that $g(2)=4$, $g(3)=9$. Waring's theorem was first proved by Hilbert; and the subsequent asymptotic researches of Hardy and Littlewood in England and lately of Vinogradow in Russia have yielded much information as to the least number of powers required,

$G(n)$, for sufficiently large integers n, which is far smaller than $g(n)$ in general. Now it is very reasonable to think that the number of powers required to express the integer $3^n - 1$* is probably in most cases the true maximum number $g(n)$ of powers which are actually required. By special methods supplementing the general abstruse researches mentioned above, Dickson has shown that $g(n)$ has this value for $9 \leqq n \leqq 400$, and so has completed in these cases the proof of Waring's theorem in its most natural form.

In a certain sense Dickson's work in these fields reveals a kinship of spirit with Gauss. It will be recalled that Gauss solved by means of square roots alone the cyclotomic equation $x^{17} - 1 = 0$ (and similar equations), and thus was able to prove that the regular polygon of 17 sides was constructible by ruler and compass; but Gauss, presumably, never speculated on the fascinating question of algebraic solubility *in general*, which Galois answered about 1830.

On the other hand, Dickson's teacher and subsequent colleague, E. H. Moore, was an extraordinarily gifted algebraist of quite the opposite temperament. But at the height of his career he forsook algebraic investigation for his General Analysis already alluded to. Two results of his in group theory are deserving of special note. He established (1893) that the group of unimodular substitutions in a Galois field is simple; and he was the first to observe(1898) that any finite group admits an invariant positive definite Hermitian form—indeed such a form is obtained at once by adding together all of the values of $\Sigma x_i \bar{x}_j$ where $x_1, x_2, \cdots, x_n$ and $\bar{x}_1, \bar{x}_2, \cdots, \bar{x}_n$ are respectively conjugate variables. This fundamental result admits of important application to the theory of orthogonal groups as Maschke soon showed.

Turning now to other algebraists, we shall mention four as having done work of especial importance. H. F. Blichfeldt gave close limits for the orders of primitive unimodular linear groups and for the powers of the prime factors occurring therein (1903); in addition he enumerated completely the finite linear groups in four variables (1904), thereby solving a difficult classical problem. More recently he has been engaged in the problem of refining still further the results of Minkowski concerning the approximate solution of incomplete sets of linear equations in integers.

A. B. Coble has contributed to the theory of algebraic invariants and to the modular groups associated with the abelian θ-functions.

G. A. Miller has written extensively on special questions concerning finite groups and has obtained many interesting results. He is perhaps the outstanding authority on special finite groups.

H. S. Vandiver has similarly taken a leading world position as an expert of Fermat's problem mentioned above and the related theory of cyclotomic ideals. It happened that the author was early interested in the theory of

* Namely, the integral part of this integer divided by 2^n, plus the remainder after division.

numbers, and that the two collaborated in an elementary study of the integral divisors of $a^n - b^n$; we proved the theorem that if a and b are without a common divisor and $n>2$, such a form always admits a "primitive divisor" except in the case $n=6$, $a=2$, $b=1$. A primitive divisor, necessarily of the form 1 modulo n, is one which divides no similar form with smaller exponent n'. In the more than thirty years which have elapsed since our youthful venture, Vandiver has continued in this one direction. He has established that the theorem of Fermat certainly holds for any exponent less than 618, thereby considerably extending his similar previous results. He has also proved the elegant result that if Fermat's equation is soluble in integers, then the following congruences hold:

$$x^p \equiv x, \qquad y^p \equiv y, \qquad z^p \equiv z \qquad \pmod{p^3}.$$

Furthermore, he has extended the criteria of Wieferich and Mirimanoff for $q=2$, 3 to $q=5$, by showing that if there is a solution of Fermat's equation, then the congruence

$$5^{p-1} - 1 \equiv 0 \qquad \pmod{p^2}$$

is satisfied; Frobenius subsequently obtained a like condition for $q=11$, 17.

With these preliminary facts before us some further remarks on each of the fields of linear algebras, finite groups, and number theory, must conclude our brief survey.

As far as ordinary algebraic theory is concerned there should be mentioned, among others who have actively contributed, H. T. Engstrom, O. E. Glenn, Olive Hazlett, M. H. Ingraham, Nathan Jacobson, C. G. Latimer, N. H. McCoy, C. C. MacDuffee, Saunders MacLane, J. F. Ritt, J. B. Shaw, and Henry Taber, of whom Taber is no longer living. A number of these are young men, and everything indicates a continued algebraic development in this country. In particular there is today a strong representation at Princeton, comprising von Neumann, Wedderburn, and Weyl.

On the other hand in the somewhat narrow field of finite groups, in which Americans have long been preëminent, there is less activity. Among the distinguished workers in this field not yet mentioned are H. H. Mitchell, whose dissertation gave a very interesting geometrical treatment of linear modular groups in three variables, and W. A. Manning who has especially studied simply and multiply transitive permutation groups. But very few young men seem to pursue the theory of finite groups, once so popular in this country.

In number theory the stimulating work of E. T. Bell has not yet been mentioned; it has largely been in the study of "arithmetical paraphrases." This really is the general theory of the parity (oddness or evenness) of functions in a general algebraic ring. The functional equations satisfied by the ordinary trigonometric functions, the θ-functions, etc., are made to yield important known as well as new identities. American work in the theory of numbers has been enhanced by the presence of Hans Rademacher who has made important contributions to the theory of partitions.

It is very satisfactory to note that there is at present a very active and able group of younger men who have number-theoretic interests and who are obtaining valuable results, as Leonard Carlitz, Marshall Hall, Ralph Hull, R. D. James, D. H. Lehmer, Gordon Pall, and Morgan Ward. Here again the outlook is very propitious.

Thus we see that there has been a great algebraic advance in the direction of a unified theory of linear associative algebras and their arithmetics, in which we have taken an important part. But while in Europe certain outstanding classical problems have been solved—such as the finiteness of complete systems of algebraic invariants and Waring's problem, both in the affirmative sense by Hilbert—we in America have scarcely reached such exalted heights of algebraic achievement. Notwithstanding this fact, however, we have every right to be very proud of what has been already accomplished among us.

Analysis

The field of pure mathematics called analysis is extraordinarily vast and diversified. In attempting to outline our very considerable advances in this field during the last fifty years, only a few significant achievements can be singled out more or less arbitrarily. This is a difficult task; and we begin by referring briefly to the work of four of the older analysts, in whose contributions there can be discerned the beginnings of most of our principal directions of advance—Moore, Osgood, Bôcher, and Van Vleck.

Although Moore was primarily a logician and algebraist rather than an analyst, nevertheless his analytical work was important, and his influence through his General Analysis already mentioned, was considerable. In this, he introduced, for example, the interesting idea of "relatively uniform convergence" of a sequence, in which the difference between the limit and the nth element becomes and remains less than $\epsilon\sigma_p$ in absolute value, where σ_p is one of the linear class of functions under consideration over the range P. Let this class of functions be designated by $\mathfrak{M}$, and the (linear) class of such uniform limits by $\mathfrak{M}^*$. It follows at once that we have $(\mathfrak{M}^*)^* = \mathfrak{M}^*$, a theorem generalizing a well known result for the class of continuous functions, for instance.

His article *Concerning transcendentally transcendental functions* (1897) is very suggestive. In a phrase which has a distinctly Moorean quality he thus designates those transcendental functions which satisfy no algebraic differential equation. This paper was stimulated by Hölder's recent proof that $\Gamma(x)$ had this property, but was much more penetrating. Moore showed just why $\Gamma(x)$ and other simple functions belong to this class. He also did a considerable amount of work on improper definite (Riemann) integrals—a subject which has for the time being lost much of its flavor to mathematicians because of the systematic use of the Lebesgue integral.

Strongly influenced by Felix Klein of Göttingen, Osgood has devoted his attention mainly to the theory of functions of a single complex variable. His article on this subject in the Encyklopädie der mathematischen Wissenschaften (1901) represents the first careful and systematic presentation of the Riemannian point of view, which is dominant today, as against the earlier Weierstrassian approach, based on the use of power series. Osgood's subsequent *Funktionentheorie*, has provided a large part of the present mathematical world with its fundamental training in this field, and remains today an invaluable adjunct to other books emphasizing more recent developments.

But Osgood has been more than a very thoughtful and systematic expositor of the subject. With much scholarly insight he has made important contributions. Perhaps the best known of these is his extremely elegant and general theorem on conformal mapping (1900), namely, that any simply connected open region in the z-plane possessing more than one boundary point can be mapped in a (1,1) conformal manner on the interior of a circle of the w-plane—a theorem previously established for regions having a much more restricted type of boundary. Similarly in an important paper by himself and A. E. Taylor (1913) they proved* that in such a mapping each neighborhood of an accessible point of the boundary was mapped on the neighborhood of a single point of the bounding circle. By these two results, Osgood has aided greatly in the understanding of conformal mapping problems. Among other American mathematicians, Wladimir Seidel and J. L. Walsh have done most in this direction.

In 1901 Osgood established a now classical, fundamental theorem of the calculus of variations, known by his name, to the effect that under very general conditions the numerical value I of an integral, along any admissible arc C joining two fixed points P_0, P_1 and contained in a fixed neighborhood of a minimizing arc C_0 joining the same points, will exceed I_0 by at least a quantity $\delta(\epsilon)>0$ where ϵ denotes the (properly defined) distance between the two curves; for example, if I represents arc length, then C_0 will be the straight line segment P_0P_1 of length $I_0=2l$, say; and the theorem asserts that the length of any rectifiable arc P_0P_1 exceeds $2l$ by $\delta(\epsilon)$ at least, where we may now take

$$\delta(\epsilon) = 2((l^2 + \epsilon^2)^{1/2} - l).$$

Finally we may refer in this all too brief account, to Osgood's well known treatment of the gyroscope by intrinsic vectorial methods (1922). As the late O. D. Kellogg showed (1923), this elegant approach gave a direct answer to delicate qualitative questions concerning the behavior of the gyroscope.

* Osgood had announced these results in 1903.

In his close association with Bôcher until the latter's all too early death, Osgood has always felt most appreciative of Bôcher's clarification of function theory in many directions. Thus Bôcher's visual interpretation of Poisson's integral as the angular average of the values marked on the circle as seen by an observer to whom light travels along circular arcs orthogonal to the given circle, aids in providing intuitive meaning to this important integral. However, scarcely any of Bôcher's contributions fall directly in the domain of functions of a complex variable.

J. L. Walsh would probably be regarded today both here and abroad as the American who has above others continued the excellent tradition in the theory of functions of a complex variable begun by Osgood. Walsh's dissertation gave very interesting extensions of Bôcher's work on the relation between the zeros of two homogeneous binary forms of degree n, and the zeros of their Jacobian—readily interpreted as results concerning the zeros and poles of a rational function, and the zeros of its derivative. Since that time Walsh has contributed extensively to the theory of the approximation to analytic and harmonic functions by polynomials and by rational functions, and to the theory of interpolation in the complex domain (see his Colloquium Lectures of 1935). He has also defined an orthogonal set of step-functions of much interest.

Bôcher's first work of importance is to be found in his book *Die Reihenentwickelungen der Potentialtheorie*, which was essentially an amplification of a dissertation under Klein in 1891. This was in the main the fulfillment of a general program sketched by Klein, to complete the formal theory of expansions in Lamé's functions, derived from the potential equation in three variables by the introduction of general cyclidic coördinates and the subsequent separation of variables. What has been called "Bôcher's theorem" enumerates the various possible degenerate forms of Lamé's functions; these turn out to include practically all of the special functions most useful in mathematical physics.

Nearly all of Bôcher's later work centered around the potential equation, which is indeed a focal point for a great part of analysis, real or complex. He never pursued, however, the difficult and complicated questions of convergence and representation associated with Lamé series of orthogonal functions. Instead, he turned to simpler related problems, in particular to boundary-value problems in one dimension. Among his various extensions of Sturm's comparison and oscillation theorems, basic for this boundary-value problem, it was an elegant short note of 1905 on the solutions of an ordinary linear differential equation of the second order under periodic conditions which first stimulated the author's interest in this special field.

Bôcher's mathematical perspective and his instinct for what was important were very unusual. He was interested in comparison and oscillation theorems simply because of the information which they give concerning solutions of ordinary linear differential equations as functionals of the coefficients. This particular topic interested him as much as any other. In his Paris lectures near the end of his life he chose it as his general theme which was expanded into the *Leçons sur les méthodes de Sturm*.

His unusual modesty led Bôcher more than once to conceal a new result under the guise of an apparently expository article. This happened for instance in his beautiful exposition of the elementary theory of Fourier series (1906), where the first detailed study of Gibbs' phenomenon was run into the body of the text almost without comment. His passion foı elegance and simplicity grew with the years. What he sought more than anything else were those simple illuminating insights which contain the germ of a real advance. For instance, he remarked in a brief note published in the same year that one could define a harmonic function $u(x, y)$ (that is, one satisfying the potential equation in two dimensions) as a function continuous, together with its two partial derivations of the first order, which satisfies Gauss' theorem on every circle C. To anyone familiar with the elements of the theory of inversion and Poisson's integral, Bôcher's proof can be presented in a few lines. In the same year the German mathematician Koebe proved that if $u(x, y)$ is merely continuous and satisfies the average-value theorem on every circle, then $u(x, y)$ will be harmonic. However, Bôcher's starting point is much closer to physical intuition, and his paper has been of at least equal importance in its general influence.

Turning now briefly to Van Vleck, we refer to his well known dissertation on continued fractions (1894) of which a partial account is to be found in his Colloquium Lectures of 1903 on *Divergent Series and Continued Fractions*. Such continued fractions are most easily defined by means of certain recurrence relations which are actually linear difference equations of the second order in n, with coefficients dependent on x, so that the functions considered are basically of the form

$$\lim_{n\to\infty} y_n(x)/z_n(x),$$

where $y_n(x)$ and $z_n(x)$ are linearly independent particular solutions of this difference equation.

Although Van Vleck wrote only one brief paper on linear difference equations (1912), the subject engaged much of his attention for several years. It was his suggestive lectures on difference equations during a stay as instructor at Madison that led the author to an appreciation of the open problems in the field. Of Van Vleck's later work we refer only to his interesting example of a nonmeasurable set in the sense of Legesgue (1908) based, as was inevitable, upon the principle of arbitrary choice, and to his studies (with F. H'Doubler) of the general solution of the functional equations satisfied by the elliptic θ functions.

With these facts in mind, let us attempt an evaluation of some of the main American accomplishments in analysis. For this purpose it is proposed to deal successively with some ten directions of effort which seem particularly worthy of consideration: (1) functional analysis; (2) functions of one or more complex variables; (3) the calculus of variations; (4) potential theory; (5) Fourier series and integrals; (6) boundary-value and expansion problems; (7) linear differential equations; (8) linear difference equations; (9) ordinary differential equations; (10) special analysis. Of these, the American tradition in functional analysis goes back essentially to E. H. Moore; in functions of one or more complex variables, to Osgood; in potential theory, Fourier series, boundary-value and expansion problems, and ordinary linear differential equations, largely to Bôcher; in linear difference equations, to Van Vleck; in the theory of ordinary differential equations, perhaps to the author; and in the important field of special analysis, to Wiener more than to any other American. It is felt that the most serious defect of our achievements in analysis is that we have as yet done very little in partial differential equations and analytic number theory. Hans Lewgaud and C. B. Morrey are beginning to make contributions to the former field.

1. **Functional analysis.** In functional analysis it is necessary first of all to consid r the school of thought which originates more or less directly with E. H. Moore and his General Analysis. Here we find much of the work of L. M. Graves, W. L. Hart, and T. H. Hildebrandt. These analysts have formulated abstract theories under which the expansion of a function of n variables in Taylor's series, and existence theorems both for n ordinary differential equations of the first order, and for a set of n implicit functional equations in n unknowns are extended to the case when n becomes infinite. Unfortunately it seems to be the fact that the really interesting special cases of these theories can often be reached by mild artifices and easy extensions of classical theories. Accordingly this kind of work seems to be serviceable rather than particularly exciting.

A somewhat less general point of view is that initiated by Fréchet in which the notion of limit is introduced in dealing with the independent variable. A classical result here was that of E. W. Chittenden (1917), establishing that Fréchet's *écart* and *voisinage* are at bottom equivalent. Several important contributions to functional analysis in the ordinary sense have been made by T. H. Hildebrandt and A. D. Michal.

The present-day trend of mathematical thought seems to be to restrict attention to a few especially significant types of spaces such as topological spaces, linear metric (Banach) spaces, Hilbert spaces, etc., rather than with Moore to use an entirely implicit space as an *omnium gatherium.* This modification of Moore's program has met with much success.

Thus in a very well known paper of 1932, von Neumann solved a problem formulated by Hilbert, as to whether or not ever'' abstract topological group whose parameter-space is locally euclidean, is equivalent to a Lie group. Von Neumann showed that the answer is in the affirmative sense for compact groups (using Haar measure). Later van Kampen treated the commutative case. Furthermore Walther Mayer and T. Y. Thomas have given a rigorous treatment of Lie group nuclei in the topological case. Garrett Birkhoff has considered the case when the parameter-space is a Banach space, while, in the neighborhood of the identical transformation, vector differences are nearly preserved, and has then arrived at a correlated Lie algebra.

Various kinds of abstract integration have been likewise considered in a similar spirit by Birkhoff, Bochner, Dunford, Graves, and others.

Such abstract developments as these are of decided importance because the underlying assumptions are so simple and inevitable, and the methods so elegant, that the abstract subject matter becomes concrete as soon as understood.

We next indicate a general program of functional analysis concerning existence theorems which was initiated by Kellogg and the author (1922). This has proved itself more effective than the obvious treatment by direct abstraction.

The ordinary implicit equations of analysis can be written in the form $f \quad T(f)$ where f is the "point" in function space whose existence is to be established and $g=T(f)$ for any f is a transformed point in the same functional space. Thus the desired existence theorem merely affirms that the transformation T of the space into a subspace admits of a fixed point. Kellogg and the author started with the fact that if a convex $2m$-dimensional region in euclidean space is transformed into (a part of) itself by a continuous single-valued transformation, there is always at least one fixed point. This immediately yielded, by a passage to the limit, various known existence theorems and other new ones. It was clear that this method could be given a more abstract form, such as the theory later took under the hands of Leray and Schauder.

Another very important class of transformations is furnished by the linear homogeneous transformations of a Hilbert space* into itself, $x'=T(x)$. These are said to be of bounded type if the distance of the transformed point from the origin $|x|$ does not exceed $K|x|$, where K is independent of x. If, furthermore, we have, for x and y real, $\sum x_i y_i' = \sum y_i x_i'$, the operator T is said to be self-adjoint. The transformation is then analogous to those affine transformations of n-dimensional Euclidean space which multiply distances in the directions of n mutually orthogonal axes by corresponding (limited) constants. Of the same order of difficulty is the so-called Hermitian case. The study of bounded self-adjoint trans-

* The real points x of Hilbert space consist of these infinite real sequences $x_1, x_2, \cdots$ for which $\sum x_i^2$ is finite; this is essentially the same as the space of all real functions f such that f^2 is integrable in the sense of Lebesgue.

formations was initiated by Hilbert. Recently the unbounded case has been treated by von Neumann and by Stone. The principal results and important applications can be found in Stone's book of 1932, *Linear Transformations in Hilbert Space.* Among the younger men F. J. Murray is especially to be named in this field.

Although we have here a very general theory embracing in a certain sense that of most of the orthogonal series defined by ordinary linear differential equations, nevertheless, just because of this generality, the theory fails to yield an easy direct approach to the discussion of such series.

A very interesting fact about extensions of known facts in n dimensions to function space, is that a trivial fact for n finite may become of prime significance for the corresponding extension. This situation can be illustrated by a theorem of the author. It is trivial in n-dimensional space that, if we vary a set of mutually orthogonal unit vectors continuously, and if initially they define the complete n-dimensional space (that is, are n in number), then they must continue to do so throughout the variation. Under certain restrictions it was proved that an analogous fact holds for function space which allows us, for example, to infer the completeness of the Sturm-Liouville series from that of the Fourier series. The same idea has been found useful in other connections.

2. **Functions of one or more complex variables.** We have already referred briefly to the work of Osgood and Bôcher in this field. Besides their results may be mentioned advances in the difficult field of algebraic functions of more than one variable by Lefschetz and J. W. Alexander, who first realized the desirability of utilizing the methods of analysis situs in this domain.

From this brief list are excluded those who are occupied with topics which involve special functions, or in which the function-theoretic aspect is present but takes a somewhat subordinate position. If such workers were admitted, the names of Boas, Bochner, Bohnenblust, L. R. Ford, Hille, Levinson, W. T. Martin, Rademacher, Radó, Ritt, Shohat, Tamarkin, Walsh, Weyl, D. V. Widder, and Wiener would have to be added since all of these men have made important contributions to one or the other of the types. This is a goodly list.

3. **The calculus of variations.** Here it is well to make mention of the strong tradition at the University of Chicago in this field. While at Chicago, Bolza became more and more occupied with the subject and published there his *Lectures on the Calculus of Variations* of 1904, later expanded into his very important *Vorlesungen über Variationsrechnung.* Bliss has followed in the footsteps of Bolza and has made substantial contributions to the subject; his students, M. R. Hestenes, and W. T. Reid, have done excellent work in the field. At Chicago there has been a systematic study of the interrelated general problems of Lagrange, Mayer, and Bolza.

Among the other younger men, E. J. McShane has been particularly concerned with existence theorems as derivable from the minimum principle itself.

The special interest of the author in the calculus of variations arose through the study of dynamical systems, since dynamical trajectories may be regarded in many cases as geodesics along which the arc length is an extremum. The problem of geodesics is indeed quite a typical one in the calculus of variations. In a paper written in 1918 he considered admissible curves made up of n short geodesic arcs, and showed that the problem of the extrema thus reduced that of the extrema of an ordinary function of n variables. He discussed not only the case of a minimum but also the case of a "minimax," in which the corresponding normalized quadratic form contains a single negative squared term. This new case was proved to be characterized by the fact that the first conjugate point A' of A along the extremal arc AB under consideration lies between A and B, while the second conjugate point lies beyond B. Furthermore, he established a simple interrelation between the number of minima and the minimaxes.

This work was then generalized powerfully by M. Morse, a central feature being his fundamental relations between the various *types* of the critical points, as characterized by the number of negative squared terms in the attached normalized quadratic form. The publication of Morse's Colloquium Lectures, *The Calculus of Variations in the Large*, in 1934, constitutes a landmark in the history of the subject. Morse's related oscillation and comparison theorems are a large extension of earlier theorems of this kind, and the purely geometric significance of his work is very considerable.

In 1935 Hestenes and the author took up the problem of characterization of all types of extremal arcs by introducing the notion of "natural isoperimetric conditions." A simple example is furnished by the closed geodesics on a convex surface in ordinary three-dimensional space, for any one of which the integral curvature on either side is necessarily 2π. Consequently, as Poincaré had noted, if we seek the simple closed curve of minimum length which divides the surface in two parts, of equal integral curvature, we obtain the desired closed geodesic. The auxiliary condition imposed is a special "natural isoperimetric condition," and the geodesic obtained is not of minimum type but of minimax type. Thus in a certain sense, the general case of an extremum can be reduced always to that of a maximum or minimum. This new approach leads in a simpler way to the results obtained by Morse and can readily be extended to multiple integrals.

From the purely abstract point of view the calculus of variations may be regarded as a critical point problem for a real function defined over a certain kind of function space. Morse has obtained significant results looking in this direction.

A very important special advance in the calculus of variations has been the solution of the famous problem of Plateau, to find the surface of minimum area bounded by one or more given closed curves. Jesse Douglas and Radó almost simultaneously solved the problem by different methods. Douglas used a boldly novel method in order to arrive at a very general result, while Radó employed a method of conformal mapping. The work of both of these men deserves the highest praise. Significant contributions to this problem were made by Courant and McShane also.

4. **Potential theory.** We have already alluded to the work of Bôcher and Kellogg, both ardent devotees of potential theory. It remains chiefly to bring to your attention some recent important advances concerning the celebrated Dirichlet problem of assigning arbitrary (continuous) boundary values to a harmonic function in the plane. If this assignment can be made for every choice of boundary values, the open region T under consideration is said to be "normal." The fact that the finite plane, less a single point, is not a normal region follows at once from Bôcher's result of 1903, that a bounded harmonic function cannot have an isolated singular point.

In dealing with an illuminating example of a normal region T in the plane having a perfect set of Borel measure 0 on a line as boundary, Kellogg in 1923 showed that, for any normal region T, any sequence of nested normal regions T_n approaching T with near-by boundary values defined a sequence u_n of potential functions which approached the solution u belonging to T. It was the especial merit of Wiener to establish in 1924 the fact that the u_n so obtained converges to a unique limiting function u, whether or not T is normal. Thus if T consists of the interior of a unit circle with the exception of its center taken as the origin, the harmonic function $(\log r)/(\log \epsilon)$ takes on the value 0 on the unit circle and the value 1 on the circle of radius ϵ. If ϵ tends to zero, we obtain a limiting potential function $u=0$, taking on the assigned values 0 on the unit circle but not the assigned value 1 at the origin.

In consequence of Wiener's result there is a unique "conductor potential" corresponding to the assignment of the boundary value 1 on any bounded set, from which we obtain Wiener's "capacity" of the set. Wiener showed how the question as to whether a region was normal or not admitted a theoretic answer in terms of the notion of capacity. For the actual answer in any case the method of "barrier functions" of Lebesgue seems to be equally serviceable.

The important question is to determine whether the boundary points are all "regular" or not; the test for regularity is equally as well that of the existence of a barrier function as it is the convergence of the critical series of Wiener, involving an infinite number of unknown capacities. If all the points are regular, and not otherwise, the region T is normal.

Evans has recently obtained the important result that regular points necessarily occur in three or more dimensions, as well as in the known case of two dimensions. In doing so he solved a problem to which Kellogg and other mathematicians here and abroad had given a great deal of attention.

5. **Fourier series and integrals.** In addition to the work of Bôcher on Gibbs' phenomenon previously noted, the Göttingen dissertation of Dunham Jackson (1911) may be mentioned first. This was an elegant contribution to the problem of the best approximation to a given function $f(x)$, $(0 \leqq x \leqq z\pi)$, by a trigonometric sum of order n

$$T_n(x) = \tfrac{1}{2}a_0 + \sum_{k=1}^{n} (a_k \cos\ kx + b_r \sin kx).$$

The first $n+1$ terms of the usual Fourier series minimize

$$\int_{\sigma}^{2\pi} [f(x) - T_n(x)]^2 dx.$$

Jackson showed that a better result (in a certain sense) would be obtained if the exponent 2 were replaced by 4. Later on he discussed the interrelation between the number of derivatives of $f(x)$ and the order of smallness of the coefficients in its Fourier series. His other work has for the most part been connected with problems of best approximation (in various senses) to a given function.

Wiener has obtained within a few years the remarkable result that if the Fourier series of a continuous non-vanishing function converges absolutely, the reciprocal function possesses also an absolutely convergent Fourier series (1934). As his books on Fourier integals (1933), and on Fourier transforms with the much lamented R. E. A. C. Paley, demonstrate, his work in these directions has been striking in quality and originality. He has also studied modified trigonometric series of the form

$$\sum_{n} (a_n \cos \lambda_n x + b_n \sin \lambda_n x)$$

and the functions which they represent. His work in the general field of harmonic analysis probably is unsurpassed. Mention should be made of the contributions to the general theory of summability of Fourier series by Hille and Tamarkin.

In conclusion it may be remarked that multiple Fourier series have been investigated by C. N. Moore, as well as questions concerning other multiple series and their summability. To one or both of these fields other significant contributions have been made by Adams, Bochner, Gergen, and others.

6. **Boundary-value and expansion problems.** As has been stated previously, it was Bôcher who first occupied himself seriously with boundary-

value problems, although it should be recalled that William Elwood Byerly's extremely useful book *Fourier's Series and Spherical Harmonics* had appeared in lithographed form in 1891. But Bôcher never occupied himself seriously with the deeper questions raised by the related infinite series, nor did any one else in our country. As a consequence when the author treated the one-dimensional boundary-value and expansion problems in his dissertation, he had never heard of Poincaré's celebrated paper of 1894, in which an analogous method of attack was used. Doubtless he ought to have made a more thorough search of the literature. However, the difference in degree of intractability between the problem for the potential equation, studied by Poincaré, and for an ordinary linear differential equation of the nth order with an arbitrary set of n linear boundary conditions, which he treated, was all in his favor. In fact he showed that basic asymptotic formulae could be established in the latter case; whereas in the former this has not been accomplished satisfactorily even today.

Briefly stated, the method was the following: (1) an asymptotic study of the solutions of the given linear differential equation; (2) then, by means of this, the asymptotic solution of the boundary-value problem for large characteristic numbers; and (3) the direct study of the related expansions in orthogonal or biorthogonal functions of $f(x)$ by means of the unfailing sum formula for n terms of the series,

$$\frac{1}{2\pi(-1)^{1/2}}\int_{C_n}\int_a^b G(x, t; \lambda)f(t)dtd\lambda,$$

where G is the explicitly known Green's function, λ is a complex parameter, and C_n is a contour in the λ-plane containing within it the characteristic values $\lambda_1, \cdots, \lambda_n$ for the boundary problem under consideration.

A year or two earlier Hilbert had established certain properties of these series in the very special real self-adjoint case by different methods.

The researches of Tamarkin, Stone, and Langer have added greatly to our knowledge of these interesting series. In particular Langer has shown how their properties may be discussed in the difficult case when the asymptotic form has a branch-point within the interval considered. This is connected with the so-called "Stokes phenomenon" of the physicists. Here the earlier interesting work of Max Mason, which utilized the methods of the calculus of variations, is to be recalled. Langer and the author have also studied the analogous boundary and expansion problems for a linear system of n equations of the first order; while Bliss has treated the same problem in what is essentially the real self-adjoint case, again from a decidedly different point of view.

The work of Tamarkin, and of Stone, has very much deepened our knowledge of these series in the "regular" case. When the conditions for regularity hold, the series are "equivalent" in a very deep sense to certain Fourier series. What can happen in the way of pathological behavior of these series, when the conditions of regularity are not satisfied, has been investigated by Stone, and quite recently by Langer, in the simplest case of the first order.

7. **Linear differential equations.** Bôcher was well acquainted with the field of linear differential equations in both real and complex variables. It was through his lectures that the author became interested in the theory of ordinary linear differential equations in the complex domain. In the two years at Chicago, he came to the conclusion that it was better to study a single matrix differential equation

$$Y'(x) = A(x)Y(x)$$

(that is, n equations of the first order) than a single linear differential equation of the nth order. Perhaps it was the conspicuous analogy between the first and nth order cases in this symbolic form which attracted me to the matrix notation.

The fundamental point of view was group-theoretic. Suppose that the singular point of the differential equation which is under consideration is taken to be at $x=\infty$, so that the elements $a_{ij}(x)$ of the square matrix $A(x)$ are analytic or have poles at $x=\infty$. It was then clear that the group of transformations

$$Y(x) = B(x)\overline{Y}(x), \quad x = \phi(\bar{x}),$$

where $B(x)$ is made up of elements analytic or with poles at $x=\infty$, with $|B|\not\equiv 0$, and where $\phi(x)/x$ is analytic and does not vanish at $x=\infty$, leaves unchanged the *essential* nature of the singularity in question. Hence the initial query of over thirty years ago took the form: Under this group of transformations of dependent and independent variables, what are the invariants? This problem was first attacked in the so-called general case, leaving aside those cases where unusual purely algebraic complications entered. These exceptional cases have recently been treated by W. J. Trjitzinsky by the same method which we jointly had found to be successful in the case of linear difference equations. The author also formulated and solved a generalized inverse Riemann problem, both in the neighborhood of a singular point and in the large.

The essential advance here was in obtaining a theory of the irregular singular point, although an extremely simple treatment of the regular singular point was found; it was this latter problem to which Fuchs had restricted his attention. In the case of only regular singular points, the Riemann problem had been solved by Hilbert and Plemelj by a different method.

8. **Linear difference equations.** Really significant American work on linear difference equations began with the dissertation of R. D. Carmichael at Princeton (1910). This marked an important advance; for under suitable hypotheses he showed the existence of a complete set of analytic

solutions possessing known asymptotic forms in the positive direction of the axis or equally in the negative direction. In the work immediately following, the author took a single matrix difference equation as fundamental,

$$Y(x+1) = A(x)Y(x),$$

and in general made similar restrictions on the roots of the characteristic equation. In this very suggestive matrix notation two formal solutions arose, namely

$$A^{-1}(x)A^{-1}(x+1) \cdots \qquad \text{and} \qquad A(x-1)A(x-2) \cdots .$$

This situation supplied the author with the cue for deriving two "principal matrix solutions" $Y_+(x)$ and $Y_-(x)$, of simple known asymptotic form in *all* directions. The formal matrix

$$\cdots A(x+1)A(x)A(x-1) \cdots$$

with elements of period 1 played an important rôle. Later was formulated and solved an inverse Riemannian problem. Full generality was obtained only recently, by demonstrating the existence of a full quota of asymptotic series and then taking up, in collaboration with Trjitzinsky, the most general case. Meanwhile C. R. Adams had made definite extensions of the first theory.

We have time only to mention the much simpler theory of the analogous q-difference equation

$$Y(qx) = A(x)Y(x),$$

first treated by Carmichael and by the author, and to refer to other important work in the field of linear difference equations by C. R. Adams, Carmichael, W. B. Ford, I. M. Sheffer, and K. P. Williams.

Very recently (1935) Trjitzinsky has obtained a complete theory for representation of solutions by means of factorial series and Laplace integrals. These were central in Nörlund's basic work on linear difference equations which was done a little earlier than the work of Carmichael and the author, although published later.

The program in these fields of linear differential equations and of linear difference equations has been to take as the unitary basis the square matrix of functions $Y(x)$ rather than the single function. The program has been illustrated further by obtaining the decomposition of a matrix of entire functions into an infinite matrix product of factors of simple type. This gave a generalization of the familiar Weierstrassian decomposition of a single entire function into an infinite product.

In the last year or two the author has been engaged in treating the still more general problems arising from one or more compatible equations in $Y(x)$ of the form

$$Y(\phi(x)) = A(x)Y(x),$$

where the elements of $A(x)$ are analytic or have poles at $x=\infty$ and where $\phi(x)$ has a pole of the first order there. It was begun with the extremely interesting case of order 1, that is, with a set of compatible linear functional equations of the type

$$y(\phi(x)) = a(x)y(x).$$

9. **Ordinary differential equations.** A piece of significant work in this field was the discussion by Osgood, early (1898) in his career, of the equation of the first order $y' = f(x, y)$. He assumed that f was merely continuous, and showed that the usual uniqueness theorem failed to hold, in which case there was an upper and lower solution passing through a point (x_0, y_0).*

An excellent study of existence theorems, in particular for ordinary differential equations, is contained in Bliss' Princeton Colloquium Lectures of 1909. In the Colloquium Lectures at New Haven in 1906, Mason gave an abbreviated general method in the linear case as follows. By suitable direct integration, convert the given differential equation into a functional equation

$$y = g + S(y), \qquad g \text{ known}.$$

If then the series

$$g + S(g) + S^2(g) \cdots$$

can be proved to converge uniformly, it furnishes the unique solution of the given differential equation. This method extends at once to n linear differential equations in n dependent variables $y_1, \cdots, y_n$. To apply this to the linear equation $y' = f(x, y)$ mentioned above, for example, we would first write it in the equivalent integral form

$$y(x) = y_0 + \int_{x_0}^{x} f(x, y(x))dx.$$

Of course, this convenient method really is nothing other than the classical method of successive approximations in somewhat more abstract form.

The work of Ritt on the polynomial differential equations $P(x; y, y', \cdots, y^{(n)}) = 0$ satisfied by algebraically transcendental functions has been of much importance. He has given a treatment of the totality of these functions in which the necessary algebraic considerations are for the first time given their proper weight. Our knowledge of the special functions so defined is still in a very rudimentary state. This fact is indicated by the two following important unsolved problems: (1) to determine whether or not any single-valued function $y(x)$, analytic throughout the x-plane ex-

* The phenomenon involved had been observed earlier by Peano who also gave an existence proof, a fact unknown to Osgood.

cept at k singular points x_i, $(i=1, \cdots, k)$, and satisfying near each such point a corresponding equation

$$P_i(x; y, y', \cdots, y^{(n)}) = 0,$$

where $P_i \not\equiv 0$ is polynomial in $y, \cdots, y^{(n)}$ with coefficients analytic in x at x_i, $(i=1, 2, \cdots, k)$, is necessarily algebraically transcendental; (2) to determine criteria in the case $n \geqq 2$ for analytic natural boundaries of the solutions.

As far as the work of the author in this field is concerned, aside from that on the differential equations of dynamics, we first mention the ideas which cluster around the "recurrent motions," which were first defined about twenty-five years ago. Here take the independent variable as the time t, and consider n real equations of the first order in $x_1, \cdots, x_n$. In any closed manifold of motions such recurrent motions will always exist. They have the characteristic property of filling their entire geometric locus in $x_1, \cdots, x_n$ space within distance ϵ in *any* sufficiently large interval of time. Every motion is either recurrent, or approaches indefinitely only to recede from a set of such recurrent motions. More recently the somewhat analogous "central motions" to which all other motions approach in the sense of time probability were defined.

These two concepts have already led to a number of further researches here and abroad. There are many open problems to be solved, of which three will be signalized: (1) In the analytic case, when there are no periodic motions in an invariant closed manifold, are the differential equations essentially reducible to

$$\frac{dx_i}{dt} = c_i, \qquad i = 1, \cdots, n,$$

where $x_1, \cdots, x_n$ are periodic variables of period 1? (2) In the n-dimensional analytic case, can the basic ordinal process by which the central motions are defined, ever involve more than n terms? (3) In the analytic case, does the set of central motions always admit of a *natural* invariant measure?

By use of surfaces of section first employed by Poincaré, it appears that the continuous transformations defined by a set of ordinary differential equations and the iteration of a particular continuous point transformation are very closely related. P. A. Smith and the author a number of years ago discussed the (1, 1) transformations of the surface of a sphere into itself. Hassler Whitney has proved recently that any continuous transformation without invariant points always gives rise to local surfaces of section. Within a year, Deane Montgomery and Leo Zippin have discussed certain nearly periodic continuous transformations. It may be safely predicted that the study of transformations of different types will lead to extensive further developments.

10. **Special analysis.** The term is here used merely for convenience to designate that part of analysis in which either simple explicit expressions are investigated or functions are implicitly defined by simple integral equations. In this domain are found Tauberian theorems and the theory of Fourier transforms to which Wiener has extensively contributed, and the theory of the Laplace transforms which Hille, Tamarkin, and especially Widder have studied to great advantage. Such work as that of C. N. Haskins on the moment problem and that of L. L. Silverman on the summability of divergent sequences would also fall under this head.

This extensive domain is more appreciated in Europe than it is in America, where we tend to take our mathematics as serious business rather than as a means of exercising our talent for free invention. In this connection it is well not to forget that many of the most astonishing mathematical developments began as a pure *jeu d'esprit.*

Geometry

In venturing upon a partial survey of American geometrical developments during these last fifty years, I feel embarrassed for two reasons. The first is my own severe limitation in the field. The second is a disturbing secret fear that geometry may ultimately turn out to be no more than the glittering intuitional trappings of analysis. At any rate the geometers are finding it more and more difficult to tell what the distinguishing mark of geometry really is. Thus in the Introduction of his *Treatise on Algebraic Plane Curves* (1931), J. L. Coolidge said that, for him, "geometry is nothing at all, if not a branch of art, and the underlying force which compels him to treat any particular topic, or to handle it in any particular way, is either that he is ignorant of any other, or else that his aesthetic sense dictates the choice: it pleaseth him to do so." In almost the same vein Veblen and J. H. C. Whitehead said a year later, in their Cambridge Tract on the *Foundations of Differential Geometry*, that a "branch of mathematics is called a geometry, because the name seems good on emotional and traditional grounds, to a sufficient number of competent people."

Whatever else such attitudes toward geometry may signify, they indicate the present lack of any program as convincing as was the famous group-theoretic Erlanger Programm of Klein, announced in 1872.

The uncertainty in point of view is largely due to two obvious causes. In the first place, the advent of Einstein's general theory of relativity made natural the surmise that all of physics might be looked at as a kind of extended geometry; this appeared most clearly in the general theory of gravitation for empty space, in which the world-lines of particles were simply the geodesics in a certain four-dimensional Riemannian geometry. Here was a first powerful suggestion that our geometrical ideas needed to be correspondingly enlarged. In the second place the extremely important and basic kind of geometry called analysis situs, in which the underlying group is formed by the general (1, 1) continuous point transformations, began to be properly appreciated. This further lessened the interest in classical geometric ideas.

As a result there have arisen two notable geometric movements. The first has led to a new theory of "generalized spaces"; and the second to an important development in the field of analysis situs. It is not too much to say that, from either a national or an international point of view, the Princeton group has been in the forefront in both of these directions. This was on the whole only to be expected, inasmuch as Eisenhart and Veblen were both at Princeton and among our most progressive geometers—likely to read aright the signs of the times, and to be among the leaders in any significant geometrical advance.

After these remarks, let us consider briefly the directions in which definite progress has been made.

In elementary geometry, Coolidge's well known work *The Geometry of the Circle and the Sphere* (1910) contained a good deal of value from his earlier researches, and has had an important influence in its field. Most of his results are obtained by means of the familiar "correspondence principle" which associates an appropriate system of homogeneous coördinates for a set of geometric objects (for example, circles, spheres, etc.) with the points of a corresponding projective space. The late C. L. E. Moore and also P. F. Smith have been active in the same domain. The *Projective Geometry* of Veblen and Young has been referred to earlier.

The more advanced field of algebraic geometry has been cultivated by A. B. Coble, A. Emch, T. R. Hollcroft, Lefschetz, F. R. Sharpe, C. H. Sisam, Virgil Snyder, and H. S. White. Unfortunately it is impossible here to attempt adequate reference to this field. Their work has been concerned largely with Cremona transformations and with some of the beautiful geometric configurations in which the subject abounds. Coble's algebraic interests have extended over a wide range.

Eisenhart is probably to be regarded as the first American who has achieved world standing in classical differential geometry. His work in the field has taken directions closely parallel to those of two great European masters of the subject, Bianchi and Darboux, and has achieved a definite place in this tradition. J. A. Eiesland, W. C. Graustein, and the late A. Ranum have also contributed valuable results.

It must be admitted, however, that there are few of our younger men who occupy themselves either with algebraic or classical differential geometry, or any other of the geometric questions which seemed most vital fifty years ago.

Closely associated with ordinary metric differential geometry is the projective differential geometry in which the projective group rather than the group of rigid motions plays the basic rôle. This is a subject with obvious claims to our interest, to which perhaps the late E. J. Wilczynski contributed more than anyone else. However, the first systematic study of the subject goes back to the French mathematician Halphen. A fundamental characteristic of Wilczynski's method is the association of an appropriately chosen set of linear differential equations with the geometric object under consideration. The simplest possible illustration, which may have served as a point of departure for Wilczynski, would be afforded by a curve (not a straight line) in the projective plane,

$$y_1 = y_1(x), \qquad y_2 = y_2(x), \qquad y_3 = y_3(x).$$

This may be associated with the differential equation having the three functions y_1, y_2, y_3 as (linearly independent) solutions,

$$y''' + p_1y'' + p_2y' + p_3y = 0$$

which may be termed the "differential equation of the curve." This equation is evidently unaltered by any projective change of coördinates. But the multiplicative transformation $y=\lambda\bar{y}$ does not change the curve, and if we choose λ so that $3\lambda'+p_1\lambda=0$, we obtain a like equation with $\bar{p}_1=0$, and with

$$\bar{p}_2 = p_2 - p_1^2 - p_1', \qquad \bar{p}_3 = p_3 - 3p_1p_2 + 2p_1^3 - p_1''.$$

Here $\bar{p}_2$ and $\bar{p}_3$ are the two "semi-invariants" of the curve, that is, the invariants of the parametrized curve. To obtain the single intrinsic invariant of the curve itself we have to combine the above transformation with a suitable change of the parameter x.

In extending this idea to ruled surfaces in three-dimensional space, for example, Wilczynski used two ordinary linear differential equations of the second order in two variables y and z,

$$y'' + p_{11}y' + \cdots + q_{12}z = 0, \qquad z'' + p_{21}y' + \cdots + q_{22}z = 0,$$

as the "differential equations of the ruled surface." Here the four coördinates of the surface have the form $cy_c(x)+dz_c(x)$, and the two parameters of the surface are c/d and x, of which x specifies the particular ruling. Such is the kind of analytical apparatus which he employed in his investigations of various questions in projective differential geometry.

Another American geometer who has shown much originality and has obtained various elegant results is Edward Kasner. He has particularly studied the invariant theory and the associated geometric characterization of families of dynamical trajectories, and the formal aspects of conformal geometry in the plane. For example, the following is a problem of conformal geometry which Kasner has treated to advantage. What are the (formal) invariants of two analytic curves at a point of intersection? The most simple invariant is, of course, $x_1'y_2'-x_2'y_1'$ if (x_1, y_1) and (x_2, y_2) are the coördinates of the two curves expressed in terms of the arc length. This invariant represents the tangent of the angle between the curves.

Kasner obtains the higher invariants as well, and answers (formally) various interesting questions such as that of the conformal bisector of an angle. His desire is above all for elegance and extreme simplicity, combined with essential novelty.

Despite this auspicious entry into these fields of projective differential geometry and the geometry of dynamical trajectories, the two fields have not been very active. G. M. Green, who died very young, did brilliant work in the field of projective differential geometry about twenty years ago. At present E. P. Lane of Chicago is following ably in the tradition begun by Wilczynski. Perhaps the most able student of Kasner's who has concerned himself with the geometry of trajectories was the late Joseph Lipka.

Let us turn now to the development of the theory of generalized spaces, which has its roots in the highly important notion of "parallel displacement" of Tullio Levi-Civita (1917). A year later Weyl proposed to take this new notion of parallel displacement as the basis of an extended geometry of non-Riemannian type. The starting point is afforded by the affine equations of parallel displacement which tell how neighboring, nearly parallel vectors are related to one another. Obviously a fundamental system of curves, taking the place of geodesics in the Riemannian theory, are those along which the tangent line is displaced parallel to itself. In 1921 Weyl pointed out that these curves did not suffice to identify completely the "symmetric affine connection" and thus he was led to a "projective" theory for the new geometry.

Very shortly thereafter (1922) Eisenhart and Veblen wrote a suggestive note in which the paths themselves were taken as fundamental. The differential equations of the paths is taken in the familiar geodesic form

$$\frac{d^2x^i}{ds^2} + \Gamma^i_{\alpha\beta}\frac{dx^\alpha}{ds}\frac{dx^\beta}{ds} = 0$$

where, however, the Christoffel 3-index symbols Γ^i_{jk} are to be looked upon as arbitrary functions. Actually the geometry thus arrived at turns out to be identical with that of Weyl in his projective theory of affine symmetric connection. Other interesting papers appeared soon by Eisenhart, T. Y. Thomas, Veblen, and others at Princeton, in which various significant results were obtained. In particular, Thomas proved in 1925 that generalized projective spaces were naturally associated with a uniquely determined affine connection, and obtained also a similar result in the conformal case which had been signalized by Weyl. As Veblen had pointed out in 1922, a suitable generalization of the normal coördinates of Riemann lends itself very well to the study of the invariants in the new generalized spaces.

Along with these American advances, much was done abroad, particularly by Emile Cartan in France and J. A. Schouten in Holland. The most recent account of the subject is given in Thomas' book *The Differential Invariants of Generalized Spaces*.

What is required, apparently, in order to validate the especial importance of the new generalized spaces, is either to show their usefulness for theoretical physics or to construct some interesting non-Riemannian cases.

Finally, we turn to the field of analysis situs, long a Cinderella in the geometrical family.

It was Veblen's readable and illuminating Cambridge Colloquium Lectures of 1916 on *Analysis Situs*, published in 1921, which served more than anything else to stimulate the remarkable activity that ensued here and abroad. Veblen concerned himself entirely with the combinatorial aspects of the subject, dealing with the manifolds made up of cells of various dimensions which Poincaré had discussed in his celebrated five papers on analysis situs (1895–1904). Thus the point-set-theoretic side of the subject and applications found no place in his book. Veblen diverged from Poincaré in that his incidence relations, etc., were all at first taken modulo 2.

With Veblen's name should be joined that of Alexander, as furnishing a strong support in the movement towards analysis situs. As early as 1911, Veblen and Alexander, then a graduate student, began to be especially interested in the subject. And before the Cambridge Lectures were given, Alexander had published an article establishing that the combinatorial results obtained were independent of the particular mode of cellular subdivision. His well known "duality theorem," his contributions to the theory of knots, and various other results, have made him a particularly important worker in the field. His papers are notable for their elegance and sustained high quality.

In more recent years Lefschetz has also occupied himself with varied questions in the field of combinatorial analysis situs, often in close relation with Alexander. Perhaps his work on fixed point theorems for n-dimensional manifolds is best known. A simple illustrative case of such a theorem is the fact that any (1, 1) sense-preserving transformation of the sphere into itself leaves at least one point fixed. More generally, it is easily proved that on a surface of any genus $p \neq 1$ there is at least one such fixed point if the given 1–1 transformation belongs to the same "class" as the identity in the sense of Brouwer, that is, can be obtained by continuous deformation from the identity. In fact the subdivision into 2-cells shows that there will be a total index of $2-2p$ for all the fixed points, so that there will be at least 2 distinct fixed points for $p=0$ and $2p-2$ fixed points for $p=2, 3, \cdots$ provided that the fixed points are simple. To arrive at such a result in Lefschetz's n-dimensional case as well as in the two-dimensional case, it is sufficient (heuristically speaking) to make the count in a single case. Thus for the sphere it suffices to note that a rotation has two fixed points of index 1. It is Lefschetz's merit to have evaluated the index sum in very general cases, and thus to have arrived at his general fixed point theorems.

Of the other men in the very strong group in analysis situs at Princeton and at the Institute for Advanced Study, Morse has been interested in certain questions in space of infinitely many dimensions which arise in connection with the calculus of variations and his critical point relations; and A. W. Tucker has contributed towards a better abstract foundation of the subject.

Among American workers elsewhere, Hassler Whitney has contributed to the theory of graphs and of differentiable manifolds. In his important work on graphs he establishes incidentally the following simple entertaining result: In any possible map on a sphere in which three countries at most meet at any point and no two or three countries form a ring, a traveler may visit all of the countries in succession without entering the same country twice.

But it was R. L. Moore who foresaw the possibilities of the point-set-theoretical side of analysis situs some thirty years ago, and who has added most to it. Single-handed he gathered around him students who have entered the field and have done work which is also of much importance. Among them may be mentioned W. L. Ayres, H. M. Cehman, J. R. Kline, G. T. Whyburn, and R. L. Wilder. There arose subsequently in Poland a second notable mathematical school with similar interests. A striking fact about the advance thus made is the following. As was first seen by Poincaré, theoretical dynamics leads immediately to an extraordinary variety of point-set-theoretic questions of very fascinating type. Somehow or other, guided by aesthetic sensibility alone, these mathematicians have formulated some of the questions of most interest to dynamics. An instance of this sort is R. L. Moore's "upper limiting sets" which have recently been found to be of central importance for the theory of dynamical systems with two degrees of freedom. In fact, there arose in this theory a division of the ordinary plane into simply connected closed sets, no two sets having a point in common. Thus with any point p was associated a corresponding set Σ_p; it is proved that if the points $p_1, p_2, \cdots$ approach a limit point p, then the corresponding sets $\Sigma_{p_1}, \Sigma_{p_2}, \cdots$ approach the immediate neighborhood of Σ_p. If now the elements Σ_p are thought of as points in a nonmetrical two-dimensional continuum, there arise the upper limiting sets envisaged by Moore.

As another illustration of the interrelation of analysis situs and dynamics, let us refer to Poincaré's last geometric theorem established in 1912 by the author. This is the following theorem about fixed points: If a (1, 1) direct, area-preserving transformation of a region bounded by two concentric circles into itself, advances points on one circle in the clockwise sense and on the other in the opposite sense, there will be two invariant points. This conjectured theorem led Poincaré to the conclusion that infinitely many periodic motions exist in the restricted problem of three bodies and similar dynamical problems. The author was able later on to give this interesting theorem a nonmetric form, and to show that there are always two geometrically distinct invariant points. No proper analogue of Poincaré's theorem has been found for spaces of higher dimensions.

So far that mysterious curiosity of analysis situs, the four-color problem, has not been mentioned. Philip Franklin, C. N. Reynolds, and the author have studied this most carefully. Franklin has very recently extended his previous results to show that any map on the sphere of at most 31 regions can be colored in four colors.

Applied mathematics

In default of a better term we use the designation of applied mathematics for that large part of mathematics which seems to be closely connected with physics or some other branch of science. Inasmuch as most of the so-called "pure" mathematics of the present day was at one time "applied," the term is a very vague one. Nevertheless, the field of applied mathematics always will remain of the first order of importance inasmuch as it indicates those directions of mathematical effort to which nature herself has given approval.

Unfortunately, American mathematicians have shown in the last fifty years a disregard for this most authentically justified field of all. It was remarked at the outset that the American tradition was at first of quite the opposite character. Nevertheless today we recall only six Americans who are deeply concerned with applied mathematics in the usual sense, of whom four were brought up in the great British tradition. These are Harry Bateman, Ernest W. Brown (recently deceased), F. D. Murnaghan, H. P. Robertson, J. L. Synge, and R. C. Tolman. Among these men it should be remarked that Brown was the world's foremost lunar theorist, while Tolman is to be regarded as primarily a physical chemist. All six men possessed an extremely broad scientific outlook. The names of Bateman and Tolman will always be mentioned among those who were closest in spirit to the special theory of relativity at the time of its discovery. Furthermore, Bateman has added to classical electromagnetic theory, while Tolman has contributed to the relativistic theory of the expanding universe in which he has shown his daring speculative spirit. Robertson has also contributed in the same relativistic direction. Murnaghan and Synge alike have been creatively interested in geometry, dynamics, classical hydrodynamics and elasticity, and relativity.

Much of the work of the author has also been in the direction of applied mathematics of a somewhat different type—the problem of three bodies and its special cases, qualitative dynamics, the foundations of electrodynamics, relativity, and quantum mechanics. It was the well known work of F. R. Moulton on the periodic solutions of the restricted problem of three bodies which first attracted the attention of American mathematicians to the fundamental advances of Poincaré. W. D. MacMillan has made significant advances in this field. It may perhaps be permitted to state one result in the theory of relativity, discovered independently later by Eiesland, namely that any spherically symmetric solution of the Einstein field equations is necessarily static. This result is of importance in the relativisitic theory of the expanding universe. It has been extended by Banesh Hoffmann.

In this connection Veblen's interesting work on five-dimensional generalized projective spaces should be mentioned. Here as elsewhere the fifth dimension affords a convenient bracket with which to provide for the electromagnetic equations as well as those of gravitation. Let us mention also Eisenhart's elegant observation that in the general theory of relativity a particle attracted by a body of finite mass moves as if directly attracted toward it in accordance with the Newtonian Law, with a variable central mass equal to the natural (rest) mass m increased by precisely $3m\omega^2$, where ω is the angular velocity in light seconds.

In concluding this cursory account of applied mathematics, mention must be made of one development which has been of extraordinary value for mathematics and for statistical mechanics, namely the further development of ergodic theory.

The well known recurrence theorem of Poincaré was stated by him in the following form. The *probability* that a closed dynamical system recurs arbitrarily closely to any initial state is 1. This kind of probability is to be interpreted in the sense of Lebesgue measure, as was first remarked by E. B. Van Vleck. The "ergodic theorem" in its final form affirms that (except for cases of probability 0) this recurrence occurs in a *metrically habitual manner*. For example, imagine an idealized billiard ball moving upon a convex billiard table. According to the theorem, there will then be a limiting mean time-interval between collisions, a mean distance between collisions, a mean angular rotation of the successive directions, a mean part of the time in which the ball is on any assigned part of the table, etc., etc. The proof which we gave involves an essentially new algorithm. The theorem itself is one of the most remarkable in dynamics. It justifies the physical intuitions of Maxwell and Boltzmann in their celebrated ergodic hypothesis, and goes far towards supplying a rigorous foundation for statistical mechanics.

What needs still to be done is to establish "metric transitivity" in the general case. G. A. Hedlund has succeeded in doing so under certain special conditions. The problem is an extremely difficult one.

In the sequence of ideas which led to the discovery of this theorem in 1931 several American mathematicians played a vital part. P. A. Smith and the author first defined the basic notion of metric transitivity. B. O. Koopman, who has always had a broad understanding of mathematical-physical ideas, then showed how to restate the basic transformation problems of dynamics as problems concerning unitary linear transformations in Hilbert space. This interpretation stimulated von Neumann, possessed of wide interests in mathematical physics as well as of an outstanding technique in the theory of Hilbert space, to establish a "mean ergodic theorem" which, however, affirmed nothing about any individual motion. At about the same time the great Swedish mathematician Carleman proved the same theorem independently. Finally, it was the stimulus of personal contact with von Neumann and Koopman, together with extensive personal experience in the difficult problem of stability in dynamics, that led the author to the proof of the ergodic theorem itself; for he suddenly saw that the essential defect in some earlier ideas was that he had used continua where he should have used measurable sets. One other name should be mentioned in connection with the ergodic theorem, that of Eberhard Hopf, now in Leipzig but then in Cambridge. Hopf immediately improved the reasoning of von Neumann, and has contributed more than any one else to the extensions of the ergodic theorem which are suggested by the applications. It is interesting to remark that Hopf has added to theoretical astrophysics as well as to other fields of mathematics.

The fact that the ergodic theorem is destined to be a fundamental theorem of Lebesgue measure theory is clear from the applications to analysis which have already been made by Wiener, Wintner, and others in this country, and by a number of mathematicians abroad. Allusions may well be made here to Wiener's researches in random functions, correlated with the phenomena of white light and the Brownian movement in physics, and to his work on the Fourier transforms which has application in questions of electrical filtering. Wintner has established the convergence of the infinite processes used by Hill in his lunar theory, and has done valuable work in analysis, theoretical dynamics, and quantum mechanics.

In connection with the ergodic theory it is natural to mention the related active fields of probability and statistical theory, in which J. L. Doob and A. H. Copeland have been applying the modern methods.

This must conclude my survey of the splendid accomplishments of American mathematics from our beginning as an organized Society in 1888 until the present day. I have felt as a traveler in a beautiful and unexplored country might feel who had taken his companions to some vantage points familiar to him so that they might enjoy the prospects which he happened to know, all the while realizing that on the morrow they would journey together towards more grandiose mountain peaks glittering along the horizon.

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