Leroy Powell Steele, a graduate of Harvard College (B.A., 1923), died January 7, 1968, and bequeathed the bulk of his estate to the American Mathematical Society to be used for the award from time to time of prizes in honor of George David Birkhoff, William Fogg Osgood and William Caspar Graustein. The twenty-second, twenty-third and twenty-fourth Steele Prizes, each of which is worth fifteen hundred dollars, were presented at the prize session at the Summer Meeting of the Society in Pittsburgh, August 20, 1981.

Steele Prizes are awarded annually in three categories:

(1) For the cumulative influence of the total mathematical work of the recipient, high level of research over a period of time, particular influence on the development of a field, and influence on mathematics through Ph.D. students. The 1981 recipient is Oscar Zariski.

(2) For a paper, whether recent or not, which has proved to be of fundamental or lasting importance in its field, or a model of important research. The 1981 recipient is Eberhard Hopf.

(3) For a book or substantial survey or expository-research paper. The 1981 award was made to Nelson Dunford and Jacob T. Schwartz jointly.

The prizes were awarded by the Council of the American Mathematical Society, based on recommendations made by the Committee on Steele Prizes, consisting of Robin Hartshorne, Reuben Hersh, M. D. Kruskal, Louis Nirenberg, Henry O. Pollak, Alex Rosenberg, Gian-Carlo Rota, Max M. Schiffer, and Gail S. Young, Jr. (chairman).

Professors Zariski, Dunford and Hopf were unable to attend the summer meeting to receive the prizes in person, and sent written responses to their awards. The text below contains the Committee's citations for the three prizes, the responses, and biographical sketches of the recipients.

CITATION FOR STEELE PRIZE FOR CUMULATIVE INFLUENCE:

The Steele Prize for cumulative effect on mathematics or very high level of work over a long period of time is awarded to Oscar Zariski of Harvard University (Emeritus) for his work in algebraic geometry, especially for the many ways he has made fundamental contributions to the algebraic foundations of this subject.

After beginning his work in Italy in 1924 very much in the style of "Italian algebraic geometry," Zariski realized that the whole subject needed proper foundations. Thus in the period 1927 to 1937 he turned first to topological questions and then in 1937 he began to lay the commutative algebraic foundations of his subject. His topological work concentrated mainly on the fundamental group; many of the ideas he pioneered were innovations in topology as well as algebraic geometry and have developed independently in the two fields since then.

In 1937 Zariski completely reoriented his research and began to introduce ideas from abstract algebra into algebraic geometry. Indeed, together with B. L. van der Waerden and André Weil, he completely reworked the foundations of his subject without the use of topological or analytical methods. His use of the notions of integral dependence, valuation rings, and regular local rings, in algebraic geometry proved particularly fruitful and led him to such high points as the resolution of singularities for threefolds in characteristic 0 in 1944, the clarification of the notion of simple point in 1947, and the theory of holomorphic functions on algebraic varieties over arbitrary ground fields. The theory of equisingularity and saturation begun by Zariski in 1965 has also been of great influence and importance.

All of Zariski's work has served as a basis for the present flowering of algebraic geometry and the current school uses his work and ideas in the modern development of the subject.

Zariski has also profoundly influenced, either as a teacher or as a colleague, a large number of outstanding algebraic geometers of which we only cite S. Abhyankar, M. Artin, I. S. Cohen, H. Hironaka, S. Kleiman, J. Lipman, H. Muhly, D. Mumford, M. Rosenlicht, and A. Seidenberg.

Oscar Zariski

I feel very honored and happy to have been awarded a Steele Prize by the Council of the Society. I regret being unable to attend the meeting at which this award will be announced. However, I am glad to give, as my response, an outline of the main features of my work up to date. This outline is an abbreviated version of my preface to the fourth volume of my collected papers (published by the Massachusetts Institute of Technology Press).

Beginning with the year 1937 the nature of my work underwent a radical change. It became strongly *algebraic* in character, both as to methods used and as to the very formulation of the problem studied (these problems, nevertheless, always have had, and never ceased to have in my mind, their origin and motivation in algebraic *geometry*). A few words on how this change came about may be of some interest to the members of the Society. When I was nearing the age of 40, the circumstances that led me to this radical change of direction in my research (a change that marked the beginning of what was destined to become my chief contribution to algebraic geometry) were in part personal in character, but chiefly they had to do with the objective situation that prevailed in algebraic geometry in the 1930s.

In my early studies as a student at the University of Kiev in the Ukraine, I was interested in algebra and also in number theory (by tradition, the latter subject is strongly cultivated in Russia). When I became a student of the University of Rome in 1921, algebraic geometry reigned supreme in that university. I had the great fortune of finding there on the faculty three great mathematicians, whose very names now symbolize and are identified with classical algebraic geometry: G. Castelnuovo, F. Enriques, and F. Severi. Since even within the classical framework of algebraic geometry the algebraic background was clearly in evidence, it was inevitable that I should be attracted to that field. For a long time, and in fact for almost ten years *after* I left Rome in 1927 for a position at the Johns Hopkins University in Baltimore, I felt quite happy with the kind of "synthetic" (an adjective dear to my Italian teachers) geometric proofs that constituted the very life stream of classical algebraic geometry (Italian style). However, even during my Roman period, my algebraic tendencies were showing and were clearly perceived by Castelnuovo, who once told me: "You are here with us but are not one of us." This was said not in reproach but good-naturedly, for Castelnuovo himself told me time and time again that the methods of the Italian geometric school had done all they could do, had reached a dead end, and were inadequate for further progress in the field of algebraic geometry. It was with this perception of my algebraic inclination that Castelnuovo suggested to me a problem for my doctoral dissertation, which was closely related to Galois theory.

Both Castelnuovo and Severi always spoke to me in the highest possible terms of S. Lefschetz's work in algebraic geometry, based on topology; they both were of the opinion that topological methods would play an increasingly important role in the development of algebraic geometry. Their views, very amply justified by future developments, have strongly influenced my own work for some time. This explains the topological trend in my work during the period 1929 to 1937. During that period I made frequent trips from Baltimore to Princeton to talk to and consult with Lefschetz, and I owe a great deal to him for his inspiring guidance and encouragement. My work during that period dealt primarily with the following three topics: (1) solvability in radicals of equations of certain plane curves; (2) the fundamental group of the residual space of plane algebraic curves; (3) the topology of the singularities of plane algebraic curves.

The breakdown (or the breakthrough, depending on how one looks at it) came when I wrote my Ergebnisse monograph *Algebraic Surfaces*. At that time (1935) modern algebra had already come to life (through the work of Emmy Noether and the important treatise of B. L. van der Waerden), but while it was being applied to some aspects of the foundations of algebraic geometry by van der Waerden, in his series of papers *Zur algebraischen Geometrie*, the deeper aspects of *birational* algebraic geometry (such as the problem of reduction of singularities, the properties of fundamental loci and exceptional varieties of birational transformations, questions pertaining to complete linear systems and complete "continuous" systems of curves on surfaces, and so forth) were largely, or even entirely, virgin territory as far as algebraic exploration was concerned. In my Ergebnisse monograph I tried my best to present the underlying ideas of the ingenious geometric methods and proofs with which the Italian geometers were handling these deeper aspects of the whole theory of surfaces, and in all probability I succeeded, but at a price. The price was my own personal loss of the geometric paradise in which I so happily had been living. I began to feel distinctly unhappy about the rigor of the original proofs I was trying to sketch (without losing in the least my admiration for the imaginative geometric spirit that permeated these proofs); I became convinced that the whole structure must be done over again by purely algebraic methods. After spending a couple of years just studying modern algebra, I had to begin somewhere, and it was not by accident that I began with the problem of local uniformization and reduction of singularities. At that time the Ergebnisse monograph *Idealtheorie* of W. Krull appeared, emphasizing valuation theory and the concept of integral dependence and integral closure. Krull said somewhere in his monograph that the *general* concept of valuation (including, therefore, nondiscrete valuations and valuations of rank >1) was not likely to have applications in algebraic geometry. On the contrary, after some trial tests (such as the valuation-theoretic analysis of the notion of infinitely near base points), I felt that this concept could be extremely useful for the analysis of singularities and for the problem of reduction of singularities. At the same time I noticed some promising connections between integral closure and complete linear systems: a systematic study of these latter connections later led me to the notions of normal varieties and normalization. However, I also concluded that this program could be successful only provided that much of the preparatory work be done for ground fields that are not algebraically closed. I restricted myself to characteristic zero: for a short time, the quantum jump to $p \neq 0$ was beyond the range of either my intellectual curiosity or my newly acquired skills in algebra; but it did not take me too long to make that jump; see especially several papers which I published in the years 1943 to 1947.

I carried out this initial program of work primarily in four papers published in 1939 and 1940. From then on, for more than thirty years, my work ranged over a wide variety of topics in algebraic geometry. I shall now indicate briefly the main topics of this work.

(A) *Foundations*, meaning primarily properties of normal varieties, linear systems, birational transformations, and so on.

(B) *Local uniformization and resolution of singularities*.

These two subdivisions correspond precisely to the twofold aim I set to myself in my first concerted attack on algebraic geometry by purely algebraic methods — an undertaking and a state of mind about which I have already said a few words earlier.

Oscar Zariski

(C) *The theory of formal holomorphic functions on algebraic varieties* (in any characteristic), meaning primarily analytic properties of an algebraic variety V, either in the neighborhood of a point (strictly *local* theory) or — and this is the deeper aspect of the theory — in the neighborhood of an algebraic subvariety of V (semi-global theory).

(D) *The Riemann-Roch theorem and applications* (again in any characteristic), the applications being primarily to algebraic surfaces (minimal models, characterization of rational or ruled surfaces, etc.).

(E) *The theory of equisingularity.*

My work on formal holomorphic functions was a natural outgrowth of my previous work on the local theory of singularities and their resolution. In the course of this previous work I developed an absorbing interest in the formal aspects of Krull's theory of local rings and their completions. In particular, I gave much thought to the possibility of extending to varieties V over arbitrary ground fields the classical notion of analytic continuation of a holomorphic function defined in the neighborhood of a point P of V. I sensed the probable existence of such an extension provided the analytic continuation were carried out along an algebraic subvariety W of V passing through P. It was wartime, and my heavy teaching load at Johns Hopkins University (18 hours a week) left me with little time for developing these ideas. Fortunately I was invited in January 1945 to spend at least one year at the University of São Paulo, Brazil, as exchange professor under the auspices of our Department of State. My light teaching schedule at São Paulo gave me the necessary leisure time to concentrate on an abstract theory of holomorphic functions. The year spent at São Paulo also presented me with a superlative audience consisting of one person — André Weil (who spent two or three years in São Paulo) — to whom I could speak about these ideas of mine during our frequent walks. The full theory of holomorphic functions — in the difficult case of complete (projective) varieties — was developed by me in AMS *Memoirs* Number 5 (1951). However, the germ of this theory, in the easier case of affine varieties, appears already in my 1946 paper written and published in Brazil. The key ingredient of the theory developed in this earlier paper is the concept of certain special rings, which later were named "Zariski rings," and properties of the completion of these rings. It is also this earlier Brazilian paper that led me to the discovery of a connection between the general theory of holomorphic functions and the connectedness theorem on algebraic varieties (and, in particular, the so-called principle of degeneration of Enriques).

The theory of equisingularity, which I initiated with two papers in 1965 and a third paper in 1968, under the common title "Studies in equisingularity" and on which I and other mathematicians continued to work up to the present time, is still largely an open field, and a definitive and convincing complete general theory of equisingularity is still not available, at least not in print. In a final paper published in 1979 I developed a general theory of equisingularity. By a "general" theory, I mean one which is based on a "satisfactory" definition of equisingularity of a given r-dimensional variety V (of embedding dimension $\leq r+1$, locally at each of its points; in particular, of a hypersurface V) along any irreducible subvariety W of V, of codimension > 1, i.e., such that dim $W < r-1$. (The case of codimension 1 was completely settled in my paper published in the *American Journal of Mathematics* in 1965.) By a "convincing" or "satisfactory" general theory of equisingularity, I mean one which, in the first place, is not contradicted, sooner or later, by counter-examples, and, in the second place, agrees with what one would expect from equisingularity when tested in examples against the behavior of V under a monoidal transformation centered in W.

In my student days in Rome algebraic geometry was almost synonymous with the theory of algebraic surfaces. This was the topic on which my Italian teachers lectured most frequently and in which arguments and controversy were also most frequent. Old proofs were questioned, corrections were offered, and these corrections were — rightly so — questioned in their turn. At any rate, the general theory of algebraic surfaces was very much on my mind in subsequent years, as witnessed — on an expository level — by my Ergebnisse monograph on algebraic surfaces, and — on a more significant research level — by the connection which I have found exists for varieties V of any dimension between normal (respectively, arithmetically normal) varieties and the property that the hypersurfaces of a sufficiently high order (respectively, of all orders) cut out on V complete linear systems. With this result as a starting point and with the conviction, indelibly impressed in my mind by my Italian teachers, that the theory of algebraic surfaces is the apex of algebraic geometry, it is no wonder that as soon as I realized that further progress in the problem of resolution of singularities would probably take years and years of further effort on my part, I decided that it was time for me to come to grips with the theory of algebraic surfaces. I felt that this would be the real testing ground for the algebraic methods which I had developed earlier. This work was also, in part, an answer to the following challenge sounded by Castelnuovo in his 1949 introduction to the treatise *Le superficie algebriche* of Enriques: "Verrà presto il continuatore dell'opera delle scuole italiana e francese il quale riesca a dare alla teoria delle superficie algebriche la perfezione che ha raggiunto la teoria delle curve algebriche?" (Note Castelnuovo's answer: "Lo spero ma ne dubito.") The theory of surfaces is still a very lively topic of research and everything points to the likelihood that this theory will reach the degree of perfection dreamed of by Castelnuovo, except that this will not be the work of one "continuatore," but of many.

In 1950 I gave a lecture at the International Congress of Mathematicians at Harvard; the title of that lecture was *The fundamental ideas of abstract algebraic geometry*. This lecture is a good illustration of how relative in nature is what we call "abstract" at a given time. Certainly that lecture was very "abstract" for *that* time when compared with the reality of the Italian geometric school. Because it dealt only with projective varieties, that lecture, viewed at the present time, after the great generalization of the subject due to Grothendieck, appears to be a very, very concrete brand of mathematics. There is no doubt that the concept of "schemes" due to Grothendieck was a sound and inevitable generalization of the older concept of "variety" and that this generalization has introduced a new dimension into the conceptual content of algebraic geometry. What is more important is that this generalization has met what seems to me to be the true test of any generalization, that is, its effectiveness in solving, or throwing new light on, old problems by generalizing the terms of the problem (for example: the Riemann-Roch theorem for varieties of any dimension; the problem of the completeness of the characteristic linear series of a complete algebraic system of curves on a surface, both in characteristic zero and especially in characteristic $p \neq 0$; the computation of the fundamental group of an algebraic curve in characteristic $p \neq 0$).

But a mathematical theory cannot thrive indefinitely on greater and greater generality. A proper balance must ultimately be maintained between the generality and the concreteness of the structure studied, and usually this balance is restored after a period in which it was temporarily (and understandably) lost. There are signs at the present moment of the pendulum swinging back from "schemes," "motives," and so on, toward concrete but difficult unsolved questions concerning the old pedestrian concept of a projective variety (and even of algebraic surfaces). There is no lack of such problems. It suffices to mention such questions as (1) criteria of rationality of higher varieties; (2) the study of cycles of codimension > 1 on any given variety; (3) even for divisors D on a variety there is the question of the behavior of the numerical function of n: $\dim |nD|$; and finally (4) problems, such as reduction of singularities or the behavior of the zeta function, which are still unsolved when the ground field is of characteristic $p \neq 0$ (and is respectively algebraically closed or a finite field). These are new tasks that face the younger generation; I wholeheartedly wish that generation good speed and success.

From *Oscar Zariski: Collected Papers*, volume 4, © M.I.T. Press 1978, Cambridge, Massachusetts.

Biographical Sketch

Oscar Zariski was born April 24, 1899 in Kobrin, Russia. He studied at the University of Kiev and at the University of Rome, where he earned his doctoral degree in mathematics in 1924.

He was a fellow of the International Education Board at the University of Rome, 1925 to 1927, and a Johnston Scholar at Johns Hopkins University, 1927 to 1929. From 1929 to 1945, he was a member of the faculty at Johns Hopkins. From 1947 to 1969, he was professor of mathematics at Harvard (Dwight Parker Robinson Professor from 1961 to 1969). He retired as professor emeritus in 1969.

He was a visiting member of the Institute for Advanced Study (1935-1936, 1939, 1960-1961), lecturer at the University of Moscow (1936), Guggenheim Fellow (1939-1940), visiting professor at São Paulo (1945), professor at the University of Illinois (1946-1947), lecturer at the University of Cambridge (1972), lecturer at the University of Kyoto (1956), and member of the Institut des Hautes Études Scientifiques in 1961 and 1967.

Professor Zariski received the Society's Cole Prize in Algebra in 1944 and the National Medal of Science in 1965. He was a member of the *Transactions* editorial committee (1941 to 1947) and served as vice president of the AMS in 1960 and 1961 and as president in 1969 and 1970. He was the Society's representative on the National Research Council (Division of Physical Sciences) from 1949 to 1952. He has served on the boards of the *American Journal of Mathematics*, the *Annals of Mathematics* and the *Illinois Journal of Mathematics*.

Professor Zariski gave invited addresses at several AMS meetings: March 1934, New York (*Some new aspects of the theory of plane algebraic curves*); December 1941, Bethlehem, Pennsylvania (*Normal varieties and birational correspondences*); January 1962, Cincinnati (*The present state of the problem of reduction of singularities*). He presented the Colloquium Lectures in New Haven in September 1947 (*Abstract algebraic geometry*) and his Retiring Presidential Address at the annual meeting in Atlantic City, January 1971 (*Some open questions in the theory of singularities*).

He was elected to the Accademia Nazionale dei Lincei (1958), the Academia Brasileira de Ciéncias (1958), the American Philosophical Society held at Philadelphia (1951), the American Academy of Arts and Sciences (1948), and the U.S. National Academy of Sciences (1944). He received honorary Doctor of Science degrees from the College of the Holy Cross (1959), Brandeis University (1965), Purdue University (1973) and Harvard University (1981).

CITATION FOR STEELE PRIZE FOR WORK OF FUNDAMENTAL OR LASTING IMPORTANCE:

The Steele Prize for work of fundamental or lasting importance is awarded to Eberhard Hopf of Indiana University for three papers:

1. *Abzweigung einer periodischen Lösung von einer stationären Lösung eines Differential systems*, Berichte über die Verhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig. Mathematisch-Naturwissenschaftliche Klasse, volume **95**(1943), pages 3–22. MR **12**, 501.

2. *A mathematical example displaying features of turbulence*, Communications on Applied Mathematics, volume **1**(1948), pages 303–322. MR **10**, 716.

3. *The partial differential equation $u_t + uu_x = \mu u_{xx}$*, Communications on Pure and Applied Mathematics, volume **3**(1950), pages 201–230. MR **13**, 846.

Hopf's 1943 paper [1] established the foundations of what is now called Hopf bifurcation. This theory predicts the bifurcation of a time-periodic solution from a stationary solution of an evolution equation at certain critical values of the parameters. Hopf's work on ordinary differential equations has been extended in recent years to partial differential equations and to abstract equations. It is now being very actively applied to problems of fluid mechanics, solid mechanics, and chemical reactor theory. In the second cited paper [2] Hopf gave an example of a system modelling that of a viscous fluid whose solutions exhibit turbulent behavior. In his analysis he introduced statistical considerations. This work is one of the chief inspirations for the ongoing effort to create a mathematically sound and physically illuminating theory of turbulence. His 1950 paper [3] analyzed the Burgers equation, which models the Navier-Stokes equations of fluid dynamics. Here Hopf treated the singular limit as $\mu \to 0$ and thereby laid the foundations of the viscosity method for treating shocks.

Eberhard Hopf

Professor Hopf was unable to attend the Prize Session, but sent the following message.

To my colleagues of the American Mathematical Society:

I wish to express my deep appreciation for my selection as the 1981 Steele Prize recipient.

I regret that failing health prevents me from joining you at the 1981 meeting and accepting the award in person.

Best wishes for a successful meeting.

Sincerely yours,

Eberhard Hopf
Indiana University

Eberhard Hopf

Biographical Sketch

Eberhard Hopf was born April 17, 1902, in Salzburg, Austria. He was educated at the University of Berlin where he received his Ph.D. degree in 1925 and his Habilitation in 1929. From 1926 to 1930 he was a member of the Astronomisches Recheninstitut and worked at the Einstein Tower. As a Rockefeller Foundation International Fellow, he worked at Harvard and its Astronomical Observatory and at Cambridge, England (1930 to 1932). He was an assistant professor of mathematics at the Massachusetts Institute of Technology from 1932 to 1936 and a professor of mathematics at the University of Leipzig (1936 to 1944) and the University of Munich (1944 to 1947). In 1947 he came to the Courant Institute at New York University as a visiting professor. Since 1949 he has been a member of the faculty at Indiana University, first as Professor and later as Research Professor. In 1972 he became Professor Emeritus.

He gave the Gibbs Lecture at the January 1971 annual meeting in Atlantic City (*Ergodic theory and the geodesics on surfaces of negative curvature*). He has given invited addresses at numerous International Congresses and Symposia. He is a member of the Academy of Sciences of Saxony and the Academy of Sciences of Bavaria. He has been an editor of the *Indiana University Mathematics Journal* since 1951.

Eberhard Hopf has made fundamental contributions in several branches of pure and applied mathematics, most notably in partial differential equations, calculus of variations, integral equations, hydrodynamics, ergodic theory, topological dynamics, and theoretical astrophysics.

CITATION FOR THE STEELE PRIZE FOR MATHEMATICAL EXPOSITION:

The Steele Prize Committee nominates for the Steele Prize for an expository work the book, *Linear Operators*, by Nelson Dunford and Jacob T. Schwartz (with the assistance of William G. Bade and Robert G. Bartle) [Part I, *General theory*, 1958. MR **22** #8302; Part II, *Spectral theory*, 1963. MR **32** #6181; Part III, *Spectral operators*, 1971. MR **54** #1009], Interscience Publishers, Inc., New York.

This monumental work of 2592 pages must be the most comprehensive of its kind in mathematics. A reviewer of Part II compared the treatise with the great texts in analysis of Picard or Goursat. But these did not reach so far into current research as this treatise, nor cover so thoroughly all previous work in the field.

In the Preface to Part I, the authors give as a prerequisite a semester of abstract algebra and a semester of complex variables, indicating their view of the book as basically expository. Presumably the graduate education of the reader continued as he read, but it is remarkable how nearly self-contained the three volumes are. A whole generation of analysts has been trained from it.

Special expository features deserve mention: The many exercises relating the theory to applications; the excellent notes at the end of the chapters; the use of arrows to single out apparently minor results of deeper significance; the clarity of style; the simplicity of notations; the willingness to prove a theorem in less than full generality if that is all that is required and the proof becomes more comprehensible.

Nelson Dunford

It is a gratifying honor to share a Steele prize with my good friend and colleague, Jacob T. Schwartz.

First, we both acknowledge the vital and tireless work of William G. Bade and Robert G. Bartle, without whose efforts *Linear Operators* would certainly have developed in a much more constrained manner. They revised and edited nearly every chapter and contributed a number of sections. In addition to these principal co-workers, a number of other young mathematicians who emerged with very distinguished careers contributed to our book: among these we mention only Gian-Carlo Rota and John Thompson.

I was first inspired by the theory of linear operators in 1925 while reading the Vanuxem Lectures delivered at Princeton in October 1912 by Vito Volterra. This interest was revived in 1931 by some papers of I. Fredholm and S. Banach. After several years of lecturing at Yale, I began to think of writing a text for graduate students that would serve as a reference for research mathematicians and mathematical physicists.

My dream was in an inchoate stage of scarcely three hundred pages when Jack arrived at Yale. I shall never forget the ease with which he adroitly tackled and solved all of the most intricate problems that arose in our work.

After Bill and Bob came to Yale, we formed the perfect team of four. All of our writing was done as a team except for the *Notes and Remarks* at the end of many chapters. Most of these were written by Bob. They contain references to the original and subsequent papers — also references to many results related to but not contained in the text. I cannot praise these meritorious and indispensable remarks too highly. The work Bob has put into them is almost incomprehensible. The notes for Chapter IV alone contain over 500 references! The *Notes and Remarks* are a most invigorating source of information for the research worker.

My further comments will be focused on a few of the many features of our work that I found most gratifying.

The first four chapters were inspired by Stefan Banach's treatise *Théorie des opérations linéaires*. The excellent treatise of F. Riesz and B. Sz-Nagy *Leçons d'analyse fonctionelle* is also close in spirit to some of the work in these chapters. These four chapters offer a fairly complete course in real function theory, developed from the basic principles of linear operators on a Banach space.

Some features of these chapters, not usually included in a course for first year students, are Stone's representation theorem for Boolean rings, the mean and pointwise Fubini-Jessen theorems for non-countable products of measures and the differentiation of vector-valued integrals. Another is the tabulation in Chapter IV which gives 391 references to results in the text concerning thirty Banach spaces of frequent occurrence in analysis.

The 396 exercises in these chapters are intended to show the applications of linear operator theory to various fields of classical analysis, e.g., summability of series including the methods of Silverman-Toeplitz, Nörlund, Cesàro, Abel, Schur-Mertens, Hardy-Littlewood and Tauber and the corresponding summability theory for integrals. Another field presented in the exercises is that of orthogonal series and, in particular, Fourier series and analytic functions. Among the results given are those of Beurling, Cesàro, Poisson, Hardy, Herglotz, Bernstein, H. S. Shapiro and Blaschke.

Two results in Chapter VIII are especially gratifying. The first is Theorem 1.19, which shows that certain perturbations of the infinitesimal generator of a strongly continuous semigroup of bounded linear operators on a B-space are also infinitesimal generators of a semigroup. The theorem also gives a representation for the new semigroup. The theorem provides a method for solving a large variety of Cauchy initial value problems, including integro-differential problems.

The second result was quite exciting to me. It is Theorem 7.17 and concerns the averages

$$(A(\alpha)f)(s) \equiv \alpha^{-n} \int_0^\alpha \cdots \int_0^\alpha (T(t_1, \ldots, t_n)f)(s)dt_1 \cdots dt_n.$$

Here $T(t_1, \ldots, t_n)$, $t_j \geq 0$, is an n-parameter semigroup of bounded linear operators on the space $L_1(S, \Sigma, \mu)$ where (S, Σ, μ) is a positive measure space. The proof of almost-everywhere convergence of these averages follows the proof of a special case proved by Dunford and Miller, but is much more difficult. This is because of the Lemma 7.11 needed to show that $\sup_\alpha |(A(\alpha)f)(s)| < \infty$ μ-almost everywhere. This lemma has the most complicated proof of any in Part I. We were fortunate in getting a reference from Einar Hille that led to Lemma 7.12 which gave us our one missing link. I was so exultant, I wanted to shout with joy and throw my pen to the ceiling when the proof was completed.

The most satisfying accomplishment in Part II was that of presenting the classical spectral theorem for selfadjoint operators in Hilbert space, together with some applications, using the terminology and notation of modern operator theory. Our methods are based upon the researches of I. M. Gel′fand.

There are so many exciting results in Part III that I shall have to be quite eclectic in choosing only a few.

One stimulating section is XVII.3 which presents Bill's penetrating study of strongly closed algebras of projections. Another most invigorating section is XVIII.3 which develops the Bade multiplicity theory. This theory suggests the possibility of a spectral representation theory, analogous to the Weyl-Kodaira theory, for boundary value problems in L_1.

I must extol the beauty and clarity with which Jack presented the applications of the general theory to the study of perturbations of operators with continuous spectra. Many such operators are either spectral operators or have closely related properties. A number of results, with some recent generalizations, due to Naĭmark, Friedrichs, Kato and Kuroda are discussed. Many of the results established in Chapter XX are applicable (or, in some cases, a more involved but similar analysis is applicable) to some profound nonselfadjoint problems in contemporary as well as in classical mechanics. These theorems place some empirical laws of physics on a rigorous mathematical foundation. Jack elegantly presents the erudite and ingenious arguments of Kato and Kuroda used in establishng a perturbation theorem (Theorem 4.9) which asserts that, under suitable restrictions, the perturbation $H + V$ of a selfadjoint operator H by a symmetric operator V is again a selfadjoint operator. The theory evolving from this study has been of great significance for the application of spectral theory to physics.

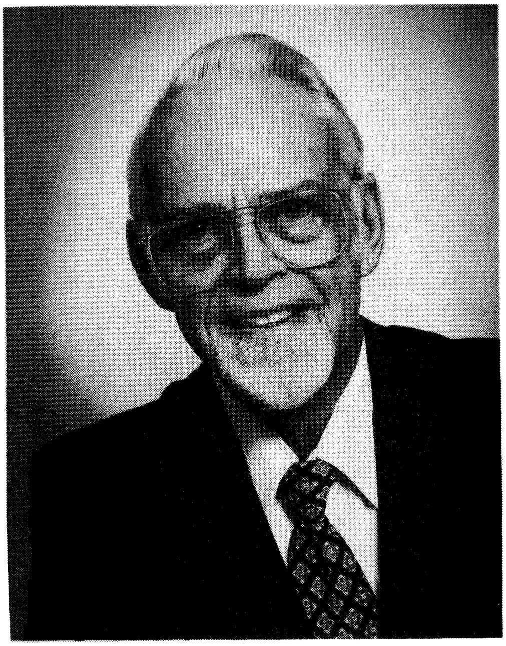
Biographical Sketch

Nelson Dunford was born December 12, 1906 in St. Louis, Missouri. He attended the University of Chicago (Ph.B., 1931; M.A., 1932), and Brown University (Ph.D., 1936).

He was an instructor of mathematics at Brown University (1934 to 1936). He went to Yale in 1936 as an instructor, was given an assistant professorship at 1938 and was made full professor in 1943. In 1950 he received the James E. English endowed chair of mathematics at Yale.

Professor Dunford was a member of the *Transactions* editorial committee (1941 to 1949) and the *Mathematical Surveys* editorial committee (1945 to 1949). He was a member-at-large of the Council from 1942 to 1944.

He gave invited addresses at the April 1943 meeting in New York City (*Spectral theory*) and the January



Nelson Dunford

1958 annual meeting in Cincinnati (*A survey of the theory of spectral operators*).

He was a member of the Institute for Advanced Study, Princeton, during 1945–1946, second term.

He was invited to speak at the International Congresses in Guadalajara (1945) and Cambridge (1950). At the former he lectured on *Direct decompositions of Banach spaces*, at the latter on *The reduction problem in spectral theory*. He was invited to speak at the International Symposium in Stillwater (1951) and at the International Conference at Budapest (1980). At the Symposium he lectured on *Spectral theory and harmonic analysis*, at the Congress he presented in absentia the paper *Spectral theory in topological vector spaces, dedicated to Jacob T. Schwartz*.

One of the classroom buildings at the Pine View School was named *Dunford's Domain* in his honor.

His areas of research are integration, linear operators, ergodic theory and spectral operators.

Jacob T. Schwartz

It is of course a source of great satisfaction that the diligence of the graduate student and the junior instructor I once was should be honored by the award of a Steele prize. My attitude to mathematics has always been marked by a mixture of awe and delight; time erodes all, but mathematics partakes of eternity. So to be honored by the community of mathematicians is to be honored indeed!

Biographical Sketch

Jacob T. Schwartz was born January 9, 1930 in New York City. He studied at City College of New York (B.S., 1948) and Yale University (M.A., 1949 and Ph.D., 1951).

He remained at Yale from 1952 to 1957, as instructor and assistant professor. He has been at

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the Courant Institute of Mathematical Sciences, New York University, since 1957, first as associate professor and, since 1959, as professor of mathematics. From 1960 to 1962 he was a Sloan Foundation Fellow.

Professor Schwartz was a member of the Editorial Committee of *Mathematical Reviews* from 1973 to 1978, member-at-large of the Council from 1968 to 1970, and a member of the executive committee in 1970 and 1976. He served on a number of other Society committees, was the originator of the AMS Short Course Series, and organized the first two such courses (Missoula 1973, San Francisco 1974), on computer science for mathematicians.

He gave invited hour addresses at the New York meeting, April 1956 (*Riemann's method and the theory of special functions*) and at the annual meeting in Las Vegas, January 1972 (*Complexity of statement, computation, and proof*).

His areas of research interest are functional analysis, quantum theory, spectral theory of operators, ergodic theory, and computers.

He was elected to the (U.S.) National Academy of Sciences in 1976.