Light Shadows: Remembrances of Yale in the Early Fifties[1]

Gian-Carlo Rota

Jack Schwartz

The first half of Jack Schwartz's life coincides with one of the greatest ages of science. The achievements in the exact sciences of the period that runs from roughly 1930 to 1990 may well remain unmatched in any foreseeable future. Jack Schwartz's name will be remembered as a beacon of this age.

No one among the living has left as broad and deep a mark on as many areas of pure and applied mathematics, on computer science, in economics, in physics, as well as in fields which ignorance prevents me from naming.

I hope you will forgive me as I declare my incompetence to do justice to Jack Schwartz's life, to his personality, to his achievements. I beg your indulgence if I resort instead to an easier task. I'd like to recall a few anecdotes from a brief period of the past, the years 1953 to 1955, when I met Jack and learned mathematics as a graduate student at Yale.

The first lecture by Jack I listened to was given in the spring of 1954, in a seminar in functional analysis. A brilliant array of lecturers had been expounding throughout the spring term on their pet topics.

Jack's lecture dealt with stochastic processes. Probability was still a mysterious subject cultivated by a few scattered mathematicians, and the expression "Markov chain" conveyed more than a hint of mystery. Jack started his lecture with the words, "A Markov chain is a generalization of a function." His perfect motivation of the Markov property put the audience at ease. Graduate students and instructors relaxed and followed his every word to the end.

Jack's sentences are lessons in clarity and poise. I remember a discussion in the mid-eighties about the future of artificial intelligence, in which for some reason I was asked to participate. The advocates of what was then called "hard A. I." were painting a triumphalist picture of the future of computer intelligence, to the dismay of their opponents. As the discussion went on, all semblance of logical argument was given up. Eventually, everyone realized that Jack had not said a word, and all faces turned toward him. "Well," he said, "some of these developments may lie one hundred Nobel prizes away." His felicitous remark calmed everyone down. The A. I. people felt they were being granted the scientific standing they craved, and their opponents felt vindicated.

Gian-Carlo Rota in 1955.

Gian-Carlo Rota was born in Italy and educated partly in Ecuador. He has spent more than half his life at the Massachusetts Institute of Technology, where he is now Professor of Applied Mathematics and Philosophy. Some remember him, in the period referred to in this article, as a fount of enlightenment about linear operators and their spectra. Many more know him as a champion of algebraic combinatorics and a purveyor of uncompromisingly telegraphic book reviews.

[1]Inaugural address delivered at Courant Institute (New York University) at a meeting in honor of Jacob T. Schwartz on May 19, 1995.

Jack Schwartz

I have made repeated use in my own lectures of Jack's strikingly apposite phrases. You may forgive this shameless appropriation upon learning that my students have picked up the very same phrases from me, and so on.

From Princeton to Yale

Mathematics in the fifties was a marginal subject, like Latin. The profession of mathematician had not yet been recognized by the public, and it was not infrequent for a mathematics graduate student to be asked whether he was planning to become an actuary. The centers of mathematics were few and far between, and communication among them was infrequent. The only established departments were Princeton and Chicago. Harvard was a distant third, and Yale was in the process of overcoming its overdependence on the College. In New York, Richard Courant was busy setting up his Institute of Mathematical Sciences at 25 Waverly Place, and he had just finished training his first generation of students in America, the generation of Lax and Nirenberg, of Cathleen Morawetz and Harold Grad. It was already clear that the Institute he was putting together was going to be a great center of mathematics.

In the spring of 1953, I was a senior at Princeton, and I applied to various universities for admission to graduate school. It soon became apparent that I only needed to apply to one graduate school.

Professor A. W. Tucker was not yet the chairman of the Mathematics Department, but he was already acting as if he were. Solomon Lefschetz, the nominal chairman on the verge of retirement, would make fun of Tucker, by lavishing in public uncomfortably high praise on Tucker's managerial skills.

There were few undergraduate majors, maybe a half dozen each year, and Al Tucker would see to it that they were sent to the "right" graduate schools. He made sure that Jack Milnor stayed in Princeton, and he sent Hyman Bass, Steve Chase, and Jack Eagon to Chicago, Mike Artin to Harvard. In April 1953, I wrote a letter of acceptance to the University of Chicago, which had offered me a handsome fellowship (in those days, it was extremely easy to be offered a graduate fellowship anywhere). On my way to the mailbox, I met Professor Tucker on the narrow, rickety stairs of the old Fine Hall. He asked me where I had decided to go to graduate school, and, upon hearing of my decision, he immediately retorted, "You are not going to Chicago, you are going to Yale!"

I had no choice but to do what he bid me; I tore up the letter to Chicago and wrote an identical letter of acceptance of a fellowship that I had been offered by Yale. In retrospect, my decision to go to Yale is one of the few right decisions I have made, and I will always thank Al Tucker's memory for guiding me to it.

Don Spencer, another of my undergraduate teachers, was first to mention Jack Schwartz's name to me. He complimented me on my choice of a graduate school, with the remark: "Oh yes, Yale, that is where Jack Schwartz is . . .". It was an astounding statement, considering that Jack Schwartz was getting his Ph.D. from Yale that very month. Spencer's remark began a process of turning Jack Schwartz into a mythological figure in my mind, a process that did not stop after I actually met Jack Schwartz a few months later. Actually, I have never been able to stop the process.

Josiah Willard Gibbs

The sciences at Yale have always played second fiddle to the humanities. At faculty meetings it is not unusual to witness a professor of literature point with a wide gesture, like a Roman senator, towards Hillhouse Avenue, where most of the science departments are located, and begin an oratorical sentence with the words "Even in the sciences . . ."

Despite the distrust that Yale College has felt towards science, Yale was once blessed with the presence of one of the foremost scientists of the nineteenth century, namely, Josiah Willard Gibbs.

Gibbs served as a professor at Yale without any stipend. Professors did not receive any salary from Yale in those happy days. Teaching young men from the upper echelons was not a salaried profession, it was a privilege. The administration did of course receive handsome salaries, like all administrations in all times of history.

One day, Gibbs received an offer from the recently founded Johns Hopkins University. It was an endowed professorship. We may hazard the guess that it was the position Sylvester had relinquished when he accepted a professorship at Oxford, after the requirement of religious vows for professors was dropped by the two English universities.

Thanks to its endowment, the Johns Hopkins professorship carried a stipend of one hundred dollars a year. It is unclear whether Gibbs was delighted with the offer; in any case he felt obliged to get ready to move to Baltimore. One of his colleagues realized that Gibbs was packing, and hastened to contact the Dean of the College. The Dean asked the colleague if he could do something to keep Gibbs at Yale. "Why, just tell him that you'd like him to remain at Yale!" answered the colleague. The Dean kept his word and did what the colleague had recommended. He summoned Gibbs to his office and generously let him know that he wanted Gibbs to stay. It was the kind of reassurance Gibbs needed. He declined the Johns Hopkins offer, and remained at Yale for the rest of his career.

Some of Gibbs's most original papers in statistical mechanics were published in the *Proceedings of the Connecticut Academy of Sciences*, a journal which I dare surmise few of us have ever seen. One might wonder how papers which saw the light in such an obscure publication could manage to receive within a short time worldwide publicity and acclaim. After I moved to Yale in the summer of 1953, I accidentally found the answer to this puzzle.

There was no mathematics library at Yale in the fifties; a mathematics library was not opened until the early sixties, after several members of the Mathematics Department had threatened to quit. Before that time, mathematics books were relegated to a few shelves in the Sterling Library, randomly classified under that miscarriage of reason that was the Dewey Decimal System.

All students had access to the shelves. In August 1953, I used to walk through the mathematics shelves of the Sterling Library and to pull out books at random, as we do when we are young. Next to an array of perused calculus books were hard-bound lecture notes of courses offered at Yale at various times by members of the faculty. Among these were some course notes by Gibbs, written in his own hand. A few additional sheets were glued to one of these volumes. The names of all notable scientists of Gibb's time were listed in these sheets, from Poincaré and Hilbert and Boltzmann and Mach, all the way to individuals who are now all but forgotten. Altogether, more than two hundred names and addresses were alphabetized in a beautiful, fading handwriting. Those sheets were a copy of Gibbs's mailing list. As I leafed through it with amazement, I realized at last how Gibbs had succeeded in getting himself to be known in a short time. I also learned an instant lesson, the importance of keeping a mailing list.

Yale in the Fifties

In the early fifties, Yale had not yet lost the charm of a posh out-of-the-way college for the children of the wealthy. Erwin Chargaff, in his autobiography *Heraclitean Fire*, describes Yale in the following words:

> Yale University was much more of a college than a graduate school; and the undergraduates were all over town. They were digesting their last goldfish, for the period of whoopee, speakeasies, and raccoon coats was coming to an end, to be replaced by a grimmer America which was never to recover the joy of upper-class life. The University proper was much less in evidence. Shallow celebrities, such as William Lyon Phelps, owed their evanescent fame to the skill with which they kept their students in a state of elevated somnolence.

The main part of the campus, consisting of nine shining colleges in the middle of New Haven, was of recent vintage. At the lower end of Hillhouse Avenue, the red bricks of Silliman College shone like the plaster of a movie set as one made one's way back to the main campus from the deliberately distant science buildings. Envious Englishmen spread the malicious rumor that the colleges that Mr. Harkness's money had built were Hollywood-style imitations of Oxford colleges. But nowadays the shoe is on the other foot, and it is Oxford that is at the receiving end of other jibes.

The graduate school was a genteel (though less and less gentile) appendage added to the University by gracious assent of the Dean of the College. The Dean of the College held the real power, and he could overrule the President. Since the thirties, professors appointed to the few and ill-paid graduate chairs had consistently turned out to be better scholars and scientists than the Administration had foreseen at tenure time. Nonetheless, evil tongues from Northern New England whispered that a certain well-known physics professor would never have made it past assistant professor in Cambridge; but he was one of the last exceptions, soon to fade into best-sellerdom.

Hard work, the kind one reads about in the hagiographies of scientists, was regarded by the graduate students with embarrassment. It was not unusual for a graduate student to spend seven postgraduate years as a teaching assistant before being reluctantly awarded a terminal Ph.D. The university cynically encouraged graduate students to defer their degrees: the money saved by hiring low-paid teaching assistants in place of professors could be used to enrich the rare book collection. Writing a doctoral dissertation was an in-house affair, having little to do with publishing or with distasteful professionalism.

On learning about the shocking leisure of graduate life at Yale in the fifties, one may seek shelter in one of the current philosophies of education, which promise instant relief from the onslaught of reality. One would thereby be led to the mistaken conclusion that "creativity" (a pompous word currently enjoying a fleeting but insidious vogue) would be stifled in the constricted, provincial, unhurried atmosphere of New Haven. The facts tell a different story. The comforts of an easy daily routine in a rigidly circumscribed environment, encouraged by the indulgent scrutiny of benign superiors, foster the life of the mind. Professors were poorly paid but enjoyed unquestioned prestige. In their sumptuous quarters in the colleges they would encourage their students with sherry and conversation.

Purposeless delectation in ideas may be as educational as intensive study. At Yale, together with the enjoyment of an absorbing range of campus activities went the lingering belief that nothing much mattered in that little corner of the world. Teachers and students were thereby led to meet the fundamental requirement of a successful educational experience: They were kept from taking themselves too seriously.

There is a fundamental difference between the quality of life in Northern New England and in Southern New England. It comes from the shadows. On a Cambridge Sunday, the sharp shadows across the Charles River cut out the outlines of the distant buildings of Boston as if made of stiff cardboard, and deepen the blue of the water. In New Haven, by contrast, the light shadows are softened in a silky white haze, which encloses the colleges in a cozy aura of unreality. Such foresight of Mother Nature bespeaks a parting of destinies.

Mathematics at Yale

The Mathematics Department was the first of the science departments to awaken. It was not until the fifties when the last of a long line of professional teachers of calculus retired from the Mathematics Department: fine, upright gentlemen of the old school, richly endowed with family values, who reaped handsome profits on the royalties of their best-selling textbooks.

The mathematicians who replaced them were eager to create a research atmosphere, and at last a few graduate students were slowly beginning to drift over to New Haven. From the beginning of the Yale graduate school all the way to the twenties, the one notable research mathematician to have taught at Yale was E. H. Moore, and two of the few distinguished mathematicians to come out of Yale until the fifties were Marshall Hall and Irving Segal. In the fifties, a sudden plethora of stars appeared, led by Jack Schwartz.

It is not clear how functional analysis took over the Mathematics Department. Einar Hille was hired away from Princeton sometime in the mid-thirties, but for several years he was one of two research mathematicians. At the time, several universities would hire one and only one "research mathematician"; Yale could afford as many as two: Einar Hille and Oystein Ore.

Nelson Dunford was next to come, as an assistant professor. Soon after his arrival, he received an attractive offer from the University of Wisconsin, and Yale took the unusual step of promoting him two steps up to a full professorship.

After the end of World War Two, Kakutani came over from Japan, and Charles Rickart from Michigan.

By the early fifties, just about every younger mathematician at Yale was working in functional analysis, and the weekly seminars were attended by well over 50 people.

The core of graduate education in mathematics was Dunford's course in linear operators. Everyone who was interested in mathematics at Yale eventually went through the experience, even some brilliant undergraduates, such as Andy Gleason, McGeorge Bundy and Murray Gell-Mann.

The course was taught in the style of R. L. Moore: mimeographed sheets containing unproved statements were handed out every once in a while, and the students would be asked to produce proofs on request. Occasionally, some student at the blackboard would fall silent. Dunford would make no effort to help, and the silence, sometimes lasting the whole 50 minutes, became unbearable to all. I suspect that Dunford wanted to minimize his teaching load, which in those years ran to 12 hours per week for full professors.

Everyone who took Dunford's course was marked by it. George Seligman once remarked to me that Dunford's course in linear operators was the turning point in his graduate career as an algebraist.

Dunford had an unusual youth. After being passed over for a graduate fellowship in the middle of the depression in the thirties, he survived in St. Louis on 10 dollars a month, while studying and writing in the public library. Remarkably, the St. Louis library did subscribe to the few mathematics research journals of the time, and while unemployed in St. Louis Dunford managed to finish his first paper, which deals with integration of functions with values in a Banach space. After the paper had been accepted for publication in the *Transactions of the A. M. S.*, Dunford was offered an assistantship at Brown, where he worked under Tamarkin. His doctoral dissertation dealt with the functional calculus that bears his name.

He was hired by Yale right after he received his Ph.D., and spent his entire career there. He retired early, ostensibly because he had made lucrative investments in art and in the stock market. But in reality, Dunford's re-

Post-retirement portrait of Nelson Dunford and his wife.

tirement could be another episode of *The Bridge of San Luis Rey.* It coincided with the completion of his life work, which was the three-volume treatise *Linear Operators,* written in collaboration with his student Jack Schwartz.

Linear Operators started out as a set of solutions to problems handed out in class; it gradually increased in size. Soon after Jack Schwartz enrolled in the course, Dunford asked him to become co-author. The project quickly expanded to include Bill Bade and Bob Bartle, as well as several students, instructors and assistant professors. It was fully supported by the office of Naval Research. There is a persistent rumor, never quite denied, that every nuclear submarine on duty carries a copy of *Linear Operators*.

Abstraction in Mathematics

The pendulum of mathematics swings back and forth towards abstraction and away from it, with a timing that remains to be estimated. The period that runs roughly from the twenties to the middle seventies was an age of abstraction. It probably reached its peak in the fifties and sixties. The fifties were the heyday of functional analysis, as the sixties were the heyday of algebraic geometry.

The two major centers of functional analysis in the fifties were Yale and Chicago. The Mathematics Department at Stanford, which consisted entirely of classical analysts, had trouble finding graduate students. The great classical analysts at Stanford, such names as Pólya, Szegö, Loewner, Bergman, Schiffer, and the first Spencer, were considered to be hopelessly old-fashioned.

At Yale you could find no analysis courses offered other than functional analysis and supporting abstractions. Algebra reached an independent peak of abstraction with Nathan Jacobson and Oystein Ore. There was a standing bet among graduate students at Yale that whenever a doctoral dissertation in analysis was turned in, the writer would be challenged to use its results to give a new proof of the spectral theorem.

In those days, no one doubted that the more abstract the mathematics, the better it would be. A distinguished mathematician, who is still alive, pointedly remarked to me in 1955 that any existence theorem for partial differential equations which had been proved without using a topological fixpoint theorem should be dismissed as applied mathematics. Another equally distinguished mathematician once whispered to me in 1956, "Did you know that your algebra teacher Oystein Ore has published papers in graph theory? Don't let this get around!"

Sometime in the early eighties the tables were turned, and a stampede away from abstraction started, which is still going on. A couple of years ago I listened to a lecture by a well-known probabilist, which dealt with properties of Markov processes. After the lecture, I remarked to the speaker that his presentation could be considerably shortened if he expressed his results in terms of positive operators rather than in terms of kernels. "I know," he answered, "but if I had lectured on positive operators nobody would have paid any attention!"

There are already signs that the tables may be turning again, and we old abstractionists are waiting with mischievous glee for the pendulum to swing back. Just a few months ago, I overheard a conversation between two brilliant assistant professors, purporting to provide an extraordinary simplification to some recently proved theorem; eventually, I realized with pleasant surprise that they were rediscovering the usefulness of taking adjoints of operators.

Linear Operators: the Past

The three-volume treatise *Linear Operators* was originally meant by Dunford as a brief introduction to the new functional analysis, and to the spectral theory that had been initiated by Hilbert and Hellinger, but that had not really taken root until the work of von Neumann and Stone. Dunford, however, championed spectral theory as a new field. He introduced the term "resolution of the identity," and he developed the program of extending spectral theory to non-self-adjoint operators.

The initial core of the book consisted of what are now Chapters 2, 4, and 7, as well as some material on spectral theory now in chapter 11; eventually, this material expanded into two volumes. The idea of volume three was a belated one, coming in the wake of the development of the theory of spectral operators.

The writing of *Linear Operators* took approximately 20 years, starting in the late forties. The third volume was published in 1971. Entire sections and even entire chapters were added to the text at various times, up to the last minute. For example, one of the last bits to be added to the first volume right before it went to press is the last part of section 16 of chapter 4, containing the Gauss-Wiener integral in Hilbert space together with a simple formula relating it to the ordinary Wiener integral. This section was the subject of a lecture that Jack gave at the famous seminar on integration in function space that was held at the Courant Institute in the fall of 1956.

The flavor of the first drafts of the book can be gleaned from reading chapter 2, which underwent fewer redrafts than most of the other chapters. Dunford meant the three theorems proved in this chapter, namely, the Hahn-Banach theorem, the uniform boundedness theorem and the closed graph theorem, to be the cornerstones of functional analysis. The exercises for this chapter, which in their first draft were rather dry, were eventually enriched by a set of exercises on summability of series. These problems are continued in chapter 4, and conclude in chapter 11 with the full expanse of Tauberian theorems. The contrast between the uncompromising abstraction of the text and the incredible variety of concrete examples in the exercises is immensely beneficial to the student who learns mathematical analysis from Dunford-Schwartz.

The topics dealt with in Dunford-Schwartz can be roughly divided into three kinds. There are topics for which Dunford-Schwartz is still the definitive account. There are, on the other hand, other topics fully dealt with in the text which ought to be well-known, but which have yet to be properly read. Finally, there are topics that are still ahead of the times, and that remain to be fully appreciated. Presumptuous as it is on my part, I will try to give some examples of each kind.

Besides the introductory chapter on Banach spaces (chapter 2), the treatment of the Stone-Weierstrass theorem and all that goes with it in chapter 4 still makes nowadays very enjoyable reading; in its time, it was the first thorough account. The short sections on Bohr compactification and almost periodic functions are also still the best reference for a quick summary of Bohr's extensive theory.

Section 12 of chapter 5 is remarkable. It presents a proof of the Brouwer fixpoint theorem. The proof was submitted for publication in a journal in 1954, but it was rejected by an irate referee, a topologist who was miffed by the fact that the proof uses no homology theory whatsoever. It does use instead some determinantal identities, the kind that are now again becoming fashionable.

Spectral theory proper does not make an appearance until chapters 7 and 8, with the functional calculus and the theory of semigroups. In those days, such terms as "resolvent" and "spectrum" carried an aura of mystery, and the spectral mapping theorem sounded like magic.

The meat and potatoes comes in chapters 10, 12, and 13; the proofs are invariably the most instructive, bringing into full play the abstract theory of boundary conditions of Calkin and von Neumann, as well as the theory of deficiency indices.

Linear Operators: the Present

There are topics for which Dunford-Schwartz was the starting point of a long development, and which have grown into autonomous subjects. Thus, for example, the notion of unconditional convergence of series in Banach spaces, which goes back to an old theorem of Steinitz and which is mentioned in chapter 2 almost as a curiosity, has blossomed into a full-fledged discipline. The same can be said of the geometry of Banach spaces initiated in chapter 4, and of the theory of convexity in chapter 5. In the sixties, several mathematicians pronounced the general theory of Banach spaces dead several times over, but this is not what happened. The geometry of Banach spaces not only managed to survive, but it is now widely considered to be the deepest chapter of convex geometry. Grothendieck once told me that his favorite theorem of his analysis period was a convexity theorem that generalizes a result in Dunford-Schwartz. Unfortunately, he published it in an obscure Brazilian journal, and he never received any reprints of the papers.

The theory of vector-valued measures in chapter 4 has equally blossomed into a chapter of functional analysis of beauty and depth. Strangely, at the time of the book's writing, we all thought that this theory had reached its definitive stage, perhaps because the proofs were so crystal clear.

The same can be said of the theory of representation of linear operators in chapter 6; here again whole theories nowadays replace single sections of Dunford-Schwartz. Corollary 5 of Section 7, stating that in certain circumstances the product of two weakly compact operators is a compact operator, has always struck me as one of the most elegant results in functional analysis, and undoubtedly sooner or later some extraordinary application of it will be found, as should happen to all beautiful theorems.

Thorin's proof of the Riesz convexity theorem had appeared a short time before chapter 6 was written, and it is here given its first billing in a textbook. I take the liberty of calling your attention to problem 15 of section 11. This exercise holds the key to giving one-line proofs of some of the famous inequalities in the classic book by Hardy, Littlewood, and Pólya.

Section 12.9 has been scandalously neglected. The classical moment problems are thoroughly dealt with in this section by an application of the spectral theorem for unbounded self-adjoint operators. It is shown in a couple of pages that the various criteria for determinacy of the moment problem can be inferred from a simple computation with deficiency indices. Partial rediscoveries of this fact are still being published every few years by mathematicians who haven't done their reading.

Linear Operators: the Future

Finally, there are numerous subjects that were first written up in Dunford-Schwartz, from which the mathematical world has yet to benefit. It is surprising to hear from time to time probabilists or physicists addressing problems for which they would find ready help in Dunford-Schwartz. The functional analytic incompetence of physicists has decreased since the fifties, but one suspects that a lot of research funds might be saved if all physicists were to be required to have some basic functional-analytic background. Once, while I was a graduate student, a physicist working in quantum mechanics, who is now one of the leading theoretical physicists of our day, asked me to describe the difference between a symmetric and a self-adjoint operator in Hilbert space, which he ignored; one wonders how much the situation has improved in forty years.

Chapter 3 on measure theory is one of the chapters inserted at a fairly late stage. It has not been read much, perhaps because every reader believes he or she is supposed to know measure theory when embarking upon the reading of Dunford-Schwartz. Actually, chapter 3 contains a number of yet-to-be-appreciated jewels. One of them is the comprehensive treatment of theorems of the Vitali-Hahn-Saks type. The proofs are so concocted as to bring out the analogies between the combinatorics of sigma-fields of sets and the algebra of linear spaces. Few analysts make use of this kind of reasoning. In probability, an appeal to the Vitali-Hahn-Saks theorem would bypass technical complications that are instead settled by the Choquet theory, for example, randomization theorems of the De Finetti type. Apparently, the only probabilist to have taken advantage of this opportunity is Alfred Rényi in an elementary introduction to probability that also has been little read. Similarly, one wonders why so little use is still made of theorem III.7.6, which might come in handy in integral geometry.

Large portions of spectral theory presented in Dunford-Schwartz remain to be assimilated. Thus, for example, the fine theory of Hilbert-Schmidt operators and the wholly original theory of subdiagonalization of compact operators in chapter 11 have not been read. The spectral theory of non-self-adjoint operators of chapters 14 through 19 is a gold mine that is still waiting for its day in the sun; only the latter parts of chapter 20, dealing with what the authors have successfully called "Friedrichs's method" and with the wave operator method, have been developed since the treatise was published. It will be a pleasure to watch the rediscovery of these chapters by the younger generations.

Working with Jack Schwartz

There are fringe benefits to being a student of Jack's. Occasionally I decline invitations to attend meetings in computer science and even in economics, from organizers who mistakenly assume that I have inherited my thesis advisor's interests.

Two traits of Jack's personality have particularly endeared him to his students. One is his instinctive understanding of another person's state of mind, his tact in dealing with difficult situations. He gives encouragement without exaggerating, and he knows how to steer his friends away from being their own worst enemies.

The second is his Leibnizian universality. It spills over onto all of us, it lifts us and points us in the right direction. Whatever topic he deals with at one time he sees as a stepping stone to some wider horizon to be dealt with at some future time. Both of these qualities shine in the pages of *Linear Operators*, the first by the transparent proofs, the second from the encyclopedic range of the material that is dealt with in 2592 pages.

I was hired to work on the Dunford-Schwartz project in the summer of 1955, together with Bob McGarvey. Immediately, Jack took us aside and let us in on the delicate matter of the semicolons. There were to be no semicolons in anything we wrote for the project. Dunford would get red in the face every time he saw a semicolon. For years hence, I was terrified of being caught using a semicolon, and you may verify that in the three printed volumes of Dunford-Schwartz not a single semicolon is to be found.

I was asked to check the problems in chapter 3, while Bob was checking problems in chapters 7 and 8. We would all get together every morning in a little office in Leet-Oliver Hall, an office that nowadays would not be considered fit for a teaching assistant. A bulky record player, which we had bought for ten dollars, occupied much of the space; we played over and over the entire sequence of Beethoven quartets and Bach partitas while working on the problems.

It took me half the summer to finish checking the problems in chapter 3. There were a few that I had trouble with, and worst of all, I was unable to work out problem 10 of section 9. One evening, Dunford and several other members of the group got together to discuss changes in the exercises. Jack was in New York City. It was a warm summer evening, and we sat on the hard wooden chairs of the corner office of Leet-Oliver Hall. Pleasant sounds of squawking crickets and frogs came through the open window, and mosquitoes were flying in through the open Gothic windows. After I admitted my failure to work out problem 10, Dunford tried one trick after another on the blackboard, in an effort to solve the problem or to find a counterexample. No one remembered where the problem came from, or who had inserted it.

After a few hours, we all got up and left, somewhat downcast. The next morning, I met Jack, who patted me on the back and told me, "Don't worry, I could not do it either". I did not hear again about problem 10 of section 9 for another three years. A first-year graduate student took Dunford's course in linear operators. Dunford assigned him the problem, and the student solved it, and developed an elegant theory around it. His name is Robert Langlands.

In the second half of the summer of 1955, after checking the problems in chapter 3, I was assigned to check the problems in spectral theory of differential operators in chapter 13. This is the chapter of Dunford-Schwartz that decided my career in mathematics. Apparently, I had less difficulty with this second round of exercises, but I made a number of careless mistakes, as I always have since.

One day, I was unexpectedly called in by Dunford. The details of this meeting have been many times rewritten in my mind. The large office was empty, except for Dunford and Schwartz sitting together at the desk in the shadow, like judges.

"We have decided to assign you the problems in sections G and H of chapter 13", they said. A minute of silence followed. I had the feeling that there was something they weren't saying. Eventually, I got it. They were NOT assigning me the problems in section I, which dealt with the use of special functions in eigenfunction expansions. I soon learned, somewhat to my annoyance, that the person in charge of checking the problems in section I was an undergraduate who had just gotten his B.A. two months before. "You will never find a better undergraduate in math coming out of Yale," Jack told me, aware of my feelings. He was right. The undergraduate checked all the special function problems by the end of the summer, and section I is now spotless. His name is John Thompson, and he went on to win the Fields Medal.

I have kept a copy of the mimeographed version of the manuscript of Dunford-Schwartz. On gloomy days, I pull the dusty 15-pound bulk out of the shelf. Reading the now-yellowed pages, with their inky smell, was once a great adventure; rereading them after 40 years is a happy homecoming.

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Hans Hahn

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