MY ACADEMIC LIFE

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REMINISCENCES

I grew up under comfortable circumstances in Brooklyn, New York. I graduated from the local high school at the age of fifteen in 1943 and although I would have liked to be sent away to college, my parents believed that I was too young so I attended Brooklyn College. It took me some time to realize how fortunate I was. The New York City colleges had remarkable faculties due to the discrimination against Jews, Italians and women practiced at this country's most famous universities. The student body was also of high quality. Of the first eight Putnam examinations, Brooklyn won three, Toronto won four and Harvard won one.

My intended field of study was either medicine or dentistry. I was admitted to dental school while still a sophomore, but decided not to go, much to my mother's chagrin. Medicine seemed very appealing and a few of my teachers were suggesting that I study history or English or biology. At this point I registered in an advanced calculus course with Roger Johnson and it changed my life completely. In the first week we explored Dedekind cuts, Cauchy sequences and upper and lower limits. It was the first course I took in college that seemed like a real challenge and I gave up the thought of studying anything else. Of the dozen who finished that course, one went to Harvard, another to MIT and four, myself included, to Johns Hopkins. I graduated in the winter of 1947 and was asked to serve as an instructor in the spring semester to replace an ill faculty member. I recall vividly a moment when I was lecturing on trigonometry. A secretary came into the room with a telegram. I read it and turned to the class and said "It's just an offer from Harvard". The telegram was from Garrett Birkhoff Jr., offering me an assistantship. I rejected it and he called me to urge me to accept. I suppose he was not used to rejection. Nevertheless, I felt committed to Hopkins and I did not like the note of condescension I detected in Birkhoff's remarks.

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The dominant figures in the Hopkins department were the chair, Francis Murnaghan, Aurel Wintner, and as a visitor, B. L. Van der Waerden. Van der Waerden was the best teacher I have ever had. His courses in topology and geometry were so exciting that the back of the classroom soon filled with faculty members and other auditors. In addition, he was remarkably friendly and helpful to the students. On the other hand, most of the students were fearful of Wintner and tried to stay out of his way. He was given to sudden changes of mood and could be most unpleasant. Hans Reiter came to study at Hopkins from Brazil, where his family had taken refuge from the Nazis in Austria. One day Wintner asked him a question in class, but Hans's English was so poor that he didn't understand. After class, he asked Wintner to please ask him questions in German. Wintner reacted by cutting off all contact with Hans. I had the opposite problem. My final examination paper in the first analysis course impressed Wintner greatly and I became his favorite. This meant that he started giving me books and problems to look at. For example, he suggested that I try to prove that the Mertens conjecture was false. I decided that I would leave after the first year. I had been looking at Antoni Zygmund's "Trigonometric Series" and applied to Chicago in the hope that he would take me as a student.

Chicago was very different. Marshall Stone was the chairman and he was very friendly and fair. He assumed that, since I had studied with Wintner, I must be very knowledgable about celestial mechanics. I did my best not to disillusion him. I greatly enjoyed the courses I took from him, Paul Halmos, Ed Spanier and Zygmund. The atmosphere was very informal in comparison with Hopkins, where the assistants were expected to wear a jacket and tie. On meeting Wintner in the hall, I always bowed slightly, exhibiting, to paraphrase W. S. Gilbert, the deference due to a person of high degree. One day I was standing in a corridor in Eckhart Hall with some of my colleagues, when Stone came striding along. I bowed and said "Good morning, Professor Stone". Once he had passed, my friends were almost hysterical with laughter and mimicked me for several days.

My association with Zygmund started only one year after he arrived in Chicago. I studied Fourier series, potential theory and $\mathcal{H}^p$ spaces with him. Later on, I graded papers for his course in measure and integral. At that time he was interested in trigonometric series and integrals in one and two variables, differentiation, harmonic functions, summability, and he also completed a set on notes entitled *Trigonometric Interpolation*, which became the basis for a chapter in the next edition of "*Trigonometric Series*". His work in singular integrals came toward the end of my stay there. Among my fellow Zygmund students were Berkovitz, Calderon, Cotlar, Shapiro and Wirszup. Other notable students at the time were Kadison, Singer, Michael, Bartle and Rosenberg. It was an exciting and stimulating environment.

It took me quite a while before I screwed up my courage and asked Zygmund if he would accept me as a doctoral student. I will never forget his response. He paused and looked at me very closely and said "Mr. Waterman, I have the impression that you are, how to say, somewhat lazy. If that is the case, you cannot work with me".

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I assured him that this was not the case and he directed me to a recent paper of his on high indices theorems and suggested that I try to generalize his result. I was concerned about his opinion of me and hit on a plan to convince him that it was not correct. He was in the habit of going into the Mathematics Library several times a day. I would arrive early in the morning and start working at a table easily visible from the entrance and stay there most of the day. This seemed to work. The basic problem he gave me was to extend his result for L^1 to $L^p, p > 1$. Within a few weeks I could do it for $p = 2$ and then for rational p. I brought him the proof for $p = 2$. He wouldn't look at a handwritten proof, saying that I could give him a lecture on it or prepare a typewritten document. I gave him the lecture and indicated the stack of papers containing the argument for rational p. His comment was "There has to be a better way". I found the general argument and wrote it up within two months of my start. When I went to him with this I naively thought that this would be my dissertation, but within a few minutes he was describing another problem in a different area. When I had done that I indicated that I had thought I was done and he responded "Do you have children?" I told him I did not. He asked if I was married. Again the answer was no. He then said "Well, in that case I'm going to keep you around for a while and see what I can get out of you". Zygmund was gone for the quarter as I was finishing my dissertation research and Graves very kindly acted as my advisor, listening to me very patiently. In my last year and a half at Chicago, I was a research associate in the Cowles Commission for Research in Economics. Herstein was there at the same time and we became friends. The Commission was about to move to Yale at that time and I was invited to go with it, but I had received a Fulbright grant to the University of Vienna and I chose to go there in 1952. Although there were excellent mathematicians there, for example, Radon and Prachar, the mathematical climate was not particularly stimulating for a person of my interests. During the year Zygmund arranged two offers for me, and I chose to go to Purdue.

Lafayette and West Lafayette were sleepy little towns at that time, far different from anything I had ever experienced. The university hired twelve instructors that year, all with excellent credentials. We were a trial for the chairman; many of us did not come from places with the standards of dress and decorum that he was trying to maintain. However, adjustments were made on both sides. I made very good friends there, Michael Golomb, Lamberto Cesari, Casper Goffman and my fellow beginner, Robert Zink. Goffman and I became very close friends and collaborators and his influence altered my view of mathematics considerably. For a time, Paul Erdos was at Notre Dame and it was his habit to give up his hotel room on weekends, pack his belongings in two suitcases, leave one at the hotel and take the bus to Purdue, where he would stay with the Golombs. Whenever he got his paycheck, he would cash it and turn up at my place with a stack of five-dollar bills. He would have a list of charities to which he wished to contribute. We would sit together and I would write checks to these charities as he handed me the corresponding number of five-dollar bills. A substantial number of these contributions were to Native American groups.

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It was from Paul that I learned of the plight of the people on the reservations, and to this day, most of my charitable contributions are to these groups.

My research blossomed in my time at Purdue. I wrote some substantial papers and produced two doctoral students. However I had a serious altercation with my dean. I taught an introductory real analysis course which was taken by undergraduates and a few graduate students. A new chair of an engineering department made the mistake of directing unqualified graduate students into the course. I was of the opinion that graduate students should be graded as undergraduates were. My dean wanted me to give graduate students higher grades than undergraduates received for similar achievement. I refused to change my grades. I was summoned to the dean's office where, in his Texas drawl, he told me "Dan'l, Purdue isn't big enough for the two of us. I guess you know what that means." It was like a scene from an old western movie.

I didn't look for another position. My friends at Purdue spoke to Morris Marden at the University of Wisconsin-Milwaukee and I was hired there. At that time, UWM was a tiny institution, recently formed by combining an extension of the University at Madison and a teachers college. It was just beginning a master's program. It turned out to be a very fortunate move for me. I met and married my wonderful wife Mudite there. In my second year there I received an offer of a full professorship at Wayne State University. Togo Nishiura, after completing his doctoral work with Cesari, had also come to UWM, and he also received an offer from Wayne.

Wayne was a stimulating environment for Nishiura and me. It had a strong group in analysis, including Vladimir Seidel, Frederick Bagemihl, Hidegoro Nakano, Albert Bharucha-Reid, Takashi Ito, and Leon Brown. The graduate students were also very strong. It seemed that Detroit had several gifted students who, for various reasons, were unable to leave the area. We also had some very good foreign students. In my years there, six students completed their doctoral dissertations with me.

Meanwhile, our family had grown; Mudite and I and our three lovely children went to Berkeley for a sabbatical during the 1967-68 academic year. On our return, we decided we needed more living space and purchased another house in Detroit. Before we were able to move, I received a very advantageous offer from Syracuse University. I accepted it, of course, and purchased another house in the Syracuse area. We now had two houses too many in the very depressed market which followed the Detroit riots of 1967. It took some ingenuity to dispose of them, but we did. Leaving Detroit was not so easy; we had made good friends there whom we would miss, particularly the Nishiuras. However Syracuse had many advantages including a superb library, an excellent environment for our children, and the presence of Wolfgang Jurkat, whose work I greatly admired.

Syracuse was an interesting institution. Don Kibbey, the chairman who hired me, was a person of great influence in the university and he used this influence for the benefit of the department. When he was forced to move upward in the administration, his power to help the department waned, and persons who had been jealous of his influence used his absence to deny the department many of

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the perquisites it had enjoyed. This made for many lean years. In addition, the main strength of the department had always been in analysis, but with the more democratic department structure which followed Kibbey's departure, some members of the other groups united to influence hiring tactics to the ultimate detriment of the department. As a frequent member of the department executive committee and ultimately the chairman, I learned much about the futility of making predictions concerning future research productivity based on early performance. Many gifted and productive people simply lose the drive which first inspired them. Perhaps in another department culture they would have fared better. Others fixate on one problem and may spend years on it without discernible progress. A strict department tenure policy would seem to be the solution, but often the faculty believes that the time allowed is too short and their personal feelings interfere with their scientific judgement.

My time at Syracuse was very satisfying. My research and my family thrived. My two daughters went to Cornell and Syracuse. They both became physicians. My son studied electrical and computer engineering at Berkeley and obtained a Ph.D. in computational linguistics at Brandeis. Mudite, who had a master's in mathematics from UWM, decided to study computer science. She reached the point of doctoral qualifiers, but decided that the children needed more attention than they would get if she continued, so she added another master's to her belt. I had eleven doctoral students at Syracuse and would have had more if I had not become chairman and if the stream of qualified students had not begun to dry up. I always found that supervising dissertation studies was enjoyable and also stimulated my own research. Being the chairman did give me some satisfaction. I was able to provide computer equipment and travel funds to the faculty that they might not otherwise have had, and with the help of my associate chair, John Troutman, was able to do some good things for our graduate students as well. During my term I made some notable appointments and, overall, I hired one quarter of the faculty.

I note that I had steady research support until I reached Syracuse. What happened then illustrates the tendency in this country for research support to follow fashion instead of relying on the abilities and judgement of the researcher. My first proposal to study generalized bounded variation met with a response on the order of "I have great respect for the previous work of the applicant, but I can't understand why he wants to do this". I was greatly gratified by the interest that was shown in my work by Eastern European mathematicians. It was widely cited and used. I had very pleasant letters from Orlicz and Chanturiya expressing their appreciation of this work. I am happy to see that the spaces of functions of generalized bounded variation I introduced are still the subject of study.

Retirement has had both its good and bad points. I have been graciously received by the mathematics department at Florida Atlantic University. I miss my friends in Syracuse and also its superb library. Modern computer technology and interlibrary loan can compensate to some extent, but nothing can replace the sensation of walking into a library with hundreds of the latest issues of mathematics journals arrayed

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on its shelves. I have managed to publish a paper per year since retirement and I also spend much time in communication with my former students and colleagues and in performing my editorial duties for the "Journal of Mathematical Analysis and Applications".

RESEARCH

My current research interests are high-indices theorems, interpolating polynomials, and statistical summability. Statistical convergence of sequences was defined by Fast in 1950, and G. G. Lorentz and Sierpinski offered equivalent definitions independently. No suitable definition for continuous limits, e.g., for Abel means, has been given. I intend to pursue this question and try to find high-indices theorems for such methods of summability. In his text, Zygmund considered the partial sums of interpolating polynomials. I would like to estimate the degree of approximation of these partial sums to functions of various classes.

In my discussion I have grouped my work by area. Several papers could have been in more than one group. Papers will be referred to by the numbers in the publication list. References to papers of others will not be given; they are easily obtained from the cited papers. I cannot describe all of the papers in each group, but I will try to describe their main thrust as well as giving some extra attention to results that I am particularly fond of. Of course there are some papers that fall into none of the principal groups and I will not discuss these.

I can always recall the projects that I tried and failed to complete. One such project is very vivid in my memory. In the middle fifties I read a paper by W. Nef concerning regular functions on the quaternions. It was very interesting, but I discovered a crucial measure-theoretic error. I described this to my colleague, Artur Rosenthal, a renowned expert in real analysis, and he told me that it was impossible; he knew Nef and didn't believe that he could have done this. I showed him the paper and he confirmed that I was correct. I was strongly attracted by the idea. When I studied Hilbert space with Stone, we did it over the quaternions and I now envisioned doing harmonic analysis over the quaternions. My next thought was: why not do it over Clifford algebras? I wrote a detailed proposal and received a grant to pursue it. Unfortunately, my personal circumstances were such that I could not really undertake a project of this scope. I was able to pursue various problems with Goffman that were more limited and could be resolved in a shorter time, but maintaining the concentration necessary for this large project was impossible. I soon became captivated by the approach to Fourier series that we were pursuing and never returned to this project. These problems have since been taken up by many researchers and Clifford analysis has become a subject of considerable interest.

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High Indices

The high indices theorem of Hardy and Littlewood is a result about Abel summability, stating that if a power series $\sum a_k x^{n_k}$ has a finite limit as $x \to 1-$, where the sequence $\{n_k\}$ has Hadamard gaps, i.e., $n_{k+1}/n_k > q > 1$, then $\sum a_k$ converges. The theorem is valid for any sequence of real numbers $\{\lambda_k\}$ increasing in the same manner. It is convenient to set $x = e^{-s}$ and consider the limit as $s \to 0+$. We then have a Dirichlet series, $f(s) = \sum a_k e^{-\lambda_k s}$. Zygmund considered absolute Abel summability, showing that

$$\sum |a_k| \leqslant A_q \int_0^\infty |f'(s)|\, ds.$$

In [1], we showed that

$$\sum |a_k|^p \lambda_k^{p-1} \leqslant A_{qp} \int_0^\infty |f'(s)|^p\, ds,$$

for $p > 1$. Note that the Hardy Littlewood theorem and Zygmund's theorem are the extreme cases of this inequality, corresponding to $p = \infty$ and $p = 1$ respectively. Other results, with weights in the integrand, were also proved. We did not consider the case $0 < p < 1$.

Note that the integral in Zygmund's theorem is the variation of f. The interval of integration in both these results can be taken to be finite. This suggests that we consider the hypothesis that f belongs to some class of functions of generalized bounded variation. We returned to this problem after fifty years and showed, in [72], that this result can be extended to $p \in (0,1)$ and that if we assume that $f \in \Gamma BV$ with $\Gamma = \{\gamma_k\}$, then

$$\sum |a_k| / \gamma_k \leqslant A_q V_\Gamma(f),$$

the variation extended over a finite interval $(0, B)$, whose length depends on q and Γ. In [81], we establish a similar result for $f \in \Phi BV$.

In [12] we consider f, a gap series as above, with $s = \sigma + it$, the function being analytic in the right half-plane. Suppose C is a curve terminating at the $s = 0$, on which $t \searrow 0$ as $\sigma \to 0+$. We give a Tauberian condition which ensures the convergence of the series at 0 if the limit of f exists as $s \to 0$ along C.* [8] involves a similar limiting process.

Reflexivity and Summability

The Banach-Saks theorem asserts that any bounded sequence in $L^p(0,1)$ or $l^p, p > 1$ has a subsequence whose $(C,1)$ means converge strongly. Banach spaces with this property are said to have the Banach-Saks property. Kakutani showed that

*We asserted that an analogous result would hold if C terminated at another point on the vertical axis. The reviewer (*MR*) misunderstood my statement and said that this was incorrect.

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for weakly convergent sequences in a uniformly convex Banach space the same conclusion holds. Since we now know that uniform convexity implies reflexivity, "weakly convergent" may be replaced by "bounded". A sequence-to-sequence summability method $T = (c_{mn})$ is regular if it satisfies the Toeplitz-Silverman conditions. A matrix satisfying the property $\sum_{n=1}^{\infty} |c_{mn}| \to 1$ as $m \to \infty$ is called *essentially positive.* A Banach space is said to have property $\mathcal{S}(w\mathcal{S})$ if for every bounded sequence there is a regular summability method T and a subsequence whose $T-$means converge strongly(weakly). In [11], Nishiura and I showed that, for a Banach space B, the following three statements are equivalent: (i) B is reflexive; (ii)[(iii)] B has property $\mathcal{S}(w\mathcal{S})$ with essentially positive T. A. Baernstein has given an example of a reflexive Banach space which does not have the Banach-Saks property.

In [19], more general summability methods are considered. T is *convergence preserving* if

(i) $\sum_{n=1}^{\infty} |c_{mn}| < H < \infty$ for every m;
(ii) $\sum_{n=1}^{\infty} c_{mn} \to c$ as $m \to \infty$;
(iii) $c_{mn} \to c_n$ as $m \to \infty$ for every n.

Here c and c_n are finite. T is regular if and only if $c = 1$ and $c_n = 0$ for all n. A method is *almost regular** if it satisfies (ii), (iii), and

(iv) $c \neq \sum_{n=1}^{\infty} c_n$,

the latter sum being supposed convergent. When $c = 1$ and $c_n = 0$ for all n an almost regular* method is *regular** or T^* in the notation of Zygmund. We showed the following result:

In a Banach space, property $w\mathcal{S}$ with almost regular T implies reflexivity, and reflexivity implies $\mathcal{S}$ with positive row-finite column-finite regular T.*

In [21] we discuss a paper of Klee in which he showed that certain Nakano spaces, l^{p_i}, contained bounded sequences with no $(C, 1)$ summable subsequences. We give necessary and sufficient conditions for the reflexivity of l^{p_i}, from which it is seen that the particular spaces he considered are not reflexive.

Harmonic Analysis

In this classification I include papers on square functions and Fourier series on groups. Zygmund showed equivalence relations for the Littlewood-Paley functions g and g^*. Thus if f is a function in $L^p, p > 1.$ on $(0, 2\pi)$, then for the corresponding g, we have

$$A_p \|g\|_p \leqslant \|f\|_p \leqslant B_p \|g\|_p \,,$$

and similarly for g^*. He then showed such a theorem for the function $s(\theta)$, which is the square root of the area of the mapping by a function $f(z)$ in $\mathcal{H}^p, p > 1$, of a kite-shaped region terminating at $e^{i\theta}$. In [4] we proved similar results for functions analytic in a half-plane. He also considered the Marcinkiewicz function μ and proved a similar result. In [2, 5] we extended this to functions in $L^p(-\infty, \infty)$. The reviewer in MR said that this was done by "methods akin to those used by Zygmund", which

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is what I wrote about the proof of one of the inequalities. The proof of the other side was unexpectedly difficult. The referee of [5] asked me to shorten the paper and I complied. After it appeared, Zygmund told me that he was the referee and that he was sorry he had asked that, for it made the proof very difficult to follow.

In [15], we considered another problem related to area. This generalizes a result due to Lusin and Zygmund for the unit circle. Suppose $f(s) = \int_0^\infty e^{-sx} d\gamma(x)$, where $s = \sigma + i\tau$, is analytic in the half-plane $\sigma > 0$. Let

$$\alpha(x) = \sup_{0 \leqslant h \leqslant 1} |\gamma(x+h) - \gamma(x)| = o(1) \text{ as } x \to \infty.$$

Suppose Ω is a region in $\sigma > 0$ bounded by a segment $[i\alpha, i\beta]$ of $\sigma = 0$ and a Jordan arc. If $\iint_\Omega |f'|^2 \, d\sigma d\tau < \infty$, then $\int_0^\infty e^{-sx} d\gamma(x)$ converges a.e. on the segment $(i\alpha, i\beta)$ and uniformly on any closed subsegment of points of continuity. If $\alpha(x) = o(x^k)$, $k > 0$, we can replace convergence by (C, k) summability. Only the argument for (C, k) summability is given It involves dividing the Laplace integral into two parts and finding a trigonometric series such that it and its conjugate are uniformly (C, k) equisummable with the two parts. This enables us to reduce the problem to that for the circle.

In [25, 29, 66] we consider functions defined on bounded $0-$dimensional, metrizable, compact, abelian groups. Using the ordering defined by Vilenkin for the dual group, in [25] we generalize a result of Salem to Fourier series of continuous functions. This has several corollaries such as an analogue of the Dini-Lipschitz test. We also defined a notion of bounded fluctuation which is weaker than bounded variation. Functions satisfying this property had uniformly convergent Fourier series. In [29] we define a notion of harmonic bounded fluctuation, resembling harmonic bounded variation for trigonometric Fourier series. Continuous functions with this property are shown to have uniformly convergent Fourier series. An analogue of the Lebesgue test for continuous functions is proved. In [66], we proved a more general version of the Lebesgue test.

Change of Variable

My interest in this subject began while working with Goffman on the convergence of the Fourier series of a function f under every composition with a homeomorphism g. In [17] we found the condition on a continuous function which ensured this. The idea behind this was based on a linearization of the Dirichlet kernel used by Salem to prove a theorem on uniform convergence of Fourier series. In [28], a similar result is proved for preservation of uniform convergence and another proof of this is given in [55]. In [32] we showed that if a function f was *equivalent* to a function F that satisfied the condition of [17], then the Fourier series of $f \circ g$ would converge for every homeomorphism g. Equivalence here means that $f = F$ except on a set of *universal measure zero,* i.e., a set E such that the Lebesgue measure of $g(E)$ is zero for every homeomorphism g of $[-\pi, \pi]$ with itself. In this paper we assumed that

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the condition of [17] was also appropriate for functions whose only discontinuities were jumps, the regulated functions. Although this result can be demonstrated by methods similar to those for continuous functions, there are substantial difficulties, and the proof of this result appears in [70].

The result of [64] with Jurkat is one of which I am very fond. We showed that if f is a continuous function on the circle group T, then there is a homeomorphism g of T onto itself such that the conjugate of $f \circ g$ is continuous and of bounded variation. This generalizes the Bohr-Pál theorem, which says that there is a continuous increasing g mapping $[-\pi, \pi]$ onto itself such that the Fourier series of $f \circ g$ converges uniformly.

We define the Hadamard functions of *bounded deviation* to be integrable on T and such that $\left|\widehat{f\chi_I(n)}\right| \leqslant C/n$ for a constant C, every integer n, and every subinterval I. In [31] we showed that $f \circ g$ is of bounded deviation for every homeomorphism g if and only if f is equivalent to a function of bounded variation. In [60] we refined this somewhat.

This result led naturally to consideration of the preservation of order of magnitude of Fourier coefficients under change of variable. Chanturiya defined the *modulus of variation* of a function f, $\upsilon(n, f) = \sup \sum_i^n |f(I_k)|$, $\{I_k\}_1^n$ running over all collections of disjoint subintervals. If $h(n)$ is a positive, nondecreasing, concave-downward function on the positive integers, then $V[h]$ is the class of regulated functions for which $\upsilon(n, f) = O(h(n))$. For regulated functions the following are equivalent: (i) $\left|\widehat{f \circ g(n)}\right| \leqslant C_f h(n)/n$ for every g; (ii) $\left|\widehat{f \circ g(n)}\right| \leqslant C_{f,g} h(n)/n$ for every g (iii) $f \in V[h]$.

Fourier Series and Generalized Variation

This begins with [27], where the notion of ΛBV was introduced and applied to Fourier series. Let $\Lambda = \{\lambda_n\}$ be a nondecreasing sequence of positive numbers such that $\sum 1/\lambda_n$ diverges. If f is a function defined on an interval such that $\sum |f(I_n)| / \lambda_n$ is bounded for all collections of nonoverlapping intervals $\{I_n\}$, f is said to be of Λ -bounded variation (ΛBV). If the λ_n are bounded, we have the classical Jordan bounded variation (BV); if $\lambda_n = n$, we have *harmonic* bounded variation (HBV). It was shown that we can replace BV by HBV in the Dirichlet-Jordan theorem. If one were to use ΛBV instead of HBV, where $\Lambda BV - HBV \neq \emptyset$, then the theorem would fail. Also, the relationship between the Banach indicatrix and ΛBV was made clear. Paper [35] proved the properties of ΛBV that were used in the previous paper and [39] gave a different proof of the generalized D-J theorem. Berezhnoi has shown that the generalization of the D-J theorem with HBV is the strongest test for uniform convergence that can be obtained with generalized bounded variation.

The second major result in this area was the localization theorem with Goffman [36, 35]. We substituted HBV for BV in the Cesari-Tonelli definition of bounded variation for a function of two real variables and proved a localization theorem for

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double Fourier series. As described for the previous result, this theorem cannot be improved. When I presented this result to Zygmund, he responded enthusiastically, saying "This solves the localization problem for double series".

We have many other papers in which we explicate the properties of functions of generalized bounded variation, estimate the magnitude of Fourier coefficients, consider the degree of approximation of partial sums of Fourier series to functions of various classes, absolute convergence and many other topics which can be readily apprehended from the titles of the papers. There is, however, one to which I would like to draw attention.

We have previously alluded to a result of Salem which was a test for uniform convergence of Fourier series. Bary said of this that it appears "at first glance to be hardly suitable for application", but it has been the origin of many of our investigations; we refer, in particular to [61]. Let $f \in L^1(T)$ and, for odd positive integers let

$$T_n(x,t) = \sum_{k=0}^{(n-1)/2} \left[f(x + (t + 2k\pi)/n) - f(x + (t + (2k+1)\pi)/n) \right] / (2k+1)$$

and let $Q_n(x,t)$ be obtained from this by replacing the $+$ sign after x by a $-$ sign. These functions were defined by Salem. Our principal result was this: Let x be a symmetric Lebesgue point of f, then the necessary and sufficient condition that the Fourier series of f converge at x is that

$$\int_{\pi}^{2\pi} (T_n(x,t) + Q_n(x,t)) \sin t dt = o(1) \quad as \quad n \to \infty.$$

Various definitions of bounded variation for functions of two variables have been proposed for over a century. Early definitions used 2-dimensional interval functions and later definitions, e.g., those of Cesari and Tonelli, employed variation over segments in one variable as a function of the other variable. Recently there have been results obtained by applying the notions of ΛBV to intervals. The most successful have been the results of Dyachenko. Among these is our joint [79]. Suppose f is defined on $A = [a,b] \times [c,d]$ and Λ is as above. We say $f \in \Lambda^* BV$ if (i) $f(a,\cdot)$ and $f(\cdot,c)$ are in ΛBV, and (ii)

$$\sup_{\Gamma} \sum_k \lambda_k^{-1} \left| f(\alpha_k, \gamma_k) - f(\alpha_k, \delta_k) - f(\beta_k, \gamma_k) + f(\beta_k, \delta_k) \right| < \infty,$$

where Γ is the set of finite collections of nonoverlapping subrectangles $[\alpha_k, \beta_k] \times [\gamma_k, \delta_k]$ of A. Various results were proved which relate this to previous definitions The principal result concerns continuity and convergence of Fourier series. We have

(i) If $f \in \Lambda^* BV$, then there exist at most countable sets $P \subset [a,b]$ and $Q \subset [c,d]$ such that f is continuous at every $(x,y) \in A$ such that $x \notin P$ and $y \notin Q$ and, at every point of A, the limit of $f(x,y)$ from within each open quadrant exists;

(ii) If f is $2\pi - periodic$ in each variable and in $\Lambda^* BV(T^{2})$ with $\Lambda = \{n/\ln(n+1)\}$, then the rectangular partial sums of the Fourier series of f are uniformly

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bounded and converge (Pringsheim) at each point to the arithmetic mean of the quadrant limits.

It is also shown that, in a certain sense, this convergence result cannot be improved.

Recent work has been in the area of trigonometric interpolation [80]. Zygmund studied the convergence of partial sums of interpolating polynomials and proved a theorem resembling the Dirichlet-Jordan theorem. We gave a different proof of this theorem, showed that bounded variation could be replaced by harmonic bounded variation, and that this result was best possible.

Representation of Functions, Orthogonal Series

A result of Talalyan states that if $\{\phi_n\}$ is a basis of some L^p space, where $1 \leq p < \infty$ then if any finite number of functions is deleted from $\{\phi_n\}$, the remaining sequence is still complete in the space of measurable functions L^0, with the topology of convergence in measure. This implies the existence of universal expansions and the existence of a subsequence $\{\phi_{n_k}\}$ which is complete in L^0 even though the complement of $\{n_k\}$ is infinite. We supplied a simple proof of a (more general) proposition where $\{\phi_n\}$ is only supposed to be complete in L^0 and the proof hinges on the fact that the dual of the space L^0 is trivial [7, 26]. With Kazarian, we made substantial generalizations of this result in [76].

Suppose $[\phi_n\}$ is a system of functions on (0.1) with $\phi_0 \equiv 1$. For $n = 2^{k_1} + 2^{k_2} + \cdots + 2^{k_s}, \{k_i\}$ an increasing sequence of nonnegative integers, set $\psi_n = \phi_{k_1} \cdot \phi_{k_2} \cdot \cdots \cdot \phi_{k_s}$. If $\{\psi_n\}$ is an orthogonal system on $(0,1)$, it is called a *W-system.* It is generally assumed that $\int |\psi_n|^2$ is constant and $|\phi_n(x)| \leqslant 1$ a.e. for every n. Many results had been obtained that paralleled standard theorems on the Walsh system$\{w_n\}$, which is derived in this manner from the Rademacher functions. We showed [22, 49] that completeness of $\{\psi_n\}$ is equivalent to the existence of a $1-1$ measure-preserving mapping η of $(0,1)$ onto itself such that $\psi_n = w_n \circ \eta(x)$ a.e. for every n. Now let us consider a rearrangement of the sequence of Rademacher functions. If we rearrange the Walsh functions with indices $\{m_n\}$ obtained as described above, the summability and convergence behaviors of the series $\sum c_n w_n$ and $\sum c_n w_{m_n}$ are the same.

Shortly after this result was obtained I was reading the text of Alexits and found a problem of Steinhaus. I realized that an idea used in the proof of this result was relevant and obtained a very short solution [24]. Let $\{f_n\}$, $n = 1, 2, \ldots$be a sequence of measurable functions on $(0,1)$. Consider the system $\mathcal{S} = \{f_1^{m_1}(x) \cdot f_2^{m_2}(x) \cdot \cdots \cdot f_n^{m_n}(x)\}$, where $m_i = 0, 1, 2, \ldots$, $n = 1, 2, \ldots$. Steinhaus asked for the necessary and sufficient condition that $\mathcal{S}$ be closed in L^2. Renyi introduced the following notion: $\{f_n\}$ is *maximal* if there is a set Z of measure zero such that if x_1 and $x_2 \notin Z$ and $f_n(x_1) = f_n(x_2)$ for every n, then $x_1 = x_2$. He showed that maximality was sufficient. Now assume that $\mathcal{S}$ is closed in L^2. Consider the function $f(x) = x$. There exists a sequence $\{P_n\}$ of linear combinations

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of elements of $\mathcal{S}$ such that $\|P_n - f\|_2 \to 0$ as $n \to \infty$. Thus there is a sequence $\{n_k\}$ such that $P_{n_k}(x) \to x$ a.e. in $(0,1)$ as $k \to \infty$. Let $Z = \{x : P_{n_k}(x) \nrightarrow x\}$. Then $m(Z) = 0$. Now if x_1 and $x_2 \notin Z$ and $f_n(x_1) = f_n(x_2)$ for every n, we have $x_1 = \lim P_{n_k}(x_1) = \lim P_{n_k}(x_2) = x_2$.

Real Analysis

Much of what we have done in this area is connected to Fourier series and so may be found in that section. There are a few things that were not related to that area and are of some interest. The first paper I wrote with Goffman [6] arose from Menchoff's work on trigonometric series. He defined the concepts of upper and lower limits in measure for sequences of extended-valued measurable functions. His method involved transfinite induction, and the main properties of these limits were difficult to establish. By using the fact that these functions form a complete lattice, we were able to define the limits in a much simpler fashion and also to derive other expressions for them, e.g., for a sequence $\{f_n\}$, the upper limit in measure is $\inf[\overline{\lim}_{n\to\infty} g_n(x) : \{g_n\} \in \mathcal{F}]$ where $\mathcal{F}$ is the class of all $\{g_n\}$ such that $\{f_n - g_n\}$ converges in measure to zero.

Denjoy introduced the approximately continuous functions in his work on derivatives. In [9] we showed that the approximately continuous transformations from an Euclidean space into a metric space are of Baire class 1, have a Darboux property and have separable images. A measurable set in E_n is *homogeneous* if it has metric density one at each of its points. E_n can be topologized by taking the homogeneous sets as the open sets, yielding the *d-topology.* The approximately continuous functions are the continuous functions in the d-topology. We discussed connectedness and the generalization of the Darboux property. Although we discovered that Denjoy had implicitly defined this topology and it had been done explicitly in the text of Pauc, it had gone unnoticed and this paper led to a revival of interest in the subject.

Summability

Suppose φ is a nonnegative function on an interval to the right of the origin, that $\varphi(0) = 0$ and that $\varphi(t) = O(t)$ as $t \to 0$. A set E is said to be φ-dense at a point p if $m(E \setminus I)/\varphi(m(I)) \to 0$ as $m(I) \to 0$, p in the interval I. If $\varphi(t) = t^\alpha$, E is said to be α-dense at p. The $\varphi\text{-}\lim_{ap}\limits_{t\to t_0} g(t) = a$ if, for every $\varepsilon > 0$, $\{t : |g(t) - a| < \varepsilon\}$ is φ-dense at t_0. Letting $\psi_{x_0}(h) = f(x_0 + h) - f(x_0 - h)$, we define the φ-approximate symmetric derivative at x_0 by $\varphi\text{-}f'_{aps}(x_0) = \varphi\text{-}\lim_{ap}\limits_{h\to 0} \psi_{x_0}(h)/2h$. A function f satisfies condition A_q at x_0 if, for some sufficiently large M, $\int_{E_M\cap(0,t)} |\psi_{x_0}(u)|\,du = o(t^q)$, where $E_M = \{t : |\psi_{x_0}(t)| \geqslant M\}$. In [16] we showed that if f is integrable and 2π-periodic, $\psi_{x_0}(h)$ is essentially bounded in a neighborhood of $t = 0$, and for $\alpha = 2$, $\alpha\text{-}f'_{aps}(x_0) = y$, then the differentiated Fourier series of f is Abel summable to y

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at x_0. In [20] we showed that essential boundedness can be replaced by condition A_q with $q = 2$. This is best possible in the sense that $o(t^2)$ cannot be replaced by $O(t^2)$.

In [67] we showed that if $\left[f(x_0 + h) - f(x_0 - h)\right]/t$ is of harmonic bounded variation in a neighborhood of $t = 0$, then the differentiated Fourier series of f is $(C, 1)$ summable to $\frac{1}{2}\left[f'_+(x_0) + f'_-(x_0)\right]$. A theorem on uniform summability is also proved.

In [71] we consider the Fourier series of functions in $\Lambda BV \supset HBV$. We assume $k/\lambda_k = O(1)$. A summability method (W, λ) is defined which yields a D-J-like result for functions of this class with convergence replaced by (W, λ) summability. A condition weaker than ΛBV is defined which yields the same result, but this condition is not a generalization of bounded variation.

Survey Papers

We wrote several survey papers, the first with Goffman on Fourier series [23]. We were very pleased that many people found this paper interesting and useful. I wrote several others on generalized bounded variation [37, 40, 41, 58] and [54] on change of variable. I regarded this as an important activity in that it often succeeded in bringing outstanding problems to the attention of others.

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