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In Memoriam

George G. Lorentz (1910–2006)



George G. Lorentz, one of the original Editors of this Journal, passed away in the early morning of January 1, 2006, after a mercifully short period of illness at his home in Chico, California. He is survived by his four daughters Mary, Irene, Olga, and Katherine, by his son Rudolph, and their families.

Brief curriculum vitæ of George Lorentz

By Manfred v. Golitschek

George was born in St. Petersburg, Russia, on February 25, 1910. He attended a Russian high school (1923–26) at Tiflis (Tbilissi), the capital of Georgia, and the Tbilissi Institute of Technology (1926–28). In 1928, he moved back to his place of birth, meanwhile renamed

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Leningrad, and became a student at the School of Mathematics and Mechanics of the Leningrad State University where he first received his mathematics diploma in 1931, and then he was awarded the degree of Candidate of Physical-Mathematical Sciences (Ph.D.) in 1936. He was a Docent (Associate Professor) at the same university until 1942.

Meanwhile, in June 1941, the war with Germany reached Leningrad, bringing horrible suffering to its population. George and his wife Tanny managed to leave Leningrad in April 1942 and moved to the Northern Caucasus, and later into refugee camps in Kalush and Torun in Poland. From Torun, George was able to send some papers to Professor Knopp in Tübingen, Germany, who was the editor of *Math. Zeitschrift*. Professor Knopp and Professor Süss arranged for him, his wife, and their newborn son Rudolph to move to Tübingen in 1944. At Tübingen, George became an assistant of Professor Kamke. He later obtained a doctoral degree there and also the "Habilitation", that is, the right to lecture at the university. He became Docent at the University of Frankfurt (1946–48) and "Honorarprofessor" at the University of Tübingen (1948–49). He had his first two doctoral students in Tübingen, W.B. Jurkat and K.L. Zeller, who became well-known professors at Ulm and Tübingen, respectively. In 1949, George accepted a fellowship of the Lady Davis Foundation at the University of Toronto. There, George had four doctoral students, among them G.M. Petersen and P.L. Butzer, now well-known professors. In 1953, George accepted a professorship at Wayne State University in Detroit. Then he moved to Syracuse University (1958–68). Among his Ph.D. students at Syracuse, J.A. Roulier and S.D. Riemenschneider became successful professors. His review paper [52][1] written there, on metric entropy, widths, and superpositions, won the best paper of the year award from *Amer. Math. Monthly* for 1962. His marvellous book on 'Approximation of Functions' [B2] was written at Syracuse. In 1968, at the age of 58, George was still not tired of moving and went to the University of Texas at Austin (1968–1980). Together with E.W. Cheney, L.L. Schumaker, and H. Berens, they built a Center for Approximation Theory. In 1973, he spent a year in Stuttgart as a winner of the A.v. Humboldt Research Prize, and was awarded an honorary Dr. rer. nat. from the University of Tübingen in 1977. Among his Ph.D. students at the University of Texas, R.C. Sharpley became a successful professor. George retired in 1980 but did not stop doing mathematics. Indeed, between 1980 and 1996, he wrote more than 30 papers and finished Volume 1 on Constructive Approximation in 1993 (with R.A. DeVore) [B4] and Volume 2 in 1996 (with M.v. Golitschek and Y. Makovoz) [B6]. In 1996, the University of Würzburg honored him with a Dr. rer. nat. He had just moved to Chico, California, to be close to his wife and their daughter Olga. Tanny Lorentz died in 1999. While George felt during those last ten years that "his abilities doing mathematics had decreased", he did not cease scientific work, he simply shifted focus, to topics in history, philosophy and ethics. For example, he wrote [143] "Mathematics and Politics in the Soviet Union from 1928 to 1953", and gave the Shanks Lecture at Vanderbilt University in May of 2000, on "Values and Ethics in Mathematics"; see [142].

George contributed to the development of mathematics as an active researcher for more than seventy years. His research papers and his books are a rich source of new ideas and had significant influence in the development of analysis in general, and approximation theory in particular. His personality, his mathematical talent, his friendship, and his support had a tremendous influence on the mathematical and personal life of many of his colleagues.

As one of this group, I now add a few personal recollections. I met George for the first time in 1968. It was at Professor Butzer's institute at Aachen where I spent the summer semester

[1] Here and in what follows, bracketed labels refer to items in the 'Publications of G.G. Lorentz' below.

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as a research fellow. I remember that George, already a highly respected professor, asked me about details of my research and gave me valuable hints about related Russian literature. I was even more impressed when I met George at the Texas Conference in 1973 at Austin. He and his wonderful wife Tanny had invited a huge crowd of mathematicians into their house. They loved to have guests, and they enjoyed combining professional and private life. My connection with George became very close after he invited me to write, jointly with him and Yuly Makovoz, a book on constructive approximation. Between 1987–1995, he spent several weeks and months at the University of Würzburg. It was a pleasure doing research with him. He always had an excellent feeling for good and promising problems and a deep source of ideas and knowledge for solving them. While doing joint research, he was always friendly, patient, and generous. This was not always the case when we worked on the book project. Then he was more critical and less patient since he feared not being able to finish the project, and he knew that his style of writing was excellent. We finished in 1995, George was already 85 years old and still in good shape. We met afterward several times, at conferences, at his home at Chico, and also, for the last time in the summer 2004, at Würzburg. I am very grateful to have met such a remarkable person and friend.

A much more detailed description of the life of G.G. Lorentz follows next, and this description also provides a first-hand view of Lorentz' mathematical work. His scientific work until about 1975 is also covered in [C2], and his work from 1975 to 1990 is the subject of [C4].

A list of the students (and even the students' students) of G.G. Lorentz can be found at the web site http://www.genealogy.math.ndsu.nodak.edu/ of the Mathematics Genealogy Project. This list is deficient only in that it fails to count K. Zeller as one of Lorentz' students (thus depriving Lorentz of more than 130 mathematical descendants).

My autobiography[2]

By G.G. Lorentz

I was born in St. Petersburg (Leningrad), Russia on February 25, 1910. My father, Rudolf Fedorovich Lorentz, was an engineer involved with construction and administration of railways. My mother, Milena Nikolaevna, came from the large family of princes Chegodaev. My father was very successful in his profession. However, in 1906, he refused to participate in the suppression of a strike on his railway near St. Petersburg. Since then, he was not permitted to work on railways owned by the state. This forced him to move to the Caucasus, where most railways belonged to private companies. In 1913–1918 we lived in Armavir in North Caucasus. This middle-sized town changed hands (from White to Red and back) three times during the civil war that followed the revolution. Later, in 1919–1922, we lived on a farm near Sochi, until the times became more stable. Next we moved to Tiflis (=Tbilissi), the capital of Georgia.

There I visited first a Russian high school, then, in 1924–26, a German one. In this way I acquired good knowledge of German (at home we spoke Russian). My college education began in 1926 at the Tbilissi Institute of Technology, where I took mathematics from good teachers: Muskhelishvili and Razmadze (the latter used to start his calculus course for engineers with a

[2] Reprinted, verbatim, from [C5], with kind permission of Springer Science and Business Media. An earlier version appeared in [C3].

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two-hour presentation of the Dedekind cuts). They encouraged me to transfer to the School of Mathematics and Mechanics of the University of Leningrad.

In 1928, Leningrad still retained the flavor of the brilliant capital of the empire of the czars, with its magnificent architecture, mighty rivers, palaces of the nobility, imperial suburbs. The museums, theaters, opera, tradition of famous writers and poets has been a fascination for me. It had been also the seat of the Academy of Sciences. With its departure to Moscow, the University of Leningrad definitely took second place to its Moscow counterpart. In 1928–30, with my friend I.I. Gordon, we happily learned to really understand mathematics. We got our diplomas in 1931.

Bad times came in 1930–33. The mathematical societies of Leningrad and Moscow were disbanded (Egorov, the president of the latter, died in prison). There was a trend to strict applications. The theory of real functions was declared reactionary, the complex functions theory, in the contrary, progressive. Only 26 students of the department completed their study in 1931, twenty of them in mechanics, only six in mathematics. Neither Gordon nor myself were retained as aspirants (=students studying for a candidate degree, which is roughly equivalent to our Ph.D. degree). Instead, I became a teaching assistant at the University with a heavy teaching load of 20 hours, which increased when I had accepted a second job at the Herzen Pedagogical Institute.

Some of my education was completed in seminars for faculty members. In one of them we studied the new theory of Banach spaces. I was the first speaker with a series of lectures based on the work of Banach, Hausdorff, Schauder. In another seminar by A.A. Markov I studied modern topology. Our department started to flourish with the influx of talented school children from the Leningrad Mathematical Olympiads. After some time, Kantorovich (he began as a Wunderkind; after the war he switched from mathematics to economics) raised several gifted analysts: Vulich, Lozinskii, Govurin, Sirvint.

It was not easy for me to write a candidate's dissertation without a supervisor. The breakthrough came when I had a free week, due to a cold. Theorems about Bernstein and other polynomials (see [3], [4]) became its[3] nucleus. In 1936 I was made a docent of the University and started to write a textbook on Functional Analysis.

In 1937, my father was arrested in Tbilissi and sentenced to eight years based on an obviously false accusation. He died in a concentration camp the next year. This had a profound influence on me. Although I had prepared some 50% of my projected textbook, and although Fichtenholz had arranged for me a tuition-free year at the University, I could not complete it, and until 1942 worked on mathematics only moderately.

I have always admired good lecturers. Both my professors Smirnov and Fichtenholz have been a model for me. I also enjoyed good mathematical books. I read the books by Hausdorff, Hardy-Wright, the simple book by Knopp (Infinite Series), and later the first (1935) book of Zygmund on trigonometric series (see the story about it in [128]). I was the editor of the book on approximation theory by Ya.S. Besicovitch (his brother A.S. Besicovitch became famous in England). I suggested many corrections, but the book (University of Leningrad Press) is virtually unknown today. It was here and later in Tübingen that I learned how to write books. For myself, I read papers on orthogonal series, summability, Hardy spaces. Paper [5], dealing with divergent series, a forerunner of [17], was the result.

The month of June 1941 saw the beginning of the war with Germany. Soon there remained only a narrow corridor connecting Leningrad to Lake Ladoga, the eastern shore of the lake

[3] Editor's Remark: See the item gg_lorentz_dissertation.pdf at the URL http://math.nevai.us/AT/LORENTZ/KANTOROVICH_LORENTZ_MS/ for L. Kantorovich's evaluation of the thesis and a hand-written version of the thesis itself.

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remaining in Soviet hands. The war was to bring the population of Leningrad horrible sufferings. During the severe winter of 1941–42, there was artillery fire into the city, but no air raids. For private use, there was no electric power, no water (the water piping was frozen), no public transportation. By some reports, one million people died of starvation. Since November, I belonged to the paramilitary group of air defense of Leningrad. Almost all my friends were either evacuated to the East or drafted into the army. After several attempts to be evacuated with my wife Tanny (née Belikov), some of them illegal, we succeeded in joining a group of faculty and students of the Herzen Pedagogical Institute. In the beginning of April, 1942, we crossed the still frozen Lake Ladoga in trucks and joined a train. After a month's journey we arrived at the Kislovodsk spa in the Northern Caucasus. Looking back at my 12 years in Leningrad, I can say that I learned quite a lot in this time, but my creative work was meager. The next barrier, of the Russian doctor's degree (which requires several important papers) seemed unsurmountable for me, for both mathematical and political reasons.

Although far away from the front, Kislovodsk happened to lie in the direction of the Summer 1942 advance of the German army. In August 1942 the city was abandoned by the Russian forces without a fight. Tanny and I were registered as ethnic Germans by the occupation authorities. The German advance stalled far away from the oil fields of Baku, one of its targets. Because of the Russian victories at Stalingrad, it was forced to hastily retreat. In the first days of January 1943 we left Kislovodsk with numerous other refugees. In this way we came to the refugee camps of Kalush and Torun in Poland.

In May–August 1942, with the regeneration after starvation, also my mathematics regenerated. This continued later, although I had only Zygmund's book with me. In 1943, from Torun, I was able to send some papers ([6], [8]) to Professor Knopp in Tübingen, who was the editor of the Math. Zeitschrift. He and Professor Süss arranged for us (and our newborn son Rudolph) a transfer to Tübingen in 1944. This university town is located in the southwest corner of Germany. We were lucky since we wanted to be as far as possible from the Soviet influence.

In Tübingen, I became an assistant of Professor Kamke who was writing books on differential equations. I obtained a doctoral degree at Tübingen, and after the French occupation also the "Habilitation" (=right to lecture at the university). The funds of Prof. Kamke dried out; instead, Prof. Knopp and I planned to collaborate on summability. But the French authorities classified me as an undesirable foreigner, probably a Soviet citizen (who, in their opinion, should return to the USSR) and the university was not allowed to offer me a fellowship. In Spring, 1946, leaving my family in Tübingen, I went to the American zone of occupation. In Heidelberg, an American officer supervising the refugees gave me an identification document stating that I was stateless. I lived with this document until my USA naturalization 13 years later. Because of the habilitation at Tübingen, I could teach as a docent, for three semesters at the University of Frankfurt. I often traveled to Tübingen, and in 1948 was appointed an "Honorarprofessor" (=Docent with a permanent salary) there.

Already in 1946, I began to use "Georg Gunter[4] Lorentz (Tübingen)" (soon switching to G.G. Lorentz) for my publications. The name "Gunter" was fictitious. In this way, I hoped to prevent or at least delay the realization in the USSR of the fact that I was identical with G.R. (R = Rudolfovich) Lorentz of the University of Leningrad.

[4] Editor's Remark: The only publication spelling this out is [6], and there it appears as "Gunther" which is probably the reason why, to this day, Mathematical Reviews and MathSciNet give "Georg Gunther Lorentz" as G.G. Lorentz's full name.

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All this time, we lived uncomfortably and in poor conditions, with food scarcity. But mathematically it was a happy time for me. In Tübingen, during the war, I repeated my lectures on Banach space theory for the faculty (Professors Knopp, Kneser, Kamke, Müller) and some students with great success. I wrote some 20 papers: joint papers with Kamke [13] and Knopp [23], papers related to differential equations [9], [13], papers on summability [8], [17], [22], [23], on Fourier series [12], papers where rearrangements play a role, [20], [24]. Some of my results were obtained simultaneously by Hardy and Rogosinski (Fourier series) and Agnew (summability). I eliminated them from my manuscripts, because due to the war and postwar conditions in Germany, they would have appeared only much later. In Tübingen, I had my first two doctoral students: W.B. Jurkat (Fourier series) and K.L. Zeller (Summability). They later became professors at Ulm and Tübingen, respectively. Both dissertations were influenced by my lectures, or used my suggestions, but were written quite independently. I could not participate in the formal doctoral promotions (being in Canada), therefore the University of Tübingen decided to name Prof. Knopp and myself as joint advisors for both dissertations.

Millions of refugees crowded the UNRRA camps after the war in Germany. We probably did not qualify for one of them. They provided ample food, shelter, and a chance to emigrate. However, even at my time at Frankfurt, Tanny and I conceived the idea of emigrating on our own. In particular, the closeness to the Russian zone of occupation seemed not to be good for our future and that of our children. I accepted a fellowship of the Lady Davis Foundation at the University of Toronto. We arrived there in July of 1949. This university had sustained losses of their faculty (R. Brauer went to Harvard) and they were eager to get new blood. They thought that I should begin from zero. I was appointed an instructor. My main service to the Department of Mathematics should be a supervisor of the Ph.D. dissertations. I had four; the first two of them, G.M. Petersen, P.L. Butzer, have become prominent later. I planned also to work with the faculty members as much as I could. I had joint papers with Abraham Robinson [37], M.S. Macphail [31], [35]. From P. Scherk I learned to appreciate some problems of additive number theory, useful for me later [38]. With I. Halperin we have had useful conversations, but have published our results separately (see [26], [30]). At both the election proceedings to the fellowship in the Canadian Royal Society of Halperin and Macphail, my ideas have been mentioned. I regularly helped Prof. H.S.M. Coxeter, the editor of the Canadian Mathematical Journal, by reviewing papers in Analysis. I also finished my first book [B1], Bernstein Polynomials. Its first version had been written in Germany, but it now contained a first exposition of rearrangement invariant spaces. This was an extension of [24], where "Lorentz spaces" were introduced, and of [27].

In 1953, only an assistant professor at Toronto, I changed to a professorship at Wayne State University in Detroit. My enemies say that this was because of money. To this I reply with a joke: No, the paycheck was almost the same. I went to Wayne State because they payed more often: twice a month instead of once a month in Toronto.

It was a very good idea to move to the USA. One had many more opportunities here, mathematical life was much more interesting. Immigrants are treated better than in any other country. I prefer the administration of the universities here to that in Russia or Germany. The worst, most controversial administrative chores are taken over by the department chairmen and deans. One can earn a good salary by good achievements. The evaluation is by colleagues, but also, independently, by grant-giving institutions. One of the attractions of Wayne State University in Detroit was its proximity to Ann Arbor. When I came to Wayne University in 1953, I found it in transition from a city college to a state university. It had some good, interested new staff (Gofman, S. Kaplan, Eisenstadt, H. Cohn). Later I saw that the University had difficulty retaining good staff — perhaps because of its location downtown.

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It was not clear to me, where to put my research effort. Summability seemed to be a bad choice, it was fast losing its popularity. Too many people worked in rearrangement invariant Banach function spaces. Nevertheless, I did not abandon summability. I wrote [37], [39]. In [39] I used my old ideas of [6]. In [37], a bad mistake happened, during the proofs: I changed the formulation of the main Tauberian theorem from the correct capital O to the weaker small o condition. This misguided somebody to prove my theorem again with the correct O-condition. The good papers [42], [43] were written with my former Ph.D. student K.L. Zeller. He was visiting Wayne State in order to learn computing, non-existent at the time in Germany. The first paper has been much generalized and imitated, among others by Petersen in his book; the first one is remarkable because it connects the so-called Riemann summability sets of a method with analytic sets (analytic sets on $\mathbb{R}^1$ are identical with projections of plane Borel sets).

I submitted a paper on rearrangement invariant spaces to the Canadian Math. J., and sent it also to Prof. Zaanen in Holland. He answered that his student, Luxemburg, had results partly intersecting mine. Naively, I suggested a joint publication with Luxemburg. But how can a Ph.D. dissertation be published jointly with somebody outside? The result was that I withdrew my paper in disappointment. Nevertheless, I wrote papers on inequalities [32], [40], and with Eisenstadt, on measures, [45], [48]. Later I came back to the subject of Banach function spaces with papers with Shimogaki, in my books, and also indirectly through dissertations of Riemenschneider and Sharpley.

In number theory, I [38] treated the famous problem of the density of the sum of two sequences. With Erdős, I had [44], about the probability of relatively prime integers x, y on curves (Chebyshev, long time ago, proved the same result for the square $0 \le x \le n, 0 \le y \le n$). During my last year at Wayne State, I decided to change my line of work. After some attempts with applied mathematics, I decided on approximation theory, which I did not know at that time. In 1958, I had offers from the Universities of Toronto and Syracuse.

At Syracuse University (1958–68), I was quite happy. Before my coming, it had a brilliant faculty in mathematics. Then came a catastrophe, with many good people leaving. Chairman Kibbey was blamed, but in reality lack of funds was the trouble. Then, in the Sputnik period, money was again available. So Kibbey hired Edrei, my student Jurkat, the probabilist K.L. Chung, myself, later the algebraists Ryser and Kleinfeld.

I started to write the book [B2]. But first I studied the brilliant ideas of Kolmogorov about widths, entropy, superpositions of functions. I wrote a review of this in [52]. Paper [49] gives weak asymptotics for widths of Sobolev balls in the two metrics L_p, L_q, $p \le q$. (The elementary proofs are not quite superseded even today, by the 1990 publications of Buslaev and Tikhomirov.) The review [61] contains many new results. The main one of them is a precise estimate for the entropy of sets in arbitrary Banach spaces. Estimates of Kolmogorov and Vitushkin are two special cases of it. The book [B2] was completed in two years and is 188 pages long. Even at this time, I do not understand how one can write an excellent book of 500 pages. For example, the first slim edition (1935) of Zygmund's "Trigonometric Series" reads much better than the later two volumes of 1959 (J.P. Kahane calls the two together "The Bible").

With the young Japanese Shimogaki, who died prematurely, we wrote 4 papers on the interpolation of operators. A final parting song were the summability papers [54], [56], [80] with Zeller, with answers to some simple, but basic questions. They should have appeared at the beginning, not at the end of this theory. (It is well known, that in his book on divergent series, Hardy does not answer some basic questions. He does not prove, for example, that the Abel Tauberian theorems by him and Littlewood are the best of their kind.) In approximation, I started a series of papers on constrained approximation, beginning with polynomials with

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positive coefficients: [51], [53], [64]. My student, J.T. Scheick, participated in this with his powerful thesis where, in some cases, he computes the exact constants. Its 5 page publication in J. of Approx. Theory is so compressed that it is non-readable. Among my other Ph.D. students at Syracuse, J.A. Roulier and S.D. Riemenschneider became well known later. At the end of my stay in Syracuse, in the Summer of 1968, I was 58 years old, and had established myself as a specialist in approximation. But this was not the end of my work.

The University of Texas at Austin, where I went to next, was in the midst of a transition. The powerful and controversial dean Silber (later the president of Boston University) had been in the process of unifying the departments of "pure mathematics" with professors R.L. Moore and Wall (both on verge of retirement) and of "applied mathematics", which contained all newcomers. In particular, he wanted to advance approximation theory. With Cheney, Schumaker, myself and Berens, we had a very good group. Later L. Gilman became the chairman, with different views for the new department.

I began with another chapter of approximation with constraints. If a function f is monotone on $[a, b]$ (for instance if $f' \geq 0$), one can constrain the approximating polynomials P_n also to be monotone (with $P_n' \geq 0$). For an arbitrary f one can require P_n to mimic the monotonicity behavior of f on subintervals. This is monotone and comonotone approximation. In [65], [67], [74] with Zeller, we gave the Jackson type theorems for monotone approximation, in [70] the Chebyshev type theorem was proved. Since then, many papers have been written on these problems. For example, R.A. DeVore and my son R.A. Lorentz treated the more difficult cases when one assumes that $P_n^{(k)} \geq 0$, for some $k = 2, 3, \ldots$, for the Jackson and the Chebyshev type theorems, respectively.

Very interesting to me was the joint work with the highly talented H. Berens (now professor at Erlangen). Our first paper [73] was on Bernstein polynomials, then we had a series of papers on Korovkin sets, [78], [81], [86], [90], (see also [113]), where we, among other things, revived the ideas of the Russian Shashkin, who is neglected in his own country.

I became acquainted with Birkhoff interpolation when I had to use it for monotone problems in [70]. In my opinion, this kind of approximation is of considerable importance, but it is underestimated. As long as there is interest in Lagrange or Hermite interpolation, it will persevere. There are two aspects to it: (a) by G.D. Birkhoff (1906), theorems with arbitrary, general sets of interpolation knots; and (b) by Turán, when the knots are very special (zeros of the Chebyshev polynomials, for example). The book [B4], "Birkhoff Interpolation", by three authors, appeared in 1983; it is based on my preliminary manuscript. In the main, it is about problems (a). As my key papers for Birkhoff interpolation, one can consider [84], then [76], with my theorem about singular matrices, [85] and [109], about the zeros of Birkhoff splines, [97] and [103], about the probability of singular matrices, also [104], [105].

In 1977, I received the honorary doctor degree in natural sciences at the celebrations for the 500 years jubilee of the University of Tübingen. The certificate, which is signed by the dean H.H. Schaefer says that it is being granted "for his fundamental investigations in summability, approximation and functional analysis, for his generous support of the young generation of scientists and for his services rendered for international cooperation in mathematics".

Even after my retirement in 1980 and close to it I had some useful research papers: [92], [98], [101] deal with incomplete polynomials P_n, which for some $0 < \alpha < 1$, have a zero of the order at least αn at $x = 0$. A generalization are weighted polynomials with weight $w(x) \geq 0$ and norm $\|P_n\| = \sup(|P_n(x)|/w(x)^n)$. With $w(x) = x^\alpha$ we come back to the incomplete polynomials. An important paper of Saff, Ullman and Varga for the Jacobi weight $w(x) = x^\alpha(1-x)^\beta$ established a connection with the potential theory. This allowed a flowering

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of the theory of weighted polynomials at the hands of Saff, v. Golitschek, Lubinsky, Rakhmanov, Totik, Mhaskar and others. Even the proof of Freud's conjecture for orthogonal polynomials has been obtained with its help. One can also define incompletion for the trigonometric polynomials T_n. Applications of this in [111] and [112] (the second paper with v. Golitschek was written before, but appeared after the second) lead us to theorems of the following type. Convergence $T_n \to f$ for a function f which vanishes on a set of positive measure on $[0, 2\pi]$ implies that the T_n's have a large number of real zeros.

One should not forget the series of papers [114], [118], [122], [126] (especially the last), mostly with R.A. Lorentz. They deal with polynomial interpolation in several variables. It so happens that the methods of one-dimensional Birkhoff interpolation, in particular coalescence of rows, work very well for several dimensions. They are needed even in simple cases. The property that one seeks to establish for a multidimensional interpolation scheme is its "almost regularity", that is, the unique solvability of the problem for interpolation points in general position.

In Austin, I had two Ph.D. students, R. Sharpley (now professor at the University of South Carolina) and L. Jaffe.

I always have wanted to rewrite my first book on approximation, [B2]. After retirement from the University of Texas, I suggested to R.A. DeVore to work together for this purpose. He agreed. For some five years we worked in this way. But then we decided to split the book into two volumes. The first, "Constructive Approximation", jointly with DeVore, appeared in 1993 as vol. 303 of Springer's Grundlehren der Math. Wissenschaften. The second, "Constructive Approximation, Advanced Problems", vol. 304, has besides myself, the two new authors: M.v. Golitschek and Y. Makovoz. The manuscript was submitted to Springer on May 1, 1995.[5]

My tastes in mathematics are conservative, in the sense that I do not rush into a very new, fashionable subject. Instead, I try to understand parts of an existing theory well, and then still find in it something worthwhile. Therefore I am reluctant to give up a theory that seems overworked, or even obsolete. G.H. Hardy once said that there are powerful mathematicians, who can overcome technically difficult obstacles, like Littlewood, and there are fine mathematicians, who have good ideas; he thought that he belonged to the second category. I would classify myself also in this way. This does not mean self-praise; there are many mathematicians better than I am on both accounts. In my books, however, I like to include very recent, striking and important results, which show the way into the future. In [B1] this was the rearrangement invariant spaces; in [B2] the new ideas of Kolmogorov. And many of them, I hope, appear in [B5] and [B6].

Austin, July 3, 1995 G.G. Lorentz

List of publications of G.G. Lorentz

Papers[6]

1. Über lineare Summierungsverfahren, Mat. Sbornik 39 (1932) 44–50.
2. Funktionale und Operationen in den Räumen der Zahlenfolgen, Dokl. Akad. Nauk SSSR (N.S.) 1 (1935) 81–85.

[5] Editor's Remark: it appeared in 1996; see [B6].

[6] Editor's Remark: Except for the correction of the occasional mistake, this is the list of papers as it first appeared in J. Approx. Theory 13 (1975) 10–11, and then appeared, augmented, in the conference volume that also contains [C3] and, augmented again, in [C5], but with further papers appended here.

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[7] Editor's Remark: In JAT 13(1) (1975) 8, this carries the additional information 'Habilitationsschrift, U. Frankfurt'.

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Two letters from G.G. Lorentz to I.P. Natanson from 1938

When preparing this obituary, Paul Nevai heard from V.V. Zhuk that there exist two letters written around 1938 by G.G. Lorentz to Isidor P. Natanson. The letters can now be looked at on the web, at: http://math.nevai.us/AT/LORENTZ/LETTERS. Here they are, in translation.

First letter

January 2, 1938

Dear Isidor!

Happy New Year to you and Elizaveta Petrovna. Celebrations in Leningrad were quite festive. In your Alma-Ata, arrival of the New Year was probably interesting as well, wasn't it?

Almost all our classes are now over, and examinations will start soon. This year, the first-year students at the University are extremely weak. I am the TA in Analysis for one of the groups, and usually 40% of the written works receive failing marks; and 60% of students in the same group failed Analytic Geometry. At the same time, many in my group were straight A students in high school and were accepted to the University without an entrance examination. I expect a catastrophe during the exams (it nearly happened last year with the first year students as well).

Among the seminars in NIIMM [9] the most flourishing one is the seminar on problems related to the theory of functions of real and complex variables. Well, the imaginary part of the seminar is so far dominating the real one. Recently, we finished dissecting long and difficult theorems by Hardy and Littlewood that were well presented by one student.

Translator's notes:

[9] Scientific and Research Institute in Mathematics and Mechanics (Nauchno Issledovatelskii Institut Mathematiki i Mechaniki) at Leningrad State University.

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How is your work on the book? Grigorii Mikhailovich[10] asked me to be its editor, and I am waiting with interest for the first chapters of the manuscript to arrive.

All the best, hope to see you soon.

Yours, Yura.

Second letter

October 13, 1938[11]

Dear Isidor!

We recently received your postcard. Nothing new is really happening in Leningrad, and the seminars have not yet started in NIIMM. Probably, that's because Kulisher[12] is absent (he is no longer the director — just a Professor at the University). He had a surprising talent for pestering anybody into seminar participation in cases like that.

At the end of September, many of us went to Moscow to participate in a session on Functional Analysis. Eight out of eighteen lectures were given by Leningradians. An absolutely astonishing lecture, in the sense of its form, was given by Fichtenholz. Govurin was refusing to give a lecture (Differential Calculus for Operations) up until almost the very end because, in his opinion, it was not interesting. He only agreed three hours before the lecture because of Leonid's insistence. This lecture took place instead of the lecture by Voronovskaya who managed to convince Vinogradov to include her lecture in the session. I was giving a lecture on the paper[13] by Kantorovich and myself on the theory of semi-ordered spaces.

I very much liked two lectures given by Krein. One of them dealt with results similar to theorems of Hilbert–Schmidt and Mercer requiring transition from one type of convergence to another. The second lecture was devoted to various applications (to the theory of Banach equations, to a Fixpunktsatz) of his lemma about the fact that the convex hull of a weakly compact set is also weakly compact. Krein was invited to visit us in the spring.

M. Frechet gave an overview lecture. He was speaking about some not very interesting works by him and his students. Gelfand, even though he was not giving an official lecture, managed to formulate two interesting theorems during discussions.

After arriving in Leningrad, I became interested in photography. The first pictures turned out to be rather satisfactory for a beginner. However, the period with long hours of darkness and cloudy weather is arriving, and so I think I will be forced to postpone photography until next summer.

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[10] Fichtenholz.

[11] An excerpt (see the item sessija_po_funkc_anal_1937.jpg at the URL that also contains the letters) from the record of the Akademia Nauk SSSR describes a session, on 27–29 September 1937, that in all particulars matches the one described in this letter, — except that the letter is dated 1938.

[12] We thank Andrei A. Lodkin who informed us about the following. Kulisher was a member of the group of "marxist mathematicians", which also included Leifert and Drozd, who, starting with 1928, opposed the "right wing" group consisting of people such as Guenter, Smirnov, and Fichtenholz. In 1930, when the first group overpowered the second one, the Leningrad Mathematical Society, with Guenter the president, dismissed itself in order to avoid the worse. Fichtenholz, Delone, and some other mathematicians even preferred to sign some loyalty declarations prepared by the Marxists. This probably helped to save some lives in Leningrad. It wasn't as bad as in Moscow where, for instance, Egorov was later expelled from the Society, arrested and deported to Kazan where he died after a hunger strike.
For more details, see [144] or google "egorov", "luzin case affair", and the names mentioned above. In addition, see also the booklet "Na Leningradskom Matematicheskom Fronte" (On Leningrad's Mathematical Battlefront) published 1931, and the papers "On the so-called Leningrad mathematical front" by N.S. Ermolaeva (MR1736913 on MathSciNet) and "The N.N. Luzin affair" by A.P. Yushkevich (MR1005316 on MathSciNet).

[13] See [129].

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What impression do you get from the students at the University? From the city? Have you received my postcard from Svanietia? The "Director" of the post office (who is also a "director" of a housing co-operative?) promised me to send it the following day.

Please say hello from me to Vera Yakovlevna.[14] Al. Fedorovna sends her regards.

Firmly shaking your hand,

Yours, Yura.

translated by Kirill Kopotun

My memories of George G. Lorentz

By Hubert Berens

I met G.G. Lorentz for the first time in August 1963 at the Oberwolfach Conference "On Approximation Theory" organized by P.L. Butzer and J. Korevaar. Obviously, Lorentz was one of the prominent participants, together with J. Favard, I.J. Schoenberg, and G. Sunouchi. I remember well the good-bye chat between Lorentz and Butzer in which Lorentz announced that he could solve the saturation problem for the Bernstein polynomials. I was then a doctoral student at the RWTH Aachen, Butzer being my advisor, and well acquainted with the whole complex of approximation by positive linear operators on the classical function spaces, with Korovkin's theorem, and in particular with the Bernstein polynomials. We were quite sceptical but, sure enough, Lorentz published his solution in the proceedings of the conference.

Lorentz was always a distinguished participant of the follow-up conferences at Oberwolfach organized by Butzer and B. Sz.-Nagy up into the 1980s. To me the conference in August 1968 was special. I had accepted an assistant professorship at UCSB and was trying to get ready to cross the Atlantic Ocean with my family. Lorentz told us at the conference that he had accepted a position at The University of Texas at Austin. He did indeed surprise us. Why Texas, and that at an age close to sixty? Lorentz knew why, and I, and my family, soon learned that Texas, the friendship state and, in particular, Austin had a lot to offer.

Lorentz[15] invited me to spend a year at UT as a visiting associate professor which I did for the academic year 1970/71. There were E.W. Cheney and L.L. Schumaker and, centered around David Young, a strong group of numerical analysts. Lorentz and I worked on approximation by Bernstein polynomials, characterizing in particular the approximation of Lipschitz spaces, the non-saturation case. The problem arose from a discussion Lorentz and I had with R. DeVore at a conference at the University of Alberta at Edmonton in the summer of 1969. By the way, DeVore solved the problem, too; he published it in his Springer Lecture Notes.

It was not easy to work with Lorentz, but it was always challenging. I learned a lot at our meetings[16]: how to look at a problem and how to attack it. I wouldn't want to miss any of them. Naturally, we didn't just meet professionally. My family and I were invited for dinner, more than just once, and on Sundays we had joint hikes, visiting the LBJ ranch or the Pedernales

[14] I.P. Natanson's mother.

[15] I never was able to address him by his first name as it is common in the US. Naturally, he called me by my first name, but sometimes he wrote in a note "Dear B . . ." or even "Dear Professor B . . .", depending on our present relationship.

[16] For most of the time, they took place at his home, and we worked at the kitchen table.

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Falls or just had a walk in Zilker Park or around Lake Austin. Mrs. Lorentz was a warm-hearted and loving person, and an excellent host. We especially enjoyed her Russian cooking, and our children her special treats. It was a wonderful year for my family and me.

We moved back to California but, after one further year at Santa Barbara, we were back in Austin for just one additional year before finally moving back home to Germany. It was an active year again. Lorentz and I worked on Korovkin Theory; we studied in particular the papers of Yu.A. Šaškin and M.A. Krasnosel'skiĭ – I think we made a few nice contributions. In addition, our group at UT organized the First Texas Conference on Approximation which took place in January 1973. It was quite a success, with many conferences to follow.

Back in Germany, we continued and, in a way, completed our studies on Korovkin Theory.

In a way, Germany had become Lorentz's second home. He started his life in the Western World close to the end of World War II in Tübingen where he found a place to stay and work at the university. In Tübingen, his four daughters were born. But in 1949 he and his family left for Canada, post-war Germany wasn't safe enough.

His time at Tübingen was somewhat special to him. He liked to talk about those years, about the support he and his family received from his colleagues, about the classes he taught, about his students – at least three of them are now emeriti at different German universities – and naturally about his work. In 1977, the University of Tübingen honored him with a doctoral degree for his fundamental investigations in summability, approximation, and functional analysis.

In 1974 he spent a year as a Senior Humboldt Fellow at the University of Stuttgart, W. Meyer-König was his host. He also visited Erlangen in the 70s as well as in the 80s. But if you wanted to meet him in the last two decades in Germany – aside from the visits to his son in St. Augustin/Bonn – you would most likely find him at the University of Würzburg where he worked with M.v. Golitschek on the second of the two encyclopedic volumes on approximation which appeared in the 1990s in the Springer Yellow Series under the title "Constructive Approximation". His second honorary doctoral degree is from the University of Würzburg.

I claimed above that Germany was, in a way, his second home. True or not, Lorentz truly suffered from the fact that for so many years he couldn't see his birthplace and the places where he grew up and studied, in Georgia and Russia. Finally, in the fall of 1998, he was able to show these places to his son.

A letter to George G. Lorentz

By Kurt Jetter

Dear George,

As one of your many pupils ("Schüler"), let me express some very personal thoughts concerning moments which we have shared during these last four decades. Meeting you was – at least for me – one of those lucky and unexpected challenges of life. As a student scholar, when studying your seminal book 'Approximation of Functions' (it must have been in 1968, in a graduate class taught by Karl Zeller, at the University of Tübingen), you seemed so beyond my reach – up in the firmament – and I could only much later appreciate the inspiring character of this wonderful textbook as an advanced course in Approximation Theory and for its influence on my mathematical education.

For a young researcher, getting to know you in person (for the first time in Karl Zeller's seminar, in 1974, which you had attended as a guest, at that time a Senior Humboldt Fellow at the University of Stuttgart), was one of these rare events in my education. Sharing your knowledge

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and wisdom in our starting scientific discussions, you have become one of my major scientific advisers. And later, yes, I dare say, you became a true senior friend.

Our scientific cooperation was short, mainly from 1975 through 1983, but our personal and scientific exchange continued. It had turned out that we had been working on similar projects, starting with zero count theorems for spline functions (those 'cockroaches' as you liked to call them), I as a young researcher growing up in the 'spline era', you as the senior scientist promoting and advancing Birkhoff's and others' ideas of lacunary interpolation. In your inimitable way, you encouraged me in my undertakings and incorporated me into your project of writing a book on the subject. You taught me your beautiful style of writing which I have tried to imitate since, with less success I must say. And it was not always easy to carry the burden of your – though friendly – criticism. You taught me authority, through your scientific competence and your profound education and general background. You taught me your way of mentorship, which is so indispensable to scientific education.

Your generosity has opened for me the door of international exchange, by directing me and by recommending me to people whom I have joined in fruitful collaborations. Already in 1976, during the first Texas conference in Approximation Theory which I have attended, you showed me the way to Approximation Theory which I attended, you showed me the way to TAMU college (as it was called then) by telling me: "Herr Jetter, take the greyhound via Brenham to College Station, I have arranged a seminar talk for you". There, I got to know my friend Charles Chui and the fine group of people around him. And during the preparation of the several drafts of the 'Birkhoff Interpolation' book, you introduced me to a former student of yours, my friend Sherman Riemenschneider. Both contacts have lasted up to this time, and I remember many good and satisfying visits to Texas A&M, and to the University of Alberta.

We have met in many conferences, at the Oberwolfach institute and in your Texas conference series of course, but also in other areas around the world. Your vitality seemed to be unbroken, up to an age where people are usually declining and slowing down. I remember meeting you and your son Rudy, during one of the Chamonix conferences, while hiking on the plateau of the Brévent. You were standing there, perhaps a little out of breath, when our group passed by, and I dared to ask whether you needed some help. And you answered: "Whom are you asking? My son Rudy or me?" That was your type of humor, which we enjoyed in so many situations.

Your travel schedule was prodigious. And only in later times did Grindelwald become one of your favorite spots in the world where you liked to recover a little, from time to time. During your many visits to Europe and Germany, you frequently took the time to stop and visit me and my family, at Hagen and in our house in Essen-Kettwig. As a visiting professor to the University of Duisburg, I could return to you at least a small part of the hospitality and generosity which I had experienced in these many years.

Our last mutual visits date back to 2001. After the SPIE conference in San Diego, while visiting Charles Chui at Stanford, my wife and I took the time to make a trip up to Chico, California, in order to visit you and the place where you had settled after retirement. It was just wonderful to see all the books lying around in your apartment. Do you remember how your wife Tanny always complained about your books occupying all the tables of your house in Austin?

And then your return visit, this time to the University of Hohenheim, the historic place of the 'bombastus of Hohenheim' (actually, Paracelsus never showed up here). Of course, we had a good meal and one or two glasses of German white wine — which you have preferred to be produced in the more traditional way, building on a perfect balance of fruitiness, acidity and sweetness. This was also your last visit to Tübingen, and to the Mathematical Institute there, the place where you had come 60 years ago during the war, where you had stayed till 1949 before

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leaving for Canada, the place giving honor to you and your scientific achievements by providing your first degree of doctor honoris causa. In some way, and we both were aware of it, this was your farewell to Tübingen.

Thank you, George! We will miss you, as a scientist and as a person. We will miss your warmth, your humanity, and your advice.

The George G. Lorentz I knew

By Paul Nevai

If I recall correctly, I first heard of George when his book "Approximation of Functions" reached Leningrad where I was an undergraduate student diligently working on some mumbo-jumbo which was a prerequisite so that Garald I. Natanson (aka Natansyn since he was the son of Isidor P. Natanson, the "Constructive Function Theory" guy) accept me as his M.S. student. My friend Ljova Kourliandtchik and I were sweating through A. F. Timan's "Theory of Approximation of Functions of a Real Variable" and digesting words such as Lebesgue constant, Legendre polynomial, and modulus of smoothness when, around 1969, during one of the weekly meetings of the seminar, Natanson started to talk very quietly, looked around cautiously and, once he had made sure that the walls weren't listening (they were), mentioned that he wanted to review a book by a former Soviet mathematician who, although a traitor and an enemy of the people, is still a great mathematician, and whose book is a welcome addition to the rather small but powerful literature on approximation theory. OK, I admit, Natanson didn't really say this verbatim, I just read this between the lines which was the normal operating procedure those days. The entire situation was bizarre and surreal, it sounded like a clandestine operation. In retrospect, it *was* one. For me, the bottom line was that using George's book I managed to understand what moduli of smoothness were about which led to my Master's thesis where I used moduli of smoothness to obtain certain best constants in estimates of trigonometric Fourier sums.

Years later, George told me that Natanson's father, I. P. Natanson, had been one of his best friends during his Leningrad years,[17] and he remembered Garald as a small boy with whom he frequently played while visiting the Natansons.

My second, still long distance, encounter with George happened in September of 1975. I was sitting in Paris waiting for my US immigration application to be approved, when George invited me to attend the 1976 Texas Conference on Approximation Theory. As it turned out, because of the pending process, my visa application for the trip to Austin was denied, despite George's (and Dick Askey's) efforts to mobilize NSF and some other scientific organizations.

George and I finally met in October, 1976, when I packed my wife and kids into our VW Rabbit, and drove down to Austin from Madison, Wisconsin. We had barely arrived when George came to our motel room in the early morning, unannounced, got us out of bed, and thus we met.

It was love at first sight based on our shared background in Leningrad and our mutual dislike of the Soviet system although for somewhat different reasons.

When George and I met, he was already a grandfatherly figure, and during our friendship I (almost) never experienced the tough guy as he was described by some of those who knew him in his younger years.

[17] George didn't mention this in his autobiography but the proof is in this obituary; see also http://math.nevai.us/LORENTZ/LETTERS/.

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What, tough guy? Can't be. Well, here is one story as told by Ron DeVore. It happened around 1987.

> When we were writing the book, there were several things we did not agree on. No need to spell them out. But George had a list of them and said to me, "Ron, here is a list of the 10 things we do not agree on. You are a reasonable man, why don't you find five of these which you could accept my way". I searched through his list and chose five. He then said, "Now we have five things we don't agree on".

Another story happened between George and me in May, 1983. He found a way of proving Bernstein's inequality using some soft arguments based on the Hardy–Littlewood–Pólya order relation. He told me that his method unfortunately worked only for trigonometric polynomials without a constant term and, thus, for the general case, an additional factor of 2 is needed. As I studied his proof, I realized that his proof, with a slight modification, worked for the general case as well. After I wrote him a letter about it, he responded in an unusually cold tone saying that "I actually hoped you can help me with this". Then I sent him the details of my argument which were just restatements of his original proof. His response came soon and he apologized profusely. As I recall, this was the one and only time when we had some unpleasant moments.

By the way, George wrote up his results in "A new form of Bernstein's inequality and applications", but (i) I don't think he ever published this manuscript, and (ii) there is not a word about the above events. What I have is a preliminary version. Later, in 1989, he and Manfred v. Golitschek published the paper [123] on Bernstein's inequality, but that one did not use the concept of the Hardy–Littlewood–Pólya order relation.

George and I have exchanged hundreds of letters, I still have practically all of them. Initially, it was purely about mathematics but as George became older the topics started to spread out to politics, the Soviet Union, Israel, Germany, nazism, chess, and so forth. He used to call me too, both at home and at my office. However, George never called me at inappropriate times and he always made sure that I was available for chatting and not engaged in dinner or even sleeping. Eventually, my letters turned into faxes and e-mails although he never made the switch himself.

One of the nicest moments I had with George was on May 15, 1981, when I had arranged that he be invited to give a Colloquium Talk at Ohio State, and I brought George and Boris Mityagin together for the first time. Two ex-Soviets of two different generations each of which knew well the other's work and they had a gazillion mathematical connections but it was this day when the two met face-to-face. It was one of those "Kodak-moments", so to speak, but I forgot to bring my camera that day.

Over the years, I took many pictures of George, including one with his son, Rudy, in January, 1989 shown in Fig. 1.

In April, 1988, both George and Dick Askey were visiting South Carolina, and I managed to make them pose for the picture shown in Fig. 2.

George was German on his father's side and Russian on his mother's. His mother's maiden name was Chegodaev, and, according to his daughter, Olga, he used to say he was a descendant of Genghis Khan through Princess Chegodaev.

Although George's mother tongue was Russian, I never heard him speak Russian until after the break-up of the Soviet Union. After that, he spoke Russian to me almost all the time, and he spent a significant amount of time analyzing the Soviet system from the point of view of

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Fig. 1. George Lorentz and his son, Rudy, in 1989.

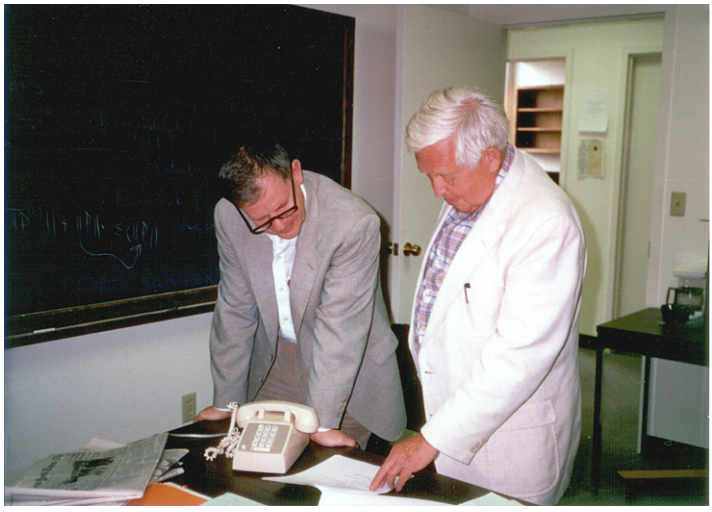


Fig. 2. Dick Askey and George Lorentz in 1989.

a mathematician. The result of his studies was the paper [143] "Mathematics and politics in the Soviet Union from 1928 to 1953". It makes great reading and I strongly recommend it, see JAT02-0001_final.pdf in http://math.nevai.us/AT/LORENTZ/, and, once there, take a look at the other stuff too.

Since George was ethnic German, and since he managed to survive WWII under extraordinary circumstances, he was never really embraced by the post-WWII Soviet mathematics establishment. He was suspected of being a nazi and even perhaps an anti-Semite. Of course, nothing could be farther from the truth. Those who thought of him this way, didn't know him in person. As a matter of fact, outside approximation theory, most mathematicians thought that George, the Lorentz space guy, had been dead for ages. The George I knew was one of the most righteous Gentiles I have ever met. I say this not because he is the only non-Jewish mathematician

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who every year, up until 2004, sent me a Hanukkah card (which is ironic since I am an equal opportunity rejector of all religions) but because of our numerous conversations about both nazis and Jews, Germany, and Israel. In particular, a couple of years ago, George talked to me about Süß, who was perceived to be anti-Semitic and a nazi, but, according to George, he was neither "pro-nazi" nor "anti-nazi" but was "pro-mathematics" and most likely not an anti-Semite. As it turns out, his serious lobbying efforts notwithstanding, George was probably wrong.[18]

The saddest moment I remember about George was when I called him once in the early nineties and his wife Tanny answered the phone and started to talk to me incoherently. It took me a long time to realize that she was in an advanced state of Alzheimer's (she died on December 16, 1999).

On the other hand, George managed to live out his life without pain and suffering. The last letter I received from him was written on March 28, 2004, in which he wrote that "I am relatively healthy, make almost each day a round about our lake, 40 min, <2 miles".

Of course, George was not fully immune to signs of old age as demonstrated by the following story as told by Jóska Szabados.

> In the Spring of 1993, we organized a workshop honoring the 80th birthday of Paul Erdős. George Lorentz was among the distinguished guests, and since he had never before visited Hungary, we had great expectations. Nevertheless, the day before the workshop, when we met the arriving guests at the airport, he did not show up and did not send any message explaining his absence. The workshop lasted three days, without Lorentz.
>
> To our greatest surprise, when we went to the Mathematical Institute the day after the conference concluded, there was Lorentz, sitting in the director's office, talking mathematics with Erdős. As it turned out, he had missed his flight, and had arrived one day later. The problem was that he left at home all papers containing information about the workshop, and could not contact us. He managed to get the number of the Institute, but this was a long Labor Day weekend, and nobody answered the phone. So he checked in a hotel, and did sightseeing in Budapest for two days, on his own.
>
> The second surprise came when he opened a small suitcase, full with presents to Erdős and the organizers, András Kroó, Péter Vértesi and myself: books, T-shirts, souvenirs of all kind, and all bearing some sign of Texas. Invited guests sometimes bring small presents, but here the quantity and quality was overwhelming.
>
> The third surprise came after he left. Opening one of the books, we found ten hundred-dollar bills, neatly stacked. Of course, even knowing that Texas is a rich state, we were not so naive as to think that this was part of the presents. What could have happened was that he hid the money in the book years before (not trusting banks?) and completely forgot about it. We returned the money, of course.
>
> This was George Lorentz's first and last visit to Hungary.

Even in his old age, George kept impressing younger people. For instance, Detlef Mache told me the following.

> I was much taken by Lorentz's *Shanks Lecture* which he gave at Nashville in May of 2000, on "How to do mathematics and be fair to oneself (and others)" in which he discussed

[18] Googling "suss nazi math" yields a lot of information.

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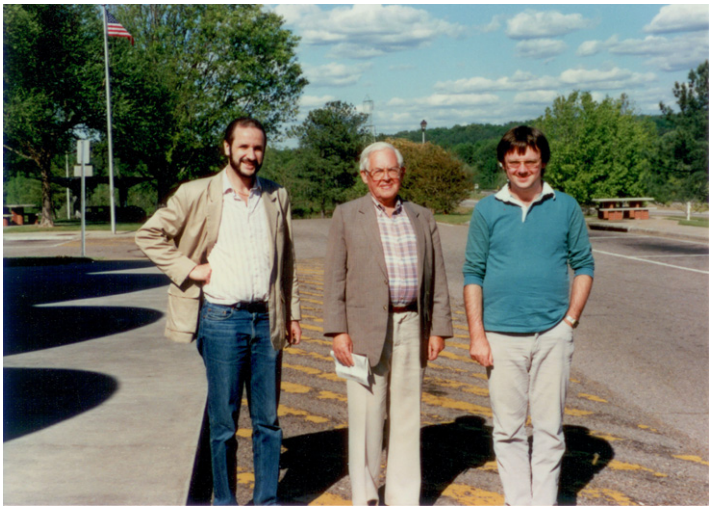


Fig. 3. George flanked by Paul Nevai and Tamas Erdélyi.

the ethical rules that should guide our relationships with friends in mathematics and our students. From this talk and the example of his lifework in mathematics, I learned the basic rule — be fair to your students and colleagues.

Let me finish by repeating George's favorite story lifted from his autobiography[19] which he told me more than a few times and, I am sure, many of the readers are also familiar with.

In 1953, only an assistant professor at Toronto, I changed to a professorship at Wayne State University in Detroit. My enemies say that this was because of money. To this I reply with a joke: No, the paycheck was almost the same. I went to Wayne State because they payed more often: twice a month instead of once a month in Toronto.

If you think this is a bad joke, then the following should convince you that George did indeed have a great sense of humor.

He told Ron DeVore that the boyfriends of his daughter used to praise him for giving his daughters good taste in classical music. However, George did not understand this praise because he did not know any other kind of music.

George celebrated his 95th birthday in 2005 by visiting Hawaii with his son Rudy and then by attending a family reunion in Las Vegas. The last postcard I received from him was dated "8/3/05" (that's March 8). It was about Hawaii, flowers, and bananas. He wrote...

Understood here what Lenin wanted to say about the crisis in physics, and what is his relation to the next 'vulgar Marxists'.

He signed it: "Love, George". I remember I felt horrible (OK, I admit, I cried) when I was reading it since I was preparing myself for the inevitable which came from Rudy on January 3, 2006, announcing the death of our beloved George on New Year's day.

Good-bye, George. I love you. Tvoy drug, Paul.

[19] Just in case the reader missed it there.

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My thanks to George G. Lorentz

By Sherman Riemenschneider

My first encounters with George were as a student in his real variables classes at Syracuse University. I was very fortunate to have drawn the years when George was teaching the three course sequence. Not only was he an excellent teacher, with enough idiosyncrasies to keep us entertained and interested, he brought some first hand glimpses into mathematicians that have somehow stuck with me. For example, when presenting the theorems of Egorov and Luzin, he mentioned a few things about them and their relationship, most of which I have forgotten, but one which I have not. Namely, that Egorov was privileged to have a student, Luzin, who was better than himself. Though George had some very good students over the years, I don't believe he ever felt privileged in that way and I think he would have liked to have been. The second half of the sixties was also the time when Lorentz Spaces and interpolation of operators were becoming important in many aspects of analysis and George introduced us to them. Of course, those concepts had a major impact on analysis and approximation theory in the next decades and that introduction was invaluable to me. George was very kind to students. He would sometimes give little presents (prizes?), and I remember in particular a calendar with the wood carvings of Tilman Riemenschneider that he gave me, my first knowledge of my famous namesake. That year he also invited the class to a party at his home, I vividly remember a mushroom dish that Tanny made and something called Akvavit.

Just as it was determined that George would be my thesis supervisor, he decided to move to the University of Texas (although we students did not realize that it would be a permanent move). The year in Texas was a pleasant adventure for my wife Rama and me, and Jim Case, his other student at the time. George made a special effort to ensure that we were included in seminars and activities of the approximation group in Austin. He also took us to places including the Expo and the River Walk in San Antonio. Not particularly liking Texas (too hot), I returned the following summer to Syracuse to complete the requirements for the degree. It definitely was an advantage to have someone of George's stature as a supervisor when it came to finding a job.

I did not have a lot of contact with George until I took a sabbatical in Austin in 1976. It was at that point that he talked me into helping him with the book on Birkhoff Interpolation, something I knew absolutely nothing about. For the next several years, I had the pleasure of visiting him, often staying in his home. He was a demanding task master, but the time was enjoyable. In 1981, I had a sabbatical at the University of South Carolina, and George visited there as well. Ron DeVore will often say that he invited us to Columbia so we could talk to each other. That was only partially true. At that time, we were trying to understand Tim Goodman's theorem on the regularity of Birkhoff spline interpolation. That may be when George first made the remarks about splines being like cockroaches, basically to the effect that they were undesirable, but were everywhere and showed up unexpectedly. The book was finished shortly thereafter, and though we shared some common interests still, George began the book writing projects with Ron DeVore, Manfred von Golitschek, and Yuly Makovoz, and I took a path with the cockroaches named box splines.

George gave me life as a mathematician. I could never have had the career I've enjoyed without his help, his support, his guidance, and his indulgence in allowing me to join him in the

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book. Through the years, Rama and I have enjoyed his company and generous nature whenever our paths crossed. We will always remember him.



Fig. 4. G.G. Lorentz on the eve of his 80th birthday.

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