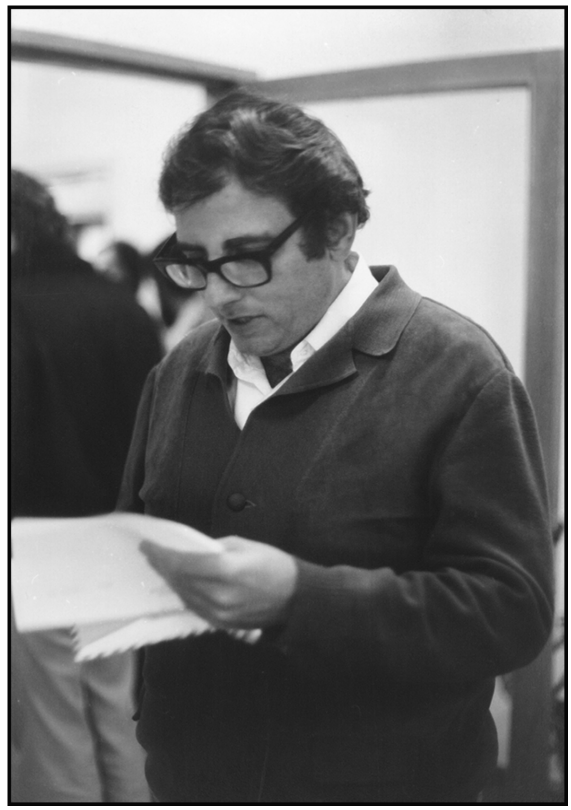
Joram Lindenstrauss, in Memoriam

Compiled by William B. Johnson and Gideon Schechtman

Joram Lindenstrauss was born on October 28, 1936, in Tel Aviv. He was the only child of parents who were both lawyers. Joram began his studies in mathematics at The Hebrew University of Jerusalem in 1954 and completed his PhD in 1962 under A. Dvoretzky and B. Grunbaum. After postdocs at Yale University and the University of Washington, he returned to the Hebrew University, where he remained until retiring in 2005. He passed away on April 29, 2012.

Joram met his wife, Naomi, during his studies at the Hebrew University. Naomi holds a PhD degree in computer science from Texas A&M University. They have four children, all of whom have PhDs: Ayelet and Elon are mathematicians at Indiana University and The Hebrew University of Jerusalem, respectively; Kinneret Keren is a biophysicist at The Technion, and Gallia is a researcher at the Institute for National Security Studies at Tel Aviv University.

Joram's PhD dealt with extensions of linear operators between Banach spaces, leading also to the study of preduals of L_1 spaces. Some of his other groundbreaking research results include a study with A. Pełczyński of Grothendieck's work in Banach space theory and applications thereof, which also led to the introduction of $\mathcal{L}_p$ spaces and their study, a topic which he continued to pursue with H. P. Rosenthal. Lindenstrauss and Pełczyński promoted in their paper the "local theory of Banach spaces," which involves the study of numerical parameters associated with finite-dimensional subspaces of a Banach space and the asymptotics of the parameters as the dimensions of the subspaces tend to infinity. With L. Tzafriri he solved the "complemented subspace problem," showing that, isomorphically, the only Banach spaces all of whose subspaces are complemented are Hilbert spaces. The proof uses Dvoretzky's theorem on Euclidean sections of convex bodies, a topic Joram returned to in an influential paper with T. Figiel and V. Milman and a related one with J. Bourgain and Milman.



Archives of the Mathematisches Forschungsinstitut Oberwolfach.

Joram, 1975.

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Personal and family photos of J. Lindenstrauss provided courtesy of the Lindenstrauss Family Album. Used with permission.

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Early in his career, in a paper on nonlinear projections in Banach spaces, Joram introduced the study of nonlinear Lipschitz and uniform equivalences between Banach spaces. Surprisingly, it turns out that often nonlinear bi-Lipschitz or even biuniformly continuous nonlinear maps preserve the linear structure to some degree. This topic appears on and off during his career, and his last publication, a research book with D. Preiss and J. Tišer, deals with Lipschitz functions on Banach spaces. The two of us were also attracted to this topic and had the good fortune to cooperate with Joram on it (sometimes together with others, notably Preiss).

In a paper of Johnson and Lindenstrauss there is a relatively simple lemma which is widely used, mostly in connection with theoretical computer science, and which is by far the most quoted result of Joram. It states that n points in Euclidean space can be mapped into an approximately $\log n$ dimensional Euclidean space while approximately preserving the pairwise distances. Curiously, but not coincidentally, the paper does not appear in the list of selected publications that Joram prepared in his last year.

Joram Lindenstrauss wrote several very influential books. His two-volume book with Tzafriri, *Classical Banach Spaces*, as well as his book with Y. Benyamini, *Geometric Nonlinear Functional Analysis*, is a must read for anybody interested in the linear (respectively, nonlinear) theory of Banach spaces. With Johnson, Joram edited the two-volume *Handbook of the Geometry of Banach Spaces*.

Joram had twelve official PhD students. All but two of them hold/held respectable academic positions, most in Israel.

Joram's many honors include the Israel Prize for Mathematics and the Banach Medal. He was a member of the Israel Academy of Sciences and was a Foreign Member of the Austrian Academy of Sciences.

Yoav Benyamini

In 1965 Joram Lindenstrauss joined the Hebrew University as a new faculty, and I was a third-year undergraduate student sitting in his Banach Spaces course. This first encounter set the course of my life.

The Banach space group in Jerusalem began with Dvoretzky, who was joined by his former student Grünbaum and their students. A phase transition occurred with the arrival of Joram, who joined Dvoretzky in the supervision of the doctoral theses of Lazar and Zippin, and Joram's influence dominated their work. Soon Gordon started to work with him on his PhD; I started to work with him on a master's thesis in 1966. Joram gave excellent basic and advanced courses, and within a few years the list of his students grew very fast, with Aharoni, Arazy, Schechtman, and Sternfeld. New faculty Perles, Tzafriri, and Zippin joined the department. We had a very active weekly seminar, many visitors, and intensive informal discussions. Soon Joram and Tzafriri wrote their Springer Lecture Notes and then the two-volume "*Classical Banach Spaces*". By reading proofs of the lecture notes and books, students knew in real time what was happening throughout Banach space theory.

Joram's supervision style was very "open." We did not have orderly weekly meetings, and he never gave me a problem for the thesis. The exposure to the different directions and problems came through his comments and criticism in the seminar and other discussions. This is also how I learned how to judge what is "important," "interesting," what is worthwhile to read, and what is publishable. His approach and views were so dominant that he was "heard" even when he was not present.

Joram was very systematic and methodical. His answers to questions sounded like he had prepared a lecture on the subject. He did not like to discuss speculations: he would send me to write up my ideas and, of course, in most cases the speculation led nowhere. But when I did hand him something in writing, he was a wonderful reader. It would come back the next day with detailed feedback. He was also a wonderful writer, and his papers, books, and lecture notes (in Hebrew) are written in his typical systematic, clear, and concise style.

It was a pleasure to work on our book *Geometric Nonlinear Functional Analysis* and to benefit from his excellent judgment, his clear view of the big picture, together with his care for details, and his clear and careful writing. Joram was quite disappointed when I was not very enthusiastic about writing the planned second volume. The two main subjects were supposed to be the theory of finite metric spaces and differentiabililty of Lipschitz functions on Banach spaces. I thought that they were not ripe yet for a book. Joram did the right thing and cooperated with two experts, Preiss and Tišer, to write their major research monograph *Differentiability of Lipschitz Functions and Porous Sets in Banach Spaces*, which will be the basis for any future study of the difficult and important topic of Fréchet differentiability. Our last conversation was when I called to congratulate him on Elon's Fields Medal, and he told me proudly that the manuscript of the book was just sent to the publisher.

Most of Joram's students remained in academia, mostly in Israel. Israeli mathematics was very

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important to him. He stopped taking new students because Israel is a small country that could not absorb more Banach space experts. As editor of *Israel Journal of Mathematics* his commitment to high standards was also influenced, as he told me several times, by the high quality we have to preserve of anything that carries the brand name of Israel. He was an active representative, on behalf of the Israeli Academy of Sciences, in the IMU and talked seriously about applying for the organization of the International Congress in Jerusalem before the murder of Prime Minister Rabin and the increased terrorist attacks put an end to this dream.

Joram set my professional career, and I tried to follow the values he installed in me. I was also very lucky to be a student of his devoted wife, Naomi. She was an exemplary TA in the real analysis course that I took in the same year, 1965—and over the years she also taught my nephew and two of my children!

Joram and Aleksander Pełczyński, 1973.

Jean Bourgain

My early memories of Joram go back to the late seventies and very early eighties with functional analysis meetings in Crete and Ohio and a workshop at the Hebrew University. He was of course an authority in the field, while I was a beginning researcher at the Belgian Science Foundation. From our first encounters, I felt very much at ease and happy to chat with him whenever I got a chance. He was a good person to talk to. Such things are difficult to rationalize, but it was probably the combination of his encouragement, an unmistakable sharpness of mind, also in mathematics outside his direct expertise or interests, and, above all, the perception of a human warmth that evolved into true friendship and affection in later years.

As a matter of fact, even at that time, Joram as a mathematician was no stranger to me. Starting from the mid-seventies, my advisor, F. Delbaen at the Free University of Brussels, had introduced me to Joram's many contributions to Banach space theory and some of the problems left open by his work: they were the background and motivation of my early research. These were questions related to extreme points, a subject that has been consistently close to Joram's heart. Also the global implications of certain local structural properties as studied in Joram's own thesis about extension of compact operators. So I was most excited to meet the man in person and get some feedback. Joram expressed some praise, although not quite as much as I had hoped for. He was indeed in the middle of preparing the second volume of *Classical Banach Spaces*, together with his long-time collaborator Lior Tzafriri, which was mostly devoted to other aspects of the theory. This reminds me of a little anecdote at the Heraklion meeting in 1978, when Joram made it clear that "stable Banach spaces," a concept then freshly introduced (and conceptualizing some striking results of D. Aldous), was the most important thing of the moment and should definitely be part of the book in writing. He showed great unhappiness that neither J-L. Krivine nor B. Maurey, who developed that concept, were present at this gathering.

Joram was focused and liked to pursue matters in depth. In the very early eighties, he came to visit in Belgium for a few days. We went sightseeing in the medieval town of Brugge, where we spent the afternoon viewing the paintings of the great Flemish and Dutch masters. His taste in this matter was different from mine, though, and I tried in vein to convince him that the scenes pictured by Hieronymus Bosch are fascinating. Towards the evening he told me he had a favor to ask. The favor was simply to check P. Enflo's recent solution of the invariant subspace problem. With that I guided him back to the train station, with only a vague promise.

Over the years, especially in the eighties and early nineties during my almost yearly trips to Jerusalem, Joram was invariably a marvelous host, and I truly enjoyed these visits. Our interaction and collaboration centered around questions in high-dimensional convexity, a topic that had been revolutionized by methods from the so-called Local Theory of Banach Spaces and in particular Joram's joint work with T. Figiel and V. Milman on large-dimensional Hilbertian sections, which is one of the most seminal contributions in the field. We worked on problems of low-dimensional embeddings (an issue that became increasingly important in theoretical computer science), fast

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symmetrization, approximations, and also on very classical themes such as the regularity of the Gauss map and optimal distribution of points on spheres. Coming back to low-dimensional embeddings, perhaps one of his most influential results (jointly with W. Johnson) is the principle of dimensional reduction in Hilbert space, which is central in modern data processing. Then, by the mid-nineties we had drifted somewhat apart as we got to work on different things. Joram had gotten back to the love of his youth, which is infinite-dimensional theory, and started a fruitful collaboration with D. Preiss and J. Tišer on Fréchet differentiability of Lipschitz maps.

Through a significant part of his professional career, especially in the later years, Joram had to struggle with severe health problems. His answer to them was a motivation and determination in his work that is exemplary to all of us.

These are some personal reminiscences and comments, but many of them are surely shared by my colleagues. Of course, we all miss him.

Nassif Ghoussoub

I knew that Joram had been seriously ill for some time, but the cryptic email announcing his passing brought more than its share of extreme sadness. Both my professional and my personal lives have been deeply touched by Joram and his family. I worked with him on several projects, and hearing that he, as well, was gone only a few short months after another friend and coauthor, William J. Davis, passed away feels like a bad dream.

But Joram was much more than a coauthor to me. I was a twenty-two-year-old "kid" when I first met him. It was in Columbus, Ohio. He was already a leader in functional analysis, my field of research at that time. It is fair to say that he was a feared leader, with extremely high scientific standards. He was tough and never minced his words, but I never felt intimidated by him, though I was well aware that many other, often smarter, mathematicians around me were. He was quite demanding of his students: he wanted them to excel, and they did.

I often wondered whether others understood this man the way that I did—this man who seemed so tough on the outside was so gentle, even soft, on the inside. A sabra! Confirmation came several years later when I got to know his children. He was as demanding as an old-fashioned patriarch could be, but they knew …

I was "fresh off the boat" on the American continent for a postdoctoral position. Joram Lindenstrauss was already a pillar of Israeli mathematics. He was the first Israeli I ever met. The encounter was one that would mark my life. His parents had left Germany for Jerusalem as soon as the Nazis came to power. He was born in the holiest of places, lived there all his life, and endeavored to make its Hebrew University one of the best, and not only in Israel. We quarreled about politics of course, yet there was always this feeling, which may seem naive nowadays, that all would end up well one day. It was there and then that I first learned—and, yes, relatively late in life—that "what we have in common is much greater and more powerful than what divides us."

All this was before I met his incredibly kind wife, Naomi, and his amazing, then-teenaged children. Watching the Lindenstrauss family together amounted to seeing humanity at its glorious best. That's how I wanted my own family to be. I've wondered lately how incredulous he, a recipient of the Israel Prize with a Fields Medalist for a son, would have been upon watching *Footnote* the movie. His children lived up to every expectation. He must have been so happy as he passed.

Joram's mathematical contributions are numerous and varied. His defining role, with Alexander Pełczyński, in uncovering the true impact and depth of Grothendick's "Résumé" is well documented. The Figiel-Lindenstrauss-Milman paper on "the dimension of almost spherical sections of convex bodies" is a classic. The Johnson-Lindenstrauss Lemma about nearly isometric embeddings of finite point sets in lower-dimensional spaces is one for the ages. The depth of his latest work on geometric nonlinear functional analysis with David Preiss and others defied the trends and defined a new age for the field. Joram was a mathematical trendsetter because he never cared whether his mathematics followed the trodden path.

Joram—colleague, mentor, friend. My life has been deeply enriched by his presence.

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William B. Johnson

I met Joram in 1972 when he asked me to speak at a conference. He and Olek Pełczyński were the acknowledged leaders of the resurgence of Banach space theory, while I was a beginning researcher, yet within fifteen minutes the great Lindenstrauss asked me a math question! Although the question was right up my alley, I had to work most of the night in order to have something for Joram the next day. This was the beginning of a friendship and collaboration that spanned five decades, and I take pride in the fact that I have more collaborations than anyone else (fifteen according to MR) with Joram.

In the 1970s Joram's family spent several summers in Columbus (I was at Ohio State then), and my family spent one year in Jerusalem. In 1981–82 both families were in College Station, where Joram and I took our sabbaticals and Joram's wife, Naomi, worked on her PhD in computer science. Our wives became close friends and our children grew up together. Once when we went for Shabbat dinner at the Lindenstrauss home, Joram, with some help from Ayelet and Elon, built a Lego city in their living room. Our son was always happy to return to "Joram's toy store." Later I was a coach of a soccer team on which my son and Joram's daughter played, and I played basketball with Kinneret and some of her boy friends. Kinneret, now well known in the biophysics community, was the best player on the court and had a successful second career as a professional basketball player. Shabbat dinners at the Lindenstrauss home after our children were grown were particularly enjoyable when some of their children were present. It was great to get to know the adult Gallia, the youngest and most widely read of the Lindenstrauss clan (Google her to find out why), who was often present helping Naomi with the preparations.

Joram's and my mathematical collaborations ranged from nonseparable Banach spaces to the geometry of finite metric spaces. To Joram these were not very different. He viewed his migrations from topic to topic as natural. Our early research was in the linear world, although even in the 1970s Joram tried to interest me in the nonlinear geometry of Banach spaces. I knew well his landmark 1964 paper, in which he laid out a blueprint for what nonlinear Banach space theory should be, but I thought I had no intuition for the topic. That changed in 1981 during our sabbaticals. Marcus and Pisier, as a consequence of their work on stochastic processes, proved a seemingly unrelated result on the extension of Lipschitz mappings from finite subsets of L_p, $1 < p < 2$, into a Hilbert space. Their theorem suggested a general result, where L_p is replaced by a general Banach space, but because of the nature of their proof, they did not get a result even for the Banach space L_1. Joram and I realized that we could solve the problem if we could prove a dimension reduction lemma in Hilbert space. After formulating the lemma (now called the Johnson-Lindenstrauss Lemma) we proved it in fifteen minutes. Still, we knew it was a neat result because it not only allowed us to solve the problem of Marcus and Pisier but also eliminated the "curse of dimensionality" in certain high-dimensional pattern recognition problems. Of course, we had no idea that this lemma would become the most quoted result of either of us, having 1,000+ references according to Google Scholar, more than three times the references for all of our other research articles combined, and getting 134,000 hits when Googling "Johnson-Lindenstrauss lemma." Joram's appreciation of the J-L Lemma is revealed by looking at the list of his selected publications that Joram drew up in the year before his death when he knew that the end was near; that is, the paper containing the lemma is not among the twenty-six articles he selected! Actually, I was not surprised by that; Joram put a premium on difficulty and was not very comfortable with the attention the J-L Lemma received.

Joram, Gideon Schechtman, and I spent a lot more time on the geometry of finite metric spaces in the 1970s and wrote three articles on the topic, but even after averaging these with the J-L Lemma paper, our results per hour of work on the topic were pretty low. At the end of the decade I thought my initial desire to stay away from the nonlinear world was correct. Then in the 1990s I explained to Joram how results in the linear theory could combine with an argument of Bourgain to show that certain Banach spaces (specifically ℓ_p for $1 < p < 2$) are determined by their uniform structure (a Banach space is determined by its uniform structure if whenever it is uniformly homeomorphic to a Banach space Y, it must be linearly homeomorphic to Y). This excited Joram and led to a series of papers, all joint with Schechtman and all but one joint with David Preiss, on nonlinear Banach space theory in an infinite-dimensional setting. It's a good thing that I let Joram drag me into the project, as the only three of our joint papers that appear on his selected publications list are from this period. (As you can see, even after all these years I long for Joram's approval.) After these collaborations, David and Joram continued in the nonlinear world and did deep research on the differentiation of Lipschitz functions, culminating in their book with J. Tišer. Joram was very happy with this work and was looking forward to doing more on differentiation theory, but, alas, that was the last mathematical contribution he was to make.

Ayelet Lindenstrauss

My father loved being a mathematician. The boy who grew up in Israel and left it for the first time after completing his PhD never ceased to marvel at being a member of the international fellowship of mathematics. To us, his children, it was also a wonderful thing. Some of my father's earliest and

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Joram holding Ayelet; parents Ilse and Bruno.

best mathematical contacts were in Poland. We knew of many people who were separated from friends by the Iron Curtain, but my father was the only person that we knew who had made friends across it. By communicating through colleagues in Western Europe, he was able to maintain these friendships as relations between Poland and Israel worsened and resume them when the Iron Curtain fell. My father had a Syrian friend and a Lebanese; none of my friends' parents knew anyone from these countries. Mathematicians my parents talked about, from faraway places, would then show up in our house.

Often my father took us along on his mathematical trips, especially when there were beautiful places to be seen. When we were young and he went on trips without us, he would hide chocolates in various cabinets around the house for us to find: easy ones in the kitchen cabinets for the first days, and hard ones in upper cabinets we rarely used for the later days. When he got back, there were always lots of presents. Once a customs agent, inspecting my father's suitcase, suspected him of being a toy salesman. When I was twelve and decided I wanted to embroider on evenweave fabric, my father returned from Switzerland with a full meter of the most glorious handwoven linen. I used it very sparingly and worked every last bit of it. Afterwards, when I needed kinds of thread which were not available in Israel, I would tell him what I wanted, and during his next trip he would go to a thread store and pick out colors. Apparently his repeated visits caused some of the salespeople to wonder what he did with the threads, but he always brought me very useful color ranges.

My father did not talk much about his work at home, and when I got to my second year as an undergraduate, he chose not to teach what had become his signature second-year analysis course because I would be taking it. (Many of my classmates were quite unhappy about this.) My main mathematical interaction with him was writing, from his outline, most of the second volume of his (Hebrew) textbook for this analysis course. It is a sampler of topics in analysis, each pursued long enough to prove a great theorem or two. My father had very high standards for mathematical writing, particularly for getting to the heart of the matter as quickly as possible and for not writing anything in a more complicated way than was absolutely necessary. I certainly learned a lot from the experience.

I do a very different kind of mathematics than my father did. He was frustrated by the distance, but maybe it is not so far fundamentally: my starting point is also geometry. When I prepare a class or write out a calculation, I often hear him preferring one approach over another.

In the last thirteen years, my father thoroughly enjoyed his new role as a grandfather. My family and I miss him very much.

Elon Lindenstrauss

My father influenced me in many ways—some that I am aware of, some that I am not. Mathematics has been tangibly present in my parents' house since I can remember. Many dinnertime conversations would be about academics; from time to time, particularly when a collaborator of my father would come for an extended visit, a mathematician would join the family for an informal dinner. Sometimes there would be more formal dinners where we kids stayed in our rooms and helped (or at least tried not to hinder) my mother's preparations. One of my mother's favorite stories is that when Erdős came to a party at my parents' house while on sabbatical in the US, he immediately wanted to see us, the Epsilons, who were upstairs in a part of the house that we (which probably mostly means my mother) had not had time to tidy up.

While academic life was frequently mentioned in our house, my father did not talk much about his mathematical work. He never pressured us in any way to become mathematicians, though both my big sister Ayelet and later I decided to go in this direction. Indeed he seemed initially ambivalent about mathematics as a career choice, even though he clearly enjoyed being a mathematician. He was certainly extremely pleased to follow our progress.

As a child I played board games with my father. Risk and Othello (aka Reversi) were among our favorite board games, and as in all endeavors, he was very systematic and thorough, developing strategies that he shared with me. When I got older and started getting interested in mathematics, he would suggest books from his large and carefully selected mathematical library. Only once have I been directly taught by him—in a course for

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mathematically inclined youth, where he would present problems and we would try to solve them. In retrospect I am very happy for this experience.

My father was very honest, had high standards both for himself and for others, and always said exactly what he thought, regardless of whether what he thought was pleasant or unpleasant to hear. From the time I was a graduate student to the last talks of mine which he attended when I was already a well-established researcher, I was always especially nervous and tried to be very well prepared when giving a talk when he was in the audience, as I was sure I would be told afterwards exactly what he thought of the talk.

Until very close to his death, even as his health was deteriorating, I have relied on his advice, both on mathematical and nonmathematical matters. He was a source of pride and strength to our family and will be dearly missed.

Vitali Milman

It was the ICM-1966 in Moscow. A lot of mathematicians arrived from the West, but my highest expectation was to meet Dvoretzky and Lindenstrauss. I knew well one of the first papers by Joram about duality for the moduli of convexity and smoothness and also read all of his work that I could find in our (poor) libraries. However, Dvoretzky indeed arrived, but Lindenstrauss did not. Dvoretzky told me that "they" (Russian authorities) wrote to Joram that there is no room in hotels (?!) and they cannot let him in. So, the first time I met Joram was in Israel in 1973 after my emigration. It was a very difficult time, after the Yom Kippur war. My family stayed in a dormitory for new emigrants in Tel Aviv. Once someone knocked on our door. I opened and saw a young, extremely nice-looking person who looked at me and said, "Joram Lindenstrauss." I remember this moment well after forty years. I lost my voice, and I hardly remember the continuation of our first meeting. Despite hundreds of days we spent together later, and despite the passing of forty years, that first image of Joram stays in my mind, comes to my mind when he is mentioned, and is not shadowed by later changes.

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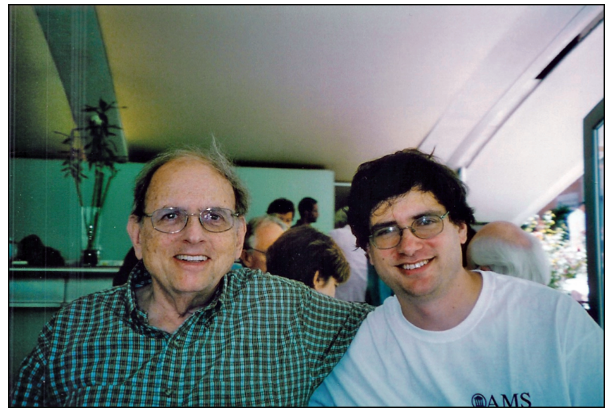

Joram and Elon at IMU, 2006.

Our serious scientific cooperation started two years later (I needed this time to learn Hebrew and English, at least to understand a little bit of both) and resulted in joint papers with Figiel (Figiel-Lindenstrauss-Milman) in 1976 (*Bulletin AMS*) and 1977 (*Acta Math.*). I heard opinions that these were the most significant results in geometric functional analysis in the 1970s. I learned a lot from working on this paper with Joram, learning from his broad knowledge and his taste. I felt that I became a different mathematician at the end of this period. Unfortunately, this cooperation stopped and returned only ten years later. We actually prepared some directions and ideas for working together, and I even wrote a few pages of notes. But one young mathematician heard the discussion on these results, quickly wrote a paper on them, and submitted it. Joram was very angry, for the "cornerstone" for a new direction we wanted to build was taken out from under us, and our cooperation was stopped for a long decade.

Our second period of research cooperation, from the mid-1980s, was joint with Jean Bourgain. It also, I think, was very successful. That time I turned to the direction of convexity, but "asymptotic" convexity, not the classical one, and "pushed" Joram to discuss this subject during our summer stays at IHES. I hope he liked the outcome as much as I liked it.

Our joint activities and cooperation were not reduced to joint research. From the start of the 1980s we organized a seminar (mostly in Tel Aviv) on geometric aspects of functional analysis, which soon became very famous and world known under the nickname GAFA seminar. For many years it met regularly, generally twice monthly on Fridays, and attracted a lot of people from all over Israel (and many foreign guests). Six books of proceedings of this GAFA seminar were published during that time, mostly by Springer, jointly edited by the two of us. Later, the health of Joram did not allow him to come regularly, and the seminar changed its appearance.

In his work and his activity, Joram always emphasized nontriviality and difficulties, but also quickly caught new ideas and had good taste. He did not allow "easy" works to come through his hands. This harsh approach of his kept the high level of research in geometric functional analysis and also had a great influence on the *Israel Journal of Mathematics* during the period he was a leading editor.

The loss of Joram is a great loss to all of us—his colleagues, friends, and mathematics in whole.

Assaf Naor

Being the last doctoral student that Joram advised before retiring, I have known him for a shorter period than the other contributors to this memorial article. For this reason I will not describe a personal story about Joram, but rather mention aspects of his impact on mathematics and mathematicians.

Joram's influence was multifaceted. I and many others who interacted with Joram associate his name with uncompromising professional standards, be it integrity, good taste in choosing research projects, or reserving praise only for results that contain truly outstanding and important new ideas. This approach inevitably made Joram an exceptionally sharp critic, perhaps somewhat intimidating at times, and always an inspiring role model.

Joram's mathematical contributions were exemplified by deep and original insights combined with remarkable feats of technical strength. This resulted in his solution of some of the oldest and most important questions on the geometry of Banach spaces. In addition, Joram had transformative impacts on mathematics by putting forth new research paradigms that shifted the focus of subsequent work, and after decades of efforts by Joram and others, his deep insights have led to rich new theories of central importance. An example of Joram's forward-looking introduction of a powerful research agenda is his work on the nonlinear geometry of Banach spaces, motivated by rigidity phenomena (some of which were his own discovery) that indicated that there should be a "dictionary" that translates insights from the geometry of Banach spaces to the setting of general metric spaces. Almost fifty years after his initial contributions along these lines, one can safely say that this approach has led to many unexpected results in metric geometry, spreading the influence of ideas that originate in Banach space theory to areas such as computer science and group theory. These cross-cutting links between mathematical disciplines were far from obvious when Joram initially formulated his questions, requiring (in hindsight) coping with new phenomena and the introduction of new tools that go far beyond what was previously understood in the linear theory. As an example, one can point out Joram's remarkable intuition, formulated jointly with W. B. Johnson, that there should be a nonlinear analog of Maurey's extension theorem (a phenomenon that was eventually verified due to ideas of K. Ball). This was put forth in his work on extension of Lipschitz functions, a paper that included, as a tool, a dimensionality reduction lemma which has had an extremely important and central impact on various aspects of computer science (exemplifying Joram being a sharp critic, he believed that this lemma, despite being his most cited work, was too simple to be considered an actual result).

Joram's death is a huge professional and personal loss to many people. It is certain that his insights and profound impact on mathematics will perpetually endure and even increase over time as more progress is made on his long-term research programs and the ideas and methods that he introduced are used in new contexts.

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Gilles Pisier

Reminiscing about Joram, the first thing that comes to mind is how incredibly nice and supportive he was to me early on. So much so that the initial awe that I had of him quickly evaporated, even though we never exchanged too many words. In fact, although this cannot be entirely true, I remember having only "serious" conversations with him, meaning all revolving around math, in sharp contrast with the discussions I had with his friend and colleague Lior (Tzafriri), which covered the whole spectrum and could be at times very funny or quite intimate. Joram always remained (mostly in my imagination) a rather tough father figure always in demand for deeper and harder theorems.

I met Joram for the first time in Oberwolfach in October 1973. He and A. Pełczyński emerged there as the two main leaders of the new field to be labeled "geometry of Banach spaces." I was only twenty-two and he still thirty-six for a few more weeks. This was also the first time I met most of my future friends and colleagues in that field, including Tadek Figiel, with whom conversations led to a theorem that to our surprise (because it looked to us as a mere combination of known results) Joram pushed us to publish together. This was

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an isomorphic characterization of Hilbert spaces as those Banach spaces E admitting an equivalent norm with (essentially) best possible modulus of convexity δ_E and one (possibly different) with best possible modulus of smoothness ρ_E. Joram had long been interested in questions related to these notions and had proved a famous duality formula between δ_E and ρ_{E^*}.

We met again in Durham in summer 1974 when I presented a more substantial theorem on renorming of uniformly convex spaces using martingales. I don't remember any comment from him, but he and Lior immediately invited me to visit them in Jerusalem, which I did for two months in December 1974. While in Durham, Joram heard that I was preparing a paper with Per Enflo on what we called the 3-space problem: If a given Banach space X has a closed subspace $Y \subset X$ such that both Y and X/Y are isomorphic to Hilbert space, is it also true for X? This was a famous question attributed to Palais (but this was never confirmed). Enflo and I could not solve it but showed that in terms of type, cotype, uniform convexity and smoothness, the space X was very close to being Hilbertian. For instance, in the n-dimensional version of the problem we concluded that X was C_n-isomorphic to ℓ_2^n with $C_n = O((\log n)^\alpha)$ for some $\alpha > 0$. Joram told me he had an approach to that problem and, to my terror, insisted that I give him a private briefing to describe our results, at the end of which he made no comment, but I remember being puzzled that he seemed happy. Perhaps it made him feel he was on the right track. Later on that same year but in Jerusalem, Joram showed me his counterexample to the Palais problem, and to my surprise (and actually against my will!) he contacted Enflo to stop the publication of our paper (on which printer composition had started) in order to make sure that he could join us as a coauthor. In Joram's ingenious counterexample, the distance to Euclidean n-space was larger than $c\sqrt{\log n}$. Thus it showed that the positive results were essentially best possible, and so the final joint paper gave a quite complete solution of the Palais problem. This took me by surprise and to this day I remember feeling embarrassed to have become, at his insistence, a coauthor of such a major breakthrough. Kalton and Peck later gave a different example (usually denoted by Z_2), which is very closely related to complex interpolation theory for the pair (ℓ_1, ℓ_∞). Kalton also gave an example where the above $\sqrt{\log n}$ is replaced by $\log n$, which is sharp.

From the 1973 meeting I remember with emotion Bob James's lectures on his nonoctahedral nonreflexive outstanding example, solving a long-standing problem. In a famous *Annals* paper from the 1960's he had proved that any nonreflexive

Joram receives the Israel Prize, 1981.

space must be square. A space is called octahedral (resp. square) if it contains for any $\varepsilon > 0$ a $(1+\varepsilon)$-isomorphic copy of ℓ_1^3 (resp. ℓ_1^2). James went on for hours in his own strange style of "hands-on" mathematics, seemingly allergic to the more commonly formalized statements others were used to, and everybody seemed lost save for Joram. James had made premature claims in the past decade, so there was some skepticism in the audience. To us junior auditors he looked like he had hit too many a wall and all he kept doing was explaining how to cleverly add "bumps," but this time he was right! Joram listened patiently till the end, and the next year he produced with James an improved and much more digestible version of James's landmark example. Moreover, with Davis and Johnson he developed a penetrating analysis of the degrees of reflexivity of a Banach space. A sample result is that while James's example shows that nonreflexivity of a space X fails to imply that X is octahedral, the nonreflexivity of $R(X) = X^{**}/X$ does. This somewhat explains why the James examples were found among spaces of codimension 1 in their bidual (i.e. $\dim(R(X)) = 1$), just like the space J for which James had become famous in the 1950's. More precisely, letting $R^2(X) = R(R(X))$ and $R^k(X) = R(R^{k-1}(X))$, they showed that if $R^k(X) \neq 0$ (i.e. $R^{k-1}(X)$ is not reflexive), then for any $\varepsilon > 0$, the space X contains a $(1+\varepsilon)$-isomorphic copy of ℓ_1^{k+1}.

This reminds me of a very dear souvenir of the Lindenstrauss family, a sort of personal treasure. In the type/cotype language, the above examples showed that type $p > 1$ did not imply super-reflexivity (or reflexivity). Thus it was natural to wonder what happened in the extreme case $p = 2$. Joram knew that I was obstinately trying to prove that type 2 implied reflexivity while in Jerusalem back in early 1975, and one day he called to say that he thought he had a proof using the iterated logarithm law (this later collapsed), but since his son, Elon, was sick and his wife, Naomi, had to go teach, he had to stay at home, so would I agree to come to his place to talk while babysitting with him. Of course I was delighted, so he picked me up and we worked for a couple of hours at his home until Naomi's return. How could I have imagined then that the charming little toddler who was jumping around under our table was a future 2010 Fields Medalist!

To complete the James saga, shortly after Oberwolfach, Joram and Bill Davis showed the existence among the James zoo of nonreflexive spaces of type p for any $p < 2$, and finally James himself showed (while we were all back in Jerusalem for a special year in 1976/77) that even type 2 does not imply reflexivity. Eventually, a few years later, Xu and I managed to exhibit (by a different method, using real interpolation), for all $1 \le p \le 2 \le q < \infty$, nonreflexive examples with type p and cotype q, except for the obvious exception $p = q = 2$ characterizing Hilbert space.

David Preiss

I feel as if I have known Joram all my life, even if for the first forty years I lived in a country whose relations with Israel were not on a level that would allow us to work together, and even our one brief meeting at a conference was not to be mentioned too loudly. My mathematics, however, has always been deeply influenced by Joram's work, as documented by a referee calling me "a mathematician of Lindenstrauss's school" long before the political situation allowed us to meet and work together. Towards the end of the eighties Nassif Ghoussoub considered inviting me for a visit and asked Joram about me. Joram responded that we had never met (and then he said something positive about my mathematics). When I later told him that we actually had met, he said, "So I was wrong" (specifying quickly "but only in one point"). I remember this because of my admiration for his "so I was wrong": it was very rare, because he was usually right; but when he wasn't, he would not waste time arguing. Just these four words, and we would start discussing new ideas.

I recall a number of nonmathematical events, such as playing basketball on a team opposing him (he was very good), going to concerts, his daughter Gallia's pictures of Jerusalem, walks in Jerusalem, exhibitions, and of course his welcoming family and the huge amount of help we were given by his wife, Naomi. Surprisingly, I cannot recall when we started talking serious mathematics. Most probably, we began by discussing the Lipschitz isomorphism problem and its possible solution using derivatives. This still-open problem asks whether, say, reflexive, separable Banach spaces E and F are linearly isomorphic provided they are Lipschitz isomorphic. A natural candidate for the linear isomorphism is the derivative of the Lipschitz isomorphism at a suitable point. In special situations the use of Gâteaux derivatives, which are known to exist but need not be surjective, combined with results from the geometry of Banach spaces gives a positive answer. Fréchet derivatives have the property that the derivative of a Lipschitz isomorphism is a linear isomorphism, but even Lipschitz self-maps of Hilbert spaces can fail to be Fréchet differentiable at any point. Nevertheless, the Gâteaux derivative of a Lipschitz isomorphism of E to F at a point x is a linear isomorphism provided that its compositions with elements of the dual of F are Fréchet differentiable at x. Whether such a point x exists (when E, F are reflexive, say) is open. This problem is naturally divided into two parts: firstly, whether any countable collection of real-valued Lipschitz functions on a reflexive space has a common point of Fréchet differentiability, and secondly, whether such a point can be a point of Gâteaux differentiability of a vector-valued function.

Much of what we did with Joram was related to the above problems. Our first paper, written mostly during my first longer-term visit to Jerusalem, was motivated by the observation that a weakening of Fréchet differentiability, called almost Fréchet differentiability, suffices to answer the Lipschitz isomorphism problem. We constructed such points for any finite number of real-valued Lipschitz functions on superreflexive spaces. We spent some time discussing whether the method, a variant of density points, could lead to stronger results, but eventually agreed that there are serious obstacles to it. In the end, in a paper with Johnson and Schechtman, we gave a much simpler proof of this result from which the main obstacle to extending it to countably many functions is clearly seen.

When we began our investigation of differentiability problems, it was known only that real-valued Lipschitz functions on spaces with separable dual have points of Fréchet differentiability. It was not even clear whether there is a single infinite-dimensional Banach space in which any two such functions have a common point of Fréchet differentiability. The key problem in trying to find such a point seems to be that the (weaker) Gâteaux differentiability requires a measure-theoretic concept of smallness, while the Fréchet requirement is closer to the use of Baire category. Mixing measure and category smallness are fraught with the danger of proving excellent results for elements of the empty set. Nevertheless, we noticed that while

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measure theoretical smallness is required in the space, smallness in the sense of category may be needed only in a suitable space of measures. Thus we defined a new class of negligible sets, which we called Γ-null sets, as those sets that are null for typical (in the sense of category) measures in a naturally chosen space of measures. In some Banach spaces, including c_0 and Tsirelson's space, we showed that real-valued Lipschitz functions are Fréchet differentiable Γ-almost everywhere, and for these spaces we therefore know that every countable collection of such functions has a common point of Fréchet differentiability. However, we also proved that with the space of measures that we have used or with similar spaces this program fails in Hilbert spaces.

An important point was our recognition that Fréchet differentiability problems are sharply divided into two categories depending on whether one requires validity of mean value estimates or not. The validity of mean value estimates means that the closed convex hull of the set of Fréchet derivatives and of Gâteaux derivatives coincide. In an "on and off" (as Joram called it) investigation lasting about ten years, joined also by Jaroslav Tišer from Prague, we found reasonably satisfactory answers in the first case. Perhaps the easiest of our results to state is that $\mathbb{R}^2$-valued functions on Hilbert spaces have so many points of Fréchet differentiability that the mean value estimates hold, but $\mathbb{R}^3$-valued Lipschitz functions may have so small a number of them that the mean value estimate fails, and analogous results hold for n functions on ℓ_n. Our rather involved proofs appeared in the research monograph *Fréchet Differentiability of Lipschitz Functions and Porous Sets in Banach Spaces*, published in February 2012. We did not consider this as the end of our joint work, but corresponded about questions we should study in order to understand differentiability without mean value estimates; better understanding of Gâteaux differentiability featured most prominently. Unfortunately, Joram's health did not allow us to continue these discussions.

Gideon Schechtman

I first met Joram when I was an undergraduate student at The Hebrew University in Jerusalem. I took Introduction to Functional Analysis from him, but strangely enough I decided to try to recruit him as my PhD advisor only after I had a magnificent course in topology with him, an area which was to a large extent new to Joram as well. Until then I was seriously thinking of completely different directions of research (first game theory, then set theory). When I approached Joram and asked him to be my PhD advisor, he was already advising at least four PhD students, and I got the (probably completely unjustified) impression that he was trying to discourage me. He gave me three papers to read. The first was Dvoretzky's spherical section of convex bodies paper, an extremely important paper that only a handful of people managed to read. I'm not one of them even to date. The second was a paper on the geometry of L_p spaces that was very badly written and in a language I was not fluent in. It contained a very nice result, and I made a great effort to understand it but failed. It later turned out that both the proof and the result were wrong. I don't remember the third paper.

I don't know if it was the intention of Joram, but actually these two papers, in spite of the fact that they caused me such frustration (and maybe because of that), had a major influence on my later research. Most of my PhD thesis revolves around the geometry of L_p spaces, and some of my later research is very much connected with Dvoretzky's theorem. I remember vividly the few encouraging words that I heard from Joram after I proved my first result. I guess this was the first time I heard some compliments from him, and I must admit that even many years later I was struggling with mathematics mostly to squeeze a few good comments from him (I'm exaggerating a bit, but this is not far from the truth). During my PhD years Joram and I tried to cooperate twice. One of these periods came after I thought I had solved the distortion problem (that was solved almost twenty years later by Odell and Schlumprecht), but nothing written came out of it. Yet, I still learned a lot from these experiences. Also, more importantly, it helped me overcome a fear of him, and little by little we became colleagues rather than a master and a student. Our first fruitful cooperation came only a few years later.

So what was Joram's main influence on me? I can't say it is the direction of research he set me to; I would probably follow him in any direction he would be in. Also, I cannot honestly say that his mathematical power and insight (which were undoubtedly great) had a unique influence on me. What really set him apart from the point of view of his influence on me were the standards he set and lived by. He had very high standards as to what constitutes good mathematics and for scientific and personal integrity. These were also very easy to absorb from him: he always said what was on his mind whether asked for it or not. Until late in my career a major component in my decision of whether or how to write a paper on a result I got was trying to estimate what Joram's opinion of it would be and trying to avoid a sarcastic remark

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Joram and wife, Naomi, in Switzerland, 1994.

from him. On the other hand, any expression of appreciation from him had double the value, because he never said something different from what he really thought.

Andrzej Szankowski

Joram was for me a very important person; I think he influenced my life more than anyone outside my family. Although he was not my official advisor, he has been for me the main guidance in mathematics.

I first met Joram in Aarhus in spring 1970. Until I settled in Israel in 1980, every year I visited him in Jerusalem. During my first visit in December 1970, I witnessed a spectacular display of Joram's abilities. He worked then, with Lior Tzafriri, on the complemented subspaces problem. A complete solution seemed out of reach, and the main effort was to settle it in special cases. And then one morning Joram came beaming to the institute and said "I worked on it the whole night and here is the solution." So it was—a very clever, clear-cut, complete solution to a major problem. This was done with a "feedback technique" which Joram mastered: in order to prove that a quantity is bounded, it suffices to obtain an inequality in which it is bounded by a decreasing function of itself.

The 1970s were the golden period of Joram's seminar in Jerusalem. Almost every week he or one of his students came up with a new result. In 1976–77 Joram organized a Banach spaces year at the Institute of Advanced Studies of the Hebrew University. This was a great year for most of the participants, for me in particular: day by day Joram and I discussed the problem of the approximation property for $B(H)$. This problem was somewhat above my limits, and I don't think I could have coped with it without Joram's encouragement and advice.

In the 1980s I collaborated with Joram on several problems. We have a nice paper in nonlinear analysis which came up in a typical (for Joram) way: he got me interested in a paper from which I observed a generalization of the Mazur-Ulam theorem; after seeing it, Joram overnight figured out how to turn it into an "if and only if" theorem, a complete result. Another paper from this period perhaps would not be remarkable, except that it contains probably the first contribution of Elon to mathematics: Elon, then a high school student, wrote for us a computer program which computed the maximum of a monstrous function that came up from our computations.

Joram was a great lecturer. He had a very special style: writing rather little on the blackboard, he made an impression of improvising, but, miraculously, everything was very well organized. I think it came quite naturally to him, without making notes or much preparation. He wrote two beautiful textbooks in Hebrew: *Advanced Calculus* for second-year [undergrad students] and *Intro to Functional Analysis* (together with A. Pazy and B. Weiss), for MSc. students.

From the moment I first met Joram, I came to like him as a person. He had sharp opinions and wasn't reluctant to put them forward. This made him a poor politician but a valuable teacher and friend. Joram set high standards for what was publishable, both for himself and for his students. He was highly suspicious about general theories which didn't seem to have interesting models. Joram was a very modest man, handling in a relaxed way numerous honors which were bestowed on him. He was very careful with superlatives, rather consciously using his own scale of appreciation, in which "beautiful" or "deep" were used very seldom, and "profound" was apparently reserved for Dvoretzky's theorem.

Joram's world was centered about two subjects: mathematics and his family. He had many friends around the world, but as he used to say, these were either mathematicians or family. He was a social man and enjoyed entertaining people. I have fond memories of many evenings I spent at Naomi and Joram's home. He took special care of young people, not only his own students. Inviting them frequently, together with influential people, he helped them to establish contacts in a natural way. He obviously took care of his students well beyond the expected.

Joram's last years were marred by his deteriorating health, but he must have felt a sense of fulfillment, both as a mathematician and as a family man.

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