Today I'm 70! ,

at least, according to the Israeli passport. My Russian (Soviet) passport says that it was yesterday, on 22 August.

I was born on 22 August, at 11:30 pm, and 2.5 hours later, on 23 August at 02:00 am my mother turned 20 (a gift for her 20th anniversary), so our family knows that we were born on the same day (more precisely, on the same night). My mother's, as well as my, birthday was celebrated on the 23rd day of August, and all my childhood I knew that my birthday was on 23. I do not even remember noticing the difference when I received the passport but I had already been aware of this before I left for Israel (in summer, 1973). However, after crossing the border, I decided to record in my Israeli passport the date to which I was accustomed - the 23rd of August which became the official date of my birth.

Following William Somerset Maugham, I try to look back on my life. Maugham wrote "Yesterday I turned 70...", and for me, too, it was actually yesterday.

The biography (was born..., grew up..., married...., got children..., retired....not yet died) - this is not what I want to understand and describe. Maugham's memoirs impressed me by the description of how psychology changes with age. Later (after 10 years) he also wrote " Looking Back on Eighty Years..." and then "On My Ninetieth Birthday..." Unfortunately (or fortunately, as Maugham would think) he did not live to be 100.

Until recently, I was a man with a very strong ego, although my mind has limited its effect on me and I have always tried to be fair, even against my ego. "Fairness" was probably a motto of my life. As far as I remember myself, I have always stopped and thought if I was fair. That is why I could no longer live in the Soviet Russia, and in 1972 I decided to apply for emigration to Israel. It was practically impossible at that time to emigrate from a restricted scientific town Chernogolovka where I lived and worked. We were the first at that place to apply for emigration. Apparently, I was very exact (and certainly very decisive and courageous. For example, I walked with a golden (i.e. yellow) Star of David on my jacket for everyone to see) because, contrary to all predictions (people called me crazy saying - "how can you do this to your family?"), and only after one refusal I was allowed to leave in July 1973. How this happened and what we did for this is a long and separate story, and I will tell it once, but I am not going to do it now.

My purpose now is to understand my own development, my success and achievements from the perspective of my age and my declining ego. I would like to write the "vanished" ego but I feel it would not be completely true. Something still remains, although it is no longer as painful as before.

So, here is my scientific credentials: I am a mathematician. When I left Russia at the age of 33, I had already defended my second doctoral thesis, so called Doctor of Science Thesis (actually, I submitted the thesis at the age of 30 and defended it when I was 31, but by the time of departure my thesis had not yet been approved: it remained "frozen" forever). I was of quite high standing although I worked in an unfashionable and not quite modern area of mathematics. Today, we know that many of my works proved to be important and subsequently contributed to the development of several areas in mathematics. However, at that time it was very unfashionable but probably quite exquisite and fine for enjoying a relatively high standing. Only the young Gromov immediately recognized the importance and beauty of my proof of the Dvoretzky's theorem (now a well-known work which has had a significant impact on the development of the "concentration of measure" phenomenon leading to the creation of a new branch in mathematics - "Asymptotic Geometric Analysis"). But Gromov held, as he does now, a special place in mathematics. In fashion does not influence his opinion (although, I think, his ego does). He creates the fashion, and later (not back then), after Gromov emigrated to the United States and then to Paris, his support and acceptance of what I did played an absolutely key role in my rise and my (formal) successes (for information: Gromov is commonly recognised as one of the most influential and best living mathematicians; many people today would call him number one World mathematician, although it is a controversial issue, especially for those who have "their own" number ones).

I see I have already started describing and assessing my scientific successes and achievements without finishing the description of my "scientific credentials". I am getting back to it now.

My emigration was followed by a long period of "rehabilitation" which lasted several years: during the difficult period of emigration my brain ceased to practice mathematics. The human brain is like a heavy train, and the better the brain, the heavier is the train: if it (the train-brain) moves, it cannot be stopped, but once stopped, it is almost impossible to get it in motion. That is why I do not allow my students to take a too long break - 2-3 weeks could be too much. However, "rehabilitation" did happen followed by a continuous (though a difficult) period of rise.

To sum up my status today, I would say I am a "very well-known" mathematician. However, it is a word play, literally taken: the word "well-known" means that many mathematicians know you (by name or even by face). Indeed, I am well known among actively working mathematicians, especially if they are not too young. There are many reasons for this. For example, I am an editor-in-chief of one of the most famous and high-level mathematical journal, GAFA, which stands for Geometric And Functional Analysis - the journal which I and Gromov created in 1990.

This in itself would be sufficient for becoming "well known", but there are many other reasons. For instance, my father, David Milman, is the co-author of the famous Krein-Milman theorem which is included in standard university courses. So the name Milman is already familiar to any mathematician. There are also other reasons for the influence. Say, the best geometer of the World Gromov is among my close friends and colleagues, and also the best analyst Bourgain and one of the best combinatorics of the world Noga Alon, the best topologists, algebraists, etc. These are not all of the reasons, but I will not go on.

I will tell you a short story, only to confirm this. Once in Vancouver, I went to the office of a professor, a very good friend of mine, Nassif Ghoussoub, who was also the Director of PIMS (Pacific Institute for the Mathematical Sciences). A lot of pictures of dozens, maybe even hundreds, of mathematicians taken at conferences, mostly in groups, were displayed on a special board. Nassif told me: "Everyone who enters this office recognises you at once, while others, even such celebrities as Bourgain, are not recognised by everyone".

All of the above reasons do not indicate how good a mathematician I am. That is why I wrote above: very well-known in literal sense, of course. However, my scientific level is another issue which needs to be analysed.

In this connection, I would like to note one surprising but natural thing: excellent organizational skills and an understanding of situations and people very negatively affect the recognition as a high-level scientist. Of course, the gift of a good organiser is as rare as the gift of a high-level mathematician. These are "independent" abilities (we would say "independent events"), so it is very rare for one person to have a combination of these skills. That is why a majority of good organisers in science are not high-level scientists. This organisational gift is instantly obvious. There is no need to analyse non-trivial scientific works to admit it. The reflex then throws into question the academic level of a scientist who is clearly successful in his organisational efforts.

Unfortunately, my successes in establishing a department of mathematics in Tel-Aviv, intensifying the scientific life in Israel, receiving and absorbing the scientific emigration to Israel, creating one of the world's best mathematical journals and many other achievements that are commonly known, recognised and appreciated for their significance.

Thus, I always had to struggle for admission to the high league, and many "well-wishers" and enviers could turn my successful achievements against me. It is sufficient to be very "positive" and say "My God, he is such a good organiser", without saying anything about the science.

I have never aspired to any official positions (only a couple of times when it was necessary for the success of the case), but I managed to do it "from outside", "on the fly", even when I did not want to play any role in it. It was not the same in mathematics: every success involved a very hard work and often looked, at first, as a partial success.

I remember a funny story also related to Nassif whom I have mentioned earlier. It was in summer 1985, shortly after the end of the 84-85th academic year when I achieved a series of very significant results; this was a real success. During that year I was in Paris, in the IHES institute (where Gromov has worked and still works as a permanent member; there were 3 permanent members - mathematicians, and the institute was the most prestigious mathematical institution in Europe). As I estimated later, I produced about 15 or 18 papers during that year - the most prolific scientific year in my life. Apart from my own works, there were works produced jointly with Bourgain, Gromov, Pisier (how impressive sound these names today!), and later already during the summer with König and Tomczak-Jaegermann. There was a very big conference somewhere in summer where I was supposed to give an hour's long talk. By that time it also became known that I was invited to give a sectional, 45-minute talk at the International Mathematical Congress in Berkeley (in 1986) which means a great honour and recognition (the Congresses are held once in 4 years and by importance and selection process, they are much similar to the Olympic Games in sport; such an invitation determines a mathematician's status till the end of his life; I received another similar invitation 12 years later to the Berlin Congress in 1998).

So, during the conference a large group of us were having our lunch when Nassif suddenly said: "Vitali, how does it feel to be an absolute winner?" Total silence followed. All my "rivals" who found it difficult to understand how I managed to outdo them were in this group. Nassif was smiling with a grin. He was a very clever psychologist (mathematician and organiser) and knew exactly what he was up to. Everyone liked him so he got away with it.

Thus, by mid 80s I gained recognition but with it came the envy and jealousy of those whom you have to outdo on your way to the top (the same as in the animal world; and we, people, are complete animals in this sense). Someone was slower, and others were already on the top and could now move (or have already started moving) only downward.

…I feel a bit scary: do I still experience the "struggle", the problems with the rise? Or are these only reminiscences that make you feel the time? I do not want to go back to the psychology of those years.

Yet, no matter how much I try to put this issue aside, I still have to describe my actual accomplishments. Well, I published about 170 scientific works; but what was their contribution and what trace did they leave in mathematics?

I want to think about it in large blocks, without any details and with almost no "theorems" which, as many people think (including mathematicians!) make a mathematics (which is totally wrong).

Still before my emigration to Israel, I discovered (an unusual word for mathematics: not "proved" but "discovered") two phenomena, two principles in the behaviour of systems with a very large number of variables (the number of variables asymptotically growing to infinity). One of them is known today as concentration of measure phenomenon and, following Gromov (see his surveys for 2000), it is called Levy-Milman concentration phenomenon. Perhaps, I will get back to it later. Another phenomenon was concept of spectrum / distortion (I called it "spectrum" in my first works on this subject in 60s) which, following Gromov again (1983) and Pestov, is now called Ramsey-Dvoretzky-Milman phenomenon (or also Ramsey-Milman phenomenon). Vladimir Pestov has recently published a book with this title. I will not describe a mathematical picture of these concepts (there are plenty of books and reviews to which we can refer specialists), but I will only say that the concentration of measure phenomenon linked geometry with the analysis and Probability Theory, and changed our view and intuition of the behaviour on multi-variable systems: instead of total chaos and increasing diversity, we find a quite organized and orderly behaviour as dimensions grow - "almost well determined" instead of "almost random" behaviour". Yes, it was similar to the Law of Large Numbers and Central Limit Theorem of Probability

Theory, but only with an amazing generality where all the ordinary and seemingly natural limitations and conditions of the Probability Theory are "swept away" and replaced with the general principle of "concentration."

The concentration phenomenon proved to be a very powerful tool. Gromov once said that there were almost no discoveries of this level in the Analysis in the second half of the 20th century. Many theorems which seemed absolutely unclear and difficult could be easily "cracked" through the correct application of the concentration phenomenon. Later I often read that my proof of the Dvoretzky’s theorem and the concentration of measure phenomenon created a modern branch in mathematics - "Asymptotic Geometric Analysis".

The second concept - the "spectrum concept" - was actually the first concept to arise in my imagination. It was the concept which led me to use the concentration (and I found the work and book of Paul Levy which was of 50 years old at that time), and as a result of this concept, I proved the Dvoretzky’s theorem and then went on studying completely non-linear objects (Grassmann and Stiefel manifolds).

My proof of the Dvoretzky’s theorem was the first after Dvoretzky (I stop so that not to say more), 10 years after him. His proof consisted of 50 pages of a difficult geometrical analysis. Hardly anyone read it in full (at least I know of no one). My work consisted only of 2-3 pages of mathematics and described various consequences of this approach which are constantly applied even today. Let us say that my estimate of the dimension of sections close to Euclidean is the key fact which is constantly applied. The mentioning of the "Dvoretzky’s theorem" mostly refers to this very estimate. There is not yet another proof of this estimate, although already 40 years have passed ever since. Then several other proofs of the Dvoretzky’s theorem appeared (mostly inspired by my proof, as their authors told me), but with no such exact estimate.

Again I recall a story from the past of how someone tried to change the history. In 1996, our group got a semester at the Institute of Mathematics at Berkeley (MSRI). Several mini-courses were provided, mostly, for the young generation, as an introductory course to the key topics of the semester. One of them concerned the concentration of measure phenomenon. I will not say who delivered the course. It is too painful for me as I regarded and still want to regard this man as my close friend. But I have always known that he had much closer friends whom he probably wanted to make happy. It has just become known that I was invited to deliver a Plenary talk at the European Congress of Mathematicians in 1996 in Budapest. Such congresses have been held only since 1992 in keeping with the style of International Congresses, as well as once in 4 years. There were 10 plenary talks in total (for the audience of at least 1,000 people), and the honour of being among the invited was very high. I should note that the most important thing for me was that I had been presented to the Scientific Committee by the great analyst of the 20th century L.Carleson who insisted on my invitation. It was restricted information but one of the Committee members told me later about this: he also said that Carleson read my works and described them in his report to the Committee. I think I was not selected immediately, and there was some struggle involved. Carleson himself called me into my office somewhere in December, as far as I remember, and asked me to accept the invitation; I pretendedto be "hesitated" for about 30 seconds and then agreed. However, it was like "rubbing salt into the wounds" of my potential rivals - human envy exceeded all my expectations.

So, this mathematician, the friend of mine, told about the concentration of measure without mentioning me. After the first (or second) lectures I even asked him about it. He said he prepared a place where he would talk about me. I calmed down a bit. Finally, he started the last lecture by saying that after completing the general overview of the concentration of measure and before giving an example of its application - the proof of the Dvoretzky’s theorem - he would like to name a person who made the greatest contribution to the development of the concentration theory, and this person was .... (I expect him to call my name) M.Talagrand, he finished with a pause. The pause was definitely made for me to expect my name, He gave a slight grin (for his friends to see what he did to me). Incidentally, Talagrand was not there. Otherwise, he would rise and say that it was nonsense. Michel Talagrand has actually worked on the concentration of measure problems since 1988 (after I have already finished my work on it and, as Talagrand himself stated, he was fully influenced by my work), and has proved a lot of great theorems. He officially dedicated his works to me, and in each of his works he wrote that he was my follower and quoted my philosophy to which he adhered in his works. The lecturer was well aware of it but he was very eager to hit a blow so that I would not "hold my head high" (I think I have never held my head high. For instance, in summer 1985 at the same conference which I mentioned earlier, professor Lior Tsafriri from the Jerusalem University, who was a very shrewd psychologist, came to me and said: "you're a good fellow, Vitali, you reached such a height in your career this year, but did not change a bit). Going back to that lecture, it was not yet the end. The proof of the Dvoretzky theorem which he then told about was certainly mine (others were known only to a few experts), and this was stated, but with a preface that it was the fourth and fifth proof after ... (names were then mentioned). The book of this mathematician published 10 years earlier contained all the references to the so-called "previous" proofs cited by him which were actually published 4-5 years after my work (only one of them was published a year or two after my work, but none was published earlier or in the same year).

I nearly got a heart attack then. I could hardly talk. This is how scientists are destroyed. Probably in the same way the best expert in functional analysis who emigrated from Russia to the USA, Boris Mityagan, lost his motivation to work for many years thanks to joint efforts of a few experts (his misfortune was that one of his real well-wishers honestly wrote in his reference about this scientist to one of the American University that finally one of the best specialists in FA arrived in the USA where currently there are no specialists of such level). However, I turned out to be stronger than Boris (whom I very much respect and love as only very few others).

After finishing this hard episode which I try to forget (but cannot), I suddenly remembered that I had been threatened and warned about the possibility of such a scenario many years before it happened, back in 1979, when, as a recent emigrant, I just started making talks more freely in English and embarked on my rise. It was summer 1979 after the Sabbaton I spent in Albany, New York, and before my second year in America, in Detroit. Almost all the experts in our field whom I knew gathered in Columbus, Ohio, under the auspices of Bill Johnson, who regularly held such summer workshops. I brought with me a work written jointly with Gromov (which is now very well-known, and was also impressive at that time), including a large number of different comments and observations which were not turned into works and which described an unusual pattern of interaction between different areas of mathematics and our field (more precisely, the things which I advocated; I "led away" the whole group to this field where it stayed for about ten years). The mathematical culture of this group was not too high so my talk made a big (too big?) impression.

I was asked to deliver 3 lectures. After two of the lectures were finished, I was suddenly visited by Lior Tzafriri in my hotel room. It was a very interesting man who soon became a professor of the Jerusalem University. Unfortunately, he recently passed away unexpectedly. He spoke good Russian (although he emigrated from Romania) and he often translated for me soon after my emigration. I have already mentioned him earlier. He was a very shrewd psychologist and excellent organiser who headed the mathematical department in Jerusalem many, many times. They were always wanted him. However, at that time, as turned out, I also was in competition with him. To be honest, I did not understand these relationships quite well. But he dotted the "i's". It was a very hard talk, in the presence of my wife, and she still remembers it. He simply said: what am I aiming at? Why am not I satisfied with my present niche in which I can exist quietly and peacefully? Why do I try to "stand out"? It is actually so simple to destroy me, for instance, by starting to interrupt me during (my) talks and by asking difficult to answer questions.... of course, I cannot remember everything. I was at a loss not knowing what the problem was and why I caused problems (and to whom - it looked like to everyone). How can I stop "standing out"? By not telling about my new results? Or to tell them too boring? Lior is a very frank and honest person and I am thankful to him for his straight talk about the problems of which I did not even suspect. He was simply the first to see that I "outdid" him, and the animal instincts perked up. We have later made it up and found a common language (as he said). But only after that lecture (in Berkeley, almost 20 years later) did I realize what he meant and how a scientist can be killed and his name in science destroyed by omitting mention of his role in his key achievements (and mentioning him only with regard to his secondary roles or in "secondary ways"). Luckily for me, the attack was late, although it is probably never late. I have always felt the reluctance to cite my name in my achievements. This mathematician often delivers courses and mini-courses in this field, and I am sure he never mentions my key role in creating the whole area of concentration of measure, although Ledoux's monograph on this subject and two reviews and a book by Gromov, state it clearly and unambiguously, not to mention Talagrand who speaks about it with almost religious reverence.

I want to go back to the Russian period before my emigration to Israel. Since mid-60s, I have introduced the concept of new geometric modulii for studying the geometry of an infinite-dimensional Banach space. I called two (dual) constructions of such modulii β- and δ- modulii. After more than 30 years, these modulii started to be applied intensively in the study of infinite-dimensional geometry (and I myself applied them once) and also in the study of nonlinear problems. They are sometimes called «Milman modulii», but more often - "asymptotic modulii", which is very correct. However, here one of the structures is called the asymptotic modulus of smoothness, and the other - the asymptotic module of convexity. I think it is not quite correct. I call it "pollution" in mathematics, but I decided not to intervene in this process. It is very difficult to explain this in such a literary essay as it concerns mathematics. In general, pollution in mathematics refers to unnecessary or poor definitions and concepts. Definitions should be thought-out as deeply as good theorems. Definitions which do not fit in with the purposes and concept clutter up the mathematics and can no longer be used where they would be more relevant later. Not quite successful mathematicians often replace bold results which they don't have simply with bold definitions which have little behind them.

The Israeli period

The process of moving to Israel – the initial refusal to emigrate from the Soviet authorities, their subsequent permission, and the first years in Israel – was not an easy one from the scientific point of view. The Yom Kippur War, which began two months after our arrival, certainly did not make it any easier, either. I did not know any Hebrew and did not speak much English at all. All this had to be learned.

Prior to moving to Israel, I loved science, any science (and especially mathematics). I attended all the physics lectures while studying mathematics, and then I worked with doctors, introducing mathematical models into their problems. First, this took place in Kharkov, and then later in Moscow, after moving there (or more precisely, to scientific center Chernogolovka near Moscow). This collaboration was successful, as I understood a whole lot and explained a lot to the doctors. They were amazed at how one could absolutely accurately guess the course of a disease based on the mathematical analysis of the data, which they supplied. Sometimes I changed the design of their experiments. I don't want to get into details, but that cooperation provided incredible intellectual pleasure and satisfaction.

At my institute in Moscow, I was responsible for the group that was solving "non-standard problems," i.e. problems, which were delivered to us from the whole enormous institute (including physics, chemistry and biochemistry), and with which people did not yet know how to deal ("non-standard" meant that there were no methods developed to solve them yet, but in reality even problem formulations did not yet exist, precise formulation of the problems often being the most difficult problems; this was art, at which I have been quite successful). I refused to sign articles on these topics ("the math is too simple," I would say), except for one problem in polymerization, which was way too elegant not to accept. But the department heads at my institute, who often received governmental awards, sometimes even such as the State and the Lenin prizes, in particularly because of solutions of such problems, valued me highly and wanted to express their gratitude, which subsequently played a large role in our receiving the permission to emigrate from Russia, from the town, which was then shrouded in secrecy.

When moving to Israel, we sent one ton of books by parcel post, hundreds of books on biology, astronomy, physics, astrophysics, etc. I had no opportunity to open these books in Israel. I quickly realized that this world did not recognize ‘universalists.’ Even within mathematics itself one first had to become an absolute expert in one particular field. Only an already recognized expert “had the right” to earn additional bonus points doing work in other areas of mathematics, but working in the fields of, say, biology or medicine was left only to the absolutely recognized world-class experts in mathematics. (However, new areas such as bio-mathematics, which sit at the crossroads of these sciences, have appeared by now, and therefore these areas have their own experts, who have not “come from the outside.”)

Thus, one had to become a highly specialized super-expert. I eventually became one, but it took me about 15 years and so much effort that I could no longer think about doing science in a broader sense (and besides, the desire of my more youthful years has by then evaporated).

I estimate my losses due to moving to Israel as an interruption in my work of about 4-5 year duration, from the time I prepared my petition for emigration circa 1971 and until I began working with Figel and Lindenstrauss during the summer of 1975 on our joint paper at Acta Math., which is considered the best work of the 1970s in Geometric Functional Analysis and which served as the milestone marking the new stage of the so called Local Theory. I do not like this title for misplacing emphasis and being misleading, as it sees the main goals of the asymptotic theory of normed spaces (which is how I called it) in the solution of the problems of the infinite dimensional theory of Banach spaces, thereby serving merely as a supplemental tool. From the very beginning, this was an independent field and an independent goal for me. On the contrary, later on, up until the mid-1980s, I transferred the results and methods, which were developed for the purposes of infinite dimensional theory (e.g., the results of Maurey and Pisier in the theory of types and cotypes), into finite dimensional language and precise estimates, without which the results wouldn't have any meaning in the finite dimensional (asymptotic) theory.

Next big step in my "personal development" took place after the first Lebanon war in the summer of 1982, in which I served as a front line soldier driving trucks in a tank unit. I described in 1982 my participation in the war in the «We» magazine published by Perelman, which printed my interview in full. This interview took place a month or two after I returned from the war. David Milman, my father and famous mathematician, died while I was at the front, and I only made it for the funeral, which was delayed until my arrival. Naturally, everything was very emotional, and so was the interview, although it contained a large number of facts, theretofore unknown to broader public and to which I served as a witness. Still, if I had to do this analysis today, I would have done it with different emphases. It takes time to be able to judge events in proper light. But here I am discussing mathematics and the war (strange, isn't it?) played a significant role in my personal development and overall progress.

But first, a few words about going to war. There are many descriptions in the literature about "going to war." But the Israelis do this differently. Even back in October of 1973, after two and a half months since our arrival to Israel, on the day the Yom Kippur War broke out, we were astounded by how the Israeli men (and not only the young ones) were going to the front. There are draft codes announced on the radio, and when your code is called up, the men take off and run towards the collection point, usually near the place where they live. Radio is always turned on for fear of missing one's code, and there are no public good-byes. The men stand in a small crowd, waiting for a special bus, without their loved ones, without parents or wives or children. Maybe those were peering through the windows, I don't know, but the boys got on the bus and right away were driven to the front. During the Yom Kippur War, that usually meant combat zone.

On the second night of the 1982 Lebanon War (6th or 7th of June) at about one a.m. a telephone rang. We got really scared that the call might be from the hospital. My father was there in the final stage of cancer and my mom returned from there to spend the night with us (whereas we were spending time with the Bernsteins, our friends from America, who ten years later emigrated to Israel and is one of the top professor of Mathematics in our department). Therefore, we got scared that the call is from the hospital and that my father is feeling worse. But a calm male voice asked, "Do you know where you have to go?" I said I did, and I had no further questions. I was getting ready "on the run," my wife still didn't know how to drive back then, and it was fortunate that my mother was spending the night at our place. She also got ready in an instant. I popped into the room to take a look at the sleeping children and ran out with my mom, who drove me to the collection point, which was not far from home and which I could have reached by foot. No farewells – neither at home, nor there with mom, only «good bye» and «see you later».

But we had to wait for a long time, calling up everyone who was called on duty from my little town took time, and only sometime between three and four at night we were picked up. Since we naturally belonged to different units and were going to different places, the bus first went to the general collection point, where they arrive from many different places, from the whole central district. In our case this was just a spot in a field, where there were many buses already. But we were not held up, an officer stopped by and informed us, who stayed on the bus with which codes, and who was getting off. Almost everyone stayed on. And then he told us, «You are going to Lebanon», and we were driven to our unit, which, as we just found out, was going to Lebanon.

I remember that bus ride very well. About five or six years ago we were having dinner in Paris with Gilles and Cecile Pisier and Michel and Wansoo Talagrand in the Gilles' apartment. It is entirely possible that it was my birthday, as Wansoo suddenly asked me, "Which event in your life comes first into your head now?" I was at loss for words and said "Nothing," but immediately corrected myself, "There is something, after all." And so I told about that bus ride.

About 40 minutes, or possibly even more, we were driving in absolute silence. The boys, many of whom knew each other (after all, they were from the same unit and from the same small city; possibly, they even went to the same school; after all, almost all of them were very young – almost children for me, at 43), didn't utter a word, I couldn't even hear them breathe. Of course, nobody was asleep. We were going to war, and each one of us was alone with his thoughts.

There was pre-dawn absolute silence outside, right before the birds wake up. I was thinking about what I would be doing now, and saw it very clearly. Then there would be war, and here, too, I had no questions (although I've never been to a war), but then there was uncertainty, the unknown, the darkness: the return from the war. There was none of this at all. No imagination.

There was a famous short story by Heinrich Böll spinning in my head about a soldier at the Russian front, who could very well feel and envision his life up to a certain date, after which he couldn't feel anything. Events develop further in that story, but his feelings do not go past that day. Finally, that day arrives, and … he is killed.

And so, with this story in my head, I was trying to break through and, in my thoughts, to feel the return. And I couldn't. Emptiness. Of course, never before I had to return home from the front, and my emotional state was unpredictable to me. But that comparison with the short story wouldn't let go. We reached the dislocation of my unit in that absolute silence, and suddenly, everything changed all at once. There was the onset of dawn, the birds began singing, and the well drilled bustle started.

Much later in life I understood that this was a very important stage, those "40 minutes." The brain was restructuring itself, changing priorities and the level of its tension, its attention to the situation. Later on, when I was driving in Lebanon, I was memorizing things and paying attention to details, which I couldn't have captured and held in my head in my usual state. I have amazing examples to that effect. This "restructuring" helps one's survival, since one must see everything and remember everything (I still remember a lot until this day, I mean the little and now insignificant details). Yet, at the same time, the brain discards everything that was "polluting" it in peacetime from the head, everything that was not needed "there." As a result, having returned home from Lebanon 40 days later (I think it was the 12th of July, as my father died the night before my return), and entering my office after sitting "shiva" for my father, I saw a desk full of papers, written in my handwriting in prior life, and I could neither remember nor understand what was written in them, what I wanted and what I was working on. I just swept them all into the trash container and was left with an empty desk.

Looking far ahead, 25 years later, on the very same day of July 12th, 2007, a telephone rang in my office, and I was informed that I was awarded the biggest Israeli prize in mathematics (EMET) for my achievements in the field of mathematics, and also for my work in raising the level of mathematics in Israel. I see certain symbolism in that coincidence. The Lebanon War cycle has come full circle and was over for me.

Back then, in 1982, there was Tessier's published paper awaiting me in my mailbox, which had to do with the classical convex problems (although, from the point of view of algebraic geometry, with which I was absolutely unfamiliar). I got interested in those problems and in the classical notions of mixed volumes, and in the geometric inequalities, which were related to them.

Several weeks later I went to America with a short stop in Paris so as to recover after the war and to return to mathematics. Of course, in Paris I have met with Gromov, and between my stories about the war, I was asking about mixed volumes. He gave me a gift of a just printed book by Burago and Zalgaller about these topics. He happened to have two copies (he bought one, and the other was sent to him by the authors from Lenigrad). This new mathematics was easily and pleasantly entering my brain, which was ravaged by the war.

Thus a new period began in my areas of interests and in the development of the whole asymptotic theory, which, from the theory of finite dimensional normed spaces ("Local Theory") became a conglomerate of the convexity theory and geometric inequalities (but with a new for this theory twist of an asymptotically growing dimension) and problems and methods of geometric (finite dimensional) functional analysis. Already a year later, in the summer of 1983, I was giving a presentation in Paris at a conference in honor of Loran Schwartz, who was retiring. At the center of my talk were mixed volumes and new problems of asymptotic theory of normed spaces, and totally new approaches to their solutions, using mixed volumes. The whole set of articles in honor of Loran Schwartz was delayed in publication for two years and was published in 1985. In addition to the new results, the article contained also very brief descriptions of some concepts and approaches of the classical convex theory (stemming from Brunn and Minkowski), which I used.

Many of "our" specialists used these chapters later in studying geometric inequalities used in functional analysis. Questions were posed in this same work, from which I have subsequently arrived (in less than a year) to the so-called Quotient of Subspace (QS) Theorem, which is a theorem about subspaces of quotient spaces, an important and very unexpected result, which was proven by me in the winter of 1984 in Paris, in a hotel room without a shower or toilet, the hotel having been jokingly called a "Polish" hotel, since it was affordable to the Poles (and me).

In that 1983 work (which was published in 1985), I already approached to that theorem and knew it up to a logarithmic factor (by the dimension). But these logarithms can be very slippery and difficult to clear away (and in many problems cannot be cleared away). There were little hopes to clear them with the already worked out methods, almost none. I have already spent an evening with Bourgain, and when he went to catch his train to Brussels (where he then lived and worked), I still went to some horrifying action movie and, upon returning very late, decided to put down a small improvement to the logarithmic estimate, which I found. I just couldn't sleep after that action film. In my tiny room there was a tiny table, on which I could fit three pieces of paper, but I decided to write nonetheless.

And all of a sudden, an insane idea came to my mind. Perfectly impossible, and at first I tried to shrug it off. But then I decided to check it, after all, and … everything began to come together, and the logarithm had disappeared! That was hard to believe, and it looked like in the Baron Munchausen story, where he was pulling himself by the hair out of a swamp – I mean the special method of iterations, which I came up with (and which for some time was called Milman iterations).

There is much to be learned from that story and I often tell it, but in conjunction with mathematics, to my students. But then I believed it and wrote everything down at night, and in the morning I met with Pisier in his office and told him that the problem has been solved (the statement of the problem had been well known by then and Gilles has been thinking about it, too). He didn't believe me. I gave him the written text (just 3 or 4 pages in all; the idea worked very efficiently) and he read it until lunch. When we went for lunch, I asked him what he thought about it. He answered, "I don't know yet," but he stopped saying that it couldn't be true. Of course, it looked too long for the super-expert – to read 3-4 pages for several hours and not to be sure, but the approach was too insane (or at least seemed as such back then, in the beginning; by now people have gotten used to it, and besides, there are other, less crazy proofs). Only by 4 in the afternoon (it was time to go drink coffee) did he accept the proof (and as it seemed to me, he even appeared disappointed). Two years later, practically because of this result, I was invited to give a 45-minute lecture at the international congress at Berkley.

Returning to the work on mixed volumes, there was a problem stated there, which later gave birth to the so-called M-ellipsoid (Milman ellipsoid, which is a commonly accepted term today, introduced by Pisier). I solved that problem in June of 1985 in Kiel, when I was staying with Hermann König. The two of us were working on the duality of entropy, and we described the first (general) important case of entropy numbers, which were proportional to the dimension of space. I drove to Kiel from Paris, where I have spent the whole previous year with my family – my wife and two little children. That was my best scientific year.

There was nowhere to put us up in the city itself, and so a small room and a half was rented, about 20 km from Kiel, on a farm, where there were huge 2-3 m long pigs walking under our window. They were the size of a horse, and I'd never seen pigs that size. And it was there, at yet another tiny table behind a curtain, where I understood the construction of M-ellipsoid and solved the problem. Once again, those were iterations, but of an altogether different sort. The proof was very complex, and only Bourgain understood it completely (I described it to him in Bonn, where he came to visit me in July), as well as Nicole Tomczak, who was already in Kiel and who was observing each step and the progress of the proof. This result (about the existence of M-ellipsoid) turned out to be the most important thing of all the work that I've done in that period. Its role in the asymptotic theory is continuing to grow to this day. It is used in a substantial way in many, if not most, results of the convex asymptotic theory of the last 20 years.

I have to say that half a year before that, still in Paris, Jean Bourgain and I proved an isomorphic version of Mahler's problem (which was an almost 50-eyar-old open problem by then). That is a very famous work with a large number of references, more than a hundred, I think, which is used well outside our field's domain. That was one of four Bourgain's results, which were specifically mentioned at the International Congress in 1994, when he was awarded the Fields Medal (the most prestigious award in the world of mathematics, although it is given only to the young mathematicians before they turn 40). And it was the only result of Bourgain, which was mentioned by the New York Times in its article about the recipients of the Fields Prize that year.

This result could also be called "the inverse inequality of Blaschke-Santalo." When proving it, we also used certain iterations and estimates of volumes, and that helped me when proving the existence of M-ellipsoid, although the structure in this case turned out to be a lot more complex. Today, of course, all these results are proven in a significantly simpler way. But the first pass through is always difficult.

Twice already I have returned and described the scientific events and progress, which accompanied me during the 1984-85 academic year, and I could continue writing about that a whole lot more. For example, about the creation of the notion of "isotropic position," which I needed in order to answer one of the questions of Bourgain (he mentioned it in his 1984 work that was published in 1986). We wrote a paper with Alain Pajor on this subject. By now this has become a central concept within the asymptotic theory. But I would like to talk about something else, about my very strange feelings, which have developed during that year, closer to the spring. I began to feel getting closer to solving problems that I was working on. Before I even knew the solution, my heart would race and I had a strange feeling inside that if only I made a little effort now, somewhere in the subconsciousness it was all clear already, and now I just needed to "accept" and not to let it go. And I was not mistaken; the solutions to the problems were coming to me. I think that during that year, every two weeks a new, non-trivial and often a well-known problem would be solved.

I will digress here so as to describe our state during and right after the First Gulf War with Iraq in 1991, when SCUDs, which were launched by Saddam Hussain, Iraq's ruler, were falling on Israel. There is a certain similarity in how it felt, but our condition during the war is understandable, and is easier to describe.

When a siren went off, we had about 90 seconds to prepare for the missile's impact. Everybody jumped up (this usually happened at night) and did their task, including even little children: turning off gas, electricity, hermetically sealing one room, in which we would all gather (we were afraid of a chemical attack), put on the gas masks, covered our heads with a mattress (in case of a close hit, glass and other objects could fall on our heads from the walls and the ceiling). Of course, a huge amount of adrenaline splashed into our bloodstream, but we didn't feel it, as adrenaline was working. Pretty soon our reflexes were so honed that a siren from an ambulance (or a police car) somewhere nearby during the day, which would be totally unrelated to an attack, would cause the same reaction and much adrenaline. However, it was no longer needed, and we understood that immediately. And a then there was a reaction to the unused adrenalin, which was very unpleasant and hard: the heart was racing and everything inside felt as if it fell and froze. This is a horrible sensation and it takes time to get over it. To not keep Israeli citizens under constant pressure, the TV and radio stations during this period had been switching off the sound in their transmissions, whenever there had been happening in those the sounds of sirens. The ambulances tried not to use the sirens, either.

To continue with the previous story, the sensations I felt when the solution to a problem would "leave" the subconsciousness and enter the consciousness were similar. But instead of the sense of falling, there was a sense of a "lingering yearning" accompanied by rapid heartbeat. It's possible that some other chemicals were released into the blood stream (or a smaller amount of the very same adrenaline), being responsible both for my condition and for the process of transition from the subconsciousness to the consciousness. By the end of the summer I got scared. I was afraid that my heart wouldn't be able to handle it, but I could not stop the onset of those sensations. My wife Ludmila remembers how I began trying to convince myself that I no longer wanted to prove theorems that I did not want to feel those sensations anymore, that I wanted a rest from them. And within a couple of months, they have, unfortunately, stopped. Several years later, when the new mathematics of that year were "digested," I tried very-very hard to stir up the same feelings within myself, to renew, as it were, my "contact" with the subconsciousness (this is a joke, although who knows), but nothing came of it. Only 20 years later, in the middle of the current decade, several times I felt that I was very close to it, but no proof came out of that during those minutes, and the event was not completed. Thus, we always want what we do not possess, and when we have it, we are afraid of it.

I have another example of the connection between the subconsciousness with the body's "chemistry." This example comes from one of the most talented mathematicians of our time, Ofer Gabber. First, a few words about him. Ofer was a 15-year-old student in his last year when I arrived in Israel. Then he went on to Harvard to earn his Ph.D. and returned home to Tel Aviv at age 18. By age 23 he was already a full professor of our department. I was then the head of the Mathematics division and was able to pull through his promotion through the Senate, which was not trivial for such a young man; however, letters about him from the best specialists in algebraic geometry (the direction, in which Ofer was working) were very high, which helped. In the end, he became the youngest mathematics professor in the history of Israel. Yet, at the same time, he was an absolute perfectionist, a difficult predicament in and of itself, due to which he published almost none of his works (although they were all neatly written down and stacked on his shelves), and is thus known only within the circle of algebraists. But in that circle he is treated almost with a religious feeling. He often answers questions about problems, which have evaded the best minds for years, and does so "off the hip," during lectures at seminars, and the whole of algebraic geometry has been moving ahead in the 1980s and 1990s under his influence. For example, expert number one in that science in those years, a Fields laureate Pierre Deligne, wrote in his letter to our university that he asked Gabber a question during a conference, which he (Pierre) was working on a whole year, and which he himself could not answer. A week of the conference has not yet passed by when Gabber brought a full solution. Pierre added, “I was already thinking that I should quit mathematics when I found out that the same thing was happening with everyone around me.”

At that time, in the beginning of the 1980s, I was spending a lot of time on this still very young man, who was unlike anyone else. It was commonly believed that he was the closest with me and that only I could exert any influence on him. Stories about him could fill a book, but here I am only interested in one. Deligne wrote a long article (about 200 pages), which was supposed to be a collaborative work with Gabber, who was supposed to read the text and give his conclusion and remarks. His perfectionism was delaying the publication of very important results, Deligne was getting nervous and asked for my help. I had a conversation with Ofer. His position was that there were mistakes in various parts of the work and that therefore he could not agree to its publication. “But it is impossible,” I said, “that you would point out an error to Deligne, yet he wouldn’t correct it.” “All is not that simple,” Ofer answered. “They (this story has lasted for several years already and the work has acquired new co-authors) want to present everything at such an abstract level, at which many details of the theory have never been properly verified and recorded. I can’t point out where not all is going exactly as described, but when I read an inexact or erroneous statement, I get a stomach ache, and when I read this text, my stomach hurts all the time!”

I had nothing to respond with. The work, which actually turned out to be a book about 350 pages long, came out without Gabber’s co-authorship, although in the very first paragraph of the introduction it was written that the authors consider Gabber as one of the coauthors of this article, who, not being a mere mortal, could not take upon himself the burden of possible potential errors.

Thus, here we have a similar sign from the subconscious, a “stomach ache,” or, more likely, unpleasant sensations inside the body. But, as I can judge from my own experiences, very unpleasant ones.

More than once I have tried to understand how my brain worked, how analogies came to mind, how an idea suddenly appeared. This is hard to “catch.” We immediately fixate our attention on the results, on the end of that chain of thoughts, which are jumping from one episode to another one, and when, even within a minute, we want to understand how the thought came about, the whole transition has already “decayed,” the brain has forgotten about it already. Just a few times I was able to grab that chain by the tail and to unroll it in reverse, while it still has not disappeared from memory.

The results were amazing. The episodes were moving from the starting one to the final one – to the result, which I was fixating, through 6-7 other episodes, each time using a very clear analogy, but there was absolutely no connection between the middle links and either the beginning or the end. At the same time, the final thought often had a meaning and was important. But the intermediate links were not important or necessary! It might be obvious that I was at that time under the influence of the “stream of conscience” of James Joyce (whose books, among others, were banned in Russia in those years for some reason). By the way, I never “caught” a string longer than 7 links, and this, as I understood decades later, made sense. In the second volume of “Visions in

Mathematics – Towards 2000" (GAFA 2000), I discussed it in the "Discussions at the Dead Sea" section, in the part about "Mathematics in Real World."

At the very beginning of my scientific work I even tried to experiment on myself. For example, I noticed that work of the duration of 10-12 hours straight, without breaks, from the evening until the morning, put my brain in a totally new state. Apparently, similarly to long distance running, a "second wind" comes, and the brain switches into another state. Those who have not tried it and have not felt this state do not know the power of their brain, do not know what a powerful tool was given to them. I did not feel it often, and by the age of 30 was already physically unable to work in such an intensive mode. It is difficult for me to describe this state today, too many years have passed since, but I am still envious with respect to my younger self, when I could experience it.

But let us return from fantasy to mathematics.

The result about M-ellipsoid, at which I interrupted my narration, was called at the time that I did it, "The Reverse Brunn-Minkowski Inequality". And that similarity of name with my previous result, joint with Jean, ("Reverse Blaschke-Santalo Inequality"), as well as the similarity of plans of proof, then led to the ironical remark of Pisier – "Now will come the reverse inequalities" (a clear allusion to his utter contempt for that new result). In conversation with Gilles (in August of that summer), I already had in my hands a short article without detailed proof for CRAS (the French equivalent of DAN in the USSR), and intended to write a detailed article with all the details for Annals (the best mathematical journal of that time). That would have been very hard work and Pisier's sarcasm put an end to my project. His opinion was very important to me, and although I still believed in the importance of that work, I had stopped believing, that it would soon also be accepted by others. I lost a lot on account of the fact that I refused to write a detailed work. Gilles quite soon understood that it was a very important result, and tried to prove it himself. He was not successful in that at first, and he tried to decipher my brief note (with a detail plan of proof). I considered that for him, what was written ought to be enough, but for some reason he could not get through it and work out the details. This went on for about two years and it seemed to me that it was his main occupation at that time. His pride did not allow him to ask me to meet and work out all the details for him. At some stage he doubted, whether I had proved that theorem and he asked Bourgain. Jean replied that he understood all the details and could tell him. For Gilles that was enough and he continued to work on his own proof. One day, I gave a three-hour lecture about that work at the seminar of Gromov at INES. On the way back to Paris in the Metro, we continued to discuss the proof. During that conversation, we understood that using the method of covering (entropy) instead of precise estimates of volume, as I did, might be useful and simplify some of the steps. I think he was the first to say "A" in that direction, but in the discussion, still open to question, and that was quickly developed by us in understanding. In such cases, I always consider that something is understood together. But on that occasion, I wanted to say "Gilles has understood it", and to make a move in his direction. Afterwards, he was taking it too seriously.

After a while, Gilles found his proof of that and similar theorems, utterly remarkable (and absolutely non-trivial}, which also gave very non-trivial additional information. Unfortunately, we are not sufficiently advanced to be able to use that information for new main facts. Only for improving estimates, for which later we have no use. But not many people understand what the new major facts are, and improving estimates with an unknown and undefined goal, is actually the main aim of semi-solid work. I wait and hope that understanding how to use Pisier's improvements, for the principle progress of the Asymptotic Theory, will come, and that will be a great day for us.

Incidentally, at the same time as Pisier's proof, I also gave a new and very comprehensible proof of both reverse inequalities. Pisier understood it at once, and was quite satisfied. I think that today, precisely that second proof of mine is known to the experts. In reviews on that subject, only that one is expounded.

Both "reverse inequalities" already had a comprehensible meaning for the experts in the classic theory of convex bodies. And these results were immediately noticed. One corollary of the reverse Brunn-Minkowski attracted particular attention. It turned out that in large dimensions, an arbitrary convex body (after a special choice of a coordinate system, the so-called M-position of the body) has very little diversity. There always exist (only) two rotations such, that convex hull of the intersection of the body with its first rotation and second rotation of that intersection are very close to a Euclidean ball (its distance to the Euclidean ball does not depend on how great the dimension is). This utterly and absolutely contradicted intuition, and experts in the theory of convexity could not imagine how that could be proved (after they had decided to believe that it was true). My cooperation with all the directions of the Convexity Theory, which continues to this day, began with that work.

I should say that a change of intuition defines for me the arrival of a new direction in science on the whole, and in math in particular. Our thinking and comprehension are based on intuition, and only a presentation of our understanding is formal. Very often, intuition is taken out of formal texts, and articles often impoverish themself. To get an "unexpected", surprising result gives the greatest pleasure, and indicates a change of intuition. Because "surprising" means not corresponding to established intuition, not continuing in the way we are accustomed to think.

I often give my students the following comparison, to explain how our brain works. We see things precisely in front of us, like horses in the town, whose side vision is totally covered, so that they do not turn aside and are not afraid of what is happening to the right or the left. Any turning to the side (off the beaten track) is tormenting, difficult, and for many, almost impossible. It is true that conservatism was important for our survival at early stages of human development. But now, we should try with all our might to turn our head, and try to see something new. Only very few mathematicians do that often and well. It is a totally different parameter in our work, then an ability to prove even very difficult theorems.

Thus, at that time, one result after another demonstrated the destruction of old intuition, built a new intuition and, likewise, the new field of mathematics. I chose for its name between many variations, and eventually, we stopped at "Asymptotic geometric analysis", and under that name it went into the programme of the last International Mathematical Congress alongside "Functional Analysis". Of course, Noga Alon, my co-author and collegue in the Department wrote it thus at my suggestion. He was the Chairman of the Program Committee of the Congress, 2006 – a great honour.

To digress, I must admit that I introduced into math many new terms in fields far removed from mine, which rapidly became generally accepted. For this it is not appropriate to give references, and I hope that my revelation will not call forth surprise and hysterics in the experts in these fields. For example, to describe briefly what Gelfand does, I called his field of activity "algebraic analysis", which quickly caught on. Then we argued with my friend, Piatetskii-Shapiro, who recently died after a long and grueling illness, with which he had struggled for several decades, that I might define all Gelfand's mathematical activity in just two words. He did not believe that such a thing was possible, but, astounded, immediately accepted that combination (this was in the first half of the eighties). Ten years later, we were strolling with Gelfand in the forest of IHES, (in "Bures-sur-Yvette", a suburb of Paris), and I said to him that two words were enough to describe all his maths, (he did not only maths), and he, who considered himself a maths encyclopedia, and that he was into everything, looked at me, astonished and angry. I pronounced those two words, and he stopped, thought it over, and pronounced: "You know, I agree with you".

Likewise, the expression "Asymptotic combinatorics" was used by me for the first time in the Proposal for the summer term at PIMS (Vancouver), as one of many directions for our semester. Seeing this, Vershik immediately called a conference in the St. Petersburg Institute of Mathematics under that name, maybe, because he liked it, (and he really is one of the leading experts in the field), but possibly, in order to "claim" the term at once for himself. There were also other terms which are generally known today.

In connection with that, I remember a funny episode. In Israel, in 1997, in connection with receiving the Wolf prize, the physics-astronomer, John Wheeler, gave a lecture. It was in our faculty in a very large auditorium, full to overflowing. So, this is what I remember that he said about the role of words in science. "Don't underestimate the importance of the well-chosen word in science," he said – "if I hadn't thought of the expression 'black hole", I would not have become famous, nobody would know anything about this field, I would not have received the Wolf prize, and would not be standing here now." Everyone laughed, but there was profound truth in that. Ill-chosen vocabulary, and poor delivery rather puts people off the field, and certainly does not help to attract them. An utterly catastrophic example in this area is given by the following expression: hereditary indecomposable spaces (abbreviated HI, and the way they pronounce it is ridiculous – "Hi", i.e. "Hello!" in English, and I would like to add, as we say in Odessa: "How do you do? I'm your aunt!") These spaces were discovered in the mid-nineties, and Tim Gowers received, for a series of works in that direction, the Fields medal (proposed, actually, by me and with my support). But he was too young, and it was not he who defined the terminology, but other experts (also experts in destroying a potentially good mathematics at the same time) I tried to intervene and proposed several other alternatives (for example, "atom", inasmuch as these spaces as well as any their infinite dimensional subspaces were impossible to decompose into direct sums of smaller parts). The general topologists, who created similar objects (without any linear structure) adopted my proposal, but for the specialists in the infinite dimensional Banach space Theory, during some time, the absence of a common sense became more or less a symbol, and "good" taste.

I return to the beginning of cooperation with the group on the Theory of Convexity and to the very rich and beautiful subject of Geometric Inequalities. It started slowly. Rolf Schneider came to my lecture in Bern in 1987 (or maybe in 1990). Ervin Lutwak was invited by us to a conference in Banf in 1988. By the way, the first Mathematical Conference in Banff, which was organized by Nicole Tomczak-Jaegermann and Nassif Ghoussoub at my request – a couple of years before that, I had traveled around that place and I had liked it very much; later, Nassif decided, that the idea was a success, and created a permanent centre there for conferences, held throughout the year, and today he is the Director of that centre.

Finally, in 1989, the invitation of Peter Gruber to the special Colloquium on the convexity was already a serious contact, after which an invitation followed, in 1990, to Oberwolfach for a Convexity Week, and the contact became permanent.

I listened eagerly to their lectures, learning from them the theory of convexity, and often problems and concepts which were new to me. If a problem pleased me, I estimated which of my students could solve it, who would like the problem. And some successes here were tremendous. For example, in 1995, at the Corton conference on the theory of convexity Rolf Schneider talked on a new progress in the theory of valuations (those were his and Klein's results). I was not acquainted before with this concept and did not know about the important problem of describing all valuations invariant with respect to the shifts. These were already problems between 50 and 70 years old (depending to whom we are attributing it) and no ideas on how to tackle it in a general form. I decided at once that my student, Semyon Alesker would be suitable for solving these questions. But for a start, I changed the question, and asked him to describe all valuations, invariant with respect to rotations. This is absolutely not a natural change of task for the theory of convexity but natural for the theory of normed spaces. I proved to be right twice. First of all, Semyon well entered these problems, and he solved all the problems opened up here, and went much further. For this, he received the prize for the best young European mathematicians in 2,000 at the European Congress in Barcelona, was invited to give a lecture to the International Mathematical Congress in Peking, and has already been a Professor, for a long time, in our Department. And second, my proposal that he considers first invariance with respect to rotations proved to be the right step. He quickly did that, and the work is published in the Annals. The compactness of the group of rotations simplified the task, and the representation theory played a leading role. After that, he already understood what math he needed to use in the case of a group of *shifts*. It was yet again the theory of representations, of Bernstein's D-modules, which he studied in his lectures. The proof is difficult and required another two years, to get to the end of the solution of the problem. It was with pleasure that I published this work of his in GAFA.

In the middle of the nineties, many students appeared before me, and between then and now, I awarded eight PhD's, and another four are studying now, or are starting their studies this year. Two of those eight received European prizes for the best young mathematicians and lectures at International congresses, and one of them, moreover, won the Salem prize, and both also were awarded many other distinctions. They are not the only ones of my students, who might have been awarded similar distinctions, (and who have been awarded other distinctions), but that was as fate would have it. I am convinced that soon that list will lengthen. So I can consider myself a good teacher (I shall not go further into that area of my activities now; the time has not yet come).

However, a natural question arises, of why I had hardly any students up to that time. Indeed, there I was already at that time older than fifty. Formally, I had signed two Ph.D. theses in 90-91, for two outstanding young mathematicians, Polterovich and Reznikov, who came to Israel without Ph.D's, but they were already established scientists. It was pre-1990s anti-Semitism in Russia that did not let them get a Ph.D. there. Likewise, I took care a lot on them, and they always noticed it. Unfortunately, Sasha Reznikov perished tragically. For the last time, I met him at the European Congress in Barcelona in the year 2,000, where he gave a lecture by invitation – also a big recognition of his achievements. Polterovich received the European prize in 1996, at the Congress in Budapest and was invited to the International Congress in Berlin in 1998. So I am very proud of them both.

My first real student in Israel since the mid-seventies, was Haim Wolfson, my first student for the master's degree, and then for the doctorate (i.e. PhD). We wrote two very serious and now, well known, works. The second of these (together with Bourgain as well) is widely used and quoted now in Computer Science. He was a remarkable student, but later changed his profession to biomathematics; now he is a professor at the School of Computer Science in our university, (Tel Aviv) and, at the present time, Dean of the whole huge Faculty of Exact Sciences, which includes mathematics. A few years ago, he received a Chair in Biomathematics, which is a special honour in our university. I only received a similar honour in '92 or '93. And now, I would like to point out why, during a long time, I did not have any official students since Haim Wolfson. I emphasize here "official", inasmuch as I was connected to young students, beginners, and influenced them. Let us say, one of the very best combinatorics in the World today, Noga Alon, served in the Army with Haim, not far from Tel Aviv University, and worked on his Ph.D. at the same time. Technically, he was a student of the University of Jerusalem, but Haim Wolfson introduced him to me, and he began to work with me. One out of the three chapters of his PhD is our joint work. Then, on my advice and insistence, he went on to do a post-doctorate at the best centre of Discreet Mathematics of that time, MIT (Boston), and not in a provincial university, for which he was heading on the advice of his official supervisor. To follow up, when he left, we began our joint work, which we have finished already by mail, which is happenned to be very important. Today, it is one of my two most often cited works, and also one of two of his. I think that that work, and the atmosphere in MIT, turned him to the central problems of Discreet Mathematics. Of course, he would have got round to that in any case, but he shorten his way to it for about five years. So much so, that, unofficially, I consider him "almost" my student, although for a mathematician of his rank, that word does not really fit. He is a scientist "from God", and not of us, men.

To get back to Wolfson, I was quite anxious, when he was finishing his Ph.D. dissertation. First of all, at the time, evaluation of the dissertation was obliged to pass through Jerusalem. What I have in mind is that Tel Aviv considered itself so provincial, that it considered it obligatory to invite one of its judges (and actually the chairman of the committee) from the Jerusalem University. And Wolfson's absolutely brilliant dissertation was considered just like any ordinary work. Lindenstrauss simply said "no". Why? Only towards the very end of my career did I understand, *that to the potentially best young people in Israel they explained in such a way, that one must get a Ph.D. in the Hebrew University (of Jerusalem).* Much later, when I was already very high up on one committee for awarding students studying for the Ph.D. the best fellowships in Israel, I heard this directly from one Professor of Physics from Jerusalem. But there is a big "secondly". I was powerless then to get for Haim the best place for post-doctoral work. To tell the truth, without the support of Lindenstrauss, I could not offer him even a regular place. To sum up, on the recommendation of other mathematicians, who knew him well, he went to the Courant Institute of the University of New York. A wonderful place, but he went there to change his direction in Mathematics. And this he did, as we see now, brilliantly.

So I decided that I had no right to have students, if I could not then lead them into the scientific world and scientific life. Ten years later (and even earlier) I was already able to do that. I had connections, influence and "strength", and students flocked to me.

A postscript to the story of Wolfson: several years after he defended his thesis, Lindenstrauss himself came to me and said that he had found out a lot more about Wolfson, and understood, that he has a very high level, and how he, Lindenstrauss, had been wrong. But for our field, this was too late.

But, as I have already said, the chapter "about students" is not yet ripe. A few more years must pass, while I "live through" that subject. I still have many students, and need time, to see the picture from afar. I am already mentioning a half-year programme in Berkeley at the beginning of 1996 in connection with an episode, which was very difficult for me. As a matter of fact, it was an exceptionally successful programme. I organized it together with Keith Ball, then still a very young mathematician from England, who was arousing high hopes.

Unfortunately, he later became seriously ill, but, I think, completely recovered. That, naturally, did not make his work easier, but his difficult character hindered his career even more. At times, I utterly, absolutely, did not understand him. I am sure that he blames me for all his troubles (I have in mind scientific advancement), although at every step of his career (and all the same he has done very well) I wrote enthusiastic references in support of him. However, some of his colleagues (and "rivals") went much further than he (and also with my support), and the pain of jealousy eclipsed in him the true vision of the world. It is painful for me to look upon that, but the absence of scientific contact between us, I think, influences him more.

But in 1996, everything was still calm between us, although there were strange, and, at that time, incomprehensible to me, outbursts of rage on his part. But I ignored them. We prepared a very good & intensive programme, with special emphasis on preparing the younger generation. And the results were excellent. Here are a few names of students who were beginners at that time, and who have already become full professors and developed into very well-known mathematicians: Alesker, Barthe, Giannopoulos, Latala, Litvak, Oleszkiewicz, Rudelson and many others (I am sure that I shall not be able to remember all of them now). For example, four of them gave lectures by invitation at International Congresses, two won European prizes for young mathematicians. Naturally, the whole area of Asymptotic Theory was bursting with new results and directions.

After 1996, we understood, how important it was to have such special programmes, and not simply weekly conferences, and we began to organize them regularly in every possible place. In the last ten years, together with Nicole Tomczak-Jaegermann, I twice organized summer semesters in PIMS (Vancouver), a summer semester in Vienna (which I organized together with Carsten Schütt), a spring semester in Paris (together with Pajor, Pastur and Pisier). And that over and above a large number of annual conferences and so-called workshops.

This kind of activity made itself known, and a new generation began to attend. Among my students were Boaz Klartag and Shiri Artstein (now Artstein-Avidan). And I can't resist mentioning Sasha Sodin, who has not yet finished his studies, but is already an outstanding mathematician. The arrival of this generation caused yet another splash and gave new directions for development. Many problems, unsolved for decades, were solved, but I fix my attention only on one direction, which in the last two or three years I have been intensively reviving, together with Shiri Artstein (-Avidan). The story of this direction is instructive for me as well, and I never cease to wonder at the new view of mathematics which is opening up.

One of the most important concepts (constructions) in geometry (and also in mathematics on the whole) is the concept of polarity, or duality. In Functional Analysis, its analogue is the concept of duality, which is also known as conjugacy or adjointness. These are absolutely fundamental concepts and non-trivial constructions lead to them. The question of how mathematics came to these constructions, the story of their creation, often interested me in the past. However, I never (in the past) asked myself, what one should call duality. We considered the complex constructions, leading to it, for something God-given and natural. And here, in one of the works together with Artstein and Klartag, we had expanded the concept of polar, and defined it for a special, widely used class of functions, i.e. not just for sets. All would have been fine, but soon the reasons why we had called the operation we had carried out "polarity", began to cave in. And it turned out that our concept could be rather pollution, than a necessary and natural definition. And the question arose as to what is polarity, and whether our definition and concept were justified. In a little while, we understood everything, (and yes, the concept we had introduced turned out to be the only one possible!). I worked on this subject with Shiri Artstein-Avidan and we deal with functional cases. In the same time I asked Rolf Schneider what he thinks on the case of Convex sets and he solved this jointly with Károly Böröczky.

And the picture, which had opened up to us, turned out to be surprising and unexpected. *Polarity* (which we also call *duality*) turned out to be an operation, reversing the direction of inequality, if we are dealing with functions, or the direction of *embedding* if our object consists of sets. Of course, the class of functions (or sets), in which we study duality, plays a central role, and in different classes different (and non-trivial) formulae arise for describing the operations of duality. In the simplest case of a linear class of *real-valued* functions, it is simply the minus sign, but for the class of non-negative functions, *inversion*, i.e. f to l/f. However, for a more interesting class of all convex functions, it is already the Legendre transform (and only that: in that lies the interest of the results: we are describing all the transforms, changing the sign of inequality for functions. Of course, those, who are interested in these questions, should turn to published works for precise definitions and formulae.

From the very first step in this research, what surprised me was how little one has to require of an operation for its unique restoration. For this, these elementary conditions (changing the sign of inequality) call forth the appearance of formulae, which are not at all obvious, different formulae for different classes. Lately, we have discovered a class of functions, for which there exist exactly two (different) types of transforms reversing the direction of inequality, and one of these types turned out to be a new transformation, a new duality, not known before. The role of Shiri in this discovery was central.

I must say, it was a certain shock for me. Mathematics turned out to be much poorer than I was accustomed to think (there is a positive way to say that as well: mathematics is stable – "rigid"): the most interesting and significant transformations uniquely arise out of the simplest and elementary conditions. I immediately decided to test this for the Fourier transform. And really, we showed, together with Alesker and Artstein-Avidan, that the Fourier transform uniquely arises out of the conditions of the exchange of the operations of multiplication and convolution (again, it is better to read a precise formulation in our work). Now we know many additional similar examples.

I shall turn aside from this story and describe another, classical example of a discovery, which will help me to give an emotional side to our last work with Shiri.

Already more than a hundred years ago, in 1888, one inequality was discovered by Brunn, developed later by Minkowski and called, for the last hundred years, Brunn-Minkowski's inequality. This inequality has a quite elementary proof (I know a dozen of its proofs) but posses incredible strength and is one of the most important geometric inequalities, having created the modern theory of convexity. Amongst its immediate consequences, for example, is isoperimetric inequality in linear spaces. This last fact has occupied the minds of mathematicians since ancient times, but for me the centuries preceding this discovery are important. Thus, such mathematicians as Euler, Gauss and many others overlooked this jewel. Was this an accident? We go ahead along this road, called mathematics and a jewel (of the type of the inequality of Brunn-Minkowski) is lying there, and we pass by and don't notice it. Perhaps this is by chance, but are there not other such slips? Our results in recent years with Shiri show, I think, one more such oversight. Our result about the Legendre transform should have been known already for at least 150 years, but it was not. And I think today that our whole road of mathematics is strewn with jewels, which we do not notice. Of course, one should notice them, and clean them, but one should remember, that they are everywhere around us. Especially for young mathematicians.

Looking through the notes, I see that I have not mentioned some significant directions of my activity at all. For example, my first work published together with Mushkis (fifty years ago), which continue to collect quotation till now, and which has received more than a hundred citations. It opened up a new direction, and I have seen books on this subject. It was a work on Ordinary Differential Equations, and I do not remember a single result from it. By the way, it was indeed done mainly by Mushkis, although the next work together with him, in which probabilistic parameters were introduced, and which, mainly, I did, also did not leave a trace in my memory.

Another most important work, about which I have not written anything, was a joint work with Pajor, from the middle of the eighties, about isotropic positions and the isotropic constant. It opened up a whole direction, which goes on very intensively till now (and it also has more than a hundred references).

I consider very important some of my works together with Giannopoulos (besides two very important surveys); a work joint with Litvak and Schechtman (and a work with Schechtman preceding it) was critically used by Paouris in his excellent work on the estimate of the tail of distribution of volume on a convex body.

What has still not received due attention is a joint work with Gluskin on random cotypes, works joint with Klartag on symmetrization (his own works in this direction are much more interesting) and on the study of isotropic constants.

My works in cooperation with Artstein-Avidan are so many, and they are so varied, that it is impossible to write about them in a few words. I note only the solution by us (together also with Szarek, and then, in another work, also with Tomczak-Jaegermann) of the very old problem of the duality of entropy numbers, the start of a new direction of algorithmic "derandomization" in the geometrical questions of Asymptotic Theory, and many other works and directions, not to speak of our latest results on the understanding of "polarity", about which I have already wrote in more detail. About all these and many other works, not described in this list, it is possible that it will be necessary to write some other time, more mathematically, but that is clearly not for now.

There is only one more work, which I should like to discuss. It is from the middle of the eighties, joint with N. Alon (in our "GAFA Seminar Notes" – it came out already in the 83/84 collection, but officially, in the Journal, it was published in 1985). That is the very same work, about which I have already written, that it is one out of my two most often quoted papers; more than two hundred references in mathematical literature, and something of the order of four hundred Google references. (I reread it now, in 2019, and the number of citations on this paper jumped to already 400). In that work, there is a series of constructions of so-called expanders. It is a particularly important object in the theory of algorithms and the theory of complexity, and is constantly used in Computer Science.

The existence of such combinatorial objects (graphs) has been known since at least 1973 (Pinsker). However, they were random objects, and not a single example of their concrete construction was first known. In 1973, Margulis gave, for the first time, an example of the concrete construction of an expander (it is always more correct to speak of a family of graphs, which represent an expander; that is an asymptotic characteristic of the family). It was a magnificent work, an absolute break-through. However, the mechanism behind the scenes of that example was not understood. Many experts worked with that example, modifying it only very insignificantly. For example, among my friends and colleagues, Gabber Ofer and Zvi Galil. In the course of the next ten years, until our work with Noga Alon, not one substantially new example of an expander appeared. Indeed, for us, the characteristic of expansion, defining an expander, was only one more example of the concentration of measure, but already in a discrete variant. Not long before that, my work together with Gromov (about which I have already written) appeared and in which the level of concentration was estimated through the first non-trivial eigenvalue of the Laplace operator. Noga and I transferred that fact onto a connected graph and wrote an estimate of the concentration on the graph through the first non-trivial eigenvalue of the Laplace operator on the graph. Expanders were received, when that value was separated from 0 by a fixed constant, and the order (degree) of the graph was thus fixed. The existence of such graphs was defined by the so-called T-property of Kazhdan. All was clear and straightforward, and very straightforward simple examples came out. That is, in a sense, the best mathematics: one could have told a schoolboy examples of our expanders, but for proof, that they are expanders, it was essential to use many kinds of modern math. Of course, a posteriori, one can say that even Margulis' example was constructed on those principles. For example, he also needed Kazhdan's T-property. However, that was not enough on the surface, for the experts to be in a position to construct even one more example.

Then events developed with exceptional speed. Our work was immediately noticed, while still in the state of a preprint. Even before publication, Noga received from Lubotsky (then still a young Jerusalem mathematician) two or three-page preprints (a joint work together with two famous mathematicians, Phillips and Sarnak), in which, with reference to our work, and using many different kinds of mathematics for an estimate of the necessary non-trivial eigenvalue, were given other, very interesting examples of expanders. However, very soon, Lubotsky understood that it was an outstandingly important direction, and the whole of Computer Science was eagerly awaiting these results. The relationship immediately changed, and those authors wrote a very long work (instead of the original short one) with considerably fewer references to us, but with exceptionally broaden pieces of generally known mathematics. In some talks, which I heard, he did not refer to us at all. Later, Sarnak introduced a certain order. For Lubotsky, it became for many years one of his main direction. I should note that he is an excellent mathematician, and it is a pity that a "little greed" sometimes overshadow the development, reducing, by a large scale, the "size" of scholars. Lately they have got very far in the development of this area and achieved a number of really surprising results. But the beginning did not appear sufficiently ethical. Incidentally, Margulis then wrote three handwritten pages, in which were all the approaches of that first work of Lubotsky-Phillips-Sarnak. This did not surprise me at all. It was just the mathematical culture of the Moscow school. In connection with this, a little incident concerning this very subject. In 1983, I received a letter from Alon with a version of our work (at that time there was no e-mail yet) in which he asked whether I knew groups with a certain property (which he described). This is important, inasmuch as it follows from our work, that such *groups* immediately lead to new *expanders*. In my office, at that time, were Bernstein and Gromov, who were visiting our University. Both smiled, and said: "The beast runs after the courier." I had to go to the airport at once to meet Kazhdan. "This is his property", they said, "so that he can explain you on the way from the airport". And indeed, back in my office with Kazhdan, I knew what the property T is and many examples of it. However, the sequel followed. I explained to all three why we needed it, and each came out to the board and, one after the other, explained various other facts and possibilities to estimate the needed eigenvalue, which would lead to *expanders*. That was precisely that original connections which Margulis had written in his pages, and Lubotsky-Phillips-Sarnak wrote (in their first work on this subject; I had already written that it had a far reaching development). I did not want to continue this activity, but reported all to Noga (in a letter). He replied that he also wanted to take a short rest from these problems and occupy himself with some other work. Only after several months did our preprint appear, and after a few more months, those events, about which I have written.

By the way, Kazhdan then explained to me that certain groups, which did not have the T-property, we could still use for our purposes, just as we use only certain *families* of representations (but not all of them). One of the immediate examples was the group $SL_2(Z)$. A few years later, I was talking about this in IAS, in Princeton, in the presence of Deligne. These applications of the partial T-property of Kazhdan pleased him very much. I recalled about this somewhat later, inasmuch as, in the 90's, Lubotsky introduced for this a new terminology of the tay-property, instead of, for example, talking about the partial T-property with respect to certain families of representations. Once again, unneeded pollution. The notation of "tay-property" had already been used for a long time before that in Geometric Functional Analysis for completely different goals.

With that, I shall stop writing about mathematics. But just a few more words about "recognition". For myself, I consider two lectures by invitation to the International Congresses and one plenary lecture to the European Congress to be the highest recognition. A few more lectures by invitation are very important to me. For example, the invitation to the conference on the hundredth anniversary of Kolmogorov, a one-hour lecture in honour of the centenary of Paul Levy, and several others.

In recent years, I have also received several prizes (some of them quite significant) and many so-called "distinguished" lectures. On my "home page" all that is recounted in detail.

However, I want to speak about that, which I did not receive. In the course of the last twenty years I was proposed ten times to the Israel Academy but never elected. The Israel Academy consists of two parts, and for me the options lay in the group of "sciences", which include also physics, chemistry, biology etc. In principle, I should not have known about it, but someone always told me about it. One day, one famous physicist said to me: "this time the mathematicians were fighting for you as never before, but in the upshot, it was not enough" I, incidentally, am not so sure, that mathematicians "fight for me" as one man; I am sure of one exception. But I write about that on account of one case, and I feel obliged to make this known.

However, first of all I should like to point out that already for a very long time this problem does not bother me. Yes, the first couple of times, when they told me that they intended to choose me, I wanted it, like mad. Probably, it meant that I was not worthy of it yet. But then it did not matter at all (in other words, I "grew up") and then I simply forgot all about it. The whole idea of the Academy looks by today's values rather ludicrous. What I have in mind are countries, where membership of the Academy does not raise salaries, let us say, as in Russia. It is necessary to those scholars, who have complexes and are not sure of their status, but is absolutely meaningless in any other case. But the story, to which I return, is not meaningless. Thus, in 1996 (I have mentioned that year already many times) Dvoretzky himself decided (and on his own initiative) to propose me to the Israel Academy. I did not know about this, but my secretary told me that he had asked for all my papers, so it wasn't difficult to guess. Dvoretzky was an exceptionally influential person. He was once President of the Academy, President of the Weizmann Institute, and had occupied many other positions, having an influence over the development of all science in Israel. I must say that he liked and respected me very much (as I did him, naturally). Some time in June we met in Jerusalem at the wedding of one of Lindenstrauss's children. He took me aside and said that he had tried to propose me to the Academy, but without success, and he was very upset and angry about it. He added: "They don't want to elect a "Russian"! Of course, all of us Russian Jews called "Russians" for short: that means they don't want to elect a Russian Jew. It was a shock for him. However, already after a previous failure at other elections, Lindenstrauss had told me the same thing. But he explained it rather pragmatically, saying that Piatetsky-Shapiro was elected to the Academy, and then he went off to America. By unwritten rules, he, himself, should have resigned from the Academy, but he did not do that, and now it influences their approach. I am not going to analyze this anecdote. By the way, he added: "If you persuade him to leave, they'll elect you", to which I replied: "I don't want to be a member of that kind of Academy!"

I put a stop to the description of my adult life. I have not analysed the events, and I didn't select them. I wrote the first thing that came into my head, what was stuck there and needed pulling out, and the order of events corresponds to whatever succeeded in coming first.

For that reason the number of episodes with a negative emotional content is greater than the positive ones. We rarely retain positive feelings in our memory, unless they are absolutely exceptional. Thus, it's fresh air that we don't notice. Difficult, unjust events stay with us for a long time, sometimes forever. They weigh down upon our memory, and I have been almost glad to "get rid of them" by writing them down.

However, I should like to make a short excursion into my childhood and youth, in order to see what drew me into mathematics.

A brief run through my childhood, and how I became a mathematician.

The Second World War came to Russia two months before my second birthday. Of course, I, myself, do not remember anything, but I know from what my parents told me, that we fled from Odessa on the ship "Voroshilov" on the 24th July, about two weeks before Odessa was completely surrounded (Odessa surrendered on the 14th October), and the beginning of the battle for the city, which, so they say, was very heroic. The enemy, mainly Rumanian forces, lost more than ninety thousand dead. So, this is what happened. We left, initially, for Sevastopol, in a convoy of three utility vessels with people, and two barges, and also, apparently, accompanied by a warship. In front sailed the (basically passenger) motorboat "Lenin"; behind it the (basically cargo) ship, "Voroshilov" and then the "Gruzia". Of course, travelling on the "Lenin" was much more comfortable. My grandfather on my mother's side, Emanuel Tsudikov, was an absolutely wonderful person, and as we shall see, it was only thanks to him, that I remained alive. He was the head of a section in some factory , business-like, very understanding, and competent. Somehow or other, he obtained tickets on the "Lenin" for the whole, very large, family, but on the day before we left, his boss saw him in the street and pounced on him with something like "Once again, you (what he had in mind was: 'Jews') have got yourselves out, and got hold of tickets for the best ship!" And he took for himself the tickets for the "Lenin", giving in exchange for them, tickets for the "Voroshilov", a cargo ship, where all of us passengers lay all together in the hold. The whole convoy went to Sevastopol, with the "Lenin" towing along, behind it, the "Voroshilov", the motors of which were damaged. In Sevastopol, they carried out rapid repairs and three of the ships, but the "Berezina", instead of the "Gruzia", and one accompanying warship, had already sailed in the direction of Yalta (although the final destination was

Novorossiysk.) That was on the 27th July. In the night, at 23:33 , the 'Lenin", which went first, was blown up and sank within seven to ten minutes. My father told me that practically no-one survived; they picked up about twenty-five people on the "Voroshilov". The other ships also saved some people. The stories my father told me about those who were saved, are impossible to repeat, without weeping. For example, the woman, holding her baby, whom they found on a barrel. When they lifted her out, they could not pull her arms apart, in order to take the baby away from her.

So that was the first bit of good luck - we were supposed to be on the "Lenin".

Then, we arrived in Novorossiysk and found ourselves in the Krasnodar region (a Cossack village – Uspenskaya . Everything there looked fine, and it was very far away from the Front. At the end of September/beginning of October, my mother's brother-in-law, Milya Kogan, came to see us. They were from Leningrad and their daughter, my first cousin and almost exactly the same age as me, was sent by them to the dacha, to Grandpa Emanuel in Odessa, and we arrived with her in the Krasnodar region. Milya worked in a position that was very important for the war effort, and he had already been evacuated to Ufa. He got permission to go and collect his daughter, and, together with her, the whole family. There was a family council, to decide what to do. On the one hand, our conditions were exceptionally good (for wartime), there was food, and my father had already started working (apparently, as a teacher, in the school); on the other hand, we were entitled to go on to Ufa, where, for us, nothing was clear. But all the same, they decided to go. I shall leave out "slight" lucky chances, without which we would not have gone away. The train, (that is, the goods train, which ran for weeks) took us to Stalingrad, and from there one had to get across the Volga, and then on to Ufa. I am describing only the chance circumstances, on account of which I survived – (otherwise, how could I have become a mathematician?) So this is what happened: soon after our departure, the German landing force disembarked in the Krasnodarsk region, where we had been living, and all the refugees were killed. The aim of the Germans was to cut off the oil of the Caucasus. Once again, another "chance" survival.

We arrived in Stalingrad, but I fell ill, and they wouldn't allow me to go any further. There were infectious diseases everywhere, and anyone with an infectious disease was not allowed to travel by train. They decided that my mother was still too young and inexperienced, so my grandfather, Emanuel, stayed with me in the hospital in Stalingrad. That's how I got over that period. Diseases followed, one after another, all around were patients with various diseases. I had already become so weak, that I could no longer walk. My grandfather understood that I would die, and signed that he was taking me out of the hospital on his own responsibility. He wrapped me up like a baby, and carried me on his back. There were no ferries across the Volga to Stalingrad. Only on my seventieth birthday, did I find out why. It turns out that on the 23rd August, 1942, the total bombardment of Stalingrad began, and Russian television reported on that date in the current year, 2009, the truth about those days. In Stalingrad up to a million civilians, half of whom were refugees, were gathered together. So this is what happened. Stalin forbade the transportation of the civilian population across the river. He was planning a great battle for the city, and declared to those who were close to him: "Soldiers don't defend empty cities". So he forbade the transportation of the people. In addition, there is a record on film of the transportation of cattle. Thousands and thousands of heads of cattle went on the crossing, and people were left behind. As a result, they nearly all perished.

It was possible for Emanuel to go along the Volga to the north (where he needed to go) or south, to Astrakhan. He went to Astrakhan and there he went across to the other side of Volga, and then on to Ufa. He looked after me on the way. That decision of his to go south is incomprehensible to me, but it was absolutely right. Again, I have only just found out, that the Soviet command reckoned that the Germans would attack from the south, and would cut off there, the way south to the oil of the Caspian Sea. Fortifications were being prepared there for defense. But the Germans unexpectedly went via the north and covered Stalingrad from the north.

And so we finally arrived in Ufa towards the winter (or in the winter?) of 1941-2. My first recollections, the very first memory of my life, take place in Ufa. Not much, obviously, only the most terrible or the most vivid. For example, how I found myself (or used to find myself?) alone in a little room at night, when my mother must have been at work. At any rate, once, I remember finding myself alone, cowering and waiting. I should mention that at first we lived there as a threesome, my mother, my father and I, but after the first winter, my father went to Central Asia, where the Pedagogical Institute had been evacuated from Odessa, and he began to work there. That was in Bairam-Ali, to the south of Turkmenistan. My mother and I spent the second winter in Ufa, and only then did we go to join him. So that in that nocturnal episode, I must have been three already, or three-and-a-half. I also remember going with some girl, slightly older than me, across enormous snow-drifts. That, apparently, was a very vivid episode for me, and my feelings, as I remember them, were very positive.

My next recollections take place already in very hot Bairam-Ali. Here, many episodes remain in my memory, mainly horrific, let us say; for instance, how, returning barefoot from kindergarten, I ran from the shadow of one tree to the next shadow. The trees stood far apart, and the white-hot sand baked my feet until the pain was excruciating. Or, when I saw from afar a lace, that I very much needed for my sandals, and I started running to get it, but it turned out to be a snake! (But maybe it wasn't really, but it was my dreadful fear of snakes, which created that image? But I remember it, and believe in it). Only two clear and happy events have remained in my memory. The first, our arrival - but I remember only Grandma Sara, my father's mother. She was running round me, and I was sitting on a chair (my feet didn't reach the floor), and I was showing her how I could read (although I couldn't at all, but in this way – I moved my finger along the cards, arousing great joy in her). And the second, which was quite a serious matter, directly changing our life: the day of the liberation of Odessa (in April, 1944). I was standing in a crowd of grown-ups, who were listening to the radio, and suddenly everyone began crying out and started jumping for joy. I looked at them all in amazement. Somebody turned round to me and said: "Run to Mummy and Daddy and say that we have liberated Odessa", and I ran off to deliver the good news. On account of that event, I remember that building, our (large) room, divided by sheets into many tiny pieces for a lot of families, and our own tiny little piece/pan.

We started at once to get everything ready for the return to Odessa, however, it wasn't all that easy. We reached Starobyelsk, somewhere on the Russian-Ukrainian border, and there, we were held up. Only half a century later, did I find out why. (My parents never knew about this). Khruschev, then the First Secretary of the Ukraine, i.e. Stalin's deputy and ruler of the Ukraine, decided that as Hitler had already rid the Ukraine of Jews, it would be a good idea not to allow them back there, at least, not if there were no need. And it was necessary to receive from Odessa a personal offer of work for my mother and father, as absolutely indispensable specialists. I think that my mother set out first in that direction, without permission, and on passing goods trains, and then she received such an offer of work for my father in the "Water Institute" (officially – the Institute of Officers of the Fleet). Then my father went (leaving his work in Starobyelsk, without permission, and that later gave rise to big trouble, where Krein was able to save the situation.)

My awareness had increased at that time, of which I already remember a great deal. Both our life with Grandma (and without my parents), and my duties in that life, (for example, collecting fallen, incompletely burnt coals from the constantly passing trains; that was a big railway junction; of course, we needed those coals to keep ourselves warm). Then I remember the arrival of my mother. I didn't quite recognize her; my memory awoke after her and my father's departure. So I got to know my mother, when I was about five. That, I remember very well. Then we went to Odessa, and again I remember very well getting to know my father. "This is Daddy", said my mother, when he rushed up to me, and I accepted that as a fact.

I turn now to one modern incident. Some time in 1989-90, one of the greatest analysts of the twentieth century, the Sweedish mathematician, L. Carleson, was staying in Israel, as my guest. One day, in a restaurant, his wife began asking me questions about how my family survived during the war. I related to her briefly approximately what I have written above. Her next remark stunned me. "You must be a very strong person, a real fighter." "Why?"- I asked – "apparently that is indeed the case, but how does that follow from my story?" It turned out that she keeps and trains racehorses. And so, she told us, for the first two years of their life, she arranged for the stallions such a hard life, as to instill in them a spirit of struggle, so that they made a great effort, and won races. "We bring them up with such an education, as you must have had to go through. The analogy is very exact, and we know the result".

So, I have told the story of my very early childhood, so as to explain the result, the source of my character.

In my school years, my most outstanding characteristic was a desire to know, curiosity about everything (but no desire for formal education in school). At that time, there regularly came out thin, inexpensive books of the series: "The Soldier's and Sailor's Library". Probably, many people may smile, reading that title, but it was a sort of stroke of genius. Under this kind of general cover, it was possible to publish many varied books, rich in content. They covered all the areas of knowledge – nature, the cosmos, science. I adored them, and had tens of them, if not a hundred. Those books were the best kind of present for me, really cheap, even for such poor people as we were, at that time.

Later on, in the higher classes, this accumulation of knowledge grew, and I became the best student (having started, as one of the worst). My very first wish was to become an astronomer, or, more precisely, an astrophysicist. But my father was able to squash those wishes, explaining time and again, that that was not science, but simply observations. My next greatest wish was to work in nuclear physics. Here, my father was more pragmatic. He simply explained that, for Jews, in our country, there was no way into that science. In a sense, he was right. Already in our class in the University of Kharkov, there were no Jews in the Department of Nuclear Physics – they did not even accept applications from Jews for that department. But Jews could be physicists, and from the point of view of science, there was no difference. Only they wouldn't let me into the nuclear reactors, and thank God for that! But I accepted his reasoning, and from then on, nothing stood between me and mathematics. Especially as I won all the first places in mathematics olimpiads (and likewise in physics - by the way, in physics, even with a much larger gap between me and the children who came after me). However, now I want to digress. Lately, all the best discoveries in physics came via astrophysics and nuclear physics (the theory of elementary particles). Of course, my child's mind didn't know about it, but somewhere or other in the whole, wide world, just such children as I, for some incomprehensible reasons, went in for astrophysics, curiosity "what's there?" in the macrocosmos, and also in the microcosmos (elementary particles), the best young minds were attracted to these sciences, and these sciences offered the best discoveries, and won, in the following decades, nearly all the Nobel prizes for physics. So that children can guess (feel) the future better than adults, and we should not hinder them. I resolved quite firmly not to interfere in the search and choice of my children, and I never interfered.

I finished school with a silver medal, which facilitated my entry into the university. Without it, I would have had to sit for examinations in the Ukrainian language (in the Ukraine), which I could not have passed. But even in Russia, it was very easy to fail the exam in Russian. A gold medal would have been even worse. With it, it would have been necessary to have a "discussion" with the University Committee, which has no rules. But with the silver one – for entrance to the Mathematics Department, there were only two math exams, one written and one oral.

However, to receive the silver medal was not easy. In the beginning, they "killed" it for me, reducing the value of the written final examination in Russian from 4 to 3. It is hard to believe this, but the president of the Examination Committee considered that we must have big money, and hoped, apparently, to receive a bribe of 5,000 roubles for my medal. They themselves got into contact with us. We did not have such a large sum of money, but the rich parents of my friend (who had paid for his gold medal) offered this to us. I had made such an incredible impression on them, that they were willing to present that money. But my parents did not want to put those (dirty) spot into my biography and they lowered my grade, and did not give me the medal. Later on, there was a struggle to get my work for the examination and see the "mistakes". In that we succeeded, and I straight away showed the official (who knew nothing about the money and the whole business) that there were no mistakes, rather that they had been "added". He agreed, but that official committee was required to look at it again. He wrote notes to summon these people, and my father was able to assemble them. They handed me a document about the medal, but that was the last day for handing over documents to the University, and my mother and I flew to Kharkov, and went straight to the reception committee. An official told me that they had just finished taking in applications, but when I, almost in tears, handed him all my official documents of victories in the Olympiads, he suddenly changed his tone and agreed to take them. So I entered the University of Kharkov, to study Mathematics. By the way, Pogorelov got my mathematics entrance examinations (and that was my good luck).

I turn now to the last stage of becoming a mathematician, which did not depend on me. That is the "assignment" to a job after finishing University. In the case of free education, "assignment" to a job after finishing one's studies was obligatory. In those years, they did not take even one Jew in Kharkov for graduate study (for a doctorate, according to the Russian system.) And although, at the end of my fifth year of undergraduate studies, I already had five scientific works published (some had already come out, and some were still in the press), an absolutely unprecedented case, there was not even a mention a possibility of a graduate study for a higher degree. But it was very important to have a job, which would give me a chance of being involved in science. For example, some Institute for Scientific Research. Just one year before I finished university, the new, grandiose Institute of Low Temperatures opened. Its founder and Director, Verkin, understood that it was easier to start to build a high level of science with mathematics. They were never quite settled, but in Kharkov, for physics, there was already a very serious Institute of Nuclear Science. Landau himself had worked there at one time, and would still come sometimes (I was present at his lecture in my last year at university). And so Verkin immediately opened five departments of mathematics (and later, it seems, a sixth), their heads (by holding two jobs at once) were the best mathematicians of Kharkov University, amongst them, my teachers, Levin and Myshkis, and also the Lenin Prize laureates for that year, Marchenko and Pogorelov. To receive two Lenin prizes in one year was an incredible honour for Kharkov. Anyway, they all explained to Verkin that he must take me on, and the Institute gave me a formal invitation.

It was the day of departure. All of us graduates, something between fifty and a hundred people, are waiting in a large hall, and they are calling us out, one by one, into a small room, where the Vice-Rector and representatives of various organizations are sitting with questions for us. They call me out first. Two of our professors, Myshkis and Marchenko, are waiting for me to be called, together with the official representative of the Institute, (incidentally, the head of the first department, that is, a member of the KGB, responsible for security; naturally, he lately hated me, but here he was with a direct order from the Director – to take Milman). And the Vice-Rector wants to let them go. I go in and sit down. I am officially invited to some kolhoz (collective farm), thirty kilometers from Kharkov to teach mathematics in the school. That is, to put an end to my chances of becoming an academic. I should explain that, in order to reach Kharkov from such kolhozes, it was necessary to walk on unmade roads for five to seven kilometers, and then to take a lift in a passing truck and bump along in its cab for a couple of hours.

I would not do well to answer, that I don't want it. The representative of the Institute intervenes and makes excuses for me. An exchange of references to the different decrees of the Party and the Government begins. Let us say, an decree about the strengthening of education in the kolhoz is cited – that is, our best students (there was no dispute, that I was the best) should go to teach in the kolhozes (incidentally, from that it follows, that higher degrees are not for the best students). In response, a decree is cited about active support in the creation of science bases in Kharkov – that is, I should go to the Institute. After five minutes of such exchanges, the Vice-Rector suggests that I leave the room – "afterwards we will call you" – and the discussion continues without me. They continue to call the other students into the room; in the main, their affairs are settled quickly. And practically everyone who comes out, comes up to me to say, that they are discussing me all the time. What is surprising, is that some of our girls, with whom I have been studying for five years, were standing in the corners and crying (on my account); they had come to study out of the depths of Donbass and places, and had met anti-Semitism for the first time, and directly in its crudest, most relentless form. They knew that I was a much better student, than all those who were not now having any problem, and they wept for shame, afraid to look at me. A couple of hours passed, I think. Suddenly, my very best friend throughout all those years of study, a Ukrainian fellow from Donbass (Tolik Kononenko), looks out of that "secret" room and runs up to me: "Vitali, they are letting me sign for your place in the Institute! I can't take it from you." I explained to him, that he could sign; for me they would find a place, if only the deputy Rector would give way. But that son of a bitch didn't want to give way. And here a quite different issue arose. The allocation of places was already nearly finished. Already there was almost nobody left without a destination. Myshkis came out (poor things, he and Marchenko had probably already been sitting there for three hours). He told me that a woman representing Oblono (the District Department for the People's Education), and to whom they were sending me – she was in charge of education in the area's of kolhozes – had offered them help. Apparently, the situation had impressed her, as well as the photography of Marchenko, who had received the Lenin Prize a couple of months previously, she saw in the newspapers, and here she had been sitting next to him already for many hours. And she said that I could sign the papers, she promised to release me. That was her right, and now I had become available, and the Institute was in a position to take me on. By the way, being in a hurry, they had forgotten to get my signature on the papers, but that alone would hardly have helped. My appointment was to that woman. She came out to speak to me, and asked me to come in to her office on a specific date, a month and a half later. I did so. Two ladies were sitting in a large office, but I knew straight away which one to go to. And she recognized me. "No need to explain. I remember," she said, and held out some paper – a document about removing my name from the register. I took it, thanked her, and started to go out. I was next to the door, when she called me. "I forgot to fill something in", she said, "Give me the paper." I stopped, looked at her with a shocked expression and said: "Excuse me, I can't give it back to you" – it was my life, my fate. She was confused, looked very understanding and said: "I understand. Go, it's not important", and I left.

That's all. That was the end of lucky chances of survival, help from others. Everything, that did not depend on me, was behind me. Ahead, everything depended on me, myself, and was hard, tormenting, bearing the tremendous satisfaction, but also, some disappointments, of becoming a mathematician.

Written 23rd August, 2009, and also on the holidays of 30th August, 6th and 12th September, in the days off between days of doing Mathematics.