Biography of Michel Talagrand

Biographies are a time to reflect on the journey that brought us to where we are. As a probabilist, I am fascinated by the incredible unlikeliness of the chain of events which make up my own journey.

Family Background

Possibly our most important influence in life is the environment in which we were born.

All four of my grandparents were born in large, poor peasant families in South-East France. At least three of them had been hungry for significant periods of their lives.

My maternal grandmother's spelling was so bad that I had to read her letters out loud to be able to understand them. But she was a woman of extraordinary character. After she married my grandfather, they set up a recycling business which succeeded through hard work and toil and allowed them to put their two daughters through higher education. My mother Raymonde (b. 1924), the elder of two sisters, earned a degree to teach French in junior high school.

After my father retired he wrote about all the incredible hurdles he had to pass to become the equivalent of a college professor. I read this inspirational text periodically. My paternal grandmother stopped her studies at age 10 to herd cattle and later served as a maid. As a result, she would never learn to write. My paternal grandfather was worse off - he lost his mother at a young age. His father remarried quickly out of necessity as he needed help, but the new stepmother abused her stepchildren and my grandfather had to leave home at age 16. He found all kinds of odd jobs to survive - carpenter, mason, butcher, etc., but could barely read a newspaper. He eventually landed a job as a railway worker and worked his family out of extreme poverty. My father Pierre (b. 1925), only surviving child, turned out to be a very good student. His teacher persuaded my grandfather to keep him in school instead of putting him to work to help feed the family, as poor families typically did. Through much dedication and suffering he succeeded in pursuing higher education.

A Rough Start

My parents met at a students' party in Paris and married in 1950; I was born two years later, the elder of two children. They settled in Lyon in 1955 and built a house in a good middle-class suburb where I was fortunate to grow up.

I was born with myopia and genetically weak retinas. When I was five, I lost my right eye due to retinal detachment. I was exempted from sport activities after that, for fear of damaging my other eye. (Many years later, I would redeem myself by running a marathon under 3h 30 minutes.) I was permitted to sit in the first row in every class, where I could just barely read the blackboard, and the impact was manageable.

When I was seven my father subscribed to a scientific magazine. I read all the articles, cover to cover, only understanding bits and pieces, but I was fascinated.

My spelling and grammatical skills were terrible. This was unfortunate, because at the time students were evaluated largely on their spelling skills. I could not spell, and therefore I should not have been admitted to junior high school. But my father, being a teacher himself, went to see the principal and I was admitted. While any competent student would be studying Latin at that age, I was enrolled in one of the few classes reserved for the very worst students where it was not taught. Only two students from my initial class graduated high-school without repeating. Despite the low competition I remained a very average student. I had some interest in science, but I was more concerned with playing with the kids in the neighborhood. My father would holler through the windows that it was time for homework, which I always did as expeditiously as possible. My father did try to get me interested in mathematics, teaching me some very basic group theory. I paid attention, but I was far from excited. But I must say that, when, more than 50 years later, I did need some basic group theory, I realized that I understood it very well!

When I was thirteen my father took me to Paris for a week, while he was on duty there. I spent the whole week in the science museum *Palais de la Découverte*. It was enchanting. They performed live experiments. I still see a flower being plunged in liquid nitrogen and becoming brittle. I talked to the people performing the experiments. One of them told me that I knew more science than the typical third year university student. I was so proud. But I was also frustrated. One display was explaining how atmospheric pressure decreases exponentially with altitude, and I had no clue what the strange formula meant. In gratitude for this enchanted week I have donated the Shaw medal to this museum, suggesting that it be used to explain to the general public how amazingly successful French mathematics are.

My father, thinking that I would become a scientist, had me take Russian classes, since at the time not all Russian scientific literature was translated. It was one of the very rare bad ideas he had. As I was terrible at grammar, these classes were a torture. As a dutiful child, I spent a third of my study time struggling with Russian.

My Health Changes my Life

Just before my 15th birthday I experienced a retinal detachment in the remaining eye. It was treated but reoccurred several times. I spent a long month in the hospital. My father came every day to see me after his work, trying to keep my brain working by teaching me mathematics while my eyes were bandaged. This is how I learned the power of abstraction. I missed school for six months. My father rescued me a second time: he persuaded the principal of the school that I should not repeat the missed class. I was permitted to drop the dreaded Russian classes (despite the fact that it was required to study a second foreign language). Ultimately the impairment to the left eye was manageable, but the psychological damage was immeasurable. I lived then and for years thereafter with the terror of going blind. The trauma made me a different person, in a way that is still mysterious to me. When I returned to school, I was, at least in math and physics, an excellent student. The teachers were fortunately extremely competent in the last years of high-school classes, having obtained the national certification known as *Agrégation*. I am thankful for the foundations they gave me. I started to put in real effort; for training, I tried to solve every problem in the textbook. In the last year of high school, I participated both in math and physics in the *Concours Général,* a French Olympiad for the top high school seniors. The person making the math problem must have been lousy, because I felt that there were inconsistencies in the problem, and I was not shy to point them out. As I am very naïve, I told this to my classmates, who made much fun of me. But I must have had a point, as I came in third, both in math and in physics. While I felt happy to win such an award, I didn't attach much importance to it.

I am forever grateful to my father and his support in these difficult years. I wish he had known about my late successes. He already had dementia when the Shaw Prize was announced - and passed away shortly after. Even worse, my younger sister has not known about it either, as she took her own life in 2017.

I should elaborate on the terror in which I lived. A retinal detachment is insidious. The detached part of the retina dies fast. When it is gone, this is forever. The sooner you get treated, the smaller the permanent damage, so a retinal detachment is an emergency situation. It is not easy to be sure whether your retina has started tearing away, and then it is not easy to see an ophthalmologist fast to check it. In my case, before I had retinal detachments, the vitreous body, the kind of jelly that fills the eye, was sticking to the retina (which it should not do). When I moved my eye, it pulled the retina, and I saw a flash (It is pulling the retina that tears it). But afterwards, I frequently experienced flashes without the retina tearing away. Each time, it was terrifying, and we had to rush to the emergency room. My poor parents had to wait with me from 8 am to sometimes 3 pm. The flashes would strike at any moment. I will explain later how I eventually rebelled against this terror, and by which miracle I was not destroyed.

At University

In France, the best tracks for higher education are taught in special schools (the *grandes écoles*) separated from other universities. To enter these schools, the very best students must go to a preparatory school for two years of intense study after high school and compete in a national admission competition. My parents, fearing for my health, encouraged me instead to stay in Lyon and attend the local university. While the students were not, by far, the best students nationally, there were many excellent professors who taught the subject matters at the highest level despite the fact that most students could not understand their teaching. Ultimately, I learned excellent mathematics there. These were wonderful years. I was so happy learning math and physics that as a hobby I worked through all the problems in some of the most famous textbooks of the time. The going got easier as I went along. There aren't too many different ideas in those books after all.

After two years of university, I had to choose between mathematics and physics. At the time, physics had no jobs for the foreseeable future so I chose mathematics. After a master's degree, I spent a year taking graduate courses, but none of them greatly appealed to me. No probability courses were offered, and I have never taken such a course. During this year I mostly prepared for the *Agrégation,* the national competition by which higher secondary school teachers are selected in France. I wasted a lot of energy there, as I had to learn useless old-fashioned mathematics like projective geometry. I solved all the exams of the previous years. I spent months preparing to deliver the high-school lecture that is part of the competition. I was very much worried about this lecture, so the previous year I had gone to Paris to watch that year's competitors. I saw a beautiful lecture by Christophe Soulé, who ranked first and later became a star of French mathematics.

Before the competition, one of my professors, Jean Braconnier, suggested that I apply for a research position with the CNRS, the National Center for Scientific Research. At the time students who had not yet completed their PhD were considered for such positions. The odds of success looked minimal as I did not come from an elite *grande école* like the *Ecole Normale*. But things turned again in my favor. Jean Pierre Kahane, a famous mathematician, was on the hiring committee. He did not know me of course, but out of his sense of duty he wrote to me personally, asking to make my point so that I would have a chance to be considered. I wrote back a long letter describing my health problems and explaining why I had not followed the standard path. I was hired by CNRS. Years later, when I thanked him, he gave me this beautiful answer: I just read your letter to the hiring committee.

I have to stress that I was hired by CNRS before I took the *Agrégation.* There, incredibly, for the dreaded high-school lecture, I got exactly the same topic as Christophe Soulé, and I mimicked his great lecture. This certainly helped me to come in first nationally. This had no impact whatsoever on my life, save the pleasure of having received a grade of 318/320!

The blessings of CNRS

French researchers are blessed that France supports research through CNRS. In mathematics, such a position can be life-time. It involves no academic or teaching duties whatsoever and comes with complete academic freedom. Overall, the system is a brilliant success, as an impressive proportion of the best known French mathematicians are supported by CNRS.

It was the great luck of my scientific life to have been offered such a position when I myself did not know whether I had any ability to do research. This set me on my life-long course of being a mathematician. Without the CNRS, I would have been offered a teaching position in one of the fanciest schools in Paris and would not have become a scientist.

I treasured my CNRS position my entire career until reaching the mandatory retirement age. I am very grateful to CNRS for having let me have all this fun and flexibility. I worked long and hard - but was also able to take long periods of recovery when needed.

Quite naturally, the freedom granted by CNRS had a price. It was challenging to bear the very many years it took for the scientific production to have any impact on the career. I never wavered in my determination to give priority to my research, but, sadly, some of the most brilliant people of my age in CNRS sacrificed some of their freedom and left.

Becoming a Mathematician

I had to leave Lyon to expand my mathematical horizons. Jean Braconnier asked me what I liked. The course I had enjoyed the most was measure theory, so I answered that I enjoyed splitting intervals into small pieces. Braconnier directed me to the group of Professor Choquet in Paris. I followed his seminar for months without having a clue about what was being said. My health problems made me a very insecure person, and I needed to get some results to feel that my position was secure. I then begged Professor Choquet for concrete research problems, which I set about solving. Some I was able to solve quite quickly. The immediate benefit is that it gave me the privilege to skip the long line of students wanting Professor Choquet's attention! I did not mind doing many small projects because it made me feel safer. Few mathematicians will ever admit to this, but over my career I have written many rather insignificant papers. These weren't wasted effort – often, what I learned in writing them proved to be a stepping stone to further significant discoveries. Besides, solving problems is fun. I remember Professor Choquet telling my father at the defense of my thesis in 1977 that I was a problem-solving machine.

I asked Professor Choquet what advice he would impart to a beginning researcher like me. He said ''Study each problem in the setting with the simplest structure where it makes sense". He added with a wink: It often helps to take a convex hull. I did not know at the time how well my future work would illustrate the usefulness of this advice. Much of my work has been done on simple structures, and one of my best discoveries involved a convex hull!

Professor Choquet was a great mathematician and an even greater human being, but the field of his mathematics was ending and not beginning. I sensed it, but still wasted a few years on fruitless directions. I tried my hand at a topic called "invariant means on groups." This had two results. First, my best, yearlong effort in this direction had exactly the following impact on mathematics: its review said "This is the most complicated paper ever written on invariant means." Second, Edmond Granirer, a specialist of the subject, invited me to spend the fall of 1978 at University of British Columbia in

Vancouver. I enjoyed the stay there, and the trip back changed my life as I explain below.

I also tried my hand at Banach spaces, since many people in Choquet's group were interested in this topic, and prominent people like Haskell Rosenthal often came to visit. As always, I worked very hard, but while I am not ashamed of my contributions to that theory, it is obvious that they are not stellar. It remains a mystery to me why one can be much better at certain topics than at others.

Fortunately, there were a great many visiting scholars to interact with, and these interactions were crucial. David Fremlin attracted me to measure theory. This was a unique experience and I never again collaborated so fruitfully with anybody. Measure theory led me very slowly to probability theory. My favorite work of the time is the characterization of Glivenko-Cantelli classes, the classes of functions on which the law of large numbers holds uniformly. I characterized them by a purely combinatorial condition. This equivalence of two properties of different nature seems to me as good a result as I ever proved. I doubted for many years that anybody at all had appreciated this theorem until I met Alexey Chervonenkis who had independently reproved it later in a sharper form and was very obviously sad not to have discovered it first.

A Fateful Stopover in Ohio

Coming back from Vancouver in 1979, I crossed the USA visiting the places where I had contacts. Joe Diestel in Kent, Ohio had appreciated the little things I had already done in Banach space theory. While I was in his office, Wansoo, a stupendous PhD student, brought the draft of her thesis. I was fascinated, so I arranged lunch with her. After having talked to her I felt madly in love, and remain so to this day. I proposed to her almost immediately. “You are crazy,” she said. But her father had convinced his children that scholarly knowledge is the supreme value of life (the four of them eventually earned PhDs). I could read in her eyes that the situation was not desperate. I had already learned to be persistent with difficult problems. I knew how fundamental this one was, and I spared no effort.

I had my closest brush with blindness in 1981. I had been obsessively concerned about my visual problems since my scare at age 15. After ten years, I was seeing flashes less frequently. I felt that this mental burden had been overwhelming, so I rejected the continuous fear in an ill-advised fashion—I stopped seeing an ophthalmologist. In the summer of 1981, to fight the despair that I had not succeeded yet in obtaining Wansoo’s long-sought consent, I traveled two months in India. While there, a pickpocket swiped my camera lenses and my prescription sunglasses from my bag. On my return I needed to consult an ophthalmologist to get a new prescription. She insisted on looking at my retina. It was on the verge of massively detaching. Without the pickpocket, and without her, I would have gone blind at that time, and there would have been little further mathematics, and no wonderful married life. Laser surgery had made such progress by that time that I had no further major worries in that area.

Just at that time, in 1981, Wansoo finally agreed to become my wife and life partner. She has since helped my work in a way which cannot be overestimated. I would certainly not have achieved what I did without her. She understood from the beginning how important mathematics are to me and did everything she could to protect my working time. Even more importantly, she kept her professional worries at work to protect my peace of mind so that I could focus on my research. She has been the perfect companion in my life and work.

Turning to Probability

My best-known contributions to probability theory concern the boundedness of stochastic processes and concentration of measure. I will be forever grateful to Gilles Pisier and Vitali Milman who introduced me to these topics.

Gilles Pisier joining our group in 1983 was a landmark event of my career. This event by itself had a low probability. Pisier would have preferred to go to Orsay (now Paris-Saclay University), but my great fortune is that Micha Gromov had applied the same year as he did. Piser had a far broader view of mathematics than I had at the time. He generously shared his ideas. And he brought to me a kind of mathematics which was much closer to my real abilities, although I did not yet know it at the time. Pisier was (besides a first rank analyst) an expert in probability in Banach Spaces. He lent me a set of unpublished notes he had written and this was a fantastic help to learn the topic, where I eventually did some of my best work.

Gaussian processes and aftermath.

Pisier also introduced me to the problem of characterizing the boundedness of Gaussian processes. Xavier Fernique had proved the best known upper bound, using the concept of majorizing measures. Mainstream probabilists considered this notion as exotic, and I myself found it extremely difficult to understand, but in retrospect there is no doubt that Fernique performed a major advance there. The question was whether this upper bound was also a lower bound (modulo a multiplicative constant of course). I believe that one should be humble, and that there was no prospect of success without first understanding majorizing measures. So I started investigating how they could be used to bound stochastic processes in a very general setting. It was a sensible step, but the success was marginal. Majorizing measures have a limited use in such a general setting, and worse, the fruitful way to use them in the Gaussian setting (which is the generic chaining that I discovered much later) is specific to that setting. Still this effort gained me at least some understanding.

I was discouraged for a while after my first attempt at the Gaussian problem itself in 1983, as the partial result (for ultrametric spaces) I obtained had already been proved by X. Fernique. At the second attempt in 1985, I cast about in wrong directions for a couple of months. I did not attempt the most direct approach to the problem (trying to show that any metric space contains an ultrametric subset of basically the same “size”) because I thought that *evidently* others must have tried and failed. Eventually I felt that I should understand better why this “obvious’’ approach did not work. But it did work, and I then solved the problem in a matter of days, proving that Fernique’s upper bound was of the optimal order. Piser told me “You are lucky” by which he meant that indeed I was lucky that such a clean result was true, and that my efforts did not result in (yet another) contorted counterexample.

This success was a big boost to my morale, and marked the beginning of a kind of magical period, which lasted about ten years, with sometimes more than one idea of lasting relevance every year.

In some sense the result on Gaussian processes meant that concerning them the mathematical universe is as simple as it can possibly be. So it was natural to hope that this would be the case for many other classes of processes. I stated some conjectures in that direction already in my 1990 book with Michel Ledoux. There were no methods at the time to approach them, it was just wishful thinking. The truly extraordinary thing is that they are all now proven.

My feeling was that to understand more general processes, the next step was to understand what I call Bernoulli processes, where the Gaussian random variables are replaced by weighted sums of random signs. At the theoretical level these are fundamental, even though Gaussian processes are far more important for applications. I made numerous advances while pursuing that goal, but I failed to reach it. For years I offered a $5000 prize for the solution of this question. It went in 2011 to W. Bednorz and R. Latała (after a key earlier contribution by Latała). My belief that these processes were important was based on philosophy, but it turned out to be absolutely correct, as the solution to this problem has been the key to all further advances.

Random Fourier series.

I must say a few words about this topic, which has been one of my life-long pursuits and obsessions, and which illustrates well the way I work. Given a Fourier series whose coefficients are independent symmetric random variables, when do the partial sums converge uniformly almost surely?

Big advances had been made on this problem by Marcus and Pisier, but somehow I felt that they did not go to the bottom of the question. I spent considerable time on it, time which was well spent, as then I discovered how to use families of distances to control non-gaussian processes, a method which has turned out to be a key tool for many questions. Gradually I got necessary and sufficient conditions in full generally, but I am certain that nobody at all read these results. In the end, after nearly 40 years of efforts, I was rewarded, I dared make the right conjecture, that the partial sums converge uniformly almost surely exactly when the Random Fourier series is a mixture of two different specific types for which this convergence is pretty obvious. It was not even very hard to prove.

Concentration of measure.

During the same period, at every conference on Banach Spaces I attended, Vitali Milman was expounding the idea of concentration of measure in the most enthusiastic way. At first, I found his obsession with this notion a little bit weird, but slowly I began to understand how deep his ideas were. In another fortunate coincidence, Vitali gave a seminar in Paris where I learned a basic "rearrangement'' method, well known in combinatorics. While learning these deep ideas I was working on probabilities in Banach Spaces, on the problem, famous at the time, of the law of the iterated logarithm. I started to wonder whether phenomena similar to concentration of measure could not exist in unsuspected directions. Generally speaking, concentration of measure is quantified by "concentration inequalities''. I lived the discovery of my first concentration inequality as a magical experience. The proof had seven steps, the discovery of each taking about a week. All this in a state of constant elation! The rearrangement idea I had learned from Vitali was crucial there. This first result was very important, because it showed that there was a whole new field of investigation. I soon discovered other directions and simpler methods. The formulation of the most important inequality required considering a certain convex hull, and it was much easier to invent after having received the specific advice of Gustave Choquet to that effect years earlier. It is sometimes called Talagrand's convexified inequality and is elementary enough to be taught at the master's level.

This inequality is responsible for one of the highlights of my career. Whenever I needed to go to the airport, I hired a taxi from a small business that did only airport transfers. One Sunday morning a few years ago, upon seeing my credit card the taxi driver asked "Are you the mathematician?'' He was no ordinary driver: he was the founder of the company, who had to do the unpleasant Sunday work himself. He had been taught the convexified inequality in business school!

First recognition.

My work on the boundedness of stochastic processes and concentration inequalities earned me the 1995 Loève prize. I could hardly believe my eyes when I received the announcement email. I had never heard of this prize, which was only in its second edition, nor had I ever dreamed that my work could be so well considered. This work also earned me the Fermat Prize in 1997, and was probably the deciding factor in my being invited as a plenary speaker at the International Congress of Mathematicians in Berlin in 1998. I must have stared at the invitation letter for a full five minutes after I opened it because yet again I had never dreamed of that. The best thing was that this letter came more than a year in advance, so I had plenty of time to relish the thought! But the most intense emotion of my mathematical life was when Gustave Choquet, sensing that his end was near, called me at home to tell me: Talagrand, you have been my best student.

Spin Glasses

Around 1995, my progress on probability theory had stalled and I looked for another related topic. I met Erwin Bolthausen at a conference, and he wrote the Hamiltonian of the Sherrington-Kirkpatrick (SK) model on a blackboard. The SK model is a model for disordered matter. The disorder is represented by independent Gaussian random variables in its Hamiltonian. As I understood Gaussian random variables very well, I had the illusion that I could easily get results about this model. By the time I understood this to be an illusion, I was already hooked. The SK model is a perfectly well-defined mathematical object, but at the time I started to study it there were basically no mathematically rigorous results about it or about similar models like the Hopfield Model. On the other hand, the famous physicist Giorgio Parisi had invented a complete description of the rather extraordinary behavior of these models, based on methods sounding like witchcraft to a mathematician. Physicists were convinced that ''new mathematics'' were needed to study these models because mathematicians had not been able to make progress on them. In retrospect I understand this belief. It is because mathematicians have been unable to make Quantum Field Theory rigorous, and the physicists believed they had a similar situation here.

I knew that some very good mathematicians had tried their hand at this problem, and that my attempt was quixotic. I took an extremely humble approach: just try to prove rigorously anything at all on as many related models I could. It was very difficult at first but gradually I started to find ways, very complicated at first, and then getting simpler. The key problem was to prove Parisi's formula for the free energy. Francisco Guerra made some fantastic contributions (which to this day I regret that I have not done myself) and had in particular proved that Parisi's formula was an upper bound for the true free energy, so the problem was to prove that Parisi's formula was also a lower bound. The key progress was the two-line observation that an upper bound on certain coupled systems would yield this lower bound. Probably I would not have observed this had I not spent many years playing with all these models. After this observation, everything followed by adaptation of known methods. Soon after, in a big conference, Giorgio Parisi honored me with the sentence "now we are sure about the solution.''

The Control Measure Problem and aftermath.

As I needed a rest from the SK model, in 2005 I thought that it was time to have another run at a well-known (but not central) problem called the control measure problem. I had studied this problem much earlier and had even written a quick paper about a very simple observation on it in 1980. The most remarkable feature of that paper is that I had (unwittingly) *stolen* it. A few years after having written it, I developed the unpleasant feeling that maybe the observation wasn't novel, but had instead been explained earlier to me by David Preiss. When I asked David about this, he had a big smile and confirmed that this was the case (although he had never said a word about it). This result had been stored in my subconscious and came out in my paper. Despite this, it was a good idea to revisit this problem as a whole, because this effort paid off. By 2005, two major new ideas had been invented relative to this problem's core question, and it took me only a few weeks to combine them to produce a beautiful *counterexample* to the control measure problem, putting to rest this 58-year-old question.

Fresh off that success, I was elated, so I decided to make a last desperate attempt to tackle my favorite conjecture, in combinatorics. After a year of intense effort, I had to give up. The remarkable thing is that I came up with a series of conjectures of increasing strength and that I could disprove none of them. I wish very much to see this problem solved before I pass away, as I think that possibly something very deep takes place there. It is described in the volume STOC'10—Proceedings of the 2010 ACM International Symposium on Theory of Computing. Sadly, I never found time to go back to this problem, so I am not even sure it is very hard.

My Style of Mathematics.

My style of studying mathematics is probably atypical. I just cannot get a feeling for a result until I have taken it apart in small pieces and digested these a thousand times. This very long process makes it nearly impossible for me to systematically learn an area. I have learned mathematics mostly by thinking about research problems introduced by others, until I could develop my own ideas. Yet, somehow, this approach guaranteed that I really mastered the basics, leading me to where I am today.

I will try to say a few words about how I approached problems, although I have no magic recipe to offer. One obvious strategy consists of studying special situations. One should not be afraid to start by fully understanding nearly trivial cases. A slightly less obvious strategy is to try to identify other problems which might be easier, but where the difficulty should be of a similar nature. Again, it helps to be humble and to start by fully understanding simple situations. When working on a conjecture I also found it helpful to alternatively try to prove it and try to disprove it. The progress comes by jumps, much like matching two pieces of a puzzle. This is nearly instantaneous. Now you see it, and the moment before you did not. After such progress, you may have a much clearer vision of the problem. Eventually you may see a road to your goal. Patience and technique are then indispensable, in the same way that mountain climbers cannot reach a summit without ropes and stoppers. Technique can be acquired only through hard work and dedication. If no progress comes after a while, the best strategy may then be to let the problem rest and turn to something else. I often made big progress on problems very soon after coming back to them.

Very often the solution of a problem needs not be far, in the sense that once it has been found, it will be easily understood by others. The whole difficulty is to look in the right direction, as there are so many possible such directions. One of my favorite discoveries is that of the generic chaining, a method to bound stochastic processes. It can be learned in ten minutes, but it took me 15 years to see it. And I saw it after coming back to the question, to which I had not thought about for 5 years. Preconception, which prevents us from looking in new directions, is the worst enemy of a research mathematician. This is also one reason why big advances are often made by younger researchers: the elder ones know too much.

The Shaw Prize and aftermath.

While checking my mail one day in May 2019, I was stunned to learn that I had been awarded the Shaw prize. I knew its existence, but the thought that I could receive it had never crossed my mind. When my son called me to congratulate me, he said “Dad, I knew you were a good mathematician, but not that good.” The truth is that I did not know it myself either.

Since I had devoted my life to mathematics, I decided to donate the award money to establish a prize in my favorite areas of mathematics. What I did not expect is how hard it is to donate money if you want the recipient to do something useful with it, but I was lucky to eventually find an organization which shared my views. The prize will be awarded every second year, starting in 2032 at the latest.

Having *real* fun at last, and aftermath.

When I reached sixty, I felt that the time for creativity was gone, and that I should enjoy learning the physics I wish I had learned as a student. I had great difficulty learning Quantum Field Theory for lack of a reader-friendly textbook written within mathematical formalism. So I took it upon myself to write the textbook I wish I had. That proved to be the hardest project of my scientific life, despite the fact that all its contents where well known fifty years ago.

I really did my best to explain this material which I barely understand. It then occurred to me that I had never put such an effort into explaining my own mathematics. So I started reworking the material of my 2014 probability book. Trying to better explain the material to others, I understood it better myself, reaching at places simplifications of almost embarrassing scale. Even more importantly, things fell into place and I could prove several of my conjectures dating back to the 1990 book as well as finding the final word on random Fourier series. I had thought about these matters for nearly 40 years. These results are available only in the new edition of the book, but I am very proud of them. Isn’t it miraculous to finish your life-time problems exactly on the very last chance attempt you make at them?

A small problem with arteries.

About 10 years ago it became clear that blood was not circulating well in my brain, so I had an MRI. It was quite an intense emotion when the radiologist told me that I had holes in my frontal lobes and had me rush to take low doses of aspirin. As I had not felt anything special, I kept living and working as usual. What else could be done? Early 2020, I fainted in my bathroom, had a bad fall, and a brain hemorrhage. The visible consequences were manageable, so again, what could I do but keep doing what was already started? The amazing thing is that I discovered the final word about random Fourier series after this hemorrhage.

The Abel Prize

I innocently clicked on a Zoom link, expecting an inconsequential chat, and I heard Gunn Elisabeth Birkelund saying that I had been awarded the Abel prize. My brain stopped functioning for a full five seconds, which I later diagnosed as the consequence of having been suddenly propelled into a parallel universe of probability zero.

The Abel Prize money has joined the Shaw Prize money, so the prize I created will be very significant. I am reasonably proud of having discovered a few inequalities, but what I am proudest of is, considering my family's origins, to have been able to donate all this money for the cause of mathematics.