Biography of 2019 Shaw Laureate 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

Our first and possibly most important influence in life is the environment in which we were born. For me, that starts with my ancestry.

All four of my grandparents were born in large, poor peasant families in South-East France. 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. He pursued higher education and became the equivalent of a college professor in mathematics, earning enough to live comfortably.

On my mother's side, beginnings were not much brighter. My 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.

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.

Early on, I had very little interest in studying, but 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 enough to pique my interest in the sciences.

I was less interested in the humanities and it showed. My spelling and grammatical skills were terrible. This was unfortunate, because at the time students were evaluated largely on their spelling skills. (The French language isn't famous for its simplicity). I could not spell, and therefore I was a bad student, and therefore I should not have been admitted to junior high school. But my father, being a teacher himself, went to see the principal, pulled some strings, and I was admitted. This nepotism came at a cost. While any competent student would be studying Latin at that age, I was an under qualified student, and was therefore enrolled in one of the few classes where it was not taught – these classes being reserved for the very worst students. Only two students from my initial class graduated high-school without repeating. Given that everyone in these programs had effectively given up, the math teachers were not much better than the students. As for myself, I remained a very average student. I may have had some passing interest in the sciences, 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.

My Health Changes my Life

I was born with a genetic disposition for 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.) Nonetheless, just before my 15th birthday I got a retinal detachment in the remaining eye. It was treated but recurred several times. I spent a long month in the hospital, wondering if I would go blind. 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 be able to progress in my studies despite my absence and to drop some language classes to catch up. 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 losing my eyesight. 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 much more competent in the last years of high-school classes than in the first ones, having obtained national certifications to land their positions. 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 ranked third nationally both in math and physics in the *Concours Général,* a French Olympiad for the top high school seniors. 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 the Shaw award, but he already had dementia- and passed away shortly after it was announced. Even worse, my younger sister has not known about it either, as she took her own life in 2017.

In France, the best tracks for higher education are taught in special schools (the *grandes écoles*), located in Paris and 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 was working 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. I then 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, and needed to spend months preparing to deliver the high-school lecture that is part of the competition. 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.

Becoming a Mathematician

Working at the CNRS set me on my life-long course of being a mathematician. In the meantime, I had ended up taking the *Agrégation* and was happy to come in first nationally. Without the CNRS, I would have been offered a teaching position in one of the fanciest schools in Paris and that would have set me on a path towards teaching secondary school. Instead, I treasured my CNRS position my entire career until reaching the mandatory retirement age. I could work in complete freedom. 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.

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 weeks without having a clue about what was being said. I then begged Professor Choquet for concrete research problems, which I set about solving. Some I was able to solve quite quickly. I did not mind doing many small projects because it made me feel like I was making progress. 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.

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. 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 was the characterization of Glivenko-Cantelli classes.

A Fateful Stopover in America

On my first trip to America, the golden hand of fate had it that I visited Joe Diestel in Kent, Ohio for a few days. While I was in his office, 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. It only took three years and many flights, including a long stay in Korea, to convince her that I was not.

Just before the end of these three years I had another close health call. I had been obsessively concerned about my visual problems since my scare at age 15. After ten years, I felt that this enormous weight was too much to bear, and I rejected the continuous fear in an ill-advised fashion -- I stopped seeing an ophthalmologist. After all, no news is good news. In the summer of 1981, I spent two months visiting India while pondering what I would do with my life if the woman of my dreams wasn’t interested. (I must admit that I never stopped working -- I had a cute mathematical idea while spending the night in a Hindu temple). While there, a pickpocket swiped my camera lenses and my prescription sun glasses 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 detaching. Without the pickpocket, and without her, I might have gone blind at that time, and there would have been little further mathematics. Laser surgery had made such progress by that time that I had no further major worries in that area.

In 1981, the woman of my dreams 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. She kept her work life at work and she never brought her professional baggage home, so that I would have peace of mind to 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 process 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. He 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 my best work. He also introduced me to the problem of characterizing boundedness of Gaussian processes. I was discouraged for a while after my first attempt in 1983, as the partial result I obtained was already known. 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 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. That was the first "big'' problem I solved, the starting point of my work on boundedness of stochastic processes, and the beginning of a kind of magical period, which lasted about ten years, with sometimes more than one idea of lasting relevance every year. Much of my work on the boundedness of stochastic processes was motivated by the desire to extend what I had done for Gaussian processes to 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).

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 phenomenon 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 what had been the highlight of my career before being awarded the Shaw prize. Whenever I need to go to the airport, I hire a taxi from a small business that does 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!

My work on boundedness of stochastic processes and concentration inequalities earned me the 1995 Loève prize. I could hardly believe my eyes when I received the announcement e-mail. 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. I eventually disproved this belief. It was very difficult at first to obtain any results at all. But gradually I started to find ways, very complicated at first, and then getting simpler. Francisco Guerra made some fantastic contributions (which to this day I regret that I have not done myself). These helped me after eight years of studying the subject to prove that a central part of Parisi's description was correct. Soon after, in a big conference, Giorgio Parisi honored me with the sentence "now we are sure about the solution.''

Later Years

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 *counter-example* 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. I wish very much to see this problem solved before I pass away, as I think that possibly something very deep takes place there. I describe it in the printed version of the Shaw conference.

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 and has taken me to where I am today.

I may then 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 understanding fully the 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 the summit of a mountain 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 as 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. 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.

Writing Books

A number of my ideas, including some I still like, unfortunately had little impact. To give them a chance not to be forgotten for good, I wrote a volume describing my contributions to the topic of boundedness of stochastic processes. While writing it, I discovered the final solution of the problem of convergence of random Fourier series, extending the classical results of Marcus and Pisier. I had worked so many years on that question. Even though current active research has moved away from this topic, I am really proud of having solved this problem. The book, "Upper and Lower Bounds for Stochastic processes'', appeared in 2014.

When I reached sixty, I thought it was time to *really* have fun and finally learn 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. I have yet to see the end of it.

While writing this book I realized that maybe I had not put enough pedagogical effort in my 2014 probability book. I reworked the material. Trying to better explain the material to others, I understood it better myself, reaching at places simplifications of almost embarrassing scale. I've found it fascinating that I can keep revisiting subjects I know well, and find new ways to express the ideas in simpler terms. The new edition of this book could be my last project, but who knows?