

Abel interview 2024: Michel Talagrand

Bjørn Ian Dundas and Christian F. Skau

Bjørn Ian Dundas/Christian F. Skau: *Professor Talagrand, first we want to congratulate you with being awarded the Abel Prize for 2024 for your “groundbreaking contributions to probability theory and functional analysis, with outstanding applications to mathematical physics and statistics.” You will receive the prize from the crown prince of Norway tomorrow.*

Michel Talagrand: Thank you.

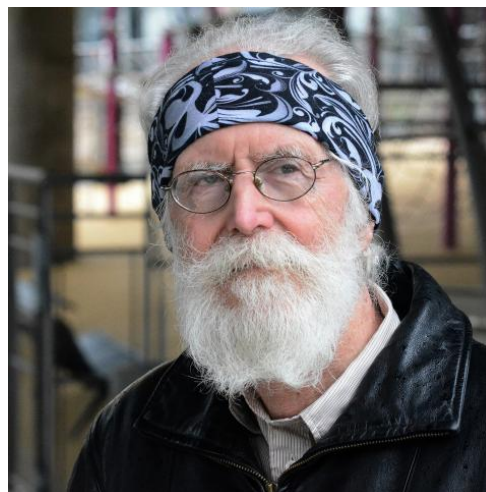
[BID/CFS]: *You’ve commented that you’ve had a career like a Brownian motion – a succession of decisive shocks – and let’s start at the beginning.*

You were born in 1952, and grew up in Lyon. Your grandparents came from large, poor families in South-Eastern France, but managed to give both your parents a good education. But even though your mother was a French language teacher, as a child you didn’t start out on your “improbable academic journey towards probability” as a great student. As far as we understand, you were ... well, your French spelling was horrible. How would you describe your childhood?

MT: It was a really very happy childhood. I grew up in an upper class neighbourhood. It was really nice, except that I had a little health problem. I lost my right eye at age five, and this was sort of looming over my life. I didn’t do sport at school. But I was not really handicapped.

Things were happy, essentially, until I got 15, and I was in high school. Then I got retinal detachment in my remaining eye. That was a very, very difficult period for me. Medically I survived because the damage was not great, but the psychological damage was really deep. I really lived in terror of becoming blind for years. To fight the terror I had to do something, and that something was to study mathematics, because my father was a maths professor and naturally attracted me there. And why mathematics? Because in all the rest I was very mediocre, including spelling. So it was an obvious choice.

[BID/CFS]: *But you did have a budding interest in that direction already?*



Michel Talagrand, Abel Prize laureate 2024.
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MT: I was interested in science from an early age, and again thanks to my father, who subscribed to a scientific magazine. Science was very much in the air, you know. That was the year of the Sputnik. The times were changing, nobody believed that Sputnik would come anytime soon. And then it came. We believed in unlimited progress. That was very naive, but that’s what we believed in and science was the natural place where things were happening.

[BID/CFS]: *Then you had this traumatic experience with your eye problem when you were 15, and your father helped you get through it.*

MT: My father helped me greatly. When I was in hospital he went to see me every day, and he talked mathematics to keep me busy and to give me a goal. It had a great effect. You see, somehow I could say that I’m sort of lucky because I had this trouble, which gave me some psychological impetus in a very positive direction. But on the other hand, my eye damage was not so bad. I could function properly. I could, okay, with some limitation, function, and

it was not an impairment in my life. While on the psychological side – in spite of the terror of becoming blind – it was a great positive, so overall I benefitted from that. I probably would not have become a mathematician, if I didn't have this health problem. I'm sure.

[BID/CFS]: *Part of that time your eyes were bandaged, right?*

MT: You know, medicine has made a lot of progress in treating retinal detachment, but at the time I had to stay in bed for months with the eyes bandaged. And my father came every day and talked to me. He explained to me integration by parts, you know, I remember that. And I felt so good, I could understand something. This is how I learned the power of abstraction.

[BID/CFS]: *You say you had very competent high school teachers.*

MT: I was in the best high school in Lyon, where my father was a professor. And the last two years there were remarkable high school teachers. The French system has a very demanding certification method for people who are teachers in the last two years of high school, and they get fantastically good results. My maths teachers in the last two years were both great, the physics teacher was great and the natural science teacher was great. It's really amazing how good these people were.

I can tell you a funny story. I was complaining to my maths teacher in the last year of high school that the exams were too easy. So she would add bonus questions just to keep me happy, essentially. And she encouraged me. And I felt good.

[BID/CFS]: *Last year of high school you participated in the Concours Général, a French Olympiad for top high-school seniors. How did that go?*

MT: I ranked third nationally, both in maths and physics. While I felt happy to win such an award, I didn't attach much importance to it. The French system is, I think, a greatly efficient system. But I could not really take advantage of it, because my parents felt it was too demanding considering my health problems. So I went to the university instead, which is not the royal road in the French education system. The best track for higher education in France is to attend special schools (the grandes écoles, like École Normale Supérieure in Paris). 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.

I went to the University of Lyon instead. And at the University of Lyon I was saved again by the high level of the lectures. I would even say too high, because it was too difficult for most of the students, but it saved me, because I could learn things in the proper way. In particular, I took a course in measure theory. I fell in love with measure theory, that was my first mathematical love, and it greatly influenced me during the rest of my life.

[BID/CFS]: *Where did you think your studies would lead you? We know you prepared for the Agrégation, the national competition by which higher secondary school teachers are selected in France. Did you have an idea where you wanted to go?*

MT: I had no idea what to do. Of course, I had to make a living, and I knew I had to make a living with the government. Because in case I became blind, they will find some spot for me. So the obvious way was to get the Agrégation, for which I prepared. I spent a lot of energy on that. I'm a dutiful person: when I start doing something I do it right. So I trained myself solving problems.

[BID/CFS]: *You did exceptionally well!*

MT: I did okay. Okay, I am still proud that my grade was 318 out of 320. That is an artificial grade, of course. It's just by comparison with others.

[BID/CFS]: *Then you got a position at CNRS. Perhaps you should first explain what CNRS is?*

MT: CNRS (Centre national de la recherche scientifique) is a research institute. When you get hired for CNRS, you have a position as a researcher for many years, and most of the time for your entire life. Essentially, you have no other duties than doing research. I'm very grateful for the French government to have given me one of these positions. There were no administrative duties, no teaching duties, you could devote yourself entirely to research. There was no control whatsoever, you study whatever you want. It's absolutely total freedom. And I benefitted immensely from that.

[BID/CFS]: *What role did Jean-Pierre Kahane play in you getting a position at CNRS?*

MT: Let me explain that to you. There are a number of positions every year, and my great luck was that in the year I applied, in 1974, this number was exceptionally large. If I had applied two years later, I would not have got the position. Since there were exceptionally many positions, they were also considering people who had not done any research yet, which was my case. The only thing I had in my favour was that my professors at the university were aware of the fact that they didn't very often get students like me. Four of them wrote a recommendation letter for me which reached the hiring committee.

One of the members of the hiring committee was J.-P. Kahane, a famous French mathematician. J.-P. Kahane was a very dutiful person. He wrote to me personally and asked me to please explain my situation: Why was I in the university and not in the grandes écoles system? Otherwise I wouldn't have a chance. So I described my health problem and why I went to the university. That I loved mathematics and wanted to do it. And I got hired!



Michel Talagrand receives the 2024 Abel Prize from His Royal Highness Crown Prince Haakon at the university aula, 21 May.
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I thanked J.-P. Kahane many years later, and he gave me this beautiful answer: “I just read your letter to the hiring committee, I didn’t do anything.” That’s what he said, and I greatly admire him for doing that.

A problem-solving machine

[BID/CFS]: *Then, after you got your CNRS position, you started on your Ph.D. study?*

MT: Yes, I did. My instinct told me that I shouldn’t stay in Lyon, it was no longer the right place for me. So I inquired where I should go, and I was “sent” to the group of professor Gustave Choquet in Paris, a very active group at that time. It ended up being a good place to be for me. Of course, it was very difficult in the beginning. I arrived there, I didn’t know any relevant mathematics, and I was not able to make any choice by myself. I tried to follow the seminars, in which I didn’t understand a word for a long time. Of course, as you begin you cannot read papers, because these refer to other papers that you don’t understand either. It’s difficult to get started.

I went to see professor Choquet and I said: “Please, I need to survive, pose me some problems that I can focus on.” He gave me some problems, and one of them I could solve pretty easily. Choquet himself had tried for a few days to solve this problem. So he gave me credit for solving it. After that I didn’t have to line up for one hour to talk to him in his office. That was a great privilege! And I got my Ph.D. in 1977, after three years of study.

[BID/CFS]: *We liked the comment that your Ph.D. advisor Gustave Choquet made to your father at the defence of your thesis. He said that you were “a problem-solving machine.”*

MT: That’s what he told my father, yes.

[BID/CFS]: *On this topic, you yourself have said that it is fun to solve problems.*

MT: But you see, this raises an important point. How do you choose the problem on which you work? First, it has to be in the right area, and then it has to be at the right level of difficulty. That’s the most delicate part. If it’s too easy, it’s an exercise, it doesn’t bring anything. If it’s too hard, you get discouraged, and you don’t have a chance. In my case, I was not in a position to choose a problem by myself, because I didn’t have any overview of mathematics.

So I asked other people what problems they had, and that was often successful. Some problems were sound. I remember one from that time: Take a Banach space and give it the weak topology and the norm topology, respectively. Do they have the same Borel sets? Okay, it’s reasonable to ask this question. Now, of course, as you expect, if you take a really large Banach space the answer will be no, but it requires some effort to prove that.

Okay, then you have a paper, but it doesn’t go anywhere. Many papers I wrote were like that, they didn’t go anywhere. But the thing is, I explored many areas, and then finally I hit some directions which were much more fruitful. I became interested in probability theory. It was measure theory that brought me to probability theory.

The first effort I did in probability theory was really thinking about vector-valued functions and what are the right notions of that? This brought me to the Glivenko–Cantelli problem, characterizing when the law of large numbers is uniformly valid on a family of functions. That again brought me to more central questions of probability theory. Furthermore, it was very important for me that Gilles Pisier joined our group in 1983. I will come back to that later.

[BID/CFS]: *What you’re telling us indicates strongly that you are more of a problem solver than a theory builder.*

MT: I am not a theory builder. I don’t have an overview of things, so I attack concrete problems, and if it works I try to push them. But I would say that the main feature of my work style is that I try to get a full understanding. I cannot stop until I feel I completely understand the problem. And that has turned out to be a fruitful way for me.

[BID/CFS]: *Choquet gave you advice on how you should attack a problem. Could you comment on that?*

MT: Okay, one of the first things I asked professor Choquet was to please advise me on how to do research. And he gave me the

most useful advice I ever got, which I called the Choquet principle, which was well known before him, but I received it from him.

When you attack a problem, always choose the proper setting for the problem. And the proper setting is that it should have no extra structure, it should be the minimal structure where the problem makes sense.

The obvious reason for that is that you don't get distracted by things which are not relevant for the problem. I have applied that advice several times, with great success I would say.

[BID/CFS]: *You go even further than that. We have the impression that when you attack a problem, you exhaust the problem, you really tear it apart.*

MT: I cannot understand things halfway. I try to understand things in depth, but that's a very slow process. For instance, I tried to read Feller's book on probability theory. I read the book, and I put it on the shelf. And one month later, I didn't remember a word because I didn't go deep enough into it; I didn't chew it long enough. Having to chew things over and over again is a time-consuming process, of course, so often I have to ask people who have an overview about things.

[BID/CFS]: *On the other hand, when you gather all this experience from doing all these calculations, you must yourself gain some overview of the subject at hand?*

MT: To get familiar with an object you have to play with it. Now, when the object is too complicated, you play with a simplified version, which you believe has some of the characteristics of what really interests you. If you are lucky, or if you look in the right direction, you end up making some simple computation which actually reflects some part of the general phenomenon. And if you can do that it orients you, of course. I spent a lot of time doing that, trying to look for a direction and understanding the basic mechanism.

The most important event

[BID/CFS]: *In 1978, you went to Vancouver.*

MT: I had done some work on a topic called invariant means. It was sort of a failed attempt, because it didn't go very far. But I got invited to spend the fall of 1978 at the University of British Columbia in Vancouver, and I had a great time there. Coming back from Vancouver, I decided to make a stopover at places where I knew people. One such place was Kent, Ohio, where I had the most important event of my life.

[BID/CFS]: *Tell us!*



Talagrand at the reception following the ceremony.
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MT: I met my future wife, Wansoo Rhee, and that was an extraordinary piece of luck. She has supported my mathematical work in a way which cannot be overestimated.

[BID/CFS]: *Wansoo Rhee was a student at the time, right?*

MT: She was a Ph.D. student. There were several people on the doctoral committee, including Joseph Diestel, who was a very respected person in Banach space theory, and who was the person who had invited me. I was in the office of Diestel discussing mathematics when she came in bringing a copy of her thesis. And I looked at her, and basically that was it! She was a student in probability theory, which I didn't know anything about at the time.

Apparently, Diestel said good words about me to Wansoo. She got somewhat interested, also because her father, who was a very prominent scholar in South Korea, had taught his children that the only real value in life is scholarly knowledge. For a mathematician to find a wife who has been told that as a child, this is an absolute miracle. She always totally respected my work, and I wouldn't be here without her constant support.

[BID/CFS]: *When did you get married?*

MT: After some years of wooing, Wansoo agreed to become my wife in 1981.

[BID/CFS]: *That same year you had your closest brush with blindness. Could you tell us about this dramatic event?*

MT: I lived in terror, and terror is the right word, to become blind. At some point I felt that it was too much, and I should free myself from that. And I did something, which in retrospect is absolutely stupid. I stopped seeing an ophthalmologist because no symptoms

had appeared for a time. In 1981, I took a trip to India. You see, I could function even though I was a little bit handicapped visually; I could travel by myself. At a train station in India, somebody took a razor blade and cut off the bottom of my bag I was carrying and got all my camera lenses, as well as my sunglasses.

I cannot function without sunglasses, so, on my return to France I went to see my ophthalmologist and asked her to please give me a prescription for sunglasses. She looked at my right eye and said: "Wait, what is your problem?" When I explained it she said: "You are crazy, you must check up your retina." So I made an appointment to check up my retina, and my retina was on the verge of detaching.

If I had not seen her, I would have become blind within a few months. And that would have been it. I was really saved by these very specific circumstances. This ophthalmologist was really a great person, because she gave me emergency laser surgery the same day the whole hospital was on strike. She had to save me, she said, strike or no strike, this goes first. You see, there are people like that. And I'm very grateful that I met someone like that.

Just after that, I finally got a phone call telling me that Wansoo would marry me. So that was fantastic!

[BID/CFS]: *She was then in Ohio?*

MT: She was at that time in Ohio, and she called me from Ohio.

[BID/CFS]: *And you were married the same year?*

MT: Yes, we were married as fast as we could!

Road to probability

[BID/CFS]: *Getting back to mathematics, could you tell us in more detail how measure theory, which you described as you first love in mathematics, led you to probability theory. How would you describe this transition?*

MT: Okay, there was this intermediate step, which was the Glivenko–Cantelli problem mentioned above, characterizing the classes of functions on which the law of large numbers holds uniformly. This is half measure theory, because you don't really have any complicated concept of probability, just the law of large numbers. You know, it's so simple. And then it's largely part of measure theory, because you have to work with the combinatorics of the classes of functions. There is no Brownian motion, no stochastic integral, this is very basic.

Then what happened was that I became aware of this field called probability in Banach space, which had already been developed for some time. What people were trying to do in probability in Banach space was that, instead of looking at probability in two-

or three-dimensional space, they looked at probability in higher-dimensional spaces. One was trying to copy what had been done in small dimensions. And it came exactly at the right time, because that's the time when high dimensionality starts to be important in mathematics, and it had not been explored before.

So there was this entire field – I didn't want to say it was waiting for me – but it had not been explored enough in depth. I arrived just at the right moment where this was gaining importance, and there were still many things to discover. The transition was through probability in Banach space, and then Gilles Pisier asked me about the suprema of Gaussian processes, which was a rather well-known problem in that area. And it was easy, having gone through probability in Banach space, to connect to that field.

You know, the world of mathematics has changed. Probability was not considered as mathematics when I was a student. In Lyon there was no graduate course in probability. In fact, I didn't take any course in probability ever, which somehow is very positive, because I was not formed by the standard way of thinking about these things. I could have a different approach.

Boundedness of Gaussian processes

[BID/CFS]: *Your solution in 1985 to the problem of characterizing boundedness of Gaussian processes was arguably your first "big" result in probability theory. This was your starting point for studying boundedness of stochastic processes in more generality, including the so-called Bernoulli processes.*

You mentioned in one of your books the practical relevance of some of this, namely if your cellar is in danger of being filled with water, what is the likelihood of that happening?

MT: All around us now we have models. We use everywhere models of stochastic processes. Now, the illustration I gave is because I had a beautiful house just beside a river. When I bought the house I didn't know that the river sometimes swells and gets into the house. I do wish it had been only the cellar, but several times we got four feet of water in the first level of the house, where our offices were located.

The level of the river is modelled as a stochastic process, because it depends on so many elements. The best way to understand it is to think of it as random. You want to know the maximum of the process, which in this concrete case means whether the river will visit you or not.

This is a very important criterion, and it applies in so many situations; for example, if you study the stock market. As for boundedness of Gaussian processes, I gave a complete understanding of that in some sense. There are two ingredients, one of probabilistic nature, and the other one of geometric nature. Indeed, the index set is naturally a metric space, and you have to deal with its geometry.

Here I should make an interesting comment: this metric space is not *any* metric space, it is a very special metric space, because it's a subset of a Hilbert space. But if you try to use the fact that it's a subset of a Hilbert space, you are dead! The successful construction is to forget that completely and think of it as an abstract metric space. Make general arguments true in any metric space. That's the key to success: forget about what is irrelevant. Of course, if it doesn't work, you can always come back to the specific features of your problem. But first forget about it. In my case this approach was greatly successful.

[BID/CFS]: *Which role did Xavier Fernique play in your solution to the problem of characterizing boundedness of Gaussian processes?*

MT: Mathematicians often mimic what has been done before. There was a standard method to bound stochastic processes, but that method will not give the right result for Gaussian processes. The person who really made the breakthrough was Fernique. He found a better way to bound Gaussian processes. It was very messy and very difficult to understand, but that doesn't matter. What matters is that he made the breakthrough.

Fernique's bound had the chance to be the correct one, while the other bounds had no chance – there were very simple counterexamples. I proved that Fernique's bound was actually of the correct order, it could be reversed, modulo the universal constant which gives you the right order of the maximum of the Gaussian process. The key step was a construction in an abstract metric space, which, after you succeed in doing it, makes you really wonder why you didn't do it before.

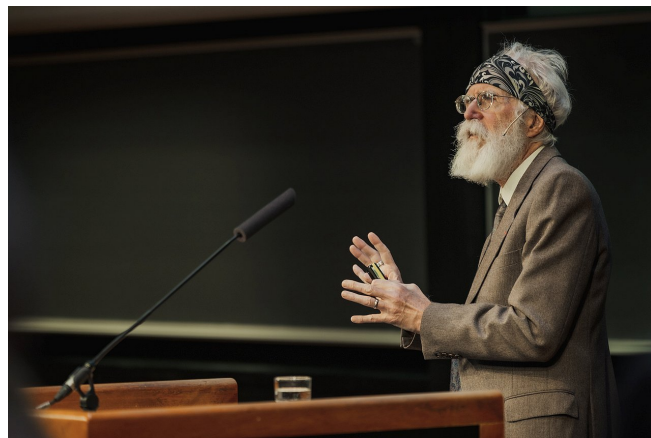
[BID/CFS]: *At the time you proved the bound on Gaussian processes, you considered this your best result, right?*

MT: It was the best known problem that I had solved up to that time. People in probability were well aware of that problem, which you could argue goes back to Kolmogorov.

[BID/CFS]: *Could you explain the concept "chaining," in particular, "generic chaining," and its significance?*

MT: The way I initially saw this was through Fernique's concept of majorizing measure. It took me a very long time to understand that these complicated things that Fernique was doing, could in fact be done in a very simple way with a slight change of perspective.

You see, this is the beauty of mathematics. You have something very complicated, and you make a slight change of perspective, and it becomes very simple. The slight change of perspective took me 15 years! And after you found it, you say: *Boy, why didn't I do that in 10 minutes?*, which is the time it takes to learn the stuff. But



Talagrand lectures at the Abel Symposium.
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that's not the way it works. In fact, preconception, which prevents us from looking in new directions, is the worst enemy of a research mathematician.

Generic chaining is really a method to bound a stochastic process. Kolmogorov was doing that by having successive approximations, which were getting more and more complex, and studying what happens when you go from one approximation to the next one; really keeping very closely track of what happens. The generic chaining does exactly the same, but the successive approximations are defined in a more flexible way.

When I solved the problem for Gaussian processes, of course, I was young and full of hope. So I say, this result about Gaussian processes tells you really – if you think about it philosophically – that the universe is as simple as it can be concerning Gaussian processes, in the sense that you can have a complete understanding of the thing just by looking at the structure of a certain metric space. It's an absolute miracle!

And I thought, why wouldn't there be more miracles like that? So I made a series of conjectures about all kinds of processes, which would essentially say that if you do the chaining the most clever way, you will really extract all essentials of the problem. I made these conjectures before 1990, and incredibly, they are all solved now in a positive way. So, concerning stochastic processes, from my point of view the universe is as nice and as simple as it could be. This is fabulous. And of course, I was lucky that it was that way. Otherwise, I would have worked for nothing.

Concentration of measure

[BID/CFS]: *Let us move to what is called the concentration of measure phenomenon. You yourself have said that your discovery of the first concentration inequality was a magical experience. Let's start*

with the classical two-dimensional isoperimetric inequality, which states that if we have a closed string of length L , then $L^2 \geq 4\pi A$, where A is the area that the string encloses in the plane. We have equality if and only if we have a circle. How is this isoperimetric problem related to concentration of measure?

MT: Okay, I will try to explain that. The example you mentioned was of course known to the ancient Greeks. So it took some time. This one takes place in the plane, while the work I'm doing is in high dimensions.

So, I have to move to, let's say, the Euclidean space of n dimensions. Let me state a simple problem very much related. You take a body of a given volume in an n -dimensional space, and you look at the set of points in the whole space which are within distance t of that body. How small can the volume of that set be? There is an extreme situation which occurs when your original body is a ball. Then you can compute everything, since the set within the given distance is another ball. That's the extremal case. But in that setting, you don't yet see concentration of measure. To see concentration of measure, you have to move to the sphere. So you take the sphere of radius \sqrt{n} (the proper normalization) in the n -dimensional Euclidean space. It's something we don't really visualize.

This phenomenon is precisely described by the famous Lévy's isoperimetric inequality, which he proved in 1917. It is remarkable how long it took for the consequences of this discovery to develop. The breakthrough was made by a probabilist, but it took well over 50 years to really influence probability.

Let me give a simple example: on the sphere you take a set of measure one half. It contains half of the points of the sphere. Now you look at the set X of points on the sphere within distance t of that set. What happens is that the set X is almost all the sphere. In fact, the points that are not within distance t , that is, the complement of X , their measure is at most $e^{-t^2/2}$. So it decreases exponentially fast, and it's independent of the dimension of the sphere. That's what is important here.

A consequence is that if you have a well-behaved function on the sphere, say a Lipschitz function, then it has a great tendency to be nearly constant. Now nearly constant means that its fluctuation will be the same however large the sphere is. That's actually a very deep generalization of the law of large numbers. I learned that idea from Vitaly Milman, and Milman and Michael Gromov worked on that. They developed the phenomenon of concentration of measure, which is sort of a generalization of what happens for the sphere.

[BID/CFS]: Did Paul Lévy see that?

MT: No, Lévy didn't see that. He died in '71. Now the sphere, that is geometry, and geometry isn't my field. My field is product spaces. Products of different measure spaces, that's what I was

thinking about. It was known that you have some phenomenon of concentration of measure in a product space. In fact, if you take a subset A of the product which contains at least half of the points, essentially every point x in the product is close to A in the sense that one can find a point y in A which differs from x on at most about \sqrt{n} coordinates. In fact, on specific examples one sees that much more is true. Namely, denoting by $I(y)$ the set of coordinates where x and y differ, y may vary over A such that the sets $I(y)$, while remaining of cardinality about \sqrt{n} , are widely different. The convexified inequality which I discuss below captures this phenomenon and quantifies it.

However, I was motivated by some rather special problems of probability in Banach space. I wondered whether you could have some other notions of closeness which would exhibit the same phenomenon. My great luck was that there were and that nobody had looked at them with the proper point of view. With the proper point of view it was actually not that difficult to discover this. So, you have this general statement that if in the product space you take a set large enough that it contains half of the points, then in various senses of closeness, most of the points are close to that set.

This has great many applications to probability, because there are so many probabilistic objects that are built on independent random variables. The remarkable thing about that is that you could argue that this comes from isoperimetric ideas, but the isoperimetry has disappeared! There is nothing like a boundary of a set, and what remains is only the concentration of measure phenomenon. I stress again that the important consequence is the concentration of measure.

I express my gratitude to Vitaly Milman, because he's the one who convinced me that that was the important idea. I must admit that the first or the second time I saw him talking of that, I thought: this guy is obsessed. What is the point of all that? No, he was a person who had a vision! Now, having the vision and being able to prove technically the thing, there is some distance. But you cannot do without a vision, and he was the one who brought that to me.

[BID/CFS]: Out of this came what is called the Talagrand convexified inequality, right?

MT: In the convexified inequality, to measure the "distance" of a point x of a product of n spaces to a subset A of this product, you take a suitable convex hull. More precisely, to each point y of A you associate a sequence of length n where the i th term is zero if the corresponding coordinates of y and x coincide, and is 1 otherwise. Then the distance of x to A is the Euclidean distance from the origin to the convex hull of this set of sequences as y varies in A . The main conclusion of the inequality is that if A contains at least half of the points, the size of the set of points at distance $\geq t$ from A is very small (like a Gaussian tail) independently of n .

There is a kind of miracle that happens. It is really a kind of abstract version of the law of large numbers which has a great many consequences. For instance, when you study combinatorial optimization you will take some random data and try to make a construction. For example, you take n random points in the unit square, and you look at the shortest path through them. Now this is a random variable. What is the fluctuation of this random variable? It turns out that the convexified inequality gives you bounds that are of the correct order.

[BID/CFS]: *And the hint about taking convex hulls came from Choquet?*

MT: Yes. Now this is unbelievable! When I asked Choquet for his advice, he explained to me the Choquet principle, which we talked about earlier. Then he said: "It's often useful to take a convex hull," which is what I did. And then he said: "It also helps very much to consider products." Now I spent my life working with product measures. I don't know if you believe in coincidences, but the funny thing is that the three advices I got from Choquet turned out to be relevant in my most successful research.

[BID/CFS]: *You have to tell us the taxi driver story!*

MT: I used to have a small taxi company to take me from my place to the airport. One Sunday I went to the airport with them. And when I gave my credit card to the taxi driver, he looked at my name and asked: "Oh, are you the mathematician?" The explanation for that was that he was the boss of the company, and he had learned about this inequality in business school. So it was considered simple enough so that it could be presented at a high level probability course in business school. Okay, being recognized by a taxi driver, that's something to be cherished!

[BID/CFS]: *Do you consider this to be your best result?*

MT: It's surely the most popular, and the one most people will learn. It's probably the one which will be referred to the longest. A host of examples shows that it captures at a deep level something which seems nearly optimal in many circumstances. So I'm glad I proved this result.

[BID/CFS]: *Terence Tao commented on the Talagrand convexified inequality, saying the following: "Talagrand's inequality implies that convex Lipschitz functions of Bernoulli variables concentrate as if they were Gaussian." Does that summarize what the Talagrand inequality says?*

MT: The inequality is fairly more general than that, but this is one of the most striking consequences. Terence Tao must have liked this inequality, since he reproduced the proof on his blog.



Talagrand lays down the wreath at the Abel monument, Oslo.
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Spin glasses

[BID/CFS]: *Let's move on to another big result that you proved, this time with connections to physics. How and when did you become acquainted with the spin glass phenomenon in physics and, in particular, the so-called Parisi formula?*

MT: At a conference I met Erwin Bolthausen, who is a well-respected probabilist, and who was interested in applied things. He wrote the Hamiltonian of the Sherrington–Kirkpatrick model on a blackboard. The Sherrington–Kirkpatrick model is a model for disordered matter. The interaction between any two given atoms is given by a Gaussian random variable. So, you have a bunch of independent Gaussian random variables, and I foolishly thought that I understood this better than anybody else, so that I would be able to make progress on that model. By the time I understood that this was an absolutely stupid illusion, I was already hooked by the problem.

This program was a big challenge in the sense that one deals with a perfectly well defined mathematical object. And the physicists were studying it by methods which, okay, I won't qualify it, but you consider n copies of the system, and you compute a quantity depending on n , and then you take n to be a negative number. Then you draw some conclusion from that formula.

[BID/CFS]: *That's what you call witchcraft, right?*

MT: That's what I call witchcraft. But this is not a coincidence! This cannot be a coincidence, there must be some deeper reason. It's like Euler's formula $1 + 2 + 3 + \dots = -1/12$, which occurs in connection with Riemann's zeta function. It's this kind of formula, there must be something which remains to be understood in the future. But there was this challenge to have this mathematical

theory being studied by methods from physics. How can you do it by purely mathematical means?

The challenge was well known, and I got this problem at a period of my life where I was stuck on stochastic processes. I was stuck on everything I wanted to do. So, I said to myself: "Why not, there is nothing to lose by trying."

[BID/CFS]: *And this was in 1995?*

MT: This is about 1995, when I got stuck because I couldn't solve what I called the Bernoulli conjecture, which incidentally was solved by Witold Bednorz and Rafał Łatała in 2011. And so I proceeded in a very humble way ...

[BID/CFS]: *As you always do!*

MT: I always say, you have to start from the beginning. If you don't understand the simple things, you won't solve the difficult ones. It sounds so obvious. So I ask myself, what's the minimum challenge? Prove anything at all on any model at all. That's low enough. Then I explored a number of models, starting from the bottom, and there was basically no mathematics which had been done on that. I worked really hard for a very long time and without much hope of success, you know.

After about eight years of very hard work I was lucky. You could summarize it in a three-line observation, which looked completely trivial. You had to find the lower bound for a certain quantity. The observation was that you take two similar quantities that you couple in a certain way, and if you can find an upper bound for that it will give you the lower bound you look for. That completely changed the problem, because there are methods to find upper bounds that Francisco Guerra had developed, and these methods could be applied with a small variation.

Thus, I was able to prove Parisi's formula for the free energy. You had to make this crucial little observation, which I made by exploring and exploring, without much hope, in fact.

[BID/CFS]: *Is it correct to say that you didn't invent new mathematics to prove the Parisi formula?*

MT: No, I didn't. The mathematicians couldn't solve that, and the reason they couldn't – and many good people tried – was because they were not humble enough. They thought they were going to solve that in two weeks. No, that's not the case. They should have started from the beginning.

The physicists thought that new mathematics was needed. Now, I understand why, because it's about quantum field theory. Quantum field theory is something that the mathematician has not been able to fix, and it's clear that new mathematics are needed for that. The physicists thought – not unreasonably – that "new mathematics" would also be needed to prove the Parisi formula,

but no, "old mathematics" worked very well for that, and I'm very happy I did that.

But I wouldn't say this is an important result. It is nice to have removed that "thorn" in the flesh of mathematicians, you know, that they couldn't duplicate the physicists' work. But mean-field models like the Sherrington–Kirkpatrick model are not really important. What would be important is to understand real models for spin glasses, like the Edwards–Anderson model, and these nobody knows how to handle, including the physicists, who even don't agree on what the solution is.

[BID/CFS]: *Proving the Parisi formula was an important ingredient in you being awarded the Shaw Prize in 2019, and Giorgio Parisi was awarded the Nobel Prize in physics in 2021.*

MT: Parisi is such a nice person. He gave a lecture at Institut Henri Poincaré, very soon after my solution, and he said: "Now we are sure that the solution is correct." Of course, every physicist was absolutely convinced that Parisi had found the right solution, but he was gentleman enough to acknowledge my contribution, which was so nice to hear.

Revisiting old problems

[BID/CFS]: *And then in 2005, you had had enough, so to say, of the Sherrington–Kirkpatrick model, and you wanted to do something else. You went back to the control measure problem. Tell us about that.*

MT: There is actually a fruitful stage in your life when you have nothing to lose, and only to gain, because you can make bets, like the bet I made for spin glasses in physics, which miraculously worked out. So I say, okay, why don't I go back to one of the problems of my youth, which was called the control measure problem, and which I couldn't solve? You can argue that the problem goes back to von Neumann, or at least to Dorothy Maharam, who introduced the concept called Maharam algebra. This problem was 58 years old, and many people had attacked it, without success, including me in my youth.

By 2005, two major new ideas had been invented (by Jim Roberts and Ilijas Farah) relative to this problem's core question. It took me only a few weeks to combine the two to produce a beautiful counterexample to the control measure problem. Technically, what I did was to construct a Maharam algebra that is not a measure algebra. This means that there exist measure algebras which are complicated and do things which you wouldn't expect. And maybe one day they will be used for something.

[BID/CFS]: *But you also revisited random Fourier series?*



Talagrand signs the protocol at the Norwegian Academy of Science and Letters. © Eirik Furu Baardsen /DNVA

MT: Fourier series are my obsession! I started being interested in random Fourier series when I wrote the book with Michel Ledoux, titled: “Probability in Banach spaces.” Random Fourier series certainly belong to that area. Great advances were made by Michael Marcus and Gilles Pisier. I read their paper, and found an alternate proof, which, from my point of view, is somewhat simpler.

However, I felt I didn’t fully understand the topic, and so I kept thinking about it, even though nobody was interested. But it was a good idea to do so, because when you try to study general stochastic processes, there are different difficulties which merge together at the same time. There are difficulties coming from the fact that your random variables are complicated, and there are difficulties coming from the fact that somehow your index space is not homogeneous, so that the same thing does not happen everywhere. You have to take this into account.

For random Fourier series one of the difficulties disappears, because there is some homogeneity over the whole space. So you get half of the difficulty away and can concentrate on the other half.

This was important, because several of the ideas which turned out to be very fruitful later on in studying general stochastic processes I discovered by studying random Fourier series. So it was a great instrument of discovery.

Marcus and Pisier had some conditions which I tried to remove. I tried to understand this in full generality, and the nice reward I got at the very end was that I dared make the right conjecture. This conjecture essentially says that the partial sums converge uniformly almost surely exactly when the random Fourier series is a mixture of two different specific types, for which the convergence is pretty obvious. So nature is as simple as it can be. It looks complicated, but that’s because you don’t know how to look. If you know how to look, there is a mixture of two very simple cases, which, notably, converge for entirely different reasons.

[BID/CFS]: *That must have been a very exciting discovery?*

MT: It was. I was almost 70 at the time, and I had had a brain haemorrhage six months earlier!

On old mathematicians and on writing books

[BID/CFS]: *And now we’re at the stage where you actually are revisiting many of your earlier ideas, and actually are improving on them. You rewrote one of your books, for example.*

MT: It is difficult, you know: how do you have a constructive end of your career when you are a mathematician? My own belief is that it is very dangerous to keep working on the same topics, because the odds are against that you will make great progress. So, I applied that philosophy in which I believed, and stopped doing research when I was around 58 years of age.

I tried to have fun, and to have fun was learning physics, which I didn’t learn at college. I ran into quantum field theory – the rest was easy. Well, you cannot say that general relativity is easy, but it is a purely mathematical theory. So, if you’re a mathematician and work at it, you will understand it very well. Now, quantum field theory is not mathematical, and there is no place where it can be learned easily. The work done by physicists is a different world, different way of thinking, different language and not the same idea of what an argument is.

I worked as much on that topic as I did on spin glasses. Of course, I didn’t contribute anything, but I explained a little bit of the theory to myself and hopefully to other people. I spent ten years doing that, and actually, I’m revising this book now, which will almost certainly be my last piece of work. The title of the book is “What is a Quantum Field Theory?”, with subtitle “A First Introduction for Mathematicians.” I never worked as hard to explain things like I did in this book.

[BID/CFS]: *You have written six voluminous books, one of them with a co-author. That must have taken a tremendous amount of time?*

MT: I’ve seen a picture of Donald Knuth, sitting in front of the books he wrote. There is an entire bookshelf, you know. So what are six books? Just a few years of work, right? Actually, except for the book with Michel Ledoux and the last two, they are just huge papers in the sense that I collected the results and unified the notation, but did not work as hard as I should have on the quality of the exposition.

[BID/CFS]: *You offered a prize of \$5000 for the solution of questions surrounding what you call Bernoulli processes. Could you tell us about that?*

MT: I certainly can. You see, after doing the characterization of Gaussian processes, you ask yourself, what's the next most important process which you should try to understand? What is the most basic random variable? It is the coin-flipping random variable. So the random variable takes value one or minus one with equal probability, it cannot be simpler than that. Now you take linear combinations of these, and you get a new random variable. Varying the linear combinations, you get a family of random variables, a stochastic process, and you have to understand the supremum of this process. So, based on purely philosophical grounds, you know this is the simplest possible situation, therefore it must be fundamental. That's really the thing. I saw that this is a fundamental problem, and I worked very hard on it.

However, I couldn't solve it, and to make it known that I considered it to be important, I offered \$5000 for the solution. This is another happy story: it was solved by two Polish mathematicians, Rafał Łatała and Witold Bednorz. Łatała essentially started on this problem when he was a student. He thought about it for 20 years before he solved it. The \$5000 prize money was well deserved for the solution of this problem. I was happy to pay them. They actually went hiking with that!

What is interesting is that the philosophical consideration that this was very simple, and therefore fundamental, was absolutely true. I mentioned earlier that all the conjectures from around 1990 were solved. The key ingredient is, as I had guessed, the understanding of Bernoulli processes. When I stressed the importance of this result, I did not know how it would be used, and it did take some time to figure this out. But philosophy turned out to be a faithful guide here.

[BID/CFS]: *The prize money of \$5000 that you paid out, that was a spin-off of your Shaw Prize money, right?*

MT: Oh, no, no, that was my own money! This was in 2011, while I was awarded the Shaw Prize in 2019. I didn't tell my wife that I was offering this money. I didn't want to be upbraided!

[BID/CFS]: *We wouldn't tell our wives, either!*

MT: With the Shaw Prize money, and now also with the Abel Prize money, I will establish a significant mathematical prize, starting in 2032, or before if I die. It's a mathematical prize not only in the areas where I work, but in areas where I understand enough to have a great respect for the work which is being done. These include functional analysis, probability theory, theoretical foundations of computer science and combinatorics. I have a special taste for combinatorics because many of my work have some combinatorial component in them.

[BID/CFS]: *G. F. Hardy, in his book "A Mathematician's Apology," said the following: "Mathematics is a young man's game." He*

also said: "A mathematician may still be competent enough at 60, but it is useless to expect him to have original ideas." Now we compare that with what you wrote in your autobiography: "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 older ones know too much." Comment please!

MT: I had Hardy's book on my desk in the office I was sharing as a student. So I read his book when I was younger, and I was greatly influenced by what he said. I was also greatly influenced by professor Choquet, who one day asked me: "Talagrand, how old are you?" I answered: "Sir, I'm 29" and he said: "You have to hurry up, you have exactly one year to prove something important." That really was traumatic, even though he said it half jokingly.

On the other hand, I witnessed the most magic experience of a mathematician: you get this idea in your mind, and you feel that it wasn't there before. It's something which is a new step, and it's marvellous enough that I could identify that clearly happening to me. During the magic mathematical period of my life from 1985 to 1995, it occurred approximately every six months. And then it became more and more rare. And then it completely stopped. At least the feeling I had completely stopped. It didn't mean that I didn't have some small ideas. But I got the message that it's time to stop. I'm not going anywhere. That's the way I felt.

On the other hand, this discovery I mentioned about the convergence of random Fourier series, which I did when I was almost 70 years old, seems to contradict that. So it's complicated.

[BID/CFS]: *Do you have any special interest besides mathematics?*

MT: Oh, I am very mediocre at anything else than mathematics. I've entirely focused my life on mathematics.

[BID/CFS]: *But you did run a marathon once?*

MT: Okay, that was to compensate from having been excluded to do any sport because of my eyes. I had to explore my own natural energy and I discovered that if you're a normal person and spend six months training for a marathon, you can do it. It's a very interesting experience. I recommend it to everybody as part of life.

[BID/CFS]: *And you've travelled widely, haven't you?*

MT: That is a family interest. We have to do things together to work as a family. I am also a cultural animal. I spend my life in museums, and I greatly enjoyed one in Oslo.

[BID/CFS]: *Well, on behalf of the Norwegian Mathematical Society, the European Mathematical Society and both of us, we would thank you for this most interesting interview.*



Christian Skau, Bjørn Ian Dundas, Michel Talagrand.
Photograph by Erlend Gjertsen, Gyro A/S

MT: I would like to thank the Norwegian people for celebrating mathematics the way they do. The way they put mathematics forward is praiseworthy, and mathematicians are greatly honoured and thankful for that.

[BID/CFS]: *Nice to hear. Thank you!*

Bjørn Ian Dundas is a professor of mathematics at the University of Bergen, Norway. His research interests are within algebraic K -theory, homotopy type theory and algebraic topology.

dundas@math.uib.no

Christian F. Skau is a professor emeritus of mathematics at the Norwegian University of Science and Technology (NTNU) at Trondheim. His research interests are within C^* -algebras and their interplay with symbolic dynamical systems. He is also keenly interested in Abel's mathematical works, having published several papers on this subject.

csk@math.ntnu.no