History of RSB Interview: Michel Talagrand

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PC: Good morning, Professor Talagrand. As we discussed ahead of this interview, the theme of our discussion is the ideas around spin glasses and replica symmetry breaking, but we're going to start a bit further back in your biography. In particular, in your autobiography for the Shaw prize[1], you mentioned that upon deciding what field to pursue at university you hesitated between physics and mathematics. I was wondering—because you've had such an impact on physics—after choosing math, did you ever look back? Were you ever in contact with the physics world? Did you keep track of what was happening there?

MT: [0:00:48] I had a general overall interest in the sense that I was reading not-so-good scientific magazines. The best science magazine I know of is Scientific American, Pour la Science[2]. It became available in France rather late. Of course, I could have subscribed to the American edition, but it was easier to get a mainstream science magazine. It was not so good, but [better] than through newspapers. I was reading that, being made aware of the most important things. If you want to understand something at the deeper level, it takes a huge amount of time and commitment. You cannot do that when you are just trying to survive, starting research, and already overwhelmed by all these people who know so much and you know so little. It is so difficult. I didn't study anything at all. Ok, I knew some very

[1] Michel Talagrand, "Biography of 2019 Shaw Laureate Michel Talagrand," *The 2019 Prize in Mathematical Sciences* (2019). https://www.shawprize.org/laureates/mathematical-sciences/2019 (Consulted October 22, 2021)
[2] Pour la science: https://fr.wikipedia.org/wiki/Pour_la_science

basic special relativity. That's about the most advanced it was. I [also] had a class in quantum mechanics, [of] which I didn't understand a word.

PC: Closer to your subject of interest, were you paying attention to the field of mathematical physics? Did you know any of the community?

MT: [0:02:24] I didn't know anybody. You know, mathematics is so vast and inside mathematics, I know so little. It works like Brownian motion. It means you get into a group, then people visit, you meet them, you talk to them, and you enlarge you interests. In order to make a plan of what you should do, you have to have an overview, which of course I did not have. All these things came really randomly to me. It's not exactly randomly, because there are things I said I don't want to do. For example, my advisor was studying analysis and was also studying something which is called potential theory[3]. He wanted me to get into that, but my instincts said no, absolutely no, and I didn't. So it was mixture of random interactions, and then following my own inclination. I knew very few people, because Université de Paris is so big that if you get involved then you cannot do anything, so I didn't get involved into anything. Just I belonged to a small group. I met the people there and that's all. It didn't happen that there was any mathematical physics.

PC: Just to help us understand a bit further. Could you tell us how would you define your community or your group? How do you self-identify?

MT: [0:04:05] Oh, well! That was the seminar run by Gustave Choquet[4], who was my advisor and who is really a great man, I greatly respect. (But somehow at the time I became his student, which is 1974, he was really at the end of his creative life. The things being done in the seminar didn't go so far.) I went there really by accident, because I studied in Lyon and there I asked some mathematician: "Where should I go?" and he said: "What do you like?" I said: "I like to split an interval into little pieces." Then, he said: "You should go with Choquet!" That's how it was decided. If I had gone with somebody else, it might have been very different. But it worked. How could I choose? It was a good place to go because many people were visiting that group. Also, I was lucky that some very good mathematicians joined the group later. The best of them is Gilles Pisier[5]. (You might know Gilles Pisier.) That was a great chance for me, because he brought me in touch with more central mathematics. None of these things is anywhere

[3] Potential theory: https://en.wikipedia.org/wiki/Potential_theory
[4] Gustave Choquet: https://en.wikipedia.org/wiki/Gustave_Choquet
[5] Gilles Pisier: https://en.wikipedia.org/wiki/Gilles_Pisier

close to ideal efficiency. No, I didn't know anything about mathematical physics.

PC: As a last framing question: do you define yourself as a probabilist or as an analyst? Or does this distinction even make sense?

MT: [0:06:01] Ok. I started as an analyst. My thesis was toward measure theory, which brought me eventually towards probability[6]. I had this incredible advantage to become a probabilist without having been educated like a probabilist but having been educated like an analyst. This is an incredible advantage, because people who go into probability are educated as probabilists. There are things which were obvious to me which would have been very difficult for them to think about. [For what] may be my best known result, which now everybody calls Talagrand's inequality[7], the first step is that you have to take a convex hull. When you are student of Gustave Choquet, the first thing you try is to take a convex hull. If you are not an analyst, you will never think of doing that. When chance has given you the proper perspective, that's a favorable condition. Again, there is no rational efficiency there, [it's] all chance. I would say that I'm a failed analyst. I liked analysis, but it's clear that the people doing analysis were far better that I was. I came in contact with Jean Bourgain[8] very early, and then that taught me what is my real place in the world. Then there was Gilles Pisier, who was nearly at the same level [as Bourgain]. So I said: "I cannot do analysis." These people are really better that I am, so I tried to do something else.

PC: Speaking of doing something else, and fast forwarding a few years, you mentioned in that Shaw Prize autobiography that you first heard about spin glasses from Erwin Bolthausen[9], at a meeting in 1993[10]. Can you please give us a bit more details about the context for that discussion?

MT: [0:08:16] The context is I was going to meetings in Banach space theory[11], I was also going to meetings in an area which is called high-dimensional probability. It used to be called probability in Banach spaces, but then it

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[6] Michel Talagrand, *Mesures invariantes, compacts de fonctions mesurables et topologie faible des espaces de Banach*, thèse d'état, Paris VI (1977). https://www.sudoc.fr/14616167X

[7] Talagrand's concentration inequality: https://en.wikipedia.org/wiki/Talagrand%27s_concentration_inequality

[8] Jean Bourgain: https://en.wikipedia.org/wiki/Jean_Bourgain

[9] Erwin Bolthausen: https://en.wikipedia.org/wiki/Erwin_Bolthausen

[10] The 9th International Conference on Probability in Banach Spaces, Sandjberg, Denmark, August 16-21, 1993. See *Probability in Banach Spaces, 9*, J. Hoffmann-Jörgensen, J. Kuelbs and M. B. Marcus eds. (Boston: Birkhäuser, 1994). https://doi.org/10.1007/978-1-4612-0253-0

[11] Banach space: https://en.wikipedia.org/wiki/Banach_space

became known as high-dimensional probability. That's the spirit of it. Supremum of stochastic processes and that kind of things. Erwin did go to these meetings. I'm not so close to what he did mathematically. It's just that he thought it could be a good idea to mention the existence of that problem. He took me to a blackboard and he wrote the Hamiltonian of the SK model. I saw these Gaussian random variables, and I got hypnotized by that. Because I had the hubris… I thought that I knew more about Gaussian random variables than anybody else, so I had an advantage in attacking this problem. That's totally ridiculous. That's the most ridiculous thing I ever thought, because this is absolutely not the case[12], but that's why I started it.

We didn't communicate really. Erwin mentioned to me that this is a famous problem, that many people have tried and have failed. But I don't think he had any specific idea. He just found that was a fascinating problem. Apparently, I found it fascinating [as well].

PC: Just a quick clarification. Did Erwin know you? Why did he think of you?

MT: [0:10:18] At the time, in 1993, I had already done… That's 20 years after I started, so I had already done the best things I did in probability… Ok, everything I did in probability was before 1995. Among the small circle of people doing that type of probability, I was known.

PC: So it was your general reputation. It's not that you were…

MT: [0:10:58] General reputation. Not at all because of the area. You see, this is interesting because he said that many people tried and failed. I have a much more modest view of things. You have no chance whatsoever to solve a problem like that just in one stroke. You have to approach the problem. Of course, I had no idea how to approach the problem, but I will give you an image. How do you walk from Paris to Vladivostok? It's a difficult project, but the first step is trivial. You take one step towards the East, and you try to iterate. It's exactly the mentality which I had. I said: "I have no intuition whatsoever. I don't understand anything about the

[12] **MT:** It was an illusion that I understood well Gaussian random variables. I had never made the effort to understand well the comparison theorems for a precise reason: they play no part in my work on the topic. In a sense, this work goes far beyond them. If I had not been lazy, I would have been able to discover first Guerra's result that the replica symmetric value of the free energy is an upper bound for the true free energy. It just follows the lines of the basic comparison theorem, called Slepian's lemma. A paper by Jean-Pierre Kahane presents a simpler proof, but it uses distribution theory, and I was too lazy to make the effort to learn that. I think it turned out that the argument of Kahane is incomplete. See J-P. Kahane, "Une inégalité du type de Slepian et Gordon sur les processus gaussiens," [A Slepian-Gordon-type inequality for Gaussian processes] *Israel J. Math.* 55, 109–110 (1986). https://doi.org/10.1007/BF02772698

problem. More generally, I don't understand anything about statistical mechanics. I don't have any idea. Let me make the smallest trivial statement I cannot prove, and try to prove it." That's what I did. Then, when I succeeded, I did it again, and again, and again. So for a long period I wrote a number of very technical and very difficult papers, which of course nobody has read, and nobody should read, because there is nothing important in these papers. They just don't do the right thing. It's a way to make progress. After you explore, then you start to make some observations in the right direction. But you have no chance to make these observations if somehow you don't start.

Humility is the key. Humility, and also, I must say, the support of the community. I submitted these papers to *Annals of Probability*[13] and *Probability Theory and Related Fields*,[14] which are the main journals in probability, and they accepted the papers[15]. I'm sure the referee must have said: "Ok. These are things which we have not seen before." But if they ask how many people will read that, they know very well that nobody will read it. But if they turn the paper down then I'll get discouraged. So they didn't think they should do that. They should encourage…

Also, I had a good reputation with these journals. To the extent to which it [matters], for *Probability Theory and Related Fields*, it was Bolthausen who was chief editor. The journal comes into numbers. There are four numbers a year. These numbers are little books. The number 2 of volume 10 consists of two long papers of mine. So I was the only author of one of these little books of which a volume is made. —I should have kept it. One paper is about the Hopfield model and the other about the SK model. This is a type of paper, which nobody is going to read, but if one doesn't let these papers appear, then the field cannot develop. It needs really trust from them and support from them, because if it doesn't go anywhere this is bad for the journal. That's really the thing, which is very important.

PC: Taking a step back. Why did this particular problem capture your attention at this particular time in your career?

[13] Annals of Probability: https://en.wikipedia.org/wiki/Annals_of_Probability

[14] Probability Theory and Related Fields: https://en.wikipedia.org/wiki/Probability_Theory_and_Related_Fields

[15] See, e.g., M. Talagrand, "The Sherrington–Kirkpatrick model: A challenge for mathematicians," *Probab. Theory Relat. Fields* **110**, 109-176 (1998). https://doi.org/10.1007/s004400050147; "Rigorous results for the Hopfield model with many patterns," *Probab. Theory Relat. Fields* **110**, 177-275 (1998). https://doi.org/10.1007/s004400050148; "Intersecting random half-spaces: toward the Gardner-Derrida formula," *Ann. Probab.* **28**, 725-758 (2000). https://doi.org/10.1214/aop/1019160259

MT: [0:14:52] It came at the right time. It came at a time when I was looking for a new topic. I had a main problem, on which I had been working for 10 years, and I saw I couldn't solve it. This problem was solved by others, much later, in 2011[16]. I would never have been able to do that. [Of the] mathematics that other people prove in my area, there are things I feel sorry about because I should have proven them and things I'm happy that they proved them because I could never have proven them. That's absolutely in the second category. This is the most beautiful piece of mathematics I've ever seen proven. Absolutely fantastic. I was right that I shouldn't pursue it.

So I needed a new topic and I said: "Why not this one?" Knowing that some good people have tried, again I thought: "They lacked humility." They must think they're going to solve it like that. No! Let us try to make progress in that direction. I was not scared, and also—again—my papers could get published. I could get funding from NSF[17], so there was really no reason not to keep working in that area. The system really supported me here.

PC: You made a few references to other people who had worked on this problem. One person who also took up this problem only a few years before you is Francesco Guerra[18]. Were you aware of his work? When did you become aware of his contributions?

MT: [0:16:59] That, I don't remember. Probably Erwin told me, because I remember reading the papers having to do with… Yes, very early. These are important papers, because these were the only papers of that general area [about] which I could understand something.

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[16] W. Bednorz and R. Latała, "On the suprema of Bernoulli processes," C. R. Math. **351**, 131-134 (2013). https://doi.org/10.1016/j.crma.2013.02.013; See also J. R. Lee, "Talagrand's Bernoulli Conjecture, resolved," *tcs math* (2013). https://tcsmath.wordpress.com/tag/bernoulli-conjecture/ (Consulted October 22, 2021)

[17] M. Talagrand, " Combinatorics, Banach Spaces, Probability, Spin Glasses," *NSF DMS-MPS* #9703879 (1997-2000). https://www.nsf.gov/awardsearch/showAward?AWD_ID=9703879; "Spin Glasses: A New Direction for Probability Theory," *NSF DMS-MPS* #9988480 (2000-2004). https://www.nsf.gov/awardsearch/showAward?AWD_ID=9988480; "Probability Theory and Spin Glasses," *NSF DMS-MPS* #0243813 (2003-2007). https://www.nsf.gov/awardsearch/showAward?AWD_ID=0243813; "Probability and Mean Field Models for Spin Glasses," *NSF DMS-MPS* #0555343 (2006-2010). https://www.nsf.gov/awardsearch/showAward?AWD_ID=0555343 (Consulted October 22, 2021).

[18] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Francesco Guerra*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 27 p. https://doi.org/10.34847/nkl.05bd6npc

The first thing I did, of course, is I took books. The book is—Erwin mentioned—the Mézard-Parisi-Virasoro book[19]. I got the Mézard-Parisi-Virasoro book. My library bought it. I took it with me. I carried it around the world for two years. I read it when my children were in the beach. I could not understand a word of it. Ok, I understood that the cavity method is an induction over the number of particles. That's the first thing I would have tried anyway, so that was not helpful. Maybe it's a good book for some people, but let's just say it was not a good book for me. What might have helped is explaining the very basics of statistical mechanics with random disorder. Very basics. What is the physics of the high-temperature regime? Just this thing, which I could not get in that book.

If I had taken maybe a course in statistical physics from the beginning maybe I would have learned it, but it's difficult to do. I didn't have anyone to guide me. I didn't know where to look. Another thing I have to say about this book is that maybe it is written to be cute somehow, but it doesn't help when immediately you go into functions of a negative number of variables. [If you're a] mathematician it doesn't help really understanding what's going on. It looks like something purely formal. I really didn't get anything out of it. I'm to blame partly, because I didn't really even understand the structure of the high-temperature regime, which I'm sure is in there… It's a cultural problem. The cultural problem is unfathomable between mathematics and physicists. I had the chance to experience that more later because I read some physics after that, but at the time I was totally defenseless. Again, it was not the most efficient process.

PC: So this was really your first exposure to the physics research literature at that point.

MT: [0:20:14] Yes. I can say really it's a shock.

I was really very lucky because I got a research position in France, before having done any research, just as a graduate student. I arrived in the Choquet seminar and tried to read the articles related to what people talk about there, and I could not understand a word of these articles for years. A references B, which references C, which… There's a whole web, and it's difficult.

Of course, when I tried to read the physics literature I was not surprised that it is the same, that I don't understand a word. The difference is that in mathematics after studying a couple of years I started understanding

[19] M. Mézard, G. Parisi, M. A. Virasoro, *Spin Glass Theory And Beyond: An Introduction To The Replica Method And Its Applications* (Singapore: World Scientific, 1987). https://doi.org/10.1142/0271

something, but in physics—maybe I didn't study long enough— I never understood anything of what they wrote.

PC: If *The beyond*, as the book is sometimes known, was not so helpful, were the theoretical physics actors themselves helpful to you at any stage?

MT: [0:21:30] I had no contact with anybody at all. I didn't try. I didn't know anybody. Ok. No. I contacted the obvious person, which was Marc Mézard[20], because Marc Mézard is in ENS, which is easy to reach. He was very kind to me. It means that he opened his door and I spent many hours on his blackboard trying to communicate things which I had done. The communication was really difficult. At a personal level, he encouraged me, he was very kind, but I didn't succeed in understanding much of what he was telling me. Conversely, he didn't understand what were the obstacles I was meeting. When I told him something I had proved in the high-temperature regime, he kept wondering why the things I could prove were so weak.

Now, I understand the overall process. Mathematicians, when they make an estimate, really have to prove it. It's not that it's likely to be true. You have to prove it. You lose an order of magnitude between what you can prove and what is just likely to hold. So communication was difficult.

There were some other attempts in France. French physicists, several people were kind to me, but scientifically I couldn't say it helped.

I have to say I have a special problem… Maybe the problem is not the system, it's just me. It's the same in mathematics. I'm completely unable to understand any piece of mathematics unless I start at the first line and I take everything apart and rebuild it my own way. It's an extremely slow process, which is why I didn't learn anything, because I would need several lifetimes to learn things. But it has a good part. The good part is what I understand, then after this process I understand better than other people because I spent so much effort on it. Being like that, of course, is not helpful to understand these somewhat not precisely formulated [things], of which there are so many in physics. You see, that's my own problem. I'm not saying the book is not good; I'm saying it was not for me. It was not helpful for me.

PC: Speaking of the physics results themselves. Were the predictions, or the ansatz from physics of any help to you? Or not even those?

[20] Marc Mézard: https://en.wikipedia.org/wiki/Marc_M%C3%A9zard

MT: [0:24:50] That's my own stupidity. I didn't understand even the structure of the replica symmetric solution. Then I had a really bad idea. Now, I see the good program would have been to try to prove the replica symmetric solution for the SK model at high enough temperature. That's the natural first step. But I had a really bad idea… I was introduced to the Hopfield model because there were people like Anton Bovier and Pierre Picco who were studying the Hopfield model[21], and I learned about them. The terrible idea was to think that the Hopfield model, [which has] one more parameter, should be easier than the SK model. That's totally wrong, because in fact it's probably much more difficult. But I spent a lot of energy on the Hopfield model[22]. I don't know if the energy I spent there benefited me later. I had to reinvent everything myself, but since I didn't understand the very basic that every physicist knows, I had to figure it out by myself, which took of course a very long time.

PC: During those first few years of struggle—if I may say—or rather of you ramping up that program, what was the reaction from the different communities, say from the theoretical physics, the mathematical and probability communities?

MT: [0:26:41] I mentioned already the really important thing was the support from the journals and the editors. Besides that, nobody had any idea… Among mathematicians, nobody had any idea of what I was doing. Simply because these papers are not readable… To read the papers I wrote, you have to be a very good analyst and work very hard, and be interested in the topic. That's an empty set. Of course physicists had no idea whatsoever. I mentioned that some physicists were very kind to me, but they have their own way to view things, which I understand and I respect, but with which I did not really agree.

So [here's some] classified information I will give. There was a determining event. In 1999, my advisor, who was a member of the Académie des sciences[23] put me up for one of the big prizes that the Académie des sciences gives. People don't know that, but Académie des sciences gives more money every year than the Swedish Academy of sciences[24]. There are some called *grands prix*, which are a great honor and they are a substantial amount of money, €50,000 or something like that. So Choquet

[21] See, *e.g.*, A. Bovier, V. Gayrard and P. Picco, "Gibbs states of the Hopfield model in the regime of perfect memory," *Probab. Theory Relat. Fields* **100**, 329-363 (1994). https://doi.org/10.1007/BF01193704

[22] See, *e.g.*, Ref. 15 and M. Talagrand, "Résultats rigoureux pour le modele de Hopfield," *C. R. Acad. Sci. Ser. 1* **321**, 109-112 (1995). https://gallica.bnf.fr/ark:/12148/bpt6k6473218d/f143.item (Consulted October 23, 2021)

[23] Académie des sciences: https://en.wikipedia.org/wiki/French_Academy_of_Sciences

[24] Kungliga Vetenskapsakademien: https://en.wikipedia.org/wiki/Royal_Swedish_Academy_of_Sciences

put me up for that prize. That prize—it's called the Ampère prize[25] [and] I think Marc Mézard got it—is one year for mathematicians and one year for physicists. That was the year for mathematicians, but the committee is always made of mathematicians and physicists. The well-known physicist who was on the committee that year opposed to my getting the prize because they explained that… Of course I don't know which words they used because I was not at the meeting. I just got some overall information from Gustave Choquet himself [who said]: "They say that what you did is uninteresting." Because at this time, there was only this work on the Hopfield model, which is technically very hard, but that the physicists consider trivial… For a physicist, everything having to do with high temperatures is trivial. The thing of interest is the low temperatures. Overall, I agree, but even getting the high temperature region for the SK model is not trivial at all. But that, I hadn't done it at the time. I had just done this work on the Hopfield model, they didn't evaluate it the way I would have liked. Again, I understand why they thought that way, but what made me really sad about that is that if I had not worked at all on spin glasses, I'm absolutely certain I would have gotten the prize, because—if you think only of mathematics—I probably had done more than the mathematician who got the prize that year. Having spent all that effort on spin glasses had this really great negative value on that specific occasion. That's life![26]

PC: A few years later, you wrote a first book on spin glasses[27] that was reformulating the advances you had made over the previous years and formalizing it in a slightly different language. Why was this a natural moment to write a book?

MT: [0:30:57] It's a natural moment, ok. There are several considerations. I tried to keep going forward, and a good way to do that is to review everything from the beginning and put it in your brain together hoping that there will be some sparks. That's the main motivation for writing the book. As a secondary motivation, I knew very well that nobody would read one line of the papers, but when put altogether in a book with [consistent] notation maybe starting is a little bit easier, and then maybe somebody will read something of the book, or at least they will have to quote the book, because it is harder to ignore a yellow book than the papers. All these together... I'm a terrible writer, which I learned only recently. If I want to write something which can be read, I have to put tremendous

[25] Ampère Prize: https://en.wikipedia.org/wiki/Amp%C3%A8re_Prize

[26] **MT:** Despite this negative story, some people (like Bernard Derrida) in the physics community had a more positive reaction. I was even invited to give a series of lectures for an informal meeting at ENS, but I failed to communicate anything of value. I would not know how to do it now either.

[27] M. Talagrand, *Spin Glasses: A Challenge for Mathematicians* (Berlin: Springer-Verlag, 2003).

effort in it, which I didn't [do] for that book. I didn't spend such a very long time for the book. Again, it might have… Probably I wouldn't have made further progress if I had not written the book. The idea is to be humble. I you cannot reach your goal, that doesn't matter. Just try to get unstuck and go somewhere, and iterate. This simple idea is that you keep repeating.

PC: The book did succeed, then, in its initial goal.

MT: [0:32:53] The book did. Probably it helped me, yes.

PC: One thing that followed closely the book is that you collaborated with Giorgio Parisi on an article[28]. Can you tell us a bit more about that?

MT: [0:33:09] Giorgio Parisi came to give a lecture at IHP[29] soon after I had proven the validity of the Parisi solution. I forget [now] what was the event, but it was about spin glasses. Parisi showed what a generous and great person he is, because he mentioned the Parisi solution and he added: "Now, we are sure it is true." That was really nice… It should be known that he said that, because he acknowledged the difference between a mathematical proof and being physically certain that it's correct[30]. After his talk, I went to talk to him, which of course is a great experience. I forget what we discussed, but there is a tiny speck of something he said which I could understand. In contrast with all these fantastic things that are in his papers but are not within my reach[31], I could understand something. So I wrote a little paper and I sent it to him and he approved. That's how the paper was. I think it's a good idea, but it can be pushed further. I would never have thought of doing that. I was just a scribe for that paper.

PC: In 2006, you published that rigorous demonstration on the validity of the Parisi formula for free energy of the SK model[32]. Could you tell us what enabled this advance? You mentioned the book being a spark…

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[28] G. Parisi and M. Talagrand, "On the distribution of the overlaps at given disorder," *C. R. Math.* **339**, 303-306 (2004). https://doi.org/10.1016/j.crma.2004.06.014

[29] Institut Henri-Poincaré: https://en.wikipedia.org/wiki/Institut_Henri_Poincar%C3%A9

[30] **MT:** Giorgio Parisi and other physicists originally thought that "new mathematics" would be needed to tackle the problem. I think that by now I have figured out the reason why Parisi thought that way. It is because of QFT, which mathematics has failed to really clarify. Naturally he thought the situation could be the same for spin glasses, but it was not.

[31] **MT:** I would compare the papers I write to crossing a desert, the rare ideas being the oases, and the vast expand of sand being the gritty technical work. Compared to this, Parisi's papers are like the jungle with a constant stream of unbelievable ideas. I wish I could understand them better.

[32] M. Talagrand, "The Parisi Formula," *Ann. Math.* **163**, 221-263 (2006). (Received May 13, 2003) https://www.jstor.org/stable/20159953

MT: [0:35:21] You have to find an approach. That's why the people who tried to solve the problem without being humble enough didn't find it. The approach has to be based on something. In writing one of these monstruous technical papers[33], I made a trivial observation which is that when you get an upper bound on the free energy of two coupled copies of the system, that in a certain way can help you get a lower bound on the energy of a single system. That's the basis of the approach. Then, Guerra came up with his replica symmetric upper bound for the SK model[34]. Once you understand this paper, you can do the same thing for coupled copies. That was the upper bound which was needed to make the trivial observation work that the expression should be what it is. When I put 2 + 2 = 4, it took one week to get the proof, except that I realized that I had missed a little detail, so I sweated a little bit, but then in the end it was ok.

If you are interested in history, we can comment a little bit on how science works. I knew I had solved a big problem, I knew I was not likely to do many more like that, so I submitted [the manuscript] to the *Annals of Mathematics*. The *Annals of Mathematics* did their job. They sent it to the obvious referee. You can guess who is the obvious referee, Michael Aizenman[35]. I don't know how much effort he put in reading the paper, but it happened that there was a conference in Ascona soon after I submitted the paper[36]. Michael Aizenman then came to me and he said: "You claim to do that. I don't understand your proof." I managed to communicate to him the overall structure of the approach. Then, he said: "Now, I understand." And the paper was accepted. I don't know if the paper was read at all, but that's how science works. It's perfectly fine. The best expert has to be reasonably convinced that the paper is sound, and then the job is done. He cannot be asked to check line by line that everything is ok.

PC: That paper, in particular, is dedicated to Francesco Guerra…

MT: [0:38:28] Because without Guerra's replica symmetry breaking bound, the paper could not exist. Guerra's replica symmetry breaking bound belongs

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[33] M. Talagrand, "Replica-symmetry breaking and exponential inequalities for the Sherrington Kirkpatrick model," *Ann. Probab.* **28**, 1018-1062 (2000). https://www.jstor.org/stable/2652978

[34] F. Guerra, "Sum rules for the free energy in the mean field spin glass model," in *Mathematical Physics in Mathematics and Physics: Quantum and Operator Algebraic Aspects*, ed. R. Longo, 161-170 (Providence, RI: American Mathematical Society, 2001).

[35] P. Charbonneau, *History of RSB Interview: Michael Aizenman*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 21 p.

[36] International Conference on Equilibrium and dynamics of spin glasses, A. Bovier and E. Bolthausen, 18-23 April 2004, Centro Stefano Franscini, Monte Verità, Ascona, Switzerland. See, *e.g.*, *Annual Research Report 2004 of Weierstraß-Institut für Angewandte Analysis und Stochastik* (Berlin: WIAS, 2005). https://www.wias-berlin.de/annual_report/2004/fb04wias.html (Consulted October 26, 2021)

to the category, as I was explaining, that I am not sorry I didn't do it because I could never have reached that level. To be able to find that bound you have to understand really the structure of the Parisi solution, or—like Michael Aizenman would say—the probability cascades. There is this underlying probability structure that the physicists understood very well, and Guerra must have understood, which gave him a way to find his bound. Since I had not understood that at all at the time, I could not have found this bound. So it was absolutely correct that this is dedicated to Guerra. You see, this is sort of funny, because in my eyes Guerra did the difficult part of that proof, which is to find that. The other part is really a schoolboy idea, but somehow that schoolboy idea was not so easy to find. Life is hard!

PC: At that point, did you know Francesco Guerra personally, or was this all through papers?

MT: [0:40:01] I do not remember when I met him the first time. He came to Paris several times. Of course, when I learned he came I met him. But there is no way I will recollect when this was.

PC: So throughout that 10-year period, you got to know him to some degree.

MT: [0:40:26] Yes. Sure.

PC: In notes that you sent us, you mentioned another result by Guerra, the Ghirlanda-Guerra identities[37], as being remarkable.

MT: [0:40:39] That's another of the same category. You do something, which just looks so simple. You take an equality of two things and you integrate… How can one invent such a thing? When I see this kind of result I just feel so lucky that chance and fate took me to results I could prove when other people can prove this kind of stuff. I don't deserve to be a mathematician. That's why I mentioned that. I have infinite admiration for these two results of Guerra. That's the only two results I know. It does not mean that the rest is not of that level. Maybe it is, but I don't know it.

PC: Did you use that identity?

MT: [0:41:40] I got some consequences of that, but I didn't go far enough. It can be used better. You get some amazing information out of that, which

[37] S. Ghirlanda and F. Guerra, "General properties of overlap probability distributions in disordered spin systems. Towards Parisi ultrametricity," *J. Phys. A* **31**, 9149 (1998). https://doi.org/10.1088/0305-4470/31/46/006

interestingly Ghirlanda and Guerra didn't do. They proved the inequality and did not draw the consequences, which is sort of sad.

PC: Following that seminal work from 2006, what was the reaction of the various communities: the theoretical physicists, the mathematical physicists, the probabilists?

MT: [0:42:30] The best people with whom I was in contact mathematically had no idea of that kind of work. The people in the spin glass conferences, of course, they knew about it and they liked it[38].

I was elected the French Academy in 2005, which was just after I had... (The solution was published in 2006, but I must have done it in [2003], so it was already known.) The same persons who didn't want me to get that prize, now said: “Now, what he has done is interesting.” Of course, they meant that “what he had done before was not interesting”. They had not changed their mind, but now this was interesting. They liked it.

PC: It was more favorable, for sure. In that second phase, between the book and that result, you also started collaborating with Dmitry Panchenko[39]. Can you tell us how this collaboration came about? In what ways were you complementary in that work?

MT: [0:44:09] Dmitry Panchenko had started [working] on more or less the same kind of probability I was doing. The really interesting question of why did he become interested in spin glasses, you would have to ask him. I must have asked him, but I don't remember. But he got interested in spin glasses. When I got interested in spin glasses, I had already had my career and had a good position. When I saw this guy starting to get interested in rigorous results of spin glasses, I thought this is too difficult. He may get killed there. I tried to discourage him. I told him: “You know, this is a very difficult topic.” He said: ”Well...” And he kept working at it. I really felt then that this guy deserved something. I tried to help him. The way I helped him is I tried to teach him everything I know. I came up with reasonably easy

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[38] **MT:** The reception from the mathematical community was mixed at first. On the positive side, *The Annals of Mathematics* accepted the paper. On the negative side, I would have thought that the result would be worth an invitation to the International Congress of Mathematicians (ICM), but this did not happen. So I did not make it in the very restricted circle of people who spoke three times at the ICM. But of course now I realize that I would never have been awarded the Shaw prize if I had not proved this result, so in the end the reaction of the mathematical community was 100% positive.

[39] See, *e.g.*, D. Panchenko and M. Talagrand, “Bounds for diluted mean-fields spin glass models, *Probab. Theory Relat. Fields* **130**, 319-336 (2004). https://doi.org/10.1007/s00440-004-0342-2; “On the overlap in the multiple spherical SK models,” *Ann. Probab.* **35**, 2321-2355 (2007). https://doi.org/10.1214/009117907000000015; “On one property of Derrida–Ruelle cascades,” *C. R. Math.* **345**, 653-656 (2007). https://doi.org/10.1016/j.crma.2007.10.035

projects, which are the joint papers. Then, I invited him in my house for one week. We spoke about spin glasses all day. It was a great investment, because soon after I realized that he understood better, and he did some great work, [for which] I was absolutely not in the right direction and I couldn't have done. I'm very glad he was successful. I'm very glad I was a small part of that.

PC: It's a very nice outcome, yes.

MT: [0:46:03] Yes, the instinct of… It's very difficult to know who is going to be successful or not because the talent and the personality are not enough. You need also a little bit of luck. [In the case of] Panchenko it's not luck, he went into the right direction. There is a good saying I love: "There are three ages in the life of a physicist." Do you know this one? "There is the age to learn, there is the age to discover, and there is the age to prevent other people from discovering." Because at some point you know everything, and you become prejudiced. That's exactly what happened to me. The line that Panchenko followed successfully I knew about it, but I dismissed it, thinking: "This cannot possibly work." Instead of being humble and pursuing it and [thinking]: "Let me see what…" I dismissed it without investigating it. I got what I deserved, which does not mean I could have done what Panchenko did, but ok. There is some moral to be learnt from that story.

PC: A few years later you published a second, two-volume edition of your book on spin glasses[40], which more or less coincides with the end of your efforts in the field of spin glasses.

MT: [0:47:37] That's called the ultimate bad timing. The idea to write a second edition is very simple. The most interesting thing is the Parisi solution and it was not included in the [first] book. So I said: "It has to be in book form." But then I made some bad decisions. I used another approach, which turned out is more difficult than the original approach. Then, I tried to rewrite everything and extend it, but already this was the time where… It was kind of a shock for me. Panchenko helped me reading the draft, and then I realized that, as I write the draft, he started to understand things. Things I'm so happy that I understood when I wrote the second edition—I had not understood before—he already knew them! He already figured them out. Then, he started showing things I had not understood. It's clear

[40] M. Talagrand, *Mean Field Models for Spin Glasses: Volume I: Basic Examples* (Berlin: Springer-Verlag, 2011). https://doi.org/10.1007/978-3-642-15202-3; *Mean Field Models for Spin Glasses Volume II: Advanced Replica-Symmetry and Low Temperature* Berlin: Springer-Verlag, 2011). https://doi.org/10.1007/978-3-642-22253-5

that he was understanding the topic better than I was. So the effort of that is largely wasted. But maybe not entirely. For things like the Hopfield model, which I don't think anybody has made progress, now there is a very detailed account of everything that you can rigorously prove. Maybe someday it will be useful. Dmitry proving the ultrametricity[41] was really determining in my decision to stop working on spin glasses, and actually to stop doing mathematics at all.

At the same time the problem I got stuck on in 1995 when I started working on spin glasses was solved in 2011 by two Polish mathematicians: Rafał Latała and Witold Bednorz[42]. Again, that's a fantastic piece of mathematics. I felt I have been solving all those problems. Now the two problems I wanted most to solve two other people solved them. Isn't that a sign of something? Well it's a sign that maybe it's time to stop doing research, which I mostly did. Stop doing research and stop working are slightly different[43]. I had [the] traumatic experience of seeing many mathematicians work when they should have stopped. They think what they do is as good as what they did and they are the only one to think that. It's not desirable to put yourself in such a position.

So don't ask me anything about spin glasses beyond that. I know Dmitry has published. I could mention a few names that have been working on that, but I have no idea what they did. Yeah, there's Antonio Auffinger[44], who is publishing in that area. And I read a paper on a different topic he wrote. This guy is amazingly good. There is no other word. This other paper is just fabulous and I have no reason to doubt that what he does on spin glasses is any lower level. It's become far too difficult now. I'm glad I didn't try to pursue. They are too good.

PC: From your perspective, how important or not has the spin glass problem become in the probability community, as you were working through it and since you left?

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[41] D. Panchenko, "The Parisi ultrametricity conjecture," *Ann. Math.* **177**, 383-393 (2013). https://annals.math.princeton.edu/2013/177-1/p08

[42] See Ref. 16.

[43] **MT:** That was just 10 years ago. Instead, I made myself happy by finally learning some physics. There is a very interesting story there because it centers on the same themes: the difficulty of communication between the physics and mathematics community, and the difference of culture between these communities. But it is a different story, so maybe you do not want to hear it. I will just say that as a result of many efforts and detours, I wrote an introductory book to QFT aiming at mathematicians, and it will come out very soon. And, despite my decision of not doing research anymore, I somewhat relented during the confinement to keep my sanity, but this looks like a tolerably good excuse and I promise never to do it again. See M. Talagrand, *What Is a Quantum Field Theory?* (Cambridge: Cambridge University Press, 2022).

[44] Antonio Auffinger: https://en.wikipedia.org/wiki/Antonio_Auffinger

MT: [0:51:58] At the turn of the century there were these big millennium conferences. I was invited to one called something like "Mathematics for the 21st Century''. The article I published there is called "Spin glasses: a new direction to probability theory."[45] Which means somehow it would span a branch of probability, but there was no ground for that. It was just wishful thinking. I don't think it had any impact of probability. It just became a subfield of probability theory, like percolation theory. Now you have, as I said, extremely good people working on that. I don't know if in the long term it will keep advancing. There are many problems which are left, because there are some models, like the Hopfield model, where it's very hard to say anything. But will we be able to solve these problems? Anton Bovier told me one time: "There's no reason why we should be able to rigorously solve these models." Maybe he was right. Maybe it cannot be done, or maybe it cannot be done in the foreseeable future. Nobody knows what will happen. But it's certain that it didn't have a major influence on probability theory. It just created a new field.

PC: During your time in Paris or in Ohio State, did you ever get to teach about spin glasses?

MT: [0:53:43] One of the things I'm proud of is that I succeeded at not doing a lot of things that all other people do. The one I succeeded the best at is administrative work, of which I did none, ever. The second one I succeeded best is not having to teach. My position in Paris doesn't require it. I can teach on a voluntary basis, which means I'm permitted to teach for free. In Ohio State I had to teach. I had a brilliant solution for that. Many people try to prove, to show to themselves how good they are, by teaching advanced courses. I tried to maximize the efficiency, and the maximum efficiency is last year of calculus, because the material is trivial, but the worst students have been eliminated because it's the last year of calculus. I thought of teaching on a voluntary basis in Paris. If I had students like Auffinger, Panchenko, I would volunteer any time to teach. But the French system is designed in such a way that the students to whom I [would] teach are the university students, they are not the students of École Normale. The odds of finding a Panchenko are low enough that it's not worth trying, which I didn't.

PC: As we're nearing the end of our discussion, is there anything else that you like to share with us about this era that we may have missed?

[45] M. Talagrand, "Spin glasses: a new direction for probability theory?," Mathematics towards the third millennium (Rome, 1999). *Atti Accad. Naz. Lincei Cl. Sci. Fis. Mat. Natur. Rend. Lincei Mat. Appl.* **Special Issue**, 127-146 (2000). https://eudml.org/doc/289708 (Consulted October 23, 2021)

MT: [0:55:38] (Was there anything in the notes? I wrote some things in the notes, which I didn't cover, but you can add them.) I was saying that I'm really not a good person to contribute for this history because somehow I know so little of it. These very small details that happened [specifically] to me, that's the only thing I know, because I was not part of the… Also, maybe it's like in probability. Maybe I was successful because I was not part of it. Somehow, coming from outside is a terrible disadvantage. I suffered a lot. But by some respect it's also an advantage. Life is hard. There is no algorithm to tell you which path you should follow to succeed, to solve problems.

PC: Maybe one thing that comes up is that you received the Shaw prize a couple of years ago, which acknowledges your work on spin glasses at that point.

MT: [0:57:10] This is due to… The stars were aligned, but that is not enough. Somebody had to volunteer and write, to put me up, to document and to do some work. This is interesting. Of course, I know who it is. It's a mathematician who is not afraid to tackle extremely difficult problems, where there is very little hope of succeeding. Somehow, he had applied the same strategy there, and against all odds he succeeded. That's extraordinary. Of course, I thanked people who might have written for me, and somebody said the physics people did their share too. I guess Parisi did something. That's what it means.

FZ: I just want to ask you one question that maybe we skipped. There was a paper by Ruelle, Aizenman and Lebowitz[46], where they were trying to prove something on high-temperature regime…

MT: [0:58:40] Yes. On the high-temperature regime, without external field. That's without external field, this paper. Without external field, it's completely different. That's one paper I read so many times. I understood the paper, but it didn't open a path to anywhere [for] me. This a purely rigorous math paper. I don't [mean to] say [that] I really got the picture, but line by line there's no problem to follow it. But it was not helpful in any way to go further.

FZ: Did you discuss with the authors of that paper about extension of that work?

[46] M. Aizenman, J. L. Lebowitz and D. Ruelle, "Some rigorous results on the Sherrington-Kirkpatrick spin glass model," Comm. Math. Phys. **112**, 3-20 (1987). https://doi.org/10.1007/BF01217677

MT: [0:59:33] Maybe I should have contacted the people. You have to think. It's a little bit before email. It was harder to contact people then. When I tried to contact people for other reasons, I didn't always get responses. It's not so easy. If you are not in a position where you meet certain persons easily it is hard. If you meet certain persons easily, you can go and go until something happens. But I didn't try to do [that] with them. Also, there is this major obstacle. These are very famous persons in a very famous university. It's sort of difficult. I'm just a nobody at CNRS[47]. Nobody knows what CNRS is. So I didn't try.

PC: The last question is about documentation. Do you still have notes, papers, correspondence from that epoch? If yes, do you have any plan…

MT: [1:00:45] I don't remember I had notes, but then I'll tell you this sad story, which happened a few years ago. The house in which my advisor was living with his wife, Yvonne Choquet-Bruhat[48], who did some very important work in general relativity—she exchanged correspondence with Einstein—had a lifetime of work of files in their offices. Choquet was already dead and his wife had to go to a retirement place because she couldn't stay by herself. So the house in which they lived, the children took care of it. The way they took care of it: they called somebody to empty the house and all the files went to the dumpster. It's interesting because one of their children is a scientist, and he knows very well that there are people in the academy—there is an archive technician—that are just happy you give them a call: "There is some document from famous Professor Choquet waiting." Then, they will put it in boxes and keep them for 100 years until some student wants to study it. But he didn't do it, so the files were dumped. Some of the Choquet files were recovered from the dumpster, but then it rained so the files of his wife were lost, including the correspondence with Einstein.

When I learnt of that, I was totally dejected. I went to university and I dumped all my own files. I said: "Ok. That's going to be done by a random process, so I'm going to dump them. This is better than if somebody else does it." Anyway, I had nothing of interest.

Among the mistakes I made in my scientific life… These mistakes are always made in the same direction: not being humble enough. I should have been more patient and make books of notes. Whenever there is a question, try to formulate it in writing very nicely. Then, whenever there is

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[47] Centre national de la recherche scientifique: https://en.wikipedia.org/wiki/French_National_Centre_for_Scientific_Research

[48] Yvonne Choquet-Bruhat: https://en.wikipedia.org/wiki/Yvonne_Choquet-Bruhat

an observation, try to put it in writing. I know some people who do that, and I really regret I didn't do that because that's the way you make little steps. You stir the stuff in your brain, then the stuff will happen. I didn't do that. So I don't have it. This could have been interesting notes, but they were never written. Maybe I would have done better mathematics if I had done that before.

PC: Professor Talagrand, thank you very much for this discussion.

MT: [1:03:58] That was fun. Thank you so much, and good luck with the transcription. I hope it eventually serves some purpose.