History of RSB Interview: Marc Mézard

November 14, 2022, 10:00 to 13:00 (CET). Final revision: July 23, 2023

Interviewers:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Francesco Zamponi, ENS-Paris

Location:
In person, in Prof. Mézard's office at Bocconi University, Milano, Italy.

How to cite:
P. Charbonneau, *History of RSB Interview: Marc Mézard*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 49 p.
https://doi.org/10.34847/nkl.abc22iqw

PC: Good morning, Professor Mézard. Thank you very much for sitting down with us. As we discussed ahead of this interview, the theme of this interview series is the history of replica symmetry breaking in physics, which we roughly bound from 1975 to 1995. But before we dive into that topic, I'd like to ask you a few questions on background. First, can you tell us a bit about your family and your studies before starting university?

MM: [0:00:48] I grew up in a small town in the center of France called Aurillac. I did all my studies there until the *baccalauréat*[1]. Then, I moved to Paris for this typical French system called preparatory school for *grandes écoles*[2]. I arrived in Paris in '74, and I entered École normale supérieure as a student in '76.

PC: What drew you to science? What was your exposure to science at that point?

MM: [0:01:23] I was attracted towards science, but had one tough choice to make, which was right after the *baccalauréat*. This is really the moment in which to decide what is the orientation for your superior studies. I was attracted both to philosophy and to science and it was not possible to do both. At some point, the French system drew me to the system of *grandes écoles*, in which it was predominantly easier for me, in some sense, to go towards science.

PC: How did you get interested in physics more particularly?

---

[1] Baccalauréat: https://en.wikipedia.org/wiki/Baccalaur%C3%A9at
[2] Grande école: https://en.wikipedia.org/wiki/Grande_%C3%A9cole

MM: [0:01:59] I got interested in physics very soon. I was already interested in physics in high school, I had a wonderful teacher there. I had also an excellent teacher of physics in the preparatory school, in *classes préparatoires*[3]. At that time, I was probably interested more in physics than in mathematics, even if I was in the mathematics section and I prepared the entrance exam of École normale in mathematics.

PC: What then led you to pursue a *thèse de 3e cycle* with Claude Bouchiat[4] in high energy physics[5]?

MM: [0:02:46] The system at that time… There existed what was called the DEA[6], that meant the equivalent of second year of master. It was a decisive moment because there was a ranking at the end of that DEA year. The best students would go for the *thèse de 3e cycle* typically either to École normale or to the Saclay theory group[7]. I was in that position, and I decided to join École normale because I thought it was the best lab of theoretical physics. The tradition was really to do the *thèse de 3e cycle* with a lot of field theory and also with some contact with the phenomenology of particle physics. That is what I did, and that's why I joined the group.

PC: With Bouchiat in particular? Was there a choice of advisor?

MM: [0:03:52] There was not much choice of advisor. It was natural. Bouchiat, at that time, was the person who would really coach, let's say, or train the young students arriving in Theoretical Physics at École normale.

FZ: Can you elaborate a little bit on this? You said that there was this choice between Saclay and École normale. I think we have an idea of the group in Saclay, because previous people described it, but we don't have a clear idea of the theoretical group at École normale. What were the options? How was the group organized?

MM: [0:04:28] The group at École normale was very largely particle physics. The leaders of the group were probably Claude Bouchiat, my advisor, Philippe

[3] Classes préparatoires: https://en.wikipedia.org/wiki/Classe_pr%C3%A9paratoire_aux_grandes_%C3%A9coles
[4] Claude Bouchiat: https://en.wikipedia.org/wiki/Claude_Bouchiat
[5] Marc Mézard, *Test de QCD et observables inclusives dans la diffusion inélastique de neutrinos*, thèse de 3e cycle, Université Pierre et Marie Curie (1980). https://www.sudoc.fr/042326508 (Consulted February 14, 2023.)
[6] Diplôme d'études approfondies: https://fr.wikipedia.org/wiki/Dipl%C3%B4me_d%27%C3%A9tudes_approfondies
[7] Service de physique théorique de Saclay: https://fr.wikipedia.org/wiki/Institut_de_physique_th%C3%A9orique_-_IPhT_Saclay

Meyer[8], who was my co-advisor with Bouchiat for the *thèse de 3e cycle* but not for the *thèse d'état*, and John Iliopoulous[9]. They were really the leaders of the group. There was a strong topic in the group around supersymmetry with Pierre Fayet[10], and supergravity with Scherk[11], Cremmer[12], and Gervais[13], [as well as André Neveu[14]]. To some extent, supersymmetry and supergravity had been founded in that lab, so they were the leading directions of research. But the tradition would be that, at the level of the *thèse de 3e cycle*, which was a very short thesis in some sense, one would start by first learning about field theory and particle physics. That was not taught in classes at that moment. I remember that during my first meeting with Bouchiat, he gave me a pile of papers on his table. It was probably higher than 30 centimeters. This was the course of Itzykson and Zuber, which was not yet published as a book[15]. He told me: "You read this, and when you are done you come back to see me." I came back something like three weeks later, and I told him: "Look, I have not understood everything," and he started to challenge me. Claude was a very kind but very tough advisor at the same time.

PC: So, there was no statistical physics group?

MM: [0:06:11] The only one who was doing statistical physics [in the Theoretical Physics Lab] was Nicolas Sourlas[16]. Sourlas was there but in some sense he was marginal in the group.

FZ: And Toulouse[17]?

MM: [0:06:36] Toulouse was not in our lab at that moment. He was in the other lab. He was in the Laboratoire de matière condensée.

FZ: But still at École normale?

---

[8] Philippe Meyer: https://fr.wikipedia.org/wiki/Philippe_Meyer_(physicien)
[9] John Iliopoulos: https://en.wikipedia.org/wiki/John_Iliopoulos
[10] Pierre Fayet: https://en.wikipedia.org/wiki/Pierre_Fayet
[11] Joël Scherk: https://en.wikipedia.org/wiki/Jo%C3%ABl_Scherk
[12] Eugène Cremmer: https://en.wikipedia.org/wiki/Eug%C3%A8ne_Cremmer
[13] Jean-Loup Gervais: https://en.wikipedia.org/wiki/Jean-Loup_Gervais
[14] André Neveu: https://en.wikipedia.org/wiki/Andr%C3%A9_Neveu
[15] C. Itzykson and J.-B. Zuber, "Notes de Cours : Electrodynamique et théorie quantique des champs'', *Faculté des Sciences d'Orsay, Université de Paris* (1974-1976), 952 p. https://www.lpthe.jussieu.fr/~zuber/Z_publications.html (Consulted January 14, 2023.)
C. Itzykson and J.-B. Zuber, *Quantum field theory* (New York: McGraw-Hill, 1980).
[16] See, e.g., P. Charbonneau, *History of RSB Interview: Nicolas Sourlas*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.2a55p6c3
[17] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse

MM: [0:06:50] Yes.

PC: And so was Jean Vannimenus[18]?

MM: [0:06:54] Jean Vannimenus also, yes.

FZ: So, students who wanted to do statistical physics had the option to do so at École normale, but in that other lab.

MM: [0:07:04] Yes. In the lab of theoretical physics where I entered, there was not an option to start with statistical physics. The only way was to start with field theory, and then maybe move to another subject later on. You have to consider that all this group was very much influenced by the history of the previous two or three decades. They had created the lab, moving from Orsay to École normale (I think it was created 10 years before I joined). At that moment, there had been some bitter separation between various people doing theory. There were field theorists, on the one hand, and others which were more some other type of phenomenology, which was not the same. For the group that settled in École normale, field theory was really the building block. It was very important. It was understood that any respectable theorist should have a solid training in field theory, first of all. That's what I learned. [This attitude was very natural as this group had been very much involved in the development of the standard model in the previous two decades]

FZ: The statistical field theory part was developed in Saclay, if we understand…

MM: [0:08:38] We had, of course, in the DEA lectures of statistical physics. Édouard Brézin[19] was my teacher. As you can imagine, he was giving beautiful lectures. So, I have been exposed to the Ising model, to phase transitions, etc., but at least in our lab it was not considered a discipline in itself. It was a branch of theoretical physics that you could do after having done your classes in the mainstream. That's a bit of a different topic, but it took quite some time to have the possibility to have students joining this lab and studying directly condensed matter or statistical physics.

PC: At that point, you were working on perturbative QCD. What were the problems that you were specifically pursuing?

---

[18] "Jean Vannimenus," *Physics Tree* (n.d.). https://academictree.org/physics/peopleinfo.php?pid=777213 (Accessed February 14, 2023.)

[19] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Édouard Brézin*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.9573z1yg

MM: [0:09:36] During the *thèse de 3e cycle*, I did some work on perturbative QCD and on deep inelastic diffusion in neutrinos. It was computing cross sections for some specific type of observables. It was kind of an exercise. Unfortunately, the experiments could not be done at that time. They were done much later, so it was slightly frustrating for a young student in theory like me to work on something on which the experiment will not come soon. Then, when I moved on from the *thèse de 3e cycle* to the *thèse d'état*, we started another topic. It happens that Claude Bouchiat had done some very important work with his wife[20] on the measurement of parity violations in atomic physics[21]. That was very complementary to the big experiments on parity valuations because it was on a different scale of energy. It was a very important result. He told me: "Let us work on the violation of parity in solid state physics to see what could be measured." So, we started to search for effects of parity violation in solid state physics that might be measurable. That was the beginning of my *thèse d'état*. I must say that after one year of working on this topic, [after] I had heard some talks about topics of statistical physics, I went to Claude, and I told him that I wanted to stop working on parity violation because I thought that it would not work. You have an order of magnitude of parity violation which is 10^{-12}, and typically most experiments in condensed matter physics do not reach this level of precision. (We know rare exceptions, now, with the quantum Hall effect for instance.) It seemed to me that it would be very difficult to find an experimental situation that would allow to measure parity violation. So, I went to see Claude and I told him that I wanted to switch to statistical physics. He had a wonderful reaction. He told me: "I understand you. I think you're right. Go ahead. I am not an expert in statistical physics, but I'm ready to still be your advisor for your thèse d'état. You will come and report to me every second week, or something, about the progress, and we'll discuss." That's what we did. Claude was a very open mind, and he also belonged to that school and generation of theorists for whom theoretical physics was a whole. He had been working on theoretical physics applied to atomic physics, he had been working on weak interactions. Why not on statistical physics? There was no problem for him to encompass the full range of physics.

PC: You mentioned attending talks that exposed you to statistical physics. Do you remember any specifics?

---

[20] Marie-Anne Bouchiat: https://en.wikipedia.org/wiki/Marie-Anne_Bouchiat

[21] M.-A. Bouchiat and C. Bouchiat. "I. Parity violation induced by weak neutral currents in atomic physics." *J. Physique* **35**, 899-927 (1974). https://doi.org/10.1051/jphys:019740035012089900; "Parity violation induced by weak neutral currents in atomic physics. Part II." *J. Physique* **36**, 493-509 (1975). https://doi.org/10.1051/jphys:01975003606049300

MM: [0:13:14] Earlier than that choice, I remember a talk that was given at École normale by Ken Wilson[22]. It was important for me because it was illuminating. So far, I had been working with the renormalization group as it was used in particle physics, à la Callan-Symanzik[23]. I knew all that. But my impression, listening to Ken Wilson, was that I started getting an intuition about what it is. It was becoming a concept beyond a mathematical instrument—very powerful, that I already knew— a concept that I could feel, in some sense. I had also heard Giorgio Parisi[24] talk, but not on the topics that we discuss today. At that time, there were some interesting papers about large *N* matrix models. It was called the Eguchi-Kawai reduction[25]. There was a very nice talk by Giorgio about that, and I liked his approach[26]. It was field theory, but it was field theory with a stat mech tendency that I liked a lot.

PC: In your *thèse d'état*, you acknowledged Sourlas as having "amené à travailler sur les sujets abordés". How did that communication take place? Can you elaborate a bit on that?

MM: [0:15:06] When I decided to switch topic and move to statistical physics, I discussed with Nicolas Sourlas, obviously, because he was the person in the lab who was working on this kind of science. He had some suggestions, which probably we did not pursue, or which did not work, but it certainly was one of the persons to whom I would talk regularly. There was a major event for me in the summer or early September 1983. I had decided in the spring of 1983 to switch to statistical physics, and then arrived Miguel Virasoro[27]. He was on sabbatical. He arrived in Paris that summer. (I think in September, but I don't remember the month.) Miguel arrived and he said: "I want to spend my sabbatical to study what are the hot topics in statistical physics." He was, of course, extremely famous and well-known. He came with the aura of his algebra[28]. In lab like ours—with its

[22] Kenneth G. Wilson: https://en.wikipedia.org/wiki/Kenneth_G._Wilson

[23] Callan-Symanzik equation: https://en.wikipedia.org/wiki/Callan%E2%80%93Symanzik_equation

[24] See, *e.g.*, P. Charbonneau and F. Zamponi, *History of RSB Interview: Giorgio Parisi*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 80 p. https://doi.org/10.34847/nkl.7fb7b5zw

[25] T. Eguchi and H. Kawai, "Reduction of Dynamical Degrees of Freedom in the Large-*N* Gauge Theory," *Phys. Rev. Lett.* **48**, 1063 (1982). https://doi.org/10.1103/PhysRevLett.48.1063; Y. Makeenko, "Eguchi–Kawai model," In: *Methods of Contemporary Gauge Theory* (Cambridge: Cambridge University Press, 2002): 325-350. https://doi.org/10.1017/CBO9780511535147.022

[26] G. Parisi and Z. Yi-Cheng, "A modified Eguchi-Kawai model," *Phys. Lett. B* **114**, 319-323 (1982). https://doi.org/10.1016/0370-2693(82)90353-7

[27] P. Charbonneau, *History of RSB Interview: Miguel Virasoro*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 7 p. https://doi.org/10.34847/nkl.a941vym8

[28] Virasoro algebra: https://en.wikipedia.org/wiki/Virasoro_algebra

background on supersymmetry and supergravity—he was a big name. Someone told him: "There is this young student there, at the end of the corridor, who is starting to work on statistical physics." I was this young student and he told me: "Let's discuss together regularly. Let's see if we can study things together." I said: "Yes! Why not?" This is how things started. When we started studying, there was one important encounter in the corridor or around the coffee, where we met with Gérard Toulouse. I was with Miguel, we told him: "We want to move to problems of statistical physics. What do you think?" and Gérard told us: "You should try to read this paper by Giorgio Parisi on spin glasses. Nobody understands it, but it looks interesting. That's something for you." That's what we did. That was a very good advice.

PC: Did you know anything about spin glasses before reading that paper?

MM: [0:17:51] No.

PC: So, this was your first encounter with it.

MM: [0:17:56] Yes. Then, of course, we went to the library to read what we could find.

FZ: The paper in question is the one…

MM: [0:18:11] Last month, I was presenting that paper in the series of lectures in Rome about the historical papers of Parisi[29]. I said it's not one paper. It's actually a series of papers. There is one in *Physics Letters*[30], two or three in *J. Phys. A*[31], and there is one in *Phys. Rev. Letters*[32]. That's a collection of papers in which he does the one-step replica symmetry breaking, and then the full continuous symmetry breaking.

FZ: Is that what Toulouse was pointing you toward?

---

[29] *The interdisciplinary contribution of Giorgio Parisi to theoretical physics: A series of seminars bridging communities*, Sapienza University of Rome, Academic year 2022/2023. https://sites.google.com/gssi.it/giorgioparisiseminars (Accessed February 15, 2023.)

[30] G. Parisi, "Toward a mean field theory for spin glasses," *Phys. Lett. A* **73**, 203-205 (1979). https://doi.org/10.1016/0375-9601(79)90708-4

[31] G. Parisi, "A sequence of approximated solutions to the SK model for spin glasses," *J. Phys. A* **13**, L144 (1980). https://doi.org/10.1088/0305-4470/13/4/009; "The order parameter for spin glasses: a function on the interval 0-1," *J. Phys. A* **13**, 1101 (1980). https://doi.org/10.1088/0305-4470/13/3/042; "Magnetic properties of spin glasses in a new mean field theory," *J. Phys. A* **13**, 1887 (1980). https://doi.org/10.1088/0305-4470/13/5/047

[32] G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

MM: [0:18:43] Yes. He was mentioning that it looked interesting for the community of condensed matter physicists who had been looking at spin glasses, which was his case, of course. He had introduced this idea of frustration[33]. They had read this paper of Parisi. They did not understand what it was about, but they saw the result and they said: "Well, there might be something there. Maybe there's something interesting."

PC: Do you know where Gérard Toulouse's interest in the topic came from? How did Gérard get interested in this topic to begin with?

MM: [0:19:19] This, I don't know. You have to ask him.

PC: In practice, how did you learn Parisi's RSB method? Was there study group or was this on your own?

MM: [0:19:33] We learnt it by reading the papers and redoing the computation.

PC: You and Miguel together?

MM: [0:19:37] Yes. We would meet very regularly. I don't remember what the frequency was, but we met probably every day or every second day to discuss what we had read. I don't remember it as difficult. It was strange, but not difficult.

FZ: So, in 1983, Toulouse was still considering that the papers of Giorgio were not very understandable, even as a mathematical statement.

MM: [0:20:15] Certainly, yes.

FZ: I thought that what was missing at the time was the physical interpretation of RSB, but at least as a mathematical construction in '83 it would have been digested or somehow.

MM: [0:20:36] Not at all! The whole mathematical construction was very strange. There was the works of people discussing the interchange of the limits between number of replicas going to 0 and the thermodynamic limit. There was a lot of discussion. It was not at all accepted as a mathematical method. Even now, it is not a standard mathematical method, but it's a well-defined procedure and we know the physics that it encodes. The idea that was very important for us, the paper that probably was even more

---

[33] G. Toulouse, "Theory of the frustration effect in spin glasses: I," *Commun. Phys.* **2**, 115-119 (1977). Reprinted in Marc Mézard, Giorgio Parisi and Miguel Angel Virasoro, *Spin Glass Theory and Beyond* (Singapore: World Scientific 1987): 99-103.

influential rather than the series of papers on RSB was a paper by Giorgio—the one that appeared in PRL in '83[34]—in which he says: "The interpretation of the order parameter function is the overlap between pure states." That was a very illuminating paper. It is really from this paper that we started to work with Miguel, because that paper gave a clue to what could be encoded in the Parisi ansatz.

PC: You quickly got to collaborate beyond Miguel. You worked with Nicolas Sourlas, Gérard Toulouse and Giorgio Parisi on this series of papers that were to come[35]. Can you tell us how this collaboration came about? Was it really the two of you, Miguel and you, working together?

MM: [0:22:32] During this fall of 1983, it was with Miguel that we were working together. Then, Miguel—because we had started to work on Giorgio's papers and Miguel had just joined Rome as a professor—told me: "I will call Giorgio." So, he called Giorgio, and he told him that we were interested in these things. So, we started to interact not very frequently—there was no Zoom at that time, and no mail—by phone. Miguel would call Giorgio from time to time, and we started to discuss at distance with Giorgio. In particular, at some point, very soon, when we were reading the papers, Miguel came to me and said: "I had Giorgio on the phone. He says that there is something strange that happens when you take three points. It seems that two of them are always close." Then, we started to do the computation of the probability of three overlaps. Then, there was this property of ultrametricity. We didn't know the name. It was Rammal[36], who was in the group of condensed matter, who told us: "What you are describing is well-known in math: it is ultrametric[37]." Then, we started to decipher what this property of the triangle meant, namely the fact that the states could be seen as the leaves of a tree. At that time, we had developed with Miguel and Giorgio the full technology of how to ask questions about the physical space and answer them with replicas. In physical space, the weights of the states and the distances between the states, the P_α and the $Q_{\alpha\beta}$. We knew how to formulate these questions in the replica space and answer them. A big surprise that came and that we saw at that time was

[34] G. Parisi, "Order parameter for spin-glasses," *Phys. Rev. Lett.* **50**, 1946 (1983). https://doi.org/10.1103/PhysRevLett.50.1946. The manuscript was received on February 1, 1983, and was published on June 13, 1983.
[35] M. Mézard, G. Parisi, N. Sourlas, G. Toulouse and M. Virasoro, "Nature of the spin-glass phase," *Phys. Rev. Lett.* **52**, 1156 (1984). https://doi.org/10.1103/PhysRevLett.52.1156; "Replica symmetry breaking and the nature of the spin glass phase," *J. Physique* **45**, 843-854 (1984). https://doi.org/10.1051/jphys:01984004505084300
[36] Rammal Rammal: https://en.wikipedia.org/wiki/Rammal_Rammal
[37] Ultrametric space: https://en.wikipedia.org/wiki/Ultrametric_space

the fluctuations of the weight, the fluctuations of $P(Q)$, that we could compute.

PC: How did Toulouse and Sourlas come to join these discussions?

MM: [0:25:08] Toulouse and Sourlas had an important influence in the beginning in telling us: "This is an important subject." That was maybe the main one important contribution, and it is crucial.

PC: I thought that Toulouse had also been thinking about ultrametricity. But you said it was Rammal who knew about this and told you.

MM: [0:25:37] Rammal told us the name ultrametric. It did not take us very long to understand it, before we knew of the mathematical concept. The property of the triangle was surprising at first, but in order to deduce from this property the existence of a tree-like organization of the states it did not take much time. It was kind of natural, because you start to group the states in clusters at a certain distance and you relate the clusters that do not overlap, so you have this structure that ramifies. That was clear. We had, in some sense, all the properties that were needed before we knew the name.

PC: In the fall of '83, another visitor to ENS was David Gross[38], with whom you also started to collaborate at that point. Can you tell us how that came about?

MM: [0:26:35] It was another wonderful encounter. I was lucky in 1983 to meet Virasoro who told me: "Let's work together," and just a few months after something similar occurred with Gross. The encounter with him was in Saclay. We had done the work on the ultrametricity and the fluctuations in the weight, and I gave a talk in Saclay, in the theory group. In the back of the room, there was a guy who was asking a lot of questions during the talk. He came to see me at the end of the talk, and he told me: "I'm David Gross." I was impressed. I knew the name and his work, of course, because I had been studying high energy physics. He told me: "That's interesting what you have done etc. I'm spending now the first part of my sabbatical in Saclay, and the second part of my sabbatical I will spend at École normale. So, let us discuss this further when I come to École normale and see what can be done." That's how we started to discuss. It was because of this talk and his reaction. He had an instantaneous reaction that said:

[38] See, *e.g.*, P. Charbonneau, *History of RSB Interview: David Gross*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 16 p. https://doi.org/10.34847/nkl.dd4f3kf4

"That's beautiful! I want to know more about it." Then, we started to work together.

PC: Was he not involved in the group discussions with Miguel?

MM: [0:28:05] No. It is just when he joined École normale for the second part of his sabbatical that we started to work together very intensely. I don't remember where Miguel was, probably he had gone back to Rome at that time. In any case, we were discussing with David a lot. There was this idea of studying the random energy model[39]. Basically, the big surprise that we had at that time was to discover that the transition in the large *p* limit of the *p*-spin [model], which maps at large *p* to the random energy model, could be studied with the replica tools and the Parisi ansatz, but then the $Q(x)$ was discontinuous. There was no instability; there was no de Almeida-Thouless line. It was a discontinuous transition. We realized that it was a transition that was kind of first order in the sense of the discontinuity, but continuous from the point of view thermodynamics. Only a function order parameter could have these properties. That was the big novelty that we realized at that moment[40].

PC: Did you plan to keep on working with him after he left ENS? Was this the start of a larger program of collaboration in your mind?

MM: [0:29:40] I don't know. When he left École normale, he went to Israel, as part of his sabbatical was also in Israel. Then, he used the same kind of tools that we had used for the *p*-spin and applied them to the Potts model with large *q*, together with Haim Sompolinsky and Ido Kanter[41]. He did that, but he soon got back to his main topics. For him, it had been one year of excursion towards looking at other topics. Then, string theory caught him back!

PC: So, you didn't personally have a program of other things you wanted to do with him after he left. It was just one point collaboration.

MM: [0:30:32] It was a collaboration, but then everyone has his own topic that he wants to pursue.

PC: What was the immediate reaction to these two series of works? The one with Miguel and the one with David Gross?

[39] Random energy model: https://en.wikipedia.org/wiki/Random_energy_model

[40] D. J. Gross and M. Mézard, "The simplest spin glass," *Nucl. Phys. B* **240**, 431-452 (1984). https://doi.org/10.1016/0550-3213(84)90237-2

[41] D. J. Gross, I. Kanter and H. Sompolinsky, "Mean-field theory of the Potts glass," *Phys. Rev. Lett.* **55**, 304 (1985). https://doi.org/10.1103/PhysRevLett.55.304

MM: [0:30:50] The work on ultrametricity and the $P(Q)$ and the fluctuations, my impression is that it attracted a lot of attention of the colleagues. In the talks, in the seminars, people would ask a lot of questions. People were curious. It gave the impression that one started to understand new things, physical things, about the spin glass. I think it was well perceived. There was quite some excitement.

PC: Did you get invited to give talks in particular venues?

MM: [0:31:41] I think the organization of science was not exactly the same as now. Things would take a little bit of time. Gradually, I would be invited to give quite a few talks, yes. There were the Heidelberg colloquiums on spin glasses that were important moments. I was invited[42]. Yes, I was invited around, with the frequency of talks that existed at that time, which was a bit different from now.

FZ: The work with Gross, the understanding of the discontinuous transition in the random energy model, and the *p*-spin, how was it received? I understand that your motivation was to explore the space of possibilities for spin glass models.

MM: [0:32:38] Yes and no. We didn't know that it would give that. Our first motivation with David was to say: "Well, we have this replica method with replica symmetry breaking à la Parisi, on the one hand, we have a solvable model which has been found by Derrida[43], [on the other hand]. Can we match the two? What can we do with these two things? Can we test this replica method, this RSB method? Can we test it versus a solvable model?" That's what we wanted to do.

PC: From the start?

MM: [0:33:14] Yes, that's what we wanted to do when applying it to the REM. The REM was understood. At some point there was, for instance, the computation of the entropy in the condensed phase of the REM. This is a subtle result that has to do with the statistics of the weight of the valleys. It's not an extensive entropy, so it's a bit delicate. Derrida had done it

---

[42] M. Mézard, N. Sourlas and G. Toulouse, "Some remarks on ultrametricity," In: J. L. van Hemmen and I. Morgenstern eds., *Heidelberg Colloquium on Glassy Dynamics. Lecture Notes in Physics* **275**. (Berlin: Springer 1987). https://doi.org/10.1007/BFb0057520

[43] P. Charbonneau, *History of RSB Interview: Bernard Derrida*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.3e183b0o

directly with probabilistic tools, and when we did it with the replicas[44], we found the same result. So, that was a very strong indication that even on a subtle thing like a finite contribution to the entropy, the replica method was giving the right result.

FZ: So, for you, the motivation was methodological. It was to understand how you could do replicas without replicas.

MM: [0:34:12] It was to understand whether we could test this RSB approach in a case in which everything was understood. It had to be tested. There was still enough mystery, even if we started to understand what it could encode, with the tree of states and ultrametricity, it did not mean that it was exact, that it was right. With the REM, we had one solvable model in which we could test it and it worked. *En passant*, it showed a very strange phenomenon, which is a mixture of a first order and a second order transition.

FZ: What was your reaction to that and the reaction of the community to this new phenomenon? Did some consider it as important, or was it just a curiosity or an exotic thing?

MM: [0:35:26] It was considered as a kind of landmark. I think it was very well received. In retrospect—seeing it from now—it seems to me that it is a kind of landmark… We all know that there were quite a few elements in favor of Parisi's solution: the fact that it had cured the problem of the negative entropy and that we could interpret the zero-field cooled and field-cooled magnetizations [and it was marginally stable]. But from the point of view of theoretical physics and math, that was a solid anchor when we could say: "You see, it is a method. It is strange, yes, you have to go to this number of replicas which is zero, and there is a negative thing et cetera, but if you apply it to this *p*-spin model that you can solve with all the standard tools of mathematics you get the right result, with a $q(x)$ function that is one step."

FZ: Just to finish on this: I think there is some link that we are a bit missing between what you did in '84-'85 of the on *p*-spin model, what Gross did later on the Potts model, and then how this arrived somehow to Kirkpatrick, Thirumalai and Wolynes[45] two or three years later. It's not

[44] B. Derrida, "Random-energy model: Limit of a family of disordered models." *Phys. Rev. Lett.* **45**, 79 (1980). https://doi.org/10.1103/PhysRevLett.45.79; "Random-energy model: An exactly solvable model of disordered systems," *Phys. Rev. B* **24**, 2613 (1981). https://doi.org/10.1103/PhysRevLett.45.79

[45] See, *e.g.*, T. R. Kirkpatrick and P. G. Wolynes, "Stable and metastable states in mean-field Potts and structural glasses," *Phys. Rev. B* **36**, 8552 (1987). https://doi.org/10.1103/PhysRevB.36.8552; T. R. Kirkpatrick and D. Thirumalai, "Mean-field soft-spin Potts glass model: Statics and dynamics," *Phys. Rev. B*

clear to us, how these ideas evolved and reached them. When we asked the question to Thirumalai[46], he didn't really recall how your result arrived to them.

MM: [0:37:23] I don't know at all. It was a time when there was no Web, there was no email, no Zoom. People read the papers. Maybe David, when he got that back to the US gave some talk on these topics, or people read it in the papers. I have no idea.

PC: Bernard Derrida and Elizabeth Gardner were also working on RSB-related ideas at that same time[47]. Were you in touch with them and did you discuss the work on the REM with them at that time?

MM: [0:38:12] I don't remember it particularly, but I am sure that I must have discussed it with Bernard at some point. I must have told him: "This is what we find." At that time, he was still in Saclay probably. I'm pretty sure that we must have met at some point, but I don't remember it.

PC: So, you don't remember his reaction?

MM: [0:38:52] Bernard was certainly interested in what we were doing. He told me several times. He had his own way of doing things, but he understood that our way was also an interesting way.

PC: You also worked on a statistical mechanics of optimization in collaboration with Jean Vannimenus at roughly the same time[48]. How did this other topic and collaboration come about?

MM: [0:39:27] With Jean Vannimenus, we started working just before the summer of 1984. There had appeared the paper by Kirkpatrick, Gelatt and

---

37, 5342 (1988). https://doi.org/10.1103/PhysRevB.37.5342; D. Thirumalai and T. R. Kirkpatrick, "Mean-field Potts glass model: Initial-condition effects on dynamics and properties of metastable states," *Phys. Rev. B* **38**, 4881 (1988). https://doi.org/10.1103/PhysRevB.38.4881

[46] P. Charbonneau, *History of RSB Interview: Dave Thirumalai*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 19 p. https://doi.org/10.34847/nkl.a03aux8z

[47] See, *e.g.*, E. Gardner, "Spin glasses with *p*-spin interactions," *Nucl. Phys. B* **257**, 747-765 (1985). https://doi.org/10.1016/0550-3213(85)90374-8; B. Derrida, "A generalization of the random energy model which includes correlations between energies," *J. Physique Lett.* **46**, 401-407 (1985). https://doi.org/10.1051/jphyslet:01985004609040100; B. Derrida and E. Gardner, "Solution of the generalised random energy model," *J. Phys. C* **19**, 2253 (1986). https://doi.org/10.1088/0022-3719/19/13/015

[48] J. Vannimenus and M. Mézard, "On the statistical mechanics of optimization problems of the travelling salesman type," *J. Physique Lett.* **45**, 1145-1153 (1984). https://doi.org/10.1051/jphyslet:0198400450240114500

Vecchi on simulated annealing that appeared in 1983[49]. It is because of the Kirkpatrick-Gelatt-Vecchi paper that we that we started to think about the traveling salesman. We understood that this was an instance of a disordered system that could be interesting for us. Certainly, if the numerical tools could be applied to it, one could also apply analytical methods. We started to work a little bit on that with Jean. I remember the date, because the summer in 1984 is when I moved to Rome as a postdoc. I went to Rome from summer '84 to summer '86—two years as a postdoc—and then came back to Paris and got back to Rome another six months. So, I spent two and half years there.

I remember that very well because when I arrived in Rome, in the fall of '84, I started to discuss with Miguel and Giorgio. I told Giorgio: "There is this optimization problem." I told him what we had looked at with Jean. With Jean, we had done a relatively small study. It was a study for starting to get familiar with the topic. It's at that moment that I started to learn what is NP-completeness[50]. Then, Giorgio told me: "Oh! That's very interesting. Let's work on that." So, we started to work with Giorgio at that moment. That's a topic I remember because I had started it with Jean, and then moving to Rome we started to work on that with Giorgio[51].

PC: Was Jean Vannimenus familiar with that problem before? Where did his interest come?

MM: [0:41:51] Like mine: it was curiosity. This paper of Kirkpatrick-Gelatt-Vecchi, I don't know how many tens of thousands of citations it has now, but it was clear from the beginning that it was an interesting paper. It showed that, what for us was a pure statistical physics method—Monte Carlo with decreasing gradually temperature—was a very versatile tool that you could apply to many other systems. I don't know if you remember, but in that paper there is a kind of proof of concept of simulated annealing, in which they do a simulation of a Travelling Salesman Problem. Then, we said: "What a nice problem, this traveling salesman." It was just a curiosity.

PC: So, it's a coffee discussion that led to the collaboration.

---

[49] S. Kirkpatrick, C. D. Gelatt Jr. and M. P. Vecchi, "Optimization by simulated annealing," *Science* **220**(4598), 671-680 (1983). https://doi.org/10.1126/science.220.4598.671

[50] NP-Completeness: https://en.wikipedia.org/wiki/NP-completeness

[51] See, *e.g.*, M. Mézard and G. Parisi, "Mean-field equations for the matching and the travelling salesman problems," *Europhys. Lett.* **2**, 913(1986). https://doi.org/10.1209/0295-5075/2/12/005; "A replica analysis of the travelling salesman problem," *J. Physique* **47**, 1285-1296 (1986). https://doi.org/10.1051/jphys:019860047080128500

MM: [0:42:47] I don't remember who first saw the paper by Kirkpatrick-Gellatt-Vecchi—[maybe Gérard Toulouse]—but once it was on our desk we considered it very interesting. For us it was natural. I was immersed into spin glasses, but I could see these new analytical tools able to study the statistical physics of many types of disordered systems. Then, came this paper which was applying numerical tools to new kinds of disordered systems. So, making the junction between the two was very natural.

PC: Before we move to the work in Rome, I'd like to get an idea of what was the general setup of the statistical mechanics community in Paris. We've talked about a few people you were interacting with, but there were others: Cirano De Dominicis[52], Henri Orland[53] and others. How did it function? How would communications take place?

MM: [0:43:57] There was a strong group in Saclay with Cirano, Henri Orland and Jacques Descloizeaux[54], who was doing polymer physics, and also Itzykson[55]. There was a big group in Saclay. In École normale, in the theoretical physics lab, there was Sourlas, in the condensed matter lab, there was Gérard Toulouse and Jean Vannimenus. There was no structure at ENS in the sense that there was no joint seminar or journal club on these topics. It was more individual choices. Our activity was very much respected by the colleagues in our lab, but it was still marginal with respect to the mainstream. It was both marginal and respected, and maybe also respected as marginal. For instance, for the jury of my *thèse d'état*, Claude Bouchiat suggested to ask Jeffrey Goldstone[56] to be part of the committee. Jeffrey was visiting École normale at that moment. He was not at all in statistical physics, but he was again one of these guys of the old school of theoretical physics for whom statistical physics of disordered systems was a perfectly suitable topic in theoretical physics. He would study it by reading the thesis, and he was extremely positive. I had a great time discussing the thesis with him.

PC: About the interactions with Saclay. How often would you typically see each other? Every month or every three months? On what occasions would that take place?

---

[52] Cirano de Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis

[53] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Henri Orland*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 18 p. https://doi.org/10.34847/nkl.1d000dgs

[54] See, *e.g.*, J. Descloizeaux and G. Jannink, *Les Polymères en solution : leur modélisation et leur structure* (Paris: Éditions de Physique, 1987).

[55] Claude Itzykson: https://en.wikipedia.org/wiki/Claude_Itzykson

[56] Jeffrey Goldstone: https://en.wikipedia.org/wiki/Jeffrey_Goldstone

MM: [0:46:14] There was no regular occasion. There would be some seminar or conferences at which we meet, but not much more than that. At that time, I didn't go to Saclay, except when I was invited for a talk or a conference.

FZ: Since you mentioned that there were all these people who respected and supported this effort. Did you also meet some resistance or opposition from people who considered it to be not worthy of theoretical physics interest?

MM: [0:46:55] No. Not in that area. The theoretical physicists with whom I was discussing were immediately considering that it was an important step. I did not feel any reluctance from their part. The debate came a bit later over the Atlantic. I don't have the dates in mind. But when Fisher and Huse discussed their models of droplets[57], the scientific debate was much more tensed. The debate was precisely about the replica solution. Initially, it was whether replica symmetry breaking could make sense at all. Gradually, it drifted towards: "Okay. Maybe it makes sense, but it does not apply to finite dimensional spin glasses, and certainly not in dimension 3."

PC: We'll get back to this, but first you went to Rome, in 1984. What were you specifically hoping to learn or achieve in Rome? Why Rome? Why then?

MM: [0:48:26] Rome, you have to understand, was an original choice at that time. In that community of theoretical physics in Paris, typically people went as postdoc to the US. That was the default choice. I don't think that anyone had gone to Rome before me. So, it was a very unusual choice from this point of view. At the same time, Giorgio Parisi and Miguel Virasoro were extremely respected colleagues. So, when I said: "I would like to do a postdoc with them in Rome," everybody said: "Oh yeah! What a good idea!". After I decided to go to Rome, the first set of European fellowships for postdocs within Europe was created (it was not yet the Marie Curie program[58]), and I got one of these. There were very few fellowships probably in that time.

What was I looking for? I don't know. I just had started for a year, or something like that, this wonderful collaboration with both Miguel and Giorgio. I had gone to Rome a little at the beginning of 1984. We were working with Giorgio on the correlation functions of the SK model[59], and I

---

[57] See, *e.g.*, D. S. Fisher and D. A. Huse, "Ordered phase of short-range Ising spin-glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601

[58] Marie Skłodowska-Curie Actions: https://en.wikipedia.org/wiki/Marie_Sk%C5%82odowska-Curie_Actions

[59] M. Mézard and G. Parisi, "Self-averaging correlation functions in the mean field theory of spin glasses," *J. Physique Lett.* **45**, 707-712 (1984). https://doi.org/10.1051/jphyslet:019840045014070700

had gone to Rome in order to work with him for one week or 10 days. So, I had seen him at work on a daily basis also. For me, the choice of going to work with Giorgio and Miguel was obvious. I really was not looking for anything in particular except continuing this collaboration with these two guys. Of course it was a wise choice, but it did not require much wisdom: it was obvious that this was the thing to do.

PC: You said you decided to go to Rome before even you had a fellowship. Had they invited you?

MM: [0:50:53] They invited me, yes. Miguel had organized this.

PC: I think that Giorgio was then at Roma Tre and Miguel at La Sapienza. Can you describe how it was to work with Giorgio and Miguel, in terms of both content and logistics?

MM: [0:51:19] I would work with Miguel very frequently. We would meet nearly every day. He had some teaching to do, but every day or every second day we could meet. We would see Giorgio quite regularly. A bit less frequently, because he was in Roma Tre, but he would come very often to La Sapienza. I don't remember the frequency, but I would say that we met with Miguel every second day and with Giorgio at least once a week.

PC: The traveling salesman problem that you started to work with Miguel and Giorgio once you go to Rome is a problem in theoretical computer science originally. What was the reception of that community to your work at that point? Were they aware of it? If yes, how?

MM: [0:52:18] No. They were not much aware of it. I should make more precise one point that you said. It's really with Giorgio that I was walking on the traveling salesman. With Giorgio, we have done several things. One was about the matching problem[60], and one was about the traveling salesman problem. One result that attracted attention later was on the random matching problem, for which we could compute the ground state. Precisely, we could compute the expectation value of the length of the optimum matching, $\pi^2/12$. It was a result of a rather long computation with replicas, and that attracted the attention not so much of the practitioners of the thing but of probabilists. There was in particular, Aldous[61], a famous probabilist. When he saw our result, he thought: "They must be wrong." He knew the existence of a simple-minded reasoning,

[60] M. Mézard and G. Parisi, "Replicas and optimization," *J. Physique Lett.* **46**, 771-778 (1985). https://doi.org/10.1051/jphyslet:019850046017077100

[61] David Aldous: https://en.wikipedia.org/wiki/David_Aldous

which is wrong, but which gives this $\pi^2/12$, and he thought that this is what we were doing. I knew that. But then, gradually he got to understand what we had done, and a few years later he actually proved that our result was correct[62]. There was no immediate absorption by this other community of these methods. Not at all.

PC: Or even the results?

MM: [0:54:12] Or even the results. Not so much.

PC: So, you didn't go to conferences about theoretical computer science or probability theory to discuss these?

MM: [0:54:20] No.

PC: Did you have exchanges with Aldous by mail? Or did he just read your paper?

MM: [0:54:26] I think he read our paper, and I read his paper when he published the proof.

PC: There were others, who tried similar approaches on similar problems at about the same time, such as Fu and Anderson[63]. How closely were you following these advances? What was your reaction to these parallel efforts?

MM: [0:54:51] Yes. Phil Anderson with Fu, they had written this paper that was mapping the SK model to the problem of graph partitioning, as far as I remember. It's long ago, but I still remember well the paper. It was clever because it was a direct correspondence. I thought that it was nice. It also shows that we were not the only ones realizing that the methods from spin glasses could be used for other systems. How we knew about it? Through the papers.

FZ: Henri Orland also had a paper in '85 about similar issues[64]. Were you in touch with him, or was it also independent?

---

[62] D. J. Aldous, "Asymptotics in the random assignment problem," *Prob. Th. Rel. Fields* **93**, 507-534 (1992). https://doi.org/10.1007/BF01192719; "The ζ (2) limit in the random assignment problem," *Rand. Struct. Alg.* **18**, 381-418 (2001). https://doi.org/10.1002/rsa.1015

[63] Y. Fu and P. W. Anderson, "Application of statistical mechanics to NP-complete problems in combinatorial optimization," *J. Phys. A* **19**, 1605 (1986). https://doi.org/10.1088/0305-4470/19/9/033

[64] H. Orland, "Mean-field theory for optimization problems," *J. Physique Lett.* **46**, 763-770 (1985). https://doi.org/10.1051/jphyslet:019850046017076300

MM: [0:55:48] I remember that. It was really independent.

FZ: So, the idea was appearing in several places all at once.

About this debate and criticism of RSB, you said it came later with the droplet paper, but the idea that you could use replica symmetry breaking to get some statement on optimization was less criticized at that point. I think in the paper of Fu and Anderson they have a very clear statement where they say: "The RSB solution of the SK model is important for this kind of problems." Was it something accepted or not?

MM: [0:56:43] Maybe not by mathematicians, but with respect to what I said before about the situation in '83, by that time—in '86 or something like that—the validity of Parisi's solution of replica symmetry breaking started to be accepted. There had been confirmations with the REM. There had been several things. It was not a mathematical statement, but people who were very reluctant at the beginning started to see what it means, what it contains. All this work that we did in order to decipher what is the physical content of Parisi solution was important in this respect. So, people can accept that it is a very strange and subtle way of encoding physical reality that makes sense, that you can compare, that you can kind of test with numerical simulations etc.

FZ: Is it fair to say in '85-'86, at least for the Sherrington-Kirkpatrick model and some optimization problems, this tool was accepted as interesting?

MM: [0:58:07] Yes. I think so.

PC: Another direction you worked on while in Rome is to formulate a replica-less, or cavity solution, of the SK model[65]. Was this effort in part a response to criticism that physicists were making about replica symmetry breaking? Or was it driven by a formal interest in getting to a mathematical proof?

MM: [0:58:35] We really wanted to obtain an alternative construction. We thought that replicas and RSB were a beautiful tool, but that there should be another way, more direct, in which we could understand the SK model. We probably did not have in mind so much the mathematical rigor. At that time, if you had asked me, I would probably have said: "Someone will come up with a mathematical framework in which RSB will become obvious." What I had in mind at that time was that it would be like for distributions.

[65] M. Mézard, G. Parisi and M. A. Virasoro, "SK Model: The Replica Solution without Replicas," *Europhys. Lett.* **1**, 77 (1986). https://doi.org/10.1209/0295-5075/1/2/006

You have the theory of distributions and Dirac's delta function[66]. Physicists do it like that. It's a bit dirty, but it works very well. Then, at some point, you have someone, [Laurent Schwartz[67]], who comes with a mathematical framework and who says: "Within the framework of linear forms and sufficiently regular functions it is fully consistent." And that's it. I thought that it would be like that. That it would be just necessary to find the right space in which replica symmetry breaking applies naturally. A that time, the way to build a mathematically solid solution of the SK model, it could have been through replicas. But we wanted to have a direct solution, more physical.

PC: There's another group in parallel that was working on formulating a direct solution on a Bethe lattice: Thouless, the Chayes, and Jim Sethna[68]. Were you following this effort? Were you at all in touch with them?

MM: [1:00:27] Not at all. We were probably not so much aware of that. For us, the idea of adding one spin to the SK model and seeing what happens self-consistently was very natural. I remember so many times Giorgio—when I arrived in Rome—on the blackboard saying: "Let us add one spin here." The thing that was missing—that was a crucial ingredient—was the distribution of free energies. There, the work that I had done with Gross on the REM was quite useful. At some point we realized that all the properties of the free energies that were encoded in the weight distribution was as if the finite fluctuations of free energies were independent and were exponentially distributed. The first paper to build the cavity method with Giorgio and Miguel[69]—before the paper on the cavity method—was about random free energies in spin glasses. That was crucial, because the big problem that we had was: "Yes, you add one spin, so you have a Gaussian local field, but then you need to take into account of reshuffling of the free energies of the states." With this reshuffling, only an exponential distribution of the free energies is stable when you add one new spin. That was the crucial point: mixing some kind of standard central limit theorem together with the exponential distribution of the free energies. What we understood at that time was that this exponential distribution was encoding all the properties of the weights. It was like the

[66] See, *e.g.*, M. G. Katz and D. Tall, "A Cauchy-Dirac delta function," *Found. Sci.* **18**, 107-123 (2013). https://doi.org/10.1007/s10699-012-9289-4; J. Lützen, *The Prehistory of the Theory of Distributions* (New York: Springer-Verlag, 1982).
[67] Laurent Schwartz: https://en.wikipedia.org/wiki/Laurent_Schwartz
[68] See, *e.g.*, J. T. Chayes, L. Chayes, J. P. Sethna and D. J. Thouless, "A mean field spin glass with short-range interactions," *Comm. Math. Phys.* **106**, 41-89 (1986). https://doi.org/10.1007/BF01210926
[69] M. Mézard, G. Parisi and M. A. Virasoro, "Random free energies in spin glasses," *J. Physique Lett.* **46**, 217-222 (1985). https://doi.org/10.1051/jphyslet:01985004606021700

Poisson-Dirichlet distribution[70]. It got some other name later on, but we understood most of it at that moment. That was the step that opened the gate for developing the cavity method.

PC: You described a lot how this work as "we"—you working with Miguel. You also described how you start working with him in that you both wanted to learn the same thing. Did it always feel like a collaboration? Were there mentors and students? How was it to work with Giorgio and Miguel at that point?

MM: [1:03:12] I never felt like I was student. I've always felt that it was just a collaboration. That's one of the beautiful things of theoretical physics: the social organization of theoretical physics. You can have a student that comes, and if she has ideas, you are equal… I think we were just a group. The social organization of science was quite different. At Rome, at that moment, we were the three of us. Yes, there were the other groups, there were other people, but you did not have the organization in one big group with postdocs and students. There was no structured PhD program at that time. There were no postdocs, or very few. It was not at all pyramidal. There was not a big group with a group leader. Not at all. It was just us in an office.

PC: Were there anyone else working with the three of you, with whom you'd be talking? Or was it just the three of you in isolation?

MM: [1:04:20] That was enough.

PC: Absolutely!

MM: [1:04:23] In the sense that we were happy with that. We were just very happy to work this way.

FZ: During your time in Rome, did you not interact with other people in Rome doing statistical physics like Jona-Lasinio[71]?

MM: [1:04:42] I would discuss with them in the corridor, tell them what I'm doing, but we were quite busy already. There was a lot of work to be done. There are quite a few papers. Maybe now one can explain all these works in a few hours on the blackboard, but there were quite a few strange things to discover. So, it was keeping me busy.

[70] Poisson-Dirichlet: https://en.wikipedia.org/wiki/Poisson-Dirichlet_distribution
[71] Giovanni Jona-Lasinio: https://en.wikipedia.org/wiki/Giovanni_Jona-Lasinio

PC: Throughout this time, when did the idea of writing *Spin Glass Theory and Beyond* emerge[72]? And why did you think it was the right time to write such a book?

MM: [1:0522] It came during my postdoc in Rome. There was the cavity method, on the one hand. About the cavity method, there is one anecdote that is interesting by the way. We were very excited that we had found a way to understand the level crossing of these pure states with the cavity. We did all the computation and we found with the cavity all the results of the replicas. So, we were enthusiastic. The week after, both Miguel and I, arrived and said: "I've drafted the first draft of the paper." So, we had two first drafts of the paper. What we did, which was a mistake in retrospect, is that we mixed the two drafts. This is probably why this paper on the cavity method is difficult to read. It turned out that the two drafts were not the same. All the formulas were the same, but the way we were thinking about the computations were different. I always take this as an example of the fact that you can work nearly every day with someone, but when it comes to writing you will have something which is different. We decided to write *The Beyond*, because… We explain it in the introduction to the book. We thought that, on the one hand, there was a full set of methods that was worth describing, and on the other hand, there started to be at least two main fields where you could provide interesting applications: optimization and neural networks. In neural networks, the Hopfield paper was '82[73] and Amit-Gutfreund-Sompolinsky was '85[74]. We started to work on the book in '86 probably.

PC: So, towards the end of your time in Rome.

MM: [1:07:38] Yes. Because we were just immersed in the cavity method, the original part of the presentation of the book was more focused on the cavity method.

PC: How did it take place? Did the three of you provided equal contributions? Or were you convincing your colleagues to write the book?

[72] Marc Mézard, Giorgio Parisi and Miguel Angel Virasoro, *Spin Glass Theory and Beyond* (Singapore: World Scientific, 1987).

[73] J. J. Hopfield, "Neural networks and physical systems with emergent collective computational abilities," *Proc. Nat. Acad. Sci. U.S.A.* **79**, 2554-2558 (1982). https://doi.org/10.1073/pnas.79.8.2554

[74] D. J. Amit, H. Gutfreund and H. Sompolinsky, "Storing infinite numbers of patterns in a spin-glass model of neural networks," *Phys. Rev. Lett.* **55**, 1530 (1985). https://doi.org/10.1103/PhysRevLett.55.1530; "Spin-glass models of neural networks," *Phys. Rev. A* **32**, 1007 (1985). https://doi.org/10.1103/PhysRevA.32.1007

MM: [1:08:08] No. All of us were agreeing to write the book. It was not hard to write this book, because the part that we wrote ourselves is not that extensive. It's 80 pages or something. So, it was an easygoing collaboration. I don't really remember how it was organized. I remember that it was done on the Macintosh. That was a big revolution with respect to the previous years. I always take this example that, of course, I have lost the disks on which I have written that. So, I have no computer copy of my book, except the scanned copy by some pirates who put it on the web.

PC: Upon your return to Paris, you started to work on neural networks with your first thesis student, Werner Krauth[75], and with Jean-Pierre Nadal[76]. How did you recruit Werner as a student to work on this topic, as a junior independent researcher at that point? And how did the collaboration with Jean-Pierre come about?

MM: [1:09:26] Werner was a student in Paris with a *Studienstiftung* Fellowship[77]. He was a very great student, and he was looking for a PhD. I proposed something to him, and it went very easily this way. It is true that at that time—around '86-'87—neural networks were a hot topic, associative memory networks. So, that was a natural collaboration.

Jean-Pierre Nadal was also very much interested in that topic. With Jean-Pierre Nadal and Gérard Toulouse[78], we were involved in a lot of discussions with colleagues in neurobiology. It was an interesting time also from that point of view. There were contacts between us and people in neurobiology, with people in psychology. For instance, at that time I met with Jacques Mehler[79], who is an expert on how children learn the languages and experiments on that. We developed contacts with Jean-Pierre Changeux[80]. There was the young Stanislas Dehaene[81], who was a student of Changeux, who was also participating in these discussion

---

[75] Werner Krauth, *Physique statistique des réseaux de neurones et de l'optimisation combinatoire*, Thèse de doctorat, Université Paris-Sud (1989). http://upsaclay.focus.universite-paris-saclay.fr/permalink/f/1gllaij/33PUP_Alma_UNIMARC21165653370006051

[76] See, *e.g.*, W. Krauth and M. Mézard, "Learning algorithms with optimal stability in neural networks," *J. Phys. A* **20**, L745 (1987). https://doi.org/10.1088/0305-4470/20/11/013; "Storage capacity of memory networks with binary couplings," *J. Physique* **50**, 3057-3066 (1989). https://doi.org/10.1051/jphys:0198900500200305700; W. Krauth, J.-P. Nadal and M. Mézard, "The roles of stability and symmetry in the dynamics of neural networks," *J. Phys. A* **21**, 2995 (1988). https://doi.org/10.1088/0305-4470/21/13/022

[77] Studienstiftung: https://en.wikipedia.org/wiki/Studienstiftung

[78] See, *e.g.*, M. Mézard, J.-P. Nadal and G. Toulouse, "Solvable models of working memories," J. Physique **47**, 1457-1462 (1986). https://doi.org/10.1051/jphys:019860047090145700

[79] Jacques Mehler: https://en.wikipedia.org/wiki/Jacques_Mehler

[80] Jean-Pierre Changeux: https://en.wikipedia.org/wiki/Jean-Pierre_Changeux

[81] Stanislas Dehaene: https://en.wikipedia.org/wiki/Stanislas_Dehaene

groups. It was a very open time from this point of view. There was both the development of neural networks, as we know it from physics, but it also created a lot of interfaces with various communities. It was the beginning of what was called cognitive science in France, with several cognitive science programs. So, I had many occasions to discuss with colleagues from completely different horizons. Also, people from computer science using neural network like the group of Dreyfus[82], for instance. That was a very rich time. The thing that did not realize very well, that did not get concrete enough, was the collaboration with the biologists, with Changeux. It did not really work. We met regularly for discussions, but we could not turn it into a long-lasting scientific collaboration.

PC: We were just discussing the work on neural networks. One of the key early meetings was in Jerusalem[83]. Were you there?

MM: [1:12:27] Yes, I was there. I spent a month or so in Jerusalem at this meeting organized by Amit, Gutfreund[84] and Sompolinsky as far as I remember. It was a very important meeting.

PC: What do you remember from that meeting? What was being discussed?

MM: [1:13:54] Very clearly, part of the meeting was more about the interface of neural network with neurobiology. It was both theoretical physics developments and the interface to neurobiology. I told you that there were other interfaces that were interesting, like cognitive science. But neurobiology was clearly the main interest.

There are two papers by Amit Gutfreund and Sompolinsky. The first one, I think, is about the finite number of patterns. Then, there comes the paper with the infinite number of patterns with a memory threshold. We were immediately interested by the first paper. I think I had started on the extensive number of patterns work when I saw the first paper, but they were already on the track in 1984 and they finished it before me. It was clearly something that, as soon as we knew about that, it was attracting quite some attention.

PC: I think that Elizabeth Gardner also was there and Bernard Derrida. Were they there at the same time as you? Were you discussing?

[82] Gérard Dreyfus: https://en.wikipedia.org/wiki/Gerard_Dreyfus
[83] D. Amit, H. Gutfreund, H. Sompolinsky, Israel Institute of Advanced Studies, Jerusalem, Israel, 1987.
[84] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Hanoch Gutfreund*, transcript of an oral his- tory conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 16 p. https://doi.org/10.34847/nkl.1adb9r42

MM: [1:14:22] I do not remember. I remember meeting with Elizabeth and Bernard in Paris, in Saclay. That is clear. I don't remember discussing with them during that meeting. I discussed a lot with Daniel Amit[85] in that workshop.

PC: There was another meeting at about the same time at the Institute for Theoretical Physics, in Santa Barbara, on a similar theme[86]. Were you at that meeting as well? Peter Young[87] was a co-organizer.

MM: [1:14:51] I don't remember it; I don't think I went there.

PC: In 1990, you published about the 1/*d* expansion for hypercubic lattices with Antoine Georges and Jonathan Yedidia[88]. How did this other direction come about? And how closely were you following the fine-*d* discussion about droplets at that point?

MM: [1:15:23] To me, honestly, as early as 1985, I considered that the main importance of what we had done on the SK model was its interfaces with other disciplines. It was *The Beyond*, the part which was the beyond that I thought most important. Even if replica symmetry breaking did not apply at all to real spin glass, never mind! It was a big thing in itself. That's why I worked in all the directions that were *The Beyond* directions: optimization, biology etc. That, to me was the most interesting part of it. Of course, simultaneously I have always been interested in seeing what happens in 3D. For the 3D problem, there was, of course, the droplet model. It became clear rather soon that the final word—if there would be a final world—could come from simulation. I learnt to do computer simulations from Giorgio. I had learnt about programming when I was at École normale, as a student. But then it was a very complicated process, because you had to write the program on a piece of paper and ask one person who was preparing the cards to fill in the machine. Then, the machine would take a long time to answer you that there is a bug in the program etc. It was a very long process. But when I went to Rome as a postdoc, we started to have terminals, and there was Giorgio who was already quite good and had a way of thinking in terms of numerical computation. I started redoing

[85] Daniel Amit: https://en.wikipedia.org/wiki/Daniel_Amit

[86] John Hopfield and Peter Young, "Spin Glasses, Computation, and Neural Networks," September to December 1986 Institute for Theoretical Physics, University of California at Santa Barbara.

[87] See, *e.g.*, P. Charbonneau, *History of RSB Interview: A. Peter Young*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.2fef8760

[88] A. Georges, M. Mézard and J. S. Yedidia, "Low-temperature phase of the ising spin glass on a hypercubic lattice," *Phys. Rev. Lett.* **64**, 2937 (1990). https://doi.org/10.1103/PhysRevLett.64.2937

numerical computation with him and since that moment I never stopped. I always consider it as a very nice tool. But I never wanted to go into large-scale numerical simulations. I'm not enough organized in some sense. I'm losing the files. That's why, for the 3D spin glass, we tried this $1/d$ expansion. It made sense with respect to what we knew about the SK model, to see if we can extend it through a $1/d$ expansion. But I did not push that much. I did one more paper on that with Olivier Martin and Florent Krzakala[89], but that was not very much.

PC: Was it your idea to do this expansion? It's sort of a different direction altogether from what you had done before.

MM: [1:18:44] This paper with Georges and Yedidia… Yes, Jonathan at that moment was a visiting us; he was a PhD student of Phil Anderson[90], and he was spending one year in Paris. In some sense, I was his supervisor during his stay in Paris, but in fact that wasn't really like that. He didn't need any supervision because he was very independent and autonomous. So, he came, and he started to discuss about doing something on the finite-dimensional spin glass. That's how it started. For me, it has never been a mainstream line of research, but it's nevertheless something that I consider interesting. If I had a bright idea that could solve 3D spin glass, I can tell you that I would stop everything else and do it right now, but this idea didn't ever materialize.

PC: By the early 1990s, you largely left the field of neural networks. What drew you away from these questions?

MM: [1:19:54] I was not the only one. A large fraction of the physicists who had been interested in neural networks stopped also around that time. There were several ingredients. An ingredient was that the interaction with biologists—in our case, in Paris—did not work well. I told you it did not materialize into anything that could become concrete. There were tensions also between Gérard Toulouse and Jean-Pierre Changeux. That was a kind of a disappointment. I preferred to quit the field rather than live with this disappointment. Then, neural networks for computer science did not show much performance. In retrospect, all the right ideas were there. Everything that has happened in the last ten years—the revolution of the deep network—its theory was already present in that time. Not only the

---

[89] F. Krzakala and O. C. Martin, "Spin and link overlaps in three-dimensional spin glasses," *Phys. Rev. Lett.* **85**, 3013 (2000). https://doi.org/10.1103/PhysRevLett.85.3013 **PC:** Mézard is not a co-author of the work but is thanked "for very stimulating discussions and for [his] continuous encouragement."

[90] Jonathan S. Yedidia, *Expansions at fixed order parameter: mean field theory and beyond*, PhD Thesis, Princeton University (1990). https://catalog.princeton.edu/catalog/995976343506421

theory, but the main steps of the algorithm, the backpropagation etc. Everything was there, but at the time it did not work. It did not work because we didn't have enough computers and we didn't have the databases. Above all, the databases were the big missing ingredient. Then, there started to be at that time also a lot of interest in new developments in condensed matter physics, like high-T_c superconductors[91]. There were many attractive problems back in the heart of physics that were interesting.

PC: In 1991, you wrote a popular science piece for *La Recherche*, "Des Verres de spin aux réseaux de neurones[92]." How did this popular science piece come about?

MM: [1:22:14] I wrote several kinds of popular pieces about spin glass and neural networks.

PC: This one is probably one of the higher profile ones.

MM: [1:22:22] Maybe yes. By that time, it was clear that it was a topic that had made a big leap, big progress. So, there were questions about that. There were people who wanted to understand it: scientists in other disciplines. As I told you, I really dedicated quite some time in the second half of the '80s discussing with a lot of people from very different communities: linguistics, biology, computer science, engineers in computer science, people who wanted to build a dedicated computers with artificial neurons inside, etc. I was pretty much in this interface with different communities, which had brought me to think about how to describe all this progress. The progress of spin glass theory is technical, in some sense very technical, but still the main ideas you can convey them to a much broader audience—of colleagues in completely different disciplines who have not much mathematical background—and try to tell what has been done. So, I had this experience from my interactions with the other communities. There were some multidisciplinary committees for fellowships. There was a full committee of the French minister of research about cognitive science, and I was sitting there[93]. So, we had to review and discuss a lot of proposals that were very diverse. It was a time during which I learned why and how to discuss with other scientific communities. I thought it was important. I still think it is. I learnt a lot; it was probably quite useful for my professional

[91] High-temperature superconductivity: https://en.wikipedia.org/wiki/High-temperature_superconductivity

[92] M. Mézard and G. Toulouse, "Des Verres de spin aux réseaux de neuronnes," *La Recherche* **22**(232), 616-623 (Mai 1991).

[93] See, *e.g.*, B. Chamak, "Les sciences cognitives en France," *La revue pour l'histoire du CNRS* **10**, 1-12 (2004). https://doi.org/10.4000/histoire-cnrs.583

life later on. When I became, in 2012, Directeur de l'École normale, having this long experience of interacting with people from very diverse backgrounds was quite useful. So, in this context, writing some kind of non-technical account of what had been done in spin glasses and neural networks was very natural.

PC: In that piece, in particular, you wrote that the analogy between spin and structural glasses was, in your words, "boiteuse, car pour décrire la structure d'un verre les positions des atomes sont les seules variables, tandis que dans le cas du verre de spin il existe des paramètres gelés." Had you paid much attention to the structural glass problem when you wrote this?

MM: [1:25:51] Not as much as I should have done. I completely missed one important thing, which is the Kirkpatrick-Thirumalai-Wolynes[94] work. I missed it. I plead guilty. Peter Wolynes[95] visited Paris at some point. I don't remember the year. He visited École normale, and he was trying to explain his ideas about the connection between replica symmetry breaking and the structural glass problem. I did not understand anything. I did not understand what he meant, what he had in mind. I just missed the point. I regret it. It's a question, in some sense, of personal fits, of personalities, and of the ways of expressing himself and myself that did not match. Had we matched at that moment, I'm sure we would have made progress much earlier, because a lot of ingredients were present. This very problem of the quenched disorder or not was really a cognitive obstacle for me. I had this impression—coming to the disordered systems from the spin glass and from the replica method— that the average over disorder is crucial. What do you do with a system which has no quenched disorder? That was, for me, a very big obstacle in order to accept the idea that the method that we had could be relevant for systems without quenched disorder. That's why we worked on this with Jean-Philippe Bouchaud[96], to try to see and understand if there could be spin glasses without disorder. At the same time, there was a group in Rome, which was doing also developing spin glasses without disorder, with Giorgio, Enzo Marinari and Félix Ritort[97].

[94] See, *e.g.*, T. R. Kirkpatrick, D. Thirumalai and P. G. Wolynes, "Scaling concepts for the dynamics of viscous liquids near an ideal glassy state," *Phys. Rev. A* **40**, 1045 (1989). https://doi.org/10.1103/PhysRevA.40.1045 and many of their preceding papers.

[95] Peter G. Wolynes: https://en.wikipedia.org/wiki/Peter_Guy_Wolynes

[96] J.-P. Bouchaud and M. Mézard, "Self induced quenched disorder: a model for the glass transition," *J. Physique I* **4**, 1109-1114 (1994). https://doi.org/10.1051/jp1:1994240

[97] E. Marinari, G. Parisi and F. Ritort, "Replica field theory for deterministic models: I. Binary sequences with low autocorrelation," *J. Phys. A* **27**, 7615 (1994). https://doi.org/10.1088/0305-4470/27/23/010; "Replica field theory for deterministic models. II. A non-random spin glass with glassy behaviour." *J. Phys. A* **27**, 7647 (1994). https://doi.org/10.1088/0305-4470/27/23/011

These are papers—maybe they are not as well-known as others—that, for me at least, were crucial papers. They were crucial papers, because they were unlocking what seemed a huge barrier between two fields that looked different: one with quenched disorder, the other without. The discovery, at least for us, went through cooking up some models in which we understood that the disorder could be self-induced by the system, by its dynamics. (That cuts a long story short, but that's more or less what we had done.) From there on, you could describe it by replicas. Then, that was, for me, the step that opened the gates towards structural glasses. What year was it now? I'm lost.

PC: It's 1994, I think.

FZ: I wanted to understand this a little bit better, so I had a few questions. When did you say that Peter Wolynes visited Paris?

MM: [1:29:30] I don't know. I do not remember. It might be at the end of the '80s. I'm not sure. He may remember.

FZ: When you left the field of neural networks, about 1990, you started working for a few years on interfaces and polymers. You have a work on the variational theory for random manifolds[98], and a series of works on polymers[99]. Why did you choose to work on this set of problems?

MM: [1:30:25] The impression that I had was the following. On the one hand, there was the whole series of works on optimization. I thought that it had come to the end of the cycle. We could do much more in the same line, but we had looked at the matching and traveling salesman problems. We knew where the frontier was. It was very difficult to go beyond, and I thought that it was good to leave it aside. It had come to the end of a certain cycle. For the question of neural networks and neurobiology, I told you. I would add to that that I have always tried, in my scientific life, to move to new topics relatively regularly. Not to be the person that would stay in a theme for decades. I always thought that it was better for me, at least. I don't know if it is better for science but better for me to move into another field and come with my ideas in some slightly different fields, not too far so that I can bring something. That was always how I worked. Indeed, in the beginning of the '90s, I decided to get back to problems in

[98] M. Mézard and G. Parisi, "Replica field theory for random manifolds," *J. Physique I* **1**, 809-836 (1991). https://doi.org/10.1051/jp1:1991171

[99] See, *e.g.*, M. Mézard and G. Parisi, "Interfaces in a random medium and replica symmetry breaking," *J. Phys. A* **23**, L1229 (1990). https://doi.org/10.1088/0305-4470/23/23/008; M. Mézard, "On the glassy nature of random directed polymers in two dimensions," *J. Physique* **51**, 1831-1846 (1990). https://doi.org/10.1051/jphys:0199000510170183100

the physics of condensed matter, and look at problems of disordered systems, but other problems of disordered systems, which were quite different, like polymers, like interfaces, the pinning of interfaces. These were also problems that were very important in condensed matter. There were experimental results on the pinning of the vortex lattice[100]. That was very important to understand how high-T_c superconductors worked. We had the Gaussian variational method with replicas with Giorgio. It was a kind of versatile tool that could be applied to a lot of topics. We applied it with Jean-Philippe Bouchaud and Jonathan Yedidia, but we had not quite well taken into account the periodicity of the lattice in the theory that we did. It is only Thierry Giamarchi that did it well[101]. It was a whole field in itself, this pinning of interfaces. It is still a whole field. It has grown into something which has become very rich. At that time, it was clear to me that it was an interesting series of problem, in which there were experiments and in which the replica method could bring something.

FZ: This is when you started working with Jean-Philippe[102]. Was Jean-Philippe at the time a PhD student?

MM: [1:33:45] In 1992, no. He was no longer a PhD student. He was established.

FZ: What was the situation at École normale at the time. How was the statistical physics group organized?

MM: [1:34:19] Statistical physics had evolved into a more autonomous branch of physics. I had my collaborators, my students, etc. It had matured with respect to what I had seen a decade and a half before, when I was a student there. There had been some tensions, because of: "To what extent could we have new students coming who did not go through the field theory applied to particle physics phenomenology?" At that time, the field itself of statistical physics had gotten more mature. Statistical physics of disordered systems was considered a respectable activity for theoretical physicists. So, it went okay. But it hasn't been like that in all labs. What is the size that you can give to a statistical physics group with respect to elementary particle high-energy physics? In École normale, there was also

[100] J.-P. Bouchaud, M. Mézard and J. S. Yedidia, "Variational theory for disordered vortex lattices," *Phys. Rev. Lett.* **67**, 3840 (1991). https://doi.org/10.1103/PhysRevLett.67.3840; "Variational theory for the pinning of vortex lattices by impurities," *Phys. Rev. B* **46**, 14686 (1992). https://doi.org/10.1103/PhysRevB.46.14686

[101] T. Giamarchi and Pierre Le Doussal, "Elastic theory of pinned flux lattices," *Phys. Rev. Lett.* **72**, 1530 (1994). https://doi.org/10.1103/PhysRevLett.72.1530; "Elastic theory of flux lattices in the presence of weak disorder," *Phys. Rev. B* **52**, 1242 (1995). https://doi.org/10.1103/PhysRevB.52.1242

[102] Jean-Philippe Bouchaud: https://en.wikipedia.org/wiki/Jean-Philippe_Bouchaud

the creation of the Laboratoire de physique statistique[103], in which there would be both theorists and experimentalists. I decided to stay in my original lab, which was theoretical physics, but I could have moved there. That would have been possible.

FZ: So, this was also the time when many students like Jean-Philippe, Antoine Georges[104], started directly into statistical physics.

MM: [1:36:04] Yes. That was why that created a little bit of tension. Some of the advisers of the old school were considering—even Claude—that probably one should still train first the people to do high energy physics, and then they would go to statistical physics. While the young students that you mention, Antoine, Jean-Philippe, Pierre Le Doussal[105], wanted to go in it right away. They considered that there was plenty of other things to learn in condensed matter physics. There was a lot of experiments to think, to learn about. Maybe it's not necessary to learn supersymmetry, but you can do other things.

FZ: Then, we've arrived to 1994. I come back to the same question I asked before, because this is something that bugs me a little bit. I don't understand how things went from the work you did in '84-'85 on what we call now the random-first order transition in the *p*-spin and the REM, then to Kirkpatrick, Thirumalai and Wolynes, who did this series of works between '87 and '89. It seems like it stayed in the US during this time, then one sees, back in Europe, this activity on finding models without disorder that could be in this universality class. There is the activity you mentioned of Marinari-Ritort-Parisi in Rome. People started to look for models without disorder and you did too. How did the ideas circulate?

MM: [1:38:10] For me, the motivation was trying to see if really this fundamental barrier between quenched disorder and not quenched disorder, that had been an obstacle to even my capacity of approaching the problem of structural glasses, to see if it was a strong barrier or not. But it was not motivated by the works of Kirkpatrick, Thirumalai and Wolynes.

[103] See, *e.g.*, "Laboratoire de Physique Statistique," *École normale supérieure* (2018) http://www.lps.ens.fr/?Presentation-generale&lang=fr (Accessed February 19, 2023.). The *Laboratoire de physique statistique* was founded in 1988. In January 2019, along with various thematically related laboratoires of École normale supérieure, it was merged into the *Laboratoire de physique*.
[104] Antoine Georges: https://en.wikipedia.org/wiki/Antoine_Georges
[105] "Pierre Le Dousal," *Physics Tree* (n.d.). https://academictree.org/physics/peopleinfo.php?pid=777200 (Accessed February 17, 2023.)

FZ: I did a little bit of research looking the papers and I found that most glass papers around 1994 cite the work of Kirkpatrick, Thirumalai and Wolynes[106]. So, people knew it, albeit often for technical things, not for the idea of connecting the two worlds of quenched and not quenched disorder. Had people not digested it?

MM: [1:39:17] At least, as I told you, I had not digested it. I think I'm not the only one. It was formulated in a different way… I'm not blaming anyone, but it's an interesting case of a development that took a few years to be acceptable. It's related to the way of thinking about the problem, the way of writing and explaining it. And of this fundamental obstacle of the quenched disorder, which I insist—to me at least—was a very important point. When you have such an objection from the very first line, you have these thick technical papers, which use replicas and you say: "What are these replicas doing here? What are you averaging over? There is no disorder, guys." It's kind of a big obstacle. At least for me, until we had the two series of works—the work that we did with Jean-Philippe and the Marinari-Parisi-Ritort—these were separated worlds.

FZ: So, the crucial moment when it became acceptable for you, and for the community in Europe at large was after these two works.

MM: [1:40:44] For me, certainly. I would not say anything about the community at large. It does not mean that the work by Thirumalai, Kirkpatrick and Wolynes I understood completely at that time, but certainly this fundamental objection was erased at that point, so it became possible to go back to that.

PC: After leaving Rome, from '86 until even 15 years later you kept on collaborating with Giorgio on projects. How was this collaboration maintained? How would you two be typically working with each?

MM: [1:4139] I had been visiting Rome from time to time. He was visiting Paris from time to time. It was very easy. With Giorgio, after all these many collaborations, we understand each other very easily. There are people who said that Giorgio is difficult to understand. To me, he's crystal clear. I have this chance. That's a wonderful chance in life. It was always going very easily. There is one moment, in which we really had to meet for a longer time and discuss on a day-to-day basis for several weeks, but that's a bit

[106] See, *e.g.*, Refs. 93, 106, and L. F. Cugliandolo, J. Kurchan, R. Monasson, G. Parisi, "A mean-field hard-spheres model of glass," *J. Phys. A* **29**, 1347 (1996). https://doi.org/10.1088/0305-4470/29/7/007

later. That's when we went to the finite-connectivity spin glasses, solving the system with finite connectivity[107]. That was a very hard thing to do. So, for this we met. I remember we spent time in Trieste working together, every day, in the park and in the office.

FZ: Before we get to that next step, after these papers of 1994, there was an explosion of works on structural glasses for a few years: your work with Giorgio on the HNC[108]; the work of Giorgio with Silvio on the Franz-Parisi potential and its application to glasses[109]; the work of Cugliando and Kurchan on aging[110]. Can you describe to us how the community functioned at the time? There was this explosion of work, but how was it organized? Were there networks? Were there conferences? How was going the communication between the Rome and the Paris groups, which were very active on this subject?

MM: [1:43:49] By those years, we had a lot of collaborations between Rome and Paris groups. We had exchanged students, postdocs, etc. Quite a lot of our students had come to Rome and reciprocally, so we had many possible contacts. The list of works that you mentioned, I would not have put them together. I would have said that there are two directions. One is the direction of the dynamics, and the other is the direction of structural gases. That's not exactly the same thing, although they get some connection at some point through mode-coupling theory, of course. Cugliandolo-Kurchan, originally, is the study of the dynamics of spin glasses with their quenched disorder, with the tools and what we know about the *p*-spin model. They related it to the landscape of the *p*-spin models. That was a very interesting work as soon as it appeared—their first work, which was on the *p*-spin. What was really nice in their work is that with respect to

[107] M. Mézard and G. Parisi, "The Bethe lattice spin glass revisited," *Eur. Phys. J. B* **20**, 217-233 (2001). https://doi.org/10.1007/PL00011099

[108] See, *e.g.*, M. Mézard and G. Parisi, "A tentative replica study of the glass transition," *J. Phys. A* **29**, 6515 (1996). https://doi.org/10.1088/0305-4470/29/20/009; "Thermodynamics of glasses: A first principles computation," *J. Phys.: Condens. Matter* **11**, A157 (1999). https://doi.org/10.1088/0953-8984/11/10A/011

[109] See, *e.g.*, S. Franz and G. Parisi, "Recipes for metastable states in spin glasses," *J. Physique I* **5**, 1401-1415 (1995). https://doi.org/10.1051/jp1:1995201; "Phase diagram of coupled glassy systems: A mean-field study," *Phys. Rev. Lett.* **79**, 2486 (1997). https://doi.org/10.1103/PhysRevLett.79.2486; A Barrat, S. Franz and G. Parisi, "Temperature evolution and bifurcations of metastable states in mean-field spin glasses, with connections with structural glasses," *J. Phys. A* **30**, 5593 (1997). https://doi.org/10.1088/0305-4470/30/16/006; M. Cardenas, S. Franz and G. Parisi, "Glass transition and effective potential in the hypernetted chain approximation," *J. Phys. A* **31**, L163 (1998). https://doi.org/10.1088/0305-4470/31/9/001

[110] See, *e.g.*, L. F. Cugliandolo and J. Kurchan, "Analytical solution of the off-equilibrium dynamics of a long-range spin-glass model," *Phys. Rev. Lett.* **71**, 173 (1993). https://doi.org/10.1103/PhysRevLett.71.173; "On the out-of-equilibrium relaxation of the Sherrington-Kirkpatrick model." *J. Phys. A* **27**, 5749 (1994). https://doi.org/10.1088/0305-4470/27/17/011

what Sompolinsky and Zippelius had done in 1981[111], they proposed something which was a kind of reinterpretation, in which instead of having the time that diverges, you had the idea of aging. It was the idea to look at the ratio between the time of the experiment and the time of the aging. It was a kind of reinterpretation of equations that are familiar, but that were all of a sudden part of something completely different. Immediately when I saw it, I was very interested. In fact, we did with Silvio the generalization to a more general ultrametric, hierarchy of several steps of replica symmetry breaking[112]. They did it, of course, at the same time. I immediately considered that it was something very interesting. Then, the main issue of that was that it gave a tool to connect to the finite-dimensional spin glass, because you could do the mapping. That, we did later with Giorgio, Silvio, and Peliti[113].

FZ: The reason why the two things are connected in my mind—I might be wrong—is that I always had the impression that one of the big successes of the work of Cugliandolo and Kurchan in '93 is that the aging of the *p*-spin that they describe is very similar to the aging of structural glasses, much more than the aging of spin glasses, because you have the two-step relaxation: alpha and beta. That's why I thought that this led to a natural connection. In your paper altogether—Bouchaud, Mézard, Cugliandolo Kurchan—you describe, and you make the connection[114].

MM: [1:47:19] Yes. In some sense, this is a review paper that wants to connect all these pieces together. So, you are right.

FZ: And this is '96, so it is still part of this explosion.

MM: [1:47:30] You are right. It is part of this explosion, but in some sense for me, I came to that from two different directions. One about studying the out-of-equilibrium dynamics in spin glasses and the other one, which is trying to work on structural glasses from first principles, in spite of the fact that they have no quenched disorder. But, of course, the two directions converge to the kind of scheme that—through the connection with mode-

[111] H. Sompolinsky and A. Zippelius, "Dynamic theory of the spin-glass phase," *Phys. Rev. Lett.* **47**, 359 (1981). https://doi.org/10.1103/PhysRevLett.47.359

[112] S. Franz and M. Mézard, "Off-equilibrium glassy dynamics: a simple case," *Europhys. Lett.* **26**, 209 (1994). https://doi.org/10.1209/0295-5075/26/3/009; "On mean field glassy dynamics out of equilibrium," *Physica A* **210**, 48-72 (1994). https://doi.org/10.1016/0378-4371(94)00057-3

[113] S. Franz, M. Mézard, G. Parisi and L. Peliti, "Measuring equilibrium properties in aging systems," *Phys. Rev. Lett.* **81**, 1758 (1998). https://doi.org/10.1103/PhysRevLett.81.1758

[114] J.-P. Bouchaud, L. Cugliandolo, J. Kurchan and M. Mézard, "Mode-coupling approximations, glass theory and disordered systems," *Physica A* **226**, 243-273 (1996). https://doi.org/10.1016/0378-4371(95)00423-8

coupling theory and the alpha-beta relaxation—theorization becomes close to each other.

FZ: On the HNC work, how did you have the idea and how did you learn about liquid state theory?

MM: [1:48:25] At some point, there starts to be this idea that you can use replicas in a system without disorder, because it allows you to compute the complexity. It was natural for us—with Giorgio—to try to understand if it applies to liquids. Of course, we studied a little bit the standard theory of liquids, which is what it is. But one thing that for me was important in all these things—in retrospect, it looks simple but at least for me it was a surprise—was this idea that when you look at a liquid with replicas the glass state will be characterized by effective molecules, in which you have replicas of each color bound together. This idea of a bound state of a molecular liquid with clones—or whatever you want to call them—now maybe it does not seem fundamentally different from what we knew in spin glasses, but for me it was new. In fact, I often use it to characterize, even for a broader public, a larger audience, to tell them what is the glass state. How do you decide that there is a glass state? You look at this replicated system, the replicas attract each other, and if you release this attraction will they remain correlated or not? That translates itself into this molecular description of the liquid. This, I think, was a nice idea. It's a cute idea. Building it involved quite a lot of steps with respect to what I knew about *p*-spins and about states in 1-RSB systems. In some sense, there were two aspects to replicas. One is the quenched disorder. You can also apply it if you count states in a system which does not have quenched disorder. That's good. The second aspect was: if you really want to use it for interacting atoms or molecules, then you have to describe it with these effective colored molecules. This was a cute idea that took some time to mature.

FZ: Throughout the time, from 1993 to 2000, did you have any discussion or interactions with Kirkpatrick, Thirumalai or Wolynes? In the meanwhile, they had kind of left; they had moved to other problems, so there might have been no overlap.

MM: [1:51:36] No overlap.

PC: In a review you wrote in 2000 about the work on glasses[115], you mentioned that there's still a lot of work to do. On the analytical side, for instance, one

[115] M. Mézard and G. Parisi, "Thermodynamics of glasses: A first principles computation," *J. Phys.: Condens. Matter* **11**, A157 (1999). https://doi.org/10.1088/0953-8984/11/10A/011

needs better approximations of the molecular liquid state allowing to go beyond the small cage expansion. You nevertheless left the field. What again drew you away?

MM: [1:52:03] There is my recurrent motor, which pushes me not to stay in a field for too long. And then at some point there was a big curiosity and big moves to make real contact with computer science through the finite-connectivity models. It was very exciting. I could not do everything. I could not keep a good activity on glasses, together with the development of this new field in computer science. I had to choose, and I chose the new one as I kind of always do. Maybe I should have stayed there, but that's how it goes.

PC: About the finite connectivity work you mentioned, how do you recall this came about?

MM: [1:53:09] This new direction we had… As soon as 1987, we had written a paper with Giorgio on finite-connectivity spin glass. I don't remember all details of what we did, but it was basically the replica symmetric case. We said: "It would be so nice to do replica symmetry breaking for this problem." We didn't know how to do it. So, it's a problem that was in our minds for more than 10 years. At some point[116], we saw how to handle that, but it was a very difficult work. It was a complicated thing. Conceptually, it's probably one of the hardest problems that we have had to work on. The paper on the Bethe lattice in spin glass, it's a difficult paper, if you read it really correctly. Even when I was giving talks about it, it was not easy, people had hard time understanding it. Even for us, it was not so easy to formulate it. Later, after a few years, thanks to the exportation of these ideas to optimization problems and the mapping to belief propagation etc., it became much more accessible. We could transform it into something that is much easier. But at the beginning it was really complicated. Always because of this same problem of the interplay of the cavity with the displacement of the free energies, the reorganization of the free energy. It's a much more subtle version [than for] the SK model.

PC: When you started working on this, why did you think that this was the right time? What was the new insight that you had that made it possible to attack RSB on a finite-connectivity graph?

MM: [1:55:36] I'm not sure there was one. I look at the papers of that time, but I do not think… I would not be able to say that. As I said, this was in our minds. We knew that it was a challenging, difficult problem, one of these

[116] **MM:** In retrospect, I think it came from an idea of Giorgio who contacted me.

problems that you know you say: “Someone has to come up with a solution to that.” In parallel, in computer science, Zecchina, Monasson etc.[117] And there was the numerical work by Kirkpatrick and Selman etc., who had started to work on satisfiability[118]. Giulio Biroli had also made a variational approach[119]. So, we knew that there was this system there, but that was something that needed to be done.

PC: So, it was then. Was the time to attack this problem because of work that you'd seen by Kirkpatrick and collaborators?

MM: [1:57:06] No, I don’t think so.

PC: What set the timing then?

MM: [1:57:11] I do not remember. I don't know. The conversation with Giorgio, I don't remember where it took place. Maybe I can think of a small influence from a different problem: when I was in Santa Barbara, I had been working with Giorgio on structural glasses, but also, we had done this paper with Tony Zee on Euclidean random matrices[120], which are built from distances between points in a *d*-dimensional space. Maybe that was giving some idea… That was a problem in which you had to think about finite connectivity, but technically what we did was very different.

PC: Once you built that machinery, you quickly went on to do random satisfiability problems[121]. Had you been paying attention to these problems throughout?

---

[117] R. Monasson and R. Zecchina, "Entropy of the K-satisfiability problem," *Phys. Rev. Lett.* **76**, 3881 (1996). https://doi.org/10.1103/PhysRevLett.76.3881; “Statistical mechanics of the random K-satisfiability model,” *Phys. Rev. E* **56**, 1357 (1997). https://doi.org/10.1103/PhysRevE.56.1357; "Tricritical points in random combinatorics: the (2+*p*)-SAT case." *J. Phys. A* **31**, 9209 (1998). http://doi.org/10.1088/0305-4470/31/46/011; R. Monasson, R. Zecchina, S. Kirkpatrick, B. Selman and L. Troyansky, “Determining computational complexity from characteristic ‘phase transitions’," *Nature* **400**(6740), 133-137 (1999).

[118] S. Kirkpatrick and B. Selman, "Critical behavior in the satisfiability of random Boolean expressions," *Science* **264**, 1297-1301 (1994). https://doi.org/10.1126/science.264.5163.1297; B. Selman and S. Kirkpatrick, "Critical behavior in the computational cost of satisfiability testing," *Artificial Intelligence* **81**, 273-295 (1996). https://doi.org/10.1016/0004-3702(95)00056-9

[119] G. Biroli, R. Monasson and M. Weigt, "A variational description of the ground state structure in random satisfiability problems," *Eur. Phys. J. B* **14**, 551-568 (2000). https://doi.org/10.1007/s100510051065

[120] M. Mézard, G. Parisi and A. Zee, "Spectra of Euclidean random matrices," *Nucl. Phys. B* **559**, 689-701 (1999). https://doi.org/10.1016/S0550-3213(99)00428-9

[121] See, *e.g.*, M. Mézard, G. Parisi and R. Zecchina, "Analytic and algorithmic solution of random satisfiability problems," *Science* **297**, 812-815 (2002). https://doi.org/10.1126/science.1073287; M. Mézard and R. Zecchina, "Random k-satisfiability problem: From an analytic solution to an efficient algorithm," *Phys. Rev. E* **66**, 056126 (2002). https://doi.org/10.1103/PhysRevE.66.056126

MM: [1:58:15] Not really. Riccardo Zecchina came as a visitor. I don't remember what year it was, but we were in Orsay, or I was about to move to Orsay. (It doesn't matter.) Riccardo came for a few months. He arrived and we started to discuss. It was a natural sense of this discussion to say… On the one hand, he had been working on satisfiability. On the other hand, we had this new way of approaching diluted systems Giorgio. So, it was natural to work on that together. That was very natural. That must be in the winter of 2001, Christmas 2001. I went on vacation, and I worked like crazy during my vacation time, because I was so excited about this problem. I was basically trying to write the cavity equation for the *k*-SAT and the XOR-SAT using the cavity method for diluted systems we had found with Giorgio. I did the two simultaneously because one helped me to check the other. I came back and we discussed all this in detail with Riccardo. We started to work the equations together. Then, at some point, we are in front of the computer, putting the equation of the computer, and he says: "But these equations that we are writing, they look like equations we could write on a given graph." We said: "Okay. Let us try it." Then, we started to write the equations on a given graph. So, it becomes an algorithm, the SP algorithm. It was a very exciting time.

PC: Your former student, Rémi Monasson[122], had worked on these problems. Had you not paid attention to that work up to that point?

MM: [2:00:50] Not really.

PC: What was the initial reaction of the theoretical computer science community to this new approach? Was it in any way different than their reaction in the '80s when you had worked on the travelling salesman problem?

MM: [2:01:05] It was completely different for several reasons. On the one hand, on the theoretical side, our methods had acquired a certain respectability in the sense that we had given solution to the random link matching problem and other problems of this type. That was one thing, but above all, the reaction came from practitioners of the satisfiability problem. There is a whole community of people who do SAT solvers, who are professionals of that. For them, when we introduced the survey propagation for the random 3-SAT[123], it was a completely different way of addressing the problem than what they had done before. They were not

---

[122] Rémi Monasson, *Physique statistique des réseaux de neurones : corrélations spatiales et apprentissage*, thèse de doctorat, Université Pierre et Marie Curie (1993). https://www.sudoc.fr/044130260

[123] A. Braunstein, M. Mézard and R. Zecchina, "Survey propagation: An algorithm for satisfiability," *Rand. Struct. Algo.* **27**, 201-226 (2005). https://doi.org/10.1002/rsa.20057;

using BP, not to say SP. It was surprising to them that it was possible at all to work with samples with 100,000 variables so close to the critical point. That was a big surprise.

I remember that when we made the code public, there were colleagues using it, and they would send a mail to Riccardo or me saying: “It doesn't work. I tried it, but it doesn't work.” “But you tried it on what size?” They’d say: “200”. We’d say: “Try 20,000” And 20,000 worked because it's an asymptotic algorithm. It’s an algorithm that is based on what happens at large *N*. Paradoxically, it might not converge at small sizes. It meant an enormous gap for a problem which is exponential. They used to run the algorithm at 200 and we were saying: “Look. It’s completely different.” So, this prompted attention.

Another point that was interesting was created before we joined the subject. It was clear before we joined the subject that there was very probably a critical point. There was no proof of that, but the numerical simulations, with finite-size scaling analysis, were pointing to the existence of the critical point. And there was a Friedgut theorem that told you that the threshold becomes sharp[124]. It had not been established that there was a limit, but still there was a critical point. For us, it was a phase transition point. So, it was clearly an interesting phase transition in computer science. For them, in particular for people who are interested in complexity theory and also practitioners, it is a factory of hard instances. The hard instances of random satisfiability are close to the critical point. That's unsurprising in retrospect, but it is a fact. So, the two communities were interested in the random satisfiability problem close to its critical point with various motivations, but at least there was a common interest. This, in my experience, is something that makes interdisciplinary contacts much easier. When you happen to have people of various communities which all consider that *this* is an important point, an important regime, it is much easier. It is in this context that we made the prediction on the critical point, and above all the existence of the intermediate glass phase, which had been hinted at by the variational approach of Biroli, Monasson and Weigt, but that was established by our solution.

PC: Were you aware of the timeliness of the questions, when you worked this out or was it just good luck? Did you know that the computer scientists were going to receive these ideas favorably?

[124] E. Friedgut and J. Bourgain, “Sharp thresholds of graph properties, and the k-sat problem,” *J. Amer. Math. Soc.* **12**, 1017-1054 (1999). https://doi.org/10.1090/S0894-0347-99-00305-7

MM: [2:05:46] It is always difficult to know if someone will receive your ideas favorably. When Riccardo arrived in Paris and we started discussing it, it was very clear to him. He had been working on that. Then, by reading the papers by Selman and Kirkpatrick—all these numerical papers—it was quite clear that there was some excitement around that topic.

PC: Did your experience working at interdisciplinary fields for a decade and a half at that point—and writing popular science pieces explaining ideas—play any role in being able to communicate these ideas to the new community? Or did it remain fairly technical?

MM: [2:06:47] Most of it remained fairly technical, but on some occasion, there was a possibility to try to address a broad audience and communicate what there was in the spin glass theory that could be useful. I had accumulated experience in doing that, of course. My first answer would have been no, but my second answer is probably yes, it has been helpful. Several things have been helpful. At that time, I had reached maturity about explaining what replicas are and what replica symmetry breaking is about. This helps, because you have to explain it to people who are completely outside of the field. Also, the maturity of what we had been doing was much better recognized. In 2000, there was wide acceptance that replica symmetry breaking was a very important method that had solved a number of problems, so it was easier to present it.

PC: But it was still before the Talagrand results[125], so it had not been formally accepted by more rigorous treatments.

MM: [2:08:21] Sure, it was still a mathematical problem, but before its rigorous solution, there was a long duration, during which it was well accepted by a lot of people, except for the question of its relevance to 3D spin glass. But apart from that, as a general method of disordered systems in theoretical physics, it was very well accepted, even if it was not proven rigorously.

PC: Since that work, you seem to have largely left the materials physics community. Is that your view as well?

MM: [2:0916] Well, it's not totally true. I was working on random heteropolymers, on other disordered systems[126]. But it is true that I was

---

[125] See, *e.g.*, M. Talagrand, "The Parisi formula," *Ann. Math.* **163** 221-263 (2006). https://www.jstor.org/stable/20159953

[126] See, *e.g.*, A. Montanari and M. Mézard, "Hairpin formation and elongation of biomolecules," *Phys. Rev. Lett.* **86**, 2178 (2001). https://doi.org/10.1103/PhysRevLett.86.2178; M. Müller, F. Krzakala and M. Mézard, "The secondary structure of RNA under tension," *Eur. Phys. Jour. E* **9**, 67-77 (2002).

gradually moving towards problem of computer science and information theory, because of the connection between the message passing algorithms and the cavity method for diluted system. The replica symmetric one is easy, and it is connected to belief propagation, which is a very well-known message passing algorithm that is used in other disciplines: information theory, Bayesian analysis etc. So, this was a very big link between other fields, and it prompted me to move towards these other fields. I think it was natural. The moment at which I really had to make a hard choice was in 2012, when I became director of École normale supérieure, because at that moment I knew that I could not continue simultaneously work on heteropolymers, work on information theory, and work on optimization problems. I had to choose. Then, I focused really on inference and information theory. Before that, I tried to maintain a diverse activity, but gradually moving towards computer science at large. But modern information theory, I would gladly incorporate it as a branch of statistical physics.

PC: Throughout these years, did you nevertheless follow the evolution of the statistical physics of structural glasses? Did you follow the work of Bouchaud, Biroli[127] and others?

MM: [2:11:38] I paid attention but a bit from far, because I could not find the time to study the papers in detail. Then, there were some topics that were also quite connected to what we did in information theory and random graphs, like point-to-set correlations[128], which are absolutely crucial for the cavity method used to study finite connectivity constraint-satisfaction problems. This, I studied in quite a lot of details[129]. Also, the four-point

---

https://doi.org/10.1140/epje/i2002-10057-5; A. Montanari, M. Müller and M. Mézard, "Phase diagram of random heteropolymers," *Phys. Rev. Lett.* **92**, 185509 (2004). https://doi.org/10.1103/PhysRevLett.92.185509

[127] See, *e.g.*, J.-P. Bouchaud and G. Biroli, "On the Adam-Gibbs-Kirkpatrick-Thirumalai-Wolynes scenario for the viscosity increase in glasses," *J. Chem. Phys.* **121**, 7347-7354 (2004). https://doi.org/10.1063/1.1796231; G. Biroli and J.-P. Bouchaud, "Diverging length scale and upper critical dimension in the Mode-Coupling Theory of the glass transition," *Europhys. Lett.* **67**, 21 (2004). https://doi.org/10.1209/epl/i2004-10044-6

[128] See, *e.g.*, A. Montanari and G. Semerjian, "On the dynamics of the glass transition on Bethe lattices," *J. Stat. Phys.* **125**, 103-189 (2006). https://doi.org/10.1007/s10955-006-9103-1; "Rigorous inequalities between length and time scales in glassy systems," *J. Stat. Phys.* **125**, 23-54 (2006). https://doi.org/10.1007/s10955-006-9175-y; G. Biroli, J.-P. Bouchaud, A. Cavagna, T. S. Grigera, and P. Verrocchio, "Thermodynamic signature of growing amorphous order in glass-forming liquids," *Nat. Phys.* **4**, 771-775 (2009). https://doi.org/10.1038/nphys1050

[129] M. Mézard and A. Montanari, "Reconstruction on trees and spin glass transition," *J. Stat. Phys.* **124**, 1317-1350 (2006). https://doi.org/10.1007/s10955-006-9162-3

correlation[130]. All this, I followed, but could not find the time to get back actively into this modelling of structural glasses.

PC: From your standpoint, what do you feel has allowed the ideas of replica symmetry breaking to thrive in some circles and not so much in others? What might have impeded its reception in some contexts?

MM: [2:13:24] I think replica symmetry breaking is a tool. It is a tool of theoretical physics, a beautiful tool that opens a lot of possibilities, but it is a tool. At least initially, it was not obviously related to a material. It was related to spin glasses, of course, and we saw initial success with the field-cooled and zero-field cooled magnetization that decouple. So, it has had some impact, but in the usual sense of a physical theory it is still a bit remote from the material itself. It is difficult to translate it into quantitative predictions saying: “You will measure this, and you will find that.” That is complicated. And it is furthermore complicated by the fact that it addresses problems, in which the other parameter corresponds to local alignment. Most of the experimental checks that you would think of, would need the ability to measure local structures in many places in your system. So, they are necessarily very complicated from the experimental point of view. That might have been an obstacle to the popularization of the method in physics circles. One might argue that it has been more popular, more accepted, in countries in which there is more a tradition of theoretical physics built from the theory, in a rationalist attitude. It's probably not by chance that the people who have constructed it came from field theory. They came from this culture of an abstract theoretical physics. Giorgio himself has very much grown in that country of field theory. It is very clear. Miguel, myself, and David Gross, were all coming from this same background. Maybe it was harder to get accepted by communities with a point of view which is more empirical, more based directly on data, and with a direct connection with data. I think this is one obstacle.

On top of this, there was the debate about applicability or not of RSB to 3D spin glass, which certainly blocked some evolution and reinforce the dichotomy that I was describing before between “theory towards experiment” versus “experiment toward theory” approach.

[130] See, *e.g.*, S. Franz and G. Parisi, "On non-linear susceptibility in supercooled liquids," *J. Phys.: Condens. Matter* **12**, 6335 (2000). https://doi.org/10.1088/0953-8984/12/29/305; S. C. Glotzer, V. N. Novikov and T. B. Schrøder, “Time-dependent, four-point density correlation function description of dynamical heterogeneity and decoupling in supercooled liquids,” *J. Chem. Phys.* **112**, 509-512 (2000). https://doi.org/10.1063/1.480541; C. Donati, S. Franz, S. C. Glotzer and G. Parisi, “Theory of non-linear susceptibility and correlation length in glasses and liquids,” *J. Non-Cryst. Solids* **307**, 215-224 (2002). https://doi.org/10.1016/S0022-3093(02)01461-8

In some sense, some of the big successes that we have seen are in fields, which are not really material science. They are fields in which you are studying devices, algorithms, etc. which you have built. If you are in information theory and you want to create an error-correcting code for communication, you build your code, and you don't necessarily have the constraints of the three-dimensional space. You may have some constraints in the end when the code is implemented as a device, but at least you have more freedom. I do not see it as a fundamental difference. A lot of condensed matter physics is elaborated—for many decades now—on systems that we build on purpose. They are human-made artifacts. You don't find a quantum hall effect device in nature. You have to build it the same way that you have to build an error correcting code. Still, this has probably been an obstacle towards material science adoption of these systems. But this has erased gradually, and it is becoming more and more accepted. The thing that I also found beautiful is that it has given rise to so many developments in math and so many other branches.

Also, one should emphasize—probably we have not emphasized enough—that it took quite some time to confirm the mathematical validity of all this. by Guerra[131], Talagrand[132] and all the mathematicians coming into play… I remember when there was the first confirmation that Parisi solution was right by Talagrand. [But this did not prove ultrametricity.] Before there was a confirmation of ultrametricity[133], I remember we had a discussion with Giorgio, saying: "Well, but ultrametricity is delicate. Maybe they will not confirm ultrametricity." We thought that ultrametricity is something else because it is very delicate: the free energy difference is of order one. It might be that the whole structure is correct for density of free energies, but it's no longer correct when you go to free energy differences of order one… So, we were still wondering whether it would be confirmed.

For many of the results that were elaborated over the years, it took a long time to confirm them rigorously. I remember when we used RSB methods for neural network with Werner Krauth, to compute the capacity of memory of binary perceptron[134]. It was a very simple piece of work which

[131] 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
[132] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Michel Talagrand*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.daafy5aj
[133] See, *e.g.*, D. Panchenko, "The Parisi ultrametricity conjecture, *Ann. Math.* **177** 383-393 (2013). https://www.jstor.org/stable/23350562
[134] See Ref. 76.

extended the well-known result for the continuous perceptron where the critical threshold—the ratio of the number of patterns to neurons—is 2. But for binary perceptron this threshold was not at all clear. With Werner Krauth we had put forward this conjecture—I would say an educated conjecture—based on a subtle 1-RSB structure that implies a critical threshold of 0.83, given by the zero-entropy point of the replica symmetric solution. I remember that when we got this result, Bernard Derrida, for instance, thought that it was not correct. He worked on proving that it was not correct for a few months, together with Griffiths[135]. At the time I was worried. We had done all the possible checks that we had in our hands, but our result was still not completely sure. By the way, this conjecture is not yet completely proven rigorously. Pieces of it are confirmed, but the threshold capacity of the binary perceptron is still not yet proven. It's a conjecture that goes back to '89. It means that there are subtle technical aspects behind all that, and that they can blur a little bit the acceptability. But now, I think the whole scheme is much better accepted.

The thing that is very nice with the replicas is that, on the one hand, it's a very compact encoding of a lot of physical properties that we know. That is very important. We can map the physical reality with what is encoded in replicas. On the other hand, you cannot cheat. The replicas, they are there. They give you something. You cannot just turn it around. You have a scheme; it gives you something. It has always been confirmed. Now, in a lot of systems it has been rigorously confirmed. There has been all the confirmation for the SK model, but also the confirmation for the constraint satisfiability problems, the beautiful work by Amin Coja-Oghlan, [Allan] Sly[136], Nike Sun[137], etc.

PC: During your time at ENS, Orsay, or elsewhere did you ever teach about replica symmetry breaking or spin glasses?

[135] B. Derrida, R. B. Griffiths and A. Prugel-Bennett, "Finite-size effects and bounds for perceptron models," *J. Phys. A* **24**, 4907 (1991). https://doi.org/10.1088/0305-4470/24/20/022

[136] Allan Sly: https://en.wikipedia.org/wiki/Allan_Sly_(mathematician)

[137] Nike Sun: https://en.wikipedia.org/wiki/Nike_Sun

MM: [2:23:22] At summer schools. I taught this topic at various schools mostly. I went quite a few times in Les Houches[138], in Cargèse[139], in Beg Rohu[140]. Then, we gave a course at the doctoral school of École normale with Andrea Montanari, in which we taught these topics at the interface between spin glass theory, information theory and optimization. It was following this course that we conceived the book, *Information, Physics and Computation*[141].

PC: When would that have been? Was this offered only once, or was it a repeated course?

MM: [2:24:22] We taught it once and then we wrote the book, but the book took us quite a few years. I remember when the book was published, in 2009. Probably we would have taught the course in 2005-2006, because I remember that we worked for at least three years to write the book.

PC: You were talking about exact results earlier. From my understanding, in the '90s, you met with Michel Talagrand to try to explain to him the physics of spin glasses. How did these interactions go? Were you involved in any way in trying to formalize the RSB descriptions?

MM: [2:25:12] No. Talagrand got interested into spin glasses and he wanted to solve this problem. I think it is fair to say that he knew nothing about spin glasses. Actually, he knew nothing about statistical physics at all. I remember he contacted me, and he told me: "Can we have a few meetings so that I ask you some questions and you will guide me?" That's what we did. We had a few meetings of this type, over lunch, where we discussed, and he would ask questions. At the first meeting, they were really kind of elementary questions about statistical physics, because he had to learn about it. Then, they became more and more elaborate and subtle questions about spin glasses. I tried to answer them as far as I could, but I was impressed by his attitude. If you think about it, what spin glass theory represents is a certain amount of work. He dedicated several years of his

[138] See, *e.g.*, Fluctuating geometries in statistical mechanics and field theory, F. David, P. Ginsparg and J. Zinn- Justin, Les Houches, France, 1994. Proceedings: M. Mézard, "Random systems and replica field theory," In: F. David, P. Ginsparg and J. Zinn- Justin eds., *Fluctuating geometries in statistical mechanics and field theory* (Elsevier, 1996).
[139] See, *e.g.*, *From Statistical Physics to Statistical Inference and Back*, P. Grassberger and J.-P. Nadal, Cargèse, France, September 1992. Proceedings: M. Mézard, "Spin Glasses: An Introduction," In: P. Grassberger and J.-P. Nadal, eds. *From Statistical Physics to Statistical Inference and Back* (Dordrecht: Springer, 1994), 183-193. https://doi.org/10.1007/978-94-011-1068-6_11
[140] "Beg Rohu History", *The Beg Rohu Summer School 2023* (2022). https://www.ipht.fr/Meetings/BegRohu2023/history.html (Accessed February 19, 2023.)
[141] M. Mézard and A. Montanari, *Information, Physics, and Computation* (Oxford: Oxford University Press, 2009).

life to learning that and to solving it. He arrived to me saying: "I want so solve that problem." And he did it. It was quite remarkable. I was involved in helping him to get into understanding the field, but the solution is entirely his.

PC: In a few contexts, you have mentioned the importance of international collaborations in the development of the field. For instance, in your popular science piece, you mentioned a collaboration entitled BRAIN[142]. How important were these collaboration in building the community between Paris and Rome?

MM: [2:27:27] These networks and these European collaborations were crucial in that they helped us to build a community. They provided support for the exchange of students, and postdocs: it has been very important. David Sherrington was probably among the first ones to create a European network on these topics. Later on, I was head scientist in charge of running two of these big collaborations, with 10 to 12 teams in various European countries, each five years or something like that. These were quite important. I described to you the type of collaboration that we had in the 1980s, when I was in Rome. Between the structure that existed then and the one that exists now, it's a completely different world. There are now many people who work on disordered systems in general. So, this community has developed thanks to these networks, fellowships, anything that would support interactions, organize conferences, exchange students, etc. It has been very important. I recognized it, and I dedicated a little bit of my time to managing these European networks.

PC: You've also mentioned a few times the role you played as academic leader. Throughout these years, you were President of École normale, but you had other leadership positions before. Did you have ever had the opportunity to tilt the scale a bit in favor of statistical physics and the study of disordered systems? Or did that never take place?

MM: [2:29:50] I tried to remain very balanced and appreciate also what is done in other in other fields, of course.

My point of view was that people at the head of big scientific institutions should be active scientists. I mean, not professional administrators or scientists who have become professional administrators. This is, I think, a very important point. It's probably one of the many reasons that made me

[142] Basic Research in Adaptive Intelligence and Neurocomputing (BRAIN). See, *e.g.*, "BRAIN: Europe Anticipates the Japanese 'Human Frontier' Challenge," *European Commission* (February 16, 1987). https://ec.europa.eu/commission/presscorner/detail/en/IP_87_68 (Accessed February 19, 2023.)

accept to become director at École normale. It's important because as a researcher, you have a certain spectrum of research that you see and that you feel is important. But certainly, if I think of what I was doing at École normale in recent years, we created a center for quantitative biology[143]. It is not spin glass or RSB, but certainly my interest toward quantitative biology come from all these interactions that started in the 1980s with biologists about neurobiology, about neural networks, how to model them etc. and that is still going on. That is one consequence of it, if you want. I can say the same thing for the *Center for Sciences of Data*[144], but it's not the only one. Many people have created centers for data science. So, there is an impact that one can see, but that's an impact at large, if you think about replica symmetry breaking as really the physics of disordered systems and its interface. What is remarkable in RSB is how many interfaces it has generated. When I give talks about that, I think in terms of number of scientific communities of various themes: math, information theory, biology. It's very broad. You cannot even list all of them in a seminar. And if you would like to list all the subtopics, you take the whole talk discussing that. So, it is like the famous sentence by Phil Anderson: "It's a cornucopia"[145]. That is certainly true, nice, and important. I think that for me it has been very useful, because it means that when I arrived as the head of École normale, I knew the context and—to some moderate extent—some vision about what various subfields were doing. Not all of them, of course. In science, I had a good idea of what they do in cognitive science, or what they do in biology, what they do in certain fields of mathematics—certainly in probabilities. It helps to have a broader vision.

PC: Is there anything else that you would like to share with us about this era that we may have missed?

MM: [2:33:41] One chapter on which we could have spent a bit more time on is the neural network studies in the end of the '80s. It was a very productive time, with a lot of new ideas. There was the idea of Gardner, statistical physics in the space of networks. These ideas, they have not yet found their ultimate impact, but I think that they are very interesting. There was this idea of creating a neural network; not just studying one that you have decided with pre-defined architecture but building the network layer by

[143] "About Qbio, the new centre for quantitative biology," *ENS PSL* (September 21, 2021). https://qbio.ens.psl.eu/en/article/about-qbio-new-centre-quantitative-biology (Accessed February 19, 2023.)

[144] "Présentation du Centre Sciences des Données," *ENS PSL* (April 16, 2021). https://csd.ens.psl.eu/?-Presentation- (Accessed February 19, 2023.)

[145] P. W. Anderson, "Spin Glass VI: Spin Glass as Cornucopia," *Physics Today* **42**(9), 9 (1989). https://doi.org/10.1063/1.2811137

layer as we did with Jean-Pierre Nadal[146]. I think there were quite a few very interesting ideas. About this idea of the landscapes of neural networks—how to study the landscape, and how our tools can be used to study the landscape—I think we could have said more. [Also, about complex systems more in general, for instance economics and finance.]

PC: Finally, do you still have notes, papers, correspondence from that epoch? If yes, do you have a plan to deposit them in an academic archive at some point?

MM: [2:35:16] I'm very bad about archiving, so the answer is probably no. I think I don't have much. I'm pretty sure I have nothing. I could look around, but I have not been very good doing these things. I should have.

FZ: What about the two versions of that certain paper with you and Miguel? It would be nice to see it.

MM: [2:35:47] Forget it. Certainly not, unfortunately. I might keep some things for a few years, but regularly, when the things are published, they are published. I consider that the published version is the one that should be the reference. No, unfortunately not.

PC: Thank you, Marc.

[146] M. Mézard and J.-P. Nadal, "Learning in feedforward layered networks: The tiling algorithm," *J. Phys. A* **22**, 2191 (1989). https://doi.org/10.1088/0305-4470/22/12/019