History of RSB Interview: Marc Gabay

June 21, 2021, 8:30am-10:30am (EDT). Final revision: September 12, 2021

Interviewers:

Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu

Francesco Zamponi, ENS-Paris

Location:

Over Zoom, from Prof. Gabay's office at Université Paris-Saclay in Orsay, France.

PC: Good morning, Prof. Gabay. It's a pleasure to get to chat with you. As we've discussed ahead of time, the purpose of this interview is to discuss the emergence of replica symmetry breaking ideas in the physics community, which we bound roughly from 1975 to 1995. Before we get to that, we have a few questions on background, if you don't mind us asking. First, in generic terms, can you tell us a bit more about your family and your studies before starting university?

MG: [0:00:39] That's interesting. My family immigrated from Turkey to France. Their general profession was [that] they were merchants. My grandfather on my father's side was selling carpets, but even at that time my grandparents felt that culture and education were of paramount importance in order to find a better life. When they moved to France, first of all, they had already a very good knowledge of French. My parents had themselves been educated in the French system while growing up in Turkey, and so quite naturally when I was born and raised they considered it very important for me to pursue a higher education [as much] as possible. Even though my father would have loved if I had joined his business, selling plastic and rubber products, I made it clear that this was not my major interest, that I was more attracted to science in general.

I would say that I was not particularly good at math or physics in high school, for that matter. I followed this track to apply for the so-called Grandes écoles [1]. There was a math professor who was really a great inspiration for me. All of a sudden, I started blooming in this topic. Similarly, I

---

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

had a physics professor who himself was from the family of Guinier[2]. They were very good scientists. In fact, my professor Guinier's brother[3] was at my current university, Université d'Orsay[4]. He taught crystallography there, and he wrote the book which is an authority in the field[5]. So this was for me some introduction to science. Then I joined École normale supérieure de St-Cloud[6], as it was called back then, which is now École normale supérieure de Lyon. I mentioned this because there, I had two classmates, who were about my age: one was Thomal Garel, the other was Henry Orland. We will come back to [them] because a lot of the studies I conducted on spin glasses and replica symmetry breaking was with them.

PC: Following your studies at St-Cloud, what led you to pursue graduate studies in theoretical physics?

MG: [0:04:36] Essentially, procrastination. This was, I would say, the easy way to continue. Of course, as I said, I had developed a strong interest in science, so certainly this was also an incentive.

The École normale system originally prepared you not necessarily to doing research, or graduate studies, but to be a professor in high school. There was this national exam called *agrégation*[7]. Essentially, at the end of the four years at École normale, you took this exam and you were offered a position in high school. I took it—as everybody else—but I knew already at that stage that this was not the career path that I wanted to follow.

During my four years at École normale, I enrolled in a masters degree program—master 2, as it known now, but at the time it was called DEA[8]. (Nothing to do with drug enforcement agency, but *diplôme d'études approfondies* in French.) I had courses with Jacques Friedel[9], who was an outstanding scientist [and] had a strong personality, André Blandin, who became my thesis advisor, and other people, [such as André] Guinier, as I mentioned before. Then I loved it. All of a sudden, I discovered real physics; not just academic physics, but the challenge of doing research. Trying to

---

[2] Georges Guinier: A. Dalançon, "notice GUINIER Georges, Ernest," *Le Maitron* (2018). https://maitron.fr/spip.php?article208826 (Consulted August 21, 2021)

[3] André Guinier: https://en.wikipedia.org/wiki/Andr%C3%A9_Guinier

[4] January 1, 2021 the university changed name to Université Paris-Saclay.

[5] A. Guinier, *Théorie et technique de la radiocristallographie* (Paris: Dunod, 1956).

[6] École normale supérieure de St-Cloud: https://fr.wikipedia.org/wiki/%C3%89cole_normale_sup%C3%A9rieure_de_Saint-Cloud

[7] Agrégation: https://en.wikipedia.org/wiki/Agr%C3%A9gation

[8] Diplôme d'études approfondies: https://en.wikipedia.org/wiki/Master_of_Advanced_Studies#France_and_francophone_countries

[9] Jacques Friedel: https://en.wikipedia.org/wiki/Jacques_Friedel; Jacques Friedel, *Graine de Mandarin* (Paris: Ed. Odile Jacob Sciences, 1999).

confront your ideas with physics. Is it right? Is it wrong? We had more ideas about things. So this particular masters program became for me a very strong incentive to continue on with doing a thesis.

At the time, there were two theses in France. One was a short thesis—one year or 18 months—somewhat equivalent to the *diplomarbeit*[10] in Germany. An introduction to research, and at the end of it either you were accepted for the PhD (or long thesis) program, or you did something else. The longer thesis typically lasted for five years for theoretical physics and ten years or experimental physics. (It's a very long time. I have no idea what's so special about 10 years. Five years is in the correct ballpark. In comparison to what's done in the US, for example, that's perfectly fine.)

As we say, I had two left hands[11] so experimentally-speaking I was not very gifted. Theory became the natural track for me. That's basically the reason why I went into theoretical physics.

PC: As you mentioned, for your thesis you joined the (larger) Friedel group. You said you worked mostly with André Blandin, but you also collaborated with Marie-Thérèse Béal during your thesis. Can you tell us a bit more about how the group functioned and what was going on at that point[12]?

MG: [0:09:45] I mentioned before that there were two theses. The short one, 18 months, was called *thèse de 3e cycle*. I did this with Marie-Thérèse Béal-Monod. One of the things that we had been trained for during the time at École normale was to be good at math. We had energy, and we could perform a lot of calculations. Marie-Thérèse Béal-Monod—she's known as Zazie—gave me a challenge. This challenge was really mathematical. To [myself], I said: "I will meet that challenge." And I did[13]. She was surprised that I met it, but I met it**.** Jacques Friedel was a member of the defense committee[14], and I remember very vividly... To me, some mathematical challenge had been posed, and I wanted to meet it. So I told the committee, very naively: "Here, I met the challenge, and I'm sure that experimentally this can be checked." I saw Friedel's face, and I realized that reality was maybe a little bit more complicated than I had thought. Certainly, from the mathematical standpoint, this was okay. It was a nice piece of work. But in

---

[10] *Diplomarbeit*: https://de.wikipedia.org/wiki/Diplomarbeit

[11] *Avoir deux mains gauches*: https://en.wiktionary.org/wiki/avoir_deux_mains_gauches

[12] For additional context, see, *e.g.*, A. Georges, "The beauty of impurities: Two revivals of Friedel's virtual bound-state concept," *C. R. Phys.* **17**, 430-446 (2016). https://doi.org/10.1016/j.crhy.2015.12.005

[13] See, *e.g.*, M. Gabay and M.-T. Béal-Monod, "Paramagnon theories below three dimensions," *Phys. Rev. B* **18**, 5033 (1978). https://doi.org/10.1103/PhysRevB.18.5033

[14] M. Gabay, *Fluctuations de spin dans des systèmes presque magnétiques pour des dimensions inférieures ou égales à trois*, thèse 3e cycle, Université Paris-Sud (1977).

terms of practical interest for the physics community, this was more questionable. However, one never knows. As it turns out, years later in Sherbrooke, Canada, there were people who used the result I had established as an unfledged researcher[15]. So one never knows. But to be honest, the practical implications of this project were not so great.

You were asking how things were in the group at that time. I would say that while I was doing this work under Marie-Thérèse Béal-Monod's supervision, I was one of the "young ones"; we were the padawans[16], using Star Wars terminology, so the untrained ones. We clustered together. I was already sharing an office with Thomas Garel, who was a family friend, but he was one year ahead of me. I would say that we reconnected at this point. We shared a lot of ideas. First, about the things that I was doing. Second, the kind of things he himself was doing. Then, there were the more senior scientists.

It's very funny. I will say something which stayed with me even though it may seem very odd nowadays. I did this work with Marie-Thérèse Béal-Monod, Zazie, back in 1977, and that was nine years after the May '68 events, which took the world by storm. Very oddly, I mean at least to me, the theory group was divided into factions. There were the anti-1968 and the pro-1968, and within the pro-1968 there were the communists and there where the radical leftists, and they did not speak to one another. It didn't mean anything to us. We were there to learn physics and we realized that there were these factions, which still lived politics even almost 10 years after the events. So we had very little contact with the grown-ups. I was in the neutral group. Zazie was definitely not a 1968 person, and she had been a student of Jacques Friedel. Friedel, I would say, was in this [same] subgroup, and Blandin the same thing. So we spoke with them, but basically students were mostly keeping between themselves.

PC: In that context, how did you first hear about spin glasses and the replica trick?

MG: [0:16:04] I had three mentors during the three-year time period when I worked on this problem. André Blandin was the first one, and I would say that André was the founder of this replica symmetry breaking story. I will

---

[15] P. Bénard, L. Chen and A.-M. S. Tremblay, "Magnetic neutron scattering from two-dimensional lattice electrons: The case of $La_{2-x}Sr_xCuO_4$," *Phys. Rev. B* **47**, 15217 (1993). https://doi.org/10.1103/PhysRevB.47.15217
[16] Padawans: https://en.wiktionary.org/wiki/padawan

explain. The second mentor was Cirano De Dominicis[17], in Saclay, and the third one was Gérard Toulouse[18], at École normale [supérieure].

What happened was [that] after my third cycle thesis—this short thesis—I went around, I shopped around to see who could be a thesis advisor for the longer [thesis]. The first person who said: "Yeah, okay, I'll take you as a PhD student," was Pierre Pfeuty. Pfeuty and Toulouse had done a lot of work on critical phenomena[19], so the general idea Pierre Pfeuty was proposing had to do with critical phenomena, but it's hard to tell in advance if some idea you're thinking about will amount to much or not. After a year, in 1978, it didn't look like anything might be coming out of that idea Pierre Pfeuty had suggested. He had taken not only me as a student, but Thomas Garel as well. So the two of us were in the same boat.

Blandin saw that we needed assistance, and because he was a very nice person, very considerate, he realized that maybe he could take over as an advisor for the two of us. Almost immediately we hit the jackpot. I very clearly remember… One day, it was a Sunday, the phone rang at my home. I picked up the phone, and Blandin said: "We've got to meet in this café in Montparnasse (I believe it was "Le Dôme"). There's something." I had no idea what this was about, but then when I arrived he was there with his nephew, who was 14 years old. Blandin said: "I've had this fantastic idea about spin glasses." To me the word did not even mean anything. What was spin glasses? I had no clue. I can only imagine that the 14-year-old nephew must have been even more mystified than I was. Then, Blandin started excitedly to jot down equations on a piece of paper and to elaborate on some ideas. Some of the things that he was trying to explain I could understand, but others did not really register at first. In retrospect, of course, I realize that he had essentially the right idea, and now I fully understand what he was sort of thinking. He said: "Look, when we think of some phase transition, some critical phenomenon, in most cases we think about some kind of order parameter, and according to Landau if there is some kind of order parameter, then it means that there is some kind of symmetry that would be broken." He said: "What I have seen, so far, in approaches about spin glasses—Edwards and Anderson[20], or Sherrington

---

[17] Cirano De Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis
[18] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse
[19] In particular, the two co-authored one of the first introductory texts about critical phenomena: G. Toulouse and P. Pfeuty, *Introduction au groupe de renormalisation et à ses applications* (Grenoble: Presses universitaires de Grenoble, 1975); *Introduction to the Renormalization Group and to Critical Phenomena* (New York: Wiley-Interscience, 1977).
[20] S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5,** 965-974 (1975). https://doi.org/10.1088/0305-4608/5/5/017

and Kirkpatrick[21] later—I don't see that there is any kind of symmetry that's being broken." This was really the idea. He had the right idea[22].

Of course, at that time, [in] 1978, there were two seminal theoretical papers on the mean-field theory of spin glasses. We know that mean-field is the first thing you do when you try to attack some problem where you suspect that there is some phase transition or whatever. For the Sherrington-Kirkpatrick model, there were replicas that had been introduced. Sherrington and Kirkpatrick introduced some kind of order parameters in terms of these replicas. The second seminal paper was by Thouless, Anderson and Palmer, in which they had written equations of motion, and they had constructed the free energy, based on some expression for ~a~ the local magnetizations on each site etc[23]. Then, obviously, they could introduce an order parameter, which looked very much like the Sherrington-Kirkpatrick order parameter.

These two approaches were there, but for both of these, Blandin said, "I don't see that there is anything that has been broken." He said: "Okay, let me try and see if there is a symmetry in this problem that can be broken." He realized that when you introduced replicas, it meant that you introduced an additional symmetry, which is the permutation group of the replicas. He said: "breaking the symmetry means breaking a replica symmetry in the problem." That was the idea. Of course, as soon as he had realized that and I had understood what he meant… [This] took a little bit of time, because first I had to get up to speed about what glasses were. I read a lot of literature on disordered systems, and then of course the papers by Sherrington and Kirkpatrick and by Thouless, Anderson and Palmer.

Then we started doing some calculations. Of course, there was a little bit of a disappointment because the problem with replica symmetry breaking à la Sherrington-Kirkpatrick stems from the fact that when dealing with discrete, Ising spins, one should not expect to find a negative entropy. Unfortunately, their solution produced **a** negative entropy. Also, an odd thing was happening, namely that in terms of this order parameter the free energy turned out to be a maximum instead of being a minimum as one would expect. With the breaking of replica symmetry, the entropy was less negative but it was still negative. Then, obviously, you could say: "Ok, I

---

[21] D. Sherrington and S. Kirkpatrick, "Solvable Model of a Spin-Glass," *Phys. Rev. Lett.* **35**, 1792 (1975). https://doi.org/10.1103/PhysRevLett.35.1792

[22] A contemporary exposition of these ideas can also be found in: A. Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Colloques* **39**, C6-1499 (1978). https://doi.org/10.1051/jphyscol:19786593

[23] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Philo. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992

have broken replica symmetry a certain way. I can come up with more educated schemes, and perhaps this will remedy these problems." Incrementally, indeed by working on more sophisticated schemes, we were able to find solutions that were getting better and better, or less bad and less bad, but still the same problems remained[24].

There was something that was missing. Of course, the one person who found the solution was Giorgio Parisi. I remember very clearly. At that time, papers on that subject were published basically in two journals. (I mean, there was *Nuovo Cimento*, of course, for Italy.) There was *Journal de Physique* or *Journal de Physique Lettres* for the French part. And there was *Journal of Physics A* for math, or *C* for condensed matter in Britain. The reason I'm mentioning these journals is because groups that were very actively involved in this story were located in France, in Britain and there was Italy to some extent. There was also [some] activity in the US, but not a lot. Not many people worked in that field at the time. The first paper by Giorgio Parisi that I read, was in *Physics Letters*, the Dutch journal[25]. It was like two pages and I said: "What is this thing? It doesn't seem to mean anything at all." He claimed that there is no negative entropy, that the problem is solved. What? I just could not understand. It took a while for people to realize that he got the right solution.

FZ: The idea of having a coupling between two replicas to break explicitly the replica symmetry[26], was this already present in this conversation with Blandin in a café in Montparnasse, that you mentioned?

MG: [0:28:07] Absolutely. That's what he was trying to explain, and I sort of failed to understand.

FZ: Because that was really an insightful idea that had been developed. It was really in his mind that the right way to do it was to add a field conjugated to the overlap between two replicas.

MG: Exactly. Absolutely. He had this back in 1978.

FZ: Another thing that I wanted to ask about what you just said is the following. Was your first contact with the Parisi scheme in the paper in which he

---

[24] A. Blandin, M. Gabay and T. Garel, "On the mean-field theory of spin glasses," *J. Phys. C* **13**, 403 (1980). https://doi.org/10.1088/0022-3719/13/3/015

[25] 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 Note that this particular work finds that "the zero temperature entropy is unfortunately negative, although it is very small."

[26] See Ref. 22.

already had the full scheme? I think that in his first paper he does only one step and then the entropy is still negative.

MG: That's right. Let me elaborate a little bit on that. As I said, we were trying to be a little more educated on this symmetry breaking scheme. So instead of grouping replicas by two, we said: "Let's group them by p." Indeed, lo and behold, you see that the entropy that you get is divided by p. So you say: "Okay! Then I have to go to limit $p \rightarrow \infty$, and then we're safe." That's not so; let me tell you. What Parisi did, as you said, is the first step. But then he said: "Let me allow p to be a variational parameter, so it will depend on temperature." You see that the trend is going in the right direction, but still it suffered from a similar kind of problem we had been faced with. It is only when he devised his very clever scheme that it was possible to cure everything.

FZ: I see. Thank you.

PC: You mentioned that Thomas Garel was involved in a lot of this, but was he not invited to the Montparnasse discussion?

MG: [0:30:42] No, he was not. Maybe because he didn't have a phone at that time. I can't remember. You have to sort of go back in time a little bit. Nowadays, that seems crazy. You buy your cell phone, and then you have your own line. But at the time getting a phone was a little bit of a challenge. I could tell you the circumstances why I had one and he didn't, but I think it's beyond the [scope of this] interview.

PC: Sure. That first paper that you wrote about replica symmetry breaking with Blandin and Garel was submitted in June 1979 and it was published in 1980. In it, you do mention Parisi. How did this interplay of you becoming aware with the Parisi solution and your own words come together?

MG: [0:31:43] Right. The thing is, as I said, essentially the root of this replica symmetry breaking dated back from 1978. It took a very long time before this thing evolved into a paper that was written. However, the idea was already broadcast through seminars, discussions that Blandin had with various colleagues[27]. And Giorgio got into the game... He had come to France on several occasions, most notably in Saclay, and so he learnt about this thing. I may be mistaken, but I don't think that he had any kind of prior experience in spin glasses. It's just that he heard about these ideas, and he had the inspiration. That was amazing, unbelievable.

---

[27] See A. Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Colloques* **39**, C6-1499 (1978). https://doi.org/10.1051/jphyscol:19786593

PC: In your thèse d'état[28], one of the first pages contains a caricature by Caran d'Ache[29], suggesting a certain degree of animosity in the spin glass field[30]. Can you describe that moment and what was the source of animosity?

MG: [0:33:35] I would say that there were some groups who were competing. The British groups, including scientists like Thouless[31], of course, A. P. Young[32], and Bray and Moore[33], were very clever, very active. They had great ideas and they were very strong competitors. David Sherrington[34]— from Sherrington-Kirkpatrick—was also one such person. Going to seminars and conferences made me realize that people wanted their ideas to be promoted and that this was a very emotional issue, which inspired me to present this Caran d'Ache cartoon.

PC: After that paper with Blandin, you worked with Gérard Toulouse, whom you mentioned was also a mentor to you. Can you tell us about how that interaction developed and how this work came about?

MG: [0:34:56] Chronologically, there was work with De Dominicis first. If you look at the history of these papers, you'll see that the work with Cirano and the work with Blandin followed two parallel paths. Of course, with Cirano this was more equation of motion [centered]. But I will answer your question about Toulouse. We can come back to the work with Cirano, which I think was very important.

Gérard Toulouse, I would say, was a very deep thinker. His way of approaching problems was usually orthogonal to the mainstream approaches. He took shortcuts, but in a very thoughtful way. So, early on, he

---

[28] M. F. Gabay, *Étude de la thermodynamique des verres de spin dans le cadre du champ moyen et de phénomènes critiques dans les cristaux liquides et les polymères*, thèse d'état, Université Paris 11 (1981). http://upsaclay.focus.universite-paris-saclay.fr/permalink/f/1gllaij/33PUP_Alma_UNIMARC21130555360006051 (Consulted August 22, 2021)

[29] Caran d'Ache: https://en.wikipedia.org/wiki/Caran_d%27Ache

[30] Caran d'Ache's most famous cartoon, "Un dîner en famille" is about the Dreyfus Affair, which divided the whole of French society around the turn of the 20th century. https://en.wikipedia.org/wiki/Caran_d%27Ache#/media/File:Caran-d-ache-dreyfus-supper.jpg

[31] David J. Thouless: https://en.wikipedia.org/wiki/David_J._Thouless

[32] 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

[33] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Michael Moore*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 26 p. https://doi.org/10.34847/nkl.997eiv27

[34] See, *e.g.*, P. Charbonneau, *History of RSB Interview: David Sherrington*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 39 p. https://doi.org/10.34847/nkl.072dc5a6

thought: "Can we make thermodynamic predictions about this model that are experimentally verifiable?" At the time, what did exist were experiments on magnetization, on susceptibility. There was the concept of field cooling and zero-field cooling, which even today are considered essential experiments in order to probe symmetry breaking. Non-linear response properties usually are indicative that you have several ground states in competition. Gérard said: "Let me see if I can make thermodynamic predictions for systems which have some possibility of having magnetization in them." The problem of Ising spin glasses had been studied, including when you add a magnetic field.

In fact, it was not realized at first that the right way to probe spin glasses, if one does not have the idea that there can be replica symmetry breaking, was to study the transition in a field. The reason for this is [that] if you have a spin glass phase, and if in this phase you have a lot of possible ground states in your system, it means that you define a lot of different length scales. As a result, a lot of characteristic momenta—a distribution of them—characterize the spin glass state of your system. If you apply a uniform magnetic field, it is not thermodynamically conjugate to the order parameter, and you can have a field-induced phase transition. (Think of an antiferromagnetically ordered system in an applied magnetic field.)

Because of randomness, the Fourier transform of the spin field constitutive of the spin glass ordering contains all the momenta, so that when you have the phase transition, essentially you see the condensation of the system into these modes. They have a characteristic distribution which is a signature of the spin glass state. At the time, this was not really understood, but this was what was behind it.

Gérard Toulouse sort of understood this thing. So he said: "Ok, let me accept this as a fact that we know the physics or the thermodynamics of a system in a magnetic field. By doing some kind of Legendre transformation, I can guess what the free energy of the system will be if I have some magnetization, meaning if I have some kind of ferromagnetic or antiferromagnetic coupling." So he did the mapping and was able to draw a phase diagram for Ising spins, which had three axes: temperature, magnetic field, and the ferromagnetic or antiferromagnetic interaction in the system. Knowing the phase diagram in the temperature-magnetic field plane, you could draw everything in the three-dimensional space or temperature, magnetic field and ferromagnetic or antiferromagnetic coupling between each state. He had been able to do that. So he published this

work, which included some equations allowing one to analytically obtain the phase boundaries[35].

I just wanted to reproduce them, and I couldn't. I mean, I did the math but it didn't seem to agree with the results he was presenting in his paper. So I was puzzled. I went to him and said: "Look, Gérard. I'm sorry, I'm probably making some stupid beginner's mistake, but there is this thing." So he looked at what I had written and said: "I don't see an obvious mistake in your calculation." I told you, Gérard was a very clever guy, and he was very good at taking shortcuts. So there was one term which he had skipped in his assumption. My derivation was done very stupidly, not using any kind of symmetry, just brute force, and, of course, I found the right answer. He was very grateful to me for pointing out this little mistake, and then our collaboration started. He said: "You gave me something; I will give you something back." This was the genesis of our work together on vector spin glasses.

Essentially, we did three things in this field. One was to study the field-temperature phase diagram for vector spin glasses. This was published in *Journal de Physique Lettres*[36], if I'm not mistaken with Vannimenus. Then, there was the equivalent of what Gérard had done for the Ising spin glass problem with ferromagnetic interaction. We did the same thing for vector spin glasses. That was the second thing[37]. Then, there was the third work which I was responsible for doing[38]. (And responsibly mistaken, I must say. Yes, I have to admit.) Probing the stability of this vector spin glass problem, what we had found was that there were two kinds of replica symmetry breaking: one which involves the components of the order parameter perpendicular to an applied magnetic field, and the other which involves the parallel component. In reality, there are not two transitions, only one. As soon as some replica symmetry is broken, everything is broken. The second

---

[35] G. Toulouse, "On the mean field theory of mixed spin glass-ferromagnetic phases," *J. Phys. Lett.* **41**, 447-449 (1980). https://doi.org/10.1051/jphyslet:019800041018044700

[36] This work was presented in two papers: G. Toulouse and M. Gabay, "Mean field theory for Heisenberg spin glasses," *J. Phys. Lett.* **42**, 103-106 (1981). https://doi.org/10.1051/jphyslet:01981004205010300; G. Toulouse, M. Gabay, T. C. Lubensky and J. Vannimenus, "On the order of the spin glass transitions in mean field theory," *J. Phys. Lett.* **43**, 109-113 (1982). https://doi.org/10.1051/jphyslet:01982004304010900

[37] M. Gabay and G. Toulouse, "Coexistence of spin-glass and ferromagnetic orderings," *Phys. Rev. Lett.* **47**, 201 (1981). https://doi.org/10.1103/PhysRevLett.47.201

[38] This publication is part of Ref. 36.

one was in effect a cross-over. I had to eat a humble pie, because Sherrington noticed it immediately, and he had his student, Dinah Cragg[39]. Obviously, they pointed out that I had made a mistake, and I said: "Yes, you're right."

PC: Is that what led to you writing a PRL, the three of you[40]?

MG: [0:46:10] Yes, indeed! You now know the context. I've laid it out for you. It's better than a retraction, of course. I was not fabricating data, but I made a mistake.

PC: You mentioned the work with Cirano as being the third leg of that thesis, and as being very important in your mind. Can you elaborate on how that came about, and then why you think that was a really important advance?

MG: [0:46:48] It came about because, as I said, Thomas Garel was sharing an office with me, but he was supported for his thesis by CEA. Essentially, he had some loose contact at Orme des Merisiers, which was where the theory group at CEA was based[41], where Brézin[42], Zinn-Justin[43], Itzykson[44], Balian[45], and all these people were. Cirano De Dominicis was also there, and Henri Orland, happened to be there as well… As I mentioned at the beginning of the interview, Henri was also at École normale supérieure de St-Cloud, so I knew him, but we had lost track of each other.

One time, Thomas said to me: "Okay, maybe it would be interesting to go to Saclay. They have these people who have some ideas about spin glasses and blabla." So we went. It was very interesting, because Brézin was the one person who had the better knowledge of condensed matter physics. Zinn-Justin, Itzykson, and Cirano De Dominics, were incredible in terms of their math skills. Absolutely incredible. Immediately, Cirano—we were talking when we visited Orme des Merisiers—said: "Look, you know,

---

[39] Dinah Cragg had already completed his studies by the time he joined Sherrington as a postdoc. See, *e.g.,* Dinah Cragg, *Some aspects of the Kondo problem in dilute magnetic alloys*, PhD Thesis, Monash University (1980). https://monash.hosted.exlibrisgroup.com/permalink/f/31uhmh/catau21133207240001751

[40] D. M. Cragg, D. Sherrington and M. Gabay, "Instabilities of an *m*-Vector Spin-Glass in a Field," *Phys. Rev. Lett.* **49**, 158 (1982). https://doi.org/10.1103/PhysRevLett.49.158

[41] Institut de physique théorique: https://fr.wikipedia.org/wiki/Institut_de_physique_th%C3%A9orique

[42] 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

[43] Jean Zinn-Justin: https://en.wikipedia.org/wiki/Jean_Zinn-Justin

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

[45] Roger Balian: https://en.wikipedia.org/wiki/Roger_Balian

there's this theoretical approach, named the Martin-Siggia-Rose formalism, which is about equations of motion[46]. If I look at the Thouless, Anderson and Palmer equations, I can use it and we can find the solution, because it does not involve replica symmetry breaking. We can use innocent replicas," as he called them. Little did we know, of course. Because if you look at the Parisi solution, you realize that at the deepest level in Parisi's hierarchy you find these Thouless, Anderson and Palmer equations. They are all the descendants, of the above generations which break symmetry. In hindsight we know that this was a quest that would not be so fruitful, but Cirano started scribbling on the blackboard[47] right away.

This is something I've never seen, to be honest. Someone who does not write on paper but does everything on the blackboard. Cirano was the one person who could align all these equations on the blackboard, never writing on paper, never once. Very good. Not making mistakes. Except that one time—this is a little story for you—there was a cleaning person who came to clean the room where he had scribbled a blackboard full of equations. She erased everything. The next day, Cirano discovers this. I've never seen anything like this. Cirano was a very nice person. Very quick temper, but an extremely nice person, and certainly very considerate towards the staff, and towards cleaning people. But this time he lost it. He totally blew a fuse. It took just one day to recover everything.

With him started a very fruitful collaboration studying the properties of the Thouless, Anderson and Palmer equations. Essentially, the focus changed a little bit. On the one hand, you have replica symmetry breaking. In some sense Parisi's order parameter is telling you that the structure of the states of the system is complex. On the other hand, working on equations of motion—Thouless, Anderson and Palmer—gives you exactly the complementary perspective on this, but seen from real space, not replica space. There was a lot to scrape there, and we did [it] with Cirano[48]. This lasted for maybe two, two and a half years.

---

[46] See, *e.g.*, J. A. Hertz, Y. Roudi and P. Sollich, "Path integral methods for the dynamics of stochastic and disordered systems," *J. Phys. A* **50**, 033001 (2017). https://doi.org/10.1088/1751-8121/50/3/033001

[47] C. De Dominicis, M. Gabay, T. Garel and H. Orland, "White and weighted averages over solutions of Thouless Anderson Palmer equations for the Sherrington Kirkpatrick spin glass," *J. Phys.* **41**, 923-930 (1980). https://doi.org/10.1051/jphys:01980004109092300

[48] C. De Dominicis, M. Gabay and H. Orland, "Replica derivation of Sompolinsky free energy functional for mean field spin glasses," *J. Phys. Lett.* **42**, 523-526 (1981). https://doi.org/10.1051/jphys-let:019810042023052300; C. De Dominicis, M. Gabay and B. Duplantier, "A Parisi equation for Sompolinsky's solution of the SK model," *J. Phys. A* **15**, L47 (1982). https://doi.org/10.1088/0305-4470/15/1/010; H. J. Sommers, C. De Dominicis and M. Gabay, "On Sompolinsky's spin glass theory approached from Thouless-Anderson-Palmer equations," *J. Phys. A* **16**, L679 (1983). https://doi.org/10.1088/0305-4470/16/17/009

I would say that with him I have learned a lot about functional integration, about these treatments of equations of motion. So I'm extremely grateful for what he brought me, and certainly in terms of the understanding. One of the benefits of working with him was we started asking ourselves questions about: How can we characterize these states that is the fabric, the hallmark of spin glasses? What do they do? How can we view them?

One concept that came into light is that for Thouless, Anderson and Palmer, the free energy is not a maximum; the free energy of the physical solution is a saddle point. It means that essentially not only the first but also the second derivative of the free energy is zero. If you think of the second derivative of the free energy, it's like the inverse of the susceptibility (for a ferromagnet, it is the second derivative with respect to the magnetization). This was indeed one of those strong predictions of the theory, namely that for the correct solution all the way from the spin glass temperature down to T=0, you would expect some susceptibility to be infinite, the so-called replicon mode, or equivalently that its inverse would be zero. This came out of the Thouless, Anderson and Palmer theory. This concept of saddle point is very familiar in physics. If one thinks of a second order phase transition for a magnetic system, in the presence of a magnetic field, one may notice two particular points in the free energy versus magnetization phase diagram. These two points are the so-called spinodal points. For spinodal points, not only the first derivative of the free energy with respect to the magnetization is zero, but the second derivative is zero as well This spinodal point, which corresponds to a saddle point of the free energy, signals that the system reaches the endpoint of a metastable phase and may break into domains to switch to the stable phase. In other words, it contains the idea that you can have a non-homogeneous—in space—kind of system. In some sense, what Thouless, Anderson and Palmer found was this, except a bit more complicated because this was a disordered system in their case. And the Parisi solution also exhibits this property, of course.

PC: You've mentioned a couple of times that you did this work in collaboration not only with senior PIs, but with students in your age group. Could you describe what was this dynamics? Who did what and how did you work together?

MG: [0:57:34] This was really a teamwork. I would say that, of the three, Henri Orland had the best mathematical skills. He knew a lot of useful techniques and mathematical tricks; we divided the main tasks into subtasks, each one of us was assigned a problem to solve, and we cross-checked our results. We had ways of testing, probing the others' results. Once something seemed to be satisfactory, and we were ready to show it, we would present our findings to Cirano.

PC: After your thesis work, and the related papers that came out, you left the field of spin glasses. What led you to pursue other directions in research at that point?

MG: [0:58:54] What happened was [that] after my thesis I went on a postdoc to UC Berkeley. There, I was introduced to ideas on interacting fermionic systems. In some sense, for my short thesis with Zazie—Marie-Thérèse Béal-Monod—I had briefly touched upon the field, but truly in earnest, I was just writing down equations. There were no thoughts about this. But during my postdoc, I was able to be introduced to Kondo problems, to mixed valence problems. In the group of Leo Falicov[49], at UC Berkeley, they were doing a lot of this.

As you can check, if you read my thesis dissertation, I worked on two completely unrelated topics. I spent three years working on spin glasses, and one year on dynamic instabilities in liquid crystals[50]. What happened was… Unfortunately, André Blandin was really a great mentor, a very nice person, but he had tremendous personal problems. After this work was published in 1980, he was not able to continue mentoring. I had to find another advisor, so I switched subject.

After four years, in June 1981, I defended my thesis, and then I decided to go on a post-doc to UC Berkeley and look for other problems. I sort of saw that, in some sense, the field of spin glasses had reached maturity. People were already beginning to look into neural networks, as well as other spin-offs of spin glasses, trying to apply the same ideas. I said to myself: "Is it something I want to get involved in, the Hopfield model[51] and all these other variations on a theme?" Then, I decided: "Well, I'd like to try something different." That was a good reason.

---

[49] Leopoldo Falicov: https://en.wikipedia.org/wiki/Leopoldo_M%C3%A1ximo_Falicov

[50] E. Dubois-Violette and M. Gabay, "The thermal oscillatory instability in a homeotropic nematic: an inverse bifurcation," *J. Phys.* **43**, 1305-1317 (1982). https://doi.org/10.1051/jphys:019820043090130500

[51] See, *e.g.*, V. Dotsenko, *An Introduction to the Theory of Spin Glasses and Neural Networks* (Singapore: World Scientific, 1995). https://doi.org/10.1142/2460

Now, as I said, the field itself evolved in different directions. One very important branch was dynamics. Already at the time, at École Normale [supérieure de Paris], there were people like Rammal Rammal[52], Bernard Derrida[53], Marc Mézard[54], Jean Vannimenus. Jorge Kurchan and Leticia Cugliandolo[55] were also there. I saw their papers on the dynamics of spin glasses, but this was not something I wanted to be involved [with]. In Saclay, there was Jean-Philippe Bouchaud[56], who was also working on dynamical aspects. In my institute, [Laboratoire de Physique des Solides d'Orsay,] several groups led by Ian Campbell[57], Philippe Monod[58], Sadoc Senoussi[59], Henri Alloul[60] were working on experimental aspects of this problem, so, in particular on the dynamics of spin-glasses.

As I said, I'm a great procrastinator. I like simplicity; I don't like complication, so I said: "Maybe it's not for me so I should look for something different." I didn't completely leave the field right away. I told you that, from studies with Cirano De Dominicis, we became really interested in the question of domains that is central to the spin glass state. There was this perhaps less complicated instance, in which you encountered a similar type of situation, namely dipolar systems, which have a tendency to break naturally into domains. Sometimes they break into a regular array of domains. So this impacts the dynamics and also the thermodynamics. Surface effects are important, just as in spin glasses, but with less complications. So we did some of that for a couple of years with Thomas Garel[61]. This was for me the way out of spin glasses.

---

[52] Rammal Rammal: https://en.wikipedia.org/wiki/Rammal_Rammal

[53] See, *e.g.*, 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

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

[55] 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

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

[57] See, *e.g.*, I. A. Campbell, J. M. Flesselles, R. Jullien and R. Botet, "Nonexponential relaxation in spin glasses and glassy systems," *Phys. Rev. B* **37**, 3825(R) (1988). https://doi.org/10.1103/PhysRevB.37.3825

[58] Philippe Monod: https://fr.wikipedia.org/wiki/Philippe_Monod_(physicien)

[59] See, *e.g.*, S. Senoussi, S. Hadjoudj, P. Jouret, J. Bilotte, and R. Fourmeaux, "The domain and the domain wall structures of an $Fe_{90}Zr_{10}$ reentrant alloy," *J. Appl. Phys.* **63**, 4086 (1988). https://doi.org/10.1063/1.340557

[60] See, *e.g.*, H Alloul, "Zero-Field NMR in : New Viewpoint on CuMn Remanence in Spin-Glasses," *Phys. Rev. Lett.* **42**, 603 (1979). https://doi.org/10.1103/PhysRevLett.42.603

[61] See, *e.g.*, M. Gabay and T. Garel, "Properties of the branched state of an Ising dipolar magnet," *J. Phys. Lett.* **45**, 989-997 (1984). https://doi.org/10.1051/jphyslet:019840045020098900; "Phase transitions and size effects in the Ising dipolar magnet," *J. Phys.* **46**, 5-16 (1985). https://doi.org/10.1051/jphyslet:019840045020098900

Of course, going back to spin glasses, which is the core of this discussion, there was the obvious question to ask. Okay, so you investigate mean-field theory**.** But when you go beyond mean-field theory—that's the approach you follow when you consider phase transitions in a system—you want to know if there is an upper critical dimensionality above which mean-field theory applies and below which you have possibly a different physics. Of course, you [also] have the lower critical dimensionality. The question which came up almost immediately was: does this replica symmetry breaking survive when you go below the upper critical dimensionality? There were two schools of thoughts. Certainly, Cirano went deep into this this issue[62]. Then again I said, "not for me".

PC: Despite you leaving the field (you've mentioned one particular direction that has been affected by your work on spin glasses), has this remained part of your career? Or is it something that you put behind and moved on completely?

MG: [1:06:27] Probably the only times when I still think about this problem is, off and on**,** whenever I see a paper devoted to this topic and I say: "Just for the sake of scientific interest, let me look at it and see if there's anything that comes out it." Also, I mentioned that spin glasses, at some point, branched into neural network approaches. I find it funny, in a way, to be witnessing a sort of revival of this field, now with these machine learning approaches. I'm not a specialist. I'm interested but it seems to me like it's *déjà vu*. Probably I was not wrong trying to find my own path in research, just doing different things.

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

MG: [1:08:06] No. I did not. I taught various courses in the master 2 program, and I could have taught one on spin glasses, but this was assigned to Bernard Derrida. This was an excellent choice.

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

MG: [1:08:43] I was just curious. I have learned that the superiority of the British over the French is that they will pursue avenues which may seem uninteresting or maybe marginal in terms of importance, but at some point you

---

[62] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Imre Kondor*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 28 p. https://doi.org/10.34847/nkl.8feanaw7

realize that they have studied this, or they have made it available, and they have the advantage. So I was wondering what motivated you two to document this replica symmetry breaking saga.

PC: We're almost near the interview. We can discuss this afterwards. One last question. 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?

MG: [1:10:08] I have some papers by André Blandin. I'd be glad to deposit them.

PC: There are definitely some options to do that in the greater Paris area. I encourage you to pursue that idea. Thank you very much for you time. It's been a pleasure.

MG: Yes. A pleasure for me too.