History of RSB Interview: Henri Orland

September 23, 2021, 8:30am-10:00am (EDT). Final revision: November 28, 2021

Interviewers:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Francesco Zamponi, ENS-Paris
Location:
Over Zoom, from Prof. Orland's office in IPhT, Gif-sur-Yvette, France.
How to cite:
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

PC: Good morning, Prof. Orland. Thank you very much for joining us. As we were just discussing, the main purpose of this interview is to go over the genesis of the ideas behind replica symmetry breaking, from roughly 1975 to 1995. But before we get to the core topic, we would like to ask a few questions on background. Can you tell us a bit more about your family and your studies before you started university?

HO: [0:00:30] What do you mean my family?

PC: What led you to pursue studies in physics, in particular?

HO: [0:00:39] Ok. When I was young I was quite interested in physics, chemistry, and playing with engineering, that kind of stuff. Then, when I grew up, I was more interested in mathematics, so I went to *classes préparatoires*[1] in mathematics. Then, I went to École Normale Supérieure de St-Cloud, which is now École Normale Supérieure de Lyon, and I took the math *concours*—the math way. But as soon as I got in, I decided to switch to physics because I was fascinated by the mathematics of quantum mechanics, which I found quite amazing and extremely beautiful. So I switched to physics, I did my studies in physics. When I started my PhD, I started in nuclear physics and quantum many-body theory, and rapidly I started to work… I did my PhD in Saclay in the lab where I am still now. I started in '76. Very soon, I started work on the functional integral

[1] Classes préparatoires:
https://en.wikipedia.org/wiki/Classe_pr%C3%A9paratoire_aux_grandes_%C3%A9coles

representations of the many-body problem[2]. Many-body problem for fermions, so it's path integrals in terms of Grassmann variables[3].

At that time, it turns out that Cirano De Dominicis[4] was working on the problem of counting the number of solution of the TAP—Thouless-Andersen-Palmer[5]—equations, which are kind of sophisticated mean-field equations, which are exact for the Sherrington-Kirkpatrick model. They go beyond the normal mean-field at order 1/*N*. To do that, he was using a formalism where you count the number of solutions by introducing a delta function which is normalized by a determinant. His calculation, when there is a determinant, was involving some Grassmann variables, and Cirano didn't know much about Grassmann variables. He came to see me because I was working on this problem for quantum many-body physics fermion systems. We started collaborating together.

There was also Thomas Garel[6] and Marc Gabay[7]. We started working on this problem of counting the solutions of the TAP equations. In the beginning, we were making a mistake. When we did the calculations correctly, we realized that there was an exponentially large number of solutions to this TAP equations. That was the beginning of my interest, and my switching from quantum many-body to classical statistical physics.

PC: Had you heard of spin glasses before then, or was it your introduction to them?

HO: [0:04:08] No, absolutely not. That was my introduction. I didn't know what was quenched disorder. I learned about disorder, about all this nice stuff, and I found it quite fascinating that by taking the log of the partition function, and averaging the log of the partition function you can describe two kinds of relaxation times in the system: the very slow relaxation time of the impurities, and the fast relaxation time of the spin variables. That

[2] See, *e.g.*, J.-P. Blaizot and H. Orland, "Boson representations for systems of fermions," *J. Phys. Lett.* **41**, 523-525 (1980). https://doi.org/10.1051/jphyslet:019800041022052300
[3] Grassmann number: https://en.wikipedia.org/wiki/Grassmann_number
[4] Cirano De Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis
[5] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Phil. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992
[6] Alain Thomas Garel, *Contribution à l'étude des transitions de phase à étoile de vecteurs d'onde et du champ moyen dans les verres de spin*, thèse de doctorat, Paris 11 (1980). http://upsaclay.focus.universite-paris-saclay.fr/permalink/f/1gllaij/33PUP_Alma_UNIMARC21145126650006051
[7] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Marc Gabay*, 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.f14cb3mt

was quite a discovery for me. This was, I think, introduced by Brout[8] if I am not mistaken, this quenched averaging procedure.

We started to work with Cirano. Actually, I finished my PhD [with him]. I did half of my PhD in quantum many-body theory, and the other half with Cirano on spin glass problems and disordered problems[9].

PC: I think that Marc Gabay, Thomas Garel and you were all at ENS St-Cloud. You were pretty close friend with Thomas, even.

HO: [0:05:35] Absolutely. I was very good friend with Thomas. We lost track of each other since he retired. He left the lab six or seven years ago, maybe. He was one year ahead of me; one year older than me.

PC: The fact that you were friendly, did that facilitate your interactions when you were working together on this project?

HO: [0:06:07] Yes. It was helpful.

PC: Can you tell us a bit how you were working, then, as friends and collaborators?

HO: [0:06:14] At that time, Marc and Thomas were in Orsay, in the Laboratoire de Physique des Solides à Orsay[10], a very famous lab, where actually Blandin was. Blandin was, of course, a very central figure in the field of spin glasses[11]. Thomas and Marc had worked with Blandin[12]. They had done quite interesting work on replicas and spin glasses with Blandin. That's how they started to collaborate with Cirano.

Cirano came to spin glasses from the dynamics point of view. As you may know, Cirano was a specialist of critical dynamics[13]. That's how he came to

[8] R. Brout, "Statistical mechanical theory of a random ferromagnetic system," Phys. Rev. **115**, 824 (1959). https://doi.org/10.1103/PhysRev.115.824

[9] Henri Orland, "Développement des théories de champ moyen en physique nucléaire et dans les milieux désordonnés," thèse d'état, Paris 11 (1981). http://upsaclay.focus.universite-paris-saclay.fr/permalink/f/1gllaij/33PUP_Alma_UNIMARC21130538840006051

[10] Laboratoire de physique des solides: https://en.wikipedia.org/wiki/Laboratoire_de_Physique_des_Solides

[11] See, *e.g.*, G. Toulouse, "André Blandin et la physique des verres de spin. Trois étés alpins : 1958, 1968, 1978," *Ann. Phys. Fr.* **10**, 85-100 (1985). https://doi.org/10.1051/anphys:0198500100108500

[12] 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

[13] See, *e.g.*, C. De Dominicis and L. Peliti, "Field-theory renormalization and critical dynamics above T_c: Helium, antiferromagnets, and liquid-gas systems," *Phys. Rev. B* **18**, 353 (1978). https://doi.org/10.1103/PhysRevB.18.353

spin glasses, because of the fact that an alternative to averaging the disorder was to do the average of time-dependent correlation functions and things like that[14]. That's how Cirano came to spin glasses. Of course, then he went to work with replicas. He gave up on dynamics and went working with replicas.

So Marc and Thomas used to come to Saclay, and we used to work here [at IPhT]. One anecdote that I can tell you. We used to have extremely big formulas. There is an amphitheater in Saclay—the Amphi Bloch—which has very big blackboards. We used to work very often in this amphitheater, so that we could write formulas which would extend over the whole blackboard. Then, of course, we would go to lunch. I remember one day the cleaning lady wiped out everything we had written, and Cirano was completely mad. We had lost all the work of the morning. But it was very handy to have these big blackboards and to be able to write these long formulas on the blackboard. It was a very friendly atmosphere, very nice. We were very good friends and it was very pleasant.

PC: And very collaborative? Would you all do the same work in parallel or would you break down the problem?

HO: [0:08:57] We would work together on the blackboard all the four of us. One after the other, depending on the… Then, of course, we would redo the calculation individually to make sure that there was no mistake. That's how it was.

FZ: Who was your supervisor for the fermion part, and was there any interaction between the two, besides the Grassmann variables that are common to the two?

HO: [0:09:29] My first supervisor was Richard Shaeffer[15], who was a nuclear physicist and switched to astrophysics and cosmology. Actually, I didn't want to follow him into this field, although we have paper on this subject in astrophysics. I started my thesis in ’76. In those days, a thesis was five-six years long. It's only later that is it was restricted to three years. In that

[14] C. De Dominicis, “Dynamics as a substitute for replicas in systems with quenched random impurities,” *Phys. Rev. B* **18**, 4913 (1978). https://doi.org/10.1103/PhysRevB.18.4913; “Toward a mean field theory of spin glasses: The TAP route revisited,” *Phys. Rep.* **67**, 37-46 (1980). https://doi.org/10.1016/0370-1573(80)90077-0

[15] See, *e.g.*, H Orland, R Schaeffer, “Tidal bore effect in heavy ion collisions,” *J. Phys. Lett.* **37**, 327-331 (1976). https://doi.org/10.1051/jphyslet:019760037012032700; “Single particle states in nuclei,” *Nucl. Phys. A* **299**, 442-464 (1978). https://doi.org/10.1016/0375-9474(78)90382-2; “Two-body collisions and time dependent Hartree-Fock theory,” *Z. Phys. A* **290**, 191-204 (1979). https://doi.org/10.1007/BF01408115

time, it was called a thèse d'état, and it was five-six years typically. In the middle, after about three years—I started in '76 and in '79—Richard Shaeffer decided to switch to astrophysics. I was always more interested in quantum many-body statistical physics. It turned out that at that time I had this interaction with Cirano, so I decided to continue with Cirano on problems of disorder and related problems.

FZ: So the quantum aspect was not there at that time.

HO: [0:10:57] No.

FZ: You mentioned that Cirano came to glasses because of the dynamics, and I know his paper on the dynamical average as a substitute for replicas[16]. What was at the time—in the early '80s—the perception of this interplay between dynamics and replicas? Why did Cirano decide to give up dynamics to concentrate on replicas?

HO: [0:11:33] I think because of… In some sense, the way to calculate the… You know, Cirano was a specialist of using the Martin-Siggia-Rose formalism for dynamics. In the Martin-Siggia-Rose formalism essentially you constrain by a delta function the equation of motion—the Langevin equation—and then you have a Jacobian which is essentially the determinant with a derivative of the equation. In some sense, he applied this kind of formalism to calculate the solutions of the TAP equations by using exactly the same formalism, except that there is no time. He started to work on this, and then we realized that the number of solutions of these equations was exponentially large[17]. That's how I think he started to come to replicas and replica symmetry breaking and this kind of things: to be better adapted to tackle the problem than dynamics.

Of course, there was Sompolinsky who was working on the dynamics. Sompolinsky and Annette Zippelius had been working on that[18]. There was a nice paper by Haim Sompolinsky alone where he introduced and gave an interpretation of the replica symmetry breaking in terms of dynamics[19].

---

[16] See Ref. 14.

[17] 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

[18] 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; "Relaxational dynamics of the Edwards-Anderson model and the mean-field theory of spin-glasses," *Phys. Rev. B* **25**, 6860 (1982). https://doi.org/10.1103/PhysRevB.25.6860

[19] H. Sompolinsky, "Time-dependent order parameters in spin-glasses," *Phys. Rev. Lett.* **47**, 935 (1981). https://doi.org/10.1103/PhysRevLett.47.935

PC: Speaking of which, I think the following paper you wrote with Marc Gabay was about the Sompolinsky functional[20].

HO: [0:13:50] I'm pulling up—while thinking—the paper, because as you may imagine… To tell you the truth, I forgot a lot of what is in this paper.

PC: It's a replica derivation of the Sompolinky functional for mean-field spin glasses.

HO: [0:14:05] You know that when you do dynamics, there are two types of order parameters which appear: one is a correlation function, one is a response function. The idea was to try to introduce replica symmetry breaking, which would introduce two types of functions also. One more or less related to correlations, one to linear response or something like that. That's what I remember. We had a scheme for… Unfortunately, I should have prepared better this interview. We're talking about a paper from August 28, 1981. It's over 40 years ago.

PC: You then left for MIT for a postdoc position, is that right?

HO: [0:15:22] Actually, I was assistant professor.

PC: Was it a tenure-track position at MIT?

HO: [0:15:26] No. I was a visiting assistant professor. I had already my position in Saclay at that time.

PC: During your time in Cambridge, did you interact at all with Sompolinsky and others from the Halperin group[21]?

HO: [0:15:46] We discussed a lot, but not really interacted. I was teaching several courses at MIT. Actually, I was teaching one course per semester and I stayed for two years, so I taught a course on quantum many-body and I taught a course on disordered systems. In this course on disordered systems, I was teaching spin glasses. I was teaching quite a few topics, but I was teaching spin glasses and replicas and replica symmetry breaking. I

---

[20] 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/jphyslet:019810042023052300

[21] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Bertrand I. Halperin*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 14 p. https://doi.org/XXXXXXXX

had quite amazing students. Mehran Kardar[22] was a student at that time. He was a graduate student. I am talking about him, because he later used replicas and things like that[23]. I had David Andelman[24] etc.

I was discussing with Sompolinsky, and also at MIT I was discussing with Nihat Berker, who was a good friend of mine. He was telling me he was doing this work on Migdal-Kadanoff renormalization of frustrated systems and chaotic trajectories. This paper was with his student, Susan McKay, and maybe Kirkpatrick[25]. You know this paper.

PC: During your interactions with Sompolinsky and maybe others in the Halperin group at that time, did you get any insight into their reaction to the ideas of symmetry breaking and how it was being received?

HO: [0:17:44] Yes. Not only that, but I saw a seminar by Anderson[26] at that time. That was '81-'82. He was convinced that it was exact, that it was the solution of the problem. It was obvious to everybody that this was the solution, but nobody knew how to prove it, of course. That was the general consensus. This is the solution. Of course, nobody understood the mathematics, the derivation. Everybody saw the steps and there was some logic in the steps, but nobody could justify anything.

PC: After that, you largely left the field for a few years, except you had a paper with Cirano[27] and one of his thesis students, François Lainée[28].

HO: [0:18:42] Actually, François Lainée was a student of Jean Zinn-Justin[29].

PC: I see. What led to that work, and you getting back into spin glass ideas?

---

[22] Mehran Kardar: https://en.wikipedia.org/wiki/Mehran_Kardar

[23] See, *e.g.*, M. Kardar and D. R. Nelson, "Commensurate-incommensurate transitions with quenched random impurities," *Phys. Rev. Lett.* **55**, 1157 (1985). https://doi.org/10.1103/PhysRevLett.55.1157; M. Kardar, "Depinning by quenched randomness," *Phys. Rev. Lett.* **55**, 2235 (1985). https://doi.org/10.1103/PhysRevLett.55.2235

[24] David Andelman: https://en.wikipedia.org/wiki/David_Andelman_(physicist)

[25] S. R. McKay, A. N. Berker and S. Kirkpatrick, "Spin-glass behavior in frustrated Ising models with chaotic renormalization-group trajectories," *Phys. Rev. Lett.* **48**, 767 (1982). https://doi.org/10.1103/PhysRevLett.48.767

[26] Philip W. Anderson: https://en.wikipedia.org/wiki/Philip_W._Anderson

[27] C. De Dominicis, H. Orland and F. Lainée, "Stretched exponential relaxation in systems with random free energies," *J. Phys. Lett.* **46**, 463-466 (1985). https://doi.org/10.1051/jphyslet:019850046011046300

[28] François Lainée, *Etude du modèle d'Ising en champ magnétique aléatoire : étude de quelques questions en dynamique critique*, thèse de doctorat, Paris 6 (1985). https://www.sudoc.fr/175905347

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

HO: [0:18:53] It was a paper by Mézard—I don't remember exactly where[30]—where they showed that the distribution of the tail of the lowest lying states was exponential, and that there was a dominant state which was exponentially rare, and things like that. Then, we had the idea: why not try to do a kind of master equation with a distribution of energies, which mimics somehow the low part of the energy spectrum of a spin glass and see what it gives for the dynamics. That was the idea. At that time I was working on master equation in dynamics and things like that.

PC: From Saclay, I guess you had a pretty good perspective on the community of spin glasses at that time. Can you tell us a bit how it was working? Who was interacting with whom?

HO: [0:20:04] As frequent visitors we had Mike Moore[31] and Alan Bray, but I think Mike Moore was here more often. He was funny. Very funny guy. We were having a good time. He was joking all the time. Cirano was a great calculator. Cirano could calculate anything, as you can see in his papers with the replicas and all these things. Of course, this virtuosity and the fact that Cirano was not afraid to take on any calculation was funny for us. That he could do anything [was funny] especially for Mike Moore. I mean it was really admiration that he could do anything like a bulldozer. Doing all this algebra. I remember, he was using this old computer paper—very big [sheets—and] he was writing with a pencil when he was doing this replica symmetry breaking stability analysis[32]. So we had a lot of visits from Mike Moore.

Then, we had also Daniel Amit[33], [who] used to come. That was a bit before, but he used to come quite often to Saclay also. He was on sabbatical in Saclay in the late '70s, so before the spin glasses. But I think that his stay in Saclay probably triggered—Sompolinsky also went to Saclay, by the way—their interest in neural networks and their analysis of neural networks with the capacity, with the transition from associative memory to the spin glass phase[34]. Probably they were, I wouldn't say

---

[30] M. Mézard, G. Parisi and M. A. Virasoro, "Random free energies in spin glasses," *J. Phys. Lett.* **46**, 217-222 (1985). https://doi.org/10.1051/jphyslet:01985004606021700

[31] 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

[32] 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

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

[34] See, *e.g.*, 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).

inspired, but it was part of ideas that they might have gotten interested in. Daniel Amit was not working on spin glasses, but he was very good friend and he loved to talk with Cirano.

PC: So it was part of the social network. There were contacts that they established in Saclay and persisted beyond their visit.

HO: [0:22:55] Yes. Gutfreund also used to come[35]. He came quite a few times. Hans-Jürgen Sommers came quite often. Then we had, of course, Toulouse[36] who was coming. Toulouse was working a lot with Roger Balian[37]. Brézin was here, by the way, at that time[38]. Édouard Brézin was still in Saclay until, I think, '84-'85. Derrida, of course, was here in Saclay[39]. Derrida came out with the random energy model, which was quite amazing. It's really quite an achievement. Such a simple model and it has so much physics, so much insight into spin glass models somehow.

I remember one year—I couldn't tell you which year, but it was probably around '84-'85—David Gross[40] was on sabbatical here. He spent half of his sabbatical here, in Saclay, half of his sabbatical in ENS, in Paris. He had discussions with Cirano. That's when they did their work on the Potts model[41]. (Was it the Potts model?) There was this famous paper…

FZ: Yes, yes. The Gross-Mézard[42]…

HO: [0:24:41] Yes, Gross-Mézard, exactly.

---

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

[35] See, e.g., P. Charbonneau, *History of RSB Interview: Hanoch Gutfreund*, transcript of an oral history 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

[36] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse

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

[38] 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

[39] 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

[40] David Gross: https://en.wikipedia.org/wiki/David_Gross

[41] The Potts spin glass analysis followed quickly after the stay in Paris, but was done in Jerusalem. 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

[42] 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

Who else was there? Next door to us. (In Saclay, there is a next door experimental lab, the SPEC, Service the Physique de l'État Condensé.) There was a big group working on spin glasses, Éric Vincent, Roger Hamman, and they were having visitors like Ray Orbach[43], who was—I think—in Riverside.

PC: UCLA.

HO: [0:25:22] Is it UCLA? But he became dean or president of a different UC. Yes. At that time he was at UCLA.

FZ: What about Elizabeth Gardner[44]? Did you interact with her? At the time, she was working on replica solutions.

HO: [0:25:45] Yes. Absolutely. I couldn't put a year, probably ['83-'84]. She came to Saclay as a postdoc of Claude Itzykson[45]. Actually, she shared my office for at least one year, maybe two. She was extremely shy, extremely modest. Initially, she was not supposed to work on this, but for some reason she started to…

Ok, we discussed quite a lot because she was in my office. Although she was shy, we ended up talking. I think she started collaborating with Bernard Derrida at that time. That's how she got involved, by all these discussions. She got involved less in spin glasses than in neural networks and all these models.

FZ: She also had a paper on the replica instability in spin glasses from that time[46].

HO: [0:26:58] Ok. Maybe.

FZ: You had a lot of discussions, but you never collaborated.

HO: [0:27:05] No. She was really peculiar. She was absolutely outstanding, but difficult to interact [with]. She was too young to have such a tragedy.

FZ: You were about the same age.

HO: [0:27:30] I'm a bit older than her, at least four or five years older.

---

[43] Ray Orbach: https://en.wikipedia.org/wiki/Raymond_L._Orbach

[44] Elizabeth Gardner: https://en.wikipedia.org/wiki/Elizabeth_Gardner_(physicist)

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

[46] 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

FZ: So Thomas Garel and Marc Gabay were around at the same time.

HO: [0:27:39] Yes, but they were in Orsay.

FZ: So they didn't really talk to her?

HO: [0:27:44] No. She was really in Saclay. Bernard Derrida was not at École Normale yet, and so she discussed a lot with Derrida and with me because I used to take her for lunch. She was extremely shy and lonely.

FZ: One last question about the community at the time. (I don't know exactly how to formulate this question, because it's something that is a bit confused in my mind.) At the time, in the late '70s-early '80s, there were these people that were already quite famous that were working on spin glasses, like Phil Anderson and David Thouless[47]. You mentioned that Anderson was excited by the Parisi solution. We have been told by de Almeida[48] that Thouless was also quite excited by the Parisi solution.

Then, Anderson also worked a little bit on the application of these ideas of RSB to optimization problems[49]. But then it seems that something happened at some point. Both Anderson and Thouless and others stopped really working on these ideas, and considering these as important ideas, and became quite cold, I think, with respect to this replica symmetry breaking. Do you understand what happened?

HO: [0:29:22] I think they were discouraged by the mathematics, because the mathematics was so weird and so unusual that nobody had a single idea on how to prove this scheme, how to prove the Parisi equation. Although they believed that the final solution was correct, and probably the equation was correct, people were a bit afraid, I think, to use it, to do this kind of algebraic manipulation.

FZ: Is this why you think the French and the Italian schools pushed this a bit more? Because their training was oriented more towards mathematics?

HO: [0:30:14] Yes. That's my feeling. Most Americans didn't touch replica symmetry breaking. They stayed away.

---

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

[48] P. Charbonneau, *History of RSB Interview: Jairo de Almeida*, 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.7de8emt7

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

FZ: When you went to teach these ideas at MIT, what was the reaction of the students?

HO: [0:30:40] They liked the formalism. They liked the way it was presented. Look, the formalism is beautiful. Although you [have to] handwave to justify it, the formalism is quite beautiful, quite appealing, and the final equation that you get is very nice. It was the first time, of course. Nobody was teaching this at the time. As a young assistant professor, I was given the possibility to teach anything I wanted, so I thought I would… I gave a course on disordered systems: percolation, polymers, spin glasses, and whatever. First of all, I was having fun because it was nice. The people were very interested.

Shortly after, maybe one or two years later, Kardar came up with his paper on polymers in random media[50], which he solved by using replicas and the Bethe ansatz. When you use replicas for this problem, you have a many-body problem in quantum mechanics with n=0, with a delta interaction, which you can solve exactly. By getting the ground state, he could get the exact energy of this model, which you can map to a Burgers equation and all these things. He got the exact result of the ground state energy and the corrections by using this replica method and the solution of the Bethe ansatz for a delta [interaction], which I was teaching in my quantum many-body course. He did a nice synthesis.

PC: A few years later, you worked again on spin glasses in the context of protein folding. Can you walk us through what lead to that?

HO: [0:32:50] I got interested in protein folding when I was at MIT, actually, because protein folding looked very much like… You have a sequence of amino acids, which is of course not random, but to a first approximation you could imagine that it's a quenched random sequence. You know that in these proteins you have amino acids, so you have residues which interact—some attract, some are repulsive—so the most schematic model you can imagine is just to have a random sequence with random interactions between the various residues. That's how we came to this problem. Of course, we used the formalism of replicas, but then it's much more complicated because you have a chain which connects everything. That's what led us to do this problem. The first paper that we did we used

[50] M. Kardar, "Replica Bethe ansatz studies of two-dimensional interfaces with quenched random impurities," *Nucl. Phys. B* **290**, 582-602 (1987). https://doi.org/10.1016/0550-3213(87)90203-3

random interactions between pairs[51]. That was our first work. Then we thought that after all there is a sequence, so it's more like you have a random-field model, somehow, where you have a sequence of amino acids and then there is an interaction depending on the amino acid, on the sequence. If you have 20 types of amino acids, 20x19/2=190 types of interactions, or something like that, whereas in the spin glass model there is not really a sequence [dependence]. Any pair *ij* will have a specific interaction. If you have the same amino acid *ij* and some different place, they will have a different interaction in the spin glass model. So we corrected our initial spin glass model to be more like a random-field model[52].

PC: What was the relationship between spin glass ideas and protein folding at that time? In particular, we note that your work was following that of Peter Wolynes[53], a year prior.

HO: [0:35:47] Actually, that was what triggered this idea, the work of Wolynes and Bryngelson. It was known experimentally that in proteins you have some kind of freezing of lateral chains, and freezing of certain sequences, and that you have to be extremely careful when you cool them to fold them, especially complex proteins—not the smaller ones but the bigger ones—because they can get trapped in various minima and things like that. From this consideration, Wolynes came out with the idea that, after all, you can describe… When you have a freezing transition you can think about the random energy model, so he made a so-called random energy model for protein folding, which was really very phenomenological and which was just to say that the system transitions between random states. The random states correspond to the various conformations of the protein in the folding process, and that the system will… When you look at the random energy model, you have the density of states, which has the shape of an inverted parabola. When you write the temperature, it's the tangent to the parabola, and at some point the slope of the tangent comes to the intersection of the parabola with the real axis, then you have a freezing tradition. Wolynes and Bryngelson used these ideas to try to describe in a phenomenological way the protein folding problem. Namely, that what is random are the different contacts between the various amino acids in the folding process. That's how we got the idea. In this model there was no chain, no polymer. With Thomas, we thought: "Can we imagine a polymer

---

[51] T. Garel and H. Orland, "Mean-field model for protein folding," *Europhys. Lett.* **6**, 307 (1988). https://doi.org/10.1209/0295-5075/6/4/005

[52] T Garel, H Orland, "Chemical sequence and spatial structure in simple models of biopolymers," *Europhys. Lett.* **6**, 597 (1988). https://doi.org/10.1209/0295-5075/6/7/005

[53] J. D. Bryngelson and P. G. Wolynes, "Spin glasses and the statistical mechanics of protein folding," *Proc. Nat. Acad. Sci. U.S.A.* **84**, 7524-7528 (1987). https://doi.org/10.1073/pnas.84.21.7524

model, which will precisely go in the direction of this, which will be a microscopic model, which will have some of the characteristics of this random energy model, and which will describe this freezing transition?" That's how we came to this model. Of course, the most natural idea was to take a chain and imagine that you have random interactions between any pair of amino acids, which we would [do] afterwards to make more random-field like because of the sequence. I forgot. You see, you're reminding me that all this was triggered by the Wolynes-Bryngelson paper.

PC: Was this by reading the paper or talking with Peter Wolynes[54]?

HO: [0:39:19] Reading.

PC: Did you have any interaction with Peter Wolynes?

HO: [0:39:24] Wolynes, I met later, in '92, I think, at a session in Santa Barbara, where we were together at the time[55]. That's also where I met Dave Thirumalai.

PC: You largely left the field of spin glasses or even spin glass applications after that work. Did you nevertheless follow the development of the field beyond then?

HO: [0:39:55] A little bit with optimization problems. I've always been interested in optimization and its relationship to spin glasses. Actually, I wrote a paper in '84-'85 about the travelling salesman problem and the assignment problem, where you have an infinite number of order parameters[56]. But I did it with no replica symmetry breaking, because it was already so complicated because you have this infinity of order parameters. My paper came out together with the paper of Mézard and Parisi on the same problem[57], where they calculate this famous $\pi^2/12$. The sum when you take the assignment problem—the bipartite matching problem, when you take the weights to be exponentially distributed—they could solve the mean-field equation exactly, and they got that the ground state cost, or ground state energy, was $\pi^2/12$, which was very beautiful and which was confirmed afterwards to be exact.

[54] Peter G. Wolynes: https://en.wikipedia.org/wiki/Peter_Guy_Wolynes
[55] Mini Program on Protein Folding, Institute of Theoretical Physics, UCSB, 1993.
[56] H. Orland, "Mean-field theory for optimization problems," *J. Phys. Lett.* **46**, 763-770 (1985). https://doi.org/10.1051/jphyslet:019850046017076300
[57] M. Mézard and G. Parisi, "Replicas and optimization," *J. Phys. Lett.* **46**, 771-778 (1985). https://doi.org/10.1051/jphyslet:019850046017077100

PC: You left the field, but how influential was your work on spin glasses on other problems that you've tackled beyond that, or other aspects of your research in your career beyond that?

HO: [0:41:28] I've used replicas many times in many contexts, in different other problems like, for instance… I've been working in soft matter, and in soft matter, for instance, I did some work on conformations of random heteropolymers. There I used replicas[58]. Since I'm a bit lazy, I usually like to use replica symmetric solutions, but in some cases one should use replica symmetry breaking. When the problems are more complex than just the pure Sherrington-Kirkpatrick model, replica symmetry breaking can really be messy and complicated. Especially if you have more than one order parameter, then it's a total mess.

PC: So it's remained with you essentially throughout you career.

HO: [0:42:53] Actually, we did one paper with Cirano and one of his Hungarian collaborators, Temesvári[59], on the random-field Ising model, where we found a spin glass phase, which apparently cannot exist. I think Lenka Zdeborová showed later that this spin glass phase cannot exist by very general principle, an inequality[60]. I think she showed me that when she arrived in the lab.

PC: You mentioned a class you taught at MIT, which touched on spin glasses. Did you ever teach other classes, or did you reuse this material in other classes?

HO: [0:43:39] Yes. I taught that at the Weizmann institute, where I was a visiting professor in '87. Then, I think I taught at ENS, in one of these specialized courses in '90 or '92. I think that was it.

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

HO: [0:44:22] It was very exciting, of course, because it was so new, and so weird somehow, so unusual this mathematics. What was really amazing is the simplicity of the SK model, and the fact that one has to go through all these weird mathematics to solve it. That was really something fascinating,

---

[58] T. Garel, H. Orland and E. I. Shakhnovich, "Random heteropolymers in layered fluids," *J. Chem. Phys.* **93**, 2043-2047 (1990). https://doi.org/10.1063/1.459081

[59] C. De Dominicis, H. Orland and T. Temesvári, "Random field Ising model: dimensional reduction or spin-glass phase?" *J. Phys. I* **5**, 987-1001 (1995). https://doi.org/10.1051/jp1:1995178

[60] F. Krzakala, F. Ricci-Tersenghi and L. Zdeborová, "Elusive spin-glass phase in the random field Ising model," *Phys. Rev. Lett.* **104**, 207208 (2010). https://doi.org/10.1103/PhysRevLett.104.207208

until of course the exact solution came out, and showed that all this was exact.

There is somebody, I think, in the field which has been a bit underestimated, which is André Blandin, who was quite influential on many people, and especially in the idea of replica symmetry breaking and all these things.

PC: Did you know him? Did you interact with him during those years?

HO: [0:45:30] A little bit, yes. I had him as a teacher when I was a student. He was teaching hydrodynamics. He was a bit messy as a teacher, but very inspiring. That was for hydrodynamics. Then, I had a few discussions with him on spin glasses, but never collaborated really. I had a lot of indirect thoughts of him through Thomas Garel and Marc Gabay, but I didn't work directly with him.

PC: He was influential in what sense, then?

HO: [0:46:19] In the sense that I think… If I remember correctly, he had a replica symmetry breaking scheme, which was essentially equivalent to the one-step replica symmetry breaking that Parisi would devise later[61]. Parisi went up to the end, but the first step of the RSB was really the same as the one Blandin had devised a couple of years before. I don't know if this is… This is in that sense.

PC: So you knew of this work because he had presented it, or is it a retrospective view?

HO: [0:47:10] No. When he presented it, all these replica symmetry breaking schemes seemed quite artificial. Why do this, and not [this]? There was no physical interpretation behind it. It was just looking for the solution of these equations that would be better than no replica symmetry breaking, so it was somehow a curiosity. I think Blandin probably had more insight in it. When you read his paper—I remember reading his paper at the time—it's not very clear why he does it. But apparently he had some insight which I didn't have at that time. It was an interesting paper.

FZ: There is a paper, which we discovered while doing these interviews, which is a kind of review that Blandin wrote. There is the beautiful idea, very

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

physical idea, of obtaining the Edwards-Anderson order parameter by coupling two replicas. Then it leads, once you've introduced additional…

HO: [0:48:36] Yes. Exactly. You have all these order parameters, intra-replica, inter-replica and things like that. Yes, I remember this paper. It looked like two real replicas almost.

FZ: Yes, it exactly contains, at the embryonic stage, the physics of…

HO: [0:49:00] At that time, it was difficult to understand what was the motivation. Why do this? Why not do that? It was very…

What else can I tell you about all this? I would have to… I'm sorry, I should have prepared it better.

FZ: It's ok.

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

HO: [0:49:46] You see in the back. At some stage, when I will retire, probably I will look through these. I'm sure I will have some notes about some calculations with Cirano or with other [people]. I don't know what to do with it. My first reaction would be to get rid of all this.

FZ: The lecture notes of the course you gave at MIT would be particularly interesting, I think.

HO: [0:50:22] This I could probably find.

FZ: It's probably the first course that has ever been taught about RSB in the US, so that would be very interesting. If you ever have letters or other exchanges with people at the time.

PC: With Sompolinsky, or with Cirano while you were away.

HO: [0:50:47] Ok. I will look for that, but I don't know what I kept on this. My course, I may have. I have to…

FZ: If you find it, we would be happy to have a look.

PC: In general, I encourage you to consider talking to an archivist. I don't know how it works in Saclay, but universities usually have archive services. In the Paris area, there are ways to get organized.

HO: [0:51:22] Ok. I will look. I will think about that. Thanks for the advice.

PC: Thank you very much for this conversation.

HO: Thank you.