History of RSB Interview: Nicolas Sourlas

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Interviewers:

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

Francesco Zamponi, ENS-Paris

Location:

Over Zoom, from Prof. Sourlas' home in Paris, France.

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PC: Good morning, Prof. Sourlas. Thank you very much for sitting down with us. As we discussed ahead of time, the theme of this interview is the formulation and understanding of replica symmetry breaking, which goes roughly from 1975 to 1995. Before we dive into that subject, we'd like to ask you a few questions on background. First, can you tell us a few things about your youth and what eventually got you to study physics?

NS: [0:00:36] I grew up in Athens, in Greece. I started engineering, at a famous engineering school in Athens[1]. I didn't like very much the practical things—practicing as an engineer—so I decided to switch to physics. Then, I came to France, that time to Orsay. (In France we had what was called a DEA[2].) I started a DEA in Orsay, doing physics. It was in high-energy physics. When I was in Orsay, my first year, Daniele Amati[3], who at that time was at CERN, came as a visiting professor and I started working with him. Amati was collaborating with Bouchiat[4] and Gervais[5] in Orsay. When he left, I continued to work on high energy with Gervais and Bouchiat[6]. Then in '74, the group moved to École Normale Supérieure, so I moved with the group to École Normale Supérieure[7]. So since '74, I am in École Normale Supérieure.

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[1] National Technical University of Athens: https://en.wikipedia.org/wiki/National_Technical_University_of_Athens

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

[3] Daniele Amati: https://en.wikipedia.org/wiki/Daniele_Amati

[4] Claude Bouchiat: https://en.wikipedia.org/wiki/Claude_Bouchiat

[5] Jean-Loup Gervais: https://en.wikipedia.org/wiki/Jean-Loup_Gervais

[6] See, *e.g.*, C. Bouchiat, J.-L. Gervais and N. Sourlas, "Dual conserved current and local field interpretation of the multiparticle Veneziano amplitude, *Lett. Nuovo Cimento* **3**, 767–775 (1970). https://doi.org/10.1007/BF02753427

[7] The Laboratoire de physique théorique de l'École normale supérieure was founded in 1974 by Philippe Meyer and Claude Bouchiat, when a group of theoretical physicists in high-energy and particle physics

FZ: Why did you decide to move to France from Greece? Why France and not another country? Did you have any connection? Was there a special connection between Greece and France?

NS: [0:02:49] No. I had a degree in engineering. I wanted to do physics. I applied to a couple of the best universities in the US. Of course, I needed a scholarship but I was turned down. In France, engineering degrees are appreciated, so I was accepted in France. That's how I came to France. I'm coming back. In '72 came out the Wilson-Fisher paper on the renormalization group and the epsilon expansion[8]. People were studying the paper. I remember Paul Martin[9] was visiting Saclay, and he gave a series of completely non-understandable lectures on phase transitions and the renormalization group. But I got interested in the subject and then the turning point was summer of '73. In the summer of '73, there was a very important summer school, in Cargèse, on the renormalization group organized by Brézin[10]. This played a very important role in Europe for the dissemination of ideas of the renormalization group. You had people like Kadanoff[11], Eliott Lieb[12], Callan, Symanzik etc. lecturing at the school. The most important figure, at least for me, was Ken Wilson[13].

I don't know whether you know Cargèse at that time. Most of the buildings did not exist, so the students were camping on the grounds of the school. Professors had rooms in hotels in the village, with one exception. The exception was Ken Wilson, who was camping the grounds of the school with the students. The other professors were doing their lecture and then went to the hotel. Ken Wilson was there all the time, and talking to students all the time, giving private lectures, private seminars. We had dinner, at time almost every day, in the village with Ken. He was at the peak of his career. He was really, really impressive. He gave, of course, his planned lectures but his interest at that time was lattice gauge theories. He was developing

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moved from the Laboratoire de physique théorique des hautes énergies of the Université d'Orsay to École normale supérieure in Paris.

[8] K. G. Wilson and M. E. Fisher, "Critical exponents in 3.99 dimensions," *Phys. Rev. Lett.* **28**, 240 (1972). https://doi.org/10.1103/PhysRevLett.28.240

[9] Paul C. Martin: https://de.wikipedia.org/wiki/Paul_C._Martin_(Physiker)

[10] Cargèse Summer School on Field Theory and Critical Phenomena, organized by E. Brézin and J. Charap, July 1973. 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

[11] Leo Kadanoff: https://en.wikipedia.org/wiki/Leo_Kadanoff

[12] Eliott Lieb: https://en.wikipedia.org/wiki/Elliott_H._Lieb

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

lattice gauge theories. This is July ’73. The paper came two years later[14]. He lectured on everything. Asymptotic freedom was very recent[15]. He had his ideas about quark confinement that were developing at the time. He was talking about projects which did not finish, like fully-developed turbulence. It was really impressive. This decided me, after finishing my thesis, to go to Cornell for a postdoc. That's how I decided to go to Cornell. I would mention another participant, another professor. This is the first time in my life I met Giorgio Parisi. Giorgio Parisi was 25 years old at that time, but he was professor at the school and I was a student. Giorgio was a professor, by far the youngest.

FZ: Was he camping or was he in a hotel?

NS: [0:08:55] I think he was in a hotel in the village. Anyway, I don't know. But we had several discussions. We were discussing also other subjects than physics. Giorgio was very much interested in what was going on in Greece. At that time it was a dictatorship[16]. I discovered that he knew Greece much better than I did. He had travelled already several times to Greece. So we were discussing a lot of time about Greece. What I discovered much later—probably Francesco is not very familiar with that—[is that] the older generation Italians, say Giorgio’s generation, had a very profound classical education.

After that, I went to a postdoc to Cornell. I went there in the fall of ’75. Just for your amusement, in my first encounter with Wilson at Cornell—I knew him quite well already from Cargèse—he said: “What are you working on?” At that time, I was working—it was a fashionable subject—on semi-classical approximations. I explained to him. He said, in his usually frank way of speaking: “There is no point. I have tried a few years ago and I convinced myself—at least for high-energy physics—this is not a useful tool, so you should look for something else.” I don't remember whether it was this time or another time, he suggested me the following problem: “You should look at the breaking of chiral symmetry in gauge theories. This is the interesting subject which you should look at.” You know what is the symmetry. The pions are the pseudo-Goldstone bosons[17]. He said: “You should look at chiral symmetry breaking.” Fortunately I did not follow his advice, because I think until today the subject is not very well understood.

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[14] K. G. Wilson, “Confinement of quarks,” *Phys. Rev. D* **10**, 2445 (1974). https://doi.org/10.1103/PhysRevD.10.2445

[15] D. J. Gross, “The discovery of asymptotic freedom and the emergence of QCD,” *Proc. Nat. Acad. Sci. U.S.A.* **102**, 9099-9108 (2005). https://doi.org/10.1073/pnas.0503831102

[16] Greek Junta: https://en.wikipedia.org/wiki/Greek_junta

[17] Chiral symmetry breaking: https://en.wikipedia.org/wiki/Chiral_symmetry_breaking

In Cornell Ken was giving a series of lectures on lattice gauge theories, most of the material was unpublished. If you look retrospectively since that time what were the major contributions to the subject, none came from the students of Cornell. They were extremely bright students. I was very impressed of the quality of the students. Michael Peskin[18], for example, was very impressive. There is an explanation for that. All the contributions people get credit for since that time in lattice gauge theory were already in Wilson's class. He had all those contributions in 1975, but he did not publish them. So it was not possible to write a paper from a student of Cornell for something which was already taught in the class.

PC: After your time at Cornell, you came back to ENS, to the LPT. Were you a CNRS member immediately?

NS: [0:15:15] Yes. I was already a CNRS member before going to Cornell. I should add a thing, which would amuse you, to show that the job situation was very different at that time. When I was a first-year graduate student in Orsay, somebody came to my office and said: "We have a job for *maître-assistant*[19] at the university and no candidate. Would you accept the job?" That's how I got my first job, without being a candidate. Then, the next year I was hired by CNRS, where I didn't have any teaching obligation.

PC: In notes you sent us, you mentioned that you got to know Giorgio more when you came back to the LPT of ENS[20]. Did you immediately start working together? If yes, what was the program?

NS: [0:16:50] We knew each other already from Cargèse. So the first contact was easy. As Francesco knows very well, the contact with Giorgio regarding scientific matters is extremely easy. The first thing we worked on was trying to understand the fermion doubling problem in lattice gauge theories. But, like many other people, we failed. Nothing came out of that. Then we did the work with Drouffe on the mean-field theory of lattice gauge theories, which means studying lattice gauge theories in large dimensions. This is the work which was published[21]. It was not the first work we published

[18] Michael Peskin: https://en.wikipedia.org/wiki/Michael_Peskin

[19] Maître-assistant: https://fr.wikipedia.org/wiki/Ma%C3%AEtre-assistant

[20] **NS:** In the fall of 1976 I came back to the ENS from a postdoc in Cornell where I had learned from Ken Wilson everything on lattice gauge theories. At the same time Giorgio Parisi arrived as a postdoc in Paris and we started working on lattice gauge theories.

[21] J.-M. Drouffe, G. Parisi and N. Sourlas, "Strong coupling phase in lattice gauge theories at large dimension," *Nucl. Phys. B* **161**, 397-416 (1979). https://doi.org/10.1016/0550-3213(79)90220-7

together, but the other one is some phenomenology work I won't mention[22]. This turned out to have importance, because at infinite dimensions, we found that there is a phase transition in lattice gauge theories, and the new phase is that of branched polymers.

It turned out that a few months later, after we published this paper, came a paper by Lubensky and Isaacson in Phys. Rev. Letters[23], studying branched polymers. They claim, in this paper, that the upper critical dimension is eight for branched polymers. This made Giorgio extremely excited, because Giorgio had an intuition. You know, if you do a high-temperature expansion, the diagrams for spins are lines. For gauge theories, they are surfaces. You go from lines to surfaces by doubling the number of dimensions. We know that the critical dimension for spins is four. Lubensky and Isaacson were claiming that for branched polymers it's eight, so this was fitting exactly his intuition. This made Giorgio extremely excited. Furthermore, if you extrapolate, you could say that this is true also for the lower critical dimension. For spins, the lower critical dimension is two, so this would mean that for gauge theories, or random surfaces, the lower critical dimension would be four. This would mean that there was no phase transition at four dimensions. This would mean that quarks were confined. The problem of quark confinement was one of the major problem of physics, and this was a very strong result that there was no phase transition in gauge theories in four dimensions, so that confinement was true. At that time, Giorgio was mostly interested in high-energy physics, in particular gauge theories, so he became very excited about it.

PC: Can you tell us a bit how you were working with Giorgio? Would you two meet every day? Just give us a hint of how that went on.

NS: [0:21:59] Yes. He had an office, so we were meeting to have lunch together. But Giorgio had many interactions in the Paris area in parallel. He was also interacting with people at Saclay, for example, Itzykson[24], Brézin, Zinn Justin, Zuber, ... He even wrote a paper on semi-classical approximation with Balian and Voros[25]. So he had many interactions. Some days he went to Saclay, some other days with me. We had very regular meetings.

[22] G. Parisi and N. Sourlas, "A simple parametrization of the Q^2 dependence of the quark distributions in QCD," *Nucl. Phys. B* **151**, 421-428 (1979). https://doi.org/10.1016/0550-3213(79)90448-6

[23] T. C. Lubensky and J. Isaacson, "Field theory for the statistics of branched polymers, gelation, and vulcanization," *Phys. Rev. Lett.* **41**, 829 (1978). https://doi.org/10.1103/PhysRevLett.41.829

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

[25] R. Balian, G. Parisi and A. Voros, "Discrepancies from asymptotic series and their relation to complex classical trajectories," *Phys. Rev. Lett.* **41**, 1141 (1978). https://doi.org/10.1103/PhysRevLett.41.1141

When we came to the subject of branched polymers, the Lubensky-Isaacson paper, Giorgio was back in Rome at that time. He was regularly writing letters to us, trying to say what he had done to understand the paper of Lubensky and Isaacson. But this was an impossible task. The paper was completely unreadable. After some time, he gave up, because there was no way to understand. (I will come back later to the branched polymers.) Lubensky and Isaacson were using replicas. That's how Giorgio first confronted the replica problem. Then, he read in the literature that replica symmetry should be broken. This was his motivation to start to understand replica symmetry breaking. His motivation was from the problem of branched polymers.

As you probably know, I wrote a paper with Giorgio on branched polymers[26]. How did it come about? At that time, papers weren't just circulating. Somebody was writing a paper, and he was sending preprints to his friends. There was no arXiv. At the time Gérard Toulouse[27] was new in École Normale. He came from Orsay. I was interacting with him, and Toulouse was a friend of Lubensky[28]. I saw on Toulouse's desk the long paper. Until then there was only the Phys. Rev. Letters paper, which was very short, and completely incomprehensible. So I borrowed the long paper, which was more explicit than the letter, but still not very understandable[29]. Then, by accident I was going to a conference in the US. I decided to stop over in Philadelphia, where Lubensky was located. I had prepared questions on everything which I did not understand in the paper. We had a session in the morning. We discussed all those questions. My understanding of the paper improved a lot, but this did not solve the problem. After that, I was for one week or something in Rome, after coming back from the US, and I explained to Giorgio what was going on. What was going on was a kind of disaster, because Lubensky *et al*. made a change of variables in the replica space to have propagators which are diagonal in the new variables, because in the original variables the propagators weren't diagonal in replica space. By doing so, the price they were paying is that they got a coupling constant proportional to $1/n$, where n is the number of replicas. Perturbation theory did not have any meaning if you have coupling constants which are $1/n$ *and* n *goes to zero*. Then, we performed the perturbation

[26] G. Parisi and N. Sourlas, "Critical behavior of branched polymers and the Lee-Yang edge singularity," *Phys. Rev. Lett.* **46**, 871 (1981). https://doi.org/10.1103/PhysRevLett.46.871
[27] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse
[28] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Tom C. Lubensky*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 13 p. https://doi.org/10.34847/nkl.f2cap2m9
[29] T. C. Lubensky and J. Isaacson, "Statistics of lattice animals and dilute branched polymers," *Phys. Rev. A* **20**, 2130 (1979). https://doi.org/10.1103/PhysRevA.20.2130

theory calculations, and we found out that at least in the one and two loop levels, the singularities were canceling out.

So we decided to sit down to understand. This convinced Giorgio that something serious was going on. We reformulated completely the problem with non-diagonal propagators in the replica space. So there were no singularities. We recognized that this was the random field Ising problem, but with an imaginary external field. We knew that, from our previous work, there was supersymmetry and dimensional reduction, which was explaining the shift of dimension by two units. So the result of Lubensky and Isaacson was correct but for different reasons. This is why we were able to find the exact critical exponents in three and two dimensions[30]. In three dimensions, I think, up to today, this is probably the only problem where you know the exact value of the critical exponents. So this is the origin of the interest of Giorgio in replicas.

PC: In notes that you sent us, you mentioned that your first encounter with the ideas of replica symmetry breaking were in Cargèse that same year, 1979[31]. Is that also where you first learned about spin glasses? Can you detail what was going on then?

NS: [0:30:26] This school was essentially on gauge theories[32]. There was nothing about spin glasses in this school. There were mathematically minded people. There were many mathematicians like Sir Michael Atiyah[33], I. Singer[34], and other famous mathematicians, participating in this school. The only person in Cargèse, who was familiar with spin glasses and replicas, was Gérard Toulouse. Gérard Toulouse had already been working in spin glasses. Probably before Cargèse I had talked a couple of times with Gérard Toulouse about spin glasses.

I should mention that Gérard Toulouse had been familiar with lattice gauge theories. The reason is the following. You know that the first papers on

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[30] See Ref. 26.

[31] **NS**: I first heard of replica symmetry breaking in a Cargèse summer school on gauge theories, in August 1979, organized mostly by Pronob Mitter. Among the participants: the mathematicians Sir Michael Atiyah, Isi Singer, Raoul Bott, and the physicists Gerard 't Hooft, Ken Wilson, Sidney Coleman, Ed Witten, Juerg Frohlich, Giorgio Parisi, Edouard Brézin, Gérard Toulouse *et al*. Parisi's seminar was on gauge theories in infinite dimensions, the work mentioned above.

[32] Cargèse Summer Institute: Recent Developments in Gauge Theories, 26 August-8 September 1979, Cargèse, France. Proceedings: *Recent Developments in Gauge Theories*, G. 't Hooft, C. Itzykson, A. Jaffe, H. Lehmann, P. K. Mitter, I. M. Singer, R. Stora eds. (New York: Plenum Press, 1980). https://doi.org/10.1007/978-1-4684-7571-5

[33] Michael Atiyah: https://en.wikipedia.org/wiki/Michael_Atiyah

[34] Isadore Singer: https://en.wikipedia.org/wiki/Isadore_Singer

lattice gauge theory in France were by Balian, Drouffe and Itzkyson[35], from Saclay. This was the thesis of Drouffe[36]. Gérard Toulouse was a member of the thesis committee examining Drouffe. He got familiarized with gauge theories—because he was a serious person and this was a new subject of him, so he learned the subject. After that, there are a couple of papers by Toulouse where he makes the connection between spin glasses and gauge theories[37]. This is the paper—I don't remember the exact reference—where he introduced the notion of frustration.

I should mention something else. You know that the annealed approximation is a very bad approximation at low temperature, because the ferromagnetic state gives the largest contribution to the average. Toulouse got the idea to force out those contributions by defining a modified annealed approximation that would add a negative gauge plaquette coupling. The negative coupling constant has the effect of suppressing unfrustrated plaquettes in the average. He made this suggestion and Bhanot and Creutz[38] did the simulations. There were doing numerical simulations for lattice gauge theories, and they simulated and established a phase diagram following this suggestion of Toulouse.

Coming back to Cargèse. Giorgio gave only private talks, essentially to Toulouse and myself, about his replica symmetry breaking scheme. But he was very anxious to interact with mathematicians. He knew that his proposal was mathematically unorthodox. As I said in the notes[39], I remember an after-dinner discussion with Raoul Bott[40]. (Raoul Bott was a professor of mathematics at Harvard. Unfortunately, he is not anymore alive.) Giorgio asked him: "Can you define a matrix with zero elements?" So in the middle of the Mediterranean night, Raoul Bott was thinking how to define matri-

[35] R. Balian, J.-M. Drouffe and C. Itzykson, "Gauge fields on a lattice. I. General outlook," *Phys. Rev. D* **10**, 3376 (1974). https://doi.org/10.1103/PhysRevD.10.3376

[36] Jean-Marie Drouffe, La Théorie des champs de jauge sur un réseau, thèse d'état, Université Paris XI-Orsay (1975). https://catalogue.bnf.fr/ark:/12148/cb359182338

[37] See, *e.g.*, G. Toulouse and J. Vannimenus, "On the connection between spin glasses and gauge field theories," *Phys. Rep.* **67**, 47-54, (1980). https://doi.org/10.1016/0370-1573(80)90078-2

[38] G. Bhanot and M. Creutz, "Ising gauge theory at negative temperatures and spin-glasses," *Phys. Rev. B* **22**, 3370 (1980). https://doi.org/10.1103/PhysRevB.22.3370

[39] **NS:** Parisi himself was very much aware of the fact that his theory was mathematically unorthodox. I very vividly remember an afterdinner discussion in a Cargèse café between Parisi and the Harvard mathematician Raoul Bott. Parisi asks Bott can you define a matrix with zero elements? Bott tried for a few minutes to find a rigorous definition. He suggested something about type II von Neuman algebras. Then Parisi asks can you define the permutation group of zero elements? And then he says this should be an infinite group. If n=m1xm2 then the permutation group S(n) has S(m1) and S(m2) as subgroups. Now 0=m x 0, i.e., S(0) has all the permutation groups with finite elements m as subgroups. Bott was very puzzled and discouraged and this was the end of the discussion as I remember it.

[40] Raoul Bott: https://en.wikipedia.org/wiki/Raoul_Bott

ces with zero elements mathematically, but he was very positive in his reaction. Not at all negative. Then, Giorgio asked: "Can you define the permutation group of zero elements." And he added: "This permutation group should be an infinite group, for the following reason. If $n = m_1 \times m_2$, it has as subgroups, the permutation group of m_1 elements and m_2 elements. As $0 = m \times 0$, all the permutation groups with finite elements m should be subgroups of the permutation group of zero element. But this was too much, as I remember, for Raoul Bott. Also, it was late in the night. The discussion stopped somewhere around that time.

PC: What was your reaction to the ideas of replica symmetry breaking. How did it look to you, as a theoretical physicist, at that point?

NS: [0:38:29] As a miracle. But this was not the first miracle coming out from Giorgio, so I was not as shocked as other people would have been. Giorgio had made many conjectures. It would be too technical to mention them. In field theory, lattice gauge theories, Giorgio had made conjectures, which turned out to be always correct.

At that time, there was a leading field theorist in Europe named Kurt Symanzik[41]. (Kurt Symanzik is not alive anymore.) This may seem strange, because the styles are completely different, but Kurt Symanzik was a very good friend of Giorgio. When Giorgio was making his sometimes wild speculations, which were backed by computations, in field theory, sometimes later Symanzik was confirming that this speculation was correct.

I should just give an example of how some of those speculations—I don't remember the details—were based. You know, at that time, people were doing extremely complicated theoretical calculations. I asked Giorgio: "How did you come to this conclusion?" And he said: "Look at this paper". It was a completely un-understandable paper by some Russian authors. (You know, at that time, there were some papers with very obscure calculations.) He said: "If you take this paper, and if you change this as an hypothesis in the beginning, you will get to this conclusion." How was he able to go through this Russian paper? It was not simple intuition. He had some other arguments to justify his conclusion.

Maybe this is not the place, but I would will to quote another example. This was about spin glasses. I went on another visit to Rome. Marc Mézard—we'll come to Marc later—had done some calculations, and Marc asked me to bring the calculations to Giorgio in Rome. Immediately, Giorgio looked at the calculation and made a conjecture what the results of the

[41] Kurt Symanzik: https://en.wikipedia.org/wiki/Kurt_Symanzik

calculations were. He had to use some properties of some special mathematical functions. We were sitting in a room Miguel Virasoro[42], Giorgio and myself. Miguel takes the book of all mathematical formulae of special functions. Then, he says: "Your conjecture is not correct, according to the book." So Giorgio becomes angry, takes the book, and says: "The book is wrong!" Then, he turned to the previous pages, he found other formulae, and he displayed that there were contradictions in the formulas in the book. He had such a strong intuition, that he was able to claim that the book—I don't remember if it was Bateman[43] or another one of the classical books of special functions[44]—at this point was wrong.

PC: You started working on replica symmetry breaking ideas a few years later. In your notes again[45], you mentioned that Claude Bouchiat suggested you work with Marc Mézard on these ideas. What convinced you to jump on this bandwagon? And do you know what were Bouchiat's motivations in suggesting that you pursue this?

NS: [0:44:38] Bouchiat didn't have in mind spin glasses. He had in mind statistical physics in general. Marc was a high-energy PhD student with Claude Bouchiat, who was his adviser. I don't know the reason why Bouchiat suggested to Marc to change subject. I think that the personal interests of Bouchiat had shifted, but this is a conjecture of mine. For example, the first statistical mechanics paper of Marc[46] was not on spin glasses. In fact, we started working together, and I suggested him a problem, which he did by himself[47]. It was on a conjecture by Toulouse and Lacour-Gayet that in the large *n* limit for random fields, there was dimensional reduction[48]. This paper inspired the famous work of Imry and Ma. But the paper of Toulouse was qualitative, it needed some better, more rigorous treatment. That's the first statistical mechanics paper, as far as I remember, that Marc Mézard wrote.

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

[43] Bateman Manuscript Project: https://en.wikipedia.org/wiki/Bateman_Manuscript_Project

[44] Possibly Abramowitz and Stegun: https://en.wikipedia.org/wiki/Abramowitz_and_Stegun

[45] **NS**: Marc Mézard was at that time a high-energy PhD student. Following the suggestion of his thesis advisor, Claude Bouchiat, we started looking together at problems in statistical mechanics.

[46] C. Bouchiat, P. Meyer and M. Mézard, "Inclusive observables and hard gluon emission in neutrino deep inelastic scattering," *Nucl. Phys. B* **169**, 189-215 (1980). https://doi.org/10.1016/0550-3213(80)90029-2

[47] M. Mézard, "Large-N reduction in spin systems and Griffiths singularities," *Nucl. Phys. B* **225**, 551-564 (1983). https://doi.org/10.1016/0550-3213(83)90533-3

[48] P. Lacour-Gayet and G. Toulouse, "Ideal Bose Einstein condensation and disorder effects," *J. Phys.* **35**, 425-432 (1974). https://doi.org/10.1051/jphys:01974003505042500

To come back to your question, what factor made me and other people jump on spin glasses? This was a seminar Giorgio gave in the fall of '82 in Bures-sur-Yvette[49]. Giorgio was visiting often. This seminar was before the publication of the paper on the physical meaning of replica symmetry breaking, the existence of several quasi-equilibrium states and their distribution in configuration space[50]. And he was making the connection between real configuration space and replicas, and how you could compute properties in configuration space for the physical system using replicas. This paper was published later, I think maybe in the spring of '83. This paper is, in my opinion, extremely important, because up to then replicas were just a magic computational trick. This paper shows that replica symmetry breaking had a deep physical content.

I decided that I should get serious about replicas. Replicas were not just technical. By that time, Miguel Virasoro, who came as a visiting professor at École Normale in Paris, was sharing an office with me. We were discussing a lot. We were discussing what to do with the problem. Toulouse came with his enthusiasm about this paper of Giorgio. Miguel had never worked in statistical mechanics before. After those discussions, he decided to start to look himself. We started the three of us, Marc, Miguel and me, also discussing with Toulouse from time to time. Discussing and doing computations, to find out the consequences of this paper by Giorgio.

Miguel, during the weekends, from time to time was visiting his family in Rome. Then in one of these occasions he went to see Giorgio and told him what we were doing. Giorgio, it turned out, had started most of the things already by himself, alone. We started communicating. We were telling the results of our computations, and Giorgio telling us what he was doing. At the end, we came up with two papers[51].

We had discovered the ultrametricity structure of the space of spin glass states, and also the absence of self-averaging for the order parameter, which was, I think, wrong to be conceived as a big surprise. The reason I am saying that is that at the '78 Les Houches school[52], Phil Anderson in his

[49] Institut des Hautes-Études Scientifiques: https://en.wikipedia.org/wiki/Institut_des_Hautes_%C3%89tudes_Scientifiques

[50] G. Parisi, "Order parameter for spin-glasses," *Phys. Rev. Lett.* **50**, 1946 (1983). https://doi.org/10.1103/PhysRevLett.50.1946

[51] 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. Phys.* **45**, 843-854 (1984). https://doi.org/10.1051/jphys:01984004505084300

[52] Les Houches, Session XXXI, July 3-August 18, 1978. Cf. *La Matière mal condensée/Ill-Condensed Matter*, Ed. R. Balian, R. Maynard, G. Toulouse (Amstredam: North-Holland Publishing, 1979).

lecture, says that it is not at all obvious that self-averaging should be correct, and people should look at violations of self-averaging. But I think at that time Phil Anderson was the only one saying that.

PC: Can you give us a bit of a flavor of how the work would proceed? You said that Marc was working with you. Would you be assigning computations or would you do computations in parallel? How would that work?

NS: [0:53:21] In parallel and with Miguel, but we were seeing each other every day. With Miguel, I was sharing an office and Marc came to our office several times a day to show us his calculations.

I should come back to the paper of Parisi on the physical interpretation of replica symmetry breaking. I should insist very much on that. Even today I know several famous people who have not understood the significance of this paper. One who understood it immediately was Phil Anderson. That's why in my notes I insist that the text of Phil Anderson to be included here as I have it my notes[53], because I think it is the best explanation I have seen of the importance of replica symmetry breaking.

I would [like to] say—many years of hindsight later—what we knew about scenarios of phase transitions. The first scenario was, I would say, Landau's scenario of phase transition, where you have spontaneous symmetry breaking[54]. For example, in a ferromagnet. The [second] scenario was that of Berezinskii, Kosterlitz and Thouless[55]. This is the topological scenario of phase transitions. I think Giorgio's contribution is a new paradigm of phase transitions, where there is neither symmetry breaking nor topological order. This is something really new and important. Giorgio's breakthrough allowed applications of statistical mechanics to several other domains outside the field of physics, like optimization or computer science, or even

[53] 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
"The idea of replica symmetry breaking and its interpretation reveals the fascinating phase space structure of spin glasses. This method has far-reaching significance since it enables one to apply statistical mechanics to a system which technically speaking does not obey statistical mechanics at all because ergodicity is broken and, worse still, because no a priori knowledge about the pattern of this breaking down is available. In order to apply conventional statistical mechanics to systems in which ergodicity is absent due to symmetry breaking, one has to know the order parameter of the system [...] Equilibrium statistical mechanics becomes inadequate without such information. [...] The power of the replica symmetry breaking formalism lies in that no such information is needed."
[54] Landau Theory: https://en.wikipedia.org/wiki/Landau_theory
[55] Kosterlitz-Thouless Transition: https://en.wikipedia.org/wiki/Kosterlitz%E2%80%93Thouless_transition

models of associated memory as the Hopfield model[56]. I think probably Anderson was the first to see that things happening in the future.

I should add another point. I met Anderson for the first time in the summer of ’85. At that time, he had not yet published, but he had finished his paper on graph bipartitioning with Fu. In this paper, he had shown that there was a connection about the optimal solution of a certain graph bipartition problem with the ground state energy of the SK model of spin glasses. So you had two completely different methods of computation. For spin glasses, this was the replica calculation of Giorgio; and for the graph bipartitioning problem there were computations by computer scientists, numerical simulations. Anderson realized that the results were identical. He told me that this convinced him that the solution of Giorgio was correct. This was in the summer of ’85, this played a certain role. Because if Anderson says he’s convinced about the correctness of the solution, many people follow him.

I would say that Anderson probably was a very important singularity in the condensed physics community at that time. Giorgio had big difficulties in getting his paper accepted in Phys. Rev. Letters. We suspect who was the referee, but I would not say that. The reason we are suspecting him is that he was insisting that many references be made to his own work. Finally, Giorgio wrote a letter to the editor of Phys. Rev. Letters saying that the referee should not be allowed all the time to ask references to himself. The editor replied: “Your point is well taken.” He’s a very well-known person in the community, this referee. Just to tell you, I know other people, very famous people, who did not believe the solution. Bert Halperin, for example, I discussed with him at that time. (He was not the referee.) There were other people.

One person—yes, maybe I should add that—who seemed pleasantly convinced was David Thouless, another important person in the community. The reason I'm saying that is that both him and Giorgio were present in the winter Les Houches workshop organized by Brézin, Toulouse and Gervais[57]. There was at that time an active group on spin glasses, particularly experiments, in Grenoble. There was a speaker from Grenoble—I’m not sure who he was, probably Souletie—who presented the first version of the droplet model. According to him, the people in Grenoble were calling

[56] See, e.g., P. Charbonneau, *History of RSB Interview: John J. Hopfield*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2020, 21 p. https://doi.org/11280/5fd45598

[57] E. Brézin and J.-L.Gervais, “Non-perturbative aspects in quantum field theory: Proceedings of Les Houches Winter Advanced Study Institute, March 1978,” *Phys. Rep.* **49**, 91-94 (1979). https://doi.org/10.1016/0370-1573(79)90100-5

that *le modèle des nuages*, the clouds model. They were giving paternity of the model to Néel[58]. Probably he didn't have the detailed scaling picture, the exponents, but the intuitive idea apparently was Néel's. Thouless turned down this for the following reason. If you look to the zero-field cooled magnetization as a function of temperature, if you cross the transition point, the magnetization becomes—experimentally—essentially flat with respect to temperature. What Thouless was saying is, first of all, if you have a ferromagnetic phase you should follow their $1/T$ curve down to zero temperature. So experiments are incompatible with this. On the other hand, Giorgio's replica symmetry breaking was predicting that. So Thouless said that this paper seems arbitrary, but the only testable experimental prediction which it makes is about the magnetization. This seems compatible with experiments.

Just to show you—because you asked me what was the reaction of the community—there were a few people. Thouless is not the ordinary condensed matter physicist, neither is Anderson. I think in the US the reaction was pretty much negative. Maybe I'm wrong, but I would say the condensed matter community in the US is less mathematically abstract minded than in Europe, with notable exceptions, of course.

Just an anecdote also. Phil Anderson was coming out for a condensed matter conference in the US, when I saw him. This was on high T_c. "What is your impression from the conference?" I asked him. He said: "You know those people don't know the difference between a pole and a cut." This is a typical Phil Anderson comment.

PC: At about that same time, you developed an interest for computer simulations. Can you tell us how you got to first work with these, and think about them as a tool?

NS: [1:06:37] The motivation was that there were some predictions of Giorgio's theories which were not taken seriously by the other people who did the simulations. For example, the absence of self-averaging for the order parameter, or the ultrametric structure. These predictions were not taken seriously. The people were just doing average over disorder and trying to locate the phase transition and compute critical exponents. What was the structure of the low-temperature phase did not interest people doing simulation at that time. Maybe I need to say an exception was a paper by Peter Young[59], who for the SK model tried to compute the *P(q)*. You're asking me

[58] Louis Néel: https://en.wikipedia.org/wiki/Louis_N%C3%A9el

[59] A. P. Young, "Direct determination of the probability distribution for the spin-glass order parameter," *Phys. Rev. Lett.* **51**, 1206 (1983). https://doi.org/10.1103/PhysRevLett.51.1206

this question, I'm not prepared to answer in detail, but that's my first reaction to your question.

PC: I'm asking because at the same time there were special-purpose computers developed by Ogielski and co-workers[60]. What was your reaction to this?

NS: [1:08:33] This question is interesting. But again, he was focusing on averages and the existence of a phase transition, the computation of exponents, not on the phase structure or something of the low-temperature phase, which turned out to be a very difficult problem.

I would add a comment that was in fact Giorgio's comment at that time. He took the example of the Heisenberg model in two dimensions. $O(n)$ model with n different from one and two. We know that there is no phase transition in these models in two dimensions. But the correlation length goes to infinity at zero temperature. If you look at properties, at length scales smaller than the correlation length, which can be extremely large provided the temperature is low enough, what you will find is a picture consistent with mean-field theory, despite the fact that mean-field theory is not correct and there is no phase transition. I think this has been verified later by François David who did some field-theory calculations. My philosophy was: we don't know what happens, but maybe at low enough temperatures, you could see things similar to what is predicted by mean-field theory.

PC: Moving on to the next topic. You subsequently moved to the study of optimization and error correcting codes. How did you identify the connection between this topic and spin glasses? And then, how did the error-correcting code community initially react to those ideas, to this connection?

NS: [1:12:26] First of all, I learned about error-correcting codes from a seminar given by John Hopfield, at the Collège de France[61]. John Hopfield had a

[60] P. Charbonneau, *History of RSB Interview: Andrew T. Ogielski*, 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.86f6z55x

[61] **NS:** Around 1987 John Hopfield came at ENS as a visiting professor. His visit was very stimulating. In particular he gave a series of lectures on neural networks at the Collège de France. At one of his lectures he presented a neural network model for error correcting codes. This lecture influenced me a lot. I discovered the subject of error correcting codes. The presentation of Hopfield was very inspiring. I had many discussions with him and he was very nice with me, as with everybody else. Suddenly I made a connection with Wilson's lectures on lattice gauge theories in Cornell and I came up with my paper on the connection between error correcting codes and statistical mechanics of disordered systems. I was feeling frustrated because I did not have any contact with the information theory community.

neural network model for error-correcting codes[62]. John Hopfield at that time was from Caltech. You should know—what I discovered much later talking to the experts—that all the essential theory of the error-correcting codes was finished already in the early '60s[63]. People in the community thought this is beautiful but useless, because nobody was using it. The situation changed with the arrival of space exploration, with satellites. Because you make tremendous savings, otherwise you should have put a lot of power on the satellite to communicate without the error-correcting codes. Weight is money, because you have to lift something much heavier to space. I don't remember the number, but people were saying that using error-correcting codes, you would save several tens of millions of dollars per satellite, using error correcting codes.

All of the space program in the US, in the first years, was managed by Caltech, at the Jet Propulsion Lab[64]. At that time John Hopfield was a professor in Caltech. As he's a curious person, he was talking to those people. He had friends in the Jet Propulsion Lab. That's how he got introduced and interested in error-correcting codes. If you see the references he gives, he was referencing Caltech people. So I discovered the subject in the seminar of John Hopfield, who, as usual, is a very bright speaker, very enthusiastic. He communicated to me his enthusiasm.

Then I made a connection in my mind, later, with lattice gauge theories. I thought this is a new way of doing error correcting codes. If you have a pure ferromagnetic spin model, you can apply a gauge transformation and you get what is called a Mattis model. Then, my idea was to transmit the couplings of the Mattis Hamiltonian, instead of the spins. There are more couplings than spins. During transmission the couplings become noisy. You decode by finding the ground state of the Hamiltonian. That was the original idea. I developed these ideas, building dictionaries from spin Hamiltonians to error correcting codes. I thought that this is a pretty new formulation of the problem. I was very enthusiastic, but I didn't have any feedback from the communication theory community. My paper was published in Nature[65]. There were very few people who bought the idea, Bill Bialek[66] and Marc Mézard among them, but both are physicists, not information theorists.

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[62] See, *e.g.*, J. C. Platt and J. J. Hopfield, "Analog decoding using neural networks," *AIP Conference Proceedings* **151**, 364 (1986). https://doi.org/10.1063/1.36240
[63] Error-correction code: https://en.wikipedia.org/wiki/Error_correction_code
[64] Jet Propulsion Laboratory: https://en.wikipedia.org/wiki/Jet_Propulsion_Laboratory
[65] N. Sourlas, "Spin-glass models as error-correcting codes," *Nature* **339**, 693-695 (1988). https://doi.org/10.1038/339693a0
[66] William Bialek: https://en.wikipedia.org/wiki/William_Bialek

Later, I discovered myself that what I was doing was not very different from what other people were doing. This was not a completely new approach, although the connection with statistical mechanics was not made before. If you transmit bits of information you can compute the probability for any sequence to be true, given the output of the transmission channel. The Hamiltonian I proposed was the logarithm of this probability. Finding the ground state of the Hamiltonian was finding the most probable sequence.

In France I tried to get in contact with the information theory community. It turned out to be impossible. I had a colleague in Paris who was the director of the high-energy lab in the University Paris VI. His name is Pronob Mitter. I talked to Pronob. He looked at the paper, and he sent a copy to his brother, Sanjoy Mitter[67], who was the director of LIDS[68], a MIT lab, one of the most important information theory labs in the world. Sanjoy Mitter invited me for one month in 1989 at his lab after reading my paper.

This was the first real contact with the communication theory community. I gave a series of lectures on the connection between error correction codes and statistical mechanics of disordered systems. The people were completely ignorant of the physics but very much interested, and had very great intellectual curiosity. They were finding the connection very exciting, and this encouraged me a lot. After coming back, I understood better what I was doing. In fact, already in this first visit I met Gallager[69]. Gallager was co-director of this MIT lab. He's a very famous person in information theory, and a former collaborator of Shannon[70]. He was very enthusiastic. He encouraged me a lot, so I continued.

Then, I made a second visit in 1998. In the meantime my understanding had improved a lot. In this second visit, people in information theory were very excited, because of the discovery of two new families of error-correcting codes, which were empirically much better than the ones that were used before. They were called turbo codes[71], and LDPC[72], low-density parity-check codes, which were in fact first discovered by Gallager in his thesis. When I gave my talks, Gallager said: "This may have some relations with what I have done in my thesis."

[67] Sanjoy K. Mitter: https://en.wikipedia.org/wiki/Sanjoy_K._Mitter
[68] MIT Laboratory for Information and Decision Systems: https://en.wikipedia.org/wiki/MIT_Laboratory_for_Information_and_Decision_Systems
[69] Robert Gallager: https://en.wikipedia.org/wiki/Robert_G._Gallager
[70] Claude Shannon: https://en.wikipedia.org/wiki/Claude_Shannon
[71] Turbo codes: https://en.wikipedia.org/wiki/Turbo_code
[72] Low-density parity-check codes: https://en.wikipedia.org/wiki/Low-density_parity-check_code

Gallager had a patent on his codes[73], and the patent was owned by a company that had been bought by Motorola[74]. So Motorola was the owner of the patent of those codes, but they were so much discouraged about those codes that a few years before their rediscovery, they did not renew the patent. I'm coming back to this second visit. People knew by numerical simulations that these new codes were extremely good, much better than the old ones, but there was no theoretical understanding why. I thought the methods of statistical mechanics could improve on that. So I came back motivated to understand whether this was true or not.

By that time, when I came back, there was a nice surprise. Sergio Caracciolo[75], from Scuola Normale Superiore in Pisa, asked me to take one of his brightest students as an intern. I said yes. This intern was Andrea Montanari[76]. He hadn't finished his thesis, so he still was working on lattice gauge theories. (You see lattice gauge theories coming again and again.) I convinced him to look with me to those new codes in parallel with his thesis, because he needed to finish his thesis on lattice gauge theory[77]. He was extremely fast, extremely good. He didn't know disordered system physics at all, but he learned extremely fast. He solved the problem of turbo codes[78]. He found that there was a phase transition at least in the replica symmetric approximation. I presented that result in a conference, where David Forney[79] was present.

I had met him in my second visit to MIT. He is a very important person in this coding community. I discovered later that he was the first in history to have conceived and constructed a modem. He had done that in 1968. At the time I met him, he was an MIT professor and vice-president research of Motorola. He had a big prestige. He was a student of Gallager himself, and very well-known in the communications community, because of his big scientific contributions. He got very much excited about the connection

[73] R. G. Gallager, "Error burst decoder for convolutional correction codes," US 3,469,236, Sept. 23, 1969. https://patents.google.com/patent/US3469236A/ (Consulted September 16, 2021.)
[74] Codex Corporation, the original owner of the patent, was acquired by Motorola in 1977. See, *e.g.*, Vanguard Managed Solutions: https://en.wikipedia.org/wiki/Vanguard_Managed_Solutions
[75] Sergio Caracciolo: https://academictree.org/physics/peopleinfo.php?pid=776839
[76] Andrea Montanari: https://pt.wikipedia.org/wiki/Andrea_Montanari
[77] Andrea Montanari obtained his PhD from Scuola Normale Superiore in 2001.
[78] A. Montanari and N. Sourlas, "The statistical mechanics of turbo codes," *Eur. Phys. J. B* **18**, 107-119 (2000). https://doi.org/10.1007/PL00011086; A. Montanari, "Turbo codes: the phase transition," *Eur. Phys. J. B* **18**, 121-136 (2000). https://doi.org/10.1007/s100510070085
[79] David Forney: https://en.wikipedia.org/wiki/Dave_Forney

with the statistical mechanics of disordered systems[80]. I think that if statistical mechanics has now been adopted by a large fraction of the community, this is mostly due to David Forney.

Forney and I decided to co-organize, with Rudiger Urbanke from EPFL, a workshop with physicists and the theorists of communication theory. We organized it, in 2001, in Trieste[81]. There were two tutorials in physics by Parisi and Mézard and two tutorials in communication theory by McEliece[82] from Caltech, and David MacKay[83] from Cambridge. This made all the ideas made known to a wider community. That's how the methods of statistical mechanics in communication theory has spread.

I should make a comment here. Sometimes, I'm having arguments with people from computer science. They say: “What is the contribution of statistical mechanics on the field?” Quite often these are people with negative attitude. But all of them acknowledge one big contribution, the realization of the existence of phase transitions in many of their problems. Those communities, did not know the existence of phase transitions in their problems before. So they acknowledge that this is a major contribution of statistical mechanics in their fields. With phase transitions they discovered finite-size scaling, critical slowing down, the existence of aging, and glassy dynamics etc. Their algorithms share this behavior, and I think this is a big contribution of statistical mechanics in those fields. Up to the point—I did not verify the last year—that at the IEEE International Symposium on Information Theory (ISIT), the big annual conference in communication theory organized by IEEE they have introduced a special session on the methods of statistical mechanics. This is not only for error-correcting code. There is the work, for example, on k-SAT by Kirkpatrick, Monasson and Zecchina[84] and also Mézard *et al*.[85] There has been a very important cross-fertilization.

[80] **NS:** He became very excited with the methods of statistical physics, up to the point to later try himself replica calculations! (Could you imagine the vice president of a big French corporation doing replica calculations?)

[81] Workshop on Statistical Physics and Capacity-Approaching Codes, D. Forney, N. Sourlas, R. Urbanke and S. Franz, May 21-25, 2001, ICTP, Trieste, Italy. http://users.ictp.it/www_users/calendar/cal2001.html (Consulted September 15, 2021)

[82] Robert McEliece: https://en.wikipedia.org/wiki/Robert_McEliece

[83] David J. C. MacKay: https://en.wikipedia.org/wiki/David_J._C._MacKay

[84] R. Monasson, R. Zecchina, S. Kirkpatrick, B. Selman and L. Troyansky, “Determining computational complexity from characteristic ‘phase transitions’,” *Nature* **400**, 133-137 (1999). https://doi.org/10.1038/22055

[85] 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

PC: Between your Nature paper and the turbo codes paper, you worked mostly on your own on those problems. At that time, was it difficult to recruit physics graduate students to be interested? Was it seen as too esoteric from the physics lens?

NS: [1:35:33] In my carrier I had the privilege to be surrounded by exceptionally gifted young people. When I was in high-energy physics I interacted and sometimes collaborated with Pierre Fayet[86], Jean Iliopoulos[87], André Neveu[88], Joël Scherk[89]. In statistical mechanics Bernard Derrida, Marc Mézard, Andrea Montanari and in both high-energy and statistical mechanics with Giorgio Parisi. After Marc left for Orsay I convinced Andrea Montanari, who was hesitant to postulate to a CNRS job.

That's another interesting story. The year he postulated, he postulated to two CNRS commissions (two fields), in CNRS. One was the theoretical physics commission, the other one was information theory, and computer science. This was a particular year, because the government had decided that computer science was a maximum priority field. There were six positions for theoretical physics and 66 for computer science and information theory. He had a very strong recommendation letter, which I saw, from David Forney. David Forney was one of the most well-known researchers in the field. Despite that, he was turned down from the computer science community. They didn't have enough candidates to fill all of the 66 position, and despite that they refused Montanari, because he was an outsider. What they said officially: "He has not published in our journals."

In the physics community, they classified him *sous la barre*, as we say in French, which means there were six positions and they classified him as seventh, with the justification: "He's so good that we should not waste one of our positions. He should be hired by the other commission." As a results, he was not accepted officially.

I made a big fuss about that. Then, the director of École Normale[90] knew the director-general of CNRS[91], and he told him about that. The only legal way to hire Andrea was a decision of the *conseil d'admnistration du CNRS*, which is the highest governing board. You side-stepped all the normal procedures. So Andrea was hired in the end at a junior level by the *conseil d'administation du CNRS*. This seems completely baroque. I think he stayed

[86] Pierre Fayet: https://en.wikipedia.org/wiki/Pierre_Fayet
[87] Jean Iliopoulos: https://en.wikipedia.org/wiki/Jean_Iliopoulos
[88] André Neveu: https://en.wikipedia.org/wiki/Andr%C3%A9_Neveu
[89] Joël Scherk: https://en.wikipedia.org/wiki/Jo%C3%ABl_Scherk
[90] Gabriel Ruget: https://fr.wikipedia.org/wiki/Gabriel_Ruget
[91] Bernard Larrouturou: https://fr.wikipedia.org/wiki/Bernard_Larrouturou

seven years, or something like that. He did an excellent job. Then, he was so good that he was hired a few years later as a professor by Stanford. So he's a professor at Stanford, but was not good enough for the computer science commission of the CNRS.

PC: Throughout all those years, you've kept on working with Giorgio as well. How did that collaboration continue? And how did you remain interested in spin glasses and the RFIM model?

NS: [1:40:49] I'm interested in it, and Giorgio as well. We had some open problems, and from time to time, when we meet, which unfortunately is not very frequent nowadays, we discuss about that and then make some suggestions, do some computations and numerical simulations, and try to understand their meaning.

I don't know if people know well enough Giorgio, but he's always very interested in physics. If you talk about something new, you have some idea or something, he gets interested. Sometimes he transforms your idea to be a nicer one. Then the interaction continues on. Hopefully, there will be a new paper coming out.

PC: During your time at ENS or elsewhere, did you ever get to teach about replica symmetry breaking? If yes, in what context?

NS: [1:42:40] I think the only place I taught about replica symmetry breaking was MIT. I don't remember any other.

PC: Were these special seminars, or was it a formal course?

NS: [1:43:05] A course. Maybe 10 courses. This was in the electrical engineering department of MIT, not in the physics department of MIT.

There was a research institute in Princeton funded by NEC, the big Japanese company[92]. They had a very active research lab. I gave a series of lectures there. Some people were very mathematically minded at that time, and were critical because it was not yet proven that the theory was correct. This was 1990[93].

[92] NEC Research Institute: https://en.wikipedia.org/wiki/NEC_Laboratories_America

[93] NS was then a visitor at the Institute for Advances Study. See, e.g., https://www.ias.edu/scholars/nicolas-sourlas (Consulted September 16, 2021.)

In École Normale, we had many younger people doing the teaching. It was good. We were particularly fortunate to have particularly bright young students. Mézard was young; Rémi Monasson was young. Also, Bernard Derrida did a lot of teaching because he had a job of professor.

PC: Is there anything else about that era that you'd like to share with us that we may I have overstepped or forgotten?

NS: [1:45:40] I don't know. I'd have to look back to my notes, which were more thought about. This is oral, so in the oral you miss things. One thing, which I would like to stress was that those years had a very strong European community. We had European programs.

This started with some personal relations. For example, Toulouse knew well Sherrington in the old years. Then there was this Les Houches schools[94]. Les Houches played a big role. Then, people from Heidelberg had some extra money and they organized a couple of very important workshops[95]. There was Giorgio, who coorganized a meeting in '81[96], in the Accademia dei Lincei[97], in Rome, which is very vivid in my memory, because during the coffee break we heard very strong gun noise, and then there were sirens of police cars. We didn't know what was going on. Later, we discovered that this was the attempt to kill the pope[98]. The meeting was in Academia dei Lincei, which is not very far from the Vatican.

In later years, for many years, there have been European networks of various programs. Every two or three years we were postulating for a new program and we got many of them. The money we got was spent on conferences or postdoc positions. I should stress the important role played by

[94] **NS:** I would like to mention the summer 1978 school session at the Les Houches, entitled ill condensed matter organized by Toulouse, the two Les Houches Winter Advanced Study Institutes one in February 1980 organized by E. Brézin, J.-L. Gervais and G. Toulouse, one in February 1983 organized by Itzykson, Pomeau and myself.

[95] Heidelberg Colloquium on Spin Glasses, University of Heidelberg, 30 May—3 June, 1983. *Heidelberg Colloquium on Spin Glasses*, J. L. van Hemmen and I. Morgenstern (Berlin: Springer-Verlag, 1983). https://doi.org/10.1007/3-540-12872-7; Colloquium on Spin Glasses, Optimization and Neural Networks, University of Heidelberg, June 9-13, 1986. *Heidelberg Colloquium on Glassy Dynamics* J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer-Verlag, 1987), 121-153. https://doi.org/10.1007/BFb0057505

[96] Proceedings of the Conference Held in Rome, May 1981: *Disordered Systems and Localization* C. Castellani, C. Di Castro, L. Peliti, L. eds. (Berlin : Springer-Verlag, 1981). https://doi.org/10.1007/BFb0012537

[97] Accademia dei Lincei: https://en.wikipedia.org/wiki/Accademia_dei_Lincei

[98] Attempted assassination of Pope John Paul II: https://en.wikipedia.org/wiki/Attempted_assassination_of_Pope_John_Paul_II

David Sherrington[99], who all those years was the overall coordinator. He did a really great job. The Italian coordinator was Giorgio Parisi; the German coordinator was Heinz Horner[100], from Heidelberg, John Hertz[101], for the Scandinavian countries and me or Marc for France. We had several meetings, and young people going as postdocs to the collaborating labs. This has been a very active community, which played a big role for the subject in Europe. I think most of the contributions on this subject are coming from European labs.

PC: In closing, have you kept any of the notes, papers, or correspondence from that epoch? If yes, do you have a plan to deposit them in an academic archive at some point?

NS: [1:50:24] Unfortunately not. Francesco knows how many times I have had to change office. The most recent was one month ago. I had to throw away a lot of my notes, because of lack of space in the new office. Unfortunately, I was not careful enough to realize the importance of keeping archives.

PC: Thank you very much for your time and for this conversation.

NS: Thank you very much.

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[99] 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
[100] Heinz Horner: https://de.wikipedia.org/wiki/Heinz_Horner
[101] John A. Hertz: https://neurotree.org/beta/peopleinfo.php?pid=7214